Causality, Realism and the Two Strands of Boltzmann's Legacy (1896-1936)

by

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Preface

The ideas presented here have a relatively long history. It reaches back to the year 1995 when I had finished my master thesis in mathematical physics and became interested in the philosophy of science. In those days, the Vienna Circle had become a great opportunity for me to simultaneously learn philosophy of science and start genuine research work. Here was a philosophy that started with the criticism of the philosophy I knew reasonably well from my earlier studies, by referring to the physics I knew or could quickly understand after working through old textbooks. Quite at the beginning of my reading and writing I bought Karl von Meyenn's nice collection about the Forman debate. I was stunned. Frank's book on causality from which I learned quite a lot, to Forman's mind, contained "largely blather". There was Hans Reichenbach who despite all scientific world-view (not: weltanschauung) rapidly "confessed" to irrationalism and camouflaged existentialism as Logical Empiricism.

I decided to write an essay review which listed all the misunderstandings of Logical Empiricists I had found. For various reasons the review never made it into print until I understood that this was a fortunate outcome. Criticizing Forman by spotting misunderstandings of still widely unknown figures, among them Frank and von Mises, would have been an original contribution, but not an interesting one. The debates about Forman had already been fought in the 1980s. Everyone knew that one could do better now! Since 1971 history of science has undergone a rapid development not the least as regards it methodological abilities. Among them is the perspective of local traditions which has shown its relevance for the Vienna Circle. I mainly owe it to my partner Veronika Hofer that my interest for and insight into the history of science proper has developed over the years.

The most important discovery for the present book was the personality of Franz Serafin Exner and his unpublished manuscript *Vom Chaos zur Gegenwart* (1923). Exner provides the missing historical link between the older generation of Mach and Boltzmann and the younger generation of Schrödinger, Frank and von Mises. It was through his synthesis of Mach's empiricism, Boltzmann's indeterminism, and Fechner's relative frequency interpretation of probability, that Vienna Indeterminism becomes at all a coherent tradition.

The first outline of the project was presented in June 1997 in a talk at the University of Trieste. It became more pointed in a paper I contributed to a *Synthese* volume on Boltzmann and in a talk given at Florence in 1999. But it only reached it final phase when I went to the University of Bielefeld in 2001. That the already existing bits and pieces finally came together in this book owes much to Martin Carrier's effective insistence to complete it as my belated Ph.D. thesis. His very detailed criticism of earlier drafts has significantly contributed to focusing the book's main thrust.

I am also greatly indebted to Maria Carla Galavotti, Michael Heidelberger, Eckehart Köhler, Friedrich Stadler, and Thomas Uebel, for so many hints and encouragement over the years. Their influence is looming at large in the discussions on probability and the history of the Vienna Circle.

In connection with those papers on which chapters of the present book are based, I owe many thanks to John Blackmore, Erwin Hiebert, Paolo Parrini, Merrilee and the late Wes Salmon, and Roger Stuewer for their criticism and suggestions.

In the final phase of the project, Don Howard, David Rowe, and Erhard Scholz have made very helpful comments on single chapters.

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All my long educational zigzag paths would have been impossible without the almost unconditional support of my parents and grandparents all of whom were still alive when I turned from physics back into philosophy. Sadly, three of them did not live to see the book completed.

Bibliographical Notes

Chapters 3, 4, 5, and 8 partly draw on material already published before. (Stöltzner, 1999, 2000a, 2002b, 2003a) I also owe thanks to various archives. The Institute Vienna Circle has allowed me to study the papers of Schlick and Neurath. For the permission to quote I thank Anne Kox and the Wiener Kreis Stichting. The Österreichische Zentralbibliothek für Physik has most effectively supplied copies from the Schrödinger letters and other material. I thank them for the permission to quote from letters of Schrödinger.

On translations

As the book is historically oriented, I put the German texts first. Different translations of different authors often destroy terminological continuities, possible allusions, implicit assent or dissent. Thus I have often intervened into translations, occasionally even into those published during an author's life time. Several translations have reduced the italics present in the German original. Even though at places this yielded more italics than corresponds to contemporary style, I have reintroduced italics everywhere except for the proper names. If not indicated otherwise, translations are mine.

Introduction

Contemporary debates about the relationship between causality and quantum mechanics, both on the historical and on the philosophical level, have been largely shaped by the conviction that causality is a concept germane to classical physics and becomes problematic or even obsolete on the atomic scale. Philosophers mostly reacted, on the one side, by developing concepts of stochastic causality and by relinquishing the demand for causality as a precondition of scientific explanation, or, on the other side, by developing interpretations of quantum mechanics that restored causality at the price of introducing unobservable entities, if not by altering the theory in a certain regime presently inaccessible to experiment.[1] In philosophical debates surrounding quantum mechanics, causality is often paralleled to determinism and realism while its failure is seen as a sign of indeterminism and an argument in favor of empiricist accounts of explanation.

Historians of science, on their part, have mostly been intrigued by the idea that the relation between acausality and quantum mechanics was a contingent historical fact embedded into the general historical context of the early Weimar republic. That even highly formalized scientific theories were thus susceptible to cultural and social influences, stood in the trend of overcoming the limitations of an exclusively internalist history of ideas and embedding scientific activity into society and culture broadly conceived.

During the last decade these two strands came into close contact – in contrast to the general trend of philosophy of science and history of science moving apart from one another. Philosophers found the historical contingency of the Copenhagen interpretation not only a convincing case of Duhemian underdetermination of theory but also a promising argument in favor of alternative interpretations. Copenhagen, so the standard gospel reads, simply won ugly, by sociological rather than by rational factors, against deBroglie's pilot-wave in 1927. And in 1952, Bohm's new interpretation – originally called 'causal' – was unfairly neglected because hidden variable theories were deemed impossible in a dogmatic fashion. Already in 1982, John S. Bell whose famous inequalities of 1965 had turned the tide in favor of a revitalized interest in the foundations of quantum mechanics, cited[2] Paul Forman's (1971) famous thesis according to which the fathers of quantum mechanics had been so strongly affected by the anti-scientific post-war milieu that they prematurely abandoned the requirement that a theory of atomic phenomena be causal. In 1994 James T. Cushing provided a rather detailed alternative history leading straight into a causal picture without being deviated to Copenhagen, and without any prospect of ever coming there. To a somewhat lesser extent, historians of science have crossed the disciplinary borders via the bridge so erected. Mara Beller (1996, 1999), for instance, supplies vast historical material to bolster the causal picture. All these authors reject any significant influence of well-entrenched philosophical convictions, or the

[1] Take (Salmon, 1984 and 1994) and (van Fraassen, 1980) as examples for the first two reactions. Salmon has always been very careful about quantum mechanics; cf. his reaction to my attempt to connect his view with one particular interpretation (Stöltzner, 1999b, 1999c). Both aspects of the other attitude are most drastically realized by the advocates of Bohmian mechanics (Cushing/Fine/Goldstein, 1996), but in principle all current hidden-variable interpretations in some way or another stress that they are after a causal picture.

[2] (Bell, 1987, p. 166).

protagonists' earlier involvement into philosophical debates, on the debates surrounding the development of quantum mechanics during the 1920s. Quantum philosophy, so Beller holds, was only erected afterwards as a rhetorical tool against the opposition.

These recent developments provide the context of the present book. By combining historical analysis and philosophical interpretation, it intends to alter the above picture in an important respect. The debates about causality in fundamental physics in the Weimar epoch – or so I shall argue – were much more subtle than the dichotomies presently in use suggest. They had begun long before 1918 and they would continue long after 1927. More precisely, in the disputes about Boltzmann's philosophical legacy the prospect of a genuinely indeterministic world-view already emerged in the first decade of the century; it found its outspoken opponents shortly thereafter. Neither in 1918 nor in 1927 the leading physicists were, accordingly, bare of well-developed philosophical convictions on the subject of causality. Although these convictions did not have a decisive influence on the course of the theory's formal development, in some cases, they had a stabilizing effect on the individual scientist's research programs.

It is true, these convictions and even more the terminology they were phrased in, developed under scientific and cultural influences. Their philosophizing was not of that kind of system philosophy which prevailing in the universities of the day. Rather did they follow the well-established model of scientist-philosopher which had emerged with Hermann von Helmholtz, for the German tradition indebted to Kant, and Ernst Mach, for the Austrian empiricist tradition.[3] In the 1920s and 1930s this type of philosophizing was an important element for the formation of precisely that style of scientific philosophy as exercised by Logical Empiricists which would shape modern philosophy of science. No wonder that some Logical Empiricists and some of their masters will figure prominently in the story I am going to tell.

As regards their embedding into academic life, the philosophical debates among German physicists followed a typical scheme. Addresses delivered to the whole university or another learned institution subsequently went through various journals of broader scope and were later assembled into separate books. The appearance of such a book typically testified the author's becoming a scientist-philosopher. After 1913, the debate would mainly take place in one particular scientific weekly, *Die Naturwissenschaften.* This autonomy of the philosophical discourse among the scientist-philosophers frees the present investigation from the notoriously futile problem whether equations are motivated by philosophical concepts or vice versa.

I shall not provide a detailed map of the whole terrain of the German debates on causality in physics between the year 1896 when the first volume of Boltzmann's *Lectures on Gas Theory* appeared and the year 1936, when Logical Empiricists gave their respective views a definitive form, at a time when most of its remaining advocates had already emigrated. But the majority of Forman's witnesses will appear on the scene, and they will appear in two camps, one advocating a break with classical causality and the other developing a more liberal view of causality. Instead of trying to classify a large heterogeneous community, I shall identify a rather well-entrenched dispute about the prospects of genuine indeterminism in physics that waged between

[3] While I shall discuss the role of Mach in great detail, for Helmholtz I refer to (Krüger, 1994) and (Schiemann, 1997).

Vienna and Berlin, from Boltzmann's death in 1906 until deep into the 1930s. Membership in either of the two camps was based upon a certain philosophical stance concerning the relation between causality and physical ontology. Either one followed, together with the Berlinese, Kant by claiming that to stand in a causal relation was a condition of the possibility of being real as a physical object (Kant called this empirical realism), or one agreed with Mach and the Viennese that causality consisted in functional dependences between the determining elements and that physical ontology dealt with facts which were stable complexes of such relations. To those standing in the Kantian tradition, the second stance was too weak and fell short of the aims of scientific inquiry.

This fundamental distinction paved the way for two interpretations of Boltzmann's statistical mechanics. The debate between Vienna and Berlin basically concerned the following aspects. (i) The starting point were the highly *improbable events* that were admitted by Boltzmann's statistical derivation of the second law of thermodynamics. (ii) In a radically empiricist perspective, the intimate relationship between Machian causality and a theory-specific ontology could be used to argue that the burden of proof was with the determinist who must provide a sufficiently specific theory of microphenomena before claiming superiority over a merely statistical theory. (iii) There existed two theories of probability which accommodated the strange events admitted by the second law. In von Kries's *Spielraumtheorie* they were integrated into a deterministic Kantian universe. In Fechner's *Kollektivmaßlehre* there existed collective objects (*Kollektivgegenstände*) and genuinely statistical laws for them which were of no other type than the familiar – apparently deterministic laws.

Vienna Indeterminism is characterized by the full acceptance of the improbable events, a radically empiricist conception of natural law and ontology, and the frequentist interpretation of probability. In this full-blown version, it only began with Franz Exner and was later advocated, with important internal divergences, by Erwin Schrödinger, on the one hand, and Philipp Frank and Richard von Mises, on the other hand. Max Planck set out from rejecting all three tenets of Vienna Indeterminism, but later reconciled himself with (i). His former student Moritz Schlick approached Vienna Indeterminism as far as (ii) is concerned, but never accepted the relative frequency interpretation in its strict Viennese reading.

These three points and the, more fundamental, disagreement regarding the relationship between causality and ontology also provided the background for how both camps reacted to the conceptual crises in atomic physics in the early 1920s and to the advent of quantum mechanics in 1926. While the Berlinese (Max Planck and the early Moritz Schlick) had to substantially modify their notion of causality, the Vienna Indeterminists (Franz Exner, Erwin Schrödinger, Philipp Frank, and Richard von Mises) could just feel themselves constantly confirmed.

Finally, in the mid 1930s, at the end of the European phase of Logical Empiricism, one can see a twist in the front lines that resulted from a change of focus. There was no longer any doubt that the basic theory of nature was statistical, but the problem of quantum mechanical ontology received different answers. Schrödinger was among those who constantly challenged the generally accepted Göttingen-Copenhagen view according to which quantum mechanics was the final word in atomic physics. In the 1930s, his philosophical thinking was conceived against this background so that, to most readers, he appeared to approach the positions of Planck and Einstein. Yet

Schrödinger never abandoned indeterminism and he continued to defend Boltzmann's Bild-realism of scientific theory. It was rather Logical Empiricists who changed their views in an important respect when developing to a growing extent the logical analysis of science. By developing a verificationist theory of meaning Schlick departed from Planck's realism and approached the Viennese empiricists. In this way he could get along with quantum mechanical indeterminism yet without granting statistical regularities the status of law. Frank not only approached Schlick's identification of causality and prediction, but he also took a more language-oriented stand based on Neurath's physicalism. These developments permitted Frank and Schlick to find in Bohr's notion of complementarity – as opaque as this declared generalization of causality was – a territory to search for rapprochement. An important motivation to establish, in this way, a strategic alliance with the Göttingen-Copenhagen group was Logical Empiricists' intention to combat an increasing number of misinterpretations of quantum mechanics in which they conceived a return of outdated metaphysics.

Before providing a more detailed overview of the book and how it is organized into chapters, I shall make some methodological reflections as to how this project sits between philosophy and history of science. I will use both philosophical analyses to sort out the basic convictions of the two historical strands, and historical methods to contextualize the causality debate as a historical phenomenon of its own. And my conclusions will be both historical insofar as I claim the factual integrity of a certain philosophical tradition supported by a historical context and philosophical insofar as Vienna Indeterminism teaches a lesson about the relation between causality and reality criteria that might prove helpful in avoiding certain conceptual confusions and the use of ill-founded pragmatic criteria of theory choice within present-day philosophical debates around quantum mechanics.

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Contacts between historians of science and philosophers of science are not without problems. A recent paper by two philosophers of science – to my mind, representing the majority view – even diagnoses a "troubling interaction" (Pinnick/Gale, 2000). According to standard disciplinary gospel, historians are after narratives, while philosophers' aspirations are normative, even for those who want to ground their normative claims in detailed historical analyses. Hence, the truth of a theory is of little relevance to historians, while philosophers investigate the context of justification of a successful or unsuccessful.

The Forman debate is a case in point, in particular because it served as an influential model for externalist analyses in the history of science. Already its title was quite explicit: "Weimar Culture, Causality, and Quantum Theory, 1918-1927: Adaptation by German Physicists and Mathematicians to a Hostile Intellectual Environment". Forman (1971) listed a great number of physicists and mathematical physicists who, to his mind, sacrificed the demand for a causal explanation of atomic phenomena long before quantum mechanics would force them to do so, simply because they wanted to conform to the requirements of the post-war Weimar zeitgeist. Scientists, in this perspective, were understood as personalities embedded in a particular socio-cultural milieu that could even influence the rational content of their theories. Thus not even physics, the most formalized and most precisely measuring discipline, proved immune against the prevailing ideologems.

The dismissal of internalist history of ideas drastically widened the gap between history of science externalistically conceived and philosophy of science, which in the 1970s stood in the middle of the classical struggles between Karl Popper, Imre Lakatos, Thomas S. Kuhn and Paul Feyerabend. In Lakatosian terms, Forman's externalist picture was decidedly elitist; it were the leading scientists who set the day's standard of what be considered scientific and they succumbed to the zeitgeist in their societal role as *Bildungsbürger*. Accordingly, all demarcation criteria intended to normatively guarantee the rationality of the scientific approach, dissolved into the broad socio-cultural context. Consequently, the sociological approach sided with Kuhn and Feyerabend's 'new epistemology'. Although normal science, so Kuhn continued to hold against Popper and Lakatos, was indeed guided by a well-entrenched paradigm and had fixed normative standards accepted by the elite, across scientific revolutions there was little common ground for the interactions or competitions which Lakatos' Methodology of Scientific Research Programs needed to fully thrive. Albeit not incomparable, the old and the new paradigm were incommensurable, and the revolutionaries could basically only await the old guard ultimately passing the scepter.

There are still many philosophical debates as to whether there are scientific revolutions, whether the incommensurability thesis is at all adequate, whether the scientific enterprise is rational, etc.[4] The present book is not intended as a case study for them. But if the concept is at all meaningful, quantum mechanics undoubtedly represented a scientific revolution containing at least two discontinuities: the first was the emergence of the quantum of action in Planck's law of radiation – here Kuhn (1978) himself contributed a seminal study –, and the second consisted in the almost simultaneous introduction of Heisenberg's matrix mechanics, which openly displayed the limits of the applicability of the concepts of classical physics, and Schrödinger's wave equation, which for a short time promised an return to classical physics.

The present book is dedicated – as has been the Forman debate – to the discussions about one basic philosophical concept, causality, that was strongly influenced by and in turn shaped the interpretative underpinnings of the quantum revolution. To Planck and the early Schlick, the demand for causality was tantamount to the scientific method and implied ontological commitments as to what scientific laws were about. Thus the Berlin tradition experienced quantum mechanics as a revolution while the Vienna Indeterminists largely did not. Due to this central role of causality, the present investigation does not deal with the development or abandonment of one philosophical concept among others. Still, it concerns a single philosophical concept rather than the scientific weltanschauung of atomic physicists in general. This will require a certain level of philosophical sophistication and to this end I shall avail myself of the standard methods of the historiography of philosophy – "Begriffsgeschichte", so to speak – but without tracing philosophical schools gathering around the interpretation a few master texts or stressing their constant adherence to a particular philosophical ancestry. The causality debate was led by leading scientists with philosophical aspirations and by scientist-philosophers rather than by professional philosophers. Together with the more famous debate on the concept of space-time

[4] See, for instance, (Hoyningen-Huene & Sankey, 2001) and the papers in the first section of (Kampis/Kvasz/Stöltzner, 2002).

prompted by relativity theory, this debate contributed to the formation of scientific philosophy, or philosophy of science as we know of today. The existence of such an autonomous philosophical discourse and its internal dynamic must not be neglected by historians of science. It represents a characteristic element of German physics, of no less importance than other academic traditions or institutions.

The present investigation is not after a full-blown hard core of Boltzmann's statistical mechanics and how this legacy developed – continuously or discontinuously – into the hard core of a quantum mechanical research program. Rather does it reconstruct the arguments in a particular philosophical debate that explicitly declared itself as such. It extended over almost four decades and involved three fundamental theories of modern physics: statistical mechanics, relativity theory, and quantum physics. These manifold influences and the long period considered make it impossible to treat both camps respectively as a set of precise philosophical assertions. The above-mentioned three aspects are thus rather a framework to be filled with specific arguments than a fixed set of dogmas. What remained constant, the relation between causality and ontology, had to be adjusted to the respective theories. The divergent tendencies resulting from these adjustments are counterbalanced by cohesive historical factors: formal and informal institutions, among them the Vienna Institute of Physics and the Exner Circle, documented self-identifications with the local tradition by members of the second generation, explicit and implicit criticisms of the other side, the existence of a renowned forum in which large part of the second half the debate took place, and the formation of strategic alliances.

Such a combination of philosophical and historical methods for the study of a philosophical topic emerging from within science proper is characteristic for a budding research field, the history of philosophy of science. This is, to my mind, the most suitable classification of the present book's general approach.

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What stage can the history of philosophy provide for Vienna Indeterminism? There exist national traditions in philosophy in a rather general sense. For a dispute over causality between Vienna and Berlin the paradigm of 'Austrian philosophy' suggests itself. With this term Rudolf Haller (1986a) has baptized an intellectual tradition prevailing in the Habsburg monarchy since the days of Bernard Bolzano that was continued by Franz Brentano and his school, on the one side, and Ernst Mach and the Vienna Circle, on the other side. Its core characteristics include a scientific attitude, anti-idealism, and the rejection of Kant's transcendental philosophy in favor of Hume's empiricism – self-identifications that appear in the 1929 manifesto of the Vienna Circle and in Neurath's later chronicles of the movement. Whatever stand one takes with respect to the Haller thesis in general,[5] one aspect severely limits the value of the Austrian philosophy paradigm for a historical contextualization of Vienna Indeterminism. Together with modern logic and the positivist tradition, general relativity became crucial to the philosophical identity of the Vienna Circle and Logical Empiricism. At about 1920, also the Germans Schlick and Reichenbach, who were by then general relativity's most prominent philosophical defenders and who had received

[5] See among others (Uebel, 2000), (Stadler, 2001), and (Stöltzner, 1998).

their philosophical formation largely by neo-Kantianism, arrived at the rejection of any aprioristic conceptions of space and time however relativized, thus establishing the future front line against neo-Kantian philosophy of space and time.

The present study demonstrates that the front lines on causality were significantly different because Schlick's break with the neo-Kantian tradition would take much longer. Moreover, the main defender of deterministic causality in physics, Max Planck, was at the same time chiefly responsible for getting relativity theory accepted by the German physics community. In contrast, some of the staunchest followers of Mach and Brentano opposed relativity theory or advocated an interpretation that was starkly different from the one that became seminal for Logical Empiricists' epistemology.[6] Still in the mid 1920s, the front line on matters of causality, as we shall see, went right through the Vienna Circle roughly separating the Austrians Frank and Mises from the German Schlick. But the reason of this division reached back to a particular philosophical position shared by both Mach and Boltzmann that was much more specific than just being the general empiricist tendency in Austrian philosophy.

Thus the national philosophical tradition has to be specialized down to the level of the local context which the single scientist belonged to. Research programs and research traditions are typically not only kept together by a hard core and auxiliary hypothesis, but also by scientific communities or other organizational structures. For philosophical concepts such a real-world contextualization is less common. As we shall see, Vienna Indeterminism emerged in a specific philosophical context within the Institute of Physics of the University of Vienna (Sect. 4.7.). There the late Boltzmann developed a more radical form of indeterminism than Planck could read in the earlier *Lectures on Gas Theory* (1896, 1898a). It was passed on to the younger generation, among them Schrödinger and Frank, by the experimental physicist Exner who enjoyed an unprecedented influence on a large circle of pupils (Sect. 4.3.). There are manifold references by Vienna Indeterminists emphasizing Exner's priority for the idea of irreducible indeterminism, most prominently by his former assistant Schrödinger. Being exceptionally cohesive in spirit and more prone to philosophical reflections than comparable institutions, the Institute of Physics also influenced a more informal circle at its periphery, the First Vienna Circle comprised of Philipp Frank, Hans Hahn, Otto Neurath, and occasionally Richard von Mises.

During the 1920s the philosophical causality debate mainly took place in the scientific weekly *Die Naturwissenschaften* which, in the struggles about relativity theory, had become the principal medium for the discussions among the scientist-philosophers, among them Logical Empiricists. (Sect. 5.1-5.4) Restricting the textual basis mainly to papers published there, one loses only very few of the texts studied by Forman. In compensation one can identify a progressive sub-milieu of scientists persisting within the anti-scientific general cultural Weimar milieu. The outer boundary of this submilieu excludes large part of German university philosophy – the 'school philosophy' in Logical Empiricists' wording – and those criticizing modern physics as a whole, among them Philipp Lenard.

Among the texts thus individuated we find a network of affirmative and critical references, in particular when alluding to the Mach-Planck polemics. While the Viennese repeatedly stressed Exner's priority for genuine indeterminism, their

[6] (See Hentschel, 1990).

opponents hardly took notice of him. Apart from such self-identifications, the most important element of cohesion is a long series of dialogues between the Vienna Indeterminists, on the one side, and their opponents Planck, von Laue, and Schlick, on the other side. They either take the form of implicit or explicit criticism in print or are stated only in private correspondence. In my understanding, these dialogues served to elaborate a philosophical position against criticism and in view of a constantly evolving theory of atomic phenomena. Responses and rejoinders were not ad hoc but rather involved a well-entrenched stance on the relation between causality and ontology that ultimately went back to the years of the Mach-Planck debate and a diverging understanding of the role of probability in physics.

Although the motive of dialogue will figure prominently in the present study, I am far from Beller's (1999) concept of dialog*ism* (Sect. 2.2.) because the latter excludes deep-seated philosophical commitments in exchange for rhetorical ad-hoc maneuvers. I am far from denying that scientists often decide to form strategic alliances in order to defend a certain stance against opposition; take the Göttingen-Copenhagen interpretation or Logical Empiricism. But forming such an alliance is a historical fact that does not exclude that in retrospect – or rational reconstruction – the internal divergences appear as strong as the cohesive traits. It just happened that in the concrete historical situation, scientists believed that a certain aspect was more important than others. This distinction will prove important for an adequate assessment of Schrödinger's position.

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Chapter 1 is dedicated to a critical analysis of the Forman thesis. By failing to recognize the aspirations of the scientist-philosophers, it oscillates uneasily between philosophical naiveté and strong conclusions about a philosophical concept, as if this were an ordinary ideologem like the ones figuring in Forman's – to my mind, much more convincing – studies on the ideology of internationalism, science policy, etc. Ironically, these studies prove convincingly that the economical crisis was less severe in German physics than in other disciplines.[7] Section 1.1. provides a critical outline of Forman's arguments. Section 1.2 summarizes relevant aspects of the scholarly debates that have ensued the Forman thesis. Section 1.3. discusses Forman's extension of his original thesis beyond 1927 and with respect to the concepts of *Anschaulichkeit* and individuality. Although *Anschaulichkeit* is philosophically a fuzzy concept and, accordingly, more susceptible to milieu influences, Forman's new case is weaker because, in the historical context of the 1920 *Anschaulichkeit* was tightly connected to the struggles about relativity theory.

Rejecting the original strong Forman thesis of an adaptation of the scientists to the anti-causal Weimar milieu and accepting the weaker form of a strong influence, Cushing (1994) outlines an alternative history of a causal quantum mechanics that he basically identifies with the deBroglie-Bohm tradition. Section 2.1. demonstrates that Cushing has indeed made a convincing case for Duhemian underdetermination. Unfortunately, he has also filed an equal rights claim according to which the deBroglie-Bohm picture should be investigated with priority for the sake of historical

[7] (Forman, 1974), (Forman, Heilbron, Weart, 1975).

justice. This new candidate on the list of pragmatic criteria of theory choice leads into many of the above-mentioned methodological problems between history and philosophy because pragmatic criteria of theory choice are not invariant under historical translation. That, to my mind, Cushing's alternative history ultimately fails, shows that scientists' discussion of philosophical concepts is embedded into the history of science, both internally and externally, to a much stronger degree than he has assumed. If one wants to understand physicists' reaction to deBroglie's pilot-wave theory in 1927, one has to take into account that some of them had been deeply involved into statistical mechanics, relativity theory, and early quantum theory. And – with notable exceptions, it is true – they checked their philosophical world views for consistency with these scientific achievements. Other than in the case of rational reconstruction, to think up an alternative history one has to modify a whole history, not just a single concept, however basic it may be.

At surface, Beller (1999) rejects both counterfactual reasoning and the Forman thesis. On her account studied in Section 2.2., prior to 1927 indeterminism was not pivotal altogether. Neither were the members of the quantum generation driven by other deep-seated philosophical motives. They were just immersed in local and creative dialogues which in 1927 suddenly turned into the creation of the orthodox Copenhagen narrative. This change of attitude was mainly driven by Bohr's and Heisenberg's ambitions to win the clumsy matrix mechanics superiority over Schrödinger's elegant formalism. To this end, they invoked rhetorically-casted philosophical arguments as ex-post justifications. Copenhagen positivism used the verificationist ax to chop down the prospect of a genuine quantum reality emerging from the theory's formalism shared by both sides. As with Cushing's counterfactual, Beller's intention is to prove that Göttingen-Copenhagen interpretation won ugly, that is, by historical contingency rather than by rational arguments.

Beller's method of dialog*ism*, emphasizes the *ad hoc* character of argument building and is thus directed explicitly against the idea that the leading quantum physicists advocated genuine philosophical positions. In both parts of her book this conclusion emerges simply from the perspective taken, because the dialogist grid either is too fine, in the local dialogues prior to 1927, or too coarse, when the orthodox narrative is directly compared to the Bohm tradition of the 1950s and 1990s.

The seminal dialogue for the present book was the polemics between Planck and Mach that sparked the German physics community in the years from 1908 to 1910. It is commonly believed that Planck's (1908a) Leiden speech expressed the farewell to his early sympathies for Mach's positivism and was motivated by his insight that Boltzmann's statistical mechanics was inevitable for the quantum theory of radiation. But this picture needs qualification because it overemphasizes the role of the notorious struggle about atomism for the protagonists' philosophical convictions. A more detailed analysis reveals a striking continuity between Mach and Boltzmann, on the Viennese side, while one discerns core elements of Planck's epistemology already in his early book on the principle of energy conservation. Chapter 3 deals with the first phase of the debate in its entirety.

Vienna Indeterminism was made possible by Mach's redefinition of causality in terms of functional dependences between sensory elements. (Section 3.1) Mach's ontology was based on facts which were constituted by relatively stable complexes of such functional dependences. Going beyond Hume, Mach expressed them in terms of concrete physical equations. He called these laws *direct* descriptions and opposed them to *indirect* descriptions, such as atomistic theories, which involved hypotheses. But in order to guarantee the integrity of functionally constituted facts, Mach had to posit an ontological principle of unique determination of the actual fact in comparison to all variations of its functional dependences. (Section 3.2)

As would the Vienna Indeterminists, Mach held that for the empiricist it was impossible to finally decide between determinism and indeterminism. Nevertheless, he still favored determinism as a regulative principle because only thus could probabilities make sense. While Mach consequently agreed with his opponent Planck that all probabilities required a determinist foundation, Boltzmann was surprisingly vague with respect to this most fundamental concept of his statistical mechanics. (Section 3.3) He simultaneously clung to the old concept of equiprobability – which was either based on causal relations or on their absence due to our ignorance – and emphasized against Planck that the highly improbable entropy-decreasing events could really occur. Only once did Boltzmann endorse in passing the *Spielraum* (range) interpretation of Johannes von Kries (1886). Accepting – at least since the mid 1880s – core tenets of Mach's epistemology, Boltzmann sought to give a proper ontology to the atoms of kinetic theory by means of a twofold reality criterion. On the one hand, he conceived of atomism as property reduction to theoretically defined universal entities and their interactions. Theories, in this perspective, were regarded as pictures (*Bilder*). (Section 3.4) On the other hand, atomism was already implied by humans' finitary reasoning powers that made it impossible to actually assess the continuum. At this point, Boltzmann surprisingly endorsed Mach's empiricist understanding of mathematics.

Boltzmann died two years before the polemics between Mach and Planck took off. Through them Planck became widely regarded as a philosopher. Compared to the profound analyses of his contributions to black-body radiation (Kuhn, 1987) and his eminent role in German science (Heilbron, 1988), Planck's involvement into contemporary debates with other scientist-philosophers has hardly been investigated. Section 3.6. prepares for the Mach-Planck polemics by reading Planck's 1887 book on the principle of energy conservation against the backdrop of Mach's historico-critical methodology. I argue that Planck's insistence on thermodynamics' being based on two independent law-like principles and his views on principles in general and causality already by then ran against Mach's approach. Undoubtedly, Planck's later interpretation of scientific progress was plainly anti-Machian, since he believed that outdated absolute concepts are relativized just in order to find deeper absolute concepts. In the controversy with Mach (Section 3.7.), Planck devised a convergentist argument in favor of metaphysical realism. Granting the Kantian proviso against knowing the thing-in-themselves, we could nonetheless approach them stepwise and, at the horizon of the right path, we envisage some relativized a prioris that direct our further step, among them causality. Section 3.8. shows that Planck also advocated a structural realism that was based on the idea of formal invariance as provided by the Principle of Least Action. The final Section 3.9. is dedicated to the consequences of the three protagonists' understanding of culture inasmuch as it was influenced by their epistemology.

Chapter 4 deals with Exner's amalgamation of Mach's radical empiricism and Boltzmann's late indeterminism. Exner's synthesis involved a substantial shift in the understanding of probability. Viewing Boltzmann's difficulties with the ontological status of probabilities, it is surprising that he never cited Fechner's (1897) posthumously published frequency interpretation of probability. Shortly after Boltzmann's death this move was made in Exner's (1908) inaugural speech as Rector of the University. Frequentism henceforth became a characteristic trait of Vienna Indeterminism that distinguished the tradition against Planck and Schlick who remained committed to von Kries's interpretation of probability.

As Exner built physical ontology upon collectives, he had to defend a rather firm empiricism in Machian footsteps because only in this way could he jettison all worries as to whether there had to be some unobservable deterministic laws at the deepest level. In his polemics with Planck in the late 1910s, Exner emphasized that all apparently deterministic laws could well be the macroscopic limit of indeterministic basic laws valid for the single particles or events. Planck instead remained committed to a deterministic foundation for the definition of physical probabilities. (Section 4.5.) More than anything else, Exner's synthesis paved the way to accept genuine indeterminism in physics without any reference to quantum mechanics.

Exner's indeterminism did not halt at the borders of physics or natural science. Prompted by Oswald Spengler's *Der Untergang des Abendlandes,* by the end of his life, Exner (1923) wrote a comprehensive physicalist theory of culture that embraced all history from the formation of the solar system until the present state of Western civilization. A major driving force of progress and decline was the second law of thermodynamics. The resulting distinction between indeterminism at the microscopic level of the individual and determinism on the very very large scale permitted Exner to simultaneously accept and reject the pessimism about cultural decline prevailing in the Weimar epoch. Science was robust enough to escape Spengler's morphological cycles. Section 4.6 amounts to a digression from the causality debate, but its import on the validity of Forman's thesis is pivotal because Exner, to my mind, represented a typical example of a Ringerian Mandarin in physics, a concept which stood at the back of the Forman's milieu analyses.

Chapter 5 is dedicated to *Die Naturwissenschaften* that became the forum and cultural sub-milieu for the causality debate after 1913. Modeled after the British *Nature,* this scientific weekly strove to follow the major developments within the whole of natural science and present them in a generally comprehensible and captivating form. Philosophical debates in this journal did not simply happen, but were carefully planned by the founding editor Arnold Berliner. Section 5.1. thus documents Berliner's editorial activity until he was ousted under Nazi pressure in 1935, his unique personality, and his views about the unity of science and culture. Some of the recollections testify the enormous influence of Berliner on the cultural identity of the younger generation of German physicists. Section 5.2. shows how *Die Naturwissenschaften* became an important stronghold in the 'defense belt' around Einstein and relativity theory. Standing in for the ideals of science also meant to take a firm stand against the Spenglerian challenge (Section 5.3.). Together with the views of Berliner himself, I take this attitude as indicating that *Die Naturwissenschaften* represented – at least for a large group of younger physicists – a well-entrenched socio-cultural sub-milieu based on some general convictions about the positive role of science in society that made its members resistant to the influences of the general milieu. This did not exclude that the same physicists, in their private role as

Bildungsbürger, reacted very differently to the feeling of a cultural decline. To my mind, the undifferentiated concept of milieu is one of the weakest points of the Forman thesis.

In Section 5.4., I provide an overview of the philosophical topics discussed in Berliner's journal and conclude that apart from a rather generally oriented education program, Berliner gave ample space to authors from the emerging tradition of Logical Empiricism. Simple counting of papers shows that *Die Naturwissenschaften* represented the principal public medium for the scientist-philosophers in the Vienna Circle, that is, for Frank, Schlick, and von Mises, until *Erkenntnis* was founded in 1930. Section 5.5. depicts an early debate about causality in atomic theory between Walter Schottky, Boltzmann's former collaborator Walter Nernst, and Mach's former ally Joseph Petzoldt. We find Nernst strongly indebted to Boltzmann's statistical mechanics yet without taking the late Boltzmann's Machian tack. Schottky redefined the concept of causality in such a way as to meaningfully speak about stochastic causality. Petzoldt saw no need of action because, to his mind, the Machian notion of causality was wide enough to accommodate all problems of quantum theory.

The reception of Exner's synthesis was typically limited to those who had closer contacts to Vienna physics. As Chapter 6 shows, Erwin Schrödinger persistently stressed Exner's priority in contemplating genuinely indeterministic laws of nature. (Section 6.1) Schrödinger followed Mach's neutral monism and developed a pronounced unease against the Göttingen-Copenhagen emphasis upon observers distinct from objective reality. Quite in line with the early phase of Vienna Indeterminism, he was searching an ontology for his wave equation in the sense of Boltzmann's universal and coherent pictures; yet neither the wave function nor – in later years – unified field theory brought him success. But he continuously rejected Copenhagen's commitment to macroscopic concepts, such as particle trajectory, the applicability of which to the atomic domain was limited in principle by the theory itself. Having been elected Planck's successor at the University of Berlin in 1927, he continued Exner's debate with Planck. Yet whereas in his 1922 Zurich inaugural speech he had considered the alternative between determinism and indeterminism as an empirical question – as had Exner – in 1929 he took a conventionalist tack and called the choice a matter of practicability. In 1931, Schrödinger dedicated two papers to his former teacher Exner. While the first represents his most mature plea for Vienna Indeterminism, the second searches for general characteristics of the cultural and scientific milieu of a certain epoch. Put against the backdrop of Exner's inaugural address, it reveals that Schrödinger rejected one element in his teacher's indeterminist theory of culture conformed to Spengler's idea of cultural cycles. All this will be discussed in Section 6.3. Before, I briefly discuss those aspects of the comparatively broad literature on Schrödinger which have resulted from projecting back his matured post-war views about quantum mechanics into his stance of the 1920s and 1930s. (Section 6.2.).

Chapter 7 accompanies Schlick from Berlin to Vienna in the transition from a rather Planckian view of causality to one that enabled him to fully accept the main tenets of the Copenhagen interpretation without subscribing to Vienna Indeterminism. Schlick's ultimate breach with Kantian roots went parallel to the final dismissal of Planck's convergent realism, the sharpening of the verificationist criterion of meaning, and the rising influence of Wittgenstein on his thinking. What Schlick, however, never accepted was the frequency interpretation of probability. Moreover, he remained committed to von Kries's *Spielraum* interpretation.

Schlick's first paper on causality was almost exclusively oriented at relativity theory. In order to rescue the principle of causality from triviality, he required that in causal laws space-time coordinates should not have any absolute significance. (Section 7.1) Still in the mid 1920s he openly took Planck's side in the polemics with Mach. On Schlick's account, the statistical character of the second law was not situated in the laws themselves, but in the initial conditions. "It is clear…that only in utmost extremity will the scientist or philosopher resolve to postulate purely statistical micro-laws" (Schlick, 1925, p. 461/61). By 1925, Schlick became increasingly aware of the problems of deterministic causality in the atomic domain; but his respective statements rather resembled a hybrid of continuing his 1920 position and admitting, as a good empiricist, that the principle of causality might have to be abandoned. (Section 7.2.).

When in 1926 the emergency case had actually happened Schlick, after five years of remaining practically silent about causality, made a complete turnaround and in 1931 he renounced all attempts to explicitly characterize the causal character of laws (Section 7.3.). "Verification as such, the fulfillment of prediction, confirmation in experience, is therefore the criterion of causality *per se*" (Schlick, 1931, p. 151/188). And commending Wittgenstein he emphasized that "at bottom a law of nature does not even have the logical status of an 'assertion', but represents, rather, a 'prescription for the making of assertions'" (Ibid., p. 151/188). Schlick's new theory permitted him to appraise Copenhagen's insight that the Heisenberg's uncertainty relations represented an in principle limit on prediction set by the laws of nature themselves, and simultaneously cling to the idea that all statistical regularity could be separated into strict laws and pure randomness – a remnant of the old distinction between nomological and ontological regularity on which the Kriesian theory was based. Schlick received many reactions from leading physicists of the day (Section 7.4.). They show that in contrast to his verificationist reading of Heisenberg's uncertainty relations, core parts of his new theory of causality were quite orthogonal to the already entrenched front lines on the interpretation of quantum mechanics. That Schlick refused to consider statistical 'laws' as genuine laws found almost unanimous objection, most pointedly, of course, by Schrödinger. In correspondence with Sommerfeld, Schlick explicitly rejected Mach's 'principle of the sloppy laws of nature' and together with other reactions one can conclude that Schlick was still much closer to the German philosophical background than to Vienna Indeterminism.

At that time, however, Schlick had long joined forces with the two Vienna Indeterminists in the Vienna Circle. Chapter 8 discusses the positions of Frank and von Mises together not only because of their common intellectual origin, but particularly because of the many affirmative cross-references in their works. In his first philosophical paper in 1907, Frank had considered the general law of causality as a mere convention. This position was motivated by the First Vienna Circle's reading of French conventionalism and did not quite conform to Frank's subsequent adherence to Mach's philosophy of science. (Section 8.1.) In his 1932 book *The Law of Causality and its Limits,* Frank largely revoked his earlier conclusion as too rash and investigated in greater detail the conditions under which the general law of causality attains an empirical content. (Section 8.5.) But he came up with a negative conclusion. There existed neither a proof of the validity nor any empirical consequences of the general law of causality. Nevertheless, we constantly presuppose the existence of special causal laws in daily life. In the introduction, Frank emphasized that his change of mind was caused by quantum theory and by von Mises's "conception of statistical laws and their relation to dynamic laws" (Frank, [1932] 1988, p. 24/12). According to this view discussed in Section 8.2., both types of law did not compete with one another; they simply concerned different observational facts. Just as the Newtonian dynamical laws govern the motions of point particles, statistical laws deal with mass phenomena which are represented by statistical collectives. Von Mises explicitly criticized Boltzmann's formulation of the second law as a blend of microdeterminism and macroprobabilism, and advocated a purely probabilistic approach instead. Von Mises and Frank gained the freedom to choose collectives as a proper ontology by supplementing Mach's concept of causality with the idea that all concepts in physical theories are coordinated to specific experiences or measurements. (Section 8.3.) Thus, in 1932, Frank could simply argue that the only modification in quantum mechanics was the statistical character of this coordination. To his mind, *Anschaulichkeit* had little space within the scientific world conception so described. If not interpreted as actual sensation, *Anschaulichkeit* typically gave preference to an outdated mechanistic world view. In both relativity theory and atomic physics physicists had seen more clearly than ever before that the realistic interpretation of auxiliary concepts was problematic. Frank's 1928 paper amounted to a vigorous protest against what Forman takes to be a wide-spread demand of the milieu. (Section 8.4.)

The final three sections of the book are dedicated to the rapprochement between both strands of the causality debate. Large part of this development, so I shall argue, was the sharpening Viennese focus on linguistic analysis of scientific theory. Schrödinger who remained committed to Boltzmann's conception of theories as picture thus, in one of the final dialogues in Berliner's *Naturwissenschaften,* found himself on von Laue's side and against von Mises (Section 8.6.). On the 1936 Copenhagen Congress for the Unity of Science, Bohr's concept of complementarity and the idea of a physicalist language provided the basis for a far-reaching agreement of Frank and Schlick (Section 8.7.). During the days of the congress, Schlick, who could not attend due to university regulations, was killed by a former student. And two years later, all major protagonists of the present book had emigrated. Here not only the causality debate, but also the European phase of Logical Empiricism terminated. (Section 8.8.)

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Let me add some words about four philosophical border lines drawn by the present investigation. I have characterized the tradition of Vienna Indeterminism by a specific relationship between the concept of causality and theory-specific reality criteria, by three specific thematic aspects, by a continued dialogue with Planck, and by other historical contextualizations. There are a few other scientist-philosophers and philosophers proper who thus have been excluded despite their partial interaction with Vienna Indeterminism.

The first border line emerges from the distinction between the physical probability – be it based on relative frequencies or ranges– and, thus, the issue of indeterminism from the probability of judgment. All scientist-philosophers discussed in the present study strictly maintained this distinction while Reichenbach, who himself advocated the statistical character of natural laws, did not. He openly rejected such a distinction and thus claimed to arrive at a probabilistic solution to the problem of induction. Apart from his open polemics with von Mises, this issue ultimately estranged him from the movement of Logical Empiricism in the 1930s. I have excluded a more detailed discussion of Reichenbach's position with a heavy heart, even his dialogue with Schlick.[8] But this would have inevitably led into much deeper investigations of the views on probability within Logical Empiricism including Rudolf Carnap's intermediate position.

Second, I have excluded a separate discussion of von Kries's theory although it was outlined on the pages of *Die Naturwissenschaften* and von Kries there (1919) commented upon an earlier paper of the deceased Marian von Smoluchowski (1918), a former assistant of Exner. But von Kries was not only a contemporary of the 1920s advanced in years. His theory of probability (Kries, 1886) had been written at about the same time as Fechner's (1897), and he never made significant modifications to it. This has conduced me to consider von Kries's works as part of the philosophical background and treat his interpretation thus on a par with the rivaling relative frequency interpretation of Fechner.

Third, it is true, there are other scientist-philosophers who repeatedly contributed to the foundations of quantum mechanics in *Die Naturwissenschaften,* among them Niels Bohr, Max Born, Werner Heisenberg, and Hermann Weyl. But they did not embark onto a debate with the Vienna Indeterminists, apart from the correspondence with Schlick (Section 7.4.) and the Vienna Circle's joint appearance with Bohr in Copenhagen 1936. Moreover, all four were strongly influenced by philosophical views which Logical Empiricists branded as "school philosophy", rather than by a neo-Kantian or Machian background. Accordingly, the Mach-Planck debate did not play such a significant role for their views.

Fourth, the rapprochement between Schlick and Vienna Indeterminism is followed by the convergence between Logical Empiricists and Ernst Cassirer, the heir of Marburg neo-Kantianism. Cassirer's *Determinism and Indeterminism in Modern Physics* (1937) made ample reference to the material discussed in the present book and at the end, the author emphasized that the gist of the matter was lying in the distinction between causality and the object figuring in the laws. Cassirer traced this thesis back to his 1910 book *Substanzbegriff und Funktionsbegriff* which had focused on the dissolution of substantialism in modern philosophy, a tendency which also provided the common background of Mach's reinterpretation of causality and Schlick's first theory of causality. Although in the end, Cassirer, as Planck, opted for maintaining a strongly relativized a priori notion of causality, Frank's review of the book was laudatory and spotted there the core thesis of Vienna Indeterminism.

> A further principal feature of Cassirer's account is that the form of the law of causality and the concepts of what one calls an *object* mutually presuppose each other. Also this is a basic thesis defended by logical empiricism which has been taken over from positivism. Today's positivism just gives this thesis a more formal turn. (Frank, 1938, p. 73)

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8 (Schlick, 1931) contained a criticism of Reichenbach's views; the response was (Reichenbach, 1931).

I have refrained from a discussion of Cassirer's work and Frank's reaction to it because treating Cassirer's book just as a chronicle of the debates investigated here, would not do justice to his the depth of his philosophical views. It remains, however, a supportive evidence for the present investigation that right at the end of the period at issue here, two philosophers so different as Cassirer and Frank agreed that the core problem as regards the alternative between determinism and indeterminism was the relationship between the law of causality and a suitable reality criterion and that both of them ultimately referred to Mach's criticism of the concept of causality.

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The present book teaches three lessons. The first is directed at the historiography of Logical Empiricism. While there has been much research concerning the departure from the Kantian categories of space and time – a move that was one of the cornerstones of Logical Empiricism side by side with modern logic – Vienna Indeterminism and its dialogues with opponents show that the respective change for the category of causality took considerably longer and happened only piecemeal. In some respects, the rapprochement between Schlick, Frank, and von Mises left substantial disagreements unsettled. This diagnosis joins in with many other recent results about the very subtle fine-structure of the movement of Logical Empiricism.

The second lesson concerns recent contacts between the Forman thesis and quantum philosophy. The picture drawn here differs enough from both the thesis' weak or strong version of the Forman thesis to prevent Cushing's and Beller's argument to obtain. Of course, theory is underdetermined by the empirical facts – Logical Empiricists would have been the last to deny this. Of course, the Bohm interpretation is worth pursuing as a viable alternative – the Copenhagen finality thesis had no adherence among Logical Empiricists in those days. But the pragmatic criteria which decide between empirically equivalent alternative formulations – and this is a lesson transcending the narrower context of the debate followed here – cannot be shifted at will back and forth in history; fertility and simplicity cannot be held responsible ex post, they can only be applied anew perhaps leading to a different result. I am afraid that there are no equal rights cases in the history of science. Although pragmatic criteria of theory choice are typically rational, not historically contingent, their application is fundamentally embedded into a concrete historical environment.

The third lesson is that the physicists of Weimar Germany are, to my mind, a bad object of study for the historiographical methods of Beller and Forman. More than in other countries, German theoretical physicists of the first half of the 20th century had philosophical aspirations. This not only brought the scientist-philosophers in contact with the emerging scientific philosophy, but it also determined their view of foundational matters. Even more, nearly all protagonists of the story to tell involved themselves into general cultural or political questions. The problem of causality even motivated a parallel exchange on questions of culture, morality and society. That I decided to cover this aspect is not merely to oppose the picture drawn by Forman, but it is also motivated by Logical Empiricists firm belief that the logic of science and the general scientific world conception were inseparable.

Although these lessons are dealt with in different parts of the book, they require mutual support to be fully convincing. There is no point in refuting the Forman thesis; one can only try to draw a more accurate picture which contained the debate investigated here as its core. Since the final demise of the received view of Logical Empiricism, historiography of this movement occasionally unearths ideas promising for today.. What the present book might offer in this respect is to demonstrate how a fertile interaction between scientist-philosophers took place around one of the key notions of physical science.

1. The Forman Theses: A Critical Assessment

The aim of the following two chapters is to critically discuss three prominent accounts of the early causality debates among the Weimar physicists that deny any significant influence of philosophical convictions on the turn of the events in physics proper and on the interpretation of the new quantum mechanics. Although the books of Forman (1971), Cushing (1994), and Beller (1999) amply discuss the philosophical convictions of key physicists, they are treated as part of ideologems rather than within the context of a genuine philosophical debate among scientist-philosophers. Although all three books have attracted considerable criticism, their shared neglect of philosophy as a determinative element of the early days of quantum mechanics still represents a view shared by many people working in the foundations of quantum mechanics.

The present book will outline a philosophical debate on the role of causality in modern physics whose roots go back to the heyday of statistical mechanics and whose branches extend to the days when the major concepts of quantum philosophy, among them complementarity, began to stabilize. The existence and development of this debate, its historical and systematic embedding, are what I have to offer. I shall not claim – thus inverting the Forman thesis – a causal influence of philosophy on the development of quantum physics; there remains a genuine element of historical contingency or theory underdetermination. Nor will my story involve all protagonists of quantum mechanics and its interpretation; there are other figures and factors beyond those treated in the present book that influenced the adoption of a statistical understanding of the theory. But what I do claim is that for a series of major players, and within the context of a single scientific journal that harbored the philosophical debates among scientist-philosophers during the Weimar days, these philosophical debates acted as a factor stabilizing their views, and not as a justification *ex post* of some astounding features of a physical theory that miraculously emerged. It is true, the outcome of the debate outlined in the present book had a stronger impact on the development of modern philosophy of science than on the interpretations of quantum mechanics, at least when interest into the foundations of this theory returned in the early 1950s.

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The aim of Chapter 2 is to provide an overview of the Forman thesis and the debates it aroused. Section 1.1 reconstructs the different steps in Forman's line of arguments. In documenting the evidence put forward, particular emphasis is given to such personalities who figured in the causality debate between Vienna and Berlin. In Section 1.3 I shall also cover the extension of Forman's original thesis to the concepts of "Anschaulichkeit" and "Individualität". My synopsis will already mention some historical details where, to my mind, Forman's account is inaccurate. To stress it again, the main objective of this book is not a refutation of Forman.

Section 1.2 highlights some of the criticism and assent the Forman thesis has hitherto received. Surprisingly, it took almost a decade until – after reviews and occasional citations – Forman's paper became both a model for case studies and the target of basic methodological disapproval. My coverage of these reactions is somewhat selective insofar as it is oriented at the discussions to come in subsequent chapters. In particular, I refrain from general discussions about externalism and internalism in the history of science. The tradition of Vienna Indeterminism and the debates with its Berlin counterpart have both a philosophical and an institutional embedding, and thus the externalist and internalist analysis will be, I believe, mutually supportive.

1.1. Forman's Thesis and Its Extension

Forman's paper "Weimar Culture, Causality, and Quantum Theory, 1918-1927: Adaptation by German Physicists and Mathematicians to a Hostile Intellectual Environment" proceeds in three steps: (a) a description of the hostile intellectual milieu; (b) a general adaptation of German physicists and mathematicians to the ideological requirements of this milieu; (c) the thesis that this adaptation also modified the content of scientific work.

At the beginning, the author provides some insight into his methodology. Forman intends "a causal analysis, showing the circumstances under which, and the interactions through which, scientific men are swept up by intellectual currents." (Forman, 1971, p. 3) These strong pretensions, problematic as they are (see below), are at odds with the feeble ending of the book: "it seems difficult to deny that the shifts in scientific doctrine exposed in this paper were *in effect* adaptations to the Weimar intellectual environment." (Ibid., p. 115) Forman's textual evidence for diagnosing an adaptive response is primarily found "in addresses by exact scientists to academically educated general audiences, and especially in their addresses to their assembled universities." (Ibid., p. 6) Although Forman calls it a fortunate circumstance that these addresses were published afterwards, he does not undertake an analysis of the respective media. This will be done in Chapter 5 below.

Again in the Conclusion, one finds another methodological bias of Forman's investigation that presumably influenced the sometimes rather pathetic rhetoric. The "most characteristic difference between those physicists who hastened to renounce causality and those who clung to it even after the discovery of quantum mechanics" was to diagnose "a failure of the human intellect" or to express "an existentialist revulsion against intellectuality", on the part of the "progressives", and the "faith in the capacity of the human intellect to comprehend the natural world", on the part of the conservatives. "And for this reason, also I have not been able to, nor indeed wished to, maintain a perfectly neutral stance in my exposé. … My sympathies have consequently been with the conservatives in the defense of reason, rather than with the "progressives" in their denigration of it." (All ibid., p. 112f.) As the "progressives" stamped "irrational" include personalities of an avowedly rational orientation, such as many Logical Empiricists, one wonders what Forman's notion of rationality is like. The Introduction echoes this bias by a reference to

> the valuations of physical science in contemporary American society, on the one hand, and the present ideological tendencies in these sciences, on the other hand … [A]s sentiments of resentment and antagonism toward the scientific enterprise – coupled with a revival of existentialist *Lebensphilosophie* – have become prominent in the last few years, so have the concessions of and concessions to the same sentiments within the sciences themselves. We are indeed witnessing in

America today a widespread and far-reaching accommodation of scientific ideology to a hostile intellectual milieu. (Ibid., p. 5)

But is it really the only option to fortify rationality by conservative standards of causal explanation, as Forman suggests? In discussing the anti-science phenomenon of the 1990s, Gerald Holton proposes a more pragmatic strategy. Spotting, as Forman, Spengler as "the ancestor of the end-of-science movements" (Holton, 1993, p. 134), he rather relies upon a certain robustness of the scientific method and the internal incoherence of the anti-science ideologems: "*No world picture is truly anti-scientific*, insofar as it always has a core component containing a functional proto-theory of the physical and biological universe" (Ibid., p. 159) which consists of "the observation, identification, description, experimental investigation, and theoretical explanation of natural phenomena" (Ibid., p. 152). Needless to say, "the use of probability and of quantum causality is not an abandonment of all causality as such." (Ibid., p. 134)

1.1.1. Milieu's Traits

Forman's sociological analysis commences from another, to his mind, characteristic trait of contemporary science. The socio-cultural milieu becomes the more influential on the scientists the more their overall prestige is questioned. During epochs in which science and technology enjoy a high social status, scientists can by and large follow their disciplines' internal logic and ignore antipathy from outside. Thus was, so Forman holds, the situation of German physics in the Wilhelmine Empire. Still in 1913 the Emperor gave a large donation to the newly founded Kaiser-Wilhelm-Gesellschaft which would quickly become Germany's most important scientific organization.

All that radically changed after the defeat of the German army and the democratic revolution in November 1918. Occultism and spiritism prospered, mysticism and irrationalism spread like kudzu throughout the constantly endangered early Weimar republic; *Lebensphilosophie, existentialism,* the philosophy of Henri Bergson dominated the intellectual scene. A science which paradigmatically conformed to the requirements of this milieu was Hans Driesch's vitalism because it rejected any reduction of the individuality of life to physico-chemical laws.[9] The "freedom from positivist causalism and determinism, the overcoming of neo-Kantian formalism" represented core elements of the "*wissenschaftlichen* revolution." (Ibid., p. 17, quoting Troeltsch, 1921, p. 1005) Among the educated Germans a far-reaching sentiment of crises spread, which had its roots in the devaluation of the intellectual capital. Although Forman, at this point, cites Ringer's seminal study *The Decline of the German Mandarins* (1969), he does not discuss how the Mandarin concept could be applied to natural scientists who were beyond Ringer's scope. Yet, there are some cases where scientists were quite explicit about culture, such that the concept can be used to partition the extremely heterogeneous group of 'converts to acausality'. (Sections 4.6.5 & 5.3.)

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[9] Nonetheless, Driesch – a renowned experimental biologist turned scientist-philosopher – himself felt the anti-scientific pressure of the milieu (Cf. Forman, 1971, p. 19 fn. 38). This classification is, of course, too simple. But accepting it here with a grain of salt gives me the opportunity to argue below in Section 5.4. that in *Die Naturwissenschaften* the causality debate in biology substantially differed from the one in physics. Also Frank ([1932] 1988) took on Driesch at considerable length; see Section 8.5.

In 1918, the first volume of Oswald Spengler's *Decline of the West* appeared and it quickly spellbound an intellectual milieu that was just longing for it. "The crisis of culture, the revolution in *Wissenschaft,* radical *Lebensphilosophie,* all proclaimed and epitomized by a sweeping theory of world history in which … physics and mathematics are treated alongside art, music, and religion as wholly culturally conditioned." (Forman, 1971, p. 30) Science, to Spengler's lights, was part of the all-embracing decline of occidental Faustian rationality leading eventually back to an epoch of belief. In this way modern science repeated the course of the Apollonian science of antiquity that "faded out between the battle of Cannae [216 BC] and that of Actium [32 BC]. And from this it is possible to predict the end of Western natural science." (Spengler, 1918, p. 555)[10] There exists no bridge of common empirical facts between both cultural cycles that would permit us to understand Greek mathematics or mechanics. They are simply part of two different organisms. Nevertheless, those organisms live similar lives. "The final terminus to which Faustian wisdom tends, even if only in its highest moments, is the dissolution of all knowledge in a colossal system of morphological-historical relationships." (Ibid., p. 639) These homologies which were displayed in long tables, were to justify Spengler's predictions about the future course of Western civilization.

Modern physics, in particular, exhibits clear symptoms of the exhaustion and self-destruction of Faustian science: excessive emphasis on theory and abstract symbolism, the abandonment of absolute space and time, and above all the crisis of the principle of strict causality which, to Spengler's lights, "is identical to the concept of law. There *only* exist causal laws." (Ibid., p. 168) Through probabilistic methods science turns back to the individual. "Statistics belongs to the sphere of the organic, to fluctuating life, to destiny and chance, and not to the world of exact laws and timeless mechanics." (Ibid., p. 627) "Within a number of radioactive atoms, only single ones meet their *destiny* while the neighboring ones are entirely unaffected." (Ibid., p. 630) More generally, Spengler's history of the world is based on the "opposition between *the idea of destiny* and the *principle of causality [Schicksalsidee und Kausalitätsprinzip*] which has not been recognized hitherto as such and in its deep world-forming necessity."(Ibid., p. 167) Mechanical causality – so he continues – reveals the "fear of the world [*Weltangst*]," while – as statistical physics ultimately has acknowledged – destiny strikes individual atoms like single human beings.

Within a milieu dominated by or even committed to crisis and decline, Forman just spots two islands of modernism: the Vienna Circle and the Bauhaus.[11] "Far from dominating German philosophy in the 1920s, the Vienna Circle and the corresponding group in Berlin – the Gesellschaft für empirische Philosophie around Hans Reichenbach and Richard von Mises [12][sic!] – with their high positive valuation of mathematical natural science represented a rather late and distinctly marginal group."

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[10] There is a certain ambiguity with the first editions of Spengler's opus. The page numbers of the version published by Braumüller, Vienna-Leipzig, which I have used, do not coincide with the Munich edition used by Forman. For the 33rd edition, the author revised the first volume and deleted some of the passages quoted here. This edition became the basis of the English translation. I have checked all quotations myself and added some not used by Forman. Focusing on this first edition is crucial because it was the one read by most of the scientists to appear in this study.

[11] There have been many links between both groups; see (Galison, 1993) and with many historical details (Dahms, 2002).

[12] Although von Mises delivered a lecture to the society and had various contacts with its members, he did not get along with Reichenbach.

(Forman, 1971, p. 21) “In *Wissenschaftliche Weltauffassung: Der Wiener Kreis*, the brochure with which the circle first came before the public, ‘their tone,’ as Ringer [1969, p. 308] rightly points out, ‘was that of exasperated outsiders’” (Forman, 1971, p. 20) Forman here neglects that Ringer so classified also the various schools of neo-Kantianism. Is thus the causality debate to be scrutinized in the present book almost exclusively led among outsiders?

Yet Forman’s account is flawed. He was unaware of the various links that had existed between Philipp Frank, Moritz Schlick, Richard von Mises, and Hans Reichenbach already in the early 1920s and before. While this might be excused by the poor state of the historiography of Logical Empiricism in 1971, another point should not have escaped a sociologist’s attention. All four Logical Empiricists figuring so prominently in Forman’s study were either examples of the classical type of scientist-philosopher or of the emerging type of scientific philosopher to be branded by the Vienna Circle. While Frank and von Mises remained well-respected scientists who extensively published on philosophical subjects, the trained physicists Schlick and Reichenbach owed their professorships in philosophy to the intervention of Einstein and Planck (and calling Schlick to Vienna was largely arranged by the mathematician-philosopher Hans Hahn). This demonstrates that within the narrower circle of German physicists, the Vienna Circle and the Berlin Group were not marginal. Nor were they for the readers of *Die Naturwissenschaften* (See Section 5.3.).

1.1.2. Ideological Adaptations

1.1.2.1. Ideology and Rationality

After the description of the milieu, Forman turns to the first type of adaptation to the intellectual environment. It concerns the effects upon scientist’s ideology, that is, “upon the professed justifications of scientific activity, upon the epistemological stance of the exact scientists, and upon their elan, their esprit, their confidence in the future of their discipline.” (Forman, 1971, p. 38) Relegating some declarations against the anti-scientific milieu, among them David Hilbert’s criticism of the du Bois-Reymond’s *Ignorabimus* (publicly reiterated on almost any occasion from 1900 until 1930), and against Spengler, among them Exner (1922) and Riebesell (1920), into a footnote, Forman moves on to the first leg of his argument. After 1918 the positive references to Mach’s positivism significantly decreased. Forman compares two public lectures of Wilhelm Wien in 1918 and in 1919, the first of which largely approved Mach while the second contained “not the faintest whiff of positivism; no mention of Mach at all.” (Ibid., p. 41) Forman concludes that the renowned experimentalist “implicitly concedes the series of equations made repeatedly by the antagonists of modern science – empiricism = positivism = narrow specialization = utilitarianism= materialism” (Ibid., p. 41).

I know of only one instance during the entire Weimar period of a German physicist venturing, in a general academic address, to mention Mach’s name with clear approbation and to associate himself with Mach’s epistemological doctrines. Nor was it a coincidence that in taking the courageous stand at the *end* of the Weimar period Richard von Mises refused to associate himself with the demand for synthesis, "counting it" – as did Mach – "the highest philosophy to tolerate an incomplete world view." (Ibid., p. 42)

Let me give the complete passage from the *Mechanics* and its context. According to Mach, the evolution of knowledge culminates in "the ideal of a *unified* world conception, which is alone compatible with the economy of a healthy mind." (Mach, 1988, p. 480/560) But this ideal is still to be reached. At present "the highest philosophy of the scientist [Naturforscher] is to *tolerate* an incomplete world view and to prefer it to an ostensibly complete, but insufficient one."(Ibid., p. 479/559) Rather than the influence of the milieu, von Mises's stand thus faithfully rehearsed Mach's radical empiricism and epistemological holism. (See Section 3.1.)

There are various reasons why he is an unsuitable indicator for the changing attitude towards causality. First of all, the polemics with Planck in 1908-1910 (Cf. Section 3.7.) estranged many German physicists from Mach's epistemology albeit manifold appreciations on Einstein's part. Moreover, most physicists followed Planck's lead in taking Mach's qualms with Boltzmann's atomism as a case in point against positivism and neutral monism *tout court*. Mach's posthumously published *Principles of Physical Optics* the Preface of which contained the infamous criticism of "the more and more dogmatic theory of relativity theory," (1921, p. viiif.) led to a further loss of credibility among those defending modern physics, because 1921 was one of the most critical years for the public acceptance of general relativity.[13] In the end, Forman himself wavers whether the name Mach is a reliable litmus paper for the attitude towards the milieu and observes "that the positivist tradition itself contained a substantial element of *Lebensphilosophie* and that, moreover, there was a solid Machian precedent for regarding natural science as the outgrowth of a basic human drive." (Forman, 1971, p. 46) To my mind however, such a position amounts to the low-brow empiricism which Holton counts on rather than to existentialist enchantment.

The most important point is, to be sure, Mach's theory of causality itself (See Section 3.1). When outlining the concepts of causality in use between 1918 and 1927, Forman apparently realizes that on the basis of Mach's causality as functional dependences one could contemplate "the denial of *any* exact laws for atomic processes" (Forman, 1971, p. 66), a view which he attributes to Charles S. Peirce and, in a footnote, to Franz Exner and Marian von Smoluchowski who were part of an persistent "subterranean anticausality current" (Ibid., p. 67).[14] In this way one could renounce the "essentially Kantian notion of causality as conformity to law" (Ibid., p. 65) which Forman rightly locates in Moritz Schlick (1920) and Hans Reichenbach (1920b). But Forman wrongly identifies Schlick's position with Spengler's above-mentioned claim that there exist *only* causal laws because Spengler's, at bottom,

[13] The Preface is dated 1913, however parts of the book go back to lectures of the 1870s. Due to the war and Mach's death, the book appeared only in 1921. Wolters (1987) doubts the authenticity of the Preface and provides circumstantial evidence for a forgery by Mach's son Ludwig. Max von Laue's review in *Die Naturwissenschaften,* interestingly, did not use the "sensational" Preface as an argument to finally dismiss Mach's philosophy. Planck's closest collaborator judged the book as a late work that lacked the great originality of the *Principles of Mechanics.* "Does it not characterize the scepticist at all costs that he repudiates his ownest thoughts in that very moment when a greater mind has developed them into something positive?" (Laue, 1921, p. 966) To the realist Planck, Mach was indeed a scepticist. (See Sect. 3.7.)

[14] In passing, Forman even suggests that Planck's (1914) criticism of this attitude is directed against Exner's 1908 inaugural address. This is indeed the case (see Section 4.5.).

mechanistic concept of law does not coincide with a simple, abstract mathematical function that uniquely determines the dynamics. Schlick's concept was modeled after relativity theory which, to Spengler's lights, was as much a sign of decline as statistical physics because both departed from the Laplacian world view. (Cf. Section 7.1.)

> It must, of course, be acknowledged that in precisely this period Mach himself, the positivist movement in general, and even neo-Kantians like Cassirer were waging a campaign against quite a different concept of causality, … the "metaphysical", "animistic", "fetishistic" doctrine of cause and effect (*Ursache und Wirkung*) as an ontological assumption, which Mach and his allies wished to replace by the mathematical conception of function. … And by 1918 this point of view had become almost a matter of course among physicists and the philosophers closely associated with them … so that "causality" stripped of all ontological overtones, was taken as equivalent to functional determination. (Forman, 1971, p. 68)

But in the end all differences are brushed away in favor of a rather superficial criterion.

> The possibility of satisfying a (weaker) postulate of lawfulness without demanding that every detail of every natural process be unambiguously determined did not entirely escape physicists in the years before the discovery of a quantum mechanics having this general character. Nonetheless, the essential point is that in the period treated in this paper every such suggestion of a relaxation of complete determinism was advanced as, and regarded as, a failure or abandonment of *causality.* In fact, as we proceed we will occasionally find the word "causality" being used in several senses narrower than, not wider than, "determinism" … And again, in many instances these special definitions of causality were advanced in conjunction with, and as a justification for, an assertion of the invalidity of the law of causality. In every instance, however, such special definitions of causality, and a fortiori the general requirement of unambiguous determination, were held to be equivalent to the assumption of the comprehensibility of nature, and repudiated or defended as such. (Ibid., p. 69-70)

All just a matter of words, so one might ask in the end, words which either met or opposed the demands of the milieu? But Forman is not after rhetoric in first place. Rather does the adaptation thesis concern a causal influence of the ideology required by the milieu on scientific methodology.

In this perspective, it is rather surprising that Forman plays down the role of philosophy at best. The reason is that, to his mind, philosophy equals classical school philosophy; accordingly there remain only few candidates such as Hermann Weyl who was deeply impressed by Fichte and phenomenology at various instances of his intellectual career.[15]

> And here, saving perhaps the case of Hermann Weyl, it was not a question of "philosophical" influences in any serious intellectual sense. By far the single most influential "thinker" was Spengler, and that only because the *Untergang des Abendlandes*, the concentrated expression of the existentialist *Lebensphilosophie* that was diffused through the intellectual atmosphere … Thus, excepting the role of Franz Exner, the philosophical theses of the latter nineteenth century to which Jammer has drawn attention, while they may perfectly well have some responsibility for the ideational content of the *Lebensphilosophie* of the Weimar period, played, *per se, an sich,* a negligible role in the sudden rise of anticausal sentiment among German physicists after the First World War. (Forman, 1971, p. 110)

[15] See the papers of Sigurdsson and Scholz in (Scholz, 2001).

1.1.2.2. Spenglerism

Spengler is, expectedly, Forman's second catchword of ideological adaptation. The typical reaction of the Weimar scientists was as such: "Of my discipline Spengler understands, of course, not the first thing, but aside from that the book is brilliant." (Forman, 1971, p. 30) Scientists typically defended the "most basic tenet of the scientific ideology, the autonomy, objectivity, and universality of scientific knowledge." (Forman, 1971, p. 56f.) This was done, to Forman's mind, by Exner (1922) and Riebesell (1920); see Sections 4.6. and 5.3. "Yet for every opponent of Spengler's thesis" to have exploded this ideology by demonstrating that there are no immanent, invariant criteria of knowledge, that science depends upon the *Lebensgefühl* of an epoch, "one can cite another exact scientist who … identified himself with this doctrinal touchstone of Spenglerism." (Ibid., p. 57) Among those cited by Forman at this point is Schrödinger's 1932 speech "Ist die Naturwissenschaft milieubedingt?" In Section 6.3.7. we will, in effect, see to what extent Schrödinger took Exner's stand.

But the first witness for a "Capitulation to Spenglerism" is Richard von Mises. In his 1920 Dresden inaugural address, von Mises argued that since the turn of the century mankind has been standing at the end of the 'age of technology' that had begun in the 1840s or 1850s; "and we have gradually entered into a period which is, similarly as the times of Copernicus, Galilei, and Kepler, characterized by a particular intellectual trend [*geistige Bewegung*], a period in which speculative natural science blooms [*Blüte spekulativer Naturwissenschaft*]." (Mises, 1922b, p. 2) Von Mises also takes up the poetic title of a paper of Arnold Sommerfeld (1920) which had appeared in *Die Naturwissenschaften* shortly before. Where Sommerfeld titled "A Number Mystery in the Theory of the Zeeman Effect", von Mises added a dash of Spenglerian cyclism. "It is a remarkable cycle of affairs [*Kreislauf der Dinge*] when one considers that in present-day atomistic numerical harmonies, even number mysteries play a role reminding one no less of the ideas of the Pythagoreans than of some of the cabbalists." (Mises, 1922b, p. 16) But Forman neglects that despite terminological coincidence both authors have rather opposite views about atomic physics. For the staunch Machian von Mises atoms are still hypothetical, while Sommerfeld constantly rehearsed Planck's (1908a) oft-repeated allegation that positivism leads to infertility.[16] Hence, causality was not at issue in von Mises' capitulation to Spengler; just the old concept of causality had to go in the case of mass phenomena (Ibid., p. 30), but the Machian functional dependences could still be maintained. (See Section 8.2.)

Forman diagnoses a substantial shift in von Mises' attitude in late 1921 when he republished his speech with an appendix. "What is perhaps most striking and appalling about the von Mises of September 1921 is the failure of nerve, the complete loss – just as Spengler predicted – of the esprit, the self-confidence which we expect from the mathematical physicist." (Forman, 1971, p. 51) Did von Mises, who had just founded the *Zeitschrift für Angewandte Mathematik und Mechanik* in order to support "theoretical foundations of technology" (Mises, 1921b, p. 269) really go "so far in assimilating the values and mood of [the] … intellectual milieu as to effectively repudiate [his] … own discipline …?" (Forman, 1971, p. 55) It suffices to read the

[16] In a letter to Schlick of 17 October 1932, Sommerfeld applied directly this to Philipp Frank, "who despite all acumen never tackled a physical problem." In the same letter, Sommerfeld also rejected Mach's empiricist notion of causality which he termed "principle of sloppy laws of nature." (See Sect. 7.4.)

appendix which mainly explained new findings in relativity theory and atomic physics. Von Mises took back part of the skepticism of the original address. He felt misunderstood to have rejected the possibility of any further development or elaboration of present-day technology. The results of the present speculative science, he continued, might well "become the basis of new hitherto undreamed-of technical achievements, in the same vein as one considers the foundation of natural science in the 17th century as the 'preliminary stage' of 19th century technology." (Mises, 1922b, p. 31) Still, this observation promised no final cure; and in the end the author poured out his troubled heart.

> History on the large scale is no unidirectional progress superposed, perhaps, by small oscillations; it exhibits birth, growth, and decay as the life of the individual, the family, and the peoples. Gigantic intellectual worlds have emerged, risen to great heights, and went to ruins: Where are the gods of Greece today and what persists from the spirit of the old Romans among the inhabitants of today's Rome? …
> There is at least a high probability that the edifice of an occidental culture based upon knowledge and achievement, which has towered up since five centuries, comes to demolition within the next centuries. From this point of view one would have to count relativity theory and atomistic among the last building stones which are destined to crown the edifice. This does also not contradict the fact that we look at a widely open and fertile field of work, in particular with respect to the second area of research; even though evening is gradually drawing near, there remains room for the work of generations.
> Strangely enough, Spengler's expositions were conceived as an expression of pessimism. As if man who ages and acts in the conscience that death will occur some day would be a pessimist! …Are then today the works of Greek art and worldly wisdom [*Lebensweisheit*] completely lost? Thus, to my mind, we can content ourselves with the prospect that posterity, once a further development of our present occidental culture and a continuation of the exact sciences in our sense have become impossible, will think of our times with similar feelings of gratitude and reverence as we show today for classical antiquity. Nothing is lost what is done pure in heart and for the sake of the cause! (Ibid., p. 32)

Surprisingly, von Mises read Spengler not as a pessimist; the last sentence rather took a Nietzschean tack to courageously face the inevitable.[17] This conclusion was supported by the anti-Spenglerian insight that the achievements of earlier cultures were not lost. Thus there are clear signs of a retrenchment – as Forman had initially assumed on the whole (Cf. Forman, 1971, p. 5) – of the scientist von Mises rather than adaptation. But clearly he does not pay mere lip service to the milieu. Rather does it seem that the scientist-philosopher von Mises and the Bildungsbürger von Mises followed diverging orientations. This is still discernible in his textbook *Positivism* where one can find a rather cautious footnote on Spengler. Cf. Mises, 1939, p. 325/223)

1.1.2.3. Craving for Crisis

The third ideological adaptation to the milieu concerned the 'craving for crisis'. "By applying the word 'crisis' to his own discipline the scientist has not only made contact with his audience, but has *ipso facto* shown that his field – and he himself – is 'with it', sharing the spirit of the time." (Forman, 1971, p. 58f.) Once again, we encounter Hermann Weyl in first line. The crisis in the foundations of mathematics proclaimed by him "was precipitated virtually out of thin air in the two or three years following

[17] Recall the end of the *Zarathustra!* Schiemann (1996, p. 358) reads von Mises's creative impulse at dusk as somewhat ironical.

Germany's defeat. With extraordinary suddenness the German mathematical community began to feel how insecure the foundations upon which the entire structure of mathematical analysis rested." (Ibid., p. 60) Unfortunately, the crisis was stirred up by the Dutchman Luitzen E.J. Brouwer. The Vienna Circle, on the other hand, took the crisis more seriously than most working mathematicians. Thus, in the case of the foundational crisis in mathematics Forman faces the paradox to classify the modernist fringe movement Vienna Circle as converts to the milieu. Let me turn to three other crises.

Once again, Forman calls von Mises to the witness stand; this time he testifies "On the Present Crisis of Mechanics" (Mises, 1922a). As we shall see in Section 8.2., Mises advocated the application of statistical methods because, e.g., in fluid dynamics the reduction to an allegedly deterministic micro-level could not produce any applicable result at all. However, the crisis of mechanics as a reductionist program already dates back to the failures to incorporate electrodynamics that had led to the special theory of relativity in 1905. Finally, the "*possibility* of the crisis of the old quantum theory was, I think, dependent upon the physicists' own craving for crises, arising from participation in, and adaptation to, the Weimar intellectual milieu." (Forman, 1971, p. 62) But, to my mind, there were clear physical indications of a failure of the Bohr-Sommerfeld theory.[18] There were internal inconsistencies, problems with the interaction between electrons, and in 1922 and 1923 the theory produced empirically wrong results about the Helium atom. When in July 1923 *Die Naturwissenschaften* published a special issue dedicated to "The first ten years of Niels Bohr's theory of the constitution of atoms", Planck's keynote paper mentioned the helium atom and many electron problems in general as a prominent example for the need of a "deep intervention into the system of ideas of the classical theory." (Planck, 1923b, p. 536) Moreover, "[a]t present an, at least to some extent, satisfactory solution of the problems raised by the introduction of quantum mechanics into atomistic is out of the question by far. Not even the question about the domain of validity of the classical theory can today be finally decided." (Ibid., p. 536) Nevertheless, Planck warned against a global indeterminism.

> There are eminent physicists who at bottom want to allow the principles of the classical theory only a statistical significance … Such a conception seems to me, however, to overshoot the mark by far, if only because with the abandonment of classical dynamics they simultaneously pull out the foundations of every rational statistics. (Ibid., p. 536)

According to Forman, those meant by Planck's criticism were "Exner, Nernst, Schrödinger, and, yes, Bohr, himself." (Forman, 1971, p. 93) But Exner had to stand the same criticism already a decade before. The fact that Planck basically repeated his earlier criticism (Cf. Planck, 1914, p. 63-64; see Section 4.5.) makes this passage practically worthless for Forman's adaptation thesis. Towards the end of the paper, Planck, interestingly, was less rigid about the validity of the principle of energy conservation, which would be indeed abandoned in the following year by the Bohr-Kramers-Slater (BKS) theory. Thus we see that, in Planck's case, the diagnosis of crisis did not entail abrogating deterministic causality.

[18] An interesting reading in this respect is (Hund, 1984, pp. 127-129) because the author of this history of quantum mechanics had himself been an active participant of these developments.

More generally, I doubt that more than a few of the protagonists had taken the old theory that was based on the correspondence principle and a calculatory machinery, as anything close to the final word. Of course, one can grant Forman that *on the rhetorical level* physicists adapted to the catchwords of the milieu. And they had not yet the slightest idea about the way out of the crisis. Sommerfeld's much-debated "Zahlenmysterium" is a case in point. The table of spectral series for the anomalous Zeeman effect was so mysteriously regular that for every physicist it was clear that it indicated an important fundamental law still to be discovered and not just an unexplainable fact. Thus was the thrust of the two final paragraphs of Sommerfeld's paper.

The musical beauty of our table of numbers is not impaired by the fact that for the time being it represents a number mystery. … In the case of the spectral series it has turned out that the arithmetic relations prevailing here have their reason in quantum theory. It is beyond doubt that our still mysterious table of numbers indicates the workings of hidden quantum numbers and quantum relations. (Sommerfeld, 1920, p. 64)

Sommerfeld was fully right: the theories of Schrödinger and Heisenberg introduced the new magnetic quantum number *m* and modified the algebraic relations for the orbital momentum *l* in such a way that the anomalous Zeeman effect became the standard case while the normal Zeeman effect represented a degeneracy of the quantum numbers. Thus John Hendry is fully right to consider the heading of the article as a "linguistic accommodation to the milieu" (1980, p. 159) – perhaps for the sake of popularization. To the German translation of Hendry's paper, Karl von Meyenn (1994, p. 209) adds that the term "Zahlenmystik" had been used in the same context as early as in 1917 by the Polish physicist A. Rubinowicz who was working with Sommerfeld in Munich. At bottom, Sommerfeld was quite convinced that one had already isolated the empirical evidence to be condensed in a law. To be sure, in later years he would at first skeptical about the radical epistemological changes required by his student Heisenberg.

The pronouncements of "crisis" by scientists investigated by the present study, and within the community represented by *Die Naturwisssenschaften*, typically combined the diagnosis of a foundational crisis with the conviction, or already the first indications, that it would be overcome by developing new foundations. This rhetorical figure can be found in at least two places appertaining to both sides of the aisle in the causality debate.

The first location is even mentioned in a footnote of Forman (1971, p. 59, fn. 135). In 1933, Karl Menger organized a much-frequented lecture series *Krise und Neuaufbau in den exakten Wissenschaften* (Crisis and Reconstruction in the Exact Sciences) the Preface of which declared: "The growing interest of ever wider circles for the exact sciences is surely above all a seeking after one of the regions which are far removed from the world of crisis. … In truth the exact sciences are by no means secure from crises and precisely in recent decades, from theoretical physics on out into logic, they have been shaken by severe crises." (From Forman, 1971, p. 59, fn.)[19] But this lecture series is no convincing evidence for Forman's case. If there was a feeling of crisis in 1933 and among these lecturers invited by Menger, it was Hitler's seizure of power. And taking Hahn's contribution "The Crisis of Intuition" (1933) as a

[19] See (Stadler, 2001, p. 420) for the further context of these lecture series.

paradigmatic example, the span of the crisis extended back to the rejection of intuition as a criterion of justifying mathematical axioms that had become the basis of modern mathematics as brought about by Weierstraß and Hilbert. Hahn rejected Kant's pure intuition and its contemporary heirs because they hampered mathematical progress. *Die Naturwissenschaften* published a review of these lectures by von Mises who concluded that they fortunately disappointed the skepticism aroused by the oft-misused word 'crisis' in the most pleasant way. "One would wish that each one of the assiduous poets of world views [Weltanschauungsdichter] take notice of the fact that also physics does not do *anything else* than what it did at each instance of its development, to wit, to adapt the intellectual images of theory to new observational results." (Mises, 1933c, p. 867)

The same rhetorical figure is constantly present in the writings of Planck who is Forman's upright fighter under the banner of causality. In an address to the 1910 *Naturforscherversammlung* entitled "The Position of Modern Physics With Respect to the Mechanical World View", Planck initially surveyed the impressive achievements in experimental techniques. "Also the theoreticians have been imparted a large part of the boldness that has emerged among the practitioners, … no physical theorem at present is secure against being called into question, every physical truth is open to debate. It appears as if in theoretical physics chaos is drawing near." (Planck, 1910, p. 25) After a sketch of the long-lasting dispute about the ideal of mechanical reductionism, Planck suggested a resolution through relativity theory. "What has led to this revolution and how the crisis [*Krisis*] caused by it will perhaps be overcome shall be outlined in the following." (Ibid., p. 26) Twenty years later, his famous article "Positivism and the Real World" also commenced with the motive of crisis.

> We are living in a very singular moment of history. Wherever we turn our attention, in every branch of our spiritual and material culture, we have got into a moment of severe crises. … Some people view this as the beginning of a great improvement, others interpret it as the herald of the inevitable decline. As has long been common in religion and in the arts, nowadays in science there is scarcely any axiom [*Grundsatz*] that is not denied by somebody, no nonsense that does not find believers and disciples somewhere or other. In the midst of this confusion it is natural to ask whether there is any truth left which is unassailable. … The moment this question is asked the mind turns, no doubt, to the most exact of our natural sciences, namely, physics. But even physical science has not been spared this universal crisis. Even on this field a certain insecurity has emerged and at places the opinions in epistemological matters differ considerably. Physics' hitherto generally accepted axioms, in some places even causality itself, are thrown overboard. After all, that such could happen just in physics is sometimes counted as a symptom for the unreliability of all human knowledge. (Planck, 1930, p. 201)[20]

Has Planck here belatedly adapted to the milieu? The body of the paper which reveals a Planck who still – though less certain than before – is breaking a lance for causality[21]. As ever he criticized the infertility of positivism and held with Kant that deterministic causality was fully reconcilable with the freedom of the will. While in 1910 relativity theory had ended the crisis, this time there was no physical remedy in

[20] Unfortunately, the differences between the German original and the English translation (Planck, 1981, pp. 65-106) are substantial and entire passages are missing. As moreover the translator Murphy had his own views about the subject (Cf. Forman, 1971, p. 108f.), I shall not use this text and all translations from Planck are mine.
[21] Cf. his dialogue with Schrödinger in the year before which will be the content of Section 6.3.5.

sight. Planck retained his optimism and called upon the united forces of science and ethics.

I hope that even those of you who have little connection with physics have gained the impression that even a single science, if performed thoroughly and conscientiously, is able to unearth valuable treasures of aesthetical and ethical nature, and moreover, that precisely the deep crises in spiritual culture which we have initially mentioned as our starting point, at the end of the day only serve to prepare the federation into an new and higher union. (Ibid., p. 218)

Also Johannes Stark, who together with Philipp Lenard would become a leader of the *Deutsche Physik* under the nationalsocialist regime, contributed a booklet on *The Present Crisis in German Physics* (1922). To him, the crisis consisted in the 'dogmatic nature' of all modern physics, be it relativity or quantum theory. Stark found a resolute response in *Die Naturwissenschaften* penned by von Laue (1923). After rejecting Stark's dislike of the bulk of modern physics and insisting that there exists no difference in value between pure and applied physics, von Laue concluded.

There is not doubt about the existence of a crisis in physics, and there is no doubt as well that it must be above all accredited to quantum theory. But the crisis is not limited to *German* science. It manifests itself in the same way in all countries participating in physical research, and it can be overcome only once science succeeds in solving the quantum riddle [Quantenrätsel]. There exists no other remedy. … All in all we wished that the book had remained unwritten, namely, in the interest of science in general, German science in particular, and not the least in the interest of the author. (Laue, 1923, p. 29f.)

As in Planck's writings, Laue clearly posed the task to end the crisis by finding an appropriate quantum theory. It was Stark who felt an all-embracing sense of crisis, not the quantum physicists. For the quantum generation, Stark and Lenard were hardly a part of the milieu to seek rhetorical and ideological company with.

1.1.3. Dispensing with Causality

Let me now turn to the third and pivotal step of Forman's thesis: "Dispensing with causality". In the years 1920/1 suddenly a series of quasi-religious confessions to acausality began. While Wien held the fortress against Spengler, the first converts were Exner and Weyl. It is true, in those days Weyl was strongly influenced by a phenomenology "of quite a different sort from his Machian ex-brothers" (Forman, 1971, p. 76). In Exner's case, the "conversion" in 1919 was a walkaway because the bulk of his ideas about acausality had already appeared in print a decade ago (1909). From a rather superficial summary of the philosophical arguments in the fourth chapter of Exner's *Lectures* (1919, 1922) Forman concludes: "What is novel is the leap from that supposition [that all natural laws are statistical] to the conclusion that causality fails. For this leap no justification is offered, and the problem of how perfectly acausal microscopic motions result in statistical regularities is not even raised by Exner." (Forman, 1971, p. 75) In Section 4.4., I shall argue that the frequency interpretation of probability, which is discussed at length in the *Lectures,* does the job together with the radical empiricist stance. To Forman's lights, this does not make up a coherent world view.

> [Exner] therefore does the best to convince his (lay) readers of the implausibility of the existence of such a causal substratum, switching back and forth between, and largely confounding, the question of the validity of the laws of classical mechanics in the atomic domain and the validity of the principle of causality in the same domain. (Ibid., p. 75)

Yet, on pain of tautology the empiricist has to be specific about the character of causal laws, and entirely unknown causal micro-laws would be just empty gibberish. Historically, Exner's *Lectures* emerged to a substantial part from the courses he had taught at the Philosophical Faculty.[22] Forman's (lay) man on the street, at bottom, seems to be a rigid determinist who rejects to fill out questionnaires from the bureau of statistics.

> Influential as Exner's lectures indeed were, they have in many respects an archaic air. Exner is a curious mixture of the philosophical currents of the two preceding generations, a self-confessed mechanist-materialist yet clearly also a positivist in his view of scientific constructs. (Ibid., p. 75)

I wonder where Exner ever confessed to materialism. To be sure, he was a physicalist of Boltzmann's breed. Forman's talk of an 'archaic air' might be justified with respect to the poor coverage of Bohr's theory of the atom despite a lot of spectroscopy; but on the other hand, the *Lectures* contain an ample discussion of nuclear physics, and in the second edition (1922) there are several pages about relativity theory, a topic not quite popular among German experimentalists.

> The quasi-religious conversions to acausality … became a common phenomenon in the German physical community during the summer and fall of 1921. As it swept up in a great awakening, one physicist after the other strode before a general academic audience to renounce the satanic doctrine of causality. (Forman, 1971, p. 80)

There fell Richard von Mises (1922a and 1922b), "the loyal scion of Austrian positivism" (Forman, 1971, p. 80), Walter Schottky (1921), and Walter Nernst (1922). Forman laments about von Mises' "'me too' tone" (Forman, 1971, p. 82) and Nernst's "resolve to sink the law of causality by hook or by crook." (Ibid., p. 84) I shall analyze the relevant evidence in Section 5.5. Suffice it to note that, strangely enough, Forman hears in all of them the "common theme of *ignorabimus*." (Ibid., p. 86) But all three alleged renunciations of causality were motivated by the desire to get around DuBois-Reymond's *ignorabimus* and to reject the idea of principal borders of human knowledge which had been a consequence of an untenable mechanist notion of causality, be in a Kantian or in a materialist setting.

The next prey of the milieu was Schrödinger whose 1922 Zurich inaugural address explicitly rehearsed the Exnerian viewpoint, but "by the fall of 1925 Schrödinger had converted back to causality for what were most probably personal-political reasons. He now conceived and developed the wave mechanics as a causal space-time description of atomic processes in opposition to the Copenhagen-Göttingen matrix mechanics." (Forman, 1971, p. 104) But at least in 1929, Schrödinger, in an exchange with Planck on the occasion of his election as a member of the Prussian Academy of Sciences (Schrödinger, 1929), entertained the same views as in the Zurich speech, which he now sent to *Die Naturwissenschaften* without modifications (1922a).

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[22] (Benndorf, 1927, p. 407) relates that Exner had to give the regular lecture course for students of pharmacy.

Reichenbach fell only in 1925. Once again, the empiricist criterion of meaning allegedly leads straight to the *Decline of the West*. While in a earlier paper Reichenbach had left the decision about causality to physics, now he held that "even without the hypothesis of rigorous causality it is possible to give a quantitative description of the course of nature which does everything that physics can possibly do." (Reichenbach, 1925, quoted according to Forman, 1971, p. 89) Thus probability became the most fundamental concept because in this way we need the minimal number of presuppositions. "Which is to say that existentialist philosophy, disguised as logical empiricism, has preempted the decision." (Ibid., p. 90) Reichenbach was quite active in the *Jugendbewegung*;[23] maybe he even wore a beard to disguise his existentialism? But suddenly in 1925 he decided to have it shaved off in Occam's barber shop for a better adaptation to the milieu. More interesting is Forman's conclusion of all such "quasi-religious experience" because it already opens up the possibility of alternative histories discussed in Section 2.1.

> When our converts attempted to demonstrate the necessity for this renunciation of causality, their arguments, as often as not, ought logically to have led to the opposite conclusion. From this I think one must infer that they fully anticipated that *any* argument advanced by a physicist as a demonstration of the failure of causality would be received by their audience with uncritical approval. (Forman, 1971, p. 90f.)

While Forman does not offer a glimpse of how these logical conclusions run, he narrative now calls upon the "Unrenegates against the Tide" who stood up to protect the banner of causality. "Among the first … was Mach's old bulldog, Joseph Petzoldt." (Ibid., p. 91) Closer scrutiny (Section 5.5.3.) will teach that Petzoldt defended precisely the Machian concept of causality which he, rightly, considered to be reconcilable with quantum mechanics.

Planck himself repeatedly raised his voice in favor of the transcendental character of the law of causality (Planck, 1923, 1929 & 1930) and hoped that quantum mechanics would ultimately return to a causal description. While Forman's description of Planck's stand is fairly accurate, I have serious doubts whether "Planck and Einstein were in complete agreement." (Forman, 1971, p. 94) Einstein's position in causal matters was rather sophisticated. Independently of Smoluchowski, he had obtained a solution of the problem of Brownian motion, and even before, he openly availed himself of statistical methods for black-body radiation while Planck did do so only stepwise (See Kuhn, 1987, Ch. VII). Moreover, Einstein was seeking, without success though, a unified field theory rather than hoping for a simple return of causality. Similar motivations might also have guided Weyl[24]; after all, the issue whether the basic description of nature be continuous (as in field theory) or discrete (as in quantum mechanics) was not just an internal physical question but a philosophical classic of the same rank as causality.

Forman now turns his attention to the situation circa 1924. Without resorting to the milieu as the reason how such a heresy could have been possible, he states that now "the atomic physicists were becoming convinced of the fundamental inadequacy of the extant theory of the atom." (Ibid., p. 97) It seems that Forman now detects a second adaptation to the milieu, the failure of mechanics, such that "the

[23] See the paper of Hans-Ulrich Wipf in (Danneberg/Kamlah/Schäfer, 1994).

[24] In private communication, Erhard Scholz has expressed serious doubts about Forman's account of Weyl.

antimechanical and anticausal movements coalesced, reinforcing one another." (Ibid., p. 98) Whatever "mechanics" is to mean here – perhaps the strict validity of energy conservation –, the most important result in 1924 was the Bohr-Kramers-Slater (BKS) theory in which energy conservation held only statistically. Of course, it was once again the milieu which guaranteed the theory a "widespread assent" until it was experimentally refuted by the experiments of Geiger and Bothe who already in 1925 showed that energy was conserved in each individual process. And again, Schrödinger's (1924a) "moral feelings" made him "clutching at it with both hands." (Ibid. p. 100) Two years later, he allegedly changed his mind when writing to Wien on 25 August, 1926: "Today I no longer like to assume with Born that an individual event of this kind is "absolutely random", i.e., completely undetermined. I no longer believe today that there is much to be gained from this conception (which I championed so enthusiastically four years ago)."[25] Yet this passage needs more profound interpretation; it will be analyzed in Section 6.3.4.

"Causality's last stand" was tenaciously defended by Wien who maintained "that, even when the laws are statistical, causality must reign at the level of the elementary process" and that "physicists will not rest until they have subjected atomic processes to the law of causality." (All ibid., p. 102f.) "The confidence and corresponding aggressiveness which Wien manifested on the issue of causality in the spring of 1926 derived chiefly from Erwin Schrödinger's papers on wave mechanics which Wien was then publishing in his journal, the *Annalen der Physik.*" (Ibid., p 103f.) Unfortunately, the next issue of the *Annalen* would contain Schrödinger's (1926b) proof that wave mechanics was equivalent to matrix mechanics. After Born's statistical interpretation and with the equivalence of wave mechanics and matrix mechanics established, Heisenberg in the spring of 1927 could proclaim victory: "quantum mechanics definitively establishes the fact that the law of causality is not valid." (according to Forman, 1971, p. 105)

Here Forman's story ends. Now there were rational reasons for the dismissal of causality on the basis "of a fundamentally acausal quantum mechanics" (Ibid., p. 110). Many philosophers subsequently would jump on the band wagon of the new theory, however, they based their "nonsense announced with great fanfare … wholly and solely upon the manifestos against causality issued by physicists before that date." (Ibid., p. 111) But whatever stand scientist-philosophers had taken in the causality debate, all of them vigorously criticized these misinterpretations. (See Sections 8.5. & 8.7.).

In the end, Forman devotes himself to the really big picture.

> [P]aralleling Ringer's observation that early in the Weimar period the 'modernist academics' tended to be 'methodologically adventurous', one finds that, by and large, those physicists who were readiest to repudiate causality had either distinctly 'progressive' views by the standards of their social class and the German academic world, and/or had an unusually close interest in, or contact with, modern literature. [The second condition is probably to save the case von Mises.]… On the other hand, with the notable exception of Einstein, those who defended causality tended to be highly principle political conservatives and/or interested in classical literature. … And finally to the causalist camp one may add the outright reactionaries: Ernst Gehrcke, Erwin Lohr, Philipp Lenard, and Johannes Stark. (Ibid., p. 113)

[25] This passage is quoted in (Forman, 1971, p. 104, fn. 235).

The book's rather feeble ending, which I have already cited at the beginning of this section, might have been conditioned by the insight that the last series of names poses a substantial danger for the stability of Forman's whole edifice because they evidence a continuity with the struggles about relativity theory which Forman has to avoid at best to convincingly make his case. Gehrcke, Lenard, and Stark were the main opponents of Einstein's relativity theory, and of modern 'abstract' physics altogether. Although the clashes around Einstein had already begun before the war (e.g., Gehrcke vs. Born, 1913), they reached the peak right in the years 1920-1922, and were thus in perfect synchrony with the alleged adaptations to acausality. If it were true that the German physicists were more prone to sacrifice the concept of causality familiar from classical physics after they had realized that the familiar concepts of Euclidean space and absolute time were untenable, the adaptation diagnosed by Forman was by far less scandalous. This intimate connection between relativity and atomic theory becomes even closer if one takes into account Forman's later extension of the adaptation thesis to the notion of *Anschaulichkeit* because the later stood in the center of the battles between Einstein and Lenard.

Before turning to *Anschaulichkeit* in Section 1.3 I shall provide some highlights of the debates ensuing from Forman's original paper. My reading will be somewhat selective in order not to lose sight of this book's main thread.

1.2. Reactions on the Forman Thesis

The most recent opinion about the Forman thesis which I have found is as decided as most comments in the three decades before. It teaches that the matter is still controversial. Let me quote a passage which ends the relevant chapter in Helge Kragh's *Quantum Generation,* a book dedicated to 20th century physics.

However, there are good reasons to reject the suggestion of a strong connection between the socio-ideological circumstances of the young Weimar republic and the introduction of an acausal quantum mechanics. Suffice to mention a few of these reasons:

1. Whereas the physicists often discussed the (a)causality question and other Zeitgeist-related problems in talks and articles addressed to general audiences, these topics were almost never mentioned in either scientific papers or addresses before scientific audiences.
2. To the extent that physicists adapted to the Zeitgeist, the adaptation was concerned with the values of science, not with its content.
3. Many of the physicists had good scientific reasons to reject detailed causality and did not need to be "converted". At any rate, only a very small proportion of the German physicists seem to have rejected causality before 1925-26.
4. Sommerfeld, Einstein, Born, Planck, and other leading physicists did not bow to the Zeitgeist, but criticized it explicitly.
5. The recognition of some kind of crisis was widespread around 1924, primarily because of anomalies that the existing atomic theory could not explain. Bohr and a few other physicists suggested vaguely that energy conservation and space-time description might have to be abandoned …
6. The first acausal theory in atomic physics, the 1924 Bohr-Kramers-Slater radiation theory, was not received uniformly positively among German physicists, contrary to what one would expect according to the Zeitgeist thesis. And those who did accept the theory were more impressed by its scientific promises than by its ideological correctness. The theory's element of acausality was not seen as its most important feature. Moreover, the theory had its origin in Copenhagen, with a

cultural climate very different from the Weimar Germany, and was proposed by a Dane, a Dutchman, and an American.

7. Among the pioneers of acausal quantum mechanics were Bohr, Pauli, and Dirac, none of whom was influenced by the Weimar Zeitgeist. The young German physicists who created quantum mechanics were more interested in their scientific careers than in cultural trends and sought deliberately to isolate themselves from what went on in society.

In conclusion, there were good reasons – internal as well as external – for why quantum mechanics originated in Germany. As far as I can judge, adaptation to the Weimar Zeitgeist was of no particular importance. (Kragh, 1999, p. 153f.)

Counts 3., 5., and 6. concern the obviously weak evidence of Forman's case seen from an internalist standpoint. Count 6. specifically illustrates the crucial role of the BKS theory for the question of causality before 1926. Here I largely agree. Counts 2. and 7. suggest a far-reaching separation between Forman's protagonists in their roles as physicists and as *Bildungsbürger* as was the case with von Mises. (Cf. Sect. 1.1.2.2.) For one group (Einstein, Planck, Bohr, Exner, Schrödinger) this tension led to active participation in the cultural discourse, while others (Dirac, von Neumann) reacted by retrenchment, as Forman had initially contemplated. Count 1 is the externalist complement to count 2. Analyzing the philosophical debate about causality I have to reject them in the strong form as they appear here, because philosophical convictions mediated between the values of science and the theory's content. Moreover, the philosophical debate took place in *Die Naturwissenschaften* that were a medium having both a scientific and a general audience. Count 4. is absolutely right, but I have some doubts whether Bohr and the later Pauli are correctly classified under Count 7. which rather seems to suit the present-day professional theoretical physicist.

Apart from an unpublished address by Jon Dorling, the first comprehensive reaction to Forman's thesis was equally outspoken. In "Weimar Culture and Quantum Causality", John Hendry concludes:

Forman's work has clearly demonstrated the poverty of a wholly internal treatment of issues such as that of causality. Physicists *were* influenced by the crisis-consciousness of post-war Europe and by the attitudes characteristic of the Weimar milieu. On the other hand, Forman has also demonstrated the dangers of a purely external treatment and the poverty of any naive social reductionism. (1980, p. 171)

In the bulk of his paper, Hendry provides sketches of an internal or rational counterpart for most conversion documents. Among them is an interesting observation concerning the declaration of victory, to wit, Born's statistical interpretation (1926 & 1927).

Born's rejection of causality, equivalent as it was to the rejection of any relevant microscopic coordinates, was also closely tied to his insistence that his theory was final and complete, whether microscopic coordinates existed and were measurable or not, and this was the most remarkable feature of his presentation. Heisenberg had built his theory upon quantities that were in principle observable, but Born restricted himself to those that were in practice, at the time of writing, and asserted that no future experiments could change the theory that he had evolved. This somewhat dangerous attitude appears to have directly stemmed from his assertion that the physical interpretation must follow uniquely from the mathematical formulation of the theory and the assertion itself from a battle waged in Göttingen the previous winter between the advocates of a mathematical approach to the new quantum mechanics (Born, Hilbert, Weyl) and the advocates of a physical approach (Heisenberg, Fran[c]k). (Hendry, 1980, p. 168)[26]

In the historical context of 1927, this difference was perhaps of minor importance. Seen in retrospect, however, with many hidden-variable theories on the market and this present situation being projected back into 1927 by philosophers, such as Cushing (1994), the finality claim becomes crucial. More generally, Born's point concerns the fact how physical ontology and empiricist meaning criteria are mutually related, and what we expect from the axiomatic approach to quantum mechanics. (Neumann, 1932) These problems will come to the fore in the differences between Schlick and Frank in 1936 (Section 8.7). Here are Hendry's conclusions.

We may, then, dismiss Forman's thesis in his own terms, but only if we are prepared to accept the naive level of his own arguments; and this, it seems to me, we cannot do. For overwhelming the detailed evidence given above and by Forman are many serious problems relating to the causality issue in general, to the overall structure of Forman's demonstration, and to the whole question as to what is meant by an "influence of the milieu", how this relates to the wider concept of sociological causation, and how this concept may be meaningfully applied to the history of science.

Hendry rightly criticizes Forman's "consequent handling of the causality debate in simple black-and-white terms. To important contributors such as Weyl and Reichenbach, both of whom were well trained in the subtleties of philosophy of science, the issue was complex and the classical position naive." (Ibid., p. 169) To Heisenberg and Bohr, the issue of causality was closely related to the applicability of the traditional space-time concepts. Moreover, one also has to take into account that regarding causality "we are dealing with a contentious and, in part at least, emotive issue." (Ibid., p. 169)

With the information available, Forman has succeeded in demonstrating the influence of the milieu upon physicists' attitudes to causality, and were he to adopt a suitable concept of historical causation he could even assert quite reasonably that the attitudes were in some (weak) sense "caused" by the milieu. But it is clear from the importance he attaches to the absence of internal motivations and from his insistence on the milieu as "the primary" cause that his concept of historical causation is in fact a very strong one, and as such it must be supported by much more than the emotive, value-laden, discussion of examples that he offers. (Ibid., p. 170)

In Hendry's final conclusion we already implicitly find the distinction between a weak and a strong version of Forman's thesis. The first merely claims a discernible influence of the milieu on some enunciations – be it on their rational content or rhetorical outfit – of major quantum physicists. The latter involves a problematic notion of strict causation by the socio-cultural milieu; or as the highly critical study of Kraft and Kroes puts it, a 'quasi-behavioristic' stimulus-response scheme of social interactions.

According to Forman, the Weimar scientists focused exclusively on these academically educated general audiences, and their relationship with these audiences bears a lot of resemblance to the marketplace, where prices are set by such audiences-without-any-profile. It is hard to avoid the conclusion that Forman's physicists and mathematicians, craving for recognition and prestige, reacted like Pavlovian dogs to the academic milieu. (Kraft and Kroes, 1984, p. 94)

[26] See also Ch. 8 of (Hendry, 1984).

Moreover, a reasonable concept of causation should support a counterfactual: "without the influence of these [external] factors, physics in Germany would have taken a different course." (Ibid., p. 96) As no such counterfactual obtains, Kraft and Kroes conclude "that Forman has created his own problem and sought for a 'chameleonlike' mechanism of adaptation as a solution of his problem." (Ibid., p. 96) As we shall see in Section 2.1. Cushing (1994) indeed supplies such a counterfactual on the basis of – or rather in support of – a weak version of Forman's thesis.

Kraft and Kroes's paper also mentions some important characteristics of German physics. First, "German physicists had become acquainted with problems of an epistemological nature: the controversy between Mach and Boltzmann concerning the existence of the atom was part of their scientific heritage." (Ibid., p. 97) A comparable tradition did not exist in other countries. To my mind, this argument can be made even stronger: the type of scientist-philosopher was nowhere so widespread as in Germany. Second, the authors propose to 'turn Forman against Forman' by citing later studies of the same author (Forman, 1974; Forman, Heilbron and Weart, 1975) according to which Germany had a disproportionately large and well-funded number of physicists in comparison to other countries.

Taking a closer look at the power structure in the *Notgemeinschaft der deutschen Wissenschaft,* we find the former Prussian minister of education Schmidt-Ott as its president; Planck and Fritz Haber were in control of the *Elektrophysikausschuß* the great importance of which for the early 1920s resulted from the fact that the endowments coming from the General Electric company and the Japanese industrial Hajime Hoshi were awarded in hard currency. Although the money was advertised for "experimental investigations in the entire field of chemistry and atomic physics,"[27] the ground-breaking studies of Heisenberg and Born received generous support from this source (Cf. Hermann, 1973, p. 90). And thus Planck, the arch-defender of causal physics, gave money to overthrow it – too perverse an adaptation, it seems. And so did Einstein in his capacity as a director of the *Kaiser-Wilhelm-Institut für Physik* (Cf. Meyenn, 1994, p. 45-49).

Kraft and Kroes also investigate the academically educated general audiences which mediated between the Weimar milieu and the German physicists. Forman "nowhere specifies the nature and the composition of these audiences." (Kraft and Kroes, 1983, p. 86) There is indeed a dilemma here between these audiences' incompetence in physical matters – which is necessary for any influence to qualify as external, –and their competence – which seems to be necessary to be able to influence physicists' theorizing. Kraft and Kroes doubt whether the lack of, or the desire for, recognition and prestige produces enough leverage to escape this dilemma. To my mind, this dilemma still smells too much of the external-internal distinction. The more promising strategy seems to be to be more specific about the connecting links, at least after a certain restriction of the focus of investigation. This is the reason why Chapter 5 attempts to provide such a connecting link for the causality debate between Vienna and Berlin.

Forman's paper also led to several case studies that took it as a sort of measuring rod. Quite interesting in the present context is Hans Radder's investigation about the Copenhagen-based Dutchman Hendrik A. Kramers who was one of the driving forces of the BKS-theory. Radder rightly argues that almost all the material

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[27] See the guidelines "Die Richtlinien des Japanausschusses", *Die Naturwissenschaften* **11**, 31-33.

piled up by Forman in favor of his third claim, which asserted an influence of the milieu on the content of physical theory, only supports, if it does at all, his second claim and witnesses an ideological adaptation of the single physicists. The only example which could win the case for the third claim is the BKS-theory, because it represents the only well-developed theory that explicitly breaks away from the principle of causality. Interestingly, "Kramers strongly contrasts the possible claims of science with a *lebensphilosophische,* anti-rationalistic and, in his personal case, religious viewpoint." (Radder, 1983, p. 170) However, "in the case of Kramers we cannot speak of a 'conversion'…: his 'romantic attitude is already apparent … from 1916 onwards." (Ibid., p. 171) Yet in a paper of 1925 written in Dutch this romanticism combines with a clear-cut positivism.

To be sure, we have to reckon with the possibility that principles like that of the conservation of energy or that of the causal course of phenomena in nature will get their feathers singed. However, in judging issues like this, one should always bear in mind that such principles are inferred, by the route of experience, from phenomena in which a great number of atoms play a part, and that, consequently, one can never with certainty conclude from such experiences anything about the elementary processes which lie at the root of such phenomena, namely, the processes that take place in individual atoms. (quoted according to Radder, 1983, p. 171f.)

We will encounter a similar stand in Exner's writings (See Sect. 4.4.). On Kramers' account, even foundational matters admit considerable leeway: "For the moment it seems to me rather a matter of taste, and perhaps remain so forever, which alternative one prefers, unique causality or probability laws." (from ibid. p. 172) Radder characterizes Kramers' positivism as epistemological, which amounts to a combination of empiricist and instrumentalist ideas. Epistemological positivism starkly differs from its metaphysical counterpart, Auguste Comte's claim that science will replace all metaphysics and obscure thinking. While the Weimar milieu is antipositivistic in the latter sense, Kramer's epistemological positivism is reconcilable with the demands of the milieu because it leaves a free space which is not covered by scientific claims, or in Kramers' words: "We can never get to the bottom of things. … We cannot claim more than a mere *description* of the relative positions and motions of the fundamental particles and of the laws governing their mutual action and their interplay with the ether." (Kramers and Holst, 1923, p. 133) As epistemological positivism thus professes du Bois-Reymond's *ignorabimus*, it is, to my mind, well reconcilable with the milieu but fundamentally at odds with Mach's positivism and Logical Empiricism both of which are decidedly anti-metaphysical. On their account, epistemological positivism would be still a rather metaphysical doctrine because it reserves a special realm for religious truths unassailable by science. Unfortunately, some confusions about positivism will be with us throughout this book.

Forman's thesis motivated also case studies in other disciplines. Mitchell G. Ash investigates the birth of Gestalt psychology in the Weimar context.

Both Forman's [1971] and Hendry's [1980] account rest on two interlocking dualisms – an assumed opposition between modern and anti-modern, or rational and irrational; and the related notion that culture and society are external to science. This article queries both dualisms by considering a prominent case from psychology, a discipline generally thought to be more prone to ideological influences than physics. … [However,] Gestalt psychology does not conform easily to such conceptions. (Ash, 1991, p. 395)

Although I do not fully agree with Ash's assessment of physics, the comparison is interesting, in particular because psychology went through a deep methodological crisis of its own.

> The difficulties for relativity and quantum mechanics were at least partly due to the fact that their procedures for theory-generation were not universally accepted even among physicists. This left them fair game for scapegoating in the popular and intellectual press as part of the general discourse of 'crisis'. Gestalt theory, too, was only partly accepted in its own community – not, however, because its methods were thought too unorthodox … but because its holism did not go far enough. … [Moreover,] the implications of that research did not, and could not, satisfy the ideological demands of the public from which the researchers came. … Yet in spite of all their holistic talk and their reaffirmation of *Bildungsbürger* values, the Gestalt theorists' competitors realized full well that combining such discourse in some way with that of instrumental reason was the key to psychology's applicability, and hence to official standing and state support. (Ibid., p. 409)

This comparison teaches, I think, four points about the particular situation of physics. First, in the Weimar period there already existed a generally visible sense of the direction of modern physics. The needle had been adjusted by the experts' general acceptance of special relativity and the Bohr-Sommerfeld quantum theory before the war. Second, *Die Naturwissenschaften* became the public forum for this modern physics in the post-war polemics, while the journal did not take a comparatively clear stand with respect to foundational debates in other disciplines. Thus, as I shall argue in Chapter 5, the causality debate took place within a well-entrenched submilieu represented by this journal. It was, accordingly, not as directly connected to the general public as relativity theory and thus less in danger of scapegoating. Third, such submilieu tampers or mediates the conflict between science and culture. Also the Gestalt concept was embedded into a context emphasizing holism and empirical advancement. Forth, in both cases the opponents' behavior enabled mutual profit. Planck, Wien, and Einstein did not at all question the scientific merits of the wild ones around Heisenberg and supported their research.[28]

The introduction to a useful German collection of the Forman debate by Karl von Meyenn (1994) and a later study of Gregor Schiemann (1996) recognize the significance of Exner and the Boltzmann school for Forman's case. Meyenn cites Boltzmann's idea of an atomic constitution of time (See Section 3.5) and his late conviction that determinism could well fail on the atomic scale as long as the average of all random atomic events reproduces the well-known macroscopic laws. He rightly considers already Exner's 1908 inaugural address as a generalization of these thoughts. "Thus already from a physical point of view there had been created a range to postulate acausal phenomena." (Meyenn, 1994, p. 57) The discovery of radioactive decay further widened this range, such that Meyenn proposes to relativize Forman's third claim to the extent that "only the various positions admitted from an internal scientific point of view could subsequently be elaborated in the direction of one or another account under the influence of personal or milieu arguments." (Ibid., p. 58)

[28] Interestingly, Gestalt psychologist discussed the motive of crisis in *Die Naturwissenschaften.* Koffka (1926) published a critical review of Hans Driesch (1926) who attempted to integrate a psychology allegedly in crisis into his vitalist outlook. When Kurt Riezler (1929) diagnosed a crisis of the traditional concepts of reality triggered by the newest developments in physical science, this provoked a criticism from Ludwik (Ludwig) Fleck (1929). And also Wolfgang Köhler (1929) had his turn.

Schiemann fully approves of Exner's criticism of a dogmatic preference of the principle of causality "which only approximately does justice to the fallible character of natural laws." (Ibid., p. 361) On Exner's empiricist account, the causal and the statistical view of nature enjoy equal rights. It is very much in the spirit of the present book when Schiemann concludes that Exner thus "names the decisive point in the physical criticism of causality of the early 1920s which was also the most important one from the perspective of cultural history." (Ibid., p. 361) Interestingly, Schiemann identifies Forman's reluctance to admit that there were sufficient physical reasons to criticize the principle of causality before 1926 with the posture of some quantum physicists after 1926, such as Heisenberg, that only the statistical character of atomic physics necessarily entails the failure of causality. How strong this link becomes in present debates will be seen in Chapter 2. Suffice it to say at this point that Exner and the Vienna Indeterminists constantly maintained a more flexible empiricist position.

1.3. Further Forman Theses: **Anschaulichkeit** *and* **Individualität**

In 1984, Forman extended his study on the concept of causality in time beyond 1927 and thematically to the concepts *Anschaulichkeit* (intuitive evidentiality or visualizability) and *Individualität* (individuality). Given the milieu as described in (Forman, 1971), it is of course no surprise "that once a nondeterministic theory of atomic processes was at hand, German physicists were disposed to view it and represent it in public as providing that liberation from causality so generally desired." (1984, p. 338) But the milieu was also longing for *Anschaulichkeit* and *Indivdualität* both of which quantum mechanics in point of principle was unable to deliver.

With quantum mechanics already established after 1927 and in view of the fact that except for acausality the theory contradicted the key demands of the milieu, Forman had to use another method than claiming causal influence.

> Each [of the three probes] opens with a statement of the *true* bearing of the theory relative to one of the three concepts or characteristics. … This is followed by a brief exposition of the *alleged* character of the theory regarding that concept – alleged first of all by the creators of the theory. These allegations are found to diverge markedly from what, a moment before, had been posited as warranted regarding these concepts. (Ibid., p. 333f)

In recent years the discussions about the interpretation of quantum mechanics have increased rather than arrived at a consensus regarding the "true bearing" of the theory. This will prove to be a substantial impediment for Forman's argument to obtain without a strong implicit assumption. Fortunately, this time Forman's analytic bias is not some foggy conservative concept of causality as in (1971), but he takes a stand clear enough to count him among the proponents of an alternative interpretation of quantum mechanics. At least, such papers typically open with the following words. (Cf. Bohm and Hiley, 1993)

> Quantum mechanics is merely a statistical theory. As Einstein repeatedly but vainly recognized, it cannot be regarded as a complete description of an independently subsisting microscopic world. Nor can it be regarded as an appropriate conceptual basis for describing our macroscopic world, where, unquestionably, we deal with individual objects and events, not statistical ensembles. Thus even categoric statements about the invalidity of the law of causality in the *physical* world go much too far,

not least because they slur over the fact that quantum mechanics is a deterministic theory of probabilities. As for the still farther-reaching world-view implications ascribed to quantum mechanics – that it ensure free will, or the impossibility of a physicochemical explanation of life – one must say that these are completely unwarranted. (Forman, 1984, p. 336f.)

There are many philosophical problems on this list. Some of them, however, were altogether not controversial among the protagonists on both sides of the aisle in the causality debate. Practically all of them rejected Pascual Jordan's (1934, 1935) views alluded to in the above passage (Cf. Frank, 1935, or Planck, 1936). But once again we read

that there was little connection between quantum mechanics and the philosophic constructions placed on it, or the world-view implications drawn from it. The physicists allowed themselves, and they were allowed by others, to make the theory out to be whatever they wanted it to be – better, whatever their cultural milieu obliged them to want it to be. (Forman, 1984, p. 344)

In Forman's new drama, no scientist-philosophers appear and defend their views but reckless academics perpetrate repeated "misuse of quantum mechanics for sweeping epistemic renunciations." (Ibid., p. 337)

A core shibboleth of Weimar *Lebensphilosophie* was individuality. Heisenberg's uncertainty principle blocks the possibility to follow the trajectory of a single particle to arbitrary precision, and the statistics for atomic particles (bosons and fermions) lead to an indistinguishability of the single particles. Forman complains that after 1926 Heisenberg never again mentioned this simple truth. Bohr, on the other hand, advanced "the perverse thesis that the main bearing of quantum theory was to demonstrate not only the individuality of atomic processes, but indeed the 'indestructible individuality' of material particles. Then and later Bohr went farther to draw explicit analogies with the individuality of living organisms and human personalities." (Ibid., p. 342) I cannot enter into the depths of Bohr's philosophy[29] here, but let me just make two remarks which are important for later considerations.

First, if individuality was indeed such a pressing demand of the milieu and if there are causal influences exerted by the milieu onto physical theory of the type Forman claims to have established for the concept of causality, one wonders why Louis de Broglie's pilot wave theory, which restored the individual particle trajectories in 1927, did not at all profit from this circumstance. To be sure, de Broglie was a Frenchman, but those dismissing his views were Germans and many of them, among them Pauli, Heisenberg, and Reichenbach, had bowed their head and converted to acausality years ago. Interestingly, Bohm (1952) would face the same opponents when he presented de Broglie's pilot-wave idea within a new setting and 25 years later to the U.S. of the McCarthy period.

Second, what Bohr conceived as a new individuality analogous to organisms has today become quite popular among proponents of alternative interpretations. Many advocates of the Bohm program, for instance, credit him for the insights that measurement apparatus and object system represent an indissoluble unity, and that every particular experimental set-up represents a single entity. This was also the

[29] There is an enormous literature about Bohr. A recent collection of essays on his philosophical views is (Faye and Folse, 1994).

background for Bell's (1986, p. 1f.) early criticism of von Neumann's No-hidden-variable theorem. One wonders why de Broglie did not win Bohr's assent in 1927.

Turning to the second milieu shibboleth, Heisenberg's matrix mechanics represented a calculatory scheme far cry off any *Anschaulichkeit*. Although Schrödinger's equivalent description initially appeared closer to physicists' common sense, the ψ waves propagate in configuration space rather than in ordinary space. Thus "Heisenberg sought to remove the stigma of *Unanschaulichkeit* by redefining the 'intuitive' quality so as to make it predictable of his irremediably unpictorial quantum mechanics. This redefinition equated 'intuitive' to 'satisfactory' in a strictly positivist sense." (Forman, 1984, p. 340) And thus Heisenberg once again obtained the applause of the milieu. So did Sommerfeld who was a principal opponent of positivism. Citing Heisenberg's redefinition, Sommerfeld held that "the *Anschaulichkeit* does not consist in theoretical ideas [*Vorstellungen*] which are extrapolated from the rough sensuality [*grobe Sinnlichkeit*] to microscopic processes, but in the narrow and critical comparison with possible or imagined experiments." (Sommerfeld, 1930, p. 165)[30] This position was, however, not far from Planck who already in 1923 wrote that "if it is the most noble task of theory to adapt the intuitions [*Anschauungen*] to the facts and not vice versa, then the physicist cannot have doubts about his position towards Bohr's theory." (Planck, 1923b, p. 535)

Thus there is an important difference here. While philosophically *Anschaulichkeit* is a concept considerably more hazy than causality, in the physics of the 1920s it had a historical connotation much sharper than causality. *Anschaulichkeit* had been the core of the infamous "cockfight" – or so it was later termed in the press – between Einstein and Lenard at the 1920 *Naturforscherversammlung.* The relevant passage from the newspaper record reads as such.

> Einstein: The phenomena in the [slowing down] train are the effects of a gravitational field which is induced by all closer and more distant masses.
> Lenard: But such a gravitational field should also otherwise cause physical processes, if I want to make its existence *anschaulich* to myself.
> Einstein: What man considers as *anschaulich*, is subject to great changes, it is a function of time. A contemporary of Galilei would have declared his mechanics as very *unanschaulich.* The "*anschaulichen*" pictures have their intricacies [*Tücken*], as the often-cited "healthy common sense". (quoted from Hermann, 1995, p. SF 83)

Einstein won the audience's laughter. This marked an important step in winning public recognition for relativity theory among the German scientists. In the general public the struggle was anything but over, but after two difficult years the next *Naturforscherversammlung* in 1922 testified the final victory for the theory within a community which Lenard, Stark, and their followers had already left. If the milieu's demand for *Anschaulichkeit* agreed with Lenard's quest, there was little to gain for the quantum generation had they taken it seriously. Even more so as Lenard already in the 1920s became the pied piper for the Nazi regime among German physicists.

It is important to bear in mind that this bridge between the both allegations of *Unanschaulichkeit* was already built in those days. Thus the "positivist redefinition" criticized by Forman stood on a broader and firmer basis than just to sweeten the transition from Bohr's planetary model, that was intuitive but ad hoc to Heisenberg's

[30] The paper is cited by (Forman, 1984), p. 340.

unituitive matrix formalism. Only by including relativity theory can one safely link the *Anschaulichkeit* around 1920 with the earlier debates between Mach, Boltzmann and Planck about the intuitive character of the atomic hypothesis. A person to walk over this bridge from the positivist side was the Vienna Indeterminist Philipp Frank. (See Section 8.4.)

The rigid end points of Forman's story remain problematic despite his later extension beyond 1927. His contention that suddenly there were internal reasons for an acausal quantum mechanics seems to repeat a widespread tendency of autobiographies written by notable scientists. Once the final version of the theory is found, full insight suddenly appears like the flash of a genius and makes all earlier failed attempts worthless in the split of a second. The same is, of course, true for the final insight that a certain program is unfeasible. However, historical reconstruction has to embrace all steps from the initial proposals, the heuristic debates, the first attempts, the further specification of the problem, the first proto-theory, possible alternatives, the form which became final in retrospect, the application of this theory to concrete models, and the development of a technical machinery and a mathematical framework (this list is, of course, not complete). In the case of quantum mechanics this process probably began with Bohr's seminal papers (1913) which brought first successes, but ran into crisis in 1923/24. After a period of theoretical experimentation, there emerged two equivalent formulations which until 1928/9 led to a flood of concrete calculations. And when in 1932 von Neumann provided a mathematical framework of the theory and its interpretation, theoretical physicists had already moved on to quantum field theory and nuclear physics. Both books discussed in the following chapter take a wider temporal horizon.

2. Quantum Counterfactuals and Quantum Dialogues: On the Cleft between Rational Reconstruction and Historical Contingency

Chapter 2 is dedicated to two recent historico-philosophical analyses of the early history of quantum mechanics both of which are – more overtly than not – committed to a realist or causal interpretation of quantum mechanics. Albeit important differences in their methodology, both James T. Cushing (1994) and Mara Beller (1999) hold some positivist Copenhagen creed, that after 1926 quickly developed into a powerful metaphysical ideology, responsible for the resistance against Louis deBroglie's 1927 pilot wave theory and David Bohm's 1952 causal interpretation which, at bottom, refreshed the former. While Cushing sketches an alternative history, Beller studies how a complex net of personal relationships and thematical dialogues developed into an ideology of the winners.

Cushing develops an alternative causal history that continues the line of those who by 1927 had not bowed to the milieu. His objective is twofold. First (Section 2.1.1.), Cushing argues that quantum mechanics was underdetermined by the factual evidence. This can hardly be doubted, and thus Cushing has provided a convincing case study for Duhem-Quine underdetermination. The only problematic aspect is his way of rigidly distinguishing between the historically contingent Copenhagen interpretation and unchangeable, empirically corroborated quantum mechanical formalism, in particular when one takes into account the philosophical aspirations of many leading quantum physicists. Their philosophical motives, taken as pragmatic criteria of theory choice are driven apart between rational reconstruction in the narrow sense and historical contingency. This misses an important historical peculiarity of the scientific cosmos of Germany before World War II, the scientist-philosopher.

Secondly (Section 2.1.2.), Cushing uses theory underdetermination, or historical contingency, to legitimate a particular class of empirically equivalent alternative theories as respectable partners, or rivals, of the prevailing standard quantum theory. Theory underdetermination is, of course, necessary for this specific claim, but not sufficient. I shall argue that Cushing's book falls short of this second objective on the factual and philosophical level because historical contingency cannot be used to compensate for a deficiency in pragmatic criteria, such as simplicity or fertility. To put my point more bluntly, there is no enforceable claim for historical justice in science. Alternative theories have to stand the pragmatic criteria in vigor at the time when a theory choice is made. Or conversely, pragmatic criteria are not invariant under a shift on the historical axis. This was a lesson that already Logical Empiricists knew well (Cf. Frank, 1954). Cushing instead uses strong regulative principles about realism and causal explanation, or understanding, as a counterweight to deficiencies of the Bohm interpretation with respect to other pragmatic criteria, and to do so he has to shift them back and forth on the time axis. Within the narrower context of Forman's thesis the alternative history once again shows the philosophical dangers of the Forman's strategy.

Mara Beller (1999) rejects both counterfactual reasoning and the Forman thesis. On her account, prior to 1927 indeterminism was not pivotal altogether. Neither were the members of the quantum generation driven by other deep-seated philosophical motives. They were just immersed in local and creative dialogues which in 1927 suddenly turned into the creation of the orthodox Copenhagen narrative. This change of attitude was mainly driven by Bohr and Heisenberg's ambitions to win the clumsy matrix mechanics a superiority over Schrödinger's elegant formalism. To this end they invoked rhetorically-casted philosophical arguments as ex-post justification. Copenhagen positivism thus used the verificationist axe to chop down the prospect of a genuine quantum reality emerging from the theory's formalism – which was shared by both sides. In Section 2.2.1., I argue that Beller has basically written two books corresponding to the two parts of her *Quantum Dialogue*. In these books the absence of philosophical motives emerges simply from the perspective taken, because the dialogist grid either is too fine, in the local dialogues prior to 1927, or too coarse, when the orthodox narrative is directly compared to the Bohm tradition of the 1950s and 1990s.

Cushing and Beller underestimate the depth and specific character of the philosophical convictions of the scientist-philosophers involved in the creation of quantum mechanics and simultaneously overestimate the import of one single trait of these philosophies, to wit, positivism. Moreover, they employ a notion of positivism which in those days was no longer on the market and contradicts core tenets of those thinkers within the German physics community who conceived themselves in positivist footsteps. In particular, the Logical Empiricists Frank and Schlick are entirely neglected in both books although they entertained closer contacts with the German physicists than did any other philosophical school.

Section 2.2.2. discusses Beller's method of dialogism and why it fails to appraise the philosophical discussions about causality and quantum theory conducted in the 1920s. This is a pity because rather than being committed to philosophical schools and master texts, German physicists and physicist-philosophers discussed their positions in close connection to physical results and in a series of dialogues featuring divergent positions. Analysis along dialogues, not dialog*ism*, proves of great value for the present book because it permits one to see how philosophical identities and the adherence to local traditions are established, communicated, and bequeathed, and how scientific results led to a modification of these traditions and new alliances.

Vienna Indeterminism became a coherent tradition chiefly through a series of dialogues with its Berlin counterparts. Apart from the classical Mach-Planck debate, there exists no master text. Vienna Indeterminism was not a movement like the Copenhagen interpretation or Logical Empiricism, but it harbored one of the major strands of the latter. This permits me, finally, to suggest a parallel between these movements as to the relationship between inward differences and outward unity. The demise of the received view of Logical Empiricism has revealed a movement that harbored fundamental internal differences and simultaneously decided to appear in public as a united front against the anti-scientific and anti-rationalistic tendencies of the Weimar and post-Weimar milieu. Might not the Copenhagen interpretation the explicit identification of which dates much later (Howard, 2002) be of the same structure as Logical Empiricism? At least this would make it understandable why the mid 1930s witnessed the formation of a strategic alliance among them at the Copenhagen Congress for the Unity of Science. (See Section 8.7.)

2.1 Cushing's Alternative History and the Issue of Underdetermination

Cushing's *Quantum Mechanics. Historical Contingency and the Copenhagen Hegemony* begins with a philosophical announcement. "The central theme of this book is that historical contingency plays an essential and ineliminable role in the construction and selection of a successful scientific theory from among its observationally equivalent and unrefuted competitors." (1994, p. xi) That scientific theory is in point of principle underdetermined by the empirical facts is a classic in the philosophy of science, typically called the Duhem-Quine thesis. It poses a problem for the realist and attributes an irreducible role to pragmatic criteria of theory selection. As regards the historical *dramatis personae* in the causality debate, this thesis was supported by Mach and all Vienna Indeterminists while convergent realists, such as Planck, emphatically opposed it. On the part of the Austrian wing of the Vienna Circle, Neurath very early took up Duhem's holism in its strong (Quinean) form and later developed it both with respect to language (protocol sentences and verbal clusters) and with respect to the social structure of science, the later encyclopedism which was to replace the scientific 'system'.[31] Duhem (1908) instead had balanced conventionalism both by common sense and by the ontological claim that although there exist no objectively real substances, science approached a natural order of a formal kind. This tradition of structural realism can be prolonged to Poincaré, the early Schlick and Cassirer (Gower 2000), and also Planck defended it (Section 3.8.).

Historical contingency can be understood in many ways ranging from the fact that the acceptance of a theory is strongly influenced by historical circumstances – even Planck (1990) assented to this version – to a sort of strong historicist program which consequently rejects any notion of scientific progress or the rationality of the scientific enterprise, as Paul Feyerabend did at places. The latter is, to be sure, not Cushing's intention, and Beller (1999) presents her dialogist methodology explicitly as a counter-strategy against Feyerabend and social constructivism. Typically, historical contingency of theory is a considerably stronger claim than Duhem-Neurath-Quine underdetermination, and contingency holds strictly only if all pragmatic criteria fail to give rationally justifiable support to one theory over its competitors. More generally, one might say that the distance between both claims is measured by the weight attributed to pragmatic criteria of theory preference and regulative principles of theory formation.

More specifically, Cushing claims that both the real history and his alternative history of quantum mechanics provide equally rational reconstructions. He explicitly sketches an argument for the fertility of Bohm's theory (Cushing, 1994, § 11.2.2). In virtue of Bell's theorem, the "choice between [locality and causality is ours] to make on purely pragmatic grounds." (Ibid., p. 22) On the other hand, the rational import of pragmatic criteria is limited and they tend to be apologetic. "Criteria such as fertility, beauty, and coherence, while often important, can have a Whiggish aspect to them if they are defined in terms of the successful, victorious, of accepted theory and then

[31] More than this brief allusion would lead too far afield. There is today a rich literature on Neurath, see (Uebel, 2000) and (Haller, 1993).

applied to a competing theory." (Ibid., p. 96) In some cases, two alternative theories might have an approximately equal score in their pragmatic virtues and due to limited resources scientists are better off to pursue only one strategy first. Already Neurath (1913) argued that in such cases it is rational to draw lots or vote. The ineliminability of decisions and other contingent elements in the course of science, accordingly, does not indicate an irrationality of the overall process of theory choice. Cushing alludes to this fact in the Preface. "I do not charge that scientists acted irrationally in selecting one theory over another. Nor do I believe that an alternative choice would have left us without foundational problems to resolve." (Cushing, 1994, p. xiii) But now, so Cushing continues, it is high time to devote ourselves to the study of the alternative theory whose elaboration was blocked by the historically contingent decision of quantum physicists.

2.1.1. Copenhagen and Contingency

> [I]f certain equally plausible conditions, rather than the actually occurring and highly contingent historical ones, had prevailed and the interpretation of quantum mechanics had initially taken a very different route from the Copenhagen one around 1925-1927, would our worldview of fundamental microprocesses *necessarily* have been brought back, by the "internal" logic of science, to our currently accepted picture of an inherently and irreducibly indeterministic nature? Could our present understanding of the behavior of the fundamental laws of nature in terms of an inherently indeterministic nature have been replaced by the apparently diametrically opposed view of absolute determinism? This book argues that the answer to the first question is no and to the second question an emphatic yes. This is not to deny that there were already serious conceptual problems for classical physics. (Cushing, 1994, p. xiif.)

Cushing's story presupposes some conceptual distinctions. The first looks rather innocent. A scientific theory is composed of a formalism and an interpretation; "formalism means a set of equations and a set of calculational rules for making predictions that can be compared with experiment. … The physical interpretation refers to what the theory tells us about the underlying structure of these phenomena (i.e., the corresponding story about the furniture of the world – an ontology." (Ibid., p. 9) One might, however, wonder whether this distinction is really adequate because the Bohm theory after all contains a second equation, the pilot-wave equation or the guidance condition, and additional concepts, the quantum potential and the particle positions, at such places where standard quantum theory has just certain intepretatory rules. Admittedly, one does not have to solve the pilot-wave equation and – apart from some more recent proposals (Valentini, 1996, Albert, 2000) – the particle positions are unobservable in principle, such that the second equation and the Bohmian positions might be counted under the interpretation. But this spoils, it seems, the whole philosophical advantage of the Bohm theory to be simply a theory about particles in motion, and one obtains a theory that is interpretable *as if* particles were in motion. Cushing's concept of formalism, to my mind, is so construed that empirically equivalent theories share the same formalism. This does not seem to meet what the underdetermination thesis was all about because it implicitly exempts the formalism from criticism, or at least it automatically directs all doubts and struggles to the interpretation only.

The second distinction already paves the way towards a philosophical predilection for the Bohm interpretation. "*Empirical adequacy* consists essentially in getting the numbers right. … An *explanation* is provided by a successful formalism with a set of equations and rules for its application." (Cushing, 1994, p. 10) While the deductive-nomological or the covering law model yield successful explanations by unification or reduction, they cannot produce *understanding*.

[Understanding] is possible once we have an interpretation of the formalism that allows us to grasp the character of an the relations among the phenomena. This is typically associated with an interpretation that can plausibly be defended as a realistic one. … My argument here really begins from the intuition … that *understanding* of physical processes involves a story that can, in principle, be told on an event-by-event basis. This exercise often makes use of picturable physical mechanisms and processes. (Ibid., p. 11)

To pick two of Cushing's examples, Boyle's law and the EPRB-correlations are empirically adequate descriptions which are explained by the formalisms of statistical mechanics and quantum mechanics respectively; but an explanation is only given by the kinetic theory of gases and the Bohm interpretation respectively. There are also two contingently necessary conditions for understanding.

'Causality' and 'locality' are logically distinct concepts, with causality being the more central in my scheme of understanding. The actual nonlocality demanded by nature turns out to be of a fairly benign variety: we cannot signal with it … We are able to construct a less incomprehensible, more nearly picturable, representation of the physical universe with Bohm than with Copenhagen. We do have the option of giving up locality while maintaining a visualizable causality. The choice is our to make on purely pragmatic grounds. The origins of the uneasiness about nonlocality may be more psychological than logical. … "*If the price of avoiding non-locality is to make an intuitive explanation impossible, one has to ask whether the cost is not too great.*"[32] My evident sympathy with views such as those on contact action expressed by Maxwell earlier clearly put me into the camp of what has been termed the *mechanistic* view of physical processes. (Ibid., p. 21f.)

Those readers who dislike the Bohm theory might be tempted to quickly subsume it under the mechanicist world-view of 19th century physics. Against these allegations advocates of the Bohm theory could reasonably emphasize that it is anything but a mechanical theory in the Newtonian sense and that is contains, in virtue of its empirical equivalence with standard quantum mechanics, strongly non-deterministic traits which might be interpreted as a form of quantum chaos.[33]

Cushing does not give a priori grounds for preferring the Bohmian ontology of particles in motion rather than Copenhagen talk about measurement results, but he leaves the choice up to pragmatic criteria of theory preference. Although accordingly the ontology of physical theory is conditioned by pragmatic criteria and factual choice, Cushing with Quine rejects a Carnapian conception of ontology as linguistic framework (1950). While the latter approach might provide 'explanations' it cannot confer 'understanding' – as the concept is defined by Cushing.

We have reached one of the crossroads of post-positivist philosophy and thus the philosophical discussion has to slow down and check the right of way. The first to start is the relation of pragmatic criteria and historical contingency. Investigating

[32] Quoted from (Bohm, Hiley, and Kaloyerou, 1987, p. 331); italics by Cushing.
[33] See (Dürr, Goldstein, Zanghí, 1992b).

Cushing's case study will remind us of the holistic character of history and teach us how pragmatic criteria transform in time and between theories. The second to follow is holism philosophically conceived. Reasonable criteria for a proper ontology of a single scientific theory should – on pain of relapsing into Carnap's frameworks (too) narrowly understood – be able to accommodate other fundamental theories as well. Otherwise intertheoretical inconsistencies across theories might become conflicts between different metaphysical views even before Kuhnian anomalies abound. Third, let me just stress that Cushing's conception of mechanism is intimately linked to the Bohmian particle picture, and thus favors a very particular type of ontology. There are other mechanist and realist accounts, according to which particle paths in the Bohmian theory do not qualify as causal processes. Most interesting is Wesley Salmon's (1984) avowedly mechanistic account of the causal structure of the world, although its author has been quite reluctant to take a stand on quantum mechanics (Cf. Stöltzner, 1999b and Salmon's comment). To Salmon, a realist explanation is reached by successful reduction to causal processes at a lower level, but these processes are not necessarily deterministic. This shows that the far-reaching identification of realism and determinism typically advocated by Bohmians – but not only by them – is quite problematic. Might thus historical contingency not be a surprisingly effective hideout for metaphysical convictions? And might not large part of the equal rights case depend on accepting this identification?

As Cushing is skeptical about pragmatic criteria, he investigates the influence of contingent historical events on the development of quantum mechanics and finds, expectedly, the Forman thesis along the way. The stage for it is set after dropping some names as "philosophical precedents for the concept of indeterminism in nature." (Ibid., p. 96) The list, which contains Charles-Bernard Renouvier, Émile Boutroux, Henri Poincaré, and Harald Høffding, but not Exner, is even less compelling than Forman's "subterranean anticausality current." (Forman, 1971, p. 67). No wonder that "philosophical trends alone did not determine the course of quantum mechanics in the early part of the century." (Cushing, 1994, p. 97) Cushing modifies Forman's outlook by two distinctions.

> [P]sychological factors play a larger … role in the specific formulation of a theory while sociological ones can be crucial for the acceptance and propagation of an already-formulated theory. Such "external" psychological or social factors are not *solely* responsible for the content of science. Science has also its own "internal" demands and constraints that must also be accommodated. I argue that the "internal" factors were most important for the emergence of the formalism of quantum mechanics, "external" ones for the nature of the interpretation that was accepted. (Ibid., p. 100)

The approximate identification of internal factors and formalism is necessary to tame historical contingencies to the interpretation and prevent bad effects on the quantum mechanical formalism which standard quantum mechanics and the Bohm theory have in common. From Forman's standpoint it is surprising why the influence of the milieu should halt at the border between formalism and interpretation. And accordingly Cushing distinguishes

> a *strong* Forman thesis, which would claim a major causal role of the cultural milieu in determining the very form and content of a scientific theory, as opposed to a *weak* Forman thesis, which would see the cultural milieu as sometimes playing an important part in the acceptance and propagation of an already-formulated scientific theory. This weak Forman thesis can remain agnostic on the internal or

external nature of the factors that are responsible for creating and shaping the theory itself. (Ibid., p. 100)

While Forman advocates the strong thesis, Cushing limits himself to the weak version. Consequently the quantum formalism was largely unaffected by the milieu and rationally justified, even before 1927.

But initially two formalisms emerged the equivalence of which was proven only afterwards, though quickly afterwards. There was the wave mechanics route favored by the heterogeneous continuity group (Einstein, deBroglie, Schrödinger) and the matrix mechanics route elaborated by the quite uniform Göttingen and Copenhagen groups (Bohr, Heisenberg, Pauli, Jordan, and Born). Not being part of the German milieu, deBroglie "did believe that *one* theory should best conform to nature. He felt that classical Hamilton-Jacobi theory provided an embryonic theory of the union of waves and particles, all in a manner consistent with a realist (continuous) theory of matter." (Ibid., p. 104) After some early interest for Bergson and Poincaré, deBroglie derived support from the philosophy of Émile Meyerson. In was the Austrian Schrödinger who developed deBroglie's ideas into wave mechanics. At first sight Schrödinger's theory seemed to support the visualizability requirement of classical theory and "one might expect that the scientific community would have been inclined to take the more conservative of the alternatives on offer." (Ibid., 107) But matters turned out in favor of matrix mechanics.

I read in Cushing's account basically three internal factors. First, wave mechanics and matrix mechanics quickly proved to be equivalent formulations quite in line with Heisenberg's and de Broglie's joint conviction that there could be only one true theory of quantum phenomena. Second, Born's stochastic interpretation of the wave function showed that the Schrödinger theory was not so classical as it initially appeared. Third, on the matrix mechanics route "[d]iscontinuities, not causality as such, were initially the key issue." (Ibid., p. 108) The failed application of the old quantum theory to molecules had already convinced experts that the old picturable electron orbitals were meaningless. This was the "crisis" of the Bohr-Sommerfeld scheme. "The failure of the Bohr-Kramers-Slater theory [moreover] … in 1925 indicated to Bohr that a complete renunciation of the usual space-time methods of visualization of the phenomena would be necessary for further progress." (Ibid., p. 109) As Beller (1999) convincingly argues, Born's peculiar consequence was to strive for a theory of particles almost at any cost. But, so Cushing and Beller hold likewise, all these physical arguments were not conclusive to win the case for matrix mechanics not least because it was initially formulated without any interpretative commentary at all. "Heisenberg [merely] believed that a successful mathematical formalism of a physical theory … was of a piece or whole and that it could not be modified in any essential way without destroying the entire structure." (Cushing, 1994, p. 114)

But the challenge from wave mechanics increased; matrix mechanics was in severe "danger of losing the war on the calculational front." (Ibid., p. 117) Schrödinger's equation quickly became the most powerful tool in atomic physics, while Heisenberg's algebraical methods were – and still are – so clumsy that certain calculations are hardly doable. This lasting difference seems to me further evidence to doubt Cushing's claim that Schrödinger and Heisenberg were using the same formalism just because the theories were rapidly proven to give the same predictions. Physicists would continue to prefer Schrödinger's formalism even if equivalence held only with a grain of salt; in quantum field theory calculations with inconsistent and thus uninterpretable formalisms abound for merely practical purposes. The more mathematically-minded would cite von Neumann's (1932) uniqueness theorem as the deeper reason for the equivalence proof and relate that it is a nontrivial feature of finite-dimensional quantum mechanics that all representations are equivalent up to isomorphism. In the quantum field theoretical perspective it appears fruitful to distinguish both formalisms. Moreover, the date when this proof was actually given is an important, but historically contingent fact that influences the distinction between formalism and interpretation in quantum mechanics. Frederik Muller (1997) has recently argued that the equivalence proof taken literally did not concern the theories actually proposed in 1926 but the modified versions current as of 1932. Conversely, one could counterfactually imagine that equivalence would have been established only much later. In the absence of a general proof, Cushing's concept of formalism would crucially depend upon the equivalence of the predictions actually made to date.

At any rate, the Copenhagen group had to organize an interpretative defense against wave mechanics. It was based on combining two philosophical principles, positivism and finality, with organizational strength. To Göttingen-Copenhagen – so Beller's more appropriate geography – positivism became the major regulative principle of theory construction. When exhibiting an operationalist attitude and rigorously eliminating all unobservables, matrix mechanicians conceived themselves widely as executing the very program which Einstein had once pursued in relativity theory. But the alleged prototype refused Heisenberg's positivist company and retorted, according to Heisenberg's (1971, p. 63) report, that "on principle, it is quite wrong to try founding a theory on observable magnitudes alone. It is the theory which decides what we can observe. … Only theory, that is, knowledge of natural laws, enables us to deduce the underlying phenomena from our sense impressions." (quoted from Cushing, 1994, p. 110) And also Philipp Frank and Moritz Schlick had to see Einstein moving towards metaphysics.[34] Yet, Einstein's philosophical convictions are quite intricate and they hardly acquiesce in the two-camp picture suggested by Cushing.

The second philosophical element of Göttingen-Copenhagen was the notorious finality claim.

> Through his analysis of scattering processes with Schrödinger's formalism, Born came to the opinion that even *perfect* initial information still led to uncertainty in the result and this implied, for him, a lack of causality. Why was the complementarity principle taken as being complete and the final word in forbidding even the in-principle possibility of a description of microphenomena that is both causal and pictured in a continuous space time? One response is that (thus far) experience has shown the validity of complementary pairs of descriptions and that belief in the ultimate *necessity* of complementarity rests on the *subjective* epistemological criterion of the need for classical concepts and on the indivisibility of atomic phenomena (i.e. Bohr's act of faith). … For Bohr 'causality' meant the applicability of the exact laws of energy and momentum conservation. (Cushing, 1994, p. 112)

Bohr's later theory of complementary pictures provided "a *consistent* story, but it does not eliminate, in principle, a causal account." (Ibid., p. 108) By a mere declaration of faith Bohr ultimately claimed that these consistency arguments ruled out even the possibility of an alternative point of view. "These were not logical or in-principle

[34] See (Feuer, 1974, p. 83f.) and (Howard, 1994).

refutations, but strong, *practical* beliefs that became dogma." (Ibid., p. 108) Now we have assembled all those commitments which, to Cushing's mind, form the intersection of the interpretations of Heisenberg, Born, and others and can thus be considered as *the* Copenhagen interpretation: "complementarity, completeness of the description (in terms of the state vector or probability amplitude), a prohibition against any possible alternative causal description in a space-time background, and a positivistic attitude."(Ibid., p. 31)

I doubt whether these three creeds are able to keep the Copenhagen interpretation together. One might even wonder to what extent the Copenhagen interpretation still historically existed as an independent position after John von Neumann's *Mathematical Foundations of Quantum Mechanics* was published in 1932.[35] There the formalism was axiomatized, and the finality claim accordingly obtained a precise meaning and was based on explicit presuppositions in the form of a No-hidden variable theorem. Moreover, the brand name "Copenhagen Interpretation" did not come up until the 1950s (Howard, 2002).

At any rate, Cushing's thesis does not really depend upon the exact philosophical content of the finality claim because he also advances an argument about the contingent course of history. Referring to works of Beller (reaffirmed in Beller, 1999), Cushing holds that the conflict between Copenhagen and wave mechanics "can be characterized as one over superiority and professional dominance." (Cushing, 1994, p. 117)

> The Copenhagen group had the talent, organization, and drive to carry the day in establishing the hegemony of its view. Heisenberg's uncertainty relation paper was a major step in accomplishing this. The Copenhagen group worked in concert, while its opponents (Einstein, Schrödinger, deBroglie) pulled each in his own direction. … The Bohr Institute in Copenhagen had an enormous influence on an entire generation of leading theoretical physicists who passed through it. … As Ralph Kronig recalled, Bohr and his close colleagues were *authority* figures and a young person did not go against them. (Ibid., p. 117)

On the 1927 Solvay Congress, the united Göttingen-Copenhagen team won a great victory against deBroglie's first pilot-wave theory which promised a resurgence of a causal and realist world view. Pauli brushed it aside by an example which de Broglie could rebut no other than by ad hoc arguments; a definitive answer to Pauli's objections was obtained only by Bohm (1952). On the congress, "neither Einstein nor Schrödinger gave positive support to de Broglie's ideas. De Broglie presented a conceptual mixture of waves and particles, which did not incline Schrödinger kindly toward it since, at this time, he wanted an interpretation based *wholly* upon the wave concept." (Cushing, 1994, p. 118) In his speech to the congress, Einstein emphasized "that any truly fundamental theory … should be a *complete* theory of individual processes (as opposed to yielding information about the statistics of ensembles *only*). … Still, Einstein remained distrustful of this particular model of de Broglie. This was likely related to his own abortive attempt at a hidden-variables theory" (Ibid., p. 118f.) In May 1927, that is, just five months before the congress, Einstein had written a paper entitled "Bestimmt Schrödingers Wellenmechanik die Bewegung des Systems vollständig oder nur im Sinne der Statistik?" (Does Schrödinger's Wave Mechanics Determine the Motion of a System Completely or Only in the Sense of Statistics?), but

[35] This is, for instance, Peter Mittelstaedt's opinion; private communication.

quickly withdrew it; the piece never appeared in print. The reason was most probably the "peculiar sort of 'entanglement' between independent systems that appears when they are described as a composite system in multidimensional configuration space." (Belousek, 1996, p. 443) According to Belousek's detailed analysis,[36] Einstein's manuscript bears some striking similarities with Bohm's theory, a fact which, however, did not prevent Einstein from rejecting the latter as "too cheap."[37] Belousek concludes quite in line with Cushing that "the failure of the 'Bestimmt' scheme engendered in Einstein's thinking an overall skepticism toward the very possibility of a wave-particle synthetic completion of quantum mechanics that conditioned both his less than enthusiastic support for de Broglie's theory and his outright rejection of Bohm's theory." (Ibid., p. 453)

Cushing is reluctant to apply a weak Forman strategy also to the fate of Bohm's interpretation; let me briefly sketch how he story continues. In 1952, Bohm's intellectual opponents were still the same deBroglie had faced in 1927, and they once again reacted in a decidedly negative way – von Neumann being a notable exception (Cf. Stöltzner, 1999d). Einstein still had concerns about locality, but he "had even deeper reasons for rejecting such hidden-variables approaches to a completion of quantum mechanics. Quite simply, they were not radical *enough.*" (Cushing, 1994, p. 147) The late Einstein counted on a unified field theory that represented a radical break with all remnants of classical mechanics. Similarly as in 1927 the internal reasons were not compelling, and so Cushing turns to historical contingency. Rejecting – reasonably – any influence of Marxist materialist ideology on Bohm's interpretation, he cites Heisenberg's contention that all opponents of the Copenhagen view wanted to return to the ontology of materialism, to a completely objective description of nature, rather than accepting Copenhagen's subjective element in the description of atomic events.

At this point we realize the simple fact that natural science is not Nature itself but a part of the relation between Man and Nature, and therefore dependent on Man. The idealistic argument that certain ideas are *a priori* ideas, i.e., in particular come before all natural science, is here correct. (Heisenberg, 1955, p. 28, quoted from Cushing, 1994, p. 153)

This was the basis on which Heisenberg took the positivist tack that observationally equivalent theories, such as Bohm's, just signify a difference of language. True, this sounds like a positivist meaning criterion. But as Cushing himself notices, already in the 1920s Heisenberg had rejected any underdetermination of theory, in stark contrast to the Viennese positivists. Moreover, if we compare Heisenberg's charge against Bohm of relapsing into materialism with Frank's (Section 8.4.), we see that Heisenberg just replaces one metaphysical world view with another. His emphasis on idealism and a priori ideas elevated, as it were, two anathemas of Logical Empiricists to lessons to be drawn from quantum mechanics and, finally, his insistence on the subjective-objective distinction was precisely that sort of dualist metaphysics which Mach's neutral monism wanted to overcome (See Ch. 3). On such metaphysical a foundation, Heisenberg's meaning criterion was anything but a genuine positivist argument.

[36] See also (Howard, 1990).
[37] In a letter to Born, 12 May 1952. In the *Festschrift* for Born, he even published a technical refutation of Bohm's theory which, however, could be successfully countered by Bohm in the same volume.

2.1.2. A Counterfactual History and Its Extrapolation

Cushing leaves no doubt that, in his view, Bohm's alternative "is arguably more coherent and understandable than the commonly accepted dogma." (Cushing, 1994, p. 174) But, "I have no final word to offer on Bohm's program, since many aspects of it remain to be developed." (Ibid., p. xi) This is a state of affairs, so Cushing believes, quantum mechanics could have arrived at much earlier. To support this claim, he develops a whole alternative history which begins around 1925-1927 and condenses into a couple of years historical developments which in real time took roughly four decades until the proof of Bell's theorem (1965). The whole counterfactual history counts almost exclusively on Einstein, on his titanic scientific abilities to quickly prove results which advocates of the Bohmian program obtained only much later, on Einstein's interpreting these results in a particular realist fashion, and on his great public prestige to change the thrust of the whole quantum mechanical research into an investigation of the Einstein-de Broglie theory. Here is Cushing's summary of the alternative scenario which, to my mind, proceeds in five steps.

> [1.]Study of a classical particle subject to Brownian motion (about which Einstein surely knew something) leads to a "classical" understanding of the already discovered "Schrödinger" equation, which is then given a realistic interpretation. A "Nelson" stochastic mechanics underpins this interpretation with a visualizable model of microphenomena. This would have made evident the possibility of a largely classical foundation for that key equation. A realistic ontology would still remain a live option. …
>
> [2.]Since stochastic mechanics [à la Nelson] is quite difficult to handle mathematically, people would likely have tackled the less formidable task of exploring the implications of the (equivalent) *linear* Schrödinger equation. The Dirac transformation theory and an operator formalism would still have been available as a *convenience* for further development of the formalism …
>
> [3.]The entanglement or nonlocality of that formalism would soon become apparent. Einstein did not like such nonlocality and would have rejected any model with this property. Yet the conceptual background existed, I shall argue, even in 1927 to prove a Bell-type theorem. If that had happened at that time, then Einstein and the rest of the quantum physicists would have perceived in sharp relief the choice between determinism and locality in *any* theory. A causal formulation of quantum theory might then have appeared less unpalatable than the Copenhagen version actually chosen by the scientific community.
>
> [4.]Einstein might have next made the transition from stochastic mechanics to "Bohmian" mechanics [that is the version of Bohm's interpretation due to Dürr, Goldstein, and Zanghì] since … stochastic mechanics turns out to be both indeterministic and nonlocal.
>
> [5.]It is well-known that Einstein was deeply committed to a realistic worldview in which microentities have a continuous, objective, observer-independent existence. (Cushing, 1994, p. 174f.)

At first sight, Cushing's alternative history has a smack of Hegelian 'cunning of reason'. Foundationalist and operationalist motivations enter the scene just in due succession; step 2, for instance, claims that the whole quantum mechanical formalism was developed out of calculatory convenience while in step 3 it became the object of foundational studies. 'Cunning of reason' is no stranger in the rational reconstructions of science. According to Lakatos' mind, it characterizes an elitist conception of science. "It is only this then, *ad hoc* authoritarian/historicist doctrine which separates élitists of this kind from the [Feyerabendian] sceptic." (Lakatos, 1978b, p. 117) Thus in a Lakatosian perspective and in the absence of explicit demarcationist criteria, Cushing just attempts to ponder what would have happened if Einstein had scored out the Göttingen-Copenhagen elite. Indeed, Cushing's alternative history is not after a full-blown rational reconstruction.

[I]t does not seem worth demanding that my counterfactual scenario *had* to go along some highly specific path. What is important is that there were precedents for such moves and that the necessary pieces were already there. … We could today have arrived at a *very* different worldview of microphenomena. If someone were then to present the (merely) empirically adequate Copenhagen version, with all its own counterintuitive and mind-boggling aspects, who would listen? (Cushing, 1994, p. 175)

There are other plausible historical scenarios. "Schrödinger's original program for the interpretation of the quantum theory need not have been abandoned so hastily. Theoretical tastes have much to do with this. But for a largely sociological accident, Schrödinger's essentially classical field theory for quantum phenomena could have been successfully pursued." (Ibid., p. 175f.) As we shall see in Chapter 6, Schrödinger would be a bad candidate for an alternative history of the type Cushing has in mind.

Of course, one might continue in this vein. Yet my ambition is not to tell the most compelling quantum tale. Rather do I intend to show that the philosophical convictions of some key scientists made them quite reluctant, or even resistant to, conversions enforced from outside or to begin a new theoretical life. There is, so to speak, a broader kind of rationality at work in the scientific process than the rational reconstruction of a theory's argumentative structure reveals. It includes criteria and methods of theory choice as well as philosophical convictions of a general nature. This is an important lesson of Mach's above-quoted holism in the history of science: scientists do not start with empty hands and eyes open, immersed into and on the basis of a provisional world view. The rationality of the scientific process as a whole does not require that all steps qualify as fully rational with the benefit of hindsight. Thus Cushing's counterfactual problem is not just a standard case of rational reconstruction within one research program, but requires a detailed comparative historical analysis.

Interestingly, Cushing's book failed in the eyes of Forman to whom it is deeply indebted. Expressing general sympathy for "a philosopher who rejects his discipline's canonical distinction between the context of discovery and the context of justification" Forman criticizes that Cushing's "argument is divided over two, only very loosely connected historical fronts", one located in the 1920s and one concerned with explaining the marginal status of the Bohm interpretation after 1952. And more generally, "to make this case [for quantum mechanical historicism] – indeed *any* counter-factual case – is considerably more difficult than Cushing appears to recognize." (all Forman, 1995, p. 1844) To this end the historical material which only goes little beyond Jammer's seminal book (1974), does not suffice. Strangely, Forman does not address the relation between the alternative history and his own investigations, (Forman, 1984) in particular.

It is true, if there exists a genuinely causal influence, there is no space for alternative histories however composed which are to establish a causal quantum theory in a milieu longing for acausality. The alternative history would have to falsely tag itself as anticausal as Heisenberg had done, on Forman's account, with respect to the

Anschaulichkeit of quantum mechanics. So if Cushing's alternative history is credible, the strong Forman thesis has to go. Forman's skepticism about Cushing's counterfactual is, to my mind, basically right although the charge of historicism misses the point because historical contingency is confined to a very small domain, the interpretation of quantum mechanics given an accepted formalism. Still, Cushing's argument bears serious dangers for the Forman thesis, or at least, it elucidates Forman's strong philosophical motivations.

The pivotal question of Cushing's counterfactual history is "what impact a 'Bell' theorem might have had around 1927." (Ibid., p. 176) Although Bell's theorem in the 1960s was received more positively than Bohm's interpretation,[38] it never spellbound the physics community as Cushing counterfactually assumes to be the case in 1927. Back then quantum physicists "could have seen the conflict between determinism and locality in *any* theory." (Ibid., p. 179) Faced with this choice and having learned of or performed himself the necessary steps 1. and 2., "finally Einstein would not have seen locality as a truly a priori [sic!] concept necessary to do physics" (Ibid., p. 265), and he would consequently have developed steps 4. and 5. of the alternative program. "The crucial issue is how, in an observer-independent reality, Einstein would have evaluated or weighted causality (or determinism) versus nonlocality, given that one of these *had* to go." (Ibid., p. 179) To ease his pain at this point, the alternative history quickly made the non-localities benign by a no-signaling theorem. "Although Einstein might still have found Bohm's theory unacceptable ('too cheap') as a *final* theory (and Bohm himself never suggested that is was final theory), he might also not have rejected it out of hand because of the (now benign) nonlocality." (Ibid., p. 182)

I do not doubt that formally Cushing's counterfactual argument is correct. However, the linch pin of the alternative history are Einstein's philosophical preferences. Here interpreters disagree even with respect to what the horns of the dilemma are. While Cushing builds largely upon Arthur Fine's (1986) interpretation of Einstein's realism, Don Howard (1985, 1990) disagrees and detects a difference between locality and the more basic concept of separability, which was responsible for withdrawing the "Bestimmt" manuscript. Although I have certain preferences for Howard's description of Einstein's philosophical views, in particular because meanwhile Howard (1994) has convincingly linked them to the encounters on realism and causality analyzed in the present book, I cannot enter into these intricate matters here. So I must simply accept Cushing's position that Einstein could have reached the philosophical conclusion to prefer causality over locality. But that won't suffice, as the remainder of this section intends to demonstrate.

However Einstein decided, the historical problem remains that in one way or another all quantum physicists accepting this view would have faced "the possibility that nonlocality may well turn out to be a feature of the *world*" (Cushing, 1994, p. 179) and that "[r]elativistic invariance could turn out to hold only at the observational level, not necessarily at the level of abstract space-time as usually envisioned in special relativity." (Ibid., p. 183) How would the scientific community and the general cultural milieu have reacted to such declarations from Einstein's side? Even if one accepts Cushing's contention that *philosophically* Einstein might have made his peace with benign non-locality, we have already seen that *historically* the causality debate was

[38] For the reasons, see (Pinch, 1977) and my Lakatosian criticism in Section 4 of (Stöltzner, 2002c).

indissolubly linked with the acceptance of relativity theory. Cushing argues that the real history was contingent in an important respect; so he must accept that also the alternative history would have been contingent too. Let us thus pick up the thread of the alternative history at step 5 and imagine a comprehensive paper of Einstein's appearing in *Die Naturwissenschaften* at about 1929. Certainly, Göttingen-Copenhagen would not be pleased. Others, Born and von Laue, would have seen the acceptance of non-locality as Einstein sacrificing his brainchild relativity theory. The fact that special relativity was rescued at the observational level could perhaps have been accommodated by Logical Empiricists after a detailed analysis of the no-signaling theorems. But Planck would have fiercely contradicted to abandoning the principle of invariance, which he held to be more basic even than causality. (See Sect. 3.8.) To be sure, Logical Empiricists would have emphasized that the Einstein-de Broglie theory – as contemporaries might have called the resultant of step 5 – did not amount to a return of the world view of classical mechanics and materialist metaphysics because the particle paths guided by the pilot wave were empirically inaccessible. Pondering whether particle paths and quantum potential, accordingly, were altogether meaningful concepts, at the example of an EPR gedanken experiment – which had been established for merely didactic purposes in step 3. –, Logical Empiricists might have investigated the relation between really existing quantum entanglement and Mach's principle. All these are quite difficult philosophical arguments would have been most suitable for publication in *Die Naturwissenschaften* but unsuitable for the popular press that would certainly have titled a "new revolution of Einstein".

Who would be most fascinated by the publicity of the Einstein-de Broglie theory? I am afraid but my guess is Philipp Lenard, Johannes Stark, and all those who rejected modern theoretical physics altogether. One of the main virtues of the Bohm interpretation is, according to Cushing, its greater intuitive appeal to people educated in classical physics. There are *anschauliche* particle paths and the statistical features of the theory are as *anschaulich* as Brownian motion. As shown in Section 1.3., the *Anschaulichkeit* debates in quantum mechanics and relativity theory were intimately linked. Still in 1930, Sommerfeld criticized in detail several misunderstandings of Stark who had objected to the rotational symmetries inherent in quantum mechanics by arguing that the electron is always on one side of an axis through the center of the nucleus. Moreover Einstein-deBroglie stochastic mechanics would not have required entering into talk about measurability familiar from special relativity and tainted with positivist philosophy. Operationalistically flavored no-signaling theorems which could hardly be assessed in any *anschaulich* manner would have convinced the conservatives that the alternative between causality (or realism or *Anschaulichkeit*) and locality was tantamount to a choice between atomic physics and relativity theory. As observational evidence decisively favoring general relativity over its competitors was still poor – because difficult to obtain – in comparison with loads of spectroscopic data indicating the superiority of Einstein-deBroglie stochastic mechanics – and the observationally equivalent matrix mechanics of Heisenberg – over Bohr-Sommerfeld, Lenard and Stark would have understood Cushing's alternative history as the desired rejection of relativity theory.

Ideological and milieu factors support this conclusion. As the Golden Twenties were drawing to their end, the great depression in 1929 caused anew the general feeling of crisis on which the Forman thesis was built. National Socialism and antisemitism were quickly rising in this milieu, and after 1928 Lenard sought Nazi company in public. For him, the Einstein-deBroglie theory would have been a great opportunity to once and for all eradicate *Jüdische Physik* – based on the work of a Frenchman – as inconsistent and self-destructive, and replace it by *Deutsche Physik*. Admittedly, all this sounds weird, but thus was *Deutsche Physik*. And thus the extrapolated counterfactual would end in a flood of polemics in daily and weekly newspapers of the kind familiar from the clashes about Einstein's relativity theory which had sparked the early 1920s.

What does this little *reductio ad absurdum* teach? Quite generally, counterfactuals are construed by varying the actual scenario in some respect leaving all other conditions fixed. This variation is motivated by what the counterfactual argument purports to prove. Cushing varies the interpretation of quantum mechanics but leaves unmodified the formalism, all other scientific theories, his protagonists' philosophical views, and the general milieu. Thus his conclusion obtains only *ceteris paribus*. Philosophically *ceteris paribus* arguments exhibit a number of notorious problems, in particular, one has to actually control the *ceteris paribus* conditions. My *reductio* argument – as strange as it is – shows that Cushing has failed to do so. The key problem was the intimate coupling of the interpretation of quantum mechanics to be varied under the *ceteris paribus* condition relativity theory. Cushing's argument only goes through if one claims, in contrast, that quantum mechanics could be treated in isolation from other physical theories.[39] But doing so leaves little space for genuine historical contingency.

Moreover, even though at surface value the strong Forman thesis is unaffected by this *reductio,* it becomes clear how strongly both Forman and Cushing are – more overtly than not – indebted to those concepts of causality, realism, and *Anschaulichkeit* which characterize classical physics. While Forman leaves matters here, Cushing is at pains to accommodate the manifest indeterministic features which the formalism simply produces and which are experimentally verified. There are basically two strategies used by Bohmians unless they modify the formalism. Either they invoke the initial wave function of the universe and the initial configurations of the Bohmian particles – whatever that means at the initial singularity of a big bang cosmology – to reintroduce uncertainty as a feature typical – in the sense of statistical mechanics – of the real world (Dürr, Goldstein, Zanghì, 1992a). Or quantum mechanical indeterminism is considered as a case of chaotic behavior. This is the key to the philosophical morals Cushing himself draws from his counterfactual.

Could, at the end of the day, determinism and indeterminism be ultimately equivalent. "Can so fundamental a property of the external world be undecidable by empirical test and can the choice be observationally irrelevant? " (Ibid., p. 208) The stark contrast between the ideal of Laplace's demon and the manifest chaotic behavior of mechanical systems much simpler than our solar system reminds us that "classical mechanics ought not naively to be equated with a determinism that necessarily allows complete and meaningful predictability."(Ibid., p. 210) Newton himself

[39] As a matter of fact, this is a characteristic trait of some present-day foundational discourse in quantum mechanics.

did not believe that the mathematical laws as represented in his *Principia* were in themselves sufficient to explain or to predict the long-term stability and future evolution of the physical universe. It was only after Newton that the determinism and complete predictive accuracy of his laws of mechanics became accepted. A general cultural background had been conducive to a deterministic gloss on the laws of mechanics. (Ibid., p. 210)

Underdetermination again. This insight that "[d]eterministic chaotic systems can be as irregular (and unpredictable) as a truly random system" (Ibid., p. 213) leads Cushing to an argument that figured prominently in Schlick's (1920) first theory of causality (See Section 7.1.). There is no warrant for believing that despite the chaotic nature even of simple mechanical systems, still "at the most fundamental level, the universe is governed by deterministic laws. … Who would be impressed by such an argument or theory, since its effective 'predictions' would be empirically indistinguishable from those that would obtain in a universe that was at base completely indeterministic?" (Cushing, 1994, p. 212) For this reason, Schlick put forward some characteristic regulative principles for causal laws, most important among them simplicity. He later (1931) revoked this position in favor of the sole criterion of successful prediction; but the pragmatic criteria remained important.

Cushing, in the end, distinguishes the pragmatic criteria of causal explanation from the causal structure of the real world.

[A]s a pragmatic matter, we can simply choose, from among the consistent, empirically adequate theories on offer at any time, that one which allows us best to "understand" the phenomena of nature, while not confusing this practical virtue with any argument for the "truth" or faithfulness of the representation of the story thus chosen. Successful theories can prove to be poor guides in providing deep ontological lessons about the nature of physical reality. (Ibid. p. 215)

As science has such feeble a bearing on ontology,

belief in determinism as being more fundamental than probabilities in the actual physical world is an act of *faith,* rather than a position demanded, or even particularly well warranted, by the laws of (even classical) physics and by the observed behavior of physical systems. Usable determinism reigns almost nowhere and chaos nearly everywhere. Newtonian (Laplacian) determinism may remain only a theorist's unattainable dream. (Ibid., p. 214)

Indeed, the kingdom of Laplace's demon has become small these days. But I wonder whether the set of alternatives to be decided by act of faith is complete. Focusing on predictability only, Cushing puts deterministic chaos and indeterminism on a par. Here I disagree. Modern theories of complexity conceive of a substantial difference between deterministic chaos and genuine indeterminism where statistical laws can take hold. It seems that Cushing's identification follows from the identification of a theory's formalism with the formal expression of its empirical content. It is very easy to formulate simple systems exhibiting deterministic chaos, but on Cushing's account the poor predictive value of these formulae utterly evaporates such a formalism. On the other hand, for phenomena such as turbulence a genuinely indeterministic approach is more suitable and predictively more fruitful than an analysis of the system's chaotic behavior. For Richard von Mises (1922a) precisely this became an argument in favor of a genuinely statistical approach.

Although, to Cushing's mind, determinism in general remains an act of faith, it still is a valid criterion of theory choice that (could have) had a strong Whiggish appeal also within the counterfactual history. So are consistency with the ontology of classical physics and the possibility of an interpretation in terms of particles in motion as a pragmatic criterion of theory choice. “Who would have listened to Copenhagen after the Einstein-de Broglie theory in place?” might be considered as a non-formal type of simplicity of the overall discourse, to wit, whether a theory simply merges with the prevailing philosophy or with the common sense of a period. (Cf. Frank, 1954)

> Either of the two theories passes a test of fertility, in the sense of possessing the internal resources to cope with anomaly and new empirical developments that actually occurred, as well as for suggesting new avenues for research and generalization. My historical counterfactual scenario … has indicated that Bohm’s theory was not an ad hoc, stillborn creation that could have matched Copenhagen only for the simplest cases … I do *not* claim that Bohm’s theory and extensions thereof have yet been generated that matched *all* the successes of the standard approach or that there are *no* internal inconsistencies yet to be discovered. *Few* people, after all, have worked on this alternative program. Nor am I claiming that more people *should* work on it. (Cushing, 1994, p. 206f)

Granted that the pragmatic criteria do not play a strictly normative role, one nevertheless might wonder how they really act in counterfactual histories. Let me pick up again the general issue of historical counterfactuals over and beyond the issue of *ceteris paribus* conditions. Even if restricted to a certain place or time frame, or to a particular thematic context, any history must be a complete history. Einstein was the most prominent pioneer of modern theoretical physics and he can hardly be reduced to a bunch of philosophical and physical convictions about quantum mechanics. This point holds both internally and externally; also the milieu cannot be switched off at will. Integrity of history proves a hobble for counterfactuals because one cannot arbitrarily shift back and forth, or stretch and condense, scientific developments on the historical scale. In contrast, this is possible in rational reconstruction. Take, for instance, conformity to classical physics. Today most scientists got used to the paradoxical features of quantum mechanics and to the even more paradoxical features of quantum field theory; the vast majority hardly would consider unobservable particle trajectories as a great pragmatic virtue. Cushing is surely right that in 1927 the score would have been just the opposite. As this pragmatic criterion is clearly not invariant under time translation, the counterfactual history does not make the Bohm interpretation an equal competitor in virtue of conformity to classical ontology. The same for fertility. Other than a few toy models, to date there has been little progress with relativistic versions of Bohmian mechanics and Bohmian field theory. Maybe higher funding could have yielded an equal score, maybe not because the theory is so overtly non-local. Despite serious mathematical difficulties, sixty years of progress in quantum field theory cannot be annihilated by just comparing the quality of the seeds in 1927, rather than the matured trees in 2002. Even a criterion that presently supports the de Broglie-Bohm theory, to wit, the interest it has recently attracted among quantum cosmologists, cannot be shifted back to 1927 into a support of the Einstein-de Broglie theory because the boom in cosmology did not start before the 1960s. Of course, there exist criteria which are (still) time-invariant. The ontological status of the quantum potential is today unclear as ever because it has no interaction with anything other than just guiding a single quantum particle and ether-like mediating their interactions. In Salmon’s terminology, it just participates in pseudo-processes. (Cf. Stöltzner, 1999b). If one considers the quantum potential only as a secondary entity parasitic on the wave function (from whose derivative it emerges) and asserts that the pilot-wave equation needs not be solved, problems with ontological parsimony emerge. In 1927, to be sure, they might have been counterbalanced by ontological conformity, but today this strategy is not viable.

Cushing's counterfactual is thus no sufficient reason to investigate the Bohm theory. But to my mind, it is not necessary as well. The pragmatic score sketched above was not hopeless. Moreover, the Bohm theory provides a very interesting model that the quantum mechanical formalism – now denoting all basic concepts and equations – can be modified in certain non-trivial ways without changing the empirical content. Against the backdrop of the philosophical analysis of scientific theory as developed since the 1920s, this seems already a sufficient motivation to study this alternative.

2.2. Beller: On Dialogues and Revolutions

In many respects Mara Beller's *Quantum Dialogue, The Making of a Revolution* can be read as the historian's counterpart to Cushing's *Quantum Mechanics*. Not only does Cushing repeatedly draw upon historical material from earlier works of Beller, they also agree in their aim to win the Bohm theory the status of an equal among equals. Thus I will not repeat historical details mentioned in the previous section, but focus on interpretative lessons of relevance for the philosophical discourse on causality.

While Cushing argues by historical counterfactual, Beller attacks "the philosophical legitimation of the Copenhagen ideology" (Beller, 1999, p. 272, fn. 5) head-on.

> It is not the historian's task to offer a specific alternative to the orthodox Copenhagen interpretation. I take no stand on the existing alternatives to orthodox philosophy. This book does not deal with the extensive and lively contemporary research of the philosophical problems of quantum physics. Rather, my historical, philosophical, and sociological analysis of the Copenhagen philosophy demonstrates the possibility and the need of a viable alternative to the orthodox interpretation. (Ibid., p. xiii)

2.2.1. Dialogical Emergence Versus Rhetorical Consolidation

Beller's book consists of two parts separated by the year 1927 and by methodological orientation. The first one is dedicated to the "Dialogical Emergence" of quantum mechanics up to 1927. The second titled "Rhetorical Consolidation" investigates how the orthodox narrative of the inevitability of the Copenhagen interpretation was established after 1927. Rather than being embedded into two distinct and pre-existing theoretical frameworks, Heisenberg's matrix mechanics and Schrödinger's wave mechanics "crystallized as the end result of a conceptually fascinating and emotionally intense confrontation among quantum physicists. In the fruitful ambiguity of the newly created knowledge, there was no place for strong 'beliefs' in indeterminism or 'commitments' to positivism." (Ibid., p. 3f.) Eventual "pronouncements on these issues were uncommitted, fluid, and opportunistic." (Ibid., p. 17) There existed a very fruitful dialogue between the various quantum physicists. Conceptual strategies and formal tools were mutually exchanged, so that a multitude of positions developed just to be modified quickly afterwards when the colleagues had announced new findings.

In this perspective basic papers – or rather: master texts – exhibit an irreducible polyphony.

First, Heisenberg's uncertainty paper (1927) was full of challenges, doubts, and contradictions on all levels above and beyond the mere formulas.

> [Philosophically] contradictory voices of positivism (operationalism), model theoretic realism (the invariant features of a successful scientific theory refer to genuine aspects of reality), and conventionalism (physicists can freely choose the basic axioms of a theory, worrying only about its consistency and empirical adequacy) are all present in Heisenberg's paper. (Beller, 1999, p. 104)

Already two years earlier, Heisenberg's efforts were not guided by a "coherent philosophical choice between positivism and realism" (Ibid., p. 52), but there prevailed a peculiar mixture of "the positivist, identifying the meaning of a concept with the procedure for its verification (in real or imagined physical interactions [like the microscope thought experiment]), and the realist, deducing the genuine features of the quantum world from characteristics of the mathematical formalism." (Ibid., p. 113f.) Yet for Logical Empiricists, these two voices in Heisenberg were not inconsistent; in his "Positivism and Realism" Schlick (1932) argued that who accepts the verificationist criterion of meaning has not problem with realistically interpreting certain terms in the formalism.

Second, Bohr's Como lecture (1927) "was not the resolution of wave-particle duality by the complementarity principle but rather an extensive defense of his concept of stationary state and discontinuous energy changes." (Beller, 1999, p. 118) What appears as "one of the most incomprehensible texts in twentieth-century physics" (Ibid., p. 8) is in fact "filled with implicit arguments with the leading physicists of the time – Einstein, Heisenberg, Schrödinger, Compton, Born, Dirac, Pauli, and the lesser known Campbell." (Ibid., p. 120) In this way the "deep conceptual gap fundamental between Bohr's wave-theoretical and Heisenberg's particle-kinematic interpretation of atomic systems" (Ibid., p. 122) becomes evident. This gap "is one of the historical roots of the inconsistencies that plague the Copenhagen interpretation of physics" (Ibid., p. 122) to date. Yet the united front had to be established very quickly to meet the challenge of Schrödinger's intuitively appealing and technically superior formalism. "In his Como lecture Bohr [also] set the historical record straight … [and] presented matrix mechanics as the culmination of his own research program, based on the correspondence principle." (Ibid., p. 141) Heisenberg complied with Bohr's leadership and thus the orthodox narrative was instituted.

The strongest challenge to the Copenhagen dogma was the Einstein-Podolsky-Rosen paper (1935) which struck the already consolidated Copenhagen hegemony by surprise. Repeating the successful rhetorical move of 1927, in Bohr's reply "sincere and open-minded, though interest-laden, interpretative attempts hardened into an ideological stand intended to protect quantum theory from challenge and criticism." (Beller, 1999, p. 9) The paper marked the "transition from legitimate, though often confused arguments for the consistency of quantum theory, to argumentative strategies promoting the inevitability of the orthodox stand." (Ibid., p. 9) The year 1935 separates two voices: reference to Heisenberg's idea of physical disturbance fades away because EPR had made it untenable, while the "emerging operational voice will culminate in unreserved verificationism." (Ibid., p. 151) Bohr's account of the EPR paper introduced philosophical weaknesses not existing in the original. While in the

EPR paper "the elements of physical reality are the physical variables that can be predicted with certainty, … Bohr reformulated the passage from EPR into a metaphysical discussion of what physicist mean when the say 'reality'. This reformulation … had a strong rhetorical effect." (Ibid., p. 154) Einstein appears as the outmoded naive realist, which – of course – he is not. Eight years further down the historical road, we find again the conundrum of Einstein's realism which as of 1927 was the linch pin of Cushing's counterfactual.

Thus the "Copenhagen interpretation was erected, not as a consistent philosophical framework, but as a collection of local responses to challenges from the opposition." (Ibid., p. 167) Copenhagen's formal consistency was elevated to its inevitability and to the finality of quantum mechanics. In the subsequent years, "the opposition's stand is delegitimized and trivialized" and "the past is manipulated to make the winners look naturally right." (both ibid., p. 10) Beller classifies this shift by adopting terminology from the influential lecture course of Nobel laureate Richard P. Feynman (Feynman/Leighton/Sands, 1969)

> In the two-slit experiment, one "need not" assume that the particle traverses a well-defined path between the two-slit diaphragm and the detector. … "Need not" is an integral part of scientific practice, without which such breakthroughs as the rejection of absolute simultaneity in relativity theory and the rejection of a strict determinist framework in quantum theory would not have been possible. "Must not" is a positivist excess, at odds with the practice of science, which relies on realistic models as a heuristic guide to discovery. (Beller, 1999, p. 174)

The allegation that positivism hampers scientific progress is not new. Planck threw it against Mach (Section 3.7.), Sommerfeld against Frank (in a letter to Schlick), and it notoriously suffered from a restricted view on the target. But the dichotomy is well-suited for the "scientific pugnacity" so heavily criticized by Neurath (1915).

While the microanalysis in the first part of Beller's book – though on a different methodological basis – complies with Neurath's advice that historians of science should investigate all intermediate positions, the second part itself lives such pugnacity and rides a rhetorical counterattack against the Copenhagen dogma. Instead of the dialogical microperspective, now the orthodox narrative is put against the backdrop of its few critics, above all David Bohm and John S. Bell. Historical succession is dissolved; pronouncements of Bohr and Heisenberg are contrasted with criticism made much later.[40] The rhetoric appeal of antirealism as a strategy to protect science and to appease internal conflicts are discussed against the backdrop of P.W. Bridgman's operationalism (Beller, 1999, pp. 176-180) instead of mentioning any realism debate concurrent with the emergence of the Copenhagen interpretation in Europe. In the end, Beller consequently does not arrive all too far from Cushing's counterfactual outlook, and her plea for an alternative interpretation faces the same methodological problems as shifting criteria of theory preference back and forth in history.

The most disturbing feature of Beller's presentation is the harsh and sometime derogatory rhetoric – somewhat reminiscent of Forman (1971). Among the bad guys is Heisenberg whose "hostility to Schrödinger's theory seems more likely to be connected with his instinctive reluctance to admit anybody else into territory that the ambitious Heisenberg considered his own." (Beller, 1999, p. 32) In his recollections

[40] Above all in Chapter 9 of the book, e.g. on p. 193 and p. 201, but also the comparison between Bohr and Bohm concerning quantum wholeness on p. 256.

Heisenberg transgressed the subtle line between "manipulation of history and deliberate deception." (Ibid., p. 213) But the really bad guy is Bohr. "As is suitable for a prophet, Bohr talked in fables and parables." (Ibid., p. 244) Being "an avid storyteller" (Ibid., p. 243) but – other than Bohm and Bell – "[l]acking advanced mathematical skills, Bohr could not build a new quantum ontology but instead had to use 'common language' and simple analogies. This personal trait, if not weakness, was canonized into the universal doctrine of the indispensability of classical concepts and the impossibility of a quantum ontology." (Ibid., p. 259) Disabled in his computational capacities, overloaded with administrative duties, depending upon his authority to make his assistants carry out his vague intuitions, despite all "hero worship" (Ibid., p. 153), "Bohr was a tragic figure." (Ibid., p. 271) "The legend that Bohr had some sort of access to nature's secrets, qualitatively different from that of other mortals, directly discouraged critical dialogue." (Ibid., p. 271) In actual fact, this legend was older than the Como lecture or the philosophy of complementarity (See Sect. 4.7.2).

Within her black and white set-up,[41] Beller did not undertake any further investigation why communication was inhibited, although an interesting sociological model is available at least for the case of Bohm and von Neumann which, to my mind, might be suitably adapted. Trevor J. Pinch commences from the following distinction.

> The research-area mode of articulation occurs when the disputed object forms part of the particular area of concern of scientists involved in the controversy. … The official-history mode of articulation, by contrast, occurs when the cognitive object is referred to in some other context than the immediate area of concern. … I regard this context as being mainly the production of cumulative history of rationalisation of how a particular field developed. (Pinch, 1977, p. 175)

Bohm's (1952) proposal challenged the interpretation of quantum mechanics in the research-area mode by producing a counterexample and publishing it in the discipline's leading journal. On the contrary, "much of the response of the [quantum mechanics] elite has been articulated in the official-history mode," (Ibid., p. 187) mainly by restating von Neumann's theorem. Bell's (1966) counterexample found better acceptance than Bohm's because he could precisely spot the defect in von Neumann's theorem and, accordingly, dragged the opponent back into the research-area mode. In the discussions ensuing the EPR-paper, this did not happen – although the paper represented the germ of Bell's later analysis. At bottom, it appears to me that large part of Beller's distinction between "dialogical emergence" and "rhetorical consolidation" boils down to the two modes distinguished by Pinch.

2.2.2. On Dialogues and Dialogism

Beller's undisputed good guy is David Bohm and he wisely provided a view of science according to which his interpretation can be judged equal among equals.

> How fitting for Bohm, a victim of this approach [quantum orthodoxy], to develop a diametrically opposed view of science – pleading for tolerance, for creative plurality, for peaceful theoretical coexistence, for a free play of imagination, for friendly, open-minded, and joyful scientific cooperation and communication. (Beller, 1999, p. 210)

[41] Cf. the fully justified criticism of Dickson (2002).

The "attitude" (Ibid., p. 325) Beller advocates in Bohm's footsteps is dialogism. "[T]he notion of an overarching scientific method that guides an individual scientist is foreign to it." (Ibid., p. 321)

> From the dialogical perspective, there simply are no final, stable, elements, or facts – everything can be questioned and doubted. Nor is there a total, final resolution of tension – conflicting voices coexist, or are temporarily put aside. Rarely are they completely extinguished. A paradoxical tension exists between the openness of a scientific text, addressed to the future, and its solid roots in the past. … The dialogical nature of creativity explains why reinforcement, support from another voice is so important. (Ibid., p. 105f.)

Dialogism takes "addressive response as the primary epistemological and social unit for the analysis of science. Thus the notion of a scientific thought presupposes the existence of an interlocutor to whom the thought is addressed or by whose statements the thought is triggered." (Ibid., p. 308) Indispensability of an interlocutor however proves problematic at places and it creates the need to fake partners. For instance, Beller cannot show that Heisenberg ever read the works of the "lesser' scientists" Norman Campbell or H.A. Senftleben. However, "Campbell's suggestion that time is statistical in nature" (Ibid., p. 97) had already been stated by Boltzmann and concerning irreducible indeterminism the name Senftleben could easily be replaced with Exner whom Heisenberg's teacher Sommerfeld knew well (Sommerfeld, 1926) and whose priority in this matter Schrödinger defended untiringly.

Dialogism accepts the "preeminence of disagreement" and considers factual agreement as "useful but of little explanatory power." Moreover: "Too much emphasis on agreement, combined with the philosophical thesis that theory is underdetermined by experimental data, has resulted in the excesses of the sociology of knowledge and the flattening of the cognitive to the social by social constructivists." (All ibid., p. 309) "In the dialogical approach, the historical, philosophical, and sociological merge into a unified viewpoint, rather than being independent perspectives." (Ibid., p. 313) In dialogism there is "no essential difference between the process of discovery and that of justification." (Ibid., p. 316f.) Such "dialogues underlay both the open-minded foundational research and the erection of the orthodox interpretation of quantum mechanics." (Ibid., p. 2)

Beller is aware of the danger that the dialogist attitude could lead into relativism. To prevent this, she heavily counts on institutionalized rules of dialogical conduct. "What became distinctive of modern science was not so much the scientific 'method', or scientific 'norms', but the strongly institutionalized communication scheme" (Ibid., p. 312), above all the Royal Society. Now if this is all to distinguish science among other activities – presumably apart from empirical adequacy – relativism or at least elitism are lurking because even outdated theories could successfully win before the Academic High Court. Beller's gentlemanlike rules of dialogical conduct are surprisingly lax. "Scientists appear to be too opportunistic; they often betray their 'beliefs' and 'commitments'." (Ibid., p. 311) "It was on the efficiency of the mathematical tools, and not on the metaphysical 'paradigmatic' issues that there was agreement in the community of quantum physicists … agreement on the potency of these tools prevented scientific practice from disintegrating, be the philosophical disagreements as large as they may." (Ibid., p. 4) But, "outside a dialogical context formulas are mute." (Ibid., p. 104) I doubt whether institutions so created suffice to keep the scientific enterprise together.

Within Beller's dialogist history, there is no such thing as the quantum revolution. She intends a "general critique of the revolutionary narratives, … an analysis of how revolutionary stories in history of science are constructed, how division between 'winners' and 'losers' is fabricated, how the opposition is misrepresented and delegitimized." (Ibid., p. xiii) The target is of course Thomas S. Kuhn who "incorporated the Copenhagen ideology into an overarching theory of the growth of scientific knowledge." (Ibid., p. 13) But the "communicative, interactional nature of scientific creativity is as alien to the revolutionary as to the revolutionary narrative." (Ibid., p. 269) Moreover,

> incommensurability excludes the possibility of being suspended between two different, incompatible worlds, of creatively participating in both, of sustaining for long a creative tension between the old and the new. Such work is possible only during a short period of crisis, disarray, inconsistency. Incommensurability logically dictates total unquestioning, dogmatic commitment. (Ibid., p. 292)

Interestingly, Beller claims that "close historical links exist between the notion of incommensurable paradigms and the ideology of the Copenhagen dogma." (Ibid., p. 287) It was Norwood Hanson (1958) who imported Heisenberg's notion of a closed theory into philosophy in order to defend the Copenhagen interpretation by developing "incommensurability as linguistic untranslatability." (Beller, 1999, p. 296) Subsequently, so Beller holds, incommensurability "was swiftly, perhaps hastily, superimposed on Kuhn's emerging *Structure,* most likely as a result of an encounter with Hanson's work." (Ibid., p. 301) By citing a favorable review of Bohm by Feyerabend (1960), on the other hand, Beller "disclose[s] the importance of Bohm's alternative to the Copenhagen interpretation in the emergence of a post-positivist philosophy of science." (Ibid., p. 287) Bohm's plea against dogmatism thus explains the "creativity and longevity of 'normal science'". (Ibid., p. 306)

I wonder whether this parallel represents anything but rhetorical consolidation. But my point here is the identification of the Copenhagen paradigm with a closed theory in Heisenberg's sense by which Hanson's book was motivated. According to Beller the upshot is that "[u]nder the Copenhagen interpretation, quantum mechanics can neither be modified by small changes nor supplemented by hidden variables." (Ibid., p. 289) This conclusion needs substantial qualification.

Heisenberg (1947) gives four criteria for a theory to be closed. First, "the concepts stemming from experience must be made precise by definitions and axioms, and their relations fixed, so that it is possible to coordinate to these concepts mathematical symbols among which a consistent system of equations exists." (1947, p. 334) This is just a standard fact of axiomatized theory in the style perfected by the Hilbert school, which since the 1920s could be found in any text written by Logical Empiricists. Second, "the concepts of this theory must be anchored directly in experience, the must 'denote' something in the world of phenomena." (Ibid., p, 334) Standard wisdom again; axiom systems may contain unintended models that are unphysical. Third, the limits of the domain of applicability of the axiomatized theory, that is the phenomena adequately described by it, are not given a priori but they are up to experience. The ideal is that the axiom system permits one to derive uniquely all relevant laws of the respective field; the axiom system is then complete and categoric..[42] Hilbert's (1900) axiomatization program took the theoretical frameworks as they were presented by theoretical physicists, so that Heisenberg's point ultimately concerns the relation between a theory formulated with mathematical precision and empirical phenomena. As I have argued elsewhere (Stöltzner 2001a), von Neumann's *Mathematical Foundations of Quantum Mechanics* (1932) was the most perfect realization of Hilbert's program of the axiomatization of physics, and Heisenberg was so tightly connected to Göttingen to know this for sure. Thus although von Neumann's name does not appear in Heisenberg's short paper, one can safely conclude that von Neumann's book stood at the back of the first three criteria of the notion of closed theory.

Only the fourth criterion moves away from axiomatics proper and steers towards Bohr's thesis of the indispensability of classical concepts.

> Even when one has moved beyond the boundaries of the "closed theory", when accordingly new domains of experience have been ordered with new concepts, the conceptual system of the closed theory nonetheless represents an indispensible part of the language in which we talk about nature. The closed theory belongs to the preconditions of further research; we can express the result of an experiment only in terms of earlier closed theories. (Ibid., p. 335)

The last sentence indicates that Heisenberg took closed theories as a sort of historically relativized a priori. Rejecting a biologically conditioned absolute a priori, Heisenberg introduced a hierarchy according to which concepts like space, time, and causality were a priori to a higher degree than the closed forms of more recent theories.

> The development [of physics since the Middle Ages] appears to us as a sequence of intellectual structures, "closed theories", which form out of single empirical questions as a seed crystal and which eventually, when the full crystal has formed, detach from experience a purely intellectual structures [geistige Gebilde]. (Ibid., p. 336)

It appears to me that here Heisenberg simply intended to locate quantum physics within the German philosophical tradition. And the solution he took was quite a familiar one form relativity theory. In 1920 Reichenbach had tried to devise the notion of a relativized a priori, but he left this position after a correspondence with Schlick who argued that relativization of any sort violated the very intentions of Kant's concept of a category. (See Friedman, 1994; and above) A relativized a priori is not simply self-contradictory, as Beller (1999, p. 199) believes; there existed various neo-Kantian relativizations at that time. Yet a mitigated transcendental-philosophical background of this kind is not required for assessing the axiomatic approach, for contemplating irreducible indeterminism, or the rejection of a particular alternative interpretation. This should be one of the lessons of the present book.
Suffice it to note at this place one important point about possible modifications of axiom systems. When Heisenberg at another occasion wrote that in closed theories "[t]he connection between the different concepts in the system is so close that one could generally not change any one of the concepts without destroying the whole system," (Beller, 1999, p. 288) this misses an important aspect of the axiomatic

[42] This notion of completeness has to be distinguished from the syntactic one figuring in Gödel's incompleteness theorems which – in Tarski's version – denotes the fact whether all formulas true in a system are provable within it. One might well ask how later developed theories of measurement relate to Gödel's notion of completeness (Breuer, 1997).

method. There are axiom systems which remain consistent even if one modifies one particular axiom that is independent of all others. Non-Euclidean and non-Archimedean geometries can be obtained in this way and at least the former has found prominent applications in general relativity. The same is true for von Neumann's axiomatization of quantum mechanics. One of the axioms used in the No-hidden-variable theorem was too restrictive – even physically counterintuitive. (See Stöltzner 2002c) Contrary to von Neumann's belief, a modification at this point makes possible to obtain empirically equivalent formulations of quantum mechanics, among them Bohm's theory. When Beller concludes that "[w]hich of the assumptions one in fact discards depends on the local, theoretical, and sociopolitical circumstances" (Beller, 1999, p. 304) rather than upon deeper philosophical commitments, this plays down the important insight reached in Bell's (1966) criticism of von Neumann's proof.

Heisenberg is not the only quantum physicist in whom Beller detects aprioristic convictions. Also Bohr's "attempts to arrive at true, certain, final knowledge by the mere analysis of the conditions of experience" (Ibid., p. 205) revived Kant's attempt to win Newtonian mechanics the status of a priori knowledge. Where has the Copenhagen positivism gone which, to Bohm and Cushing's mind, stood in the back of the notorious finality claims? Reverting the direction of influence, Beller holds that "it was the need, or the desire, to argue for finality against threats from the opposition that led Heisenberg and Bohr to take an forceful operational stand." (Ibid., p. 203) Among quantum physicists, positivism was initially a concept "in flux" which only later became canonized for rhetorical purposes.

"A strong belief in, or commitment to, any metaphysical presupposition acts too much like a straitjacket in a creative, conceptually fluid phase of scientific activity" (Ibid., p. 214) in which the actors are driven by a network of dialogues, opportunist motives and the power of formal tools.

> The point is not that philosophy cannot influence science. Creative scientists might adopt a certain foundational stand (sometimes indistinguishable from a traditional philosophical stand) in order to pursue a definite line of research. Such a philosophical orientation is, however, local and provisional. The longevity of philosophical "commitment" is conterminous with its usefulness in solving the problem at hand. (Ibid., p. 58)

Since philosophy has only limited influence on scientific practice, "scientists may give all authority in interpretative matters to a few leaders, whose philosophy they are willing to accept." (Ibid., p. 4) Thus, philosophical influences exist: "the idealist German philosopher Fichte might have been a surprising source of Heisenberg's idea of the reduction of a wave packet." (Ibid., p. 58)[43] But even when in the phase of rhetorical consolidation authoritative scientists refer to such a background, one cannot and one should not measure them by professional standards. Beller calls it a myth "that philosophical writings necessarily deal with 'eternal' epistemological or ontological issues, such as the 'realism' of atoms … [and] that philosophical writings are in principle intended to produce a systematic contribution to their subject." (Ibid., p. 173) This weak requirement seems to me at odds with Heisenberg's philosophical ambitions (See below).

[43] Weyl seems to be a particular well-suited case in point here because his philosophical interests changed as did his scientific orientation. (See Scholz 2001).

More generally, the concept of philosophy used by Beller is too restrictive to assess the conceptual developments within the quantum generation. It does not suffice to count only references to and influences from academic or classical philosophy – or what Frank had called 'school philosophy' – because that particular epoch witnessed the emergence of a scientific philosophy that rejected autonomous philosophical objects and an independent philosophical method but announced an analysis of science which availed itself of scientific methods. Even though Logical Empiricism was not dominant at the time, it stood in the tradition of the German scientist-philosopher who crossed the boundary between the respective discourses. Classical examples of relevance in the present context were Hermann von Helmholtz, Johannes von Kries, Gustav Theodor Fechner, Ernst Mach, and Max Planck. Philosophical commitments in this tradition were close enough to the scientific development to be modified if deep changes – not necessarily revolutions – so required and they were in turn capable to influence scientific practice. This tendency was reinforced by the fact that trained scientists, such as Schlick, moved into professional philosophy but continued to publish in media of the scientific community, such as *Die Naturwissenschaften,* and consequently were read by and corresponded with scientists; compare the many letters Schlick received by top scientists reacting on his second theory of causality (Section 7.4.).

This brings us eventually back to the Forman thesis. To my mind, Beller willy-nilly introduces a substantial milieu-dependence of science just because she rejects the stabilizing force of philosophical commitments which are independent of rhetorical goals and not backed by authorities.

> The variety of audiences to which orthodox quantum physicists addressed their statements was a major source of contradictory elements in their writings. Heisenberg adopted Bohr's positivist approach for mathematically unsophisticated audiences, yet he employed elements of a realistic ontological interpretation when addressing his mathematically skilled colleagues. (Ibid., p. 172)

Incoherent as they were, within the range of his philosophical convictions, Heisenberg's orientation was strongly influenced by the respective audience. This was precisely Forman's starting point, and he investigated academic addresses to prove that the influence was even causal. Moreover, "[i]n dialogical accounts that acknowledge the essential formative role of scientific controversies, the line between the "cognitive" and the "social" becomes blurred." (Ibid., p. 144) Accepting dialogism seems to entail that the Weimar scientist either acted by retrenchment, in case they found enough addressees within a sufficiently rich scientific community, or had to interact directly with the general cultural milieu. However scientists reacted, the influence of the milieu could not reach the formalism because, as did Cushing, Beller separates the formalism – here scientists act primarily as opportunists – and the rhetorically conditioned interpretation. So it seems that the weak Forman thesis could get well along with Beller's account. This however is not her intention.

> In their recollections, the founders of quantum mechanics described their efforts to construct the new quantum mechanics as guided by a belief in indeterminism. Historians and philosophers of science often follow this lead, seeking the sources of such beliefs in the cultural milieu (Jammer 1966; Forman

1971). Yet we find no strong opinions expressed on the issue of causality during the creative stages of the erection of the new theory. (Beller, 1999, p. 26)[44]

The same applies with respect to Forman's (1984) extension. Admittedly, there was "social pressure (for *Anschaulichkeit*)." (Beller, 1999, p. 109) "Yet Heisenberg showed little desire to tackle the issue of *Anschaulichkeit*, until subjected to concrete scientific pressure – by Schrödinger." (Ibid., p. 70) Moreover, "description of acausality in quantum mechanics as an expression of the zeitgeist (Forman 1971) … provides only a limited perspective. The notion of zeitgeist is itself a monological notion." (Beller, 1999, p. 313) Beller's decisive argument against Forman, however, is that there simply was no genuine indeterminism before the advent of quantum mechanics.

I know of no quantum physicist before 1927 who did commit himself to indeterminism. Physicists at the time thought statistically *along classical lines* (the uniqueness of quantum probabilities was recognized only after Born's interpretation). In classical statistical theory one starts with an assumption about equally probable cases (elementary probabilities) and derives from there more complicated probabilities … . In quantum theory, however, complex probabilities (such as transition probabilities) were introduced a priori, without being reduced to elementary probabilities. As long as no consistent theory of probabilities was developed within quantum theory, there could be no verdict over indeterminism. For Born, Jordan, and Pauli, this reasoning was an undercurrent of their struggles with the issue of indeterminism. (Ibid., p. 61f.)

This assessment is flawed in various respects. First, Schrödinger constantly emphasized his adherence to indeterminism (1922a, 1924, 1929). Second, by 1927 even classical statistical physicists did no longer talk much about classical equiprobabilities. To be sure, the second edition of von Kries' book (1886) appeared precisely in this year and the concept of range (*Spielraum*) signified less radical a departure from equiprobabilities than the relative frequency interpretation; but only few physicist remained committed to this interpretation. Third, the frequency interpretation had become widely acknowledged, and since Exner (1909) it was – together with a firm empiricist stand – the main argument in favor of indeterminism. Although the relative frequency interpretation did not solve all problems with probability in quantum mechanics, still in the mid 1930s it was strong enough to make von Neumann leave his brainchild Hilbert spaces (See Rédei, 1996). All this were interesting philosophical problems which will return again and again in the present book.

Without admitting the gradual and sometimes monological development of philosophical positions, Beller can only wonder why Heisenberg introduced a discussion about acausality into the uncertainty paper (1927) although the motive was absent from the previous correspondence with Pauli.

We can only speculate about what caused Heisenberg to turn to this issue… Did Heisenberg read, hear, or see something of the acausal spirit prevalent in his cultural milieu? We probably will never know the answer. Yet realizing how contingent the introduction of the acausality issue into the uncertainty paper was, and how closely tied to the original content of the paper it later became, is very instructive. (Beller, 1999, p. 111)

[44] Notice that Beller identifies causality with determinism (Cf. ibid, p. 59).

But at least the ambitious Heisenberg got his call to a great philosophical problem just at the right dialogical moment in time.

> Strong sentiments against causality quickly arose precisely because causality was an intrinsic part of the threat from the opposition. … The interpretative attempts of quantum theorists took place against a background of philosophical controversies about the changed status of space and time concepts implied by Einstein's relativity. Young Heisenberg witnessed the emotional and politically charged confrontation between Einstein and the neo-Kantians. … [Being an] intensely ambitious man … Heisenberg wanted to be the new Kant [his teacher Sommerfeld had called for] – in his initial presentations of the uncertainty principle to academic audiences, he always described the abandonment of the "Kantian category of causality" as a natural continuation of Einstein's overthrow of Kantian space and time as forms of intuition. (Ibid., p. 195)

By that time, however, this honor was no more available. Long time ago Vienna Indeterminists had executed Mach's criticism of the Kantian categories without distinguishing treatment of space-time and causality. In an article for *Die Naturwissenschaften* surveying the development of quantum mechanics from 1918 to 1928, Heisenberg wrote alluding to the issue of causality:

> As with relativity theory, the physical understanding of quantum theory was accordingly only possible on the basis of a revision and extension of the world of classical concepts, that is, on the basis of a careful epistemological investigation of the concepts to be introduced in the theory. (Heisenberg, 1929, p. 495)

Summing up, Heisenberg's position about causality agreed with Logical Empiricists while his concept of closed theory introduced a relativized form of synthetic a priori which Logical Empiricists should have emphatically objected to. Surprisingly, this difference did not appear during the discussion following Heisenberg's (1931) talk at the Königsberg meeting for "Epistemology of the Exact Sciences." But Logical Empiricists were usually reluctant to criticize leading scientists in public; they rather attempted to include them into their strategic alliances to combat metaphysical misinterpretations of modern science – even if such misinterpretations derived support from certain enunciations of these scientists themselves.

What can we ultimately make of the dialogist approach? Dropping the problematic indispensability of a dialogue partner or social context of addressees for any new claim advanced by a scientist, dialogism becomes close to truism. However, because of its narrow focus the dialogist analysis, on some instances, becomes close to watching Brownian particles under the microscope. One can hardly see how long-term commitments unfold and stabilize; to continue the picture, Beller could just notice that a sort of phase transition occurred circa 1927 and had to chance the temporal magnification of dialogical analysis. In both cases neither philosophical convictions nor Lakatosian research programs could be discriminated because, to my mind, they require a larger timescale to unfold. On the microscopic scale it is impossible from the very beginning to assess the commitments of the German scientist-philosophers and to verify whether philosophical positions played a role in their concept of science. This is a pity because, to my mind, they acted much more in a dialogical fashion that did the classical German philosophical schools, above all neo-Kantianism which by and large identified themselves by self-declared adherence to a certain philosophical school. But my main point here is that philosophical traditions if not committed to schools emerge from dialogues with opponents and they involve precisely that kind of combination of outside self-identification and substantial internal disagreements which Beller found so characteristic of the Göttingen-Copenhagen dogma. In particular the Vienna Circle combined manifestos, a large series of historical self-identifications, the foundation of various projects and institutions which sought to integrate wider circle – from the Ernst Mach Society up to the International Congresses for the Unity of Science, the Encyclopedia Project, and the various organizations in exile.

During the last two decades, detailed historiographical investigations into Logical Empiricism have uncovered a multitude of diverging voices, unraveled many internal strands, and found basic concepts in flux. Examples of explicit dialogues abound, among them the discussion with Wittgenstein that lead to a major division of the Vienna Circle after 1930 (Cf. Bergmann, 1993), and the protocol sentence debate between Carnap and Neurath (Uebel, 1992).[45] This diversity stood in strange contrast to the party-like unity exhibited in the manifesto (Hahn, Neurath, Carnap, 1929) and Neurath's historiographies of the movement. They felt themselves immersed in a hostile milieu dominated by 'school philosophy'. Due to similar historical scrutiny we have meanwhile learned that the Copenhagen interpretation was of the same kind.

In its second phase (Chapters 7 & 8) the causality debate represented such an internal development. Without hiding their disagreements, in 1936 Schlick and Frank nevertheless sought a united stand against misinterpretations of Bohr's concept of complementarity. (See Section 8.7.) Dialogues in these cases were more implicit than outspoken. But there were also open polemics, departing from the one between Planck and Mach (Section. 3.7.) In a very general sense, so I would claim, the entire philosophical debate about genuine indeterminism developed as a sequence of dialogues between Vienna and Berlin that extended over roughly two decades. The main dialogues will be Planck's encounters with Mach, Exner and Schrödinger, Schlick's and Frank's diverging assessment of Mach, the mutually supportive dialogue between Frank and von Mises, Schlick's dialogues with his teacher Planck, Schlick's correspondence with Schrödinger, a group of less prominent encounters between Schottky, Nernst, and Petzoldt and between von Laue, Schrödinger, and von Mises. Important differences go back to distinct roots, most important the Machian and the neo-Kantian heritage which set the initial frontlines in the dialogue about Boltzmann's legacy. Against the alternative between rational legitimation and historical contingency, I shall emphasize the stabilizing factor of honest – that is: not rhetoric-driven – philosophical convictions and spot a forum in which many dialogues took place.

[45] For a comprehensive account see the books of (Haller, 1993) and (Stadler, 2001).

3. The First Phase: Mach, Boltzmann, Planck

In an interview with Thomas S. Kuhn, Philipp Frank in 1962 recalled his early years at the Institute of Physics of the University of Vienna where he had studied under Boltzmann, became a *Privatdozent* in 1909, and taught until his call to Prague in 1912.

Boltzmann once said to me: "You see, it doesn't make any difference to me if I say all atoms are merely pictures. I don't mind this. I don't require that they be absolute. I don't say this. 'An economical description', Mach said. Maybe the atoms are an economic description. This doesn't hurt me so much. From the viewpoint of the physicist this doesn't make a difference." Ludwig Boltzmann was rather philosophical about it. He did not require that you believe in the existence of atoms. And there wasn't I would say any opposition from Mach's viewpoint. The opposition only existed, so to say in the philosophical realm. Yes, it did exist there. Also, strange as it was, in Vienna the physicists were all followers of Mach *and* Boltzmann. It wasn't the case that people would hold any antipathy against Boltzmann's theory because of Mach. And I don't even think that Mach had any antipathy. At least it did not play as important a role as is often thought. I was always interested in the problem, but it never occurred to me that because of the theories of Mach one shouldn't pursue the theories of Boltzmann. (quoted from Blackmore/Itagaki/Tanaka, 2001, p. 63)

In a letter to Arthur Eddington written in 1940, Schrödinger gave a similar testimony. Schrödinger who was three years younger than Frank had just begun his studies by the time of Boltzmann's death and he stayed at the Institute of Physics as an assistant of Franz Serafin Exner until 1920.

Filled with a great admiration of the candid and incorruptible struggle for truth in both of them, we did not consider them irreconcilable. Boltzmann's ideal consisted in forming absolutely clear, almost naively clear and detailed 'pictures' – mainly in order to be quite sure of avoiding contradictory assumptions. Mach's ideal was the cautious synthesis of observational facts that can, if desired, be traced back till the plain, crude sensual perception. … However, we decided for ourselves that these were just different methods of attack, and that one was quite permitted to follow one or the other provided one did not lose sight of the important principles … of the other one. (quoted from Moore, 1989, 41)

Quite contrary to this local Viennese synthesis, most German physicists shared Arnold Sommerfeld's 1944 summary of the frontlines on the legendary 1895 Lübeck *Naturforscherversammlung.*

Helm from Dresden gave the report on energetics; behind him stood Wilhelm Ostwald and behind both stood the *Naturphilosophie* of Ernst Mach, who was not present. The opponent was Boltzmann, seconded by Felix Klein. The conflict between Boltzmann and Ostwald resembled, both externally and internally, the fight between and bull and a subtle fencer. But on this occasion, despite all his swordsmanship, the toreador was defeated by the bull. (quoted from Deltete, 1999, p. 56)

Max Planck, who would become the explicit and implicit dialogical counterpart of the tradition of Vienna Indeterminism, by then entertained a autonomous position emphasizing that both the first and the second law of thermodynamics were independent principles which were not reducible to molecular motions. Although his discovery of the law of radiation in 1900 chiefly contributed in turning the tide in favor of Boltzmann, Planck needed considerable time and intermediate steps to fully reconcile himself with the probabilistic nature of the second law. Ultimately, he would do so only after the dialogue with Exner to be studied in Section 4.5., during which he still considered molecular disorder as a principle supplementing Boltzmann's second law.[46] Contrary to Exner, Planck continued to emphasize that any probabilistic law requires a determinist foundation.

The first half of the present chapter (Sect. 3.1-3.5) argues that Frank's and Schrödinger's accounts of the Viennese tradition are basically right. There existed important philosophical continuities between Mach and Boltzmann which escaped Planck's attention, presumably because Boltzmann's philosophy sharpened and developed only after the publication of the *Lectures on Gas Theory* (1896, 1898a). Philosophical issues became central to Boltzmann after 1903, when he took over Mach's courses on natural philosophy. Mach had ceased to lecture after his stroke in 1898 and took early retirement in 1901. But Boltzmann's philosophical activities only led to four publications which were assembled together with earlier papers in the *Populäre Schriften* published in 1905. The effect of Boltzmann's philosophical ideas was thus confined to the closer Viennese context, among them the Exner Circle (See Chapter 4), the Philosophical Society of the University of Vienna, and those young scientists forming the "First Vienna Circle".

Here are Frank's recollections of Boltzmann's philosophy lectures; they appear in an article investigating "The place of philosophy of science in the curriculum of the physics student". They show an important element by which scientific philosophers wanted to go beyond the classical scientist-philosopher.

> Scientists who have been teaching the philosophy of science have mostly offered a kind of incoherent digest of philosophic opinions. … We meet the eclectic attitude even in the writings of such excellent scientists as Jeans of Planck. I remember the lectures of a great physicist, Boltzmann, on the philosophy of physics which I attended as a student. Despite the personal greatness of the lecturer, the effect of the course was slight, because of a lack of coherent approach. We can notice, on the other hand, that scientists who built their books around a central idea have shaped the minds of science students for decades. I mention, just as examples, Mach, Poincaré, and Bridgman. (Frank, 1961, p. 244).

Today's scholars can access Boltzmann's notebooks for the philosophy lectures (Fasol-Boltzmann, 1990) and his correspondence (Höflechner, 1994).

John Blackmore, who seems to be somewhat perplexed about Frank's testimony that the Viennese physicists were followers of both Mach and Boltzmann[47], has recently diagnosed a substantial shift in Boltzmann's ontological attitude in favor of critical realism – by which Blackmore means the position of Arthur Lovejoy – after the meetings with Franz Brentano in April 1905.

> One can see the apparent changes … by comparing his December 1904 criticism of Brentano's philosophy with his later May to June criticisms. In the earlier period, he attacked concepts and the so-called a priori as mere words and above all dismissed the value of philosophy itself. But by May and

[46] Cf. (Kuhn 1987) for Planck's piecemeal conversion to Boltzmann. Apart from the *Preisschrift,* Planck wrote on philosophical matters only after 1908. His own interpretation of the 1900 discovery, to wit, that he had obtained a new constant of nature, did not significantly change thereafter. Hence the most contested point of Kuhn's book, whether Planck actually thought about a quantum theory from the very beginning, is irrelevant for the scope of the present investigation.

[47] (Blackmore 1995a, 133 note 18) asserts "that some caution is in order" because Frank was almost 80 years old at the time of the interview.

June he had not only changed most of his targets of criticism but begun to classify concepts, develop his own a priori theory, and above all his new classification of world views. (Blackmore, 1995b, p. 171)

To my mind, Blackmore's investigations remain inconclusive simply due to the sparse and often cryptic material in the lecture notes. A very late change of mind is of course possible, but I am skeptical especially because Boltzmann had already adapted Mach's epistemology to a justification of his atomism. In a realist setting, Boltzmann's two-tired reality criterion (See Sections 3.4. & 3.5.) would make no sense at all.

This Chapter is arranged as follows. Section 3.1 introduces the basic tenets of Mach's philosophy of science. Sections 3.2 – 3.4 assess how Boltzmann's thinking developed against and departed from the Machian background. Their writings embarked onto a dialogue that also involved divergent interpretations of third parties, among them Ostwald and Petzoldt. Section 3.5 investigates Boltzmann's mathematical atomism which was the basis of the irreducible indeterminism characteristic of his late philosophy.

Max Planck twice stood in dialogues with Mach, an inner one and a fierce polemic. First, Planck's allegedly Machian *Preisschrift* on *The Principle of Conservation of Energy* (Planck, 1908b) contained manifold references to Mach's earlier study (Mach, [1872] 1909) and was indebted to his historico-critical method (Section 3.6). Second, the open polemics with Mach (Section 3.7) set the stage for Planck's later encounters with the Vienna Indeterminists and they remained a point of reference ever since. Planck explicitly charged Mach's positivism as irreconcilable with the progress achieved by Boltzmann's treatment of the second law of thermodynamics. In Section 3.8, I shall deal in more detail with the core tenets of Planck's principle-based methodology and his combination of convergent and structural realism. The final Section 3.9. is dedicated to the consequences those diverging views had on the three protagonists' understanding of culture. I shall remain very brief here because the objective of this section it only to demonstrate that all three actively participated in the ideological and cultural debates of the day; in this respect there will be a solid tradition among the Vienna Indeterminists.

Boltzmann's main motivation for embarking onto philosophy was that statistical mechanics unearthed foundational problems of that kind which typically challenged a scientist-philosopher. Energeticists' and Mach's philosophical criticisms forced Boltzmann further into this terrain. Most important was the criticism which Mach's *Theory of Heat* had leveled against Boltzmann in 1896. Boltzmann's rejoinder in the following year asserted that Mach's "writings on these matters have substantially contributed to the clarification of my own world view." (Boltzmann, 1905, p. 142/51) The principal thesis of the first half of the present chapter supports this allusion. In Boltzmann's hands, Mach's views about scientific method proved a surprisingly useful tool in defending his brainchild statistical mechanics. Above all, Boltzmann supplemented Mach's approach with a modern conception of physical theory, but he remained committed to the empiricist basis and to Mach's notion of causality. And indeed the Machian tools were sharp enough to spot the deep ontological cleft between Boltzmann's main opponents, the energeticists Ostwald and Helm, and Mach himself, a cleft which escaped the attention of most German physicists. Boltzmann's transformation of Mach, it appears, paved the way for Franz

Serafin Exner's synthesis of radical empiricism and indeterminism which became the local philosophical credo among the Vienna physicists.

Does all this mean that the philosopher Boltzmann was just the rhetorical consolidation of the atomist Boltzmann? After all, statistical mechanics was a rather clumsy theory the foundations of which were buried in Boltzmann's writings; the situation only changed with the encyclopedia article of the Ehrenfests ([1912], 1990). I do not think that Boltzmann's philosophy is exhaustively appraised by such a dialogical reading. Admittedly, Boltzmann was not a systematic philosopher, he followed no coherent approach and often developed his own views in correspondence or by criticizing other philosophers. Nevertheless, the most important themes of his philosophy remain constant and his views do not oscillate depending upon the addressee. Also Mach's philosophy, apart from the clear-cut general thrust, contained plenty of rough edges, in particular when later editions of Mach's works assented to new results which were hardly reconcilable with the main message of the book. Thus I think Boltzmann is a reasonable scientist-philosopher. As we shall see below, some philosophical views of his, like the concept of reduction, have a rather modern appeal.

Mach's rank as a philosopher – although he always rejected this classification – is beyond doubt. Nevertheless, he is rarely read and his prestige among later colleagues strongly depended on the author's intention. Views range from presenting the historico-critical analysis as an indication that Logical Empiricists in their European period did not disregard history at all, to equating Mach's neutral monism with what is commonly believed to be Berkeley's radical idealism; on the latter reading, Mach dissolved the world into a flood of directly observable elements – a brand of sensualism which can hardly form a basis of modern science. Mach's philosophical legacy within physics was both praised and contested already during the last years of his life. His historico-critical investigation of the development of mechanics, first published in 1883, earned him great recognition and – other than his first pronouncement of these ideas eleven years before (Mach, 1909) – gained substantial influence on physics proper when Einstein took Mach's criticism of the basic concepts of mechanics as the starting point of his special theory of relativity. Thus far the merit of Mach's *Mechanics* remained unchallenged until Planck's polemics against Mach disputed the value of the whole approach for physical science. The polemics prompted many German physicists to take Planck's side, in particular, when the number of criticisms against relativity theory by various Machians increased. Comments about Mach subsequently either described him as a forefather of relativity theory or emphasized that he had achieved no more than the negative part unraveling the implicit presuppositions of the Newtonian concepts of absolute space and time. It was the polemics with Mach that made Planck a widely recognized scientist-philosopher, and in the years following 1910 he played this part to an increasing extent. For instance, both contributions to the physics volume of the encyclopedia *Die Kultur der Gegenwart* (1915a, 1915b) were of a rather philosophical kind. In later years, when Planck had become the true successor of Helmholtz as the "Reichskanzler" of German physics (Cf. Heilbron, 2000), he took the increasing number of public addresses as an occasion to intervene into ethical and religious discourses as well.

The basic philosophical set-up for the struggles about Boltzmann's legacy statistical mechanics between Vienna and Berlin contains three parts. (i) The starting point are the highly *improbable events* that were admitted by Boltzmann's statistical derivation of the second law of thermodynamics. (ii) On the basis of a Kantian or neo-Kantian epistemology causality is intimately linked to empirical realism because to qualify as empirically real some thing had to fall under the category of causality however relativized. When Mach replaced causality by functional dependences this bond was severed, such that – in compensation – an new reality criterion had to be sought. More generally this point can be considered as a radically empiricist conception of natural law which shifted the burden of proof on the determinist. (iii) There existed two theories of probability which accommodated the strange events admitted by the second law. In von Kries's *Spielraumtheorie* they were integrated into a deterministic Kantian universe. In Fechner's *Kollektivmaßlehre* there existed collective objects (*Kollektivgegenstände*) and genuinely statistical laws for them; they are of no other type than the familiar – apparently deterministic laws. In brief, Vienna Indeterminism is characterized by the full acceptance of the improbable events, a radically empiricist conception of natural law and ontology, and the frequentist interpretation of probability. In this full-blown version, Vienna Indeterminism began with Exner and was advocated by Schrödinger (Chapter 6), and Frank and von Mises (Chapter 8). But we have to bear in mind that, as outlined in the Introduction, one cannot treat this tradition simply as a philosophical school endorsing theses (i)-(iii). Physics and the philosophy closely linked to it were too much in flux, such that a more historical approach is in order that accommodates a variety of intermediate positions as long as the tradition in itself remains clearly visible.

In this perspective, the main step launching the tradition of Vienna Indeterminism was Mach's separation between causality and realism (ii). The empiricist notion of causality as such can well be prolonged back to Fechner[48] and Hume. After long and painstaking reflections, however, Mach severed the bond with Fechner's realism that had been expressed in the idea of a gravitating planetary atom and atomistic conception of electricity. "Mach accordingly has played off the psychophysicist and anti-metaphysicist Fechner against the atomist and metaphysicist." (Heidelberger, 1993, p. 213)[49] Consequently Mach did not adopt Fechner's irreducible indeterminism and the frequentist interpretation of probability that stood at its back. Mach considered determinism as empirically unprovable, but still an unavoidable regulative principle. Regarding the highly improbable events, he remained neutral because atomism was just a – somewhat problematic – hypothesis to explain our familiar sensations of temperature in which the strange events of (i) have not yet be seen. Boltzmann, however, utilized Mach's radical empiricism as the epistemological basis to win statistical laws an equal status as compared to the other laws of physics. Although his views on probability developed after he had, in passing, endorsed von Kries's interpretation, he never arrived at the frequency interpretation.

Above and beyond his adaptationist conception of experience and knowledge, Mach's own – apparently modest – reality criterion consisted in the uniqueness and approximate stability of facts. While for Mach theories were in first place an

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[48] Heidelberger (1993, p. 380), for instance, finds this motive in von Mises (1921, 1930); cf. Sect. 8.2. & 8.3.

[49] There is also indirect evidence that Mach's concept of causality paved the way for indeterminism. Karl Pearson to whom *The Analysis of Sensations* was dedicated based his *Grammar of Science* on the Machian methodological program of careful classification of facts and comparison of their mutual relationships and sequences (Cf. Porter, 1994, p. 146f). Denying accordingly the integrity of scientific objects, the prospect that all science is fundamentally statistical becomes viable.

economically conceptualized and structured inventory of facts, Boltzmann insisted that theories must reach beyond the facts already known in order to be testable. Conceived ontologically as intuitive and purposive (*zweckmäßig*) pictures, theories gained a certain independence, such that Boltzmann could require of them mathematical precision, uniqueness, simplicity and pervasiveness of the underlying mechanisms without immediately caring whether they were an economical representation of a given fact. But mechanistic or atomistic reduction only amounts to a reality criterion if an ontology for these basic entities is specified. Rejecting, as Mach, materialist mechanicism, Boltzmann turned to a constructivist account of mathematics that, surprisingly, recurred to Mach's basing mathematics on empirical acts of counting. Boltzmann's indeterminism still suffered from an insufficient theory of probability, since earlier or later he had to invoke a priori assumptions to justify probability distributions for the atoms within the classical setting.

Planck, initially rejected all three points of Vienna Indeterminism; after 1914, however, he would resign himself to their presence in the physical world view. But he never abandoned the position that any probabilistic theory required a determinist foundation. One of the core advocates of relativity theory thus remained committed to the Kantian set-up as far as causality was concerned. This outlook concerning causality was combined with a strong emphasis on universal formal principles, such as the Principle of Least Action. They formed the basis of Planck's reality criterion which can be seen as a combination between structural realism as regards the formal principles and convergent realism as regards the constants of nature and core objects instantiating the principles, such as the metric of space-time. Finally, to Planck the distinction between reversible and irreversible processes assumed categorical status while for the empiricists Mach and Boltzmann it was just of a relative kind.

3.1. Mach on Economy, Monism, and Causality

In his 'Leading Thoughts', Mach condensed his biological-economical theory of knowledge into a single slogan: "*Adaptation of thoughts to facts and adaptation of facts to each other*." (Mach, 1910, p. 226/133f.) This adaptation starts from instinctive experiences that make us isolate those features of the world which are of relevance to our practical life and imitate them in thought in order to communicate them to others. "But, within the short span of a human life and with man's limited powers of memory, any stock of knowledge worthy of the name is unattainable except by the greatest economy of thoughts." (Mach, 1988, p. 501/586)

The principle of economy was one of the pillars of Mach's adaptationist epistemology. The many misunderstandings which one can find in the literature are not least a consequence of the principle's opaque and variegated character. Economy of thought emerges from three distinct roots: the practical economy of craftsmanship, the biological superiority of economically organized forms, and its didactic value. While these roots are of a descriptive nature, when it comes to scientific theories Mach emphasized the principle's methodological character calling it a "very clear logical ideal" (Ibid., p. 508/594). Yet economy of thought was merely a "teleological and provisional leitmotif," (Ibid., p. 508/594) or – in Kantian wording – a regulative principle. It was anything but a determinative principle – comparable to a Kantian category – because Mach constantly emphasized that "there is no result of science which in point of principle could not have been found wholly without method." (Ibid., p. 501/586) "Only when the essential point has been found, method can straighten up and finish off." (Mach, 1991, p. 319) This antimethodological tendency in Mach's epistemology escaped Planck's attention (Cf. Sect. 3.7).

Mach's adaptationist epistemology contained a stark asymmetry between experience and theories. Theories never exhaust the manifold of experience and constitute merely a most economical representation of scientific facts. This led to a rather queer terminology within Mach's three-stage account of the development of science. In a sheer inversion of common usage, *The Science of Mechanics* talked about the 'principle of the lever' (instead of 'law'), while one finds the 'theorem of least action' – to Helmholtz and Planck the 'principle' *par excellence*.[50]

> But even after we have deduced from the expression for the most elementary facts (the principles) the expression for more common and more complex facts (the theorems) and have intuited [*erschaut*] the same elements in all phenomena … [t]he deductive development of science is followed by its *formal* development. Here it is sought to put in an order easy to survey, or a *system*, the facts to be reproduced, such that each can be found and reproduced with the *least intellectual effort.* (Ibid., p. 444/516)

How does this intuition which "is the basis of all knowledge" (Mach, 1991, p. 315) work? Mach demonstrates how the law of inertia is intuited from the 'principle of the inclined plane'.

> Galileo runs his eye over several different uniformly *retarded* motions, and suddenly picks out from among them a uniform, infinitely continued motion, of so peculiar a character that if it occurred by itself alone it would certainly be regarded as something altogether different in kind. But a very minute variation of the inclination transforms this motion into a finite retarded motion, such as we have frequently met with in our lives. And now, no more difficulty is experienced in recognizing the identity between all obstacles to motion and retardation by gravity, wherewith the ideal type of uninfluenced, infinite, uniform motion is gained. (Mach, 1988, p. 296/335f.)

Mach's epistemology, accordingly, relies on a principle of continuity that backs this method of variation as the basic tool of the scientific enterprise (See below). Intuition marks both the beginning and the endpoint of a lengthy adaptive process. On the one end, "all so-called axioms are such instinctive experiences" (Mach, 1987, p. 221). The impossibility of a perpetuum mobile neatly corresponds to our everyday experiences. On the other end, Mach's historico-critical analyses – in particular the *Optics* (1921) and the *Mechanics* (1988) – broadly discussed various apparatus – historical and self-constructed ones – at which the student could intuit the fact in question by varying the determining circumstances. The correctness of the law or principle thus intuited "we not only *see* but *feel*". (Mach, 1988, p. 339/394)

Scientific knowledge is of humble beginnings. Once we have abstracted some sufficiently constant elements within our experiences and found some constant relations among them, we seek to find a measure and units to numerically tabulate these relations and to find a comprehensive rule generating these values. By

[50] Admittedly, Mach does not consequently stick to this terminology throughout the book and in *Knowledge and Error* (first published in 1905) he returns to the common use for the abstract 'principles'. The authorized English translation by McCormack uses the word 'principle' in all cases. For Helmholtz's influential view, see (Hecht, 1994).

distinguishing accidental and constant relations we are led to the concepts of *cause* and *effect*.

As soon as we can characterize the elements of events by means of measurable quantities, as is possible immediately for space and time and by detours for other elements of sense perceptions [e.g. heat], the mutual dependence of elements is much more completely and precisely represented by the concept of function than by those of cause and effect. (Mach, 1991, p. 278/205)

Close analysis almost always reveals that the so-called cause is only the complement of a whole complex of conditions [German: *Umstände*] that determine the so-called effect. (Ibid., p. 277/204)

The law of causality is sufficiently characterized by saying that it presupposes a dependence of the phenomena. … The law of causality is identical with the supposition that among the natural phenomena $\alpha\ \beta\ \chi\ \delta\ \ldots\ \omega$ there exist certain equations [of the form $f(\alpha\ \beta\ \chi\ \delta\ \ldots\ \omega)=0$ or an equivalent form]. … The existence of changes in nature demonstrates that the number of equations is lesser than the number of $\alpha\ \beta\ \chi\ \delta\ \ldots\ \omega$. (Mach, 1909, p. 35f.)

In speaking of cause and effect we arbitrarily give relief to those elements to whose connection we have to *attend* in the reproduction of a fact in which it is important to us. There is no cause nor effect in nature; nature has but an *individual* existence; nature simply *is.* [Die Natur ist nur *einmal* da.] Recurrences of like cases in which *A* is always connected with *B,* that is, like results under like circumstances, that is again, the essence of the connection of cause and effect, exist but in the abstraction which we perform for the purpose of mentally reproducing the fact. Let a fact become familiar, and we no longer require this putting into relief of its connecting marks, … and we cease to speak of cause and effect. (Mach, 1988, p. 496/580)

Thus for Mach, talk about cause and effect was just the initial stage in attaining scientific knowledge by mentally reproducing the facts in the most economical fashion. If we find measurable quantities to represent the elementary experiences, we can write down a table of simultaneously measured values. If there exists a functional dependence between these elements, this indicates a causal law. While in his 1872 booklet on *The History and Origin of the Theorem of the Conservation of Work* (1909) this concept of causality was limited to the domain of physics, in *The Analysis of Sensations* it became the basis of Mach's neutral monism. The first step to acquire causal laws is the analysis into elements.

[D]ifferent complexes are found to be made up of common elements. The visible, the audible, the tangible, are separated from bodies. The visible is analysed into colors and into form … [and further on into] the primary colors, and so forth. The complexes are disintegrated into elements, that is into their ultimate component parts, which we have been unable to subdivide any further. The nature of these elements need not be discussed at present; it is *possible* that future investigations may throw light on it. We need not here be disturbed by the fact that it is easier for the scientist to study relations of relations of these elements than the *direct* relations between them. (Mach, 1918, p. 4/5f.)

Machian elements are not ultimate, a priori indivisible and irreducible substances, but the particular elements of the analysis depend on the stage of scientific research. Ultimate elements would have plainly contradicted Mach's anti-atomism. Moreover, even in practical research it is not necessary to reduce scientific assertions to their elements; Mach's point was just that such a reduction remains possible in principle. Denoting by the letters *A B C* the physical elements, by the letters *K L M* the physiological elements of our body, and by the letters $\alpha\ \beta\ \chi$ the psychological elements, such as volitions or memory-images, we obtain the possibility to characterize various complexes through functional relations existing between elements.

Usually, now, the complex $\alpha\ \beta\ \chi$ ... $K\ L\ M$ …, as making up the ego, is opposed to the complex $A\ B\ C$ …, as making up the world of physical objects; sometimes also, $\alpha\ \beta\ \chi$... is viewed as ego, and $K\ L\ M$ ... $A\ B\ C$ ... as world of physical objects. Now, at first blush, $A\ B\ C$ appears independent of the ego, and opposed to it as a separate existence. But this independence is only relative, and gives way upon closer inspection. (Ibid., p. 7/9)

On this basis the contrast between 'reality' and 'appearance', between a stick put half into the water and its image, dissolves into simply two facts which represent "different combinations of the elements, combinations which are differently conditioned." (Ibid., p. 8/10) What is commonly called a body, or a thing, or the ego are just complexes of a sufficient permanency. "As soon as we have perceived that the supposed unities 'body' and 'ego' are only makeshifts, designed for provisional orientation and for definite practical ends … [t]he antithesis between ego and the world … vanishes." (Ibid., p. 11/14) "The ego is as little absolutely permanent as are bodies." (p. 3/4) Or more bluntly: "The ego is unsalvable [*Das Ich ist unrettbar*]." (Ibid., p. 20/24) "The primary fact is not the ego, but the elements (sensations). … The elements constitute the ego (I)." (Ibid., p. 21/23) The same holds for bodies. Again Mach did not intend an ontological reduction to sensations because the latter are neither atomistic nor subjective – in the sense of classical idealism. Functional dependences easily cross the woolly border line between the approximately stable complexes we experience as 'subject' and 'object'.

It is therefore important for us to recognize that in all questions in this connexion, which can be intelligibly asked and which can interest us, everything turns on taking into consideration different *basic variables* and different *relations of dependence.* That is the main point. Nothing will be changed in the actual facts of in the functional relations, whether we regard all the data as contents of consciousness, or as partially so, or as completely physical. The biological task of science is to provide the fully developed human individual with as perfect a means of orienting himself as possible. No other scientific ideal can be realized, and it is meaningless. (Ibid., p. 29f./36f.)

Let me recapitulate four points about Mach's neutral monism. First, the ontological basis of everyday experience and science are elements and the functional dependences among them. These elements are, however, not universal entities as Boltzmann's atoms but which elements we pick depends upon interest and purpose. One should not be misled by the phenomenalist suggestions carried by Mach's repeated identification of elements with suggestions. Mach's "sensations" are neither mental nor physical. Mach and later William James and Bertrand Russell believed that this neutrality was tenable, but many other philosophers did not believe so.[51] Second, certain complexes of functional dependences are sufficiently stable to count as a fact or an object which is constituted by facts and the perceptual relations. It may well be that in the course of time some elements of the same fact are replaced by others if we investigate the fact from a different angle, or guided by different interests. Facts can become rather large, in which case we obtain a whole complex of conditions. Third, there is nothing which escapes being part in functional dependences; there is no Fichtean pre-reflective ego. Fourth, the basis of Mach's neutral monism and of his whole epistemology is biological corroboration, aka survival. For this reason, instinct and naive realism of the

[51] Concerning the further development of this conception, see the entry "Neutral Monism" in the *Routledge Encyclopedia of Philosophy* penned by Nicholas Griffin.

man on the street are more effective, viz. adaptive, than philosophy. After Boltzmann, this strong biological orientation disappeared from Vienna Indeterminism and gave way to a sociological approach. (See Sect. 8.5.) Let me return to causality and determinism more specifically.

Mach's *concept of function* circumvents the notorious time-series problem of classical causality inasmuch "all direct dependences [appear] as mutual and simultaneous" (Mach, 1991 p. 279/206). Although Mach stressed that by considering chains of simultaneous functional dependences one can describe irreversible and non-instantaneous processes, the simultaneity of functional dependences excludes any kind of irreducible indeterminism for these basic entities themselves. Apart from the concept of function, the second part of the researcher's methodological equipment (Cf. Mach 1991, p. 286/210) is the method of variation which is "the basic method of thought experiments, as with physical experiments." (Mach, 1991, p. 191/139) Variation is grounded in the principle of continuity which permits to apply the method of comparison and to build analogies of different strength. Analogy represents an "abstract similarity. Analogy may in some circumstances remain entirely concealed to direct sense observation and reveal itself only through a comparison between the *conceptual* interconnections [Beziehungen] of the characteristics of one object with the interconnections of the characteristics of the other." (1991, p. 220/162) Among the examples Mach discusses, one can discern three types of analogies: More or less instinctive ones as that between the flow of heat and the movement of a substance (1987, p. 274) far-reaching and fruitful ones, such as between light and sound on which Huygens's optics is based (1918, p. 372); finally, purely formal ones, such as empirically equivalent mathematical expressions. In Mach's view, analogies act heuristically in theory formation as well as for the purpose of communication. But they never establish factual identities. Thus, their validity is limited in time, and a hitherto fertile analogy can impede scientific progress. The analogy of light and sound, for instance, prevented Huygens from discovering diffraction which seems to follow naturally from his theory of light.

Wholly in accordance with his opponent Planck, Mach considered determinism as a regulative principle of the scientific enterprise, but he went on to maintain (in contrast to Boltzmann and Planck) that theorizing *never* exhausts the manifold of facts.

> There is no way of proving the correctness of the position of '*determinism*' or '*indeterminism*'. Only if science were complete or demonstrably impossible could we decide such questions. These are presuppositions that we bring to the consideration of things. … However, during enquiry every thinker is necessarily a theoretical determinist, even if he is concerned with mere probabilities. Jacob Bernoulli's law of large numbers can be derived only on the basis of deterministic presuppositions. … The propositions of the calculus of probability hold only when chance events are *regularities masked* by complications. Only then can the mean values obtained for certain time spans make any sense. … [But:] No fact of experience repeats itself with absolute accuracy. … Therefore even the extreme theoretical determinist must in practice remain an indeterminist, especially if he does not wish to speculate away the most important discoveries. (Mach, 1991, p. 282f./208)

Mach's views on *probability* were rather traditional and he played down what Fechner – to whom Mach was intellectually indebted in so many respects – had accentuated, to wit, that changes in the initial conditions are an incessant source of indeterminism. To the extent that Mach treated probability as something relative to deterministic presuppositions, he agreed with von Kries's view. Of course, to Mach determinism was only a regulative principle, not a category. Moreover, Mach's natural laws were approximately stable functional dependencies, so that the distinction between nomological regularities, the natural laws, and ontological regularities emphasized by von Kries's was blurred.

Fechner was a realist concerning the existence of gravitating atoms. Mach, on the contrary, wanted to avoid any association of his elements with sensual atoms or substances. Even on a more general level than explained above, on the level of science as a whole, functional dependences constituted the basic elements of Mach's ontology. "One recognizes the *relations* between condition and *conditioned*, the equations which cover greater or less domains, as the *inherent permanency*, *substantiality*, as that whose ascertainment makes possible a *stable world picture*." (Mach, 1919, p. 431/390)

This radically relational ontology had, first of all, consequences for the status of scientific theories which Mach conceived as "bringing into parallelism a domain of facts with another more familiar domain" (Ibid., p. 461/415), or as an *indirect* description. Examples show even more clearly that Mach, in effect, considered theories as a kind of analogy which enable a *physical phenomenology* that is void of hypotheses. *Direct* descriptions are even free of theories. "Imagine that the ideal of complete, direct, conceptual description has been obtained for a domain of facts: we can, I think, say truly that this description achieves everything the investigator can require." (Ibid., p. 404/370) So far, there exist only fragments of this goal, "for instance, d'Alembert's (or Lagrange's) equations which comprise all possible dynamical facts, or Fourier's equations which comprise all conceivable facts of heat conduction." (Ibid., p. 461f./415).

In *The Science of Mechanics*, Mach considered all other theorems of mechanics, among them the Principle of Least Action, as versions of d'Alembert's theorem. Hence, they were "new only in *form* and not in *matter*." (Mach, 1988, p. 389/452). They constitute rules by means of which problems can be treated "by routine forms" (Ibid., p. 325/376) such that only mathematical difficulties remain whereas their physical content can always be intuited at a model, such as the equilibrium of strings.

Within today's physical science one could rephrase Mach's 'physical phenomenology' as a brand of 'effective theory' that fits certain parameters to experimental data in order to avoid any specification of an underlying microstructure. This strategy derives support from Mach's definition of a scientific concept as "a *stimulus* to a precisely determined though often complicated, testing, comparing or constructing *activity*." (Mach, 1919, p. 403/369) Yet, present-day experimentalists' pragmatism is often coupled to naive realism while Mach's ontology was based on functional dependences *directly* describing a fact.

In Mach's works one can discern facts of different size: the 'principles' of mechanics, effects such an interference or heat conduction, and 'grand facts', such as d'Alembert's principle or energy conservation. Facts admit different descriptions. "It is the same grand fact" (Mach, 1988, p. 312/356) which is present in Newton's axioms built upon by the concepts force, mass, and momentum, and in Huygens's 'sphere of ideas' applying the concepts of work, mass, and *vis viva*.

In the end, Mach's relational ontology runs into a dilemma. On the one hand, a given fact can – depending on our practical purposes – be functionally described in various ways that might even have equal economy. On the other hand, Mach excludes the reduction to a set of primary qualities – as atomists hold – because this gains an illusory unity at the price of artificial hypotheses. Moreover, functional descriptions will ultimately exhaust the fact. Given conventionalism *and* the ideal of an ultimately stable description – though not in a absolute sense –, one accordingly wonders how Mach's epistemology by conceptual adaptation could delimitate and keep together its single 'grand facts' – and so avoid the pernicious alternatives, that there is either just one grand fact, to wit, our entire world, or myriads of small facts the boundaries between which are drawn by practical interests only.

3.2 Action Principles, Uniqueness, and Stability

Mach avoided this dilemma by introducing a further ontological principle: uniqueness. It acts as a selection criterion for the factual solution among all possible ones. This type of reasoning, to Mach's mind, is expressed in the Principle of Least Action which states that the actual dynamics is distinguished among all possible ones by the stationarity of the action integral. The set of all possible dynamics is construed in terms of variations which necessarily vanish for the dynamics making the integral stationary. Let me make this point more precise by first investigating Boltzmann's stand on the Principle of Least Action. My emphasis here lies on the joint relationships between variation and uniqueness, on the one hand, and extremality of a quantity and stability, on the other hand, rather than on the details of variational principles.[52]

Boltzmann's first major contribution to physics sought "to give a purely analytical, completely general proof of the second law of thermodynamics, as well as to discover the theorem in mechanics that corresponds to it." (Boltzmann, 1866, p. 195) But he arrived at his goal, the Principle of Least Action, only for a strictly periodic system – not quite a generic case in thermodynamics – and he could not give any hint for a generalization. Although he would subsequently assign ever increasing importance to statistical concepts in understanding the second law, as late as in 1899 he returned to his old idea – at that time as a vague outlook closing his lectures at Clark University. "It turns out that the analogies with the second law are neither simply identical to the Principle of Least Action, nor to Hamilton's Principle, but that they are closely related to each of them." (Boltzmann, 1905, p. 306) In the second volume of his *Lectures on the Principles of Mechanics,* Boltzmann further elaborated on this analogy (1904, §§47-48). This persistent adherence to a rather unfruitful idea seems surprising, in particular, if one takes into account Planck's contention (See Section 3.8) that the Principle of Least Action was governing all reversible processes in nature while the second law introduced a fundamental irreversibility into physics. There are, to my mind, two possible reasons why Boltzmann never gave up this unsuccessful idea to bridge a gap which Planck considered as the most fundamental one in physics. First, in virtue of their wide range of applicability, Boltzmann might have hoped for a suitably generalized extremal principle to do the job. Second, he did not consider the distinction between reversible and irreversible processes as fundamental. This was a consequence of adopting Mach's empiricist account of natural law. Thus, in a certain sense, this persistent adherence to his 1866 idea is the

[52] The Principle of Least Action prompted many philosophical discussions in the period under investigation. While Planck and Hilbert cherished its unificatory powers, Logical Empiricists suspected it of illegitimately crossing the border between the synthetic and the analytic (See Stöltzner, 2003b).

counterpart of the later view of Vienna Indeterminists that reversible processes are just idealizations or macroscopic limits of random processes. (Cf. Chapter 4)

Boltzmann emphasized that the Principle of Least Action yields all equations of motion while energeticists had to add further propositions, such as *independent* energy conservation for each direction in space. Although Boltzmann did not claim that the "appearance of equations analogous to the mechanical ones in the theory of heat, electricity, and so on" (Boltzmann, 1904, p. 136/257) justified a reduction of these fields to hidden mechanical motions, he nevertheless contended:

> Only it would certainly be clearer if we could explain not only all phenomena of motion in solid, liquid and gaseous bodies but also heat, light, electricity, magnetism, and gravitation by means of the idea of motions of material points in space; that is by means of a single unitary principle, instead of requiring for each of these agencies a whole inventory of entirely alien concepts like temperature, electric charge, potential and so on … (Boltzmann, 1904, p. 137/258)

Mach, instead, did not see any ontological advantage in such a unifying principle, although he

> admit[s] that it is possible to discover analogies for the Principle of Least Action in the various departments without reaching them through the circuitous course of mechanics. I look upon mechanics not as the ultimate explanatory foundation of all the other provinces, but rather, owing to its superior formal development, as an admirable prototype of such an explanation. (Mach, 1988, p. 406/471)

To Mach, Boltzmann's 1866 linkage between the Principle of Least Action and the second law was initially a surprising result.

> Yet there is no reason for being surprised. When once it has been found that quantity of heat behaves like *vis viva*, and thus an analogue of the theorem of *vis viva* is applicable to it, it is not to be wondered at that the remaining mechanical principles (which are not essentially different from this principle) may also be applied in the theory of heat. (Mach, 1919, p. 364/334)

Hence, Boltzmann did not discover "a *new* proof of the *mechanical* nature of heat." (Ibid.) In view of Boltzmann's subsequent piecemeal elaboration of the theory against the criticisms of Loschmidt and Zermelo, it is surprising that Mach regarded the plan of the 1866 paper as already accomplished and merely rejected its interpretation.

There are, to my mind, two sources of Mach's misapprehension. First, similarly as the energeticists, Mach considered the Principle of Least Action and energy conservation as at bottom equivalently complete descriptions of a mechanical system, and he hardly distinguished between variations and differentials. Euler's precaution in 'perfecting' this analogy is deemed as "singularly timid" (Mach, 1988, p. 457/532). In 'A Word from Mathematics to Energetics', Boltzmann (1905, p. 106) spotted the energeticist Helm's principal fault precisely in this erroneous identification. Second, what for Boltzmann represented a unifying theoretical principle of mechanics, for Mach and even more for his Berlin ally Joseph Petzoldt was intimately linked to the ontological principle of uniqueness.

To Mach, the core of the Principle of Least Action lies in variation within a system of determining conditions. The feature of minimality present in it only stems from its historical origin in the world-view of a materially economical (or parsimonious) nature.

Notice that the Principle of Least Action, like all other minimum principles in mechanics, do not express other than that in the instances in question precisely *so much* happens as possibly *can happen* under the conditions, or as is *determined*, viz., *uniquely* determined by them…[T]he principle of *unique determination* has been better and more *perspicuously* elucidated than in my case by J. Petzoldt in a work entitled *Maxima, Minima and Economy*…: "In the case of all motions, the paths *actually* traversed can be interpreted as *distinguished* [German: *ausgezeichnete*] instances chosen from an *infinite number of conceivable instances*…"…I am in entire accord with Petzoldt when he says: "The theorems of Euler and Hamilton, and not less that of Gauss, are thus nothing more than analytic expressions for the fact of experience that the phenomena of nature are uniquely determined." The uniqueness of the minimum is decisive. (Mach, 1988, p. 404f./470f.)

In the cited article Petzoldt argued that those curves for which the variation does not vanish occur at least pairwise. As this was at bottom a Leibnizian idea, so is its philosophical interpretation.[53] "Thus one can conceive the Principle of Least Action and the related theorems within their domain of validity as *analytical expressions for the principle of sufficient reason*." (Petzoldt, 1890, p. 216) Pondering that a reversion of particle motion would violate the principle of uniqueness, Petzoldt in 1895 even concludes that the unidirectionality of physical and physiological processes is closely connected to this principle which he declares "the *supreme law of nature*" (Petzoldt, 1895, p. 203) although it was not based on positive experience, but represented a regulative condition of the possibility of knowledge. Analogously, Ostwald had proposed his 'principle of the distinguished case' as a generalization of all minimum principles. "If there is present an infinite number of possibilities for a process, then what actually happens is distinguished among the possible cases." (Ostwald, 1893, p. 600) Of course, a "certain difficulty in applying the principle is to find in each case the characteristic quantity the variation of which is to vanish." (Ibid., p. 602) However it is philosophically framed, the requirement of uniqueness does not in general suffice for a derivation of the equations of motion.

In his 1890 paper, Petzoldt set up an even higher barrier against assessing Boltzmann's statistical interpretation of the second law than Mach had ever done when approving Boltzmann's 1866 idea. He argues that Mach's merely subjective principle of economy cannot provide a measure to treat objectively purposive biological structures. Instead, Fechner's global tendency toward stability permits a reconciliation of teleology and causality on a general level.[54]

Purposiveness [*Zweckmäßigkeit*] as large as possible is the goal of all development. Thus the teleological principle coincides with the principle of tendency toward stability, and the latter mediates between the former and the law of causality. This conception, however, signifies a generalization of the notion of purpose, since one has to declare *all* stable states as purposive. (Petzoldt, 1890, p. 226)

In my view, this central role of the principle of stability is a consequence of Petzoldt's and Ostwald's assumption that the possible worlds really existed. This hypothesis was not shared by Mach who had insisted on the individual existence of nature. (See Sect. 3.1)

In the *Theory of Heat*, Mach defends his principle of economy against Petzoldt by insisting, firstly, that in physics there "is no choice between the *actual* happening and *another*. For this reason I have not used the notion of economy in any way in this

[53] See (Stöltzner, 2000c).

[54] See (Heidelberger, 1993) for a general account of Mach's and Petzoldt's reception of Fechner.

domain." (Mach, 1919, p. 393/360) But Mach does not further insist on this principal difference between economy and stability and considers it mainly as a matter of terminology. At another place, Mach detects in Petzoldt's account the danger of "falling into a kind of *Aristotelian* physics by ascribing to organisms a tendency toward stability." (Ibid., p. 382/351)

Boltzmann had, in passing, expressed similar misgivings about possible world arguments. "Nor do I examine whether, as Herr Ostwald holds, the actual world is a special case among all possible ones, or whether the latter are just fancy combinations of the actual in a slightly modified arrangement." (Boltzmann, 1905, p. 131) And in the lecture notes, one can find a passage in which Boltzmann rejects any strong form of modality. "All that is, is necessary; there is nothing which could be otherwise as well." (Fasol-Boltzmann, 1990, p. 161)

Petzoldt, on his part, even extended the principle of stability to the realm of mental phenomena, to aesthetics and ethics. In this way, Mach's principle of economy of thought is changed into an objective cognitive stability.

> The economical order of a system of conceptual reactions [*Begriffsreactionen*] means nothing but such an arrangement for which there exists no longer a condition for further change in the relations among the single concepts and between them and the complexes of sensations and ideas eliciting them. (Petzoldt, 1890, p. 429)

We shall see in Section 3.8. that this daring extension of Machian economy comes rather close to Planck's convergent realism which was based on the ideal of a stable physical world view, that is, a stable system of relations between basic physical concepts expressed in a few principles.

Despite his valid criticisms against Petzoldt, Mach still cherished stability as the core of the second law.

> It may be mentioned that Boltzmann, presumably without being acquainted with the views just mentioned [among them Fechner's and Petzoldt's], demonstrated that a physical system when left to itself, gradually goes over into "more probable states" and finally into the "most probable state". Closer consideration shows that this "most probable state" is at the same time the most stable. (Mach, 1919, p. 381/351)

Boltzmann (1905, p. 154/53 n. 9) frankly admitted his ignorance and retorted:

> The assertion that a system of very many bodies in motion tends, bar unobservably few exceptions, to a state for which a specifiable mathematical expression denoting its probability becomes a maximum does seem to me more than the almost tautological statement that the system tends towards the most stable state. (Boltzmann, 1905, p. 154/53)

Boltzmann also observed "that all concepts of [thermodynamic] phenomenology are derived from quasi-stationary processes and no longer hold good for turbulent motion." (Ibid., p. 148/45) And he kept repeating that far from equilibrium only the mechanical approach yielded definitive results.

To summarize the above discussions, if general ontological principles, such as uniqueness and stability, are employed in the constitution and explanation of facts, they are in danger of relapsing into Kantian a priori conditions of the possibility of knowledge or at least relativized versions thereof. Petzoldt's move to reclassify the erstwhile synthetic a priori judgments as less suspicious regulative principles was not an unprecedented strategy in those days; thus several neo-Kantians moved from the first to the third *Critique*. Clearly distinguishing minima, maxima, and economy from their objective counterparts, uniqueness and stability, Petzoldt still revealed dualistic tendencies which Mach, in his need for a reality criterion, partially endorsed. For both reasons, Mach's redefinition of causality in terms of functional dependences could not fully thrive. By separating more clearly the facts and the theories – which do not face facts instantaneously and one by one – Boltzmann could better avail himself of the Machian conception of functional dependences as an ontological basis for physical theory. To him, uniqueness was the major requirement imposed upon theory and it rendered atomism – on the theoretical level – inevitable. Yet, Boltzmann's setting apart facts and theory created the need for a new reality criterion because, like Mach, he rejected any a priori knowledge. In their correspondence about causality, Boltzmann puts his finger on Mach's ontological problems and appeared – in sheer inversion to the received view – as the more determined empiricist. This shall concern us next.

3.3 Boltzmann on Causality and Probability

On October 1st, 1893, Boltzmann initiated a correspondence with Mach.

> Are you still holding that the energy law can be deduced from the principle of causality? [55] … Can you prove the impossibility of such a perpetuum mobile from the law of causality alone? I believe that the impossibility of the perpetuum mobile is a purely empirical proposition that can be always refuted by experience in cases not yet examined. (from Höflechner, 1994, p. II 199)

Unfortunately, all letters by Mach are lost. But in Boltzmann's letters one finds no trace of polemics – just as he had announced in his first letter. In his answer to Boltzmann's inquiry, Mach had apparently invoked his principle of unique determination, but Boltzmann did not regard this as conclusive because the functional dependences could be more complicated than Mach assumed, so that further experience was needed to obtain energy conservation. Moreover, "[t]he law of causality does not seem to require that, if there are n equations among $m+n$ phenomena, every phenomenon is a unique function of arbitrary m of them." (Ibid., p. II 202) By the end of the month, both agreed that the theorem of energy conservation is empirical.

Except for the notorious notion of uniqueness, Boltzmann's ideas about causality largely agreed with Mach's. He replaced cause and effect by functional dependences and emphasized that identical conditions never return. Regularity is both an experience and a presupposition of scientific research. "[W]e are thus free to denote [the law of causality] either as the precondition of all experience or as itself an experience we have in conjunction with every other." (Boltzmann, 1905, p. 163/75) When Mach diagnoses a human 'desire for causality', Boltzmann discerns a general tendency that our mental habits, among them causality, which have evolved throughout the centuries, 'overshoot the mark' by still seeking explanation or definition of the inexplicable elementary concepts. Boltzmann also found a pictorial expression for Mach's insistence that the value of concepts lies in their making us act

[55] Boltzmann refers to (Mach, 1909) and modifies Mach's example of the tuning fork.

successfully: “With the concepts of cause and effect one cannot operate a tramway.” (Fasol-Boltzmann, 1990, p. 280) Likewise Boltzmann treated the notion of continuum (See Sect. 3.5).

There are, however, two important differences. First, beyond restating Mach’s linkage of functional causality to psychology and sense physiology, Boltzmann connects the issue of causality much closer to classical philosophy, in particular to Kant’s theory of rationality. All laws of thought are inherited habits of thought that have become a priori conditions of knowledge, “but it seems to be no more than a logical howler of Kant’s to infer their infallibility in all cases.” (Boltzmann, 1905, p. 398/195) In the Kantian antinomies, adaptation overshoots the mark.

> Indeed people racked their brains over the question whether cause and effect represent a necessary link or merely an accidental sequence, whereas one can sensibly ask only whether a specific phenomenon is always linked with a definite group of others, being their necessary consequence, or whether this group may at times be absent. (Ibid., p. 354/166)

Second, in a fragment for the lectures, titled “Cause and Effect”, Boltzmann links causality to probability.[56]

> Before any experience takes place, both [an accidental sequence or a causal link between phenomena] is equally probable. But my repeated experiences render it infinitely improbable that all observed *regularity* would be accidental, and infinitely probable that actual actually takes place. (Fasol-Boltzmann, 1990, p. 282)

Still at the time of his philosophy lectures, Boltzmann considered probability as degree of certainty and he seems to have favored the logical interpretation of Johannes von Kries. In his 1886 academy address on the probabilistic character of the second law, Boltzmann approvingly quoted von Kries’s seminal book (1886), yet without further discussing his approach or mentioning the *Spielraum* (range) concept (Cf. Boltzmann, 1905, p. 37/22). As the relevant chapter of Kries’s book titled “On the Application of Probability Calculus in Theoretical Physics” was almost exclusively based on Boltzmann’s writings, one obtains the impression that Boltzmann was just glad that somebody had accomplished the task of providing a philosophical basis to his statistical mechanics. Yet Boltzmann was aware that applying logical probability requires “that the mechanical conditions of the system are known.” (Boltzmann, 1905, p. 37/22) As Martin Klein has shown, prompted by his critics Boltzmann through the years made several major changes in his use of the concept of probability, most of which were not fully noticed by his readers. The first of them in 1877 led from “a theory emphasizing kinetics and based on the special assumptions about collisions underlying the Boltzmann equation, to a theory emphasizing combinatorial statistics and independent of collision analysis.” (Klein, 1973, p. 84) In a second reinterpretation, Boltzmann “took the probability of a distribution to be the fraction of any sufficiently long time interval during which one could expect to find the gas described by this distribution.” (Ibid., p. 88)[57] And he, finally, insisted against Zermelo on “the typical or representative character of the Maxwell distribution” (Ibid., p 91) within the set of initial conditions.

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[56] For a more extensive discussion of ‘A5’, see (Blackmore, 1995b), ch. 7.

[57] For a general discussion about Boltzmann’s statistical mechanics, see (Brush, 1990) and (Cercignani, 1998) who, unfortunately, repeats the old stereotype of the titans’ war between Boltzmann and Mach.

But despite these modifications, developing statistical mechanics as a science of its own right that studies "the properties of a complex of very many mechanical systems starting from the most varied initial conditions" (Boltzmann, 1905, p. 360/171) was aggravated by obtaining a proper concept of equiprobability. "However, this being the fundamental concept, it cannot in turn be derived and must be regarded as given." (Ibid., p. 361/171) On January, 31st, 1906, Boltzmann's notes for the philosophy class read:

> Knowledge by the law of causality not in the same way from experience. Source of experience. We stand[58] under its influence. One seeks probability from a priori probability. [This] only [makes] sense, if equally possible cases. Necessarily subjective from our classifications or after known causal law. (Fasol-Boltzmann, 1990, p.145)

To my mind, Boltzmann here argues that in the same way as we necessarily order experiences by (functional) causality, we pose equiprobabilities in order to base probabilistic laws. Both are achieved either by classifications, e.g. the symmetry of a die, or according to already empirically known laws, such as: "We can infer from experience that in lotto every move is equally probable." (Boltzmann, 1905, p. 163/75) To be sure, the Kriesian account combined aprioristic and empiricist elements because given the range (*Spielraum*) of possible outcomes their relative weight was determined empirically. Already at the time when he endorsed von Kries's theory of probability, Boltzmann had accepted Mach's empiricist notion of natural law. The Boltzmann of the philosophy notebooks is still further from Kriesian territory when he contemplates that there could be "deviations from the principle of energy [conservation], perhaps only of the second law, also from the area law, or from the center of mass law." (Fasol-Boltzmann, 1990, p. 106)

But Boltzmann never made the final step to base probability entirely on experience, although in a letter to Felix Klein in 1899 he expressed his misgivings about Emanuel Czuber's abstract definition of the object of probability calculus (cf. Höflechner, 1994, p. II 318) and paralleled them with his earlier criticisms of "the boring and uninformative definitions of number, addition, etc." (Ibid., p. II 270). While in the latter case he calls – citing Mach – number 'a purely empirical concept', he apparently was not acquainted with Fechner's relative frequency interpretation of probability posthumously published in 1897. Recall that as Fechner and Mach, Boltzmann considered the law of causality not as an a priori precondition of experience but as a very general empirical fact. Moreover, the relative frequency interpretation would fit so neatly to his own definition of statistical mechanics as an autonomous science. Thus in effect many piers of the bridge toward this interpretation had already been set. The only problem would have been to find the appropriate collective objects (Kollektivgegenstände).

Why did Boltzmann never read Fechner; not even after Mach had publicly criticized his ignorance? Perhaps it was the context of the tendency to stability emphasized by Mach and Petzoldt which made such reading unpalatable to Boltzmann. And as Boltzmann's rebuttal of Zermelo's criticism depended upon the existence of a measure on the set of possible initial conditions, the Kriesian notion of range (Spielraum) provided an appropriate framework. But one could also argue the

[58] Here I cannot make sense from the transcription from Boltzmann's shorthand other that changing 'entstehen' (originate, emerge) into 'stehen'. See (Blackmore, 1905a, p. 169) for a translation of the entire note.

other way round. According to Fechner's, indeterminism arose from the novelty of initial conditions, so that even laws of nature may change on a cosmological scale. This could have given some philosophical justification for the strange events admitted by the second law. As did Mach and all Vienna Indeterminists, Fechner held that it is impossible to ultimately decide the conflict between determinism and indeterminism. (See Heidelberger, 1993, § 7.1)

Boltzmann's ignorance is even more surprising if one takes into account that after 1897 the *Kollektivmaßlehre* was immediately discussed in the literature and that it was clearly recognized as an alternative to the Laplacian definition and to von Kries. Shortly after Boltzmann's death, "about 1908 Fechner's theory of collectives apparently was standard knowledge for everyone working on probability theory and statistics in the German-speaking area." (Ibid., p. 376) Thus matters remain puzzling. I think that Michael Heidelberger is quite right that Fechner's thoughts about probability were too much embedded into his general and often hermetic outlook to be quickly accessible for someone who was – in stark contrast to Mach – unfamiliar with their philosophical context.[59] Major support for this conclusion derives from the fact that Franz Exner, who was familiar with Fechner's writings, quickly accomplished the missing step towards the relative frequency interpretation and subsequently turned Boltzmann's second law it into a rather comprehensive world view. (See Sect. 4.1.)

Boltzmann himself, however, both having rejected Mach's principle of unique determination and ignoring Fechner's collectives had to find an ontology for statistical mechanics in another way.

3.4. Theory Reduction, Pictures, and Ontology

Comparing atomism and phenomenology, Boltzmann was at pains to distinguish his main opponents, energeticists, from Kirchhoff and Hertz's mathematical and Mach's general phenomenology. In 1897 he contrasted atomism to mathematical phenomenology and to energeticist phenomenology emphasizing that the first two agree to the extent that "differential equations … are evidently nothing but rules for forming and combining numbers and geometrical concepts, and these are in turn nothing but mental pictures." (Boltzmann, 1905, p. 142/42) He subsequently advanced the constructivist argument to be discussed in Section 3.5. Two years later, he deemed energetics a relapse into metaphysics and distinguished it from phenomenology which he now divided into a mathematical and a general branch. General phenomenology was characterized by a dictum of Mach, who before had been listed as a mathematical phenomenologist: "electricity is nothing but the sum of all experiences that we have had in this field and still hope to have." (Ibid., 221/95) After the pattern of Hertz's *Principles of Mechanics* (1894), mathematical phenomenology follows a modified and reinterpreted Euclidean approach. Instead of positing a priori self-evident axioms, the axioms are justified only by comparing the laws derived from them with experience.

[59] Michael Heidelberger, private communication. Indeed both contexts of Fechner's indeterminism listed in (Heidelberger 1993, 338-353), "Freedom and Physiology" and "Epigenesis and Philosophy of History", were quite extraneous for Boltzmann. His preparatory notes for the philosophy lectures do not mention Fechner either, cf. (Fasol-Boltzmann 1990, 13).

In his lectures at Clark University delivered in the same year, Boltzmann compared the deductive and the inductive approach in mechanics.

Since the deductive method does not constantly mix external experience forced on us with internal pictures arbitrarily chosen by us, this is much the easiest way of developing these pictures clearly and consistently. For it is one of the most important requirements that the pictures be perfectly clear. … There is not the slightest proof that one might not excogitate other pictures equally congruent with experience. This seems to be a mistake but is perhaps an advantage at least for those who hold the above-mentioned view [of theories as pictures]. … However, it is a genuine mistake of the deductive method that it leaves invisible the path on which the picture in question was reached. (Ibid., p. 262/107f.)

Bringing forward Mach's historical-critical method against Hertz's approach is not the main reason why Boltzmann pursued a different route. Granting the superb simplicity of Hertz's concrete pictures, he nevertheless considered them as a program for the far future. "For the time being, however, alongside Hertzian ones we shall not be able to forgo simple and directly useful pictures that can be worked out in detail." (Ibid., p. 269/113) This is the reason why Boltzmann's *Lectures*, in formulating the general equations of motion, prefer to "start from action at a distance and only later to deduce Hamilton's principle." (Boltzmann, 1897, p. 24/241) Boltzmann repeatedly credited Hertz for the picture conception of scientific theory, but he rejected Hertz's "demand that the pictures we construct for ourselves must obey the laws of thought" (Boltzmann, 1905, p. 258/104). This was too Kantian a requirement for Boltzmann's pictures because the habits of thought were only acquired in a biological adaptation process.[60]

In the second and third Clark University lectures Boltzmann confronted his students with a genuinely inductive presentation "by starting directly from the facts as they present themselves to unprejudiced observation, letting the pictures grow gradually from these facts and introducing each abstraction only when there is no way left of avoiding it." (Boltzmann, 1905, p. 270/114) Maintaining such purism proved difficult and it rendered Boltzmann's sketch extremely clumsy. No wonder that the comparison *in actu* resulted in favor of the deductive mode of presentation. In particular, "only in the possibility of an exact presentation of all special cases possible the clarity and consistency of the pictures can be tested." (Ibid., p. 301) In contrast, Mach's inductive approach directed attention only to few special cases, the historically relevant standard models, and despite his critical investigation of alternative routes he did not fully assess the mathematical universality of the principles.

Boltzmann also set Machian phenomenology far apart from energeticist substantialism.[61]

[A]s regards Ostwald's energetics, I think it rests merely on a misunderstanding of Mach's ideas. Mach pointed out that we are only given the law-like course of our sense impressions and ideas, whereas all physical magnitudes, atoms, molecules, forces, energies and so on are mere concepts for the economical representation and illustration of these law-like relations of our sense impressions and ideas. The last are thus the only thing that exists in the first instance, physical concepts being merely mental additions of our own. Ostwald understood only one half of this proposition, namely that atoms

[60] See also (D'Agostini, 1990).

[61] Robert Deltete (1999) has recently argued that Georg Helm, who was Boltzmann's opponent on the 1895 *Naturforscherversammlung,* did not substantialize energy.

did not exist; at once he asked: what then does exist? To this his answer was that it was energy that existed. In my view this answer is quite opposed to Mach's outlook. (Boltzmann, 1905, p. 368/175f.)

In his attempted 'reconciliation' between mechanical and phenomenological physics, Mach had advanced a similar argument against Boltzmann's pictorial atomism which he considered as 'not altogether chivalrous' because of the "fearful earnestness and naiveté [in which] these ideas were taken by the great majority of distinguished investigators." (Mach, 1919, p. 363/334).

Boltzmann emphasized "that it cannot be our task to find an absolutely correct theory but rather a picture that is as simple as possible and that represents phenomena as accurately as possible" (Boltzmann, 1905, p. 216/91) – a demand that could be fulfilled equally well by two theories. Already in 1873 he had praised Maxwell's electrodynamics,

partially because he starts from precisely determined assumptions and proves with mathematical acuteness that all magneto-electrical interactions can be explained from them, partly because this theory yields some consequences which still await confirmation by experiment and can thus serve as a touchstone for their correctness and the admissibility of this view. The other theories only reach as far as the phenomena are known, but not beyond that. (Ibid., p. 11)

Beyond what one could call, in Lakatosian terms, the theory's excess content, Boltzmann insisted that "without any hypothetical features one could never go beyond an unsimplified memory mark for each separate phenomenon." (Boltzmann, 1897, p. 2/225) Already differential equations expressing the functional dependences "are nothing more than rules for constructing alien mental pictures, namely series of numbers." (Ibid., p. 3/226) Establishing Machian 'grand facts' is illusory since "of a comprehensive area of fact we can never have a direct description but always a mental picture." (Boltzmann, 1905, p. 142/42) Hence, no reasonable unification can be achieved without conceptual simplification and the construction of basic universal pictures.

To Boltzmann's mind, atomism has most closely approached phenomenologists' ideal of finding a set of a universally valid equations. To Mach's mind, however, theories were rather an economical 'scaffolding' for construing the functional dependences. Thus the difference between Mach and Boltzmann did not only lie in the hypothetical character of theories – which Mach stressed by far stronger than Boltzmann – but also in the systematic structure and conceptual purity which distinguished a theoretical system from an economically well-ordered catalogue.[62] There has to be a certain hierarchy in such a system according to which a successful explanation consists in the reduction to a few basic universal concepts that make possible a unified picture of physical phenomena.

What is the philosophical status of such a picture realism, in particular, as regards the most basic entities? The rich literature on this topic is far from an agreement.[63] While Erwin Hiebert considers Boltzmann as both a philosophical

[62] As a matter of fact, Einstein judged that Mach had only provided "a catalogue, but not a system" which should be built upon a few simple principles. See Société française de Philosophie (ed.): *Comptes rendus des séances du 6 avril 1922*; quoted according to (Wolters, 1987, p. 109).

[63] The conception of theories as pictures is often described in the literature; a particular focus is to what extent Boltzmann's views were indebted to Hertz and which traces of it in Wittgenstein's thinking. See among others (Hiebert, 1980), (Curd, 1978), (D'Agostini, 1990), (de Regt, 1996 & 1999), (Wilson, 1989).

idealist and "pragmatic realist" (1980, p. 181), Henk de Regt calls for distinguishing the realist on the *ontological* level, the advocate of a picture theory on the *epistemological* level, and a scientist who "endorsed realism on the methodological level." (De Regt, 1996, p. 42) Boltzmann indeed held that by recognizing processes independent of our thoughts and volitions, "we first obtain the concept of objective existence as something independent of momentary memory." (Boltzmann, 1905, p. 172/64) To my mind, one should be careful in drawing ontological consequences. As we shall see below, the 1897 paper 'On the Question of the Objective Existence of Processes in Inanimate Nature' quickly takes a strongly pragmatist tack by making a linguistic turn. In a later paper, de Regt distinguishes between the epistemological or representative role of pictures, to wit, the theory acting as analogy, and the methodological role of pictures, the employment of "specific mechanical analogies in order to obtain visualizations." (De Regt, 1999, p. 116)

Boltzmann's concept of reduction to universal entities can thus be seen as an attempt to go beyond description in the sense of Mach and Kirchhoff and work out a concept of scientific explanation. And indeed we read at the beginning of his Viennese inaugural lecture: "It is the ubiquitous task of science to explain the more complex in terms of the simpler, or, if preferred, to represent [*anschaulich darzustellen*] the complex by means of clear pictures [*Bilder*] borrowed from the sphere of simpler phenomena." (1905, p. 334/149) To Boltzmann's mind, mechanical pictures are the simplest and most intuitive ones because they closely correspond to a successful mode of action. Thus instead of a realist preference of mechanicism we find Mach's biological corroboration at work. "What, then, is meant by having perfectly correct understanding of a mechanism? Everybody knows that the practical criterion for this consists in being able to handle it correctly. However, I go further and assert that this is the only tenable definition of understanding a mechanism." (Ibid., p. 335/150) This is the first pragmatist argument for the methodological superiority of a realist view on atomism.

The second one emerges from Boltzmann's linguistic turn. Emphasizing the objectivity gained by learning a language, he ultimately considered "the realist mode of expression more purposive [*zweckmäßig*] than the idealist one." (Boltzmann, 1905, p. 186/75) This choice is not a metaphysical commitment because "the simplest preconditions of all experience and the laws of thought one can, I think at best describe. Once admit this and all contradictions vanish that one previously met in the attempt to answer certain questions," (Ibid., p, 186f./75) to wit, the paradoxes of classical metaphysics. It has been observed[64] that this argument is a forerunner to what among Logical Empiricists became a pretty standard way to purge our thinking from metaphysics. Rather than taking their verificationist route, Boltzmann himself set out from an individual version of what Mach would call a direct description. "Our world picture would be ideally perfect if for each of our sensations we had a sign and a rule by which to construct from these signs the occurrence of all our future sensations and the way they depend on our volitions." (Boltzmann, 1905, p. 166/59) But such a conception would be unable to transcend the limits of a given individual and to give any clue about what today is called the 'problem of other minds'.

[64] Blackmore (1995b) even speaks about Boltzmann's 'linguistic philosophy'; see also (Visser, 1999).

If … one believes to have proved that matter is merely the expression of certain equations between complexes of sensations [Empfindungen], so that the assertion that matter exists in the same way as our sensations exceeds our task of merely describing the phenomena, it would be well to remember that this would be proving too much; for in that event the sensations and volitions of all others could not be on the same level as the sensations of the observer, but would have to be taken as merely expressing equations between his own sensations. (Ibid., p.168/61)

Boltzmann's target is a still wide-spread misreading of Mach's neutral monism which rests upon the dualist assumption that the subject is already constituted as an independent entity. It stood at the back of Planck's attacks (See Section 3.7). But Mach approached the problem of other minds by us recognizing the analogy of certain characteristics of our own experiences, or sensations, and behavior with similar characteristics of other individuals. Rather than emphasizing the biological perspective, Boltzmann gave this insight a linguistic turn.

The question whether the unicorn or the planet Vulcan exists in the sense in which the stag or the planet Mars exists has naturally a quite definitive sense, which is clear from our empirically known relation to the second two items. If, however, someone were to assert that only his sensations existed, whereas those of all others were merely the expression in his thinking organ [*Denkorgane*] of certain equations between certain of his sensations (let us call him an ideologist), we should first have to ask what sense he gives to this and whether he expresses this sense in an appropriate [*zweckmäßig*] way. Evidently he would still have to denote alien sensations with the same signs analogously arrayed with which he denotes his own; subjectively it would be indifferent to him whether he said that those sensations belonged to others who exist or to others whom he imagines, since for him others are only imagined. But since we use the verb 'not to exist' when we find that expectations expressed by certain mental signs are not confirmed by experience …, it would be inappropriate [*unzweckmäßig*] to say that all others, save the person here thinking, did not exist. (1905, p. 168/61)

Thus the notorious problems of the absolute reality of the Ego, of the external world, and of other minds are meaningless insofar as the empirical notion of existence cannot be appropriately applied to it. But on the other hand, language acts as an objectifying factor which stabilizes the Machian functional dependences – the 'equations' in Boltzmann's wording. "Language must use … terminology that is equally appropriate for all persons, 'we must adopt the objective point of view'." (Ibid., p. 173/64)

Boltzmann was aware of the fact that the manifold of newly discovered types of radiation and radioactivity fortified his atomist position because he did not consider invariability and indivisibility as essential features of atoms.[65] In this respect, the domain of validity of the atomistic hypothesis could be extended.

[T]he ray of hope for a non-mechanical explanation of nature came … from an atomic theory that in its fantastic hypotheses surpasses the old atomic theory as much as their elementary structures surpass in smallness those of the old atoms. I need not mention that I mean the modern theory of electrons. (Boltzmann, 1904, p. 138/259)

Boltzmann's mature atomism did not aim at a mechanical reduction of physics, but involved a reduction argument that, in modern terms, can be considered as a type of theory reduction. Making this point more precise will help to understand his rather surprising 'proof' of atomism from the consistency of the differential quotient *d/dt* and it will also help us getting hands on Boltzmann's reality criterion.

[65] See (Boltzmann, 1905, p. 150/52 & 357f./168f.).

Although Boltzmann agreed with Mach that science starts with instinctive experiences, he did not extend the principle of continuity to the level of theories. Their development is "full of discontinuities." (Boltzmann, 1905, p. 201/79) Thus physical theory – in particular if it proceeds deductively – is free to choose the most purposive basic concepts as long as they are well-defined and the theory agrees with experience. In this way, atoms are defined by ascribing to them properties necessary to describe a certain domain of facts in such a way that the picture tentatively reaches beyond them. "Obviously all properties of bodies that do not arise simply from the joint action of the large number of elements must be ascribed to the elements themselves; there is no other way of obtaining a picture of extended and apparently continuous bodies having these properties." (Ibid., p.160/56) In modern terms, Boltzmann's atomism can be described as theory reduction to (hypothetical) universal pictures and their interactions. In his philosophy lectures, Boltzmann discussed the following example which goes back to the mathematician Georg (Jurij) von Vega (1754-1802).

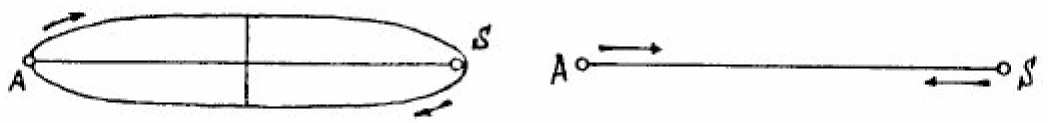

Figure 1.

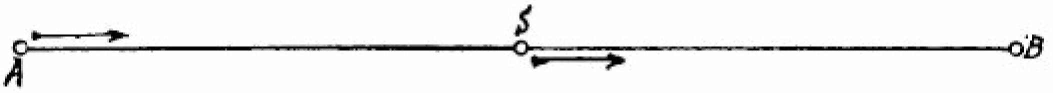

Figure 2.

In classical point mechanics the gravitational *-1/r* potential becomes singular at the origin *r=0.* Thus, within celestial mechanics one cannot predict whether a point particle moving on a head-on orbit to the origin is reflected by the singularity or goes right through it. Boltzmann studies two possible approaches to this problem which represents an obvious failure of the principle of unique determination in Newtonian mechanics. If we suppose (*Fig.1*) that the pointlike planet *A* moves on an extremely eccentric orbit around the pointlike sun *S* and pass to the limit, then we obtain that the planet is reflected. If, on the other hand, we suppose *A* already on the head-on orbit (*Fig.2*), but suspend the gravitational force within a very small but finite sphere around *S*, then the planet goes through *S* and reaches the point *B*. If we do not choose either limiting procedure and stick to the notion of actual infinity, viz. to the continuum, we are left with a paradox.

Today one resolves this ambiguity by performing the classical limit of the corresponding quantum mechanical time evolution. This yields reflection.[66] Also Boltzmann's original conclusion referred to a more fundamental discrete empirical theory, to wit, mathematics.

We must consider it [matter] to be composed out of a finite number of discrete [material] points, if we are to be capable of drawing reliable inferences. We can let the number of points grow enormously, yet the inferences always stay unique. But if we think it actually as a continuum, then we get into set

[66] See the example in (Thirring, 1995).

theory; every minute we reach places where we cannot infer uniquely, and the purpose of thinking is just to infer uniquely everywhere.…That is in fact the proof for the atomistic constitution of matter expressed in our philosophical language. (Fasol-Boltzmann, 1990, p. 200)

3.5 Mathematical Atomism and Constructivism

Boltzmann's qualms about the continuum even concern the mathematical structure of dynamics.

For my feeling there is still a certain lack of clarity in the differential quotients with respect to time. Except for the few cases where one can find an analytic function that has exactly the prescribed differential quotients with respect to time, then in order to set up a numerical picture, one will always have to imagine time as divided into a finite number of parts before one proceeds to the limit. Perhaps our formulae are only very closely approximate expressions for average values that can be constructed from much finer elements and are not differentiable in a strict sense. As to that, however, there are so far no indications from experience. (Boltzmann, 1897, p. 26f. /243f.)

Since, to Boltzmann's mind, a consistent definition of the differential quotients requires a limiting procedure, he calls "differential equations merely symbols for atomistic conceptions." (Boltzmann, 1905, p. 145/44) Recalling his conception of physical atomism cited above, one wonders to which 'mathematical' atoms and their interactions the macroscopically observable properties should be ascribed. In Boltzmann's writings I find four different though not mutually independent answers.

First, he considered heat conduction and electrodynamics as atomistic theories because their 'conceptual objects' are "dependent only on the immediate neighborhood." (Ibid., p. 146/51) Phrased in modern terms, atomism is already suggested by local interactions while mechanics was based on action-at-a-distance.

Second, "we are also aware of examples of very rapid oscillations and cannot prove exactly whether in certain cases there might not be motions, such as the thermal motion of molecules, that are better represented by a some kind of Weierstraß function than by a differentiable one." (Ibid., p. 283/123f.) Boltzmann (1898b) also contemplated whether the H-curve was a non-differentiable function.[67] In the discussion following a lecture of Klein in the *Philosophical Society of the University of Vienna* in October 1905, Boltzmann even considered Weierstraß functions as a typical feature of practical measurement.

It is noteworthy that those non-differential curves enjoy a certain similarity with those of practical physics as recorded registering devices which, for instance, picture the change of temperature during one day. If one takes a very precise registering device the spring flickers up and down and yields a curve which is not altogether continuous. (Boltzmann, 1906, p. 9)

Third, Boltzmann's ideas about a discontinuous dynamics were based on his assumptions about the atomistic nature of time that lapses like the pictures in a cinematograph (Cf. Fasol-Boltzmann, 1990, p. 105). In a letter to Brentano[68], Boltzmann even estimated the number of atoms in a second as $10^{10^{10^{10}}}$ – a number

[67] See also his correspondence with Felix Klein (Höflechner, 1994, p. II 277-280).
[68] See (Höflechner, 1994, p. II 384) and (Blackmore, 1995a, p. 125).

which grossly exceeds its counterpart for matter, the number of atoms in a gram molecule $6 \cdot 10^{23}$ named after Loschmidt and Avogadro. "The number of points of time can be made so great that the probability becomes great that a very improbable condition can occur in the whole world." (Ibid., p. 282f.) Thus, in the (presumably finite) Universe there could be regions in which the entropy decreases and time flows backward. And thus the "force law must differ in time depending on whether one proceeds in time in one or another direction." (Ibid., p. 283) This cosmological idea was another characteristic of the local Viennese tradition; it attracted criticism from the other side of the aisle. (Cf. Sections 3.7. and 5.5.2.)

Fourth, space and time are still not at the most fundamental level because atomism is only our mental picture of reality. To attain an ontology for the basic concepts a physical theory is reduced to, Boltzmann develops a constructivist reality criterion that replaced Mach's intuition of sufficiently stable complexes of functional dependences among the determining elements. It involved the empiricist foundations of mathematics.

Space and time are continuous. Our pictures, numbers in first place are discontinuous. Matter is continuous, our pictures, atoms in first place are discontinuous because we cannot think infinitely, but nature can do it. (Ibid., p. 106)

Mach and Boltzmann insisted against Kant that space and time were not based on a priori pure intuitions, but abstracted from experience. But while Mach based space and time in our physiological organization (Cf. Mach, 1991), Boltzmann emphasized that "space [and also time] has been construed by means of the concept of number alone, without any help from intuition." (Boltzmann, 1905, p. 388/187) Assenting to Felix Klein's criticism of the Kantian concepts of space and time, he bluntly asserted: "Intuition does not prove anything. Intuition just repeats what we have sensually perceived." (Boltzmann, 1906, p. 9) In his philosophy lectures, he even elucidated the concept of a manifold by a non-Euclidean color space in order not to bias the foundations of geometry by our common intuitive experiences. Numbers, however, are empirical concepts themselves. In his letter to Klein and in the lectures, Boltzmann approved Mach's definition of number.[69]

Numbers are also names. Numbers would never have originated had we possessed the capability of picturing with absolute distinctness to ourselves the members of an arbitrary set of like objects as different. We count where we desire to record the distinction between similar things; i.e., we assign to each of the like things a name, a distinguishing sign. (Mach, 1919, p. 67/69)

I regard the propositions of arithmetic to have been reached by [inner] experience. … I long ago characterized mathematics as *economically ordered experience of counting, made ready for immediate use*, the purpose of which is to replace direct counting…by operations previously performed. (Ibid., p. 68/70)

On this basis, Mach considered the continuum as a 'convenient fiction' and rejected Boltzmann's mathematical atomism. The latter, however, played again the part of the firm empiricist and rejected all kinds of absolute truths. "The concepts of differential and integral calculus divorced from any atomist notions are typically metaphysical, if

[69] See (Fasol-Boltzmann, 1990, p. 159) and (Höflechner, 1994, p. II 270). Boltzmann's philosophy has recently been the object of several investigations: (Tanaka, 1999), (De Courtenay, 2002), (Wilholt, 2002).

following an apposite definition of Mach we mean by this the kind of notion of which we have forgotten how we obtained it," (Boltzmann, 1905, p.160/56) to wit, by starting from finitary operations and passing to the limit.

In modern terms, Boltzmann's mathematical atomism appears as constructivism, however with significant qualifications. He accepts actual infinities in pure mathematics or geometry, and, albeit 'suspecting' the principle of the excluded middle of entailing an inconsistency under certain circumstances, he nonetheless considers it 'convincing' for number theory – but not for the liar paradox that 'overshoots the mark'. Similarly, complete induction is justified by the fact that this 'whole mechanism' has so far held true in all mathematical experiences.[70] Boltzmann naturalizes these mechanisms and considers "the brain as an apparatus or organ for producing world pictures" and speaks of a "mechanism that has developed in the human brain." (both Boltzmann, 1905, p. 179/69) He even imagines the representation of psychic phenomena by a machine. In the end, an atomistic or mechanistic (but non-mechanical) theory of our laws of thought makes the constructivist reality criterion converge to the reduction criterion by singling out the basic entities of a universal theory. As Alois Höfler who had led so many discussions with Boltzmann in his *Philosophical Society*, would remark in his obituary: "Boltzmann was atomist until the impossible." (Höfler, 1906, p. 2) This sigh could have easily been echoed by Max Planck who shared Höfler's Kantian background and thus rejected any physicalist reduction of ethics and religion. (See Sect. 3.9.)

3.6. How Machian Was the Early Planck?

The relationship between Mach's positivism and Boltzmann's atomism provides the background of and, perhaps, the motivation for the polemic which Planck launched against Mach in December 1908. Planck's attacks surprise by their harshness, in particular, because in subsequent years he would hardly mention the names of those he criticized – see his protests against Exner's irreducible indeterminism (Sect. 4.5.) or against Pascual Jordan's basing free will upon quantum theory (Planck, 1936). Although in letters to Wien and von Laue, Planck denied any intention to "wound a worthy old man", he believed "to owe it to his convictions." (Cf. Thiele, 1968) Heilbron rightly wonders "how Planck believed that his answer would not wound Mach." In the rejoinder "Planck's inner compulsion pushed him over the limits then allowable in philosophical combat." (All Heilbron, 2000, p. 55) And Planck continued his inner dialogue with Mach's positivism throughout the years in all papers dedicated to philosophical questions; and thus he shaped the image of Machian positivism among German physicists.

> The exchange with Mach gave Planck the status of a philosopher. To him it did not represent a promotion: philosophy, he said, was arbitrary, and every man was entitled to choose his own; science was "obligatory", the same for all mankind, and more important in proportion to its large constituency. But his new status promoted him in others' opinion and opened a wide field of activity. "People complain" wrote theologian Harnack, "that our generation has no philosopher. Unjustly, they now belong to other faculties. Their names are Max Planck and Albert Einstein." (Ibid., p. 59f. using a letter from Planck to Harnack from 1914 and a statement of Harnack made in 1911).

[70] Cf. (Fasol-Boltzmann, 1990, pp. 167-171).

To be sure, theologian Harnack was not the standard representative of the *Geisteswissenschaften* of the day. In 1913 he became the founding president of the *Kaiser-Wilhelm Gesellschaft* and thus the head of several newly founded research institutes for basic and applied science.

The ardor of Planck's criticism is often linked to three factors: his final conversion to Boltzmann's statistical mechanics which, according to Kuhn (1987), occurred at about this time; Planck's initial sympathy for Mach's philosophy which proved an impediment for his own research; and more vaguely, Boltzmann's tragic death in 1906, that is, about the time when radioactivity and black-body radiation were beginning to turn the tide in favor of atomism.

Planck's personal relationship to Boltzmann, to be sure, never was an easy one. Ernst Zermelo whose recurrence paradox had sparked a heavy polemic against Boltzmann's statistical mechanics in 1896-1897 was Planck's assistant and, in a somewhat mitigated tone, Planck supported Zermelo's view. In the following year also Planck himself had a short confrontation with Boltzmann. It concerned irreversible radiation phenomena and took place in the Berlin *Sitzungsberichte* of 1898; Boltzmann showed that Planck's reasoning had been too general. Boltzmann's correspondence with Klein (Cf. Höflechner, 1994, p. II 279-281) reveals that he took the matter rather seriously until Planck published Boltzmann's rejoinder. Boltzmann commented to Klein that "as regards uprightness and fairness he [Planck] has grown in my opinion." (Ibid., p. 281) But their relation never became an easy one. After Boltzmann's death the University of Vienna made an offer to Planck to succeed Boltzmann, but Planck decided to remain in Berlin.

In his *Scientific Autobiography* written early in 1945, Planck describes their complex relationship. The passage quoted here contains pieces which Planck had already published in 1933 in a paper dedicated to "The Origin and Impact of Scientific Ideas", such that his frustration is not just an 86 year old man's regret. Right before the passage quoted here Planck sketched how energeticists compared the transition between two temperatures with the raising and lowering of a weight. On this account, endorsed by Mach as well, the second law could be derived without any reference to irreversibility and, as there existed only differences of temperatures, there was no absolute zero either. To Planck, the difference between reversible and irreversible physics was fundamental to all physics.

It is among the most painful experiences of my scientific life that I succeeded only rarely or yet, I am inclined to say, never in winning general approval for a new assertion for the correctness of which I could provide an entirely cogent but only theoretical proof. Thus happened also here [as regards the irreversible character of the second law of thermodynamics]. No one listened to all my good reasons. There was simply no way to prevail against the authority of men like W. Ostwald, G. Helm, and E. Mach. To be sure, I was entirely certain that my claim of a fundamental difference between heat conduction and the falling of a weight would ultimately prove to be correct. But it was annoying that I did not have the satisfaction that my assertion had prevailed, but that its general approval was brought about from an entirely different angle which was in no connection with the considerations by which I had justified my assertion, to wit, by the atomistic theory as advocated by Ludwig Boltzmann. …[71]

Thus the factual development of these matters amounted to a victory of my assertion …, but my involvement into this fight was completely superfluous; for even without it the about-face would have taken place just in the same way. …

[71] A passage very similar to this paragraph can be found in (Planck, 1933, p. 147).

After what has been said I could only play the part of Boltzmann's second [in the fight against Ostwald] whose service was, however, not acknowledged at all, yet not even welcome. For Boltzmann knew quite well that my viewpoint was substantially different from his. He was particularly annoyed that my attitude towards atomic theory, which represented the foundation of his whole research work, was not only indifferent but rather a bit negative. The reason was that at the time I attributed to the principle of increase of entropy the same exceptionless validity as to the principle of the conservation of energy, while in Boltzmann the former principle only appears as a probabilistic law which, as such, admits exceptions. There are cases in which the quantity *H* increases. In his derivation of the so-called *H*-theorem, Boltzmann did not deal with this feature, and my talented student E. Zermelo vigorously indicated the lack of a rigorous justification of the theorem. Indeed, in Boltzmann's calculation the assumption of molecular disorder was not mentioned, which is indispensable for the validity of the theorem. He seemed to have presupposed it as evident. At any rate, he cuttingly responded to the young Zermelo with a harshness that was also directed at myself who had approved the publication of Zermelo's paper. In this way it occurred that Boltzmann all his life, even at later occasions, both in his publications and in our private correspondence, maintained an irritable tone against myself which only in the last years of his life changed into a friendly assent after I had reported to him the atomistic justification of my radiation law.

After all it was evident for me that Boltzmann would ultimately prevail in the fight against Ostwald and the energeticists. The fundamental difference between heat conduction and a purely mechanical process was generally acknowledged. In all this I had the opportunity to recognize an – or so I believe – notable fact. A new scientific truth usually does not prevail in the way that its opponents become convinced and declare themselves informed about the change, but that the opponents die out one after another and that the new generation from the very beginning becomes accustomed to the truth.[72] (Planck, 1990, p. 14f.)

The last paragraph sounds as if copyrighted by Thomas S. Kuhn. As it was originally written down in 1933, one may wonder whether back then it did not also describe Planck's own position with respect to the younger generation of quantum physicists whom, to be sure, he generously supported despite his deep-seated reluctance to accept genuine indeterminism. Browsing through Kuhn's (1987) comprehensive study of Planck's works on black-body radiation, we see that real or alleged scientific revolutions are not necessarily created ex post in order to meet clashes with the opposition by suitable rhetoric. At least to Planck's mind, there occurred two conceptual ruptures. Immediately in 1900 he realized that he had discovered a new constant of nature which within his philosophical world view (See Sect. 3.8) signified an important step towards the ideal of absolute knowledge. But if Kuhn's account is right, the need for the quantum discontinuities – or the quantum revolution – occurred not before 1906 and only in 1908 Planck had understood how radical a change was required. This is the year at the end of which he delivered his Leyden lecture on "The Unity of the Physical World View".

In his rejoinder to Mach's response (1910), Planck (1910a) contended that in his Kiel years (1885-1889) he considered himself "to be one of the most committed followers of Mach, which as I freely acknowledge exercised strong influence on my physical thinking. But later, I turned away from it, because I had begun to see that the glittering promise …, the elimination of all metaphysical elements from physical theory of knowledge, could in no way be carried out." (1910a, p. 1187/142) Hiebert extends the time span back to Planck's dissertation in 1879. "His earliest reactions to Mach's views appeared in 1887 in an essay which merited the second prize of the philosophical faculty of Göttingen." (Hiebert 1967, quoted from Blackmore, 1972, p.

[72] The final two sentences appear in (Planck, 1933, p. 248) in a biblical wording.

218) Blackmore concludes that after the black-body theory had made Planck "a strong admirer and sword-bearer of Boltzmann", he "looked for and found a scapegoat who he thought was the primary cause for the unjust isolation and disdain which had been forced on Boltzmann's ideas for so many years by so many of his physical colleagues and which in large part had been responsible for his own fruitless investigations and wasted years." (Both Blackmore, 1972, p. 220) Stressing that the Leyden lecture was delivered fifteen months after Boltzmann's death, Kuhn rightly calls for qualifications. On the one hand, Planck overrated both his own failure and the merits of Boltzmann in convincing physicists of the independent character of the second law. On the other hand, in Planck's early works there are passages "not easily reconciled with his having taken a positivist position." (Kuhn, 1987, p. 279)

How Machian was Planck's 1887 *Preisschrift* on *The Principle of Conservation of Energy*? In a certain sense it stood in direct comparison with Mach's 1872 booklet *On the History and Origin of the Theorem of Conservation of Work* which, according to later declarations of the author, already contained the gist of the epistemology expounded in the *Mechanics*. And indeed, Planck made repeated references to it, while Mach's *Mechanics* speaks quite favorably of Planck's study. The introduction to the second edition of the *Mechanics* (Mach, 1988, p. 16/xxv) mentioned the *Preisschrift,* and in § 5.1 Mach considers Planck's formulation of the law of causality as "different only in form" (Ibid., p. 519/607).[73]

In the Preface to the first edition of the prize essay, Planck gives quotations from a letter accompanying his submission. There one finds several Machian passages, and affirmative references to Mach continue throughout the historically oriented first chapter of the booklet, in particular, as regards the epoch prior to Helmholtz. The principle of conservation of energy, according to Planck, is of such a general and universal nature "that one cannot be careful enough in purging it from all those hypothetical ideas which one is inclined to make up so easily in order to facilitate the overview over the lawful connection of the most diverse natural phenomena." (Planck, 1908b, p. viii) Thus Planck's presentation intended to base the principle primarily on purely empirical facts. The first reasonably precise formulation of a the principle was the impossibility of a perpetuum mobile which he describes as "a merely empirical fact, because humans more and more cared to gain work than lose it." (Ibid., p. 4) Huygens was "instinctively convinced" (Ibid., p. 6) of its correctness because it was so closely related to our primary experiences. Stressing Julius Robert Mayer's priority for the general principle and the definition of the mechanical equivalent of heat, Planck remained critical about Mayer's metaphysics-laden elucidation of the result. "Never could an effect emerge without a cause, or conversely no cause remains without effect. … Cause and effect thus are equal in a certain sense." (Ibid., p. 25) Yet when judging this train of thought one should not forget, so Planck, that in virtue of its generality the principle cannot be proven deductively in an ordinary sense, such that Mayer's elucidations are the best available source "to obtain a clear and intuitive understanding of the principle, that is, to link it with the ideas and principles familiar to us." (Ibid., p. 30)

We also find Mach's principle of historical continuity at work. In Helmholtz's hands "the principle of conservation of energy has now become parallel to the

[73] Interestingly, the broader context of the passage contains Mach's general plea for energeticism in the theory of heat.

principle of conservation of matter, a principle which we are familiar with already for a long time and which has become, as it were, part of our instincts." (Ibid., p. 41f.) But when discussing the mechanical world view expressed in Helmholtz's enunciation of the principle, Planck significantly departed from Mach.

From the validity of the principle [of conservation of energy] one can by no means deduce the necessity of the mechanical conception of nature, while conversely the principle indeed turns out to be a necessary consequence of this conception, at least if one assumes central forces. This latter circumstance together with the desire to form a coherent view of the action of natural forces sufficiently explains the fact that the mechanical theory was accepted so quickly and without objection. And indeed this theory has been confirmed everywhere with flying colors; or at least I do not presently share the fears as to whether this theory – as an overly narrow-minded conception of natural phenomena – is feasible in general. (Ibid., p. 57f.)

A footnote citing (Mach, [1872] 1909) leaves no doubt whose fears are meant. At the end of his historical tour, Planck discusses the acceptance of the mechanical theory of heat around 1860.

In these days, a new epoch for the development of natural sciences began. Until date one everywhere depended upon the inductive method unless one had already succeeded in finding those basic laws from which all single phenomena emerge, such as in mechanics or astronomy. From now on, one was in possession of a principle which, well-tested in all known domains by careful research, provided an excellent guide also for wholly unknown and unexplored regions. (Planck 1908b, p. 101)

Thus already in Planck's most Machian writing the goal of a unified world view is visible, a motive which would become one of the cornerstones of Planck's later philosophy of science. Admittedly, also Mach had emphasized the inescapability of a unified world conception, but rather than being stable this world conception was too provisional to derive any reductionist endeavors from it. To Planck, the idea to base all science on a few principles was the undisputed guiding star (See Sect. 3.8). Compare the beginning of Planck's 1887 booklet with the opening passage of his 1915 entry on "The Principle of Least Action" for the encyclopedia *Die Kultur der Gegenwart.*

There are two principles which serve as a foundation for the present edifice of exact science: the principle of conservation of matter and the principle of conservation of energy. Ahead of all other laws of physics however comprehensive, they hold an unchallenged precedence; for even the great Newtonian axioms … only extend over a special part of physics: mechanics. (Planck, 1908b, p. 1)

As long as there exists physical science, its highest desirable goal had been the solution of the problem to integrate all natural phenomena observed and still to be observed into a single simple principle which permits one to calculate all past and, in particular, all future processes from the present ones. It is natural that this goal has not been reached to date, nor ever will it be reached entirely. It is well possible, however, to approach it more and more, and the history of theoretical physics demonstrates that on this way a rich number of important successes could already be gained; which clearly indicates that this ideal problem is not merely utopical, but eminently fertile. … Among the more or less general laws which manifest the achievements of physical science in the course of the last centuries, the Principle of Least Action is probably the one which, as regards form and content, may claim to come nearest to that final ideal goal of theoretical research. (Planck, 1915a, p. 68)

Reading these emphatic lines one may safely consider the Principle of Least Action as the embodiment of Planck's unificationist methodology of science. As we shall see below, what in the Leyden speech had changed as compared to the 1887 position is that the unifying principles had become of a more abstract kind. Planck's later reductionist aspirations became redirected at formal theoretical principles rather than basic quantities, such as matter or energy, and accordingly were finally decoupled from the mechanist world view.

In the second, the systematic part of the *Preisschrift,* Planck's realism is clearly discernible. Initially, he started with a primary definition of energy that was not far from Mach's. But when contemplating whether a substantialist interpretation of energy based on analogy could be useful, he seems to have forgotten Boltzmann's above-quoted admonition against Ostwald's energetics which, to my mind, was a fair account of Mach's antireductionism. A "substantialist interpretation of energy not only increases the *Anschaulichkeit*, but also represents a direct progress in research" (Planck, 1908b, p. 117) because now one could further investigate the transitions from one form of energy into another. Planck was not after *Anschaulichkeit* in first place, be it based on energeticism or mechanics, but after unification.

Rejecting a mechanical deduction of the principle of conservation of energy as not general enough, Planck provided an indirect proof of it from the empirical impossibility of a perpetuum mobile which crucially depends upon "the assumption that it is always possible *in some way* to transform a material system from a given state into any other." (Ibid., p. 159) Through this condition, Planck's proof of this *empirical* principle heavily relies upon experiences *not yet made* and thus violates the basic presuppositions of Mach's empiricism. In the first section of the *Mechanics,* Mach had strongly criticized Archimede's geometrical proof of the law of the lever because it contained implicit assumptions about the determining conditions which despite the argument's elegance must have been previously intuited in nature. And Mach concluded that "the aim of my whole book is to convince the reader that we cannot make up *properties* of nature with the help of self-evident suppositions, but that these suppositions must be taken from *experience*." (Mach, 1988, p. 44/27) Thus Planck's search for deductive proofs ran counter to Mach's methodology, and while Boltzmann had emphasized the mutual completion of the deductive and inductive methods, already the early Planck exhibits a clear preference for theorizing.

> However swaying the number and significance of those inductive proofs [of the principle of conservation of energy] appears to us, probably nobody is so inveterate an empiricist not to feel the need for another proof which, built upon a deductive foundation, lets the principle emerge in its most comprehensive meaning from some even more general truths. (Planck, 1908b, p. 149f.)

No surprise to find a footnote in which Planck asserts not to agree with all the views outlined in Mach's 1872 study. (Ibid., p. 156) Although Planck emphatically rejects all attempts "to accept the mechanical theory as an a priori postulate of physical research", he nevertheless considers "the mechanical world view as the *goal* of research, a goal which will possibly and probably be reached." (Ibid., p. 155) It seems to me that Planck's mitigated mechanical reductionism was not so far from Boltzmann's views of theory reduction to basic entities and their interactions.

Considering the second law of thermodynamics as an independent principle, Planck at that time would however have rejected Boltzmann's reality criterion as insufficient because, beyond Planck's Mach-inspired dislike of atomism, it did not involve any principle of comparable generality and introduced irreducibly probabilistic elements. To the 1908 edition, Planck added two footnotes in which he insists on a strict separation between both laws of thermodynamics. While the first law can be derived from the principle of the conservation of energy, the second "until to date can be explained no other than by way of probabilistic considerations." (Ibid., p. ix) But science, or so Planck would continue to stress in various dialogues with Vienna Indeterminists, could not content itself with this state of affairs. The reason was Planck's Kantian conception of causality which can also be found in his prize essay right after the author has rejected aprioristic forms of mechanicism. "Natural science only knows one postulate: the principle of causality; for this is the condition of its existence." (Ibid., p. 155) This was far cry off from Mach's functional dependences which emerged out of our biological interaction with the physical world. And also Boltzmann had fought against bestowing the status of a necessary precondition upon what is just a practically successful habit of thought. Thus we see that Mach's above-quoted assent to Planck's concept of causality was only appropriate for Planck's criticism of Mayer.

3.7 The Planck-Mach Controversy

Let us look closer at Planck's Leyden speech. Since, at bottom, several core elements of Planck's attacks on Mach – above all the Kantian conception of causality – were already present in his early philosophy, the new anti-Machian thrust mainly required a change of emphasis within the diverging strands of Planck's early thinking and an elaboration of the realism based on formal principles and constants of nature. This broader perspective is useful not to be misled by the combatants' talking at cross-purposes in important respects.

At the beginning, Planck distinguished two mutually enhancing and correcting methods in science. Careful description in the sense of Kirchhoff and Mach, on the one hand, is confined to observations as the only legitimate basis of physics. Theoretical research, on the other hand, boldly generalizes particular results and seeks a conceptual unity in the manifold of experiences. While originally physical science had been divided along the lines of our distinct senses into acoustics, theory of heat, etc., modern theoretical physics amalgamated and unified many originally distinct domains.

Mach's reply, the "Leading Thoughts", emphasized that "certainly no one has any objection against [unifying systems in physics], least of all representatives of the *economy of thought.*" (Mach, 1910, p. 230/136) But Mach could never assent to reducing one domain of experiences to another; for instance, the *Mechanics* does not consider hydrodynamics and acoustics as part of mechanics proper. (Cf. Mach, 1988, p. 224/246) This was not to say that different senses corresponded to different physical worlds; in contrast "[e]very event belongs, strictly speaking, to all domains of physics which are separated only by a classification which is partly conventional, partly physiological, and partly historical." (Ibid., p. 510/596)

While Mach and the empiricist tradition defined the basic quantities of physics by reference to specific sensory experiences, such as heat and muscular effort, Planck diagnosed "that the human-historical element in all physical definitions has significantly diminished." (Planck, 1908a, p. 3) Today "temperature is theoretically defined by the absolute temperature scale which is taken from the second law of thermodynamics, in the kinetic theory of gases it is defined by the living force of molecular motion, practically by the volume change of a thermometric substance or by the deflection of the scale of a bolometer or thermometer." (Ibid., p. 3) Mach and the energeticists, to be sure, had rejected the grounds of both theoretical definitions. To Planck's mind, the whole development of theoretical physics was characterized "by the unification of its system which was reached by a certain emancipation from the anthropomorphic elements, in particular from the specific sense impressions." (Ibid., p. 4) Having achieved this emancipation for the second law of thermodynamics, was the "life work of Boltzmann" (Ibid., p. 14), while Mach's epistemology signified a relapse into an outdated anthropomorphism.

Planck outlined this process of simultaneous de-anthropomorphization and unification at the example of the reinterpretation of the second law. Carnot's cyclic processes did not lead to a precise notion of the irreversibility of the process of heat conduction because the whole problem was "too much tailored for human demands primarily interested in the gain of usable work. If one wants to obtain a definitive answer from Nature, one has to approach her from a more general viewpoint that is less interested in economy." (Ibid., p. 9) And Planck's clue to Nature's secrets was the separation between reversible and irreversible processes. Being "much deeper than, for instance, the opposition between mechanical and electrical processes, this distinction accordingly … will become the most distinguished explanatory reason [*vornehmste Erklärungsgrund*] for classifying all physical processes and finally play the lead in the physical world view of the future." (Ibid., p. 11) This distinction of processes corresponds to the distinction between the two basic principles in Planck's mature world view.

As Helmholtz had shown, all reversible processes are governed by the Principle of Least Action which, according to Planck's above-quoted commendation, comes closest to the ideal of scientific inquiry. Since this principle is more general than the principle of conservation of energy and makes possible a unique answer to all problems of reversible physics, Planck even claimed that this side of the physical world view could be considered as completed. "In the realm of irreversible processes, however, the Principle of Least Action is no longer sufficient because the principle of entropy increase introduces an entirely novel element into the physical world view that is in itself extraneous to the action principle." (Ibid., p. 11) Thus for Planck, Boltzmann's tenaciously associating the second law with the Principle of Least Action (See Sect. 3.2) was doomed to failure from the outset. Of course, Planck admitted that "the disadvantage of reversible processes is that they are merely ideal; in real nature there exists not a single reversible process because every natural happening more or less involves friction or heat conduction." (Planck, 1908a, p. 11) Nonetheless, he attributed a clear explanatory priority to reversible processes because they permitted a causal description of nature. More explicitly than Boltzmann, Exner would stress precisely the opposing view. (See Sect. 4.1)

While following Clausius' definition, the second law remained still associated with thought experiments based upon ideal processes, that is, upon the impossibility of a (de-anthropomorphized) perpetuum mobile, full emancipation of the second law as a true principle was achieved by Boltzmann's "general reduction of the concept of entropy to the concept of *probability.* ... Nature simply prefers more probable states to less probable states insofar as only transitions in the direction of increased probability occur." (Ibid. p. 14) But there is a prize to be paid for this progress, "the renunciation of a truly complete answer to all questions concerning the details of a physical process." (Ibid., p. 14)

A second disquieting drawback seems to be the introduction of two types of causal connection between physical states: absolute necessity, on the one hand, and mere probability, on the other hand. … If a warmer body loses heat to a colder body contiguous to it, this is only enormously *probable* and by no means absolutely necessary; for one could well imagine very particular arrangements and velocity states of the atoms for which just the opposite happens. Boltzmann has drawn therefrom the conclusion that such strange events contradicting the second law of thermodynamics could well occur in nature, and he accordingly left some room for them in his physical world view. To my mind however, this is a matter in which one does not have to comply with him. For, a nature in which such events happen … would no longer be our nature. As long as we have to do only with the latter, we will be better off not to admit such strange processes but, conversely, to search for that general condition – and assume it to be realized in nature – which excludes from the very beginning those phenomena which contradict all our experiences. Boltzmann himself has formulated that condition for gas theory [which excludes these phenomena], it is generally speaking the 'hypothesis of elementary disorder'. … By introducing this condition the necessity of all natural events is restored; for if this condition is realized the increase of entropy directly follows in virtue of the calculus of probability, so that one can nearly call *the principle of elementary disorder* the essence of the second law of thermodynamics. (Ibid., p. 14f.)

Planck's reasoning makes clear that despite the praise of Boltzmann, and the adoption of statistical mechanics, he does not admit the improbable events into physics. While the late Boltzmann admitted violations of the second law, Planck posited a general principle excluding this: elementary or molecular disorder. While Boltzmann contemplated that in disjoint regions of the universe time could run in different directions because one of them locally violates the second law – yet without touching the law's validity for the universe as a whole –, Planck assumes that such a subsystem with decreasing entropy is always coherent with another one like two light rays stemming from the same source. Thus the price for a categorical validity of the second law seems to be some spooky non-local entanglement, not unlike ideas popular in present-day interpretations of quantum mechanics. "Do these strange consequences not remind us of mysterious relationships in intellectual life which often remain concealed to us and can accordingly be neglected without loss, but which may exhibit effects wholly undreamt-of once particular external circumstances are concurrent." (Ibid., p. 17f.) To a Viennese ear, this treatment of molecular disorder had to sound metaphysical. Compare Mach's response.

I cannot deny my aversion to hypothetico-fictive physics. Thus I have developed my own particular opinion about Boltzmann's probability investigations concerning the second law as based on the kinetic theory of gases. If Boltzmann discovered that processes in accordance with the second law are very probable while those contrary to it are only very improbable, then I cannot accept that it has been proved that nature behaves according to this theorem [*Satz*]. Also, I don't think it is right for Planck to accept the first part without wanting to accept the second part, for both halves of the conclusion are inseparable from each other. (Mach, 1910, p. 231,137)

Of course, Mach rejected Boltzmann's proof of the probabilistic second law from hypothetical atoms and remained committed to phenomenological thermodynamics. Neither did he accept Planck's attempt to restore causality by banning the actual occurrence of improbable events through the condition of molecular disorder. Mach's worries about the probabilistic character of the second law did not stem from the problem to accommodate it into his wide notion of causality, but from his combined dislike of atomism and mathematical deductions in physics. Once the latter obstacles had been removed, Mach's conception of causality as sufficiently stable functional dependences came to buttress the probabilistic character of basic physical laws.

In the fourth section of his Leyden speech, Planck launched a vigorous polemic against Mach. It commenced from his joint belief – supported by the above historico-critical analysis of the second law of thermodynamics – that, on the one hand, modern physics had already become sufficiently disentangled from human economy and other anthropomorphic features, and that, on the other hand, our physical world view had reached a sufficient uniformity and stability. This progress, he concluded, was irreconcilable with Mach's epistemology. Planck targeted, above all, Mach's anti-realism and the principle of economy. Both are deemed fruitless maxims for scientific research. "By their *fruits* shall ye know them!" (Planck, 1908a, p. 24/132) – a biblical allusion which more than anything else was to provoke Mach. And it did.

> One sees that physicists are on the best way to becoming a church and they have already acquired the familiar means. Let me answer plain and simple: If belief in the reality of atoms is so essential to you then I renounce the physical way of thinking …, in short, I thank you for the community of believers. Freedom of thought is more precious to me. (Mach, 1910, p. 233/138f.)

Mach's rhetoric makes the issue of atomism appear more central than it actually was. Whether the old Mach really admitted to have seen atoms in the spinthariscope or not[74], his epistemology undoubtedly had problems to integrate theoretical entities above and beyond their use as successful hypotheses. Mach's attitude towards atomism, accordingly, was not at all different from his general position towards abstract principles, such as the Principle of Least Action or molecular disorder. Planck was of course right to observe that

> Machian positivism was a philosophical expression of the unavoidable disillusionment [of the exalted ambitions of the mechanical world view]. In the face of a threatening scepticism [prominently expressed in Emil du Bois-Reymond's *Ignorabimus*], he deserves full credit for having rediscovered that the only legitimate starting point of all scientific research is sense perception. But he overshoots the mark by degrading the physical world view together with the mechanistic one. I am firmly persuaded that the Machian system, when really carried through, contains no internal contradiction, but equally it seems certain to me that its basic significance is only formalistic. It does not really touch the essence of natural science at all, the demand for a *constant* world picture which does not depend upon the changing epochs and peoples. Mach's principle of continuity offers no substitute; for continuity is not constancy. (Planck, 1908a, p. 130)

From a Machian standpoint, Planck's quest for absolute constancy sounded tantamount to renewing the shattered absolute foundations simply by replacing the basic substances with abstract principles. Mach could just retort that Planck's "concern for a physics which is valid for all peoples and all times, up to the Mars dwellers, while so many physical problems of the day press upon us, seems very premature to me, indeed almost comical." (Mach, 1910, p. 232/136) Even the Mars dwellers, to Mach's mind, would have to care for their survival in the most economical fashion and develop their own science step by step.

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[74] See the recollections of Stefan Meyer translated in (Blackmore, 1992, p. 151f.). As a matter of fact, Mach saw α-particles at the newly founded Institute of Radium Research.

Planck's criticism of Mach's positivism distorted the anti-substantialist ontology of neutral monism into a Berkeleyian sensualism, holding "that there are no other realities than one's own sensations and that all natural science in the last analysis is only an economic adaptation [*Anpassung*] of our thoughts to our sensations by which we are driven by the struggle for existence …. The essential and only elements of the world are sensations [Empfindungen]." (Planck, 1908a, p. 20/129) Mach put Planck's wording right and countered with his famous slogan about the task of science: "*Adaptation of thoughts to facts and adaptation of facts to each other*." (Mach, 1910, p. 226/133f.) Contrary to Planck's belief, Machian *facts* were not isolated *sensations*, but they are constituted by relatively stable functional dependences between the (non-atomistic) sensational elements. While Mach's relational ontology avoided any absolutist commitments, to Planck's lights, an increased constancy of the world picture warranted stronger ontological conclusions. "This constancy which is independent of every human – especially every intellectual – individuality, is that which we now call the real [*das Reale*]." (Planck, 1908a, p. 22/131)

And in later years Planck did not hesitate to subject even the allegedly most Machian theory of modern physics to this general convergence to absolute reality. To his mind, relativity theory taught us that outdated absolute concepts are relativized just in order to find deeper absolute concepts. "Yet when space and time have been denied the character of being absolute, the absolute has not been blotted out, it has just been moved more backward, to wit, into the metric of the four-dimensional manifold." (Planck, 1925, p. 154) Interestingly, Planck derived support for convergent realism from Kant's critical philosophy. Since there is no way to distinguish between 'world view' and 'world', we can interpret 'world' itself as the ideal aim of all scientific research. Evidently Planck's convergent realism was fundamentally at odds with the highly flexible reality criterion used by Mach and the Vienna Indeterminists.

While in the Leyden speech, Planck mainly attacked the practical infertility of the principle of economy as compared to the quest for absolute knowledge, to serve as a maxim for the working scientist, the rejoinder to the "Leading Thoughts" regarded economy as an element of the practical life world. By Mach's "generalizing it without further ado, the concept of economy … is transformed into a metaphysical one." (Planck, 1910a, p. 1187/142) And Planck ironically reminded his readers that Mach had branded as metaphysical those "concepts of which one has forgotten how one had arrived at them." (Ibid., p. 1188) Answering criticism by Adler (1909), Planck rejected Petzoldt's (1890) redefinition of Machian economy as stability.

In reality, these two concepts are worlds apart from one another. For economy is inseparable from purposiveness [*Zweckmäßigkeit*] while the concept of stability has not even the slightest thing to do with purposiveness. One could just as easily make variability or capacity for evolution a demand of 'economy'. (Planck, 1910a, p. 1188/143)

We have seen in Section 3.2. that there was indeed an important cleft between both conceptions, a cleft in virtue of which Petzoldt's stability was a metaphysical principle while Mach's economy of thought was not. Thus Planck's charging the principle of economy of metaphysics, at bottom, hit only Petzoldt's objective reinterpretation of it. But Planck explicitly agreed with Adler that stability of our world view was a worthy goal of the scientific enterprise, a goal which could not be reduced to economy. And he thus implicitly assented to the Fechnerian metaphysical tendency standing behind Petzoldt's conception of stability. At bottom, Planck's convergent realism was a metaphysical position that substantially departed from a verbal reading of Kant's critical philosophy.

Turning from the ontological to the methodological aspects of Machian economy, in the above passage Planck criticized that this principle is in retrospect adaptable to whatever scientific progress. Planck was right in so far as Mach's usage of the principle of economy in his historico-critical writings was so all-encompassing that it could not be cashed out into set of precise methodological rules for the advancement of science. But Planck misunderstood the descriptive-normative nature of the principle of economy (Cf. Sect. 3.1). It is a biological-economical principle that factually governs the development of science from instinctive experiences onward. Only at later stages of the evolution of science, its application becomes regulative. Still then the principle of economy is no methodology guaranteeing success, so that Planck's criticism partly missed its target. Planck, on his part, considered the quest for a stable world view as the only reasonable advice to the working scientist.

> Therefore the physicist, if he wants to promote science, has to be a realist, not an economist, which means that in the flow of appearances he must search above all for that which is lasting, unchanging, independent of human senses. In this economy of thought serves as means but it is not a final purpose [*Endzweck*]. (Planck, 1910a, p. 1190/146)

Planck's rejoinder also contained two specific criticisms of Mach's physical works. Firstly, in the *Theory of Heat* Mach conflated both kinds of perpetuum mobile that are connected to the first and second law of thermodynamics respectively. "Mach does not even devote a syllable to the fact that the two basic principles about the impossibility of perpetual motion are completely different from one another, that the first is reversible …, but the second is not. … I must particularly stress that at the time Mach wrote his book (1896), the facts had already been made completely clear … forty years ago." (Planck, 1910a, p. 1189) Yet one must add that while for Planck this difference was the most important in present and future physics, a characteristic trait of energeticism had been to play down irreversibility at best and, accordingly, reject the idea of an absolute zero. Still in 1915, Mach called energetics "a fully seething field." (Mach, 1915, p. 18), at a time when Ostwald (1909) had long abandoned his aversion against atomism. Secondly, Planck assailed

> Mach's strenuously fought for but physically entirely useless thought that the relativity of all translation movements also corresponds to a relativity of all rotary movements, that therefore, one cannot decide at all in principle whether, for instance, the fixed stars rotate around the Earth at rest of the Earth rotates around the fixed stars. … The conceptual errors about physical matters which this unallowable transfer of the principle of the relativity of rotary movements, from kinematics into mechanics has already caused, … would lead us too far astray. It therefore naturally follows that Mach's theory cannot possibly account for the immense progress which is intimately associated with the introduction of the Copernican theory. (Planck, 1910a, p. 1189f./145)

The relativity of all rotary motion attacked here was the motivation for what became known as Mach's principle. And this relativist maxim proved extremely fertile for extending the theory of the solar system beyond Copernicus and Newton, because together with the principle of equivalence it became a cornerstone in the early history of general relativity. A passage in Philipp Frank's textbook *Philosophy of Science* demonstrates that contemporaries – or at least those who were as active in the field as Frank – were fully aware about the target and the inconsistency of Planck's position.[75]

> [Planck] had definitively approved Einstein's theory of relativity, but he regarded Mach's theory, according to which the rotation of the Foucault pendulum is due to an action emanating from the fixed stars, as a fantastic assertion which has its source in Mach's theory of knowledge. … However, Einstein started a new analysis of Newtonian mechanics which eventually vindicated Mach's reformulation. (Frank, 1957, p. 153)

Although Einstein later dismissed Mach's principle, still today it continues to attract considerable attention in its various forms.[76] Until the 1920s it also remained a research topic of the Vienna physicists (Cf. Thirring, 1918; see Sect. 4.7.2).

3.8. Formal Principles and Planck's Realisms

Let me turn back to the issue of theoretical principles, now in a more general setting. Planck's convergent realism as outlined in the polemics with Mach combined the tendencies of de-anthropomorphization and unification. Universality and simplicity outscored all *Anschaulichkeit.* With respect to the high demands which special relativity posed to our abstractive capacities, Planck concluded that the "measure of success of a new physical hypothesis is not its intuitiveness [*Anschaulichkeit*] but its efficiency. Once the hypothesis has proven fertile one gets used to it, and a certain intuitiveness arises little by little." (Planck, 1910b, p. 36) And in his inaugural speech as Rector of the University of Berlin he provided an impressive list. "[I]n all recent conflicts [between facts and theories] the great general physical principles held the field, namely, the principle of conservation of energy, the principle of conservation of momentum [*Bewegungsgröße*], the Principle of Least Action, the laws of thermodynamics," (Planck, 1913, p. 44) while well-accustomed intuitive foundations had to give way. This notion of principle was diametrically opposed to Mach's directly intuited principles, e.g., of the lever or the inclined plane.

If Planckian principles were to suit the quest for absolute reality, they had to be of a sufficiently abstract kind and be adaptable to scientific progress. Otherwise they would face the classical objection that in the course of history almost all successful scientific theories have turned out to be false. This seems to me the reason why the Principle of Least Action and constants of nature play such a pivotal role within Planck's philosophy of physics. While the latter provide something unchangeable and really absolute, the former corresponds to a kind of structural realism rather than to a world formula in the sense of the old mechanical reductionism. More than previous advocates of the Principle of Least Action, Planck, rightly, emphasized that only after a precise mathematical specification of the Lagrangian and of the conditions for the virtual displacements the principle ceased to be "an empty form" (1915a, p. 70) and became at all meaningful. Different physical theories correspond to different

[75] See also (Norton, 1995, p. 36f.)

[76] Today Mach's principle is rather an umbrella term for a motley group of different philosophical and physical principles. See the list on p. 530 of (Barbour & Pfister, 1995).

Lagrangians, and if certain types of constraints, focal points, or caustics are present one has to be extremely careful not arrive at inconsistencies.

This role as an invariant but 'empty' form corresponds to that type of structural realism launched by the French conventionalists Poincaré and Duhem.[77] Structural realism became a common theme of the neo-Kantian Cassirer (1910) and the pre-positivist Schlick (1920). "Theories in the exact sciences … can inform us about the real structural relations though not about how these structural relations are realized." (Gower, 2000, p. 92) Barry Gower has shown that there existed a structuralist tradition from Richard Dedekind to David Hilbert according to which "[n]o particular physical system need be determined by a scientific theory, so understood, and it does not provide information about any physical system in which the structure is realized." (Ibid., p. 75) As a matter of fact, Hilbert's trust in the universality of the Principle of Least Action and his pursuit of formal invariants even exceeded Planck's (Cf. Stöltzner, 2003b). To be sure, structural realism so conceived heavily draws upon the motive of unification, but – other than for Hilbert – the theoretical physicist's job is not finished by singling out the mathematically deepest level, an axiom system or a set of invariants which holds true equally for 'points', 'lines', and 'planes' as for 'tables', 'chairs', and 'beer mugs'. Constants of nature and the metric of space-time thus supplemented Planck's structural realism as presumed final, or at least more absolute, elements instantiating the universal structures. Hence, Planck's structural realism incorporated a non-substantialist type of convergent realism. One reason for this joint strategy was that Planck, other than Hilbert, did not assign any ontological qualities to mathematical truths. To him mathematics was, at least partially, "an empirical science about intellectual culture." (Planck, 1914, p. 55) While such a formulation still agreed with an empiricist foundation of mathematics in the style of Mach and Boltzmann, in the following year, Planck insisted on a principal difference between mathematics and physics. Unlike physical theories, mathematical theories cannot contradict one another, "such that in mathematics one cannot speak of an opposition of theories, but only of an opposition of methods." (Planck, 1915b, p. 79)

Let us follow Planck's combination of structural and convergent realism in more detail. As had Boltzmann, Planck emphasized that the Principle of Least Action is stronger than the principle of energy conservation, but full clarity is obtained only in relativity theory where this principle "contains all four world coordinates in fully symmetrical order" (Planck, 1910b, p. 38) and is invariant under Lorentz-transformation, while energy and momentum are not. Thus, the Principle of Least Action unites the energeticist view of nature based on the conservation of energy and the mechanical view of nature based on the conservation of momentum, a unification in virtue of which the principle was enthroned over all reversible physics. Planck's reverence for its universality rehearsed the 1887 praise for the principle of conservation of energy, but additionally gave it a mathematical twist. "The fundamental importance of the Principle of Least Action became generally recognized only when it proved its applicability to such systems whose mechanism is either completely unknown or too complex to think of a reduction to ordinary coordinates." (Ibid., p. 76) In contrast to the differential equations of motion, the Principle of Least

[77] Structural realism is a position commonly associated with Poincaré. Here I follow Gower's (2000) analysis that also Duhem's notion of natural order can be interpreted in such a way.

Action as an integral principle is independent of any choice of coordinates and *a fortiori* invariant under coordinate transformations.

Planck's emphasis on invariance extended to other fields as well. In 1910, he admitted that his law of black-body radiation required a fundamental break with classical electrodynamics in favor of an elementary discontinuity in nature because classical physics unavoidably yielded Jeans's law, in blatant contradiction even to everyday experience.

In my opinion, one will not for this purpose have to give up the Principle of Least Action, which has so strongly attested its universal significance, but the universal validity of the Hamiltonian differential equations; for those are derived from the Principle of Least Action under the assumption that all physical processes can be reduced to changes occurring continuously in time. Once radiation processes do no longer obey the Hamiltonian differential equations, the ground is cut from Jeans's theory. (Planck, 1910c, p. 239)

Apparently, Planck considered the applicability of the Principle of Least Action to discontinuous functions as a major virtue. Such functions had indeed become an important source of progress in the genuinely mathematical development of variational calculus; and also Boltzmann had been very interested in Weierstraß functions for the H-theorem (See Sect. 3.5.).

Except for a statement made much later in the context of science and religion[78], Planck was at pains to avoid any smack of teleology however provisional or heuristic within his cherished Principle of Least Action, even though, to his mind, it led to a certain extension of the concept of causality.

Who sticks to the principle of causality alone will demand that causes and properties of a motion can be made comprehensible and deducible from earlier states regardless of what will happen later on. This appears not only feasible, but also a direct requirement of the economy of thought. [sic!] Who instead seeks for higher connections within the system of natural laws which are most easy to survey, in the interest of the aspired harmony will from the outset also admit those means, such as reference to the events at later instances of time, which are not utterly necessary for the complete description of natural processes, but which are easy to handle and can be interpreted intuitively. (Planck, 1915a, p. 71-72)

In mathematical physics, for instance, one keeps redundant variables in order to maintain the symmetry of the equations. Similarly for the Principle of Least Action and its kin, "[t]he question of their legitimacy has nothing to do with teleology, but it is merely a practical one." (Ibid., p. 72) This was an interesting mixture of pragmatism and pursuit of higher unity.

Let me turn to the constants of nature and Planck's convergent realism. His quantum of action and Boltzmann's constants characterizing thermal radiation plus the gravitational constant provide a universal system of units that does not depend on convention. "By them it is possible to define units of length, time, mass, temperature which necessarily remain valid for all times and for all cultures including

[78] The Principle of Least Action, according to the late Planck, introduced the *causa finalis* into physics, but the teleological and the causal approach represented only different mathematical forms of the same fact. However, in a religious perspective it was important that there existed an objective regularity "which admits a formulation that corresponds to purposive action. This represents a rational order of the world, to which both nature and man are subjected to." (Planck, 1937a, p. 303)

extraterrestrial and extrahuman ones." (Planck, 1908a, p. 16). Two years later he provided a longer list with respect to special relativity.

> If … the notion of a mass point that has hitherto been assumed as fundamental loses the properties of constancy and invariability, what then is the truly substantial [*Substantielle*], what are the invariable building blocks from which the edifice of the physical world is composed? … The invariable elements of the system of physics based upon the principle of relativity are the so-called *universal constants:* in particular, light velocity in the vacuum, the electric charge, and the rest mass of the electron, the "elementary quantum of action" gained from thermal radiation …, the gravitational constant, and probably many others. These quantities have a real meaning insofar as their values are independent of the constitution, the position, and the velocity state of an observer. (Planck, 1910b, p. 39)

Both invariances are of a different kind. In the first case, Planck praised the invariance under change of scale, that is, that due to these fundamental constants there exists a way to define the length independent of the ell measure of the present king. In the second case, the invariance is weaker and holds only with respect to relativistic transformations between observers. The most fundamental type of invariance appeared in Planck's encyclopedia entry dedicated to "The Mutual Relation of Theories". Not only did the abstract principles always prevail in the changes of physical theory, but each major step towards the ideal aim of absolute knowledge uncovered a hitherto unknown constant of nature. First, "[t]he modification brought into mechanics by the principle of relativity contains as its essential part the introduction of a new universal constant alien to classical mechanics, the velocity of light in vacuum." (1915b, p. 82)

> [Second,] the initially stark opposition between dynamics and theory of heat was overcome by the principal renunciation of the assumption of absolute lawfulness in all thermal and chemical phenomena, combined with the introduction of the atomistic approach which operates with a number of new characteristic constants of nature, the atomic weights. … But [the sacrifices of dynamics] are probably not over with the discontinuity of matter. The laws of thermal radiation, specific heat, electron emission, radioactivity unanimously indicate that not only matter itself but also the effects originating from matter … possess discontinuous properties, which once again is characterized by a new constant of nature: the elementary quantum of action. (Ibid., p. 83f.)

After a long-winded development (Cf. Kuhn, 1987) Planck, in those days, had finally accepted discontinuity although the quantum remained an alien element within his semi-classical theory. But he called for further unification. No doubt, in a different form and under a different name, the quantum would remain "an integrating part of a general dynamic." (Planck, 1915b, p. 84) In 1915, the principle of elementary disorder that had been the linch pin of his criticism against Exner in the year before (See Sect. 4.5.) had disappeared from his account. Planck cited Brownian motion as direct evidence for statistical oscillations of an equilibrium state. What, however, remained forever a core element of his thinking was the priority of dynamical laws over merely statistical ones. Instead of molecular disorder, he continued to insist that statistical regularity and probability calculus are "based upon the determination [*Festsetzung*] of equally possible cases. The situation is not changed by putting these determinations into the definitions, as is for instance done when developing probability calculus from the notion of collective." (Planck, 1937b, 316f) Planck's insistence shows how closely related, in effect, empiricism and the relative frequency interpretation were in those years. Throughout the years investigated by the present study, Planck remained a Kriesian in the interpretation of probability.

3.9. Mechanics, Mechanicism, and Culture

The polemics between Planck and Mach sparked wider philosophical circles. Opponents and followers of Mach took side. On Mach's side there were Petzoldt, Adler (1909), and Frank (1910). At the beginning of his rejoinder, Planck (1910a, p. 1186) noted that he had received positive reactions from neo-Kantian philosophers. And also his former student Moritz Schlick would take Planck's side until the 1930s (See Sect. 7.1. & 7.2.). Quite interesting is Ostwald's short review published in his own *Annalen der Naturphilosophie*. Ostwald who three years before had given up energeticism in favor of atomism, accepted the tendency of abstraction which Planck considered as the core tenet of modern physics but he rightly reminded his readers of Mach's basic intention to provide a unified methodological view for the whole of empirical science that did not halt at disciplinary boundaries. Compare Mach's remark in the "Leading Thoughts": "Physics does not own the entire world; there is *biology* as well and it is part and parcel of the world view." (Mach, 1910, p. 237)

> Let us imagine that the ideal world picture intended by the author [Planck] has been completed. It will inform us about all observable physical phenomena insofar as it permits us to calculate them in advance from the given data. … But what will this world picture tell us about the biological, physiological, and psychological facts? Obviously nothing, and the more exactly nothing the more perfectly it is elaborated in the sense of Planck's exposition. For the elimination of these other elements is (with complete justification) the duty of the creator of a *physical* world picture. (Ostwald, 1911, p. 105)

Mach's antireductionist stance was not only a consequence of his epistemological holism, but also of neutral monism as outlined in *The Analysis of Sensations*. "If there is no essential difference between the physical and the psychical, we shall hope to trace the same exact connection, which we seek in everything that is physical, in the relation between the physical and the psychical also." (Mach, 1918, p. x/xli) Consequently, the *Mechanics* ended with a section dedicated to "The Relations of Mechanics to Physiology."

> A philosophy is involved in any correct recognition of the subsumption of special knowledge under the great body of knowledge at large – a philosophy that must be demanded of every special investigator. The lack of it is asserted in the formulation of imaginary problems, in the very enunciation of which, whether regarded as soluble or insoluble, flagrant absurdity is involved. Such an overestimation of physics, in contrast to physiology, such a mistaken conception of the true relations of the two sciences, is displayed in the inquiry whether it is possible to *explain* feelings by the motions of atoms. (Mach, 1988, p. 521/610)

Among Mach's targets was probably also Boltzmann who in his 1900 Leipzig inaugural address had developed an all-encompassing conception of mechanics grounding all natural sciences, medicine, and the intellectual realm. Advocating a mechanical interpretation of Darwin's theory, Boltzmann extolled that now "we can explain the genesis of the concept of beauty, just as that of the concept of truth, in terms of mechanics." (Boltzmann, 1905, p. 314/134)

> It is of great advantage to united action in peace and war if young men are inspired to great and noble things, friendship and love, freedom and patriotism, but how easily this drive degenerates into empty phrases…. In this way, we understand from mechanical reasons why one youngster is aglow with the poetry of Schiller while many condemn the poems of Heine, which nevertheless have a powerful and irresistible influence on others. (Ibid., p. 315/134)

There always exist antagonistic tendencies which remain in due equilibrium: between Schiller and Heine, between the conservatives and the emancipated, between wealth and the mortality of the rich. Most interestingly, Boltzmann was quite reluctant to introduce his indeterminist ideas into these considerations, and the stationary equilibria do not emerge from chance but are mechanically maintained. Exner's theory of culture will be more radical in this respect.

Mach was loath to reduce culture to mechanics. In a small booklet titled *Culture and Mechanics* which appeared in the year before his death, Mach attributed to the biological and physiological a priority over mechanics and culture of a much more general kind than within the adaptationist program prevailing in the *Mechanics*. Already there Mach had described the continuous evolution from man's first instinctive experiences to craftsmanship as stepwise economical adaptations and a symbolical transfer – by means of language, functional dependences, and theories – of the skills to posterity until the age of modern science, when this knowledge was systematized and methodically extended. But while back then Mach had focused on the various facts and theories, *Culture and Mechanics* reached back past the starting point of the *Mechanics* and investigated the instinctive origin of tools, weapons and machines, the material prehistory of culture in general.

The prehistory of mechanics begins in the age of slavery by subsuming the forces of many individuals under a common goal. The first technical inventions thus consisted in "mimicking and simultaneously multiplying the work of the human hands…. The sweeps of a gristmill are, as it were, hands arranged continuously." (Mach, 1915, p. 16) The wheel and the nut and the screw (figure 3) developed instinctively from our sense of touch. Even the intuition of pure metals emerges necessarily once the appropriate ores happen to get into the fire. "As in a child, in the case of need again and again reminiscences pop up like a flash – need, unabashed force, almost automatically makes use of everything that is in an individual – thus identical things came off simultaneously at different places, but in most different ways." (Ibid., p. 27) What is more, discoveries are even enforced by the physical conditions. Once "magnetite, pyrite, argillaceous earth, lime and coal" (Ibid., p. 80) are together at disposal, the development of the burning oven, of ferrous metallurgy, and even of nitroglycerin results from a continuous sequence of accidental discoveries and instinctive experiences.

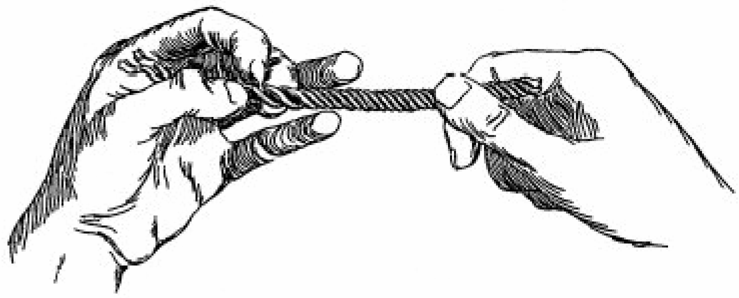

Fig. 3. The screw sensed on a rope (right) and the nut sensed by the finger nails (left).

Most startling, however, is that Mach considered this evolution of material culture as necessary for the development of our present stage of culture and as more basic than all theoretical insights. To prove this thesis he imagined a large-scale thought experiment.

> Let us imagine that during one night all our possessions [tools, machines, bridges, etc., taken over from past generations] would be lost; to be sure, knowledge, skills, experiences would have remained, but any connection between yesterday and today would be missing, nothing, absolutely nothing would be at our disposal – we would be in dire need. Wouldn't we be forced to improvise, as during infancy, the hammer by a stone, and wouldn't we run in a circle of hopeless embarrassments despite all our knowledge? We would have to start anew!
>
> Certainly, our knowledge, our intellectual heritage, would spare us millennia of detour and going astray, but … what would be the use of Michelson's analysis of the sunken meter in wavelengths of the cadmium-line – wouldn't we have to take the trouble to modestly carve our first screws from wood …? With the wooden screw in our hand … we would see the real and inexorable demand of continuity, which cannot be abridged by any wit or knack, that lies between the primitive start, the improvement and the final completion.
>
> We would have to build machine after machine, in a long and uninterrupted chain, one improving and complementing the other, each one would have to run and produce for a certain time, filling its place in the chain in order to bring about the final goal. (Ibid., pp. 84-85)

For later readers, this argument was probably as stunning as Boltzmann's atomistic explanation of poetry. Both ideas, and Mach's entire booklet, remained practically unmentioned during the further phases of Vienna Indeterminism although most protagonists of this tradition, and its opponents, actively involved themselves in matters of culture or society.

Notice two peculiarities of Mach's thinking.. First, although physics represents the formally most developed scientific theory, it does not occupy a preferred position either in a methodological or ontological respect. With due qualifications concerning ontology and his Machian methodology, Boltzmann put atomistic first in both respects. Planck similarly declared physics as the paradigm of all science, but in contrast to Boltzmann and following the Kantian model he strictly distinguished empirical science from the noumenal realm of ethics and religion. They were not hermetically separated but determinism and free will complemented one another as did knowledge and action. Although Logical Empiricists would reject this distinction as metaphysical, they nevertheless accepted the primacy of physics and demanded that psychology follow similar methods. Physicalism would acquire two different meanings in the Vienna Circle: while Carnap required a reducibility of all basic statements to statements of physical science, Neurath settled for requiring that only expressions about spatio-temporal phenomena occur in them. This did not mean that neutral monism or the reduction to observational elements disappeared, it just became detached from the causality debate until questions about quantum measurement arose.

Second, what completely disappeared after Mach and Boltzmann, was the biological and physiological basis of science. In particular, the principle of economy developed into a principle of simplicity and accordingly moved from biology into the field of language. This development already started with Exner, who also devoted more space to the intellectual and cultural realm than any other advocate of Vienna Indeterminism.

4. Exner's Synthesis

On September 8th, 1906, the morning edition of the *Neue Freie Presse* published two obituaries of Boltzmann on its front page. After a sketch of Boltzmann's scientific career, Ernst Mach emphasized that despite his myopia the deceased was "almost unparalleled as an experimenter". Moreover, Boltzmann combined extraordinary mathematical abilities with a complete knowledge of the literature. Discussing "Boltzmann's lifework", the experimentalist Franz Serafin Exner focused on the kinetic theory of gases and the atomistic world view, in which Boltzmann "found the best mainstay in the struggle against the lately popular, but unclear ideas of energeticism, which are propagated in particular by Ostwald and his followers. Against all these theories which signify, in effect, a step backward, Boltzmann fought a stubborn, but righteous and meritorious struggle in which his sharp mathematical weapons always led him to victory."

When roughly a decade later, Exner in his capacity as secretary general of the Austrian Academy of Sciences wrote an obituary for Mach in its *Annalen*, the tone was less emphatic.[79] He emphasized that Mach's most important scientific achievement were his historical-physical studies which laid the ground for his international fame.

> Two moments guided his historico-critical studies: to point out how during the course of the historical development of science the laws of nature are found and formulated by those who discovered them; moreover to show the way how this formulation can be carried through by comparison and analogy with already known and familiar laws, with a minimal expenditure of work by avoiding anything superfluous or unnecessary which only entices the wrong track of false pseudoproblems. By eliminating all that the investigation of which has no sense, there emerges more clearly what the single sciences can really investigate: the manifold universal [*allseitige*] inter-dependence of the elements. This economical principle of science was one of his leading thoughts. (Exner, 1916, p. 333)

Exner was not a blind propagandist of Boltzmann, even though in his Curriculum for the Austrian Academy of Sciences he recognized the "valuable stimulations" (1917, p. 9) he had received from Boltzmann during his first Vienna period.

What Exner achieved – so the present chapter argues – was that particular synthesis between Boltzmann's and Mach's thinking which Frank and Schrödinger diagnosed in their recollections as the local philosophical creed prevailing among the Vienna physicists. He did so by strengthening the empiricist traits in Boltzmann's thinking and supplementing his late indeterminism with a more suitable interpretation of probability. Boltzmann had initially studied Mach's epistemology against the backdrop of the mechanics-energetics controversy, and it played an important role within his defense of the kinetic theory of gases. To Boltzmann, mechanics, after having overcome the old materialist approach, was an all-encompassing picture even applicable to Darwinism and culture. Exner, too, was a physicalist but he understood physicalism in a broader and less reductionist sense than Boltzmann. This placed him half way between Boltzmann's atomism and Mach's antireductionist naturalism. As we see in his obituary, Exner read Mach almost exclusively as a physicist or a

[79] This difference should not be exaggerated; the front page of an newspaper and the annals of an academy represent very different rhetorical contexts.

philosopher of physics, and thus from Exner on the key role of biology and psychology in justifying scientific theories disappeared from the tradition of Vienna Indeterminism.

Exner's return to the wider Machian horizon made possible what represents, to my mind, his most important contribution to Vienna Indeterminism. In his 1908 inaugural address as Rector of the University of Vienna, he integrated the relative frequency interpretation of probability into a firm empiricist outlook to obtain a better foundation for the irreducible indeterminism characteristic of the late Boltzmann. On Exner's account, exact laws were just the macroscopic limit of purely random events at the microlevel. This signified a major step of abstraction. Neither Machian elements nor pictorial atoms were the ontological basis of scientific theory, but events and processes. This move led to a substantial increase in the flexibility of scientific theorizing.

Exner's new way of interpreting Boltzmann's indeterminism was not just a matter of being familiar – in contrast to Boltzmann – with the philosophical context of Fechner's thinking into which the frequency interpretation was deeply embedded. Rather than being a natural consequence of pursuing the descriptivist ideal in physics as defended by Mach and Kirchhoff, a genuinely statistical world view was germane to psychology and the social sciences in general. Bridging the gap between the sciences and the humanities and integrating the idea of indeterminism as defended by the late Boltzmann into an indeterminist world view required such a broadly-minded physicist as Exner. A polymath in many respects and driven by a great veneration for the arts, in Exner a third guiding influence can be discerned, Alexander von Humboldt's physical description of the world. All this is the agenda for the first two sections of the present chapter.

Beyond the thematical aspects, Exner's personality (Section 4.3) played a crucial role in disseminating what – before the causality debates began at about the mid 1920s – could have been regarded as a mere syncretism of a philosophizing physicist. Exner's synthesis and his genuine indeterminism became the credo for a whole group of younger Viennese physicists. Exner neatly fitted into an institution that was both more philosophically-minded – inspired by the local philosophical tradition of Mach and Boltzmann – and more cohesive on the personal level than other comparable physics institutes in the German-speaking world (Section 4.7). Thus Exner and his circle became the connecting link between the late Boltzmann and the second half of the tradition to be investigated in later chapters.

No surprise that Exner's introductory *Lectures on the Physical Foundations of the Natural Sciences* written down during the years of the war contained a long philosophical chapter "On Natural Laws" in which his empiricist and indeterminist theory of causality was elaborated at considerable length. The last section of that chapter represents his rejoinder to the criticism which Planck, right at the beginning of the war, had directed at Exner's Inaugural Address. Section 4.4. is dedicated to the *Lectures*, and Section 4.5. reconstructs the dialogue with Planck. In core philosophical respects, empiricism and causality foremost, this dialogue continued the polemics between Planck and Mach, and it would in turn be continued by Exner's former student Schrödinger, after he had become Planck's successor in Berlin (See Sect. 6.3.5) and when he criticized Schlick's ignorance of Exner (Sect. 7.4.).

When right at the end of the war, Spengler's *Decline* appeared and quickly spellbound the German intellectual milieu. Spengler severed the bond Humboldt's *Cosmos* had woven artistically between the empirical world and its internal mirror image in the individual. The harmonic picture of Nature was torn into pieces. The humanist Exner was prompted to react in a twofold way. Accepting Spengler's pessimism regarding the arts, he emphatically defended the objectivity and constant growth of the sciences against the relativism of *Decline*. This two-tired response for and against Spengler was based on Exner's particular version of indeterminism and historical causality. At the end of his life, Exner even embarked onto revitalizing – or rather extending – the Humboldtian project against Spengler by writing a comprehensive indeterminist theory of culture, within which culture comprised everything from the formation of the stars until Western civilization. Section 4.6. shows more than anything else how inappropriate the picture of the German cultural milieu drawn by Forman is for the local Viennese context.

4.1 The Inaugural Address and Its Context (1908)

At noon October 15th, 1908, Exner delivered his inaugural address as Rector of the University of Vienna "On Laws in Science and Humanistics." His objective was nothing less than to oblige his colleagues from all faculties to pursue the same goal, "the study of truth, of that objective truth that exists unaffected by human sentiment and thought." (Exner, 1909, p. 3) Against the standard argument – at least since the times of Wilhelm Dilthey – in favor of a distinct methodology for the *Geisteswissenschaften,* Exner rejected any absolute methodological difference between the sciences [*Naturwissenschaften*] and humanistics and instead ascribed the factual differences between both realms of knowledge to a difference in the objects studied by them. This intention was also reflected by the Rector's somewhat idiosyncratic terminology. The German original "Humanistik" refers to "humanistisch" which is not only the adjective to "Humanismus" (humanism), but also to the Greek and Latin classics and the type of gymnasium centering around them. Benndorf's obituary relates that Exner "remained an unconditional supporter of humanistic education and held that nothing proves better the necessity of humanistic studies than the attacks put forward by its adversaries." (Benndorf, 1927, p. 398).[80]

Exner's unifying move, of course, immediately raised the classical question as to why the humanistic disciplines – in contrast to the sciences – never formulate mathematically precise and universal laws, but obtain weak regularities at best. Surprisingly, the Rector addressed exactly the opposite question and investigated why physics was at all able to obtain strict laws. He asserted that all processes are processes in nature, be they biological, historical, economical, or linguistic ones. Thus they fall

[80] After some illuminating remarks about the rather recent origin of the English "humanities", Hiebert (2000, p. 10) translates "Humanistik" as "the humanities". In contrast to the German "Geisteswissenschaften" this wording captures a key aspect of what Exner was after. Still, although the pair "science–humanities" does not coincide with the German "Naturwissenschaft–Geisteswissenschaft", it is typically associated with a certain methodological dualism of the sort Exner set out to reject. What has finally conduced me to concoct the translation "humanistics" for "Humanistik" (instead of "humanities") is that both terms sound equally queer and cannot be found in a comprehensive vocabulary of English and German respectively. As a matter of fact, Exner used "Humanistik" only in his Inaugural Address.

under the laws of physics whose common basis is Boltzmann's atomism. But Exner combined Boltzmann's physicalism with Mach's radical empiricism, thus finally severing the former's bond with the mechanistic world view. The sciences' common quest for truth notwithstanding, he held that "these laws do not exist in nature, only man formulates them and avails himself of them as linguistic and calculatory means." (Exner, 1909, p. 7) From random collisions between atoms or molecules the kinetic theory of gases yields the laws of phenomenological thermodynamics in the limit of very, very many particles. Having adopted Mach's flexible and more general notion of causality, Exner was able to apply Boltzmann's probabilistic approach to any kind of natural process. The strict laws physicists observe emerge as the macroscopic limit of a very large number of random single events. And Exner went still further: in the molecular dynamics of a gas "we even observe regularities which are brought out exclusively by chance." (Ibid., p. 13)

To render meaningful such a genuinely probabilistic approach, two points have to be observed. First, since laws justified on the basis of random micro-events hold strictly only in the limit of infinitely many single events, there cannot exist any absolute law, but only average laws, the probability of which is so high "that it equals certainty for human conceptions," (Ibid., p. 16) that is, the statistical fluctuations stay below the threshold of measurability. Moreover, laws of nature can change on the cosmological scale without us detecting them in short-time experiments in the laboratory – an idea that reached back to Fechner. Second, "where the random single events succeed one another too slowly there can be no talk about a law." (Ibid., p. 14) This is the case not only in humanistics but also in the descriptive sciences, regardless whether they concern living beings in biology or inanimate matter in geology. In principle, any discipline is able to reach relatively strict average laws during its development over eons upon eons. But within the long period needed for these laws to stabilize as statistical equilibria, the boundary conditions for the random single events typically change in such a way that the limit of infinitely many events changes too. Thus, the descriptive sciences can only reach weak regularities that explicitly take account of the constantly changing circumstances and complex boundary conditions. Biological laws, for instance, hold only *ceteris paribus,* that is, if the physical and chemical milieu does not change too drastically.

The second point clearly shows that Exner already argued on the basis of the relative frequency interpretation of probability. For, only if existence and uniqueness of the limit of relative frequencies is ensured for each randomly chosen subset of the set of single events, these events form a statistical collective and make it possible to define probability as this limit. This was a move Boltzmann had never made, but Exner was well prepared to do so already in 1908 (See Sect. 4.2.). When throwing two dice sufficiently often we see that – so Exner illustrated this approach – the *more probable* numbers of spots occur *more frequently.* This manifests the law of large numbers that is "unprovable but taken from the thousandfold experience of men and constitutes the basis of probability calculus. As the one and only law it indeed governs all happenings in nature." (Ibid., p. 19) Looking around us, we at first do not discern any lawlike regularities, but rather that all natural processes are directed. Thus, to Exner's mind, the second law of thermodynamics becomes the basic principle in nature. Boltzmann "was the first to give a definite and clear interpretation of this direction …, showing that the world ceaselessly develops from less probable into more probable, and hence more stable, states." (Ibid., p. 9f.) Also mankind as a biological species is part of the "development from the more uniform hence less probable state of the single cells…, to the more probable state of the differentiated species and genera. The constancy of our species within surveyable times indicates that rather probable states have already been reached." (Ibid., p. 33)

Indeed, the second law of thermodynamics entails that ordered states exhibiting symmetries or other structural features are less probable than disorder. Any meaningful application of the second law, however, requires a precise definition of the respective measure of order. While this is easily possible in statistical mechanics by partitioning phase space, the above-mentioned biological example shows that it is far from evident in other fields. Already during Exner's lifetime, the second law was typically invoked to argue that an organism's growth not only requires energy, but – even to a larger extent – negative entropy. Taking metabolism into account, single cells are hence much more probable than a complex organism, a conclusion in stark contrast to Exner's intuition. If one even wants, as does Exner, to turn the second law into a physicalist foundation of humanistics, the definition of order and disorder substantially determines the value and meaningfulness of the conclusions reached. Due to this problem Exner's theory of culture in certain cases only produces explanatory tautologies. (See Sect. 4.6.2)

Exner generalized another feature figuring prominently in kinetic theory into a principal distinction between two levels. Natural laws represent a *macroscopic* order that constantly arises from molecular disorder at the *microscopic* level. If one jumbles up a well-ordered library by putting back the books at random, an ordered macro-state changes into a disordered macro-state. Similarly, perfect equality of all humans with respect to wealth, social status, and work corresponds to a highly improbable perfectly ordered society that is analogous to a gas with molecules of identical velocities.

> If we compare the state of a cultured nation [*Kulturvolk*] with that of savage tribes or even of the prehistoric human…, we find a remarkable uniformity on the former side in contrast to plenty of inequalities in physical, intellectual and social respect on the latter which increase the more the older the culture becomes. (Ibid., p. 26)

Statistically, however, the men in the street prevail by far. The inescapability of every culture's historical course does not force the single human into fatalism or "to pay homage to the oriental kismet," (Ibid., p. 40) because despite the law-determined *global* (or macroscopic) distribution of a property there is no constraint on its *local* (or microscopic) distribution, that is for an individual or a small group. But social engineering cannot hold up the course of nature in the long run.

> Often the call for retributive justice, or perhaps even for complete equality in wealth is raised. But this would be as irrational as complaining that the brickstones of a house under construction do not go up by themselves. This distribution cannot be changed because it is simply the necessary consequence of the respective state of the culture. (Ibid., p. 40)

The evening edition of the *Neue Freie Presse* of the same day published large parts of Exner's Inaugural Address on its third page and reported an "exceptionally large attendance of the students" and a "thunderous and continuous applause" of the audience.[81] In a long obituary, Hans Benndorf, Exner's former student and a close friend of the family, related that the speech "made a great stir" even outside academia. (Benndorf, 1927, p. 403) Similar recollections can be found in Kuhn's interview with Philipp Frank.

Frank: I would say that Vienna was a big school of physics. … I studied mostly with Boltzmann. But Exner, you see, became connected with the basis of quantum theory by one thing, by one lecture. I think it was his inaugural lecture as *Rektor.* – It was about the role of statistics in physics. And then he said one thing which became rather famous; he said that it may be that the basis of physics will be statistical. And it may not be that every statistical law can be derived from dynamic laws. Because generally, of course, the [reigning] idea had been that statistics only gave an average. The basic laws were the Newtonian laws. But it was Franz Exner who already said that maybe this was not true. It could be that the basic laws are statistical. I think that this concept had a great influence on Schrödinger.

Kuhn: I haven't read Exner's address but I did read Schrödinger's [1922] essay on this. But was this something the rest of you also knew? Or has that statement only become famous since Schrödinger?

Frank: Oh no, no. It was already known at that time. It was widely discussed even when I was a student … All the physicists in Vienna were interested in the philosophy of science. Hence if there was anything connected with philosophy of science [such as the status of statistical laws] it would be widely discussed. Admittedly, Franz Exner was not well-known for his interest or publications in philosophy of science, but in this matter he did clearly lead the way. Exner was an experimental physicist who usually left philosophical matters to Mach and Boltzmann who was the theoretical physicist. At this time, I think that Exner's main field was connected with electricity, atmospheric electricity. It was the beginning of ion theory – of ions and electrons at this time. Oh yes, it was much discussed whether the foundation of this was statistical or not, because everything was connected with philosophy. (quoted from Blackmore, 2001, p. 61f.)[82]

These testimonies show that in Vienna Exner's viewpoint was not so "subterranean" as Forman assumes. The Inaugural Address made enough of a stir to prompt a reaction of Planck. Frank and in particular Schrödinger would constantly point to Exner's priority as regards the fundamental statistical character of physical laws. How did Exner arrive at this idea or, rather, what motivated him to go beyond the late Boltzmann? Exner's philosophical background shall concern us next.

4.2 Preconditions of an Indeterminist World-View

In his Curriculum Vitae, Exner remembers:

During the senior years at the Gymnasium the proclivity to the natural sciences came out more and more and if I should say what in those days had been of particular influence on me, then I would have to mention above all the writings of A. v. Humboldt which already at the time aroused the strongest impression on me because of their universality. It was perhaps an unconscious tradition [of his deceased father Franz Exner] that I felt the wish to occupy myself with purely philosophical problems, such as in particular with Herbart's system, especially with his psychology and metaphysics …. Among all the natural sciences, physical geography fascinated me in particular, and I thought of dedicating myself to it; but already during the Gymnasium I realized that to this end a reliable physical

[81] Evening edition (*Abendausgabe*) of the *Neue Freie Presse,* October 15, 1908, p. 3.
[82] I have deleted some brackets inserted by Blackmore because they are irrelevant for the present argument.

basic knowledge is necessary, and thus I wanted to devote myself to the studies of physical phenomena which I actually did. (1917, p. 3)

Exner remained a physicist all of his life, not least because of the generality of that science. But philosophy and physical geography would come into their own right.

Apart from the dominant influence which Herbart's philosophy exerted in Austria during the days of Exner's studies, there was also an important family context. His father Franz Exner (1802-1853), who was a professor of philosophy at Prague University and a leading figure in the educational reforms of the post-revolutionary Austria, was a renowned Herbart scholar. Due to his eye troubles Franz Exner needed a reader, and the job was given to the young student of chemistry Josef Loschmidt. To Loschmidt, who was of humble beginnings, the house of the Exner's became the entrance door to another world of philosophy, literature, and the arts. After Franz Exner's and his wife's early deaths, Loschmidt was among those taking care for the Exner children in Vienna. And finally in 1891, Franz Serafin would become Loschmidt's successor as professor of physical chemistry.[83] In two commemorative articles on Loschmidt, Exner (1895, 1921) relates that once his father had seen the growing philosophical interests of his reader, he gave him a philosophical problem to solve.

The problem … to carry through Herbart's psychology in a strictly mathematical fashion became the reason why Loschmidt finally renounced philosophy and dedicated himself to the natural sciences. The conviction which he arrived at during this work, that it is entirely hopeless to advance on this way because the application of mathematics to psychology is erroneous in point of principle, made him a renegade, and – so he used to say – "renegades are the worst enemies." Nonetheless, Loschmidt never regretted the efforts dedicated to philosophy; to the contrary, he always spoke of this time with the highest gratitude. Indeed I believe that the philosophical composure and … his mentality constantly directed at the essentials had their roots in those days. (Exner, 1921, p. 178)

In the rather similar passage of his 1895 newspaper article, Exner made clear that he shared Loschmidt's verdict by adding the following parenthesis: "still today many philosophers stick to the applicability of mathematics in this realm although no one knows how to apply it." (Exner, 1895)

From Exner's Curriculum Vitae and the episode with Loschmidt one can conclude that he had thoroughly studied Herbart's philosophy and it is safe to assume that he knew Fechner's manifold criticisms of it.[84] Exner almost certainly knew Fechner's work in physics, and at the beginning of the new century the interest for the philosophical work of Fechner had increased. To be sure, many ideas of Fechner were contested by the interpreters. Franz Serafin's brother Sigmund, elder by three years, was a world-renowned physiologist and held a chair at the University of Vienna as well. So he was probably well-informed about the recent state of psychophysics. On the other hand, he was skeptical towards mathematical psychology and in the *Lectures* he neither took up the psychologist elements in Mach's thinking nor Mach's theory of measurement. Apart from Exner's pondering that the laws of nature change on the

[83] The bulk of the biographical information reported in the present chapter can be found in (Karlik/Schmid, 1982), which in turn often relies upon (Exner, 1917) and (Benndorf, 1927). A succinct summary can be found in (Hanle, 1979, pp. 228-232).

[84] On the multifarious relation between Fechner and Herbart which was by no means a simple opposition, see (Heidelberger, 1993).

cosmological scale, I have not found any incontrovertible allusion to Fechner's writings – and this idea was at bottom already touted in Boltzmann's philosophy lectures. (See Sect. 3.5) As the documentary evidence remains inconclusive, it remains open whether Fechner's philosophy was a particular motivation for Exner to adopt the relative frequency interpretation of probability.

Thus I shall rather pursue a different line of influence here that brings into play the whole of humanistics. This wider context is backed by the manifold references to issues of society in the Inaugural Address and by Exner's later indeterminist theory of culture.

In a recent paper, Erwin N. Hiebert has spotted a broad tendency "in which probability and chance, as generated from within the social and humanistic disciplines, came to inspire and motivate investigators in the physical sciences to take a deeper look (deeper than classical mechanics allows) at processes that occur in nature." (Hiebert, 2000, p. 7) An entire constellation of Austrian physicists around Exner "initiated an algorithm of statistical and probabilistic thinking that was borrowed from the humanities." (Ibid., p. 10) What Hiebert terms the "Austrian Revolt in Classical Mechanics" had been prepared by Boltzmann and his teachers Loschmidt, who found the size and diameter of molecules, and Josef Stefan, who obtained important results in kinetic theory; and it was the younger generation of Exner's students Stefan Meyer, Marian von Smoluchowski, Egon von Schweidler, Friedrich Kohlrausch, and Erwin Schrödinger[85] who "were able to carry the atomic-molecular-kinetic theory of matter (wedded in this case to probabilistic thinking) into newly discovered arenas, such as radioactivity and quantum thinking. … Indeed radioactive decay was the first example of discovery of a natural phenomenon that exhibits genuine probabilistic nature." (Ibid., p. 13) This was a remarkable step across a cleft which gave so ample living space for Spengler's *Decline*.

> Before the turn of the century, all appeals to mechanics had shared the common feature of being embedded in classical, deterministic, and causal thought leaving no room for probability, chance, randomness, or chaotic behavior. By contrast, contingency, probability, chance (absence of assignable cause), randomness, and chaos were known components of the world of phenomena that characterize the social sciences and the humanities. Indeed it was contingency that radically set them apart and alienated them from the mechanical model that had been put in place for the exact sciences. (Ibid., p. 9)

Hiebert takes the existence of the Austrian revisionist mechanics as an example of a reverse case of influence in which a less exact science donated to a more exact science. Such influence blocks to a large extent – so Hiebert argues – those reductionist endeavors virtually characteristic of modern physics. And indeed Exner gave the reduction issue a new twist. Had Boltzmann maintained that biology, psychology, and the humanistic disciplines could profit from a reduction to atomistic theory, Exner's indeterminism "courageously changed the nature of the search for common frontiers by demonstrating that the exact sciences and the humanities are complementary in that they both are anchored in the probability and not the certainty of events." (Ibid., p. 25) The new theoretical basic entities were of a different kind.

[85] Hiebert's list also includes Richard von Mises, who had studied at the Technical University in Vienna, and Reinhold Fürth, who had studied with Anton Lampa and Philipp Frank in Prague, and became a professor there. I am pretty fine with adding those important indeterminists of Austrian origin who not members of the Exner circle, and suggest to amend the list with Frank.

> For Exner there is nothing out there in the world except a continuum of events that are more or less readily, or with more or less difficulty, brought in line with acceptable explanation in search of objective truth. There is no end in sight, no final solution; there is no theory of everything. The task is endless and open-ended in the exact sciences, as in the humanities. Their boundaries need not be cast in stone, but unrestrained extrapolation from one discipline to another undercuts the rich depth of human learning. (Ibid., p. 25f.)

Hiebert is right in stressing Exner's combined rejection of mechanist reductionism and of a foolproof methodology for the natural sciences; both rejections were part of the Machian heritage. But Exner did not take the Machian tack countering ontological reductionism by stressing that physics, biology, and psychology stood on a par the provisional unified world view (Cf. Section 3.9.). To Exner, the objects of the sciences were different in kind, but probability calculus provided a unified methodological approach that was not committed to strong forms of reductionism be they based on the existence of atoms or the fitness for survival. Although Exner endorsed Boltzmann's atomism, he did not advocate a theory reduction to universal pictures as specific as Boltzmann. (Section 3.4.) The relative frequency interpretation of probability allowed Exner a more general approach because it dispensed him of an explicit description of the basic entities – be they inferred from the interactions or hypothetically assumed. It was sufficient that one could spot a sufficiently large number of single events or processes that, in the limit, reproduced the macroscopically observable laws. Exner might thus be called an areductionist on the ontological level who, at the same time, advocated a methodological reduction to abstract theoretical entities, collectives of events and single processes, rendering consequently the second law a meta-principle basic to all sciences and humanistics. This also prevented him to repeat Boltzmann's exaggerations about the mechanical nature of cultural phenomena (Cf. Section 3.9.).

Exner's new outlook was embedded into a historical perspective according to which the scientific world view, and all of the single laws or weak regularities in nature had emerged in the course of the world's history. Once found they represented a possession that, in stark contrast to Spengler, could never be lost entirely. Taking Hiebert's perspective, it becomes clear why Exner felt so strongly about *Decline* that he devoted the entire Preface to the second edition of his *Lectures* to it. He was not the hard-boiled physicist who could simply dismiss the book because of its blatant misunderstanding of scientific facts. "Exner knew that he was speaking for a generation of scientists who had recognized, or more correctly, were beginning to recognize, that the search for truths in physics implied an openness to ideas coming from disciplines traditionally thought to belong to areas of learning beyond the borders of physics." (Ibid., p. 22) Exner did not react with adaptation, but rather sat down to write a counterproject. (See Sect. 4.6)

One of Exner's manifold inspirations for seeking common frontiers and methodological unity of science and humanistics in a weaker sense than Boltzmann's theory reduction and Mach's economy of thought stemmed from the intellectual sweetheart of his youth, Alexander von Humboldt's physical geography. In his most famous work *Cosmos: Outline of a Physical Description of the World,* Humboldt outlined a particular way to mediate between the scientific and aesthetic aspects of nature. Each of the five volumes that appeared between 1845-1862 quickly became a bestseller. Hanno Beck writes: "People scrambled for the book which after the adventures of a wildly speculating Naturphilosophie,… after a long reign of Romanticism befriended the German intellectual world with empirical science, which – presented comprehensible and in stylistic perfection – bewitched a bourgeoisie [*Bürgertum*] committed to education;" (Humboldt, 1993, vol. 2, pp. 410f.)[86] a bourgeoisie – one might add – that after World War I would scramble for the two volumes of Spengler's *Decline* mirroring their deep-seated pessimism and skepticism toward modern science and technology. Humboldt's physical description of the world [*physische Weltbeschreibung*], instead, built a bridge between science and human sensitivity. Its main impulse was "the endeavor to conceive the appearances of the material bodies within their general context and to comprehend Nature as a whole that is moved and animated by its inner forces." (Ibid., vol. 1, p. 7)

Most importantly, Humboldt understood physical description of the world as an autonomous scientific discipline embracing both the Earth and outer space. Its highest goal consisted in the recognition of unity in the manifold, in the investigation of the inner connections in sidereal and tellurian phenomena. But Humboldt's new universal science was not tantamount to an encyclopedia of the single sciences. Although physical description presupposes, for instance, physical science, "it teaches the distribution of magnetism on our planet according to relations of intensity and direction, not the laws of magnetic attraction and repulsion or the means to cause strong electromagnetic effects." (Ibid., vol. 1, p. 45) In virtue of its universal objective, Humboldt's new science contained only the general laws of orography or hydrography, not a comprehensive cartography of single mountains, vulcanos, or currents.

In Machian terms, Humboldt's physical description of the world could be interpreted as a phenomenological science that aspired at a direct description of the observed facts, especially the distribution of certain phenomena across the world by means of specific equations, without attributing more than auxiliary value to – ontological or theory – reductionist hypotheses. Interestingly, the first volume of *Die Naturwissenschaften* contained an interpretation of *Cosmos* against precisely this background. Erich Metze opposed the then-common view that *Cosmos* was antiquated because contrary to Humboldt's aesthetical mode of *description*, modern science aspired to a sober *explanation* of nature. "If it is furthermore claimed that the scientific methodology of Humboldt is *in principle* different from today's, then this is based on a grave self-delusion. For us there exists no true difference between *description* and *explanation* of the world." (Metze, 1913, p. 912). Quoting Kirchhoff, Metze concludes: "Even a sublime intelligence possessing the world formula as Laplace and du Bois-Reymond have imagined, could never transcend mere description." (Ibid., p. 913) Although Humboldt was aware of the distinction between description and explanation of the world, our limited knowledge forced us to content ourselves with the empirical laws while the causal nexus remained an unreachable ideal goal. Thus, the distinction was only of a practical kind and Humboldt, after all, considered explanation and description of the world as identical.

To my mind, this identification of Metze's slightly overshoots the mark. In contrast to Mach, Humboldt's new science did not stop at tolerating an incomplete world view, but intended to show by painting a preliminary unified image of *Cosmos* the existence of a unity of nature based on eternal causal laws. But Humboldt was well

[86]This edition contains only the first two books of the *Kosmos* which give the picture of Nature.

aware of the dangers of prematurely reducing all phenomena to a single principle – as had attempted Greek atomists and Pythagorean mathematicians. But contrary to the *Ignorabimus* and beyond a straightforwardly Machian approach that would unite the various branches of phenomenological knowledge *per analogiam*, Humboldt believed that physical description "by meaningfully arranging the phenomena lets us presage their causal connection." (Humboldt, 1850, p. 4f.) Thus the prospect of a genuine explanation going beyond description remained.

Already in his early geographical work about the distribution of plants,[87] Humboldt applied statistical methods to the distribution of various physical and biological properties, an approach that in those days had been restricted to political geography. In the Preface to *Ideas* ([1807] 1960), Humboldt explicitly acknowledged the help and influence of Laplace and Jean Baptiste Biot. The Humboldt connection thus confirms Hiebert's thesis that Exner's indeterminism represented an intrusion of non-physical methods into physics.

Let me come finally to that aspect of *Cosmos* which Exner would carry out in *From Chaos to the Present*. A parallel between both works was already suggested by Benndorf: "What the young man had attracted in Humboldt's Cosmos, the universality of the approach, he worked out for himself in the course of a long, unusually rich life and, as it were, intended to leave it to posterity as the legacy of his personality." (Benndorf, 1927, p. 404)[88] Humboldt's *Cosmos* described the objective content of the physical description of the world as "the real empirical view of the whole of nature in the scientific form of a picture of Nature [*Naturgemälde*]." (Humboldt, 1993, vol. 1, p. 43) The first part of the second volume investigated "the reflection of the picture received from the external senses on the sentiment and the poetically disposed imagination." (Ibid., vol. 2, p. 3) It studied the respective ways of stimulation, the aesthetic and literary description of Nature, landscape painting, and the cultivation of tropical plants. Finally, Humboldt outlined the "History of the physical description of the world: the leading factors in the development and extension of the concept of the cosmos as a natural whole." (Ibid., vol. 2, p. 88) As these sciences differ from the natural sciences, so their history does not coincide with a history of the natural sciences. Instead, Humboldt's theme was the historical development of one particular thought, of the idea of the unity of natural phenomena. In view of the popular talk about the "heat death" of the universe, it might seem surprising that Exner indeed believed that his general application of the second law of thermodynamics represented, constructively, such a unifying principle of science and humanistics.

Studying the emergence of the objective world view in *From Chaos to the Present,* so I argue in Section 4.6, Exner followed Humboldt's model of the history of the physical description of the world and of the emergence of the notion of the cosmos. But he pursued a still more ambitious program embracing also those aspects of human culture which Humboldt had set apart. Although I would think that *Decline* and the post-war cultural developments it was embedded in, prompted Exner to actually write down his history of culture and try to get it published, it was anything but a bolt from the blue. Already in 1908 he had clearly expressed his comprehensive notion of culture

[87] See the map in (Humboldt, [1807] 1960). Much more data is processed in (Humboldt, 1831).

[88] The fact that Benndorf still in 1937 cited the manuscript with the wrong title *Vom Chaos zur Jetztzeit* suggests that he had never seen it, which suggests in turn that the Humboldt context was evident to or even discussed in the Exner circle.

and much of the ideas written down after the war had already circulated in the Exner circle for years. The recollections gathered in the following section shed light upon the particular nature of Exner's personality which was instrumental in transmitting Vienna Indeterminism to a new generation.

4.3 Exner and His Circle

When Exner began his rectorate in 1908, "he had reached the height of his activity, [and was] surrounded by a bevy of pupils who respected him like a father." (Benndorf, 1927, p. 403) In this way, Exner who had been extraordinary professor since 1879 and succeeded Loschmidt in 1891, became "during one generation the center of Austria's physical life" (1927, p. 27) – so relates the obituary of Arnold Sommerfeld, who had formed his own school of students in Munich among them Heisenberg and Pauli. The most famous of Exner's students included the early deceased Marian von Smoluchowski who developed a theory of Brownian motion independently of Einstein and Fritz Hasenöhrl who became the successor of Boltzmann, the Nobel laureates Viktor Hess who discovered cosmic radiation and Erwin Schrödinger who throughout his life stressed Exner's priority for the idea of irreducible indeterminism, and Stefan Meyer who became the first director of the Institute for Radium Research [*Institut für Radiumforschung*] founded by Exner. In the 1920s and 1930s all but one Austrian chair in experimental physics was held by a former student of Exner's. To be sure, Exner's central role in Austrian physics also resulted from the fact that Boltzmann, in his second Viennese period (1902-1906), was no longer able to establish a circle around him and suffered from severe health problems. But the decisive point was Exner's unparalleled personality.

Exner's "exceptional understanding for the younger generation" (Benndorf, 1927, p. 407) was based on his behaving as an equal among equals instead of commanding formal respect. Benndorf describes the intellectual atmosphere in Vienna's Second Physical Institute as follows:

> In late afternoon we gathered for tea around "Väterchen" [Exner]. Everyone had to report on his work, there were no secrets and no priority claims. … Exner considered the fear of intellectual theft as a sign of intellectual poverty. We were talking about everything under the sun, discussed excitedly and argued about scientific matters. … For a scientist, Exner had an amazing historical knowledge and interest in the history of culture. The mutual dependence of different cultures, the laws of their development, were issues about which he had thought much and with which he could stimulate his audience.
>
> Exner was of a gregarious nature, although he never went to parties and paid no visits; he abhorred such conventional undertakings. But in his own house … the closer and wider circle of friends typically gathered after dinner, without invitation everyone could come and go whenever he pleased. The spiritual center of the circle was Exner; he knew how to direct the conversation to interesting subjects…; he could narrate masterfully, in particular of his voyages. By means of pictures he let the miracles of India arise anew before our eyes…. That Exner was actually of an artistic nature including the respective weaknesses, can be seen from his vivid relationship to any kind of art. (Benndorf, 1927, p. 408)

Exner's voyages were an essential part of his life; they even fill several pages in his curriculum vitae for the Academy of Sciences. The vivid descriptions of archeological sites and monuments that one finds throughout *From Chaos to Present* leave no doubt that the author had personally seen them, and they give an impression why his circle was so fascinated by the evening narrations.

According to Franz Exner, the son of Franz Serafin's brother Adolf, the "Serafin evenings" were held every Saturday uniting both members of the Exner family and members of his Circle. Franz Exner's recollections assembled in a privately printed booklet *Exnerei* confirm Benndorf's description. He reports that one of his uncle's students one told him that

> what they had learned from Serafin Exner within the narrower field of physics was much less important than the deeply lasting impression which his personality and his attitude towards science and scientific research exerted on all his disciples. … As his brother Adolf, Serafin was a master of the art of living [*Lebenskünstler*], yet in a different, peculiar way. My mother called him a pasha because he knew how to arrange his life completely in accord with his individual wishes and moods, however his wife and daughters might have sighed about it. (Exner, 1944, p. 23)

The Exner family must be viewed as one of the leading academic families in the Habsburg Empire: an early-deceased father who played a major role in the academic and educational reforms after the failed revolution of 1848, four sons holding four university chairs, Adolf in law, Sigmund in physiology, Franz Serafin and Karl in physics, and one daughter, Marie von Frisch whose correspondence with Gottfried Keller documents the importance of the arts within the Exner family. And also four of their children would become university professors. Felix Exner, the son of Sigmund, once characterized the four elder brothers by four superlatives: "Adolf: the most intelligent; Schiga [Sigmund]: the most industrious; Serafin: the most educated; Karl: the most genial." (Ibid., p. 30) Apart from Karl who was working in Innsbruck, the families made many common activities – the younger generation, for instance, attended Boltzmann's philosophy lectures with great pleasure – and the families of Adolf and Franz Serafin Exner lived closely together in the "Pelikanwinkel" in Vienna. Four of the five Exner families spent all summer vacations together in their houses at Brunnwinkl at the Wolfgangsee, which thus became a family village unique of its kind.

Benndorf and the more comprehensive account written by Berta Karlik and Erich Schmid (1982) depict Exner's scientific achievements as such. His early research was devoted to the physics of crystals and to the point density maximum of water. His mature works concerned four areas: (1) Electrochemistry or, more specifically, the theory of the galvanic element led Exner into an intense polemic with Ostwald about the drip electrode. Exner's earlier criticism of Alessandro Volta's contact theory, which still enjoyed a certain reputation in Germany, in favor of a chemical theory of the galvanic element earned him Mach's recognition. On January 26, 1883, Mach wrote to him:

> For your polemic I wish you good luck. German professors understand slowly, in particular if they have no mind to do so. Incidentally, this is always a matter of time. For *me* a main argument in this matter is that the chemical process which is able to *replace* a lacking difference in the potential niveau will also be able to *generate* it.[89]

(2) By devising a simple and portable measurement apparatus, Exner became a pioneer of research on atmospheric electricity. He was also the first to assess the field in a

[89] Österreichische Nationalbibliothek, Nachlaß Exner, 294/30-1.

systematic way. Through various expeditions and by organizing a world-wide network of measuring stations, a rather extensive cartography of the variations of the electromagnetic field of the earth and of atmospheric electricity was reached. The results, by the way, falsified Exner's own theory of the mutual dependence of these two phenomena. (3) The Viennese collection of meteorites propelled Exner to embark on a large-scale project in spectroscopy. Together with his assistant Eduard Haschek, they measured 100,000 spectral lines of all known chemical elements within 28 months. Their large number of publications since 1895 were later assembled into three volumes full of tables and photographs of the different spectra of all known elements (Exner/Haschek, 1911). Once again, Exner's main achievement consisted in finding a universal and simple method that without sacrificing much precision produced easily surveyable results and had an remarkable efficiency. Moreover, this method consisted in direct observation, so that it left no doubt about the maximum sharpness obtainable for a given line. (4) In the theory of colors, Exner strongly favored the Young-Helmholtz three-component theory over Ewald Hering's four-component theory. This launched a controversy with Franz Hillebrand, a psychologist and former disciple of Hering and Brentano, that only was resolved in the mid-1920s by Schrödinger.

Summing up Exner's scientific achievements, Benndorf relates: "Although among them there are no epochal discoveries, Exner has, after all, put some solid and enduring stone into the building of science and he even partially laid foundations." (Benndorf, 1927, p. 407) Yet, "in a certain sense the most important achievement" (Ibid., p. 404) were his *Lectures on the Physical Foundations of the Sciences* written during the war and first published in 1919.

4.4. Exner's **Lectures** *(1919 and 1922)*

The 95 lectures were divided into four chapters: (1) Space, time, matter, and some general concepts; (2) Matter and its constitution; (3) Ether; (4) On natural laws; the final lecture contains an overall summary. The Preface to the second edition (1922) was entirely dedicated to a criticism of Spengler. Chapters 1-3 provided a fairly exhaustive treatment of then standard topics, occasionally including broad remarks about the historical development of these fields, but Exner also dedicated ample space to themes investigated by his school, such as the theory of colors and radioactivity.

In the fourth chapter, Exner commenced with an introduction to probability calculus and emphasized that only if we consider chance not as based on 'imperfect knowledge', but as an objective feature of nature, can we reconcile chance and causality by considering the law of causality as expressing that "on average the course of the phenomena is lawful." (1922, p. 675) Exner adopted Mach's redefinition of causality in terms of functional dependences. "Ernst Mach to whom one surely must attribute an influential voice, says: 'There is no cause nor effect in nature; nature has but an individual existence'." (Ibid., p. 675, quoting Mach, 1988, p. 496/580) As did Mach and Boltzmann, Exner considered the validity of the principle of causality as an empirical question.

We shall not forget that the principle of causality and the desire for causality [*Kausalitätsbedürfnis*] have forced themselves upon us exclusively through experiences made at macroscopic objects and that their transferal to microscopic phenomena, hence the presupposition that each single event is strictly causally determined, has no justification based on experience. (Exner, 1922, p. 703)

This demise of the Kantian notion of causality can be related to the first lecture of the first chapter where Exner rejected the Kantian synthetic a priori concepts of space and time by reference to the special theory of relativity.

Our most basic experience, Exner holds, is that all natural processes are directed. This fact – which is today typically called ‘arrow of time’ – could only be explained by the second law of thermodynamics in its statistical form because all other laws of physics (known back then) were reversible. This won the second law a superior status among all natural laws and thus indeterminism became primary. Referring to quasi-instinctive basic experiences was an argumentative figure of Machian origin, which was, to my mind, at odds with Boltzmann’s atoms as theoretical pictures. But the basic experience of directedness directly confronted Mach’s (1919) foundation of thermodynamics on the sensation of temperature.

To be sure, by 1919, atomism was beyond doubt. Still, as no measurement is absolutely precise, the firm empiricist can only impose one condition upon the (unobservable) elementary mechanisms that underlie an (observable) average law. “[I]f physical phenomena result from many identical, mutually independent single events, then the causes assumed by the determinist act just as if there were no causes at all, but mere chance ruling” (Ibid., p. 681). Given the primacy of randomness, Exner even proposed to distinguish the macro-level and the micro-level according to their degree of lawfulness.

Whether processes are to be conceived as microcosmic or macrocosmic does not depend upon the nature of the investigated matter, but whether the single case or the average of very many identical cases is the object of consideration. One could almost conversely infer the macrocosmic nature of processes from the existence of laws and infer their microcosmic nature from the lack of those. (Ibid., p. 695)

Of course, this distinction is not an absolute one. Apart from the extreme cases of physics where strict laws emerge and humanistics where statistical collectives can hardly be formed, there is ample space to formulate weak regularities. While probability has an objective meaning, order does not.

The most probable state of a macrocosm, however, is always that of disorder, for the simple reason that very few possibilities of ordered states always correspond to a great number of possibilities of disorder. In this we comprehend the concepts of order and disorder only from the human point of view, depending on whether it stands out by certain attributes. (Ibid., p. 696)

Exner subsequently repeated the example of the jumbled library from the Inaugural Lecture. The proper designation of the ordered state against the disordered states in fields like biology and history still remained an unsolved problem. This lacuna will prove a major hobble for the laws or regularities in Exner’s indeterminist theory of culture.

Exner’s insight that all laws we experience hold only on average, gives the empiricist law of causality a new twist insofar as it “expresses nothing else but the fact that natural processes, to the extent we can observe them macroscopically, that is on average, are lawful.” (Ibid., p. 674) This was already Boltzmann’s position in the last years of his life when he pondered that the force law depends on the direction of time valid in a certain part of the Universe. On this basis, Exner rehearsed the old Fechnerian idea that it was "presumptuous to claim that any law, e.g., gravity as it appears to us today, had also been valid in all earlier epochs of the World, or will be valid in all subsequent ones." (Ibid., p. 667)

In the Preface to the first edition of his *Lectures*, Exner even appears to have shared Mach's critical stance towards hypotheses the main target of which had been Boltzmann's atomism. "May the hypothetical with its manifold possibilities stimulate reflection, facts are and remain the basis of research." (Ibid., p. iv) Not even the most exact science can claim eternal validity. "We must content ourselves with knowing the facts, as they are presented to us by nature, as precisely as possible and being able to represent their mutual relations in such a way that we do not run into contradictions before long." (Ibid., p. 66) Exner, however, objects to Mach's narrow conception of theory and advocates Boltzmann's program of explanation instead. "The kind of natural studies which had as its final aim only a *description* of nature in terms of systems of equations is unsatisfactory. And even though this ideal was in place for a while, today research is directed toward a molecular-mechanical understanding of natural processes." (Ibid., p. 721) Nevertheless, facts, natural laws, have "objective reality" (Ibid., p. 724) while theories change drastically in the course of time.

Exner's stance in the issue of realism, on the most general level, differed significantly from Boltzmann's preference of a realist language and even more from Mach's neutral monism. The second chapter of the *Lectures* commenced with some metaphysical considerations on the reality of the external world, or – so Exner – the problem of the Cartesian cogito.

> The world of sensations represents the immediately given. If we assume that each of these sensations is correlated to certain objective processes in the external world, then this amounts to a theory which we willy-nilly put forth, without whose assumption all human research would have to appear superfluous. This theory is based on the presupposition that the sensations are conditioned by processes in a real world external to us, and we can consider it an important task of research to ascertain the kind of correlation between internal and external world. … It may correspond to a philosophical desire to pursue this question further than it seems promising as regards positive results. For the natural scientist matters are different. He knows that all knowledge, even the most exact one, ultimately remains a theory that is only valid for who accepts certain assumptions, axioms, such as for instance for the theorems of geometry, without further proof. With someone denying the basic axioms of planimetry, we cannot come to terms at all on this subject matter. And thus we will consider it an axiom of physical research that the external world and the processes in it are real. (Ibid., p. 287f.)

Does this passage refute my claim that Exner worked out a synthesis between Mach and Boltzmann? Indeed, large part of it could have been authored by Planck, the only difference being that he would have taken a transcendental rather than a linguistic tack and preferred to speak about a condition of the possibility of experience instead of an axiom. But when we look at those later passages in the book where Exner takes up the issue again, we see that he is after something else than Planck. Instead of advocating a convergent or structural realism, Exner's remarks concern the problem of psycho-physical parallelism. At the end of the third chapter, he motivates his four lectures on the theory of color as such.

> We have to infer the external world from the internal world by way of complicated physical, physiological, and psychic processes, the interpretation of which, however, we typically have learned already during our childhood in years of practice. This presents a certain danger for the researcher. For the habits of thought which we have acquired from the experience of daily life are not always valid also in those new domains of experience which the researcher faces, yet they can even become a mighty impediment of further progress in this domain. … For this reason it is not superfluous to emphasize this border between direct perception of sensation [*Empfindung*] and its correlate, the external world, at least at one example. (Ibid., p. 626)

Exner here, in effect, rejected Mach's claim that this border line between the physical and the psychic was woolly if not inexistent and that it could easily be crossed back and forth by functional dependences. His claim was even stronger. "Strict and quantitative laws, accordingly, cannot be set up in the domain of sensations, and this difference forever separates our experiences in the external and the internal world." (Ibid., p. 656) To properly understand this distinction we have to bear in mind that the strictly quantitative laws of the external world emerged from chance and that Exner consequently rejected all synthetic a priori categories for laws of nature. "When Kant set out from the presupposition that there must exist absolute knowledge, this represents a subjective and entirely unfounded opinion, which in him was caused by occupying himself with the theorems of geometry; but we shall quickly see that even those do not enjoy an absolute but only conditioned validity." (Ibid., p. 9) In this way, having admitted the meaningfulness of the external world problem, Exner returned onto Machian territory by rejecting a priori knowledge within science and by a physicalism that was both all-encompassing and relaxed because the basic physical entities were no longer any substances of mechanical processes. As in the Inaugural Address, he asserted that all processes in nature are subject to the laws of physics; yet these laws were of a statistical nature, and there was a great leeway in the character of the individual events as long as they produced the macroscopically observable laws. The empiricist could be open-minded, for instance, whether the ether was a kind of matter or a kind of space or whether we simply ascribe properties such as polarizability to the vacuum. (Cf. ibid., p. 615)

Exner's indeterminism did not only entail that all laws were at bottom average laws, even basic concept of physics, to his mind, were meaningful on average only. Here relativity theory and Boltzmann's indeterminism twice conspired in a surprising way. First, special relativity has taught us that space and time cease to be absolute concepts. Boltzmann's determination of the direction of time by means of the tendency towards more probable states entails that time ceases to be absolute also in another sense.

> The coexistence of progressive and retrogressive times is no more absurd than the simultaneous above and below in space; in both cases we are confronted with relative concepts which we derive from our respective experiences. … [I]t depends upon the standpoint from which one judges. For us time is an average value which progresses in the direction of more probable states. (Ibid., p. 65)

Second, pointing to Kepler's speculations of regular oscillations of the Earth's gravitational attraction, Exner held that

> it is not excluded that the gravitational force might be subjected to fast oscillations of its strength [*Intensität*] and that its constant strength, as it appears to us, is only a statistical average over many and rapid oscillations. A recent theory of gravitation even assumes such periodic oscillations occurring

within very short time intervals as its foundation and presupposes that they propagate through space as longitudinal waves. (Ibid., p. 70)

Gravitational waves were one of the distinctive consequences of Einstein's general theory of relativity.[90] In the fourth chapter Exner even contemplated that gravitational force might be replaced by a statistical process in which the falling body moves along by fits and starts or even on a zigzag path. "Boltzmann has in conversation entirely agreed to this opinion and has considered it not only possible, but even very probable." (Ibid., p. 670)

To obtain an ontological foundation for such average laws and concepts, Exner needed a reality criterion that was independent of the particular nature of the single events and consistent with the law of large numbers. His only option here was to accept the collectives of Fechner's frequency interpretation as basic ontological entities. Since these are only realized in the limit of infinitely many events, all apparently deterministic laws admit exceptions – as long as their probability renders them inaccessible to experiment – and they cease to hold below a certain number of single events. On the other hand, the second law of thermodynamics, the probabilistic law *par excellence* becomes meaningless in microscopic domains.

Quite generally, Exner's indeterminism intended to justify all types of regularities found in the world. Depending on the number of events studied by the respective science, the degree of probability varies between zero (in most humanities) and one (in physics). All descriptive sciences lie in-between, in particular because the external conditions change too rapidly for exact laws to stabilize. This was the main motive at the end of the Inaugural Address. Although this time Exner made only little mention of society and culture, in the fourth chapter he admitted that, at least temporarily, the inescapable tendency to equilibrium might be suspended on the macroscopic level.

Also the acts of the single humans belong to the microcosm [where the second law does not hold], in contrast to those of human society; the latter are essentially directed at artificially withholding the most probable state, that of disorder. Decrees and laws, written and unwritten ones set by law and customs serve to this end. It is open to question whether similar tendencies also prevail in the realm of vegetable and animal organisms; but this seems well possible and in this case the second law would not hold true for organic forms as little as it does for a library kept in order, because the presuppositions of its validity, chance and disorder, would be absent. (Ibid., p. 699)

This mitigated the conclusion of the Inaugural Address where any social interventions had been considered futile.

The continuity between Boltzmann and Exner has sometimes been misrepresented in the literature. Paul A. Hanle, for instance, holds that on the basis of Exner's indeterminism "we cannot in principle apply any mechanistic program of physics to molecular processes," (Hanle, 1979, p. 256) while this was the case with Boltzmann's. But Hanle simply misunderstands Boltzmann's concept of atomism as if it relapsed into mechanical or deterministic explanation and he accordingly presupposes Planck's anti-Machian reading of Boltzmann as the only possible one. As Exner broadly discussed his fundamental indeterminism in connection with the

[90] Although their existence had soon become evident from his papers at about 1918, Einstein later erroneously convinced himself in print that gravitational waves could not exist. (I thank David Rowe for this hint.) At present, their detection is the focus of a large-scale research project.

example of Brownian motion (Exner, 1922, pp. 412-417), Hanle criticizes his "failure to distinguish between indeterminacy in principle and the practical inability to analyze the determinate causes in an aggregation of micro-physical events." (Hanle, 1979, p. 227) Here Hanle does not appraise the ontological consequences of the frequentist interpretation of probability and Exner's staunch empiricism; both will also inspire Richard von Mises to advocate a purely indeterminist approach to Brownian motion. (Cf. Sect. 8.2.) The following passage from Forman on the basis of which Exner is classified as the earliest convert to acausality, is even further from understanding his philosophical stance.

> Although Exner cannot consistently maintain his empiricist posture and also categorically deny the existence of causality at the microscopic level, he wants very much to do so in order 'to arrive at a unified world picture' in which *all* law is purely statistical, a world of pure chance. He therefore does his best to convince his (lay) readers [*sic*!] of the implausibility of the existence of such a causal substratum, switching back and forth between, and largely confounding, the question of the validity of the laws of classical mechanics in the atomic domain and the validity of the principle of causality in the same domain. Influential as the lectures indeed were, they have in many respects an archaic air. Exner is a curious mixture of the philosophical currents of the two preceding generations, a self-confessed mechanist-materialist yet clearly also a positivist in his view of scientific constructs (Forman, 1971, p. 75)

– a fact that links him to "late nineteenth century positivist-monist repudiations of causality." (Ibid., p. 74) After all, no surprise that Forman only mentions Exner's criticism of Spengler without examining its content.

The fourth chapter of Exner's lectures was not just an "appendix" (Ibid., p. v), as the author modestly called it. From the perspective of the present investigation it can be seen as the most concise and pointed philosophical account of the first phase of the tradition of Vienna Indeterminism, the first in which all three criteria mentioned in Chapter 3 were advocated firmly and in philosophical detail. This important role of the fourth chapter is not only manifested in later references by Frank and Schrödinger. Also the first footnote of Reichenbach's classic *Philosophic Foundations of Quantum Mechanics* cites Exner as "perhaps the first" (1965, p. 1) to have criticized the assumption of strict causality. Reichenbach knew Exner's *Lectures* very well because he had reviewed their first edition (1919) for *Die Naturwissenschaften*. He criticized their epic breadth and that the author remained silent about Einstein's theory of gravitation. The second point is not quite correct. Apart from the above-mentioned remarks about gravitational waves we find a clear approval of Einstein's successful generalization of the principle of relativity. "It is remarkable that there are facts, such that the progressive perihelion motion of mercury which hitherto could not be explained by the old, but are a direct quantitative consequence of the new theory." (Exner, 1919, p. 63; 1922, p. 62) Expectedly, Reichenbach wanted to hear more than just half a page; presumably the passage was inserted only at a late stage of Exner's writing down the book that had emerged from his regular lectures to students of pharmacy. But in assessing Reichenbach's remark we should not forget that he was a 30-year old physicist-philosopher who would become a main defender of relativity theory and an advocate of the indeterminist character of quantum physics. Exner, on his part, was a 70-year old experimental physicist, nine years older than Planck and thirty years older than Einstein. The first edition of the *Lectures* appeared just at the beginning of the public struggles about relativity theory in which many experimentalists took the side of the skeptics if not of the critics. Without qualification Reichenbach's review commended the

> unbiased attitude of the natural scientist who dislikes metaphysical speculations and who is conscious of the inductive character of all regularities discovered, even of the most general ones. …. Of particular importance seems to me that Exner unequivocally advocates the objective meaning of the probabilistic laws in which he rightly conceives a very general regularity of nature. (Reichenbach, 1921, p. 415)

This was, of course, also Reichenbach's own position developed in the previous year (Reichenbach 1920a & 1920b). A subjective notion of chance built upon our ignorance of the true causes cannot be reconciled with an empirical content of the principle of causality.

The reviewer of the second edition of the *Lectures*, the physicist Wilhelm Westphal accentuated Exner's defense of objectivism against Spengler subjectivism. "It deserves emphasis that despite all criticism Exner – precisely as a physicist – is objective enough to gladly welcome Spengler's work." (Westphal, 1923, p. 113).[91] Westphal referred to a passage in the Preface in which Exner acknowledges that Spengler's

> extraordinarily interesting work, rich with original ideas and fascinating suggestions is doubly welcome in our banausic time. Nonetheless, the natural scientist – and only in this capacity shall I be judging here – cannot withhold serious doubts concerning the methodology of this work which in a way represents the antipode to the present book. (Exner, 1922, p. vi)

While Spengler advocated a radically subjectivist view, to Exner's mind, science was virtually characterized by its objective method; despite a constant change of theories we encounter the same facts as did the old Egyptians. Basing science on subjective intuitions or necessities of thought, among them causality, too often has led science astray, while "precisely the statistical approach to processes [*des Werdens*], at least to some extent, has clarified the notion of causality and has shown that in its generally accepted form it is not tenable." (Ibid., p. xi) Against Spengler (1918, p. 556), Exner insisted that physics deals with processes and that Heraclides's *dictum* was precisely the most general expression of this modern idea. Thus Spengler's identification of laws and determinism is beyond the point of modern physics. Exner's criticism, however, was not only of a methodological kind. And here he took up the above-discussed rejection of Mach's neutral monism.

> Indeed physical research from its beginning to the present has always pursued the same goal: the greatest possible detachment of the object from the subject, and in the course of time it has come considerably close to this goal, it has furnished the proof that there exists, no doubt, an object without a subject. (Exner, 1922,, p. vii)

The empiricist distinction between facts and theories constitutes a core element of Exner's criticism of Spengler in 1922. In contrast to the subjectivist, "[t]he physicist knows that his picture of reality hardly corresponds to reality completely, but additionally he does know that it is consistent with the facts – to the extent he knows

[91] Hanle (1979, p. 255 fn. 112) lists further reviews of the *Lectures*. In view of the sub-milieu thesis defended in Section 5.3., I limit the discussion to reviews that appeared in *Die Naturwissenschaften.*

them so far – in all its essential properties, and if it does not exhibit these properties, he discards it as false and unsuitable." (Ibid, p. vii) Thus "one must not identify the ever changing theories with the results of research." (Ibid., p. ix) Precisely this erroneous identification constitutes the basis of Spengler's thesis of the incommensurability of ancient and modern science. Moreover, Spengler confuses "a culture with its makers or its bearers; the latter are in fact organisms, individuals and peoples, and die as such. But – to stay within our realm – the result of exact research persists for all times." (Ibid. p. x) This would become an important distinction within Exner's own theory of culture.

4.5 Dialogue at War Times: Exner Versus Planck

On August 3, 1914, Planck – then Rector of the University of Berlin – delivered the annual speech commemorating the founder of the University "On Dynamical and Statistical Regularities." After an introduction alluding to the patriotic virtues indispensable in the war that had just begun, Planck rejected a distinction in principle between absolute and exceptionless lawfulness [*Gesetzlichkeit* or *Gesetzmäßigkeit* are used interchangeably] in the natural sciences and arbitrariness and chance in the intellectual realm. So far Planck's thesis was in accord with what Exner had advocated in the Inaugural Address; but the lines of argument were starkly different, if not in direct opposition to one another. Exner had put the law of large numbers and the second law of thermodynamics on top. Planck countered as such:

On the one hand, for all human thinking, even on the highest heights of human intellect [*Geist*], the assumption of an absolute lawfulness superior to arbitrariness and chance represents an indispensable prerequisite; and, on the other hand, the most exact of all sciences, physics, is often compelled to operate with processes the lawful connection of which for the time being remains in the dark, and which, accordingly, can safely be called random in a properly understood sense of this word. (Planck, 1914, p. 55f.)

Planck emphasized that for practical investigations statistical methods are unavoidable and that statistical quantities already intrude into physics by the simple fact that each measurement contains errors. But this only teaches "the fundamental importance of performing an exact and fundamental separation between … the *dynamical*, strictly causal, and the merely *statistical* type of lawfulness for understanding the essence of all scientific knowledge." (Ibid., p. 57) This distinction finds its expression in the sharp contrast between reversible processes, which are subsumed under one dynamical law: the Principle of Least Action, and irreversible processes governed by the second law of thermodynamics. While in the field of practical physics the causality violations implied by statistical laws do not justify any objection to them, the theorist must insist on the distinction between necessity and probability.

This dualism which has inevitably been carried into all physical regularities by introducing statistical considerations, to some may appear unsatisfactory, and one has already attempted to remove it – as it does not work out otherwise – by denying absolute certainty and impossibility at all and admitting only higher or lower degrees of probability. Accordingly, there would no longer be any dynamical laws in nature, but only statistical ones; the concept of absolute necessity would be abrogated in physics at all. But such a view should very soon turn out to be a fatal and shortsighted mistake. (Ibid., p. 63)

Planck here precisely targeted the program Exner had launched in 1908. That his opponent's name was not mentioned was characteristic for Planck's style of scientific argument – except for his repeated personal attacks on Mach.

Exner well understood that the above passage was directed at him and responded to Planck point by point in his 94th lecture, the last before the summary. While he himself studied how probabilistic macroscopic laws *emerge*, so Exner related, Planck *assumed a priori* the existence of an absolute causality as a necessary precondition to understand both Nature and the intellectual realm. "But Nature does not ask whether man understands her or not, nor are we to construe a Nature adequate to our understanding, but only to reconcile ourselves as much as possible with the given one." (Exner, 1922, p. 709) Mach could not have said it better. Exner also criticized Planck's unjustified trust in our habits of thought, which made it likely "to fall into a sort of physical mythology" (Ibid., p. 709) by distinguishing a real world in which all natural processes are irreversible and an ideal world of frictionless motions, undamped oscillations, and the like. In empirical fact, one only encounters "irreversible processes that can come, however, arbitrarily close to reversibility." (Ibid., p. 710) Between both idealizations there are many intermediate cases. "Whether a process is reversible or irreversible in fact only depends upon whether the recurrence of a certain state is practically observable." (Ibid., p. 711)

The other crucial disagreement between Exner and Planck concerned probability theory. "It is claimed that in its applications probability calculus cannot dispense with the assumption of absolutely dynamical laws for the elementary processes" – here Exner almost literally quoted Planck (1914, p. 64) – "[however in actual fact] the assumption suffices that the elementary processes be equally characterized by average laws." (Exner, 1922, p. 712) While Planck called for a dynamical explanation of statistical laws, Exner, on the contrary, asserted: "Nothing prevents us from regarding the so-called dynamical laws as the ideal limiting cases to which the real statistical laws converge for the highest degrees of probability." (Ibid., p. 713) The only empirically justifiable requirement for the microscopic scenario is that in the limit it reproduces the macroscopically observed laws.

Planck's rectorial address – in the same vein as the Leyden speech – emphasized instead that one could very well find an exact formulation of the second law that makes a precise assertion about single processes by amending it with the hypothesis of elementary disorder, that is, by claiming that each individual process does not deviate too strongly from the average of very many processes.

Experimentally there exist no other means than repeating the respective experiment many times in succession, or equivalently to have it reproduced by distinct observers working independently from one another. Such a repetition of a particular experiment, or performing a whole series of experiments, is in fact precisely that procedure which is generally applied in practical physics. No physicist will ever limit himself in his measurements to a single experiment, if just because of the unavoidable measurement errors. (Ibid., p. 65)

Planck's reinterpretation of statistical laws by adding a condition about the relation between single experiments and their average result proves too much. It would even solve the notorious paradoxes of quantum mechanics because nobody doubts that this theory accurately describes a long series of similar experiments. Moreover, pointing to experimental practices seems to be rather disingenuous for someone who advocates a categorical notion of causality. To be sure, Planck's worries about quantum mechanics would be of a more basic kind because it became in principle impossible to posit a dynamical microlevel. Notice that after the 1914 speech he would no longer repeat the argument about elementary disorder in this form and stick to the aforementioned general argument about the necessary deterministic foundations of any probabilistic theory.

Planck also made clear that he intended to defend his reading of Boltzmann against Exner's. Concerning the atomist hypothesis, Planck commended that "Boltzmann apparently avoided to endanger the import of his views and calculations by charging forward too boldly; it was important to him to describe the atomistic hypothesis as a mere picture of reality. Today we are able to go beyond this." (Planck, 1914, p. 60) This tamed Boltzmann neither reflected the universality of his atomism nor agreed with his partial adoption of Machian epistemology; for Boltzmann there was nothing beneath the atomistic pictures. At bottom, thus, Planck simply rehearsed the argument which he had already leveled against Mach: positivism can well provide an unassailable better-safe-than-sorry strategy but becomes infertile in the long run.

The final pages of Planck's address returned to the humanities and intellectual life, using a piece of terminology which Exner would take up in the *Lectures*.

> [In this realm,] strict causality becomes much less important than probability, the microcosm totally falls behind the macrocosm. But nevertheless, also here in all domains up to the highest problems of human will and morality, the assumption of absolute determinism is the indispensable foundation of all scientific enquiry. But this requires some caution …, that the process to be measured is not disturbed in its course by an investigation. (Ibid., p. 66)

And Planck introduced an argument that would remain, in increasingly detailed versions, his proof for the reconcilability of free will and absolute determinism. It is only possible to give a complete account of other personalities; if the thinking subject coincides with the object of investigation it constantly changes as knowledge of it proceeds.

> In this way science sets itself its own insurmountable limit. But man in his incessant aspiration cannot content himself with this limit, he desires to and must transcend it because he needs an answer to the most important, constantly recurring questions of life: How shall I act? – And a complete answer to this question he cannot find in determinism, nor in causality, but only in his moral sentiments, his character, and his world view [*Weltanschauung*]. (Ibid., p. 66)

The difference to Exner is substantial because Planck posited a prereflective and spontaneous Ego that could be approached only by morality – the only field where Kantian reason can give laws to itself. For Mach the dualist Ego was unsalvable, while for Exner it was at least no fixed ground upon which to erect a Kantian-style ethics. To be sure, Exner rejected a mathematical description of our subjective sentiments. But he naturalized ethical feelings and religion; they emerged in the history of culture as did objective science itself.

4.6 Exner's Indeterminist Theory of Culture

In this section I shall investigate Exner's theory of culture insofar as it contributes to the objective of the present book. In *From Chaos to the Present,* Exner, as it were, brought the statistical approach back to the field of society from which it had been imported into the natural sciences. But he also amended it with Boltzmann's atomism in an abstract sense thus rendering the second law a meta-law for the whole phenomenal world. This permitted him to pursue a two-tired strategy with respect to the Spenglerian challenge. Exner agreed to the negative diagnosis about cultural decline, but he emphatically upheld that scientific progress as an emergent phenomenon of the macrocosm was immune to all threats from the cultural milieu however constituted. This was anything but an adaptation in the sense of Forman, but rather the attempt to amalgamate and actively reintroduce two traditions of the 19th century, Boltzmann's atomism and Humboldt's physical description of the world, into the post-war milieu. Exner's theory of culture was not a spontaneous product of the post-war years. It reaches back at least to the year of the Inaugural Address.

4.6.1 The Simple Astronomy

As a document of Exner's narrative skills and his magnificent style of lecturing, Benndorf (1927) cited a small booklet privately printed on February 14, 1908, under the title *The Simple Astronomy First and Second Part.* It had a baroque title page, was written in an antiquated style and garnished with verses.

The not quite anonymous author Ω.Σ. (Uncle Serafin) explains to a young female reader the contemporary knowledge about the stars, the planets, meteorites, etc., all of which are governed by Newton's law of gravitation. In the second part, he depicts the natural history of heaven and earth. The booklet clearly stands in the classical literary tradition of educational books, but there are several places where Exner in effect reverted to an adult reader, above all when outlining the book's purpose.

> There are enough hours in life when without purpose one reaches for an arbitrary book and hence a random grip in the library once played into my hands a work which bore the title "General History of the World.". They were two imposing volumes and after having browsed through them I found therein listed, as is common, which wars humans waged in the days of the Persians and Egyptians, what happened under the Romans, which emperors and kings governed thereafter until lately…. And thus I have further brooded over what this couple of countries and millennia of whom this history of the world deals are to signify and whether only what has been created by man belongs to the world or whether all deeds of mankind are not rather a work of nature as thousands of others, and so brooding I loved to recognize what it actually is to make up a history of the world, and even a general one. And thus happened roughly what is written in this opus.… [T]his booklet shows that what is commonly called history of the world does not embrace more than a point in space and a moment in time. (1908, p. 268-270)[92]

These lines reveal Exner's comprehensive understanding of history and culture, both of which rest upon a nature that not only exhibits physical but also aesthetic and

[92] The sparse punctuation of my translation follows the German original.

religious properties. If we look at the stars, we look into the history of the Universe because we discern what happened eons ago. Conversely, astronomy can predict events that will happen in millions of years in the future. Hence a main tenet of Mach's empiricism is well confirmed: "Vain hope to glimpse into another world, there is only *one* world and this is the message that the meteorites confer directly to us." (Ibid., p. 148)[93] Spectral analysis of the light of the fixed stars yields the same result.

Again, the indeterminism that dominated Exner's Inaugural Address of the same year came to the fore. In Newton's days, the Universe had been considered as a huge clockwork. "[A]ll these worlds [the stellar clusters] do not lead their existence in complete independence of each other; for one elemental force joins them all however far they are mutually separated: gravitation which according to Newton's law governs the action of each upon the other." (Ibid., p. 135) In the second part, Exner broadly discussed the Kant-Laplace theory according to which the stars and planets were formed by matter aggregation. But the picture he drew for the Universe *on that large scale* replaced the Newtonian clockwork with Boltzmann's kinetic theory.

Why the fixed stars are in motion, nobody knows the answer; but that much is sure and can be easily ascertained, even the fixed stars closest to us cannot exert such a force at the sun that suffices to explain its motion. Perhaps it happens in the large as in the small; our air consists of small spherical molecules which are at large distances from one another and which all move in disorder in random straight lines without one exerting the smallest force upon the other unless two of them collide by chance. Hence in effect, our air presents the same scene in the small as do the clusters of stars in the large; however, what according to the dimensions here takes place in a split second there requires countless millions of years. (Ibid., p. 109f.)

To Exner's mind, this parallel has philosophical consequences:

παντα ρει, in English: "Everything is in flux," is the conclusion arrived at by the philosophers of all times and places, and not only the old thinkers of India but also the younger and contemporary explorers of nature. But, naive man, though he daily experiences the changes, does not believe in them and considers nothing so unchangeable as nature with its mountains and rivers. (Ibid., p. 235)

Exner's *Simple Astronomy* clearly demonstrates that his Inaugural Address was not the spontaneous declaration of an indeterminist philosophy that he would spell out only in his *Lectures*, at a time when almost everyone had accepted Boltzmann's atomism.

4.6.2 From Chaos to the Present

Exner's cultural aspirations went considerably further. But it took until his retirement in 1920 and the challenge exerted by Spengler's *Decline* before he wrote down what had matured over the years and had been discussed at length in the Exner circle. Spengler's book met with many reactions among scientists, who were responsive to his cultural diagnosis but decidedly negative with respect to his treatment of the sciences. In Exner's case, the Spenglerian challenge did not only aggravate the statistical physicist who defended the values of science and intended to apply his approach – to the extent that this was possible – to all humanistics. Spengler's thesis about the decay of ancient science provoked also the humanist, which Exner

[93] This passage contains one of very few emphases in the book.

understood in a more global sense than Humboldt. Spengler's *Decline*, it will turn out, was the true heir of those two imposing volumes "General History of the World" which had motivated Exner's *Simple Astronomy*.

Exner did not live to publish *From Chaos to the Present*, but the 436-page mimeographed typescript must be considered as a full-fledged first draft. At least he sent the manuscript whose Preface consists of eight strophes and is dated November 17, 1923, to the Viennese publisher Julius Springer[94] in the spring of 1924, half a year before he temporarily lost his eyesight by a brain stroke from which he would not recover fully until his death. Still in 1925, a small booklet *From Prehistoric Times* appeared in Steyrermühl's *Tagblatt*-library which contained material from the initial chapters of the manuscript.

Let me give a brief overview of Exner's entire manuscript. After his poetic Preface, Exner stressed that his main aim is not just to broaden the horizon of classical humanistic ideas about the history of the world. "Who in himself feels the urge to understand the world surrounding him and its phenomena will soon become aware that such is possible only if he recognizes how all that has come into existence, and conversely he tries in vain to fathom the past as long as the present remains unknown to him." (Exner, 1923, p. 4) Just a glimpse at the stars teaches us that the present includes the laws of nature and the past extends back to the formation of our solar system. Consequently, the first chapter contains roughly a summary of those topics that had made up the *Simple Astronomy*. Exner then moves on to outline the development of the Earth up to its present shape and discusses basic results of physical geography and paleontology. Observing that both the Earth and life are unfinished, he once again pronounces his leitmotif παντα ρει, which "for the most part strikes the eye in all living" (Ibid., p. 68) and opens the way for evolution and development. When discussing Greek culture later in the book, Exner remarks that "Heraclides was called the obscure, probably because the problems treated by him were far from the spirit of the day. But with his παντα ρει ('Everything is in flux') he only expressed what later, much later became the basis of all natural science." (Ibid., p. 259)

Two particular aspects of Exner's précis of primeval evolution are of interest in the context of indeterminism. First, the founder of the Institute of Radium Research happily pointed out that nuclear decay naturally furnishes a geological and paleontological chronology that is no longer based on mere comparison of different forms. Second, his exposition of Darwinism linked up to his frequentist account of probability.

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[94] In Exner's papers one finds three documents with the letterhead of the Viennese publisher Julius Springer. On February 9, 1924, Leo Friedlaender, the *Prokurist* of the publisher, wrote:

> Dear Herr Hofrat!
> I kindly ask you to commit your manuscript to the bearer of this letter and this letter at the same time counts for an acknowledgment of receipt of your manuscript (without preface and index).

And on April 17, 1924, Exner receives another letter:

> Dear Herr Hofrat!
> I have the honor to deliver to you the attached acknowledgment of receipt of your manuscript by the Steyrermühlverlag (signed Dr. Winkler) and I give my most respectful regards.
> ppa Julius Springer Leo Friedlaender.

The acknowledgment dated April 16, 1924, reads: "The signed publisher confirms receipt of the manuscript of Hofrat University Professor Dr. Franz Exner 'Vom Chaos zur Gegenwart'." (Österreichische Nationalbibliothek, Nachlaß Exner, 294/86).

It appears as if this evolution initially proceeded slowly, but later became faster and faster, to wit, the lower the organisms the slower their progress to higher levels. It looks as if nature must find the ways of development by trial and error, where that has died out which was not capable of development. And do we not meet the very same in other areas again? In every science, in every art the same repeats itself again and again, the first groping attempts take most of the time…. (Ibid., p. 97)

It was a prerequisite for the empiricist reading of the frequentist account of probability that there was enough time that all possibilities could be actually tried out.[95]

After discussing the various prehistoric ages distinguished by their prevailing technology, Exner turns to the historical time – from 4000 BC on – that once again started from a chaos of peoples because mankind presumably emerged at different places. "Only where a certain appreciation of outstanding individuals as against the mass gains ground, where weighing has replaced counting, there can be talk of a beginning culture." (Ibid., p. 157) Exner subsequently discusses millennium after millennium, studies how culture after culture emerges and decays, how they develop new institutions and ideas, the viable core of which is passed on to the cultures supplanting them and which are exchanged with others until the culture of Europe attains global character. In all of this, *From Chaos to the Present* impresses the reader with its enormous factual material that also gives due space to non-European cultures. And on his journey through history and the cultures, Exner discovered various (*ceteris paribus*) regularities in the development of both nature and culture.

4.6.3. The Emergence of the Objective World View

As did Mach and Boltzmann, Exner advocated a naturalist epistemology. Thus, also the emergence of the objective world view and of the single sciences represented objective events in the cultural evolution of mankind. In *From Chaos to the Present,* Exner initially discusses some sciences that were developed very early by various peoples, for instance, the grammar of the Indians or the four millennia of Chinese objective history. The crucial point in such cases was that the drive to know transcended the purely practical requirements. In ancient Egypt this had not been the case; the pyramids were built exclusively by means of ramps; however "with an appropriate understanding the merely empirically employed principle of the inclined plane would have sufficed to make use also of all the other simple machines," (Exner, 1923, p. 201) to wit, the lever, the block and trackle, the screw, and the wheel and axle. This passage contains two implicit references to Mach's *Mechanics.* First, Mach called the inclined plane, the lever, etc., "principles" while he downgraded energy conservation, etc. to "theorems" (Cf. Sect. 3.1). Second, he claimed the factual equivalence of all simple principles or machines.

When the Phoenicians sailed around Africa (or when the Vikings discovered America), the cultural effect was poor.

Had these voyages been undertaken some centuries later, their results would have been entirely different, we would have learned also about the peoples then inhabiting the African coast. But, in such enterprises it always matters that they are carried out at the right time, at a time when mankind is already capable of understanding and adapting their results. (Ibid., p. 221)

[95] See Frank's treatment of probabilistic evolution (Sect. 8.5.).

This capability required a radical change in culture which – at least in the Occident – separates two distinct worlds. It took place in the Hellenistic school of Alexandria from 300 BC to 300 AD, from Euclid to Ptolemy and Hero.

> The natural sciences clearly stood in the center of this development and the transition to them was a step whose significance is often not sufficiently acknowledged. It was a step leading from the subjective world view to the objective one, the question was no longer how the world appears to us but how the world is and how it would be even if there were no humans, and not only how the course of events is but also why this happens thus and not otherwise. (Ibid., p. 278)

Belief was replaced by the critical search for truth. After proof and experiment had become the criteria of knowledge, all findings could be reproduced and disseminated more easily, in like manner as writing had accelerated the contacts among peoples. Moreover, "precisely due to the objective truth of its results, science has the remarkable property that there is no 'way back' in it, but only a necessarily arising expansion, and this will survive all future epochs as long as mankind still has ideals." (Ibid., p. 281) To properly apprehend Exner's continuist thesis and render it consistent with the cultural pluralism admitted by the statistical regularities, recall that – in contrast to Spengler – Exner distinguishes facts and theories. While the latter are often subjected to dramatic upheavals, factual knowledge once attained never gets lost, at best it is temporarily inactive.

While science in general emerged in Alexandria, Exner credits the Arabs – Geber's early chemistry foremost – for developing experimental science that is characterized by the "wish for a theoretical understanding of natural processes…, of a classification of the phenomena into groups that belong together according to their causes." (Ibid., p. 330) "The sober and entirely objective approach to natural processes by the Arab researchers was a counterweight against Greek speculation that should not be underestimated." (Ibid., p. 364)

In an admittedly defective form, Exner here accentuated an insight – which has become standard today – that the emergence of modern science requires both a theoretical and a practical component. But he assumed only a very loose connection between socio-political and spiritual-cultural developments. Observing that the Incas and Mayas directly advanced from the Neolithic Period to the Historical Age without developing a culture based on bronze or iron, he asserts: "This fact … proves that the connection between cultural and technical progress actually is less intimate than is commonly believed." (Ibid., p. 126) Nevertheless, Exner considered the usual sequence of the archeological ages in which the cultures are classified according to the prevailing technology as a *ceteris paribus* regularity (Cf. Exner, 1925).

4.6.4. Culture is a Natural Product

> One ought to be blind not to see that the cultural achievements of Stone Age man have been preserved to the peoples until the present day…hammers, chisel, saw, awl and needle, cloth and pottery, bow and arrow, and daggers and sword, all that lives today as in those days and awaits being replaced by better ones. … But it is not only the technical achievements of the prehistoric man which persist in all cultures, it is also the spiritual and ethical sentiments, provided that we can assume them in the earliest

times. The age-old custom of human sacrifice is retained in all cultures, but in an always more spiritualized form. (Exner, 1923, p. 141)

Although material tools played an important role, throughout *From Chaos to the Present* Exner – in contrast to Mach's thought experiment (Sect. 3.9.) – put main emphasis on spiritual content, on the scientific and cultural ideas, and on the social and ethical development. They reveal a continuous – but not deterministic – progress while the life of the individual cultural organisms is full of ruptures and discontinuities. This difference resembled the distinction between the microcosmic level where discontinuous and random changes occur like molecular collisions in a gas and the macrocosmic level where certain weak regularities emerge and admit reasonable predictions. Also for Exner historical predictions were possible.

By the same necessity [as the quality of a semen determines the fruit] our present state results from all previous events, and who is ignorant of this connection or even denies it, is at loss when confronting the future; for only one who has recognized how something has come into existence can justly foresee the further development. (Ibid., p. 277)

But all historical predictions yield only average values and tendencies to equilibrium which, in stark contrast to Spengler's fatalist determinism, admit manifold exceptions. Yet, to apply his indeterminist theory and to compare cultural units with microparticles, Exner needed a further hypothesis to exclude any historical teleology that could counteract, positively or negatively, the tendency to equilibrium. Here physicalism came in.

Only where very catastrophic events – be they of physical or political kind – occur, what has been achieved already long before could go to ruins, as has, for instance, been caused to the old cultures of Central America by the brutal invasion of the Spaniards. But this is a highly singular case that can be explained by all kinds of circumstances; usually even the greatest political events, such as the marches of Alexander the Great, may well create or destroy empires but not cultures. In the cultural life of a people, political circumstances generally play a minor role; culture is a natural product with its own laws of growth which are not affected by political deeds to any considerable extent. What brings about culture is in first place the spiritual inducement [*geistige Veranlassung*] of the people and no less the external physical influences, the properties of the soil, the conditions of rain and irrigation in the country. (Ibid., p. 397)

Combining physicalism and indeterminism, Exner was able to maintain simultaneously the idea of cultural and scientific progress of mankind and a theory of the single culture's life that in some of its consequences was not all too different from Spengler's. This can be seen from a drawing in the Conclusion of *From Chaos to the Present* (figure 4). Exner emphasized that the "heights which are to illustrate the respective state of the cultures are entirely arbitrary; for who could numerically compare the heights of different cultures… . One immediately realizes that the four older cultures have undergone a much slower development than all subsequent ones, a fact which, of course, was caused by the relative secludedness of these countries in earlier times." (Ibid., p. 434) This observation provides the clue to two very general weak regularities that can be discerned in Exner's manuscript and that are often called "laws".

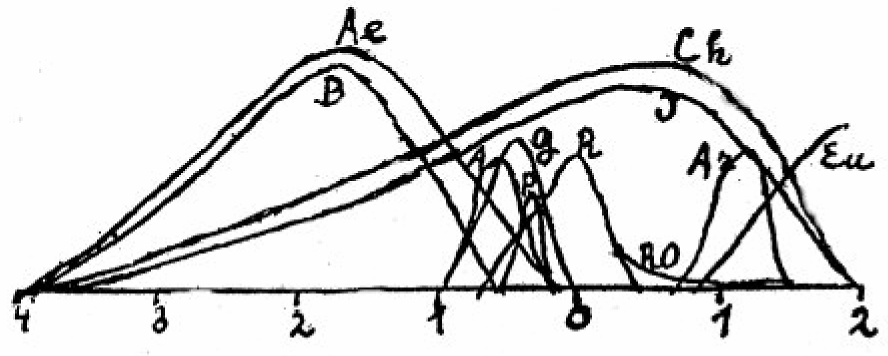

Fig. 4. The succession of different cultures. The letters denote Babylonian (B), Egyptian (Ae), Chinese (Ch), Indian (I), Greek (G), Assyrian (A), old Persian (P), Roman (R), East Roman (Ro), Arab (Ar), and European (Eu) culture.

First, the life of a single culture is the shorter the higher its cultural level has become. "It is comprehensible that in the course of progress random external influences attain more and more weight and bring a more rapid pace into the development. The very same we had seen with respect to the geological epochs, to wit, the higher organisms stand, the quicker are they subjected to changes into more purposive [*zweckmäßigere*] forms, and the same may well hold true for cultural evolution too." (Ibid., p. 119) As Exner identified the more purposive states in biological evolution with the more probable ones, this meant that the higher a culture had become the more rapidly external influences changed the most probable state.

Second, an increase of interactions between various sectors of a single culture and, in particular, between different cultures accelerates the evolution in the same sense as an increase in molecular collision rate, that is, a higher temperature, leads to the equilibrium state more rapidly. Exner's unswerving physicalism makes it tempting to compare (morphologically) the above figure to a probabilistic distribution function familiar from statistical mechanics, for instance, the Maxwell-Boltzmann distribution for the velocity of molecules in a gas (figure 5). Perhaps figure 4 then could be seen to measure the new scientific and cultural ideas contributed by the culture at a certain stage of its development. Within such a setting the cultural world might be viewed as a permanent interaction of different national cultures, and these in turn as the interaction of certain sub-cultures within them, such as science, arts, or economy, all of which are represented by a distribution function whose multiplication yields the overall distribution function. Perhaps these remarks exaggerate what for Exner – who had explicitly denied the measurability of culture – only represented an analogy. Yet they provide an interesting illustration of how his theory of culture perfectly availed itself of the micro-macro distinction of statistical mechanics.

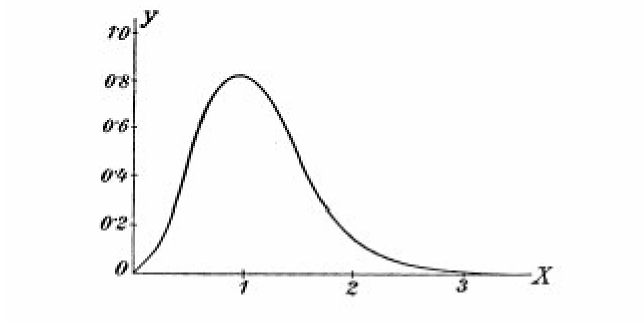

Fig. 5. The Maxwell-Boltzmann distribution

Still, the main problem of the probabilistic approach remained to define a measure of order and disorder. Thus in the concrete applications of Exner's probabilistic laws the specter of tautology is lurking; in particular, when we reach our own cultural sphere, traditional value judgments step in as an allegedly physicalist measure.

The effect of this law [that the higher development yields the faster progress] was substantially supported by the favorable situation and arrangement of the Mediterranean area, a circumstance which in the history of the world was never offered to mankind. In a most favorable climatic position, equally far from the frosty pole and the overly hot equator the adjacent peoples bore in themselves the germs of cultural development, and the extended coastline and the numerous large and small islands in-between offered the most favorable opportunity for navigation and thus for trade, for the intimate contact among the peoples, and for the foundation of colonies in remote countries. (1923, p. 217)

Exner's introduction to the first millennium BC comes rather close to arguments familiar from natural teleology. The boundary conditions are so suitably arranged that interaction and progress follow with bare necessity and without any statistical fluctuations. Here Exner's interaction law simply degenerates into an idle tautology. Yet this caveat against the nomological value of the interaction principle shall not lessen its importance as an antidote against Spengler's segregation of the independent cultural organisms. Moreover, Exner's detailed comparison of the interacting Mediterranean cultures with the isolated cultures of India and China is quite suggestive.

Taken in this weaker form as an empirical fact, interaction was a necessary condition for Exner's main thesis about continuous progress in cultural history because it enables other peoples to take over and adopt the intellectual and scientific achievements of decaying peoples and extinct tribes. For instance, the most important deed of Alexander the Great was the planned dissemination of Greek culture in Asia, an impact that lasted much longer than Greek statehood. European culture stands at the end of the tendency of increased interaction among cultures because owing to its technological superiority it is about to become a global culture. Once again, we see that indeterminism and physicalism do not safeguard against writing justificationist history.

The emergence of the objective worldview was intimately linked with the circumstance that religion develops into ethics, which accordingly constitutes the common root of religion, art, and science. "What is basic to all of them, what constitutes their root is the ethos. With its awakening also culture begins, and this can be described as the state of a people whose life is essentially organized according to ethical principles." (Ibid., p. 159) Philosophical ethos emerges from religion in various ways. "One will expect each religion to originate from the belief in a deity [as a primitive explanation of natural phenomena], but in Buddhism the old Gods of the Brahman religion have been dethroned and replaced by the principle of causality which solely rules the world." (Ibid., p. 292) But this important intellectual achievement of India was short-lived. Buddhist philosophical ethics was too complex, so that the Indian people soon returned to the old Gods. But this relapse was less harmful than it appears. "[T]he cultural effect of the religions does not lie in their How, but in the first established ethical principles for life, and according to nature these are quite the same in all religions." (Ibid., p. 380) Despite their progressive ethical content, all religions are in danger of ossifying into mere dogmatism.

Exner's distinction between macrocosm and microcosm and the two laws also found their expression in biological terms. "The most general law of nature which is valid for all vegetable and animal life … reads: Nature does nothing for the individual, but everything for the genus. The individuals do not enjoy the protection of Nature, as long as the genus only is preserved." (Ibid., p. 157) In cultivated nations this law of primitive society might seem suspended, but in reality it constitutes precisely the basis of their instability. "If higher culture truly has originated in the esteem for the intellectual characteristics of individuals, if thus a people brings the individual in a certain opposition to the genus, then it acts against the general law of the organic and accelerates its decline." (Ibid., p. 158) In the struggle for existence between cultivated and uncultivated peoples as well as between educated and uneducated men, the latter "will always have the advantage, his acts are solely determined by the goal to be reached, not by the means leading to it, the educated one will find on his way everywhere inhibitions of ethical nature which prevent him to seize the most effective means." (Ibid., p. 206) Thus, uncultivated tribes without individual differentiation – a highly improbable state, as Exner had remarked back in 1908 – can be united under a common goal while the distribution of individual characteristics represent a stable equilibrium state that cannot be changed so easily.[96]

> After all, culture appears to be identical with the possession of ideas, in the single human as well as in the people; after them man aspires when philosophizing about religion, in artistic work, or finally when seeking the truth in science. In this sense one can say: ideals are the only thing that truly exists. (Ibid., p. 159)

Agreeing mainly with Spengler that all fine arts, except for music, have reached the stage of exhaustion and degenerate into decorative or formalistic exercises, Exner emphatically exempted science from the overall decline. The age of science was not over at all but full of steady progress.

> Although for most of the arts these days [of growth] seem to be already over, this is certainly not so for the sciences, and even less so because the highly developed technology equips the scientist with ever new means. The experiences of the past two centuries show that each science generates other sciences in its wake, and before our eyes the fields extend almost to infinity out of which always new ones sprout and which for interminable times promises rewarding work for the searching mind. (Ibid., p. 431)

[96] Needless to say, a decade after Exner's death some of the most cultivated nations of Europe had been united under a fascist leader's will with devastating consequences for their interactions.

4.6.5. A Ringerian Mandarin?

Here ends chapter 33. In the subsequent Conclusion Exner turned his interaction thesis into a lament against the degeneration of culture into civilization.

> [W]hat initially had been the product of a long development on a national basis and from ethical principles gradually becomes the outcome of habitual and conventional forms, the sense for the history of the present, the question as to *why* something is so and not otherwise is lost and it suffices to know *how* it is. In this way dies not only the comprehension of all institutions, but also of the whole surrounding of man. A telling example is the great American union; undoubtedly ahead of Europe in civilization, it is nevertheless constrained to satisfy its cultural needs from there. (Ibid., p. 435)

Recalling the productive role of interaction for cultural and scientific development Exner had repeated over and over again, this severe and pessimist criticism surprises. But already in the next sentence the author poured out his heart and returned to a motive that had already figured prominently in his Inaugural Address.

> Yet a banausic trend prevails in the world and everyone cries for equality. Nature, however, is an enemy of equality and everywhere turns the equal into the unequal. From the uniform chaos solar systems and their planets developed, from the uniformly melted masses of the earth internal forces created the most different types of rock, mountains and valleys, from the first monocellular organisms all the multifarious forms of plants and animals evolved, and even the hordes of the most primitive humans in which all were of equal right finally brought out craftsmen, artists, kings and philosophers. We thank what we are to all these generations passed away long before, in particular, to our ancestors from the ancient world.
>
> Shall thus the hollow phrases about equality of those who only live in daily triviality and who constantly tend to burn the bridges leading back to antiquity, and who believe to be able to replace culture by civilization, shall these phrases and their aftermath be spared to mankind as long as the sun shines. But the future lies in the dark. (Ibid., p. 436)

With these rather pathetic words Exner's impressive opus ends. All the scientific objectivity so vigorously defended against Spengler has passed away once cultural history has reached the present. That such a conclusion was at all possible reveals once again some of the internal incoherencies and shortcomings of Exner's indeterminist theory of culture, above all, the problem of defining an appropriate probability measure. While in the case of the Mediterranean area the very specific boundary conditions chosen rendered probabilistic evolution a mere tautology, here all is dominated by a value judgment necessary to identify culture with a national distribution of properties that was of Gaussian, Maxwellian, or similar type. Civilization is, on the one hand, the final product of the interactions fostering cultural growth. On the other hand, is it characterized by vast ignorance of this process and by slogans heading back into the primordial state of total equality and sharp individual values of all societal attributes that, after all and in accord with Spengler, makes civilization the enemy of culture. Evidently Exner's long-defended progressivist account of culture collided with a brand of Spenglerian pessimist cyclism for humanity as a whole.

What were the motives for Exner's radical about-face when entering his own epoch? Let me provide some circumstantial evidence for his motivations that derive from interpreting Exner – in contrast to Mach – as a Ringerian mandarin. In a seminal study, Fritz K. Ringer has analyzed the feeling of menace prevailing within Germany's intellectual nobility between 1890 and 1930. Ringer's study deliberately did not include natural scientists, in particular because they exhibited a different attitude with respect to positivism and technology. But in his theory of culture Exner speaks not exclusively as a natural scientist.

During the four decades covered by Ringer the mandarins' only capital, their education, suffered various devaluations. To check for some of the characteristic traits, the mandarins vigorously defended the idea of the rational state. (Cf. Ringer, 1969, p. 8ff.) Indeed, Exner (1923) ranked the law of Hammurabi and the subsequent development of state institutions much higher than the Babylonians' achievements in war. Another typical characteristic of the mandarin is the above-detected rigid differentiation between culture and civilization. The cry for equality, for participation of the people in politics, threatened the exclusive position enjoyed by the mandarin, a position that was not based on high birth but on a qualification passed over within the family. The mandarin followed the doctrine that the "state derives its legitimacy not from divine right, for that would stress the prince's whim, nor from interest of the subjects, for that would suggest a voting procedure, but exclusively from its services to the intellectual and spiritual life of the nation." (Ringer, 1969, p. 11) Similarly, Exner's history of the world had judged each nation according to the value of the ethical and scientific ideas achieved by it, while it had strongly reduced the role of great kings and emperors which dominated in the state historiography. Moreover, this doctrine explains Exner's opposition both against national organisms centered around a sovereign and against worldwide civilization. Compare his attitude during World War I. Benndorf's obituary bitterly complained that "Exner was lacking any understanding for the war; he only viewed in it a stigma of human morality [*Gesittung*] and ignored its historical necessity; and from his point of view he could never grasp how a man of science could deliberately prefer to take part himself in the fight murdering the nations." (Benndorf, 1927, p. 404) This passage alludes to Exner's former student and Boltzmann's successor, Fritz Hasenöhrl, who was killed as a war volunteer in 1915. Apparently, Exner's teaching was not successful in every respect. As a matter of fact and contrary to Benndorf's belief, Exner considered wars as "a necessary consequence" of the appearance of new peoples; "but fortunately they play only a minor role in the development of humankind." (both Exner, 1923, p. 187)

Despite Exner's ambivalence with respect to modern civilization, it is crucial that a major item on Ringer's list does not apply. While the mandarins bemoaned the decline of science, Exner exempted science from his final mandarin pessimism about civilization, at least as long as science did not degenerate into merely banausic technological know-how. In Exner's case, this difference cannot be fully explained by the positivism and support of technology required from any practicing natural scientist. Rather do I think that the economical situation of the Vienna physicists in the days of Exner was exceptional because they stood in the middle of a belated "Gründerzeit".

4.7. The Institute of Physics

Founded in 1850 by Christian Doppler, the Institute of Physics was the oldest institution of its kind in the German-speaking world. However, until the eve of World War I it was lacking an appropriate building, and its funding could not compete with comparable German departments. It was only Franz Serafin Exner who in 1910 could open the new Institute of Radium Research of the Austrian Academy of Sciences and who in 1913 succeeded in ending the six decade long accommodation malaise by a spacious building next to it. As we shall see from Exner's and Boltzmann's recollections of the earlier epoch of Stefan and Loschmidt, the situation was aggravated – to remain in Ringer's economical picture – by self-imposed trade barriers and a poor publicity that rendered their scientific capital non-convertible. Boltzmann and Exner were the first to hold positions in Germany.

Exner's activity as a founder might explain the split outcome when checking for the mandarin criteria: in cultural matters Exner undoubtedly resounded the core mandarin ideas but he remained an unswerving optimist in scientific matters. In this section I shall provide some material how Boltzmann and Exner viewed the history of their institution, in particular in order to show that despite the mentioned deficiencies there existed a remarkable team spirit the importance of which cannot be overestimated for assessing the later cohesion of Vienna Indeterminism, in particular in the case of Schrödinger. Exner thus continued – with the above-discussed very personal twist – the local traditions of an institution unique of its kind.

The second characteristic of the Vienna Institute began only with Mach and Boltzmann. Here is another passage from Kuhn's interview with Frank who studied there from 1903 to 1907 that stresses the philosophical aspirations prevailing at the place.

TSK: In retrospect, how would you say doing science in Vienna was different from, say, if you had done it at Munich or Göttingen?
F: I would say that one of the [most important] things was that in Vienna [the professors and students] were more interested in the philosophy of science, and definitely more than elsewhere. It was very strong. It was probably the influence of Mach at that time, as well as other people who followed a similar line. Yes, it was strong, very strong, I would say. (quoted from Blackmore, 2001, p. 66)

This philosophical orientation has guided my considerations for the period after Exner because my aim here is not to contribute a brief history of the Institute of Physics.[97]

4.7.1 The Era of Loschmidt and Stefan Seen by Boltzmann and Exner

Here are Boltzmann's recollections about the remarkable spirit of the old Institute at Wien-Erdberg.

Stefan and Loschmidt … were different in many respects. Stefan was universal and treated all chapters of physics with equal love; Loschmidt was one-sided, when brooding over an issue day and night, he turned his mind away from practically anything else. Stefan was practical, he treated the applications of his science to technical and industrial purposes with pleasure and skill; although once active in factories, Loschmidt was the prototype of an impractical scholar.

In one respect, however, both were completely identical, in the infinite modesty, simplicity, and straightforwardness of their character. Never did they attempt to express their intellectual superiority in academic conventions. Albeit a student at first and then a long-time assistant, I have never heard from them any word other than as a friend addresses a friend; and altogether the Olympian

[97] For a collection of the relevant historical facts, see (Binder, 1949) and also (Karlik and Schmidt, 1982). Binder's thesis is rich in material but sometimes meager in conclusions. It should be read with care concerning all events between 1934-1945; still in 1949 certain things had not officially happened in Austria.

cheerfulness, the sublime humor which for the student turned the most difficult discussions into joyful play, have become ingrained to my mind so deeply that it became, as it were, part of my own character. I did not even suspect at that time that it was unseemly for me (a learner) to join in discussions in my normal tone of voice. On my very first day in the laboratory in Berlin when I harmlessly spoke in my usual manner, a single glance from Helmholtz explained the situation to me. When I tried to depict that glance to Herr Glan who was then an assistant there, he answered with pride: "You are here in Berlin." (Boltzmann, 1905, p. 102)

As would do Exner, Stefan "spellbound the academic youth." (Ibid., p. 101) Boltzmann continued his teachers' informal conventions and impressed his students and visitors, among them the young Walter Nernst (See Sect. 5.5.2.). According to a 1944 letter written by Stefan Meyer to Hans Benndorf, Boltzmann left complete freedom to his assistants and "was not only a great teacher, but was in spite of his eccentricities a truly good man, with a strong and outspoken sense for his family and good will for others. But one could not get as close to him as to our Exner." (quoted from Höflechner, 1994, p. III 9) All this fostered the production of new ideas.

Yes, for my whole life, Erdberg has remained for me a symbol of serious, inspired [*durchgeistigt*] experimental activity. When I first succeeded in bringing some life into the physical institute in Graz, I named it Little-Erdberg as a kind of joke. Spatially it was not small; in fact it was twice as big as Stefan's Institute, but for a long time I was not able to capture the Erdberg spirit there. Even in Munich when the candidates for doctor's degrees came to me and would have gladly worked at something, they were at a loss what to do. I thought to myself that we at Erdberg were a different breed. Today we have the most beautiful instruments sitting around us, and one weighs the question accordingly what to do with them. We [at Erdberg] always had enough ideas; our concern was where to get the equipment. (Boltzmann, 1905, p. 100f.)

Boltzmann combines his enthusiasm with a critical note. "To the best of my knowledge, neither Stefan nor Loschmidt ever traveled outside their native Austria. At least they never attended a *Naturforscherversammlung* and never entered into closer personal contact with foreign scholars. I must disapprove this; with less secludedness they could have achieved even more." (Ibid., p. 102) Their recognition severely suffered from their this restraint. Loschmidt, so Exner complained, "who had given to the world the secure foundation of atomistic remained an unknown on many occasions." (Exner, 1921, p. 177) And he demurred that Loschmidt's number often was given the name of Avogadro "who had absolutely nothing to do with it." (Ibid., p. 179)

All restraint notwithstanding, the Vienna physicists closely followed the developments elsewhere and imported the spirit of modernity. Thematically there existed in effect a double axis between Vienna and England until the days of Boltzmann. First, most scientists had become so used to the two electric fluids that the ideas of Faraday and Maxwell attracted little attention outside England. "Only two physicists of the Continent immediately recognized the importance [of Maxwell's theory]: Helmholtz and Stefan." (Boltzmann, 1905, p. 96) The relatively great influence of British empiricism in Austria presumably also facilitated the reception of the philosophical aspects of Maxwell's work at the Vienna Institute. Second, even during the heydays of energeticism Vienna remained a stronghold of the kinetic theory of gases in the works of Stefan, Loschmidt, and ultimately Boltzmann.

The most important contribution of these early days was Loschmidt's paper "On the size of the molecules of air" of 1865. "On a few pages it contains the solution of a problem which for millennia, since Democrit and Epicure, has occupied the best minds … Tremendous is the effect of measure and number." (Exner, 1921, p. 179) In his newspaper article Exner extolled it as "by all means the most important and most far-reaching idea ever originating from the University of Vienna." (1895) For there opened a "boundless cleft separating, since the beginning and until the end of time, *Naturphilosophie* and natural science" (1895) Interestingly, Boltzmann placed the discovery into the borderland between physics and philosophy and stressed the differences between the old and the new atomism. "While in those days [of Loschmidt] one was seeking for the ultimate elements of being, of matter in itself, today one asks from which elements to compose the ideal pictures in order to reach the best agreement with the phenomena." (Boltzmann, 1905, p. 241)

"Loschmidt was a master of experimentation … but the majority of his experiments failed partly because the means were insufficient, partly because his experiments were devised too fine such that the chances of their success were too low." (Ibid., p. 232) Loschmidt's last experiment ended after three years of fight against the insufficiency of his equipment with the following notebook entry "Given up because building is shaking." (according to Exner, 1895)

When Stefan and Loschmidt died in 1893 and 1895, this intellectual atmosphere continued under their successors Boltzmann and Exner. During his final Viennese period, Boltzmann had to "share 'his' pupils with Exner" (Höflechner, 1994, p. 239). The old Physical Institute had been divided into three institutes and all the introductory teaching had been given to Exner. Although on the occasion of Boltzmann's return in 1902 both had a disagreement concerning laboratory equipment, which in large part was transferred to Exner, the available documents indicate a mutually friendly atmosphere including due respect for the excelling scientist on Exner's part.

This sketch reveals two features of relevance for establishing a local philosophical tradition. Firstly, the Institute of Physics suffered from a certain isolation from the German physics community. Secondly, the institute's intellectual atmosphere fostered philosophical and cultural discussions, such that it may be considered as a forum complementing Alois Höfler's 'Philosophical Society of the University of Vienna'. But most important for the dissemination of Vienna Indeterminism was the unique team spirit and the inclination to philosophy that extended beyond the narrower circle around Exner.

4.7.2 The Institute after Exner's Retirement: The Example of Hans Thirring

To be sure, by 1918 the Olympian age of Vienna physics had ended although certain areas of physical research remained on a respectable level. After Exner's retirement, the influence of his ideas persisted through his former students and assistants. Indeterminism and statistical causality were also basic tools for the daily research on radioactivity in the Institute for Radium Research now directed by Stefan Meyer.[98] Against this philosophical background, one can also understand the particular way how the Viennese physicists assessed statistical fluctuations in radioactive decay (See Coen, 2002).

[98] See the respective sections of (Karlik/Schmid 1982) and more specifically (Reiter, 2001).

In the following I shall, however, focus on the theoretical physicist Hans Thirring (1888-1976) and discuss one of his papers in *Die Naturwissenschaften*. Thirring was one of the local Viennese bridges across which the philosophical discussion moved on into the days of the Vienna Circle. His paper represents an important document that Forman's diagnosis about the Weimar milieu cannot simply be extrapolated to Vienna – although the German and Austrian scientific communities were closely linked – and shows that, contrary to Beller's belief, Bohr was viewed as a kind of prophet even before 1926.

When in 1918 Gustav Jäger succeeded Fritz Hasenöhrl, he required a position for Thirring to assist him as a lecturer in modern theoretical physics. After Exner's honorary year was over in 1920, Jäger moved to Exner's higher-ranking chair and Thirring obtained the chair of theoretical physics. Thirring's most important result (1918), which was later improved together with Josef Lense (1918), concerned rotary motion in general relativity. By considering two rotating concentric cylinders isolated from any other masses in the universes they studied the dependency of centrifugal force from the inertial frame. The discovered frame dragging effect was a quite specific and experimentally verifiable instantiation of Mach's principle of the relativity of motion which Planck had so emphatically rejected. (See Sect. 3.7)

In the 1920s, Thirring was quite active in the popular and philosophical debates about relativity theory and he published several papers and reviews in *Die Naturwissenschaften.* Quite interesting is his rebuff of Einstein's archenemy Philipp Lenard. Quoting long passages from the attempted alternative to general relativity, Thirring curtly concluded "that by introducing a sufficient number of additional hypotheses absolutely everything could be explained – science in the end will decide in favor of the explanation which is the simplest." (Thirring, 1923, p. 229) Lenard's strategy to avoid Einstein's revolutionary but well-rounded and coherent theory "leads into a thicket of hypotheses and if pursued further it would become lost in endless speculations." (Ibid., p. 230)

In later years Thirring became a board member of the Ernst Mach Society which acted as the public forum of the Vienna Circle and frequently lectured there (Stadler, 2002, Ch. 7.2.1 – 7.2.4). Together with the Society's president Moritz Schlick, he spoke on the occasion of unveiling Mach's bust in the Vienna Municipal Park in 1926. After 1933 Thirring took part in each of the lecture series organized by Karl Menger (Stadler 2001, p. 420f.) the first of which bore the programmatic title "Crisis and Reconstruction in the Sciences" (Cf. Sec. 1.1.2.3). Thirring lost his chair after the Anschluß in 1938; after the war his main activity was in international peace politics and wrote on various general issues, among them a joint criticism of Spengler and Nietzsche (Thirring, 1947, Ch. 9).

Thirring's 1921 inaugural lecture at the University of Vienna qualifies as a source text for the Forman thesis and for the philosophical debates waged in *Die Naturwissenschaften.* Perhaps it has escaped Forman's attention because it contains neither a direct reference to causality nor an explicit mention of the phenomenon of crisis. Rather is the speech dominated by an optimistic tone and characterizes "the present epoch as by all means an unprecedented bloom of physics." (Thirring, 1921, p. 1027). Combining technological and Machian vocabulary, he asserted that "the description of nature made by theoretical physics is the most efficient and most perfect with respect to the economy of thought we know of." (Ibid., p. 1024) Interestingly, Thirring contemplated a philosophical agenda for the next centuries to "investigate which kinds of mathematical description of natural phenomena exist and which one is the most efficient one." (Ibid., p. 1025) At present, however, there are basically four different methods of inquiry. Thermodynamics, for instance, is built upon three basic laws from which the whole theory follows (without further hypotheses) by mathematically simple deductions. Although methodologically thermodynamics, accordingly, wins the palm among all physical disciplines, Mach and Ostwald went too far in opposing Boltzmann's atomism. "It would, however be regrettable to consider classical thermodynamics as over and done with or as a mere corollary of statistics." (Ibid., p. 1025) Rather is it a pattern of methodological purity that in many respects is paralleled by relativity theory; the major difference being that relativity it is not grounded in phenomena so intuitively evident as the absence of a perpetuum mobile but in revolutionary new concepts. No wonder that Thirring glorified Einstein's achievements.

Notice Thirring's hymnus on Bohr in the section dedicated to the "*atomistic* [Atomistik] of matter and electricity as well as the mystic of radiation, *quantum theory*." (Ibid., p. 1026)[99] Methodologically, this field stands in sheer opposition to thermodynamics and is dominated by tentative hypotheses about unobservable particles, which are advanced in a trial and error fashion. Yet this was not a sign of indigence.

> Atomistic and quantum mechanics are, however, sciences of imagination; in a certain sense they remind one of the sciences of antiquity and the middle ages. The Greeks worked a lot with imagination, and perhaps even more our ancestors in the middle ages. If some holy man, who enjoyed sufficient authority among his ecclesial brothers, had an inspiration, this was straightway raised to a divine dogma – and in a certain respect something similar still happens in physics today. One only works now more efficiently and tolerates the dogmas only as long as they yield fruitful consequences that agree with experience.
>
> A man so divinely gifted that he apparently succeeds in tracking down the secrets of the microcosm in a purely intuitive way, is … Niels Bohr in Copenhagen. Bohr's theory of the spectral series has not arisen on a secure deductive path out of empirical facts but in an intuitive manner from the imagination of his head. (Ibid., p. 1027)

At the surface level, Thirring's praise for Bohr seems to fulfill the demands of the post-war milieu insofar as he commended the dominant roles of imagination and of the individual within contemporary physics. It is also quite interesting that Bohr's authority apparently is not just a post 1926 creation as Beller (1996, 1999) suggests.

Yet all this rhetoric of genius was blooming in the field of a rather low-brow empiricism based on trial and error. Consequently, the absence of the simple and unifying deductive framework in atomic physics did not trigger a diagnosis of crisis. Here is how the quoted passage continues. "As long as it [Bohr's theory] proves successful with respect to experience and helps us to unravel the mysterious labyrinth of the spectral series, we have no reason to ask for the origin of its laws." (Thirring, 1921, p. 1027) And even if Bohr's speculations ultimately failed, they would still enjoy the historical merit of having motivated many important experiments. Thus Thirring at bottom argued that, for the empiricist, methodological pluralism was not at all problematic because it resulted from the different historical evolutions of the single

[99] Thirring here obviously alluded to Sommerfeld's (1920) paper that had attracted a lot of attention; see Sections 1.1.1.2. & 1.1.2.3.

fields of inquiry. His repeated emphasis on efficiency and economy can be traced back to the pragmatic aspects of Mach's principle of economy. Although Thirring sided with Boltzmann as to the independent character of physical theory, he rejected any realist connotations even of successful physical concepts. The Mars dwellers would suffer the same empirical facts but describe them by means of totally different concepts embedded into their intellectual culture. This was of course an allusion to the Planck-Mach polemics, and Thirring visibly took Mach's side. When comparing this account to the First Vienna Circle's emphasis on French conventionalism, Thirring appears closer to Mach's original teaching, or rather to the synthesis of Mach and Boltzmann characteristic for the Institute of Physics. This is, to my mind, also the reason why the issue of causality was not mentioned explicitly. Within an empiricist account and granting a plurality of the methods used in different fields of inquiry, there was no philosophical basis to a priori require a particular form of physical law or causal explanation. Or put differently, predictive success of certain functional dependences automatically implied the weak Machian notion of causality.

The Machian link also sheds some light on the issue of imagination in atomic physics as mentioned by Thirring. Mach continuously emphasized that hypotheses are created by the reconstruction, connection, and mutual adaptation of experiences in thoughts. This attributes a constitutive role to imagination, as Haller (1986c) rightly emphasizes. In the progress of science and in practical life, imagination is counterbalanced by the principle of economy and controlled by the empirical adequacy of the imagined facts. With Boltzmann and against Mach, Thirring asserted that this process does not hold at the level of our life-world, e.g., the experiences of heat and pressure, but continues into the microscopic realm and the expanses of the universe. This teaches that Thirring's concept of imagination had only a partial overlap with the quests for *Anschaulichkeit* originating from the cultural milieu. In a nutshell, imagination concerned the context of discovery while *Anschaulichkeit* purported to play a role in the context of justification as well. It was a weakness of the Machian concept of intuition [*Schau*] – and perhaps a reason for his skepticism towards some parts of modern physics – that after a long process of refinement intuition together with biological corroboration came back as a final justification. (Cf. Sect. 3.1)

5. *Die Naturwissenschaften* as a Forum for Scientist-Philosophers

In its next phase the causality debate between Vienna and Berlin enters the short years of the Weimar Republic and the First Austrian Republic. After the lost war and the decline of the empires, the political conditions and the cultural milieus underwent severe changes. In spite of the far-reaching continuity of the academic elites, German and Austrian scientists suffered from the economical collapse no less than other comparably situated citizens. Moreover, German scientists were initially banned from international congresses and institutions. The generous support for physical research mentioned in Section 1.2. was thus one among few exceptions. Less dramatic, to be sure, but essential for the causality debate was a major change in the forum for philosophical debates among natural scientists that had occurred just one year before the war. It is the focus of the present chapter.

As Forman (1971, p. 6) rightly observed, the philosophical reflections of scientists were typically made in front of a general academic audience on the occasion of the inauguration of a new rector or a new university professor, or during a celebration of an academy. But also the plenary sessions of the *Naturforscherversammlung* gave ample space for general and philosophical topics. After all, Lorenz Oken had founded this society in 1822 amidst German *Naturphilosophie.*

Often such speeches were printed as a separate booklet, but they also went through several journals, among them the *Physikalische Zeitschrift* in which the Planck-Mach polemics had taken place, and the journals of the learned societies and professional organizations. Later these papers were assembled in collections bearing titles such as *Populäre Schriften* (Popular Writings) – in the case of (Boltzmann, 1905) or (Mach, 1987) – or *Physikalische Rundblicke* (Physical Panoramas) – so reads the first collection of Planck's papers which developed into (Planck, 1944).[100] The appearance of such a book typically testified the author's becoming – self-consciously of willy-nilly – a scientist-philosopher. One of the very few journals suitable for philosophical debates among scientists had been Ostwald's *Annalen der Naturphilosophie;* yet in 1921 they ceased publication.

In 1913 the media landscape for scientist-philosophers changed. Philosophically oriented academic addresses from now on appeared to a growing part in the weekly journal *Die Naturwissenschaften* that strove to follow the major developments within the whole of natural science and present them in a generally comprehensible and captivating form. Other than within the publications of learned societies, there the general philosophical discourse was well-planned by the journal's founder Arnold Berliner. This includes the second phase of the causality debate between Vienna and Berlin (Chapters 6-8).

Here is how Berliner and Curt Thesing, the first in a series of co-editors stemming from the biological sciences[101], described the scope of the newly founded

[100] The dissemination of Planck's philosophical papers can easily be tracked in the bibliography of his non-technical writings edited by Heilbron (1977).
[101] These were August Pütter (1914-1921), Hermann Braus (1922-1924), and Hans Spemann (1925-1934) who remained on the editorial board until 1940.

"Weekly Journal for the Progress of Natural Science, Medicine, and Technology" – so the initial subtitle[102] – in a brief introduction.

The rapidly progressing specialization in all domains of natural science [*Naturforschung*] makes it harder for the individual scientist to orient himself even about the adjacent domains. Utterly impossible, however, becomes the orientation about the more distant domains. On the other hand, the intellectual need not to lose the connection with the whole, is felt by everyone the more strongly the more he is forced to narrow down the field of his own work – after all in most cases he depends upon the help of other branches of natural science. … [*Die Naturwissenschaften*] intend to inform everyone active in the domain of natural science about the progresses in the whole of natural sciences. ("Zur Einführung", *Die Naturwissenschaften* **1**, 1913, p. 1)

The editors' introduction was followed by two short papers of the physicians Oskar Hertwig "Natural Science and Biology" and W. His "The Physician and Natural Science" which functioned as greeting addresses emphasizing the intimate connection between the life sciences and the natural sciences.

When proposing his journal project to the publisher Ferdinand Springer on 6 May, 1912, Berliner had in mind a German analogue to the British *Nature.* Berliner's more detailed letter of 6 August, 1912, contained a passage very similar to the above-quoted introduction and continued as such:

The endeavor to be topical makes it advisable to publish the *Naturwissenschaften* weekly, the endeavor to be captivating yields the type of its collaborators: It is necessary to interest [as possible contributors] the leaders of the single domains or at least those fully oriented about it, in particular those who can describe matters in an interesting and comprehensible fashion. (quoted from Autrum, 1988, p. 2f.)

As planned by Berliner, every issue began with a 'top quality' article by a well-known scientist. After reviews, authors of which were rewarded with a honorarium, there came reports of scientific meetings of general interest, brief communications (*Zuschriften*), and an overview of scientific research and teaching at universities and academies. In particular after the war, the brief communications were an important vehicle to rapidly disseminate new findings. (Cf. Holl, 1996, p. 133). While initially the percentage of reviews and communications was rather large, it decreased over the years. When in 1924 *Die Naturwissenschaften* became the organ of the *Naturforschergesellschaft* and the *Kaiser-Wilhelm-Gesellschaft,* they also published the news and communications of these societies, research reports of the KWG-institutes, and the main lectures of the *Naturforscherversammlung.* Interestingly, the plan to make *Die Naturwissenschaften* the official organ of the *Naturforschergesellschaft* dates back to the founding year 1913, but the respective proposal encountered some opposition within the society. (Cf. Sarkowski, 1996, p. 194f.) Nonetheless, there were discernible parallels from the very beginning. For instance, the journal's table of content typically followed the system of the *Naturforschergesellschaft.*[103]

It took as breathtakingly little as eight months until Berliner's proposal to Springer had materialized and the first number of *Die Naturwissenschaften* was out. It is also impressive that the editor reached his main goal almost instantaneously;

[102] In 1922 the subtitle was changed into "Weekly Journal for the Progresses of Pure and Applied Sciences".
[103] Perhaps due to lack of time, this was not done for each volume.

browsing through the 1913 volume one finds many prominent authors writing survey articles about a great variety of topics. It appears to me that Berliner's *Naturwissenschaften* also gave a fair coverage of the whole domain of the natural sciences, technology, and medicine. To rigorously prove such a thesis would of course require a large-scale research project availing itself of modern bibliometric methods. Here I can only cite obituaries and recollections of scientists – mainly physicists – documenting that the weekly magazine had indeed informed them well about the whole of natural science.

All testimonies about Berliner emphasize the singular nature of his personality and how intimately it was connected to his brain child *Die Naturwissenschaften.* This was certainly the basis of his amazing success. For this reason the first section of this chapter assembles various fragments about a core figure of the Weimar scientific community that has hitherto been neglected almost entirely by historiography. My main objective is to show how deeply some younger scientists felt about Berliner and to what extent they admired his embracing equally science, technology, and the arts. It appears that Berliner, both a "man of culture" (Kulturmensch) and a "technical physicist" – using characterizations by von Laue and Ewald–, made many of them not feel estranged from arts and culture by their pursuing science and technology. One might view the unity in Berliner's outlook as a continuation of the Humboldtian bridge between natural science and aesthetic sentiment which Exner had cherished so highly, of Helmholtz's unified approach to the sciences and scientific aspects of the arts, and perhaps, above all, of the universalist Goethe, the poet, naturalist and manager. As we shall see, Helmholtz and Goethe figured prominently in Berliner's thinking and his journal, and Metze's paper on Humboldt (Section 4.2) did not slip into the first volume by accident. But there existed a substantial difference between Berliner's appreciation of modern art and Exner's feeling of a decline of art and culture. Moreover, the former director of the A.E.G. filament lamp factory and director of a journal dedicated to the progress of science and technology was far from being a Mandarin albeit his severe criticism of the contemporary state of the culture of writing. If one compares Berliner's and Exner's introductory textbooks, however, the latter clearly takes the more modern approach. What remains from Ewald's testimony, though, is that there existed an important cultural influence on the younger physicists – being of the same age as the founders of the Vienna Circle – other than the well-known anti-scientific trends of Weimar culture.

Deeper scrutiny would be required here, but it seems to me justified to assume that those mainly young scientists gathering around Berliner and his journal felt not so fundamentally estranged by an allegedly crude technological and positivist rationality that they would readily succumb the anti-scientific Weimar milieu by the simple adaptive mechanism claimed by Forman. No wonder that Berliner's *Naturwissenschaften* firmly took a stand against Spengler (Section 5.3.). Berliner's perspective about the relation of science and culture – which was, of course, not shared by a number of authors of the journal – was embedded into a clear commitment for scientific modernism combined with a rejection of those opposing tendencies that emerged after the war, such as irrationalism or vitalism (with qualifications), and above all the *Deutsche Physik.* For this reason I shall claim with respect to the notion of cultural milieu the Forman thesis is based upon, that *Die Naturwissenschaften* can be considered as a modernist and science-oriented submilieu that provided scientists with a cultural identity more specific than just being *Bildungsbürger*, such that they did not face the general milieu solely and directly. Considering the journal as the expression of a submilieu derives support from the strong emotional adherence to Berliner and his journal characterizing most recollections and obituaries.

Of course, dichotomies "modern" versus "anti-modern" are notoriously problematic. And even if the case could be made successfully in one science, matters probably would differ significantly in another science; for instance, while physicists would have considered teleology or *Zweckmäßigkeit* as anti-modern *tout court*, many biologists were searching for modern and empirically meaningful definitions of them. But we do not need a fixed set of methodological credentials and a submilieu thesis of full generality to assess the consequences of the present chapter for the Forman thesis and its wake. It suffices to repeat a result broadly discussed in Chapter 1. The history of physics between 1906 and 1945 was simply characterized by the fact that there existed a clear-cut division line between modernism and anti-modernism that exhibited the familiar scheme: the praise of progress versus the accusation of degeneration. The progressive sub-milieu – which embraced people conservative in other respects, among them Planck – constituted itself in the struggles about relativity theory which began right after the war. Already the first volume of *Die Naturwissenschaften* contained a strong criticism against relativity theory by Ernst Gehrcke (1913) which was rebutted by Max Born (1913). The encounter was yet far from the polemics that took place after the war and in which Gehrcke would become one of the leading figures on Lenard's side, while *Die Naturwissenschaften* became an important stronghold in the "defense belt" (Cf. Hentschel 1990) around Einstein. It was in particular von Laue, Thirring, Schlick, and Born who participated in Berliner's program to explain the new theory to the general audience of German scientists. As we see in a letter of Berliner to Schlick (Section 5.4.) this educational objective was more important to the editor of *Die Naturwissenschaften* than polemics as such, although he constantly published rebuttals of the works of Stark and Lenard. (See Section 2.1.2.3.)

After the Nazis had seized power, Berliner himself – as a Jew and a close friend of Einstein – quickly became the object of defame from the side of the *Deutsche Physik*. Although the journal was never directly political, and although one finds several authors in Berliner's *Naturwissenschaften* who after 1933 became active national socialists, there are some papers which could be read as an expression in favor of the Weimar republic (Section 5.2.). At any rate, there were areas such as relativity theory where being committed to scientific rationality and modernism was tantamount to a political statement. Thus the dichotomy between those who – at least after some time – accepted relativity theory and those who combated it, and the criteria according to which such a decision was made, provides the coordinate system for all those debates about causality for which *Die Naturwissenschaften* provided a forum.

Apart from specific differences between German and British science, it was this strong and persistent philosophical element by which *Die Naturwissenschaften* differed from the model of *Nature*. Already the sheer numbers are impressive. (See Section 5.4.) The general overview of Ernst Lamla – then editor of *Die Naturwissenschaften* – commemorating the 50th volume rightly observes that an "exceptionally large number of papers [from 1924 until 1944] is dedicated to causality." (Lamla, 1963, p. 10)

However, *Die Naturwissenschaften* were neither striving for a maximally broad coverage of philosophical topics relevant to scientists nor following the trend of the profession. The selection of topics and authors reveals a clear stance, and Berliner's guidance was perhaps stronger than in any other field. His correspondence with Schlick provides an example of how he approached authors and developed a topic he wanted to be covered in his journal. With a few exceptions, there were basically two classes of philosophical contributions up to 1930. On the one hand, a certain set of eminent philosophers, among them Kant and Schopenhauer, were covered in survey articles which are of a rather discursive kind; Berliner evidently held that they represented important intellectual background for his readers. Interestingly, most of the articles were authored by the philosopher and writer Moritz Kronenberg while university philosophers without any scientific background rarely appeared as contributors. On the other hand, *Die Natuwissenschaften* were the main journal for the scientists within the emerging movement of Logical Empiricism until this movement finally detached itself from the classical pattern of the scientist-philosopher and established themselves as a new scientific discipline, scientific philosophy, which found its expression in the manifesto of 1929 and the foundation of *Erkenntnis* in 1930. This will be the core of Section 5.4.

In the final section, I shall briefly investigate a debate about causality that took place in *Die Naturwissenschaften* during the early 1920s between Walter Nernst, Walter Schottky, and Joseph Petzoldt. It shows that the philosophical terminology was considerably in flux. We find a Nernst strongly indebted to his teacher Boltzmann yet without having taken the late Boltzmann's Machian tack. Schottky redefined the concept of causality in such a way as to meaningfully speak about stochastic causality and simultaneously introduces non-local interactions, while Petzoldt saw no need of action or sign of crisis because the Machian notion of causality was wide enough to accommodate all problems of quantum theory. This dialogue shows that the front line investigated in the present book extended beyond the narrower circle of personalities starring in the other chapters.

5.1. Arnold Berliner and the Orchestration of Natural Science

5.1.1 The Personality

Obituaries and commemorative articles emphasize the eminent role of *Die Naturwissenschaften* for Weimar science and the singular nature of Berliner's personality. There is a mournful tone in all of them. Compare von Laue's report of Berliner's tragic end written down in 1942 but published only in 1946.

> Berliner had wished to remain editor of *Die Naturwissenschaften* for twenty-five years, that is, until 1938. Things turned out differently. In the summer of 1935 the publisher felt urged to dismiss him over night. Berliner finally got over the suddenness of the dismissal, and until the end he felt love and thankfulness for the house Springer. But it hit the roots of his nature [Wurzel seines Wesens] that his work was at all ended by force. Nevertheless he lived for another seven years, more and more oppressed by the growing persecution of Jews, more and more restricted in his activities. Finally, he retired like an hermit to his beautiful domicile in the Kielgan street which he only left when it was

absolutely unavoidable to visit a doctor or an authority. Two journeys to the USA, in autumn 1935 and in summer 1937, were gleams of hope within this misery. Unfortunately they did not lead to a position which had enabled him to subsidize himself there; and he proudly declined the greathearted offer of good friends to allow him a sort of pension over there. What still kept him alive then was partly the hospitality with which … he could entertain the friends who remained faithful to him until the end; *many* visitors came to the Kielgan street. Moreover, his mind remained active. Again and again he read in the books which were his whole life, wrote amendments and corrections to his textbook although it could not be published any longer; for he still hoped that matters would turn for better. He did not live to see it. When they even wanted to turn him out of his flat, his last refuge, he carried out a decision taken for this case long ago and took his life [on 23 March, 1942]. (Laue, 1946, p. 258)

After the last phase of the holocaust had started, even Berliner's influential friends were unable to protect him any longer. Through them the news of his death reached England, and in September *Nature* published two obituaries by his emigrated old friends Peter Paul Ewald and Max Born.[104] They were well aware of the circumstances, as Ewald ended: "Now his friends deplore in his tragic escape from life the loss of one who represented much of the best cultural traditions of a bygone Germany, and of a warm-hearted and helpful friend." (Ewald, 1942, p. 284) Ewald emphasized another important aspect.

Berliner addressed himself mainly to the then young generation of men of science. Much of the success of the journal was due to Berliner's vivid personality, his close contact with the majority of young physicists and mathematicians and his initiative in formulating the subject of articles he wanted written for his journal. Thus *Die Naturwissenschaften* became a mirror reflecting the development of science during 1913-30. (Ewald, 1942, p. 284)

Affection of this generation for Berliner did not decrease with temporal distance. Wilhelm Westphal's commemoration of the tenth anniversary of Berliner's death joins in with the above-quoted obituaries. Evidently, for a group of young Berlin physicists, Berliner had become an intellectual father figure not unlike what Exner had been for his circle. Just compare the following passage with Benndorf's recollections about the afternoon teas at Vienna (Sect. 4.3).

[The volumes of *Die Naturwissenschaften*] reflect the entire dramatic development of the natural sciences in the first half of our century, and there exists hardly an eminent natural scientist [*Naturforscher*] who did not appear there at least once as a contributor. … Moreover, may we not forget that Berliner has written the first *Textbook of Physics* [1903] arranged according to the modern point of view. After having served for more than two decades as a most valuable source of knowledge for the younger generation, in the year 1933 it went up in flames on the disgraceful stake in front of the University of Berlin. And may we not forget the *Concise Dictionary of Physics* [1924] co-edited with his close friend Karl Scheel. … May we also remember with gratitude his silent activities as a counselor of the Springer publishing house. He has thus done more good for German science and technology that became known to the general public.

But may we above all not forget the man. We, the Berlin physicists of the years circa 1910 to 1940 have the right to say: He was one of us. [*Er war unser*.] For us those decades are altogether unthinkable without Arnold Berliner. He represented a spiritual center around which we gathered again and again, in particular during the after hours [*Nachsitzungen*] of the Physical Society and the venerable Colloquium – and on most of these occasions until the last tram. We all loved him, this man of an exceptionally universal education, this fine mind open to all beauty, this rough diamond with a

[104] Communication across the war front was difficult and could be effected, if at all, only via neutral countries. The same number of *Nature* contains an obituary of the Welsh physicist A.L. Selby who had died on July 22, 1942. So it took presumably about three months until the news of Berliner's suicide had reached his friends.

grim humor but with a warm heart, he whom nothing could infuriate more than when he believed that someone had been wronged.[105] Whoever of us one day should write his memoirs, Arnold Berliner for sure will not be missing in them. (Westphal, 1952, p. 121)

Born depicted Berliner's appearance and character in quite similar terms.

There was no Naturforscher-Versammlung and no meeting of the Physikalische Gesellschaft where his small and powerful figure with the characteristic beard and big spectacles did not appear, no scientific conference where his wise counsel was not welcomed. He never claimed to be a man of science, but only a "poor technician". But he insisted that every article in *Die Naturwissenschaften* should be written in such a way that his "simple mind" could understand it. How few of the contributions proved up to the high standard which he set, and how lively was the ensuing correspondence. He had a collection of the most remarkable extracts from these letters, as material on the "psychology of the scientist", whom he liked to describe as "mimosenhaftes Stachelschwein" (a hybrid of mimosa and porcupine). Perhaps he did not realize how well this description fitted his own character, which was the strangest mixture of infinite kindness, generosity, greatness of outlook and personal touchiness.

He was a technician by profession, an amateur scientist; but his real life was in literature, art, and music. He read abundantly and remembered everything. He knew the great galleries of Italy and Germany, and filled his home with good modern pictures. But his greatest pleasures were his annual visits to the Bach, Beethoven, Brahms festivals and to Bayreuth. Of his numerous friendships with men of importance he valued none higher than that with Gustav Mahler, the composer. (Born, 1942, p. 285)

Berliner was born in Breslau (today Wroclaw) in Silesia on 26 December, 1862. Von Laue relates that Berliner's father unfortunately took his son from the humanistic gymnasium to the Realschule, "him who more than hardly anyone was destined for a humanist!" (Laue, 1946, p. 257) Similarly as it would be for Born many years later (Cf. Born, 1975, p. 122, and Holl, 1996), the house of his cousin Albert and Toni Neißer opened to the young Berliner the world of science, art and music. Since his *Realschul* diploma did not allow him to study medicine, Berliner became a physicist. In the rapidly growing A.E.G. company chaired by Emil Rathenau Berliner made a brilliant career. After some time in Hamburg and the United States he became the director of the Berlin filament lamp factory. After 25 years he left in 1912 because of irreconcilable differences with Rathenau. According to Ewald, Berliner's technical achievements were considerable.

Berliner belonged to the first generation of 'technical physicists' and worked in close connexion with Emil Rathenau as head of the physics laboratories of the A.E.G. during the period of rapid expansion of this firm. The development of the incandescent lamp carbon lamp and of X-ray bulbs owes much to him – he introduced the first 'getter', phosphorus, in the manufacture of lamps, and the large scars on his arms bore testimony of the early stages of experimenting with X-rays when their dangers were not yet realized. Among other technical problems advanced by Berliner was that of the phonograph. (Ewald, 1942, p. 284)

It was presumably the house of his cousin Neißer in which Berliner first met Gustav Mahler. Their friendship developed during the early 1890s when both worked in Hamburg. Berliner supported some of Mahler's projects, and Mahler sometimes stayed

[105] Let me give the German original of this passage: "Wir haben ihn alle sehr geliebt, diesen ganz ungewöhnlich allgemeingebildeten, feingeistigen und allem Schönen offenen Menschen mit der rauhen Schale und einem oft grimmigen Humor, aber mit dem warmen Herzen, ihn, den nichts mehr empören konnte, als wenn er glaubte, es geschehe jemandem Unrecht."

in his friend's Berlin domicile. Mahler's (1982 & 1983) letters testify their intense aesthetic and philosophical discussions,[106] such that Mahler's biographer, the sociologist Kurt Blaukopf, even considers "Berliner as a key figure to understand the thinker Gustav Mahler." (Blaukopf, 1980, p. 222) No wonder, after all, that Berliner chose a distinction from music to describe his own role with respect to pure science.

For all his scientific talent and for all his love of knowledge, Berliner was not a scientist. He once described his position towards the latter by comparing it with the position of a conductor towards the composer. Despite his artistic sense which by no means can be appraised high enough he was, to be sure, even less of a creative artist. He belonged to a type of man that is, to the disadvantage of mankind, in danger of extinction: He was purely and simply a man of culture [Kulturmensch] in the sense that he was striving for an overview of as large as possible a domain of our culture with the goal to reach certainty about its authenticity, its concordance with ethical demands. (Laue, 1946, p. 258)

So far I have provided a rather modernist account of the *Kulturmensch* Berliner. But there was an eminent conflicting voice.

On the occasion of Berliner's seventieth birthday on 26 December, 1932, his *Naturwissenschaften* published a *Festschrift*. The double issue contains no less than 35 contributions in which prominent scientists explained a current topic of their own discipline on two or three pages. They are preceded by three papers which deserve special interest here. In a sort of preface, Einstein restated the importance of Berliner's goals as outlined in 1913, in particular, the need to overcome the growing specialization. After letting out Berliner's characterization of a scientific author as a hybrid of mimosa and porcupine, Einstein asserted that "Berliner's achievements were only possible because in him the desire for a clear overview over a maximally broad realm of research is exceptionally vivid," (Einstein, 1932, p. 913) an attitude which also made his textbook (Berliner, 1903, 1928) so valuable for students.

Berliner's struggle for clarity and the general idea has enormously contributed to problems, methods, and results of science coming to life in many minds. The scientific life of our times cannot be imagined without his journal. To make knowledge come to life and keep it alive is equally important as to solve single problems. We all know what we owe to Arnold Berliner. (both Einstein, 1932, p. 913)

The second contribution was authored by Wolfgang Windelband, head of the department of personnel of the Prussian ministry of education (1926–1933). He was the son of Wilhelm Windelband, the founder of the Southwest-German school of neo-Kantianism and inventor of the distinction between nomothetic and ideographic sciences, a philosophical distinction that was to underpin the rigid methodological separation between "Naturwissenschaften" and "Geisteswissenschaften." And Wolfgang Windelband clearly followed his father's lead by emphasizing that it is only Berliner's great interest in history, in particular, the history of the church, which gives a historian the right to "violate the basic principle of Berliner's editorial activity, to let only real specialists in the wide realm of the natural sciences get a word in his journal." (Windelband, 1932, p. 914) Windelband characterized Berliner as an

[106] The starving addressee of the letter no. 414 was, to my mind, certainly not Berliner. Herta Blaukopf has told me that Alma Mahler was not all too careful when assembling the edition of the letters.

adherent to the classical Prussian virtues and, interestingly, emphasized the feelings of decline so characteristic for the German mandarin.

> Among all those features of our times which repel him and direct his wistful look back to a more beautiful past, the decrease of general education to him is an instance of particular anguish. With amazement and sorrow he follows the symptoms of this course of disease, and harsh without reserve are his judgments about this result of our present system of education. …
>
> It is also his sense for the arts which again and again makes him furious about … the poor stylistic quality of scientific works. (Ibid., p. 914)

In the end, the reader witnesses an adoption of the *Realschüler*, physicist, and factory manager by a representative of the German "Geisteswissenschaft."

> When we take the image of his personality and its relation to the spiritual content of life as a whole, then his life rounds into a superb and complete humanity. In this sense he, who often jokingly described himself as a "Klippschüler" [i.e., someone going to a second-rate school], succeeded in becoming a humanist of the best kind. (Ibid., p. 915)

How can we interpret Windelband's characterization of Berliner? Certainly, Berliner was a humanist and a *Bildungsbürger* "of the best kind", but the motive of cultural decline seems a surprising epithet for a technician and manager dedicated to scientific progress and modern art. We would need more historical material here, but it seems to me that there exists no split in Berliner's cultural attitude of the kind we encountered in Exner (Sect. 4.6.5). After all, educational reforms were on everyone's agenda. The constant specialization in the sciences made the general overview an almost impossible task. With a look back to the days of Helmholtz, Berliner founded the *Naturwissenschaften;* with reference to the French encyclopedists, Otto Neurath in the 1930s would embark into a similar mammoth enterprise. Hence, we do not get far with simple dichotomies here.

In actual fact, Berliner did not approach the question of style by mournful contemplation. Kronenberg discussed concept formation of philosophical concepts (1917a) and the issue of foreign terms in science (1918b). After 1934 Edmund O. von Lipmann published each year a list of the most blatant stylistic lapses from the chemical journals. In May 1935, Hendrik A. Kramers discussed scientists' style of writing. He diagnoses a philosophical attitude in present physics that is akin to the one prevailing in the age of romanticism. But due to our present lack of knowledge, there is nothing bad in this. Bohr's "incessant struggle for expression" (Kramers, 1935, p. 301), the struggle for the right form of quantum mechanical results was also a struggle for clarity.

Windelband's adoption of the "Klippschüler" Berliner, to my mind, became only necessary by his presupposing a rigid distinction between science and the arts that in effect was at odds with Berliner personality. Windelband's rhetorical *captatio* by modesty missed the point in another important respect. The dividing line between "Naturwissenschaften" and "Geisteswissenschaften" did not coincide with the border set by Berliner for *Die Naturwissenschaften.* To the contrary, the border line went right through academic philosophy. Apart from philosophical papers by scientists, Berliner accepted many contributions from guilded philosophers who were educated in or felt close to the natural sciences and their method – be they neo-Kantians or Logical Empiricists –, but he strictly excluded those philosophies which insisted on the unbridgeable gap between nomothetic and ideographic disciplines (Cf. Section 5.4). Already in the first volume “The intrusion of scientific methods into the *Geisteswissenschaften*“ was discussed in a contribution by M. Brahn who stressed the constantly increasing value of scientific methods, e.g., in historical investigations. “Neither the historian nor the pedagogue can accomplish their work with [scientific methods only]; to this end they need a broadening of their considerations which only imagination can create, to this end they also need an instinctive attitude that does not originate from the sciences.” (Brahn, 1913, p. 69)

The eminent role of the discussions on causality in *Die Naturwissenschaften* becomes clear from the third contribution to the *Festschrift* authored by Max von Laue. Since von Laue thus entered into a discussion with Richard von Mises I postpone a closer discussion of his paper to Section 8.6.

5.1.2 Berliner’s Textbook

Berliner’s *Textbook of Physics in Elementary Presentation* appeared in five editions between 1903 and 1934. To Laue’s mind it was

> probably the only book of this kind whose author never was active in teaching. … Until the end of his life [Berliner] constantly completed and amended it to keep it up to date. It is profound as the man himself who never wrote anything down until he had completely understood it and thought it through in various directions. When in his old years some new findings proved to difficult for him, he had the respective section written by a friend. (Laue, 1946, p. 257)

Among these friends were such eminent scientists as Walter Nernst, Fritz Haber, Walter Gerlach, and Otto Stern; cf. the Preface to the fourth edition (1928). The book sold well and found unanimous praise. Nernst, who gave the introductory course in physics, recommended the book to his students “as particularly profound, versatile and well thought out.” (Autrum, 1988, p. 2). Westphal, who himself authored an introductory textbook, called it the first modern textbook. According to Ewald, Berliner’s book was “conspicuous for stressing the application of physical knowledge to technical problems, many years in advance of a recognized ‘technical physics’.” (Ewald, 1042, p. 284) And indeed Berliner’s book contained many remarks about technical instruments and applications.

In the Preface to the first edition, Berliner explained that his “book is elementary in particular with respect to the form of the presentation, i.e., in the *detailedness of the description* which everywhere intends as much as possible to clearly explicate the particular features and to facilitate the reader’s own work.” (Berliner, 1903, p. iii) It is also elementary insofar as it presupposes only a basic mathematical knowledge and strives for clarity and distinctness of the subject matter, that is, for a simple arrangement by which the student does not face a new topic unprepared. Berliner’s preface is followed by a short foreword of the physician L. Hermann who recommends the book to the students of medicine. Had we not encountered a very similar structure in the first number of *Die Naturwissenschaften* this could have been seen merely as a maneuver to increase the readership. Most interestingly, in all his struggle for clarity Berliner never talked about *Anschaulichkeit.*

Berliner's introduction leaves no doubt how strongly the book was indebted to the heritage of Helmholtz. It is the task of physical science "to seek the laws by which the single processes in nature can be reduced to general rules and be determined from the rules." (Ibid., p. 1) And the author also assents to Helmholtz that it is the primary aim to reduce all physical phenomena to phenomena of motion. Admittedly, many areas of physics are far away from that goal, but even in these cases "we do not have any reason to assume that this reduction is *impossible* but only that it is not possible *at the present state of science.*" (Ibid., p. 2) Still in the fourth edition of 1928 this ideal is maintained in a mitigated form. Compared to 1903 the introduction is shorter and contains a footnote stating that the development of the last two decades has led away from the mechanical world view "because the electrodynamic processes in free ether cannot be deduced from a coherent mechanical hypothesis. But the student finds in the basic idea [of the mechanical world view] such a clear guide that it would be unsuitable to introduce him into physics on a different route." (Berliner, 1928, p. 1)

Let me add some words of comparison between the introductory textbooks of Berliner and Exner. Both were written in an equally broad and comprehensible style and presupposed only elementary mathematics. While Exner arranged his presentation into the three chapters space and time, matter, and ether, Berliner still followed the historical classification of subdisciplines, such as mechanics of points and liquids, acoustics, theory of heat, reorganizing though the sequence of topics. This has the consequence that while Berliner set out with the mechanics of mass points as the most easily comprehensible topic, Exner's readers find themselves quickly driven into geometry and special relativity. Both authors emphasized topics close to their hearts. While Exner broadly covered the theory of colors and atmospheric electricity, "Berliner's predilection for everything connected to optical imaging" (Laue, 1946, p. 257) found its expression in a chapter on optics longer than all others and sophisticated folded figures which were to avoid the misunderstanding of perspective drawings. The main difference between both books perhaps lies in their philosophical outlook. While Berliner was, at least for pedagogical reasons, indebted to the classical tradition of Helmholtz, Exner added an entire philosophical chapter about natural laws which adopted the statistical point of view throughout. Thus Exner's fourth chapter pointed to the future of physics much more than the physics of the textbook. Berliner's equally strong philosophical interests remained under the surface of his book, such that its most modern aspect – apart from the fact that the book was constantly updated – might be seen in the intimate connection of science and its application.

5.1.3 Berliner at Springer

> Indeed the publisher could not find a better editor than this one who apart from his love for and understanding of the 'exact sciences' had a heart for all biological matters due to his acquaintance with [the physician Albert] Neisser, who owing to his superb knowledge of human nature was capable to get near to the leading representatives of all these disciplines, and who made close friends with quite a few of them. (Laue, 1946, p. 258)

Von Laue's characterization extended beyond the narrower scope of *Die Naturwissenschaften.* "Berliner's importance for the connection between the various scientific disciplines and the Springer-Verlag can hardly be estimated high enough."

(Holl, 1996, p. 48) Not only did he steer the flagship journal, he was also an irreplaceable counselor to Ferdinand Springer. In his history of the Springer publishing house, Heinz Sarkowski writes.

> Springer's internal advisers were scientists, with some of them being full-time journal editors, such as Victor Salle (from 1910) for internal medicine; Arnold Berliner (from 1913) for the natural sciences, especially physics and mathematics; Arthur Hübner (from 1928) for surgery; and Ernst Urban for pharmacy (from 1920). They received an monthly retainer and expenses, but were not employees in a legal sense. In addition there were numerous advisers who had close relations with Springer as author or editor: Richard Courant for mathematics, Max Born for physics (later Karl Scheel also), Richard Goldschmidt for biology (later Fritz von Wettstein), Walter Kaskel (until 1926) followed by Hans Peters for political science and economics, Karl Wilmanns for psychiatry from 1914. (Sarkowski, 1996, p. 316)

Apart from these activities Berliner co-edited with his friend Karl Scheel a *Concise Dictionary of Physics* (Berliner/Scheel, 1924), and he launched a series *Scientific Monographies and Textbooks* (Naturwissenschaftliche Monographien und Lehrbücher) that was joined to *Die Naturwissenschaften.* Although there are only nine volumes,[107] the series expressed the combination of applications and foundations of physics advocated by Berliner. The first book in the series was Schlick's *General Theory of Knowledge* (1918), the second Moritz von Rohr's *The binocular instruments* (1920), the third Born's popular book on *Einstein's theory of relativity and its physical foundations* (1920)*,* the second edition of which had the subtitle "in elementary presentation", and the sixth Peter Paul Ewald's *Crystals and X-Rays* (1923). Further volumes covered stellar clusters, geophysics, and telescopes.

After Hitler had come to power, his followers among the scientists were installed into powerful positions; Stark became the director of the Physikalisch-Technische Reichsanstalt and Lenard had the last word in all professorial appointments in physics. The autonomous organizations, such as the German Physical Society, to a certain extent could resist an immediate *Gleichschaltung*. The Springers came under pressure because the founder Julius Springer had been a baptized Jew. The first campaign against the "Jewish publishing house" was launched already in 1933 by the Nazi organization of doctors and it reached its first peak in 1935 when Julius Springer jr. was forced to retire. Ferdinand Springer jr. managed to steer the house until 1942 largely by utilizing the thicket of diverging interests and conflicting administrative competencies for scientific literature. (Cf. Sarkowski, 1996, pp. 342-375)

Quickly Berliner became the target of a campaign. The fifth and last edition of his *Lehrbuch* came out in 1934, although it had already been burned by the Nazis on 10 May, 1933.[108] Moreover, Berliner lost many of his authors either by emigration or because they suddenly felt *Die Naturwissenschaften* to be unsuitable. Thanking Sommerfeld for the submission of a paper in 1934, Berliner wrote "Each paper which I now receive is truly a great help in misery." And he added the copy of a letter by Hugo Dingler who rejected a review of a book of his "as beneath his dignity for obvious reasons"[109]. Dingler felt triumphant because over the years the reviews of his books

[107] See (Sarkowski, 1992, p. 172).
[108] I have no reason to doubt that Westphal's recollection is trustworthy although Berliner's book was not on the list distributed to the Nazi students by Göbbels' ministry.
[109] Sommerfeld-Nachlaß, Deutsches Museum, Munich.

had become more and more negative as he had become a harsh opponent of relativity theory. For instance, in 1933 Dingler's *History of Natural Philosophy* (1932) was reviewed by the Logical Empiricist Edgar Zilsel (1933) who strongly criticized how the author had treated the historical sources and forced them into his epistemological outlook.

On 14 January, 1935, Professor Ubbelohde of the Berlin Technical University accused Berliner of "'extreme propaganda activities on behalf of the results of Jewish scholars' and pointed to Einstein's birthday essay in *Die Naturwissenschaften,* 'which can be considered as characteristic of the principle of mutual praise among Jewish scholars. – It seems particularly disquieting that one can still read on the title [page] of *Naturwissenschaften* that this journal is the official organ of both the *Gesellschaft deutscher Naturforscher und Ärzte* ... and of the Kaiser-Wilhelm-Gesellschaft.'" (Sarkowski, 1996, p. 333f.) Rudolf Mentzel, personal representative of the minister of science, also stressed the second point in a memorandum on 9 April, 1935.

> I am of the opinion that it is simply unbearable that the official organ of the KWG is directed by a Jew. I am of the opinion that we as the supervisory ministry must put forward the demand, either: Berliner disappears from the editorship of *Naturwissenschaften* or the KWG is forbidden to call the journal the official [organ] of the KWG. I am in favor of immediate action! (from ibid., p. 334)

Although Berliner had influential advocates, among them the president of KWG Planck, the matter could only be delayed, and Berliner had to resign as editor on 13 August, 1935.

The central role of Berliner who throughout the years had run the journal together with a single co-editor from the biological sciences, is best seen by the impressive board that took over in 1935. Initially, the editor was Hans Matthée together with P. Debye, H. von Ficker, O. Hahn, M. Hartmann, F. Kögl, M. von Laue, F. Sauerbruch, H. Spemann, H. Stille, F. von Wettstein. In the next year Fritz Süffert, a former assistant to Spemann, took over, and the number of co-editors increased to twelve in 1936 and to fourteen in the year 1937. So one can conclude that Arnold Berliner, not among the scientific composers himself, was the conductor of a large and world-renowned orchestra of scientific voices. He thus must be considered on a par with the other Berlin conductors, some of which composers themselves, including Max Planck, Friedrich Schmidt-Ott, or Walter Nernst.

5.2 Relativity and Politics

Library scientists hold that "scientific journals are comparable to standards round which the comrades-in-arms are gathering." (Holl, 1996, p. 132) While in the 1920s progressive physicists tended to publish in the *Zeitschrift für Physik* edited by Scheel, the conservatives preferred Wien's *Annalen der Physik* or even the *Jahrbuch der Radioaktivität und Elektronik* founded by Stark. Because of their ample scope, *Die Naturwissenschaften* do not really fit into this classification scheme – which, as the case of Schrödinger will show, has its limits in other respects. Yet there is at least one fight in which Berliner's journal became the most important standard of the modernists.

After the war the struggle about relativity theory became extremely polemical and poisoned by antisemitism. Rather than taking part in the polemics, Berliner set himself the goal to explain the theory and its various experimental and philosophical consequences to the readers from all scientific disciplines. A major figure in this program of education and dissemination was Planck's former student turned philosopher Moritz Schlick, but also von Laue, Born, Thirring, Riebesell and Einstein (1918) himself contributed to it.

Upon recommendation of the philosophers Erich Becher and Benno Erdmann who had already published in *Die Naturwissenschaften,* Berliner approached Schlick in 1916 to write a physically and philosophically comprehensible paper on the theory of relativity. The result (Schlick, 1917) impressed Berliner, and already in the following year he not only published Schlick' *Erkenntnislehre* (1918) but also asked for a contribution on how the results of modern physics had influenced the concept of substance.[110] Schlick apparently agreed, and Berliner kept pressing him softly year after year until Schlick remarked that all he had to say found entrance in his textbook chapter "Naturphilosophie" (Schlick, 1925). In the meantime Schlick had published a long paper on the principle of causality (Schlick, 1920) that was strongly motivated by relativity theory while atomic physics only played a marginal role (See Section 7.1.). On 31 May, 1920, Berliner wrote to Schlick that he had read the galleys of the paper several times and received valuable stimulations.

> One only advances toward a true understanding of Einstein's thoughts step by step, but I believe now that I have again got on some steps further …
>
> It am not so much astonished that the opposition against the new doctrine [Lehre] is large. For instance, there are strictly speaking no resources from which the philosophers could have informed themselves about the philosophical foundations of the theory of relativity. The physicists should have done more in this respect because not even the *allegedly* "popular" [allgemein verständlich] book of Einstein is *truly* popular, even less the work of Freundlich. One has, to my mind, not made sufficient concessions to those philosophers who are not at the same time physicists, and yet there are only very few of these. It is surely a great pity that in Halle there was nobody to explain to the guild of philosophers that in actual fact they probably have not yet fully understood the physical foundations. Apart from those philosophers who do not go along because the do not want to, there were certainly also those who would want to go along if the could obtain a true insight into the issues. In a few months, however, the philosophers will get into their hands such a book authored by Max Born which only presupposes the mathematics of the gymnasium and expounds in a clear and pleasantly readable way the foundations of mechanics and the special and general theory of relativity. After that, nobody who is truly concerned with penetrating into Einstein's world of ideas will have the excuse that there is no literary aid for it.

The letter continues with some remarks on similar ideas in the works of Helmholtz and Schopenhauer.

Two weeks before, Berliner had explicitly asked Schlick "in the name of a number of followers of Einstein"[111] to defend relativity against the criticism of the Brentanist Oskar Kraus and Hans Vaihinger's fictionalists. In his comprehensive study of the interpretations and misinterpretations of relativity theory, Klaus Hentschel (1990, Sect. 3.4.2) interprets the rather intricate events surrounding the Halle meeting, in particular the debate to find a suitable advocate for relativity theory, as a clear

[110] Letter of Berliner to Schlick, Rijksarchief Noord-Holland. Longer passages from the correspondence between Berliner and Schlick are given in (Stöltzner, 2000a).
[111] Berliner to Schlick, 17 May, 1920.

indication for the formation of a "defense belt" around Einstein. This belt was not homogeneous but consisted of various strategies that were guided by different interpretations of the theory, a fact by which "the discussions about relativity theory were encumbered with philosophical disputes." (Ibid., p. 164; boldface omitted) The background of the defenders ranged from Petzoldt's orthodox Machianism, Schlick's moving away from neo-Kantianism to conventionalism and empiricism, to those, including Planck and von Laue who did not believe that relativity required a departure from Kantian philosophy at all. Part of this genuinely philosophical debate was conducted in *Die Naturwissenschaften.*

Long after the struggles about relativity theory, Berliner approached another politically controversial topic that concerned the appropriate assessment of the history of physics. His letter to Schlick of 21 February, 1927 shows that his editorial strategy had not changed.

> Almost six years have gone by since Dühring's death, and the enormous antipathy [against him] which has gradually developed in all his contemporaries, has prevented that someone worth speaking of has written about him at some point. Although Dühring has made it difficult for all his readers, who did not belong to his blind party-liners, to do justice to him to some degree, the fact remains that he was an extraordinary personality who beyond doubt was very stimulating and inspiring. Unfortunately, for instance, his Critical History of the Principles of Mechanics is completely unknown to the nowadays young generation, and this proves that exactly that which made Dühring famous, and rightly made him famous, has almost fallen into oblivion. Dühring has caused this himself by his excessiveness. But it would be desirable, for sure, to point to that out of Dühring's life work which merits being kept in mind, and for this reason I would very much like to publish a paper on Dühring when the occasion arises. With this I intend a paper exclusively about the thinker Dühring without any biographical particularities, which would only cause an unpleasant polemic of his fanatic adherents.

Dühring's *Critical History* anonymously submitted to the University of Göttingen won the renowned Beneke award; it was the first comprehensive philosophical analysis of the development of the principles of mechanics. Coming a decade before Mach's ([1883] 1988) much more systematic assessment, the first edition (1873) enjoyed a considerable respect and was positively mentioned by Mach and Neurath (1915). Yet Dühring rewrote the second edition into a veritable diatribe against the Berlin mathematicians which ultimately led to the withdrawal of his *venia legendi* by that university. In the Weimar days, Dühring was an leading figure in some political circles close to the social democrats. The planned paper never appeared.

Apart from a critical solidarity with the Weimar Republic, there was no general political thrust of Berliner's journal. Authors included scientists who later would become active national socialists, others who arranged themselves with it, later émigrés and – in the first years after 1933 – already emigrated scientists, and people with outspoken socialist views. The core of the spectrum was probably the set of political attitudes prevailing among the Berlin scientific elite, from Einstein to Planck. Here are some instructive examples.

Still during the war, Born (1918) published an obituary for his fallen student Herbert Herkner who, to his mind, had been the greatest talent Göttingen mathematicians had seen for a long time. Certainly this was a protest against the war. The number of 7 November, 1919, that is, a year after the fall of the monarchy, started with an article by Paul Jensen titled "Science and Democracy" which polemizes against the then popular idea that democracy contradicted human nature because extant inequalities justify unequal treatment. Instead, Jensen argued that democracy fitted within a scientific world view.

A treatment from a lofty standpoint teaches us … that, on the one hand, the overall evolution of the world, at least still for a long time, … runs in the direction of an increased *harmony*, or scientifically speaking, of an increased "*stationary state*" (dynamical equilibrium) and increased *variety*; namely, by a furtherance of the mutually stimulating and an equilibration of the mutually conflicting differences. On the other hand, a scientific analysis teaches us that in general this equilibration of differences does not happen by force but slowly and gradually, with relatively little destructions. For all happening is governed by *energy differences* and it consists in an equilibration of energy differences, both in organismic and in anorganic nature. (Jensen, 1919, p. 821f.)

After the necessary revolution, democratization has thus become an educational and ethical task. Jensen's views about society were in stark contrast with what Exner had outlined in his inaugural address (1909) and would reaffirm in his theory of culture (1923). While Exner held that the second law of thermodynamics, at least in the long run, blocked the planned equilibration of social differences – for this would amount to an improbable state –, Jensen adopted an equilibrium point of view that sounded a bit like belated energeticism. It is obvious that both conflicting conclusions crucially depended upon the definition of order or energy (Cf. Sect. 4.1). Politically, to be sure, Jensen's paper was an important signal.

Two weeks before, Berliner himself had taken up his pen, which he rarely did in his journal, and defended in a rather patriotic tone his friend Fritz Haber, whose name is not mentioned, against public allegations connected with gas warfare. "Not a single nation or even a single person, but the long duration of the war and the fact that trench warfare was elaborated to an perfection undreamt-of before has to be blamed for this development." (Berliner, 1919, p. 795) The philosophical justification of this defense was that science is entirely value-free. "[T]he task of the scientist must be entirely separated from the usage made of the results of his scientific research and from the question whether this usage conforms to the norms of international law." (Ibid., p. 794) When in 1925 Haber was accused by *The Times* of continuing war research in violation of the Versailles treaty, Berliner published an open letter of protest by Haber's deputy director H. Freundlich.[112]

Haber's fate was in a tragical way akin to Berliner's. Both were of Jewish origin and had significantly contributed to building up the Berlin scientific environment under the Emperor and the Weimar republic. On January 29, 1934, Haber who had rejected to remain director of his institute in virtue of his veteran status, died in exile of chronic heart disease. The issue of *Die Naturwissenschaften* of February 16, 1934, had an obituary for Fritz Haber on its front page. Von Laue praised character and scientific achievements of the deceased.

His institute, as long as it existed, represented a widely renowned place of broadly conceived scientific research. On May 2, 1933, Haber turned in his resignation. Themistocles has passed into history not as the expatriate at the court of the Persian king but as the victor of Salamis. Haber will pass into history as the genial discoverer of the process to synthesize nitrogen with hydrogen which is at the basis of the technical extraction of nitrogen from the atmosphere. (Laue, 1934, p. 97)

[112] *Die Naturwissenschaften* **13**, p. 10-11.

Some weeks later and against the protest of minister Rust, Planck organized a memorial service.[113]

Another sign of political dissent was Edgar Zilsel's obituary on the murdered Schlick in 1937. Schlick had been the main philosophical defender of relativity theory and some reports in the Vienna press treated Schlick erroneously as a Jew and held his philosophy virtually responsible for the tragic events.[114] This was certainly not compensated by the fact that the most disgusting article came form a Professor Dr. Austriacus and might have been dismissed as a move by the Austrofaschists. Moreover, the author of the obituary was Jewish and a declared Marxist who had written several reviews critical against those now powerful under the Nazi regime.

That the obituary could at all appear was almost certainly owed to the intervention of Schlick's old friend von Laue, who was a member of the editorial board, and the academic teacher of both, Planck. When on the occasion of his 80th birthday in 1938, Planck was asked to mention those among his students who remained closest to him, we find only two names: Von Laue who had become "both a famous physicist and a faithful friend." "And I would like to mention another name that stands on quite a different side: Moritz Schlick who after completing a solid physical dissertation changed to philosophy and was carried off by a tragic accident." (both Planck, 1938, p. 75) This was, as it were, an extremely guarded way of putting things at the end of a celebration that in itself represented a provocation to the Nazi authorities. Planck had directed the medal named after him into the hands of Louis de Broglie and as the honored had fallen ill, the French ambassador accepted the award in his place. Heilbron (2000, p. 183) is certainly right that mentioning Schlick's name was a political gesture, but their mutual esteem as scientist-philosophers much greater than their differences of opinion in the 1930s suggest.[115]

5.3 The Spengler Debate

Apart from Westphal's praise for Exner's objectivity in assessing the *Decline of the West* (See Section 4.4.), Spengler's opus received two decidedly negative treatments in *Die Naturwissenschaften.* The second of them, authored by the biologist Herrmann von Voß can still be subsumed under what Forman takes as the typical response of Weimar scientist to simply defend their disciplinary standards. (Cf. Sect. 1.1.2.2.) Similarly as Exner, von Voß held that "no doubt, quite a few unbiased observers will assent to [Spengler's thesis about the decline of Western culture], for the signs of degeneration, of a decrease of spiritual force are all too clear and numerous." But von Voß intended to "condemn once and for all the faked and sensational in the book and to open the eyes of wider circles to how the author handles the facts … [in particular that] he entirely disregards biology and only occasionally lapses into invectives against 'Darwinism' and the Darwinists." (both von Voß, 1921, p. 757)

In the year before, Paul Riebesell who in earlier volumes had already twice stood up for relativity theory and modern theoretical physics in general (Riebesell,

[113] See (Heilbron, 2000, p. 162ff.) for how these events were embedded into Planck's scientific policy as President of the KWG.
[114] Cf. the documentation in (Stadler, 2001, pp. 866-909).
[115] See, for instance, their correspondence in the Schlick-Nachlaß.

1916, 1918) attacked Spengler's thesis of the incommensurability between Greek and modern mathematics. After all, modern axiomatics was shaped after the model of Euclidean geometry. Most interestingly are Riebesell's remarks about causality.

> Science – not the philosophy of nature – will now as before stick to the principle of causality and will approach precisely Spengler's problem of the predetermination of history with its new methods. For, by means of statistical laws – which *Spengler* incidentally does not recognize as mathematical laws – one has already successfully analyzed those mass phenomena, which historical questions are all about, to such an extent that results for the whole ensemble can be derived without knowing the psychological and physiological laws of the single humans. (Riebesell, 1920, p. 508)

Evidently, Riebesell defended a wider concept of causality that incorporates statistical laws as genuine laws for mass phenomena and, accordingly, adopts the relative frequency interpretation. Contrary to Spengler's historical determinism he advocated the statistical investigation of social and historical phenomena. History is about mass phenomena, not about individual cultural organisms. While Exner's (1923) theory of culture had used the micro-macro distinction to provide a physical basis for a morphology of cultures – though in a non-cyclic and partly progressive sense –, Riebesell advocated the standpoint of modern empirical sociology. Thus he was much closer to the physicalist ideas advocated in the Vienna Circle, for instance, by Otto Neurath than to the physicalism prevailing within the Exner circle.

By its wider horizon Riebesell's argumentation escapes Forman's scheme. Neither can it be classified as an adaptation because the concept of causality is defended, nor did he, effectively and in rhetorical disguise, undermine the concept of causality because Riebesell expects that more phenomena can be explained by his wider notion of statistical causality.

Comparing Riebesell and von Voß, we encounter a certain difference between the single sciences. While the physicist was eager to have a clear-cut front line against Spengler, the biologist, understandably, was more open to morphology. This also corresponds to the rather wooly fringe in the contemporary biological debates about causality and teleology.

But the difference between both reactions also shows that the submilieu which I find expressed by *Die Naturwissenschaften* is not homogeneous with respect to the Spenglerian challenge. It is true, both reactions were not just about mandarin prerogatives, as Forman seems to suggest, but about the methodological standards of scientific inquiry. But this common philosophical response combined with (at least) two different orientations on the cultural level. There were the *Bildungsbürger* who felt attracted by Spengler's style and pessimism, among them Exner and perhaps also von Voß and Westphal. And there were those, including Riebesell, who rejected Spengler on much deeper grounds and who adhered to a modernist conception of science and culture. This second stance was commonly taken by Logical Empiricists. Forman is right that they were a fringe group within the general cultural context, but they were an important voice within the submilieu harboring the philosophical causality debates among German physicists.

To see how the Vienna Circle – by then still an inofficial group – formulated their alternative to Spengler, let us take a quick look at Otto Neurath's 1921 booklet *Anti-Spengler*. Neurath criticized Spengler's often circular arguments, that the selection of material was extremely biased, and that his theory of cultural cycles elevated possibilities of cultural development into fatal necessities. Neurath's principal criticism, however, targeted Spengler's universal determinism and false rationalism.

It is not the individual wrong results, the wrong facts, the wrong proofs, that make Spengler's book so dangerous, but above all his method of conducting proofs, and his reflections on proof as such. Against this one must defend oneself. Anyone who wants to shape a happier future with hope and striving should know that none of Spengler's 'proofs' is enough to prevent him; and whoever wishes to come to terms with the idea of 'decline' should know that he does so on the basis of a resolution, and not a proof. (Neurath, 1921, p. 142/161)

This does not mean that we are free to act however we please. Neurath's main thesis against Spengler is rather that the need for decisions is ineliminable from human practice, be it in science, culture or politics. Even the best scientific means can, in some cases, only lead to a set of equally rational alternatives.

The wish to found action on perfect insight means to nip it in the bud. Politics are action, always built on inadequate survey. But a world-view, too, is action; embracing the manifold universe is an anticipation of unpredictable efforts. In the end all our thinking depends on such inadequacies. We must advance even without certainty! The only question is whether we are aware of it or not.

Our pseudo-rationalists dare not to face that fact. … And so the pseudo-rationalists press reason until it shows only one. (Ibid., p. 140/159)

Although in a less expressive outfit and with some qualifications, the criticism of pseudo-rationalism remained a cornerstone in Neurath's mature thinking. It was strongly indebted to Mach's epistemological holism according to which each scientific result is embedded into a provisional world view. And the insight that not all actions can be determined by rational principles went well along with Vienna Indeterminism which, already on physical grounds, rejected Spengler's universal determinism as metaphysical.

5.4 Philosophy in the Naturwissenschaften

In this section I provide an overview of the philosophical discussions in *Die Naturwissenschaften* during Berliner's editorship. With a few notable exceptions they can be divided into three groups. First, until about 1920 there are several contributions by authors close to neo-Kantianism. Second, *Die Naturwissenschaften* quickly became an important forum of the German scientist-philosophers, that is, we find contributions of trained scientists which were clearly of a philosophical nature. Their number increased after the war when foundational problems in physics became more pressing than before. While one class of authors, among them Planck, von Laue, Schrödinger, von Kries, and von Mises, were renowned scientists who published also non-philosophical articles in *Die Naturwissenschaften*, for those scientists who had in effect become philosophers, among them Frank and Schlick, Berliner's journal became their main forum until the foundation of the *Erkenntnis* in 1930. This marked the constitution of scientific philosophy as a discipline of its own right. Third, as we saw in the case of the planned paper on Dühring, Berliner ran an education program in general philosophy for which the preferred author and reviewer was Moritz

Kronenberg. Contributions in this third class did not require a close connection to scientific methodology or basic problems of contemporary science.

Among the scientists writing on philosophical matters in the first volumes of *Die Naturwissenschaften* there seems to be a certain dominance of biologists who repeatedly addressed the dispute between developmental mechanics and vitalism, a debate in which initially the journal favored the former. In the first volume, for instance, Julius Schaxel held that Henri Bergson "cannot expect any applause among natural scientists" (Schaxel, 1913, p. 796) and in the third volume Schaxel (1915) and Albert Oppel (1915) contributed further critical assessments of Hans Driesch's vitalism, which were rounded up by the co-editor August Pütter's listing "The characteristics of life" (1915) without any ontologically independent qualities of living entities. After the war, so it seems, Driesch and Jacob von Uexküll enjoyed a better press.[116] It was not Berliner who had changed his mind; in 1926 he wrote to Schlick: "Much as I should like to, even now I cannot get accustomed to the much too general terminology of the biologists, and I have long given up the hope to grasp the doctrine propagated by Driesch."[117] Rather does it seem that *Die Naturwissenschaften* gave a fair coverage of the general trend of biology and its philosophical foundations.

The most prominent physicist-philosopher was Max Planck. Although typically his popular lectures appeared as separate booklets before going through various journals (Cf. Heilbron, 1977), still a third of those published between 1913 and 1931 which Planck later assembled into the fourth edition of his *Wege zur physikalischen Erkenntnis* (1944)[118] appeared in *Die Naturwissenschaften* (1919, 1925, 1926). Fritz Reiche (1914, 1915, 1921) reviewed a further third (Planck, 1913, 1914, 1920); the first review cited the criticism of Exner's indeterminism. Two of the missing three concerned explicitly philosophical topics: the freedom of will (1923a) and the reality of the external world (1930); the remaining one is Planck's second Leiden lecture (1929a). But among Planck's physical papers in *Die Naturwissenschaften* at least two (1923, 1927) significantly contributed to the debates on causality. In the year 1932 we find a brief summary (1932b) of Planck's Guthrie lecture on "The concept of causality in physics" that later appeared separately as "Causality in nature" (1932a). Both collections of Planck's popular writings were reviewed: G. Laski (1923) wrote on the *Physikalische Rundblicke* (1922) and Werner Heisenberg (1933) on the first edition of the *Wege zur Physikalischen Erkenntnis* (1944) because Schlick had declined owing to illness.[119]

Berliner published papers of the fathers of quantum mechanics even before 1926. But the new theory led to a substantial increase of the philosophical discussions about its proper interpretation. A bibliography on "Causality and Probability" published in the second volume of *Erkenntnis* (pp. 189-190) contains 23 papers out of 67 from *Die Naturwissenschaften.* Apart from those papers which have been and will be discussed in more detail in the present study, one finds (Bohr, 1928, 1929, 1930), (Born, 1929), (Heisenberg, 1929), (Jordan, 1927a, 1927b). Thus Göttingen-Copenhagen was well represented in the discourse among the scientist-philosophers.

[116] For a decidedly positive paper, see (Meyer, 1934). I am indebted to Veronika Hofer for having indicated to me the importance of the debate about developmental mechanics and vitalism.
[117] Letter of Berliner to Schlick, 26 August, 1926.
[118] This was the last edition during Planck's lifetime.
[119] Letter of Berliner to Schlick, 24 March 1933. Schlick (1924) had reviewed the *Physikalische Rundblicke* for the German review journal *Deutsche Literaturzeitung,* see Sect. 7.1.

The physiologist-philosopher Johannes von Kries was one of the most prolific contributors of the first decade. Covering the whole of his wide sphere of interests, he wrote on physiology (1921), (1923a), (1923b), Goethe (1919b), Kant (1924), the necessary and unique determination of the physical world view (1920), and the application of his interpretation of probability in physics (1919a) – a paper also listed in the above-mentioned bibliography. No wonder that we find also an obituary for him (Frey, 1929).

Let me now turn to the professional philosophers active in *Die Naturwissenschaften.* In the third volume we find a paper on non-Euclidean geometry by the neo-Kantian Richard Hönigswald (1915), a survey article on monism (Herbertz, 1915), and several historical or commemorative works. Authors include Benno Erdmann on Emil duBois-Reymond (Erdmann, 1914) and Leibniz (Erdmann 1916); Erich Becher (1921) with an obituary for Erdmann, a paper on Lotze's psychology (1917), and a review of a book by Hertwig (Becher, 1918); Theodor Ziehen on the philosopher Haeckel (1919) and with various reviews most important among which was a long critical discussion of the first edition of Schlick's *Allgemeine Erkenntnislehre* (Ziehen, 1920); Aloys Riehl (1921) who played off Helmholtz as the true heir of Kant against the Marburg neo-Kantians;[120] Oskar Kraus with a criticism of Leonhard Nelson (Kraus, 1918) and a paper about Francis Bacon methodology of imperialism (Kraus, 1919). In 1924, *Die Naturwissenschaften* celebrated Kant's 200th birthday with papers of Adolf von Harnack (1924), a renowned theologian and president of the KWG, and von Kries (1924) on "Kant's doctrine of space and time and its relation to modern physics". There was no scholarly neo-Kantian writing on that occasion.

The philosopher and independent writer Moritz Kronenberg (1865-1935)[121] furnished no less that 13 survey articles. They mainly addressed historical topics, among them "Democritus and Natural Science" (1915a), "Historical and Scientific Materialism", Schopenhauer (1919), "Goethe's view of nature" (1924) and "Fechner and Lotze" (1925), and general philosophical issues, such as "Individuality" (1923) or "Fiction and Hypothesis" (1915b). In a programmatic article for the first volume titled "On the history of *Naturphilosophie*", Kronenberg diagnosed a renaissance.

> [During the last 10-20 years] problems peculiar of *Naturphilosophie* stand in the center of the advancing research in the natural sciences, and in the most distinct domains it were questions of world view [Weltanschauung] …, philosophical basic terms which became the shibboleth of the parties or the keywords of exact scientific concept formation. (Kronenberg, 1913, p. 888)

So Berliner's philosophical education program was following a trend that already existed at the time of the foundation of *Die Naturwissenschaften.* A review of Kronenberg's introductory work on Kant makes clear why he was just the right man for Berliner's objectives.

> The author understands to properly choose his wording in an easily intelligible way without lapsing into trivialities, which partly can be explained from the fact that he never approached the questions as a partisan with passionate pros and cons. … At least one sees in the book – and this is no drawback –

[120] Berliner's letter to Schlick from 16 November, 1920, reveals that originally Ernst Cassirer had agreed to write the respective contribution on the occasion of the 25th anniversary of Helmholtz's death.

[121] Walter Killy und Rudolf Vierhaus: *Deutsche Biographische Enzyklopädie (DBE),* München: K.G. Saur, Bd. 6 (1997), S. 117.

that the author himself is inclined towards the side of idealism …; but this idealism is never only romantic and speculative, but based on a comprehensive knowledge of the whole area of culture, in particular also towards the scientific side. (Buchenau, 1918, p. 602)

With the debates about relativity theory and atomic physics after the war, the philosophy in *Die Naturwissenschaften* would take new forms and involve new actors, thus separating the classical philosophical topics from those discussed by the scientist-philosophers. The contributions by guilded philosophers, in particular, by neo-Kantians significantly decreased in number after 1920, the only exception being Kronenberg. The coverage of such matters was shifted into the review section, and once again the most reviews came from Kronenberg. The only other scholar with a comparably large number of philosophical reviews was the Logical Empiricist Zilsel[122]; as a matter of fact, in most of his 26 reviews he was more outspoken than Kronenberg. There was certainly also an economical factor behind these numbers because both Kronenberg and Zilsel had no academic positions and welcomed the honorarium. But this division between these two types of philosophical contributions, between the educational background papers and the philosophical discussions among scientist-philosophers, was also characteristic of the main papers.

After 1925, there are very few genuinely philosophical papers. Apart from a commemoration of the Göttingen philosopher Leonard Nelson by Otto Meyerhof (1928) and the mathematician Paul Bernays (1928), and the exchange of Riezler (1929) and Fleck (1930) on the concept of reality, we only find an article by Helmuth Plessner (1930) "On the problem of nature in contemporary philosophy". This indicates that the philosophical shibboleth which Kronenberg in 1913 had diagnosed within natural science under the influence of the struggles about relativity theory had moved to the edge of the submilieu represented by *Die Naturwissenschaften*, separating thus scientist-philosophers, some of them occupying chairs in philosophy, from those philosophers who kept insisting on the priority of philosophy over the sciences. But this was more than a mere reaction to the debates about space and time. In the volumes of *Die Naturwissenschaften* of the 1920s we also witness the emergence of a philosophy of science in the modern sense. The relative weight of Logical Empiricists among the contributions in the section "General issues and philosophy" is considerable. Their 17 papers even outrank Kronenberg's.

During the 1920s *Die Naturwissenschaften* were the top journal for those Logical Empiricists who had been educated or were still active as physicists or mathematicians. The situation would only change at the end of the decade when in 1929 the Vienna Circle constituted itself publicly as a subgroup under its own manifesto (Hahn, Neurath & Carnap, 1929) and when in 1930/1 the first number of *Erkenntnis* appeared after long-winded efforts. (Cf. Hegselmann/Sigwart, 1991) Thus Logical Empiricist had finally constituted themselves out of the submilieu represented by *Die Naturwissenschaften* not only as a group but also as a discipline of scientific philosophy which needed its own scientific journal and its own congresses. Apart from Zilsel's reviews the number of their contributions to *Die Naturwissenschaften* decreased after 1930. Another relevant factor was that during the 1930s Neurath energetically pursued the internationalization of the movement, a strategy in which the

[122] For a list of these reviews, see (Dahms, 1993).

Italian *Scientia* – which had a somewhat wider scope than *Die Naturwissenschaften* – became an important forum a broader discourse.

Evidence for the significance of Logical Empiricists within *Die Naturwissenschaften* is the review of the first volume of *Erkenntnis* authored by Kurt Grelling, a member of Reichenbach's circle in Berlin.

> The readers of the *Naturwissenschaften* are not unfamiliar with the fact that in the course of roughly the last decade a new philosophical direction has developed in close connection with the natural sciences, the exact ones in particular, which is characterized by the names Schlick, Reichenbach and Carnap. This direction has now begun to organize itself in a twofold way: in 1929 in association with the Prague meeting of mathematicians and physicists, and in September 1930 in association with the *Naturforscherversammlung*, it organized meetings which were much-frequented and produced highly interesting debates; and now it has created the present journal in which it introduces itself to the profession. (Grelling, 1931, p. 41)

Grelling's presentation in particular emphasized the unity of this new direction; despite their apparently diverging research programs both Schlick and Reichenbach were ultimately pursuing the same goal. *Die Naturwissenschaften* also took notice of the organizational activities of Logical Empiricism. Both the 1929 and 1930 "Tagungen für Erkenntnislehre der exakten Wissenschaften" were announced as parts of the 1929 meeting of the German Physical Society and the 1930 Naturforscherversammlung[123], and Reichenbach (1930a) published a report of the Königsberg meeting.

A mere listing of the philosophical contributions from the trained scientists among Logical Empiricists between 1917 and 1931 shows a high percentage of papers appearing in Berliner's journal. The only exception was Reichenbach. In Chapters 7 and 8 we will see that those papers were their most important ones for the debate on causality.

Looking through Philipp Frank's list of philosophical or popular papers[124] from the years 1917-1931, we find four papers in *Die Naturwissenschaften* (Frank, 1917, 1919, 1928, 1929),[125] one in the *Physikalische Zeitschrift* (Frank, 1918), and his short opening speech of the Prague "Erkenntnislehre" conference in *Erkenntnis* (1930).

Richard von Mises has five papers in *Die Naturwissenschaften* (Mises, 1919, 1922, 1927a, 1930a, 1930b), one in his *Zeitschrift für Angewandte Mathematik und Mechanik* (1921); his Dresden inaugural address (Cf. Sect. 2.1.2.2.) appeared in the *Zeitschrift der Vereinigung Deutscher Ingenieure* (1920), and he had an article on the occasion of the 80th birthday of Joseph Popper-Lynkeus in an aviation journal (1918). The opening lectures of Frank (1929) and von Mises (1930a) at the Prague meeting of German physicists – on which the Vienna Circle went public – were republished in the first volume of *Erkenntnis*.

In contrast to Frank, von Mises continued to publish in *Die Naturwissenschaften* after 1930 and even after his emigration to Istanbul. Apart from a paper on population statistics (1932a), he wrote reviews about philosophical books and

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[123] Cf. *Die Naturwissenschaften* **18** (1930), 1067-1068, with the wrong title "Tagung für exakte Erkenntnislehre" but with abstracts of some of the talks.

[124] As done in Planck's case I do not count newspaper articles; this is also a reasonable restriction adopted by Forman (1971). For Frank, Schlick, and von Mises, I have used the bibliographies in the respective volumes of the Vienna Circle Collection published by Kluwer.

[125] Additionally, Frank published there a review of Charlier's lectures on statistics (Frank, 1922).

books addressing the relation of science and culture: (1933a), (1933b), and in particular about the popular Viennese lecture series arranged by Menger and his colleagues (1933c) and (1935).

In a short note of 1921, von Mises introduced his newly founded *Zeitschrift für angewandte Mathematik und Mechanik* (1921b) that intended to overcome the separation between mathematics and technology. When the Göttingen mathematician Richard Courant (1927a) argued that the separation between pure and applied mathematics, albeit useful for a certain time, must now be overcome because of the deeper unity of mathematics, von Mises (1927b) wrote a harsh response which Courant (1927b) tried to appease.

Von Mises (1932b) published a long positive review of Frank's (1932) book on causality. Two years later, he (1934a) had an exchange with von Laue (1934) in which he took a firmly probabilistic approach against von Laue's insistence on the necessity of a deterministic basis for natural science. The bulk of von Mises's reviews was dedicated to the mathematical theory of probability – (Mises, 1932c) and the review essay (1934b) where he returned to his earlier criticism of Marbe (1919). Von Mises (1928b) is a critical review of the second edition of von Kries's *Prinzipien der Wahrscheinlichkeitsrechnung* (1886).

Between 1917 and 1931 Moritz Schlick published four articles in *Die Naturwissenschaften,* three articles in the *Kant-Studien,* and one paper each in three other journals. Together with (Schlick, 1917), (1920), (1931) I also include (Schlick, 1922) because the next *Verhandlungen der Gesellschaft Deutscher Naturforscher und Ärzte* were already part of *Die Naturwissenschaften.* Among the various papers discussing relativity theory, Schlick's (1917) was certainly the philosophically deepest one. Schlick also contributed a considerable number of reviews, and many offers from Berliners were passed on to Zilsel who himself published a paper on "The Asymmetry of Time and the unidirectionality of causality" (1927).[126] As we have seen above, Berliner would have published more papers of Schlick. The list of offers includes contributions to the *Festschrift* numbers on the occasion of Planck's 60th birthday in 1918[127] and the 60th anniversary of Planck's Ph.D. in 1929. Berliner's respective letter of 29 January, 1929, shows that he automatically counted Schlick among those close enough to Planck to be natural candidates.

When I tell you that by the end of June Herr Planck celebrates the 60th anniversary of his Ph.D. then you conclude by your inborn logic that *Die Naturwissenschaften* will publish a special issue … and that, as a matter of course, I invite you to cooperate and, at the same time, rely on your earlier promise given half and half to take a stand concerning the philosophical side of the newest developments of theoretical physics. This would be a most valuable conclusion of the whole number … and you will find yourself in the best physical company one can imagine, provided that all accept the invitation.

Planck's above-quoted listing of Schlick was thus not just a consequence of his tragic assassination.

For Hans Reichenbach we find only three papers between 1917 and 1931 together with a short communication (1919), the above-cited conference report (1930), and a series of reviews in *Die Naturwissenschaften* amidst no less that 39

[126] As already stated, Zilsel's approach to probability is closely linked to the "Anwendungsproblem" and thus it falls outside the scope of the present investigation for the same reason as does Reichenbach's.
[127] Cf. Berliner's letters of 5 October, 1917, and 20 October, 1917.

philosophical papers. After his two early papers on probability theory (1920a, 1920b), it took eleven years until Schlick's criticism of his views on causality prompted another paper of his (Reichenbach, 1931). Reichenbach was already in Istanbul when a long paper on "Kant and natural science" (1933), appeared. In it he argued that scientific philosophy was a consequent continuation of Kant's orienting philosophy at the model of natural science.

The greatness of a historical achievement does not consist in *presaging* the future development, but in *creating* it. And this is the judgment which we contemporaries have to pass on Kant: … his doctrine belongs to the past in the same way as does the scientific world view of the 18th century – but most certainly he is among those few whose philosophical work has paved the way on which present-day philosophy of natural science strides on. (Reichenbach, 1933, p. 626)

This was indeed quite a conciliatory statement if compared with the harsh farewell Schlick had given to Kantian philosophy a decade before at the peak of the debates about relativity theory. Evidently Reichenbach intended to stay in dialogue with the German scientists which were still influenced by neo-Kantianism. Such an aim was outside the scope of *Erkenntnis* because "Carnap and particularly Neurath did not want to concede space to the dispute with traditional philosophy" (Hegselmann/Siegwart, 1991, p. 464) but rather considered their journal as an internal forum for scientific philosophy. Reichenbach's paper once again exhibits the difference between his approach to causality and the debates investigated here. After showing how relativity theory dismantled the Kantian a priori categories of space and time, he came to the principle of causality which, to his mind, had a more general position within the Kantian system.

It was not only modern quantum theory which has overcome this idea. Rather has the criticism of the Kantian conception of causality begun already earlier by purely philosophical considerations, more precisely in connection with the philosophical critique of the concept of probability. (Reichenbach, 1933, p. 606.)

Rather than reporting a simple historical fact, Reichenbach referred to his own view according to which both causality and probability acted both on the level our judgments and in objective nature. This made Reichenbach an indeterminist or a probabilist simply because scientific and every-day reasoning invoked probabilities. In this sense, for him, the alternative between determinism and indeterminism did not concern the physical world in first place.

5.5 A Causality Debate: Nernst, Schottky, and Petzoldt

Let me now turn to a debate on causality in *Die Naturwissenschaften* in which none of the main protagonists of Vienna Indeterminism appeared but which contained many characteristic elements of the debate analyzed in the present book. Both Schottky and Nernst accepted genuine indeterminism but remained committed to an ontology of single basic entities rather that mass phenomena. This placed them half way between the Vienna and Berlin traditions.

5.5.1 Schottky and the Prehistory of Quantum Non-locality

In his paper "The problem of causality in quantum theory as a foundation of modern natural science altogether. Attempt of a popular presentation", the theoretical physicist Walter Schottky (1921) followed a dramaturgy familiar to both sides of the aisle in causal matters. He diagnosed a crisis in modern physics which, hopefully and expectedly, would be overcome after substantial modifications of basic concepts. Schottky's historical point of reference was not just Newtonian mechanics but the materialist world-view of substance (Stoff) and force.

> Not only is [by the year 1900] the whole world electric, but it is also much finer, more ethereal, and, as one could also say, more spiritual [geistiger]; for the view of nature no longer comprises only the material particles, their motions and forces, but also the whole domain *in-between* those material particles, and the states in this intermediate domain follow laws of their own, yet in constant active and receptive interaction with the motions of material particles. (Schottky, 1921, p. 493)

The main lesson in modern physics, so he continued, was taught by relativity theory. It "required a complete modification of our conceptions about space and time" (Schottky, 1921, p. 439) which brought victory to local physics [Nahwirkungsphysik]. Locality in the precise sense of relativity theory supplemented the notion of a causal determination of natural phenomena with the idea that no causal influence could come as a bolt from the blue. The next, and perhaps the deepest change of world view was necessitated by quantum theory which "is not just the product of arbitrary moods and a craze for change, but has emerged from the discovery of entirely catastrophic disagreements between the consequences of the principle of locality and the true facts in nature." (Ibid., p. 494) There were various attempts to bar or tame the demise of the principle of locality; for instance, Planck's second theory of radiation in which the field remained unquantized. But at bottom this involves

> a distinction between different field actions according to their origin [which] contradicts the most characteristic idea of the theory of local action [Nahwirkungstheorie]. … Thus we have to concede – and this is a consequence which so far only few physicists have made their own – that these state variables of the theory of local action, whose reality was out of question for decades, strictly speaking have no relevance for natural science. (Ibid. p. 495)

Schottky's positivism was of a mild brand and quite far from Heisenberg's (1927) meaning criterion. Instead he emphasized the inescapability of a concept of measurement that would become the backbone of Frank's theory of causality (1932) in quantum mechanics and, as a matter of course, of many subsequent interpretations of quantum mechanics.

> [The physicist] cannot avoid to introduce auxiliary quantities and believe in their reality, as long as observations correspond to the presupposed behavior of these quantities. … [Yet] the basic and never misleading question is … what am I able to measure and what happens when I have found out this and that, or when I am doing this and that. However in the present state of affairs, precisely this simple question puts us in a most embarrassing position. … The *law of causality itself,* with its complete determination of future phenomena by the present and past ones *seems to be questioned in its present form.* (Schottky, 1921, p. 496)

The key unsolved problem of local physics is to find suitable measurable quantities. And Schottky apparently associated the concept of causality with local physics rather than Laplacian determinism in general. Nonetheless the second part of Schottky's paper continues in quite an optimistic tone.

[T]he assumption of a local action in space-time which as the more precise form of the law of causality has become part of us, apparently must be abandoned. As a miracle from these shambles rises the edifice of quantum theory. With very few laws … one comprises and orders a wealth of regularities [Gesetzmäßigkeiten] for which the more exact and detailed field theory provided no explanation. (Ibid., p. 506)

The paper continued with a consideration which has become a popular starting point of present-day interpretative discourse in quantum mechanics. Interference experiments teach us that one cannot simply remove objects which have not been in the actual path of light. Thus a light quantum or a light knot[128] cannot be localized, rather "it puts out its feelers, as it were, in all directions." (Ibid., p. 509)[129] Obviously, such a conception runs into conflict with relativity theory – another indication that the principle of causality based on local physics has been shattered by modern quantum theory. Being aware that he was moving into the field of conjectures and speculations, Schottky now made virtue of necessity and explicitly introduced an action-at-a-distance that thread-like linked processes with absorption or emission of an elementary quantum of light energy. By introducing these threads "we do not distinguish between cause and effect, but only establish relations between the changes of state of the different particles." (Ibid. p. 510) Historically this picture can be seen as a kind of Machian holism that was based on a network of relations only. Within this relational ontology so far only statistical laws were possible; they correspond to a view from the distance in which the individual threads cannot be distinguished from the classical continuum. "What can be ascertained by the concept of causality, what is determined by the temporal laws of nature, are only the conditions for the frequency of the occurrence of elementary events (threads) of a certain kind." (Ibid., p. 511) As he would stress in a response to criticism, the existence of such laws was essential because "partial indeterminacy of the elementary processes would be absurd if one could not think *macroscopic* laws of a statistical but very general kind at its side. (Schottky, 1922, p. 982)

Schottky's terminology was clearly in flux, and he did not give a precise definition of what he understood under the principle of causality and how he distinguished "Gesetze" from "Gesetzmäßigkeiten". On the one hand, he took the principle of locality as elaborated by Einstein's theory of relativity as the relevant instantiation of the Laplacian ideal. This concept of causality was so much in crisis in atomic physics that Schottky proposed to introduce non-local actions. With respect to the Laplacian ideal in general, he was wavering.

For the time being one will still try to meet the "Laplacian requirement" by introducing unknown mechanisms …; but there are enough reasons, in particular from the side of quantum theory, which make probable, at least in the microcosm, a principal deviation from a principle of causality interpreted in this [Laplacian] sense. (Schottky, 1922, p. 982)

[128] "Lichtknoten", a terminology introduced by Einstein.
[129] This description can be found in the Feynman lectures (1969).

On the other hand, there remained strictly valid statistical laws for the atomic domain which Schottky took as causal laws valid for the frequencies of the elementary events. This was at bottom equal to the point of view defended by Riebesell, von Mises, and Frank who preferred to speak of mass phenomena and took them as new basic entities. While Frank and von Mises, accordingly, considered the individual events as outside the ontology accessible by quantum theory,[130] Schottky remained committed to an ontology of single material particles. And as many present interpretations of quantum mechanics he introduced non-local interactions to this end and arrived at a holistic and relational picture years before Bohr's talk about measurement apparatus and the indivisible experimental set-up would start. This makes Schottky's paper quite interesting for the present book, in particular when we recall that initially Bohm (1952) had called his non-local pilot wave theory a causal interpretation. Hence in a rudimentary form non-local entanglement of atomic processes was already on the market by 1921, and it had been discussed in a widely read journal. In contrast to its present defenders among them (Cushing 1994) and (Beller, 1999), Schottky coupled non-locality with a positivist emphasis of measurement and an unabashed acceptance of statistical causality. None of these features were required by the Weimar milieu.

5.5.2 Nernst and the Ontological Basis of Randomness

Schottky at the time was still a Privatdozent. Walter Nernst instead stood at the height of his public recognition. He had just received the Nobel prize for chemistry, was rector of the University of Berlin, president of the Physikalisch-Technische Reichsanstalt, he was in the company of some of the political leaders of the Weimar republic, and he had even been courted to become German ambassador to the United States – despite his involvement with gas warfare.

Nonetheless, Nernst was anything but a scientific Mandarin. According to Frank, he "exhibited the mentality of a member of the merchant class. He had no national or class prejudice and was imbued with a type of liberalism that is often peculiar to business men." (Frank, 1948, p. 134) This included occasionally unconventional manners and a "buoyant optimism." (Barkan, 1999, p. 196) Nernst enjoyed close relationships with industry, in particular with Emil and Walter Rathenau at the A.E.G.

In his rectorial address commemorating the founder of the University of Berlin "On the Emergence of New Stars", Nernst initially remembered his friend Walter Rathenau who had been assassinated some days before and saw this kind of deeds as in blatant contradiction to the ideals of university education. Rathenau "firmly believed in a logical development of polity [Staatswesen] under the modern conceptions and thus the preference for a special form of government did not find any support from him." (Nernst, 1922b, p. 8) And although Nernst remained equally neutral with respect to the new republic, he emphasized "that after the gloomy October and November days of the year 1918 … nevertheless a good many things developed in a way better than many troubled citizens have imagined back then. Who of us, in particular, would have dared to hope that the cultivation of science at the German universities would undergo a continued growth that was essentially unhampered –

[130] See Sections 8.3. and 8.5.

even though, of course, not removed from the hardships of these days." (Ibid., p. 3f.) The crisis was thus no reason to be pessimistic about science.

Most results of Nernst's research were intimately connected with concrete problems and industrial applications. He had a passion for automobiles and worked on combustion processes. He sacrificed many years for the construction of an electrolytic lamp, which required the accumulation of an enormous amount of empirical data about suitable materials. As Diana Kormos Barkan (1999) convincingly shows, this work was intimately connected to his most theoretical result, the third law of thermodynamics or Nernst's heat theorem.

Among the characteristic elements of Nernst's style of inquiry, Barkan notices, first, "the use of analogies from related domains in order to clarify processes for which he had not yet formed firm and intuitive notions." (Barkan, 1999, p. 51f.) Such model building by analogy enabled him to use quantum theory at a very early stage. Second, Nernst showed an exceptional "experimental dexterity and ingenuity." (Ibid., p. 127) "Yet he never became a pure empiricist. Instead, he kept theory and theoretical speculations in the foreground of his activities, and most of his experimental and instrumental innovations arose from theoretical elaborations of practical problems, where practice meant solving the questions at hand." (Ibid., p. 53) Third, "despite his endorsement of the molecular hypothesis and the ionic dissociation theory … Nernst refrained from introducing or speculating about material entities still unknown or insufficiently explored. His contentment with *formal relationships* constitutes another characteristic of Nernst's work in general." (Ibid., p. 66) This was quite in line with the descriptivist ideal of Mach and Kirchhoff. Fourth, "Nernst insisted on seeking intuitive, workable thermodynamic cycles in addition to theoretical calculations." (Ibid., p. 69) His quest for *Anschaulichkeit* accordingly was not prompted primarily by the Weimar milieu but rather continued the traditions of Mach, Boltzmann and Hertz. For this reason, Nernst was not attracted by the abstract principle view of thermodynamics that Planck advocated before he would accept Boltzmann's atomism and invoke it for the justification of his radiation formula. Instead "Nernst [already then] sought to find positive correlations between kinetics and thermodynamics and insisted on always checking one against the other." (Ibid., p. 74)

This was quite natural for a former student of Boltzmann working in physical chemistry. According to Barkan, "Boltzmann's influence looms larger than anyone's else in Nernst's scientific biography." (Ibid., p. 31) In the academic year 1885-6, Nernst studied in Graz and did his first independent scientific work. After his Ph.D. in 1887, he returned there for several months. Although Nernst had worked at Ostwald's institute in Leipzig from 1887 to 1890, he explicitly supported Boltzmann against Ostwald's energetics at the 1895 Lübeck *Naturforscherversammlung*. "Boltzmann's influence looms large in Nernst's career, not only in his atomism but in the tolerant and anti-dogmatic philosophy and history of science." (Ibid., p. 242) And indeed both philosophically oriented works written in the early 1920s set out with reminiscences of Boltzmann. This historical perspective will lead us to a reading of Nernst's abandoning causality that is starkly different from Forman's.

Nernst's inaugural address as rector of the University of Berlin "The Domain of Validity of Natural Laws" concluded with a reverence to the *Geisteswissenschaften*.

It is true, the exact sciences have been blamed to have tyrannized philosophical research. Perhaps one has to admit that the hitherto customary formulation of the principle of causality as an absolutely strict law of nature laced the mind [Geist] in Spanish boots, and it is thus at present the task of natural science to loosen these fetters sufficiently so that the free stride of philosophical thought is no longer restricted. (Nernst, 1922a, p. 495)

The Postscript significantly widened the philosophical perspective.

Obviously it is less important whether or not one considers the principle of causality as strictly valid, but much more whether one conceives the processes in nature to be comprehensible or whether one holds that the human mind is incapable of following these processes down to their last details. For example, most religions well adhere to the doctrine that all events occur according to the will of a highest intellect, that is, with perfect logic, which coincides with what the principle of causality requires. Until recently, physics generally adopted the view that – at least in principle – all events can also be recognized by the human mind as taking place logically; this was at all times contested by the doctrines of all religions. Thus if in actual fact the at present repeatedly discussed view, which was also discussed above, according to which only statistical averages of the events are accessible to our scientific knowledge, would turn out to be justified, then one would indeed have to state a striking and hitherto hardly foreseen parallelism between theological and physical conceptions. (Ibid., p. 495)

Notice that Nernst's 'conversion to acausality' sounded somewhat hypothetical. But apart from a rhetorical *captatio* in front of the zeitgeist, what was the rational basis of this negative conclusion regarding causality? At the begin of the paper we find a confession of faith to empiricism as defended by his teacher Boltzmann. "[A] law of nature is nothing but idealized experience, a fortunate combination of larger or smaller a number of observational facts." (Ibid., p. 489) Often the first step towards a new law of nature is based on analogy, as did, for instance, Fourier when treating heat analogous to a liquid substance. Or when Nernst himself "transfer[red] concepts from the theory of solutions to the then inconclusive conceptions of electronic structure and conductivity." (Barkan, 1999, p. 179f.) Already the example of Fourier echoes Mach's constant insistence of the principle of comparison as one of the specific instantiations of the principle of economy (Cf. Sect. 3.1). With reference to Boltzmann, Nernst also followed Mach's historico-critical method: the step from an older conception [*Vorstellungsweise*] to the next does not replace falsity by truth, but leads to a more purposive [*zweckmäßigeres*] picture of the facts. The change of conception necessarily implies a change of the laws of nature. "Often one imagines the natural law as something rigid and unalterable; but we have to correct this idea once we enter into a more profound, historical consideration … [after which] the conviction forces itself upon us that we do not possess any natural law in final form." (Nernst, 1922a, p. 491) Even in relativity theory the absolute constancy of the velocity of light could turn out to be merely an approximation. Analogously, quantum mechanics shows the limits of the theory of electromagnetic actions-at-a-distance. Yet on this view, the supplanted laws remain valid within certain domains and the full force of new conceptions and laws is visible only in more or less extreme cases, such as the perihelion of Mercury.

At this point, however, Nernst departed from Mach's and Boltzmann's teachings and considered the historical tendency that the laws of nature constantly become simpler not as owing to a biological advantage but rather as a "healthy kernel" of identity philosophy (Identitätsphilosophie), notwithstanding the wholesome criticism of *Naturphilosophie* by Kant and of the remaining negative traits of *Naturphilosophie* in Kant himself.

If one assumes that all laws of nature are only approximately true, "the curse of inexactness lies heavily upon each application" which Nernst considers "catastrophic in a logical perspective." (Ibid., p. 492) This put the principle of causality into question, at least when one followed Nernst's definition of it. "If one presupposes that the existence of absolutely strict natural laws is assured, … this yields necessarily the so-called principle of causality." (Ibid., p. 492) Nernst here understands causality as making possible, at least in principle, the existence of a Laplacian demon. But the Laplacian ideal of complete determinism has not only become questionable because in history so far every natural law has reached a limit of its validity. It was rather the second law of thermodynamics which showed a principal limitation. Similarly as Boltzmann and Exner, Nernst did not consider the second law as a stranger dwelling at the edge of the physical world. To the contrary, "[a]mong all laws [of physics] the thermodynamical ones occupy a distinctive position because unlike all others they are not only of a special kind, but applicable to any process one can imagine." (Ibid., p. 492) If one related all physical laws to the second law of thermodynamics, this would not reduce their significance or rank; "it would however put an end to the logical overuse of the laws of nature." (Ibid., p. 493)

In the following lines, Nernst did not invoke the epistemological argument which Exner had put forward (Sect. 4.4), that for the empiricist there is no preferred choice between determinism and indeterminism and that he has to content himself with the approximate character of all laws of science. Rather did he introduce a very specific working hypothesis according to which the oscillations of the zero-point energy in the luminiferous ether explain why a given atomic nucleus decays at this very moment. At first glance this seemed to salvage the principle of causality but only at the price that "we arrive at an infinitely extended system in the face of which our laws of thought fail." (Ibid., p. 494) Again, this was a move familiar from Boltzmann who had based his constructivist argument for atomism (Section 3.5) on our finite faculties of thought. Moreover, Nernst's ether fluctuations represented an argument of theory reduction to a more fundamental level, this time in order to explain why the principle of causality was only approximately valid.

According to Forman's reading of this passage, "it is clear that although Nernst wishes with all his heart and soul to renounce causality, he is simply unable to free himself from the implicit assumption that the world *really is* causal." (Forman, 1971, p. 85) There is some truth in this suspicion but Forman is wrong to surmise that when Nernst had become aware of this defect, he added the above-cited postscript to leave no doubt about his will to confess. It suffices to read further in the text.

> It is … very likely [wahrscheinlich] that all our natural laws are of the same kind as the second law of thermodynamics, that is, that they are essentially of a statistical character. Even within the domains mentioned above a natural law could accordingly fail to a large extent every once in a while, it is only so extremely improbable for such a case to occur that for all practical applications it needs (at least in general) not be taken into account.
>
> However, one cannot refuse to anybody the logical operation with absolutely exact natural laws as an abstraction, and thus one is also allowed to operate logically with the principle of causality in its strictest form as long as one remains conscious that in this way one leaves the grounds of experience and enters into the realm of purely speculative thought. (Nernst, 1922a, p. 495)

Although rigid determinism was of a speculative kind, to Nernst's mind, it remained practically useful. Having loosened the Spanish boots in itself did not commit the working physicist to abandon the principle of causality as long as he was clear about its limits. Indeterminism, on the other hand, was less speculative, but Nernst found some undesirable features in the then current version of Boltzmann and Exner, which prompted him to introduce the zero-point fluctuations of the luminiferous ether. From a historical perspective thus there was indeed some inconsequence in his view. Nernst closely followed Boltzmann's empiricist ideas about causality and he fully accepted the approximative character of natural laws. But the zero-point fluctuations showed that there existed substantial philosophical differences to the Viennese reading of Boltzmann. This becomes clearer in a small booklet titled *The Universe [Weltgebäude] in the Light of New Research* which was published in the year before and was based on popular lectures given at Berlin, Vienna and Prague. Its introduction started with a reminiscence.

When I was studying at Graz in 1886, Boltzmann delivered his inaugural address to the Vienna Academy of Sciences on the second law of thermodynamics [1905, pp. 25-50]. Among other things he there asserted that all attempts to rescue the universe from heat death will remain without success and that he will not make such an attempt. (Nernst, 1921, p. 1)

In his booklet, Nernst did embark onto such a rescue project which remains, as he frankly admitted, as speculative as all cosmology. Since the laws of the theory of heat are "the most general and most reliable laws we possess," (Ibid., p. 13) their fatal consequence can be avoided only by a process consistent with them but running in the opposite direction. There are two special cases of this general problem of universal thermodynamics. First, the long-range nature of gravitation leads to a big crunch of all matter of the universe. Nernst's strategy to avoid all these problems was a stationary universe with spontaneous matter creation that bears some similarities with the theory of Sir Fred Hoyle (1995). Second, the radioactive decay of heavy atoms provides the energy for the stars. Extrapolating then available knowledge Nernst took radioactivity to be a generic feature of all chemical elements down to the smallest ones. The universal decay chain ultimately leads to a "death of matter" (Ibid., p. 16) and its transformation into zero-point energy of the luminiferous ether. Nernst's modernized heat death was compensated by a random process spontaneously creating heavy atoms from the ether which contains a very high amount of zero-point energy. Even an extremely low probability for this process sufficed to avoid the fatal heat death. In his commemoration address, Nernst (1922b) sketched a cyclic model for the fixed stars. Based on his stationary universe and the conviction that on average just as many fixed stars are formed as have perished within a given time, he estimated that fixed stars flash up approximately eight times during their life. The novae which appear to us as world catastrophies on the firmament are, on this view, simply a normal feature within the life cycle of a fixed star.

What was the philosophical basis for Nernst's stationary cosmology? There are two conceptions about natural laws which Nernst emphatically opposed. First, he rejected the idea of an evolution of the Universe that involves an evolution of natural laws. This had been the old Fechnerian idea endorsed by Exner and the late Boltzmann.

It is impossible for the scientist to assume that at a certain time the whole world was in a chaotic state, out of which the blazing suns condensed, to ultimately arrive at a state in which the re-formation of suns is no longer possible. In other words: the idea that all happenings of the worlds, as it were, began at a certain day and become completely extinct at a certain day, is in itself [extremely] improbable. (Nernst, 1921, p. 13)

Secondly, Nernst was aware that it is non-trivial to assume that our laboratory-verified natural laws are valid for the whole universe. But he firmly kept to this principle, today typically called the cosmological principle, and considered it as a precondition of natural laws.

It was occasionally stated that there are regions of the cosmos in which, as in our Milky Way, entropy increases and such regions in which it decreases. In this way one simply denies our laws of nature; with such a trend of taste this booklet has nothing to do. (Ibid., p. 41)

This was of course an allusion to Boltzmann who had even contemplated that the direction of time would differ in such regions. (See Sect. 3.5). Against this backdrop Nernst's mechanism of matter creation appeared as a supplementary law posited to exclude a highly improbable but unwanted consequence of the genuinely indeterminist view. Precisely this had been Planck's strategy to avoid recombining fragments and evaporating oceans by introducing the 'hypothesis of elementary disorder' (Sect. 3.7). But while Planck had argued for the inescapability of determinism, Nernst contended that laws of nature were approximately valid only.

Although Nernst could accept that the basic processes of nature were random, there had to be a mechanism that was sufficiently simple to be described by a statistical law of nature. Recall his remarks about the increasing simplicity in our knowledge of the laws of nature. But there was, to my mind, a more important ontological aspect. When requiring that some basic ontological entity, the luminiferous ether, carry the irreducibly indeterministic feature of our Universe, Nernst rejected neutral monism and radical empiricism according to which the basic ontology of physical world was construed in accordance with the basic laws of nature. There had to be a single deepest level of physical reality.

5.5.3. A Defense of Petzoldt's Mach

The papers of Schottky and Nernst prompted a reaction by Mach's old ally Joseph Petzoldt. In quite a schoolmasterly letter to the editor Petzoldt wrote that "the questions which both the articles of Schottky and Nernst have raised regarding the validity of the principle of causality have essentially been treated by the epistemology of natural science some decades ago and they were brought to a conclusion." (Petzoldt, 1922, p. 693) Petzoldt then recapitulated Mach's rejection of the concepts of cause and effect and Mach's "reduction of causality to the mutual functional dependences of coincidences between sensations [Empfindungskoinzidenzen] solely which are expressed in the equations." (Ibid. p. 693) This conception can be applied equally well for local and non-local actions because the real basis of the concept of causality is the unique determination of natural phenomena. So far this was a faithful interpretation of Mach's flexible notion of causality though in the notoriously misleading wording of 'sensations' instead of 'elements'.

In his rejoinder, Schottky argued that Petzoldt at best addressed the old problem of the temporal orientation of natural laws; "however the novel feature is that the mentioned elementary processes, which are closely and rigidly connected, must be without any explicable connection with other elementary phenomena both forward and backward in time.” (Schottky, 1922, p. 982) This was indeed a blank spot of Mach’s relationalism and holism. The network of functional dependences had to be cut either by the approximative stability of a complex of functional dependences, by the intervention of our interests as investigators, or by a principle of unique determination. Schottky simply claimed that this was impossible in quantum theory.

Petzoldt’s more explicit criticism of Nernst reverted to the problem of induction. “[T]he laws of nature have attained a statistical character not only with the kinetic theory of gases and the second law of thermodynamics, but they have always possessed it and they had to possess it. Each law … is obtained inductively, deduced from a larger or smaller number of observations.” (Petzoldt, 1922, p. 693) And he explicitly mentions the method of least square distances. Accordingly, Nernst’s question whether natural processes could be partially chaotic “cannot altogether be decided by a direct experimental inquiry into nature.” (Ibid., p. 694) For, experimental physics is always approximative physics and can thus never completely apprehend the unique determination which is expressed in the physical equations only. That the decision between determinism and indeterminism remains open was also the position of Mach (Sect. 3.1), and Vienna Indeterminists would take this firmly empiricist stance without however relating probability and inductivist procedures.

As back in his (1890, 1895), Petzoldt invoked a determinative notion of uniqueness and effectively reduced both the principle of causality and the problem of induction to that single principle. More than in his earlier papers, stability became an all-encompassing property of the world. The stability of the animate world rests upon a greater stability of the physico-chemical world. “By this [convergence of instabilities to zero,] the principle of uniqueness rises to the rank of a postulate and ultimately to a law. The law of uniqueness is the inescapable presupposition, the logical a priori of the empirically given stability of ourselves and our [biological] environment.” (Petzoldt, 1922, p. 694) Since the alternative between determinism and indeterminism cannot be decided empirically, Petzoldt came rather close to a transcendental argument for uniqueness in lieu of causality. This was certainly not a basis to assess the genuine indeterminism that was, with qualifications though, present in Nernst’s paper. But as we have seen above (Sect. 3.2.), Petzoldt’s use of the concepts of uniqueness and stability was substantially different from Mach’s, all positive cross-references notwithstanding. The manifestly ontological character of Petzoldt’s reasoning becomes now clearer than in his earlier dialogue with Mach. Schottky (1922) was right to remark that the macroscopic statistical laws rigorously following from quantum theory suffice to explain the biological stability claimed by Petzoldt however the individual events be connected.

In December 1928, Petzoldt wrote his last letter to the editor of *Die Naturwissenschaften.* It shows that his principle of uniqueness led to a view of probabilistic laws that was almost verbally equivalent to Planck’s – not quite an expected outcome for Mach’s closest German ally. The main target of the letter was Heisenberg.

More and more frequently and particularly by younger physicists, the claim is made that all material events could be founded on the entirely lawless basis of atomic processes; macroscopic causality would not be harmed by that and find its expression in probabilistic laws. This completely ignores the previous question: how is probability and statistical regularity at all possible without the unique determination of natural events? To pose this question already means to answer it in the negative. (Petzoldt, 1929, p. 51f.)

Precisely this was Planck's position which he had put forward against Exner's indeterminism and which he maintained even after he dropped the requirement of elementary disorder (Cf. Sect. 3.7 & 4.5). Petzoldt once again argued by a chain of logical a prioris without invoking biological stability any longer.

With the concept of probability of any natural events the uniqueness of natural phenomena is implicitly posited in the same way as with Kepler's laws Newton's law of gravitation is implicitly given. The latter is the indispensable presupposition, the logical a priori of Kepler's laws. Equally the complete and unique determination of natural events [is the logical a priori] of all statistical regularity. (Ibid., p. 52)

Petzoldt's letter concluded with a complaint about the quantum generation.

[O]ne recognizes that physical research can proceed without any loss of facilities, that all its formulas, in particular also all its probabilistic laws are justified, even if one does *not* doubt causality, that consequently the skeptical attitude with respect to the law of uniqueness of natural phenomena furthers physics not in the least, and that it is accordingly superfluous. Without deriving the least profit from it, the new physicists only cause useless and infertile difficulties to positivist epistemology which after all follows their strenuous path with sincere sympathy. (Ibid., p. 52)

The quantum generation, Born and Heisenberg foremost, had explicitly used positivist arguments to justify the finality of quantum mechanics against future causal alternatives. And here one of the great old men of positivism charged them of tergiversation and faithlessness to the ideals of positivism on counts which could have come from Mach's arch-opponent Planck – except for the organismic stability. What has gone wrong here?

At the surface level, Petzoldt followed the Machian notion of causality and accepted the fundamental inexactness of all natural laws. This made possible a full acceptance of statistical laws as the second law of thermodynamics, as did all Vienna Indeterminists, Schottky and Nernst, and it went against Planck's quest for absolutely exact natural laws. But similarly as Nernst and Schottky, Petzoldt was unhappy about the ontological side and could not follow Mach's firm empiricism to the extent that there were some highly improbable exceptions to the general determinacy. In contrast to Nernst and Schottky, however, Petzoldt did not devise a specific mechanism to ground randomness, but invoked a transcendental argument not so different from Planck's arguments in favor of a determinist basis of physical science. Nernst probably would have considered Petzoldt's uniqueness as a metaphysical principle. This might also be a reason why the contacts between Petzoldt and Logical Empiricists were less intimate during the 1920s than one might have suspected at first glance. And Petzoldt's letters are not listed in the above-mentioned comprehensive bibliography about "causality" in the *Erkenntnis* of 1931.

6. Schrödinger: Indeterminism and Picture Realism

Whenever Erwin Schrödinger wrote about causality and determinism, he acknowledged his teacher Franz Serafin Exner for the priority of a genuinely indeterminist conception of physics. Schrödinger abundantly cited Exner's *Lectures* and dedicated the booklet (1932a) to memory of his. Today best known is Schrödinger's 1922 Zurich inaugural address which was published in *Die Naturwissenschaften* only in 1929. On many other occasions Schrödinger explicitly defended the core theses of Vienna Indeterminism, and in 1929 he did so overtly continuing his teacher's debate with Planck. By this time he already held Planck's former chair at the University of Berlin – the most prestigious position in Germany a theoretical physicist could aspire at. The biography of Walter Moore (1989) provides ample testimony that Planck himself had done everything to attract him to Berlin. When Schrödinger came to Berlin in 1927 the confrontation between his wave mechanics and the Copenhagen-Göttingen matrix mechanics had already become a dispute about basic philosophical principles such as causality and reality. So what attracted Planck and the other Berlin physicists in Schrödinger's thinking despite his unequivocal stand on these issues?

Above all, there was the fact that wave mechanics appeared closer to classical continuum physics and was more intuitive than the abstract and clumsy matrix formalism. But both theories were mathematically equivalent and Schrödinger's interpretative efforts could not reach such a seemingly clear-cut stage as what later became known as the Copenhagen interpretation. Schrödinger derived his wave equation from an action principle in full correspondence with Planck's firm conviction how progress in physics be made (See Section 3.6 & 3.8.).[131] There was also a philosophical alliance between Schrödinger and the Berlin physicists in their quest for a realist interpretation of quantum mechanics. They repeatedly stood up against the positivist type of arguments applied by Bohr, Heisenberg and Born. Schrödinger's respective activities appear at odds with his constant adherence to Exner's indeterminism and Mach's positivism.

This situation leaves present interpreters with basically two alternatives. Either, Schrödinger repeatedly changed his mind: after, in 1924, emphatically endorsing the dismissal of causality inherent in the BKS-theory, in 1927 he favored unambiguously causality and realism, while at about 1930 and after his own failed interpretative efforts he reconciled himself with Copenhagen just to return to his causal and realist program after Einstein's EPR-paper. Or, Schrödinger's philosophical conception – as sophisticated as someone could be who had occupied himself with philosophy more broadly than most of his colleagues – contains in fact various levels that combine in different ways with the various conceptual problems posed by quantum mechanics. The latter interpreters often distinguish an ontological, an epistemological and a methodological level where Schrödinger follows different models among them Mach, Boltzmann and Vedanta. If this view is correct, it represents a drawback for those who want to count Schrödinger as an advocate manqué of the pilot-wave program, among them Cushing (1994) and Beller (1999).

[131] Planck's letter to Schrödinger of 2 April, 1926, expresses particular delight about this fact.

To these two interpretative strands, the present book adds the historical perspective of Exner's synthesis of Mach's empiricism and Boltzmann's atomism that was characteristic for the Vienna Institute of Physics. Part and parcel of this heritage were the empiricist rejection of all finality claims for any given scientific theory and the historico-critical perspective on theory change. More than just perpetuating the local creed, Schrödinger returned to the master texts themselves and introduced some new elements into Vienna Indeterminism. While Exner had considered the alternative between determinism and indeterminism as an open empirical question that presumably could never be finally decided, Schrödinger considered it a matter of convenience and referred to Poincaré's conventionalism. Yet unlike the early Frank (See Sect. 8.1.), he did not count the law of causality itself as a convention. Schrödinger modified Boltzmann's conception of theories as pictures in such a way as to allow him to favor a continuist picture of wave functions over a discontinuist atomistic picture. In this Schrödinger amply used the leeway inherent in the relatively weak empiricist conception of physical ontology. Due to the intimate connection of the basic ontology with the respective law of nature both wave functions and mass phenomena were admissible elements of reality. A concept of physical reality that depended upon the intervention or observation of a subject and the metaphysical dualism inherent in the Copenhagen interpretation, however, remained unacceptable for the neutral monist Schrödinger. But there are two important qualifications to this Machian heritage; also they hark back to Exner who had surprisingly exempted the subject from his all-embracing physicalism. And so did his former pupil Schrödinger who would follow the all-in-one view of the Vedanta in order to circumvent the problem of other minds which is often seen as the weak spot of Mach's neutral monism. In contrast to Logical Empiricists and similarly as Planck, Schrödinger considered metaphysical questions not as meaningless but of great importance to humans. Yet they were only loosely connected to the philosophy of physics.

The first section of the present chapter provides the historical context for classifying Schrödinger as a Vienna Indeterminist. In the second, I give a critical overview of the rather diverging opinions about the consistency of his philosophy in the present literature. This will involve texts that fall outside the period of the present investigation. The third section analyses in detail Schrödinger's philosophical papers relevant for the issues of atomism and indeterminism from 1913 until 1932. The discussion of Schrödinger's views will be complete by an analysis of his 1931 correspondence with Schlick in Section 7.4. and his dialogue with von Mises in Section 8.6.

My findings favor those who argue that Schrödinger largely did not change his mind with respect to indeterminism. But there was some development in his views about the suitable ontology for his wave mechanics. Yet he never became a metaphysical realist, but stayed on the ground of the weak ontology characteristic of Vienna Indeterminism. It was, accordingly, rather ontology than causality which marked the difference between him and the Göttingen-Copenhagen interpretation. But the intimate connection among both aspects, the peculiar tenet of Vienna Indeterminism, did not properly fit into the German philosophical context such that Schrödinger's views were notoriously misunderstood by his contemporaries.

6.1. Schrödinger and Vienna Physics

Schrödinger had two scientific fathers: Hasenöhrl in theoretical physics and Exner in experimental physics. “No other person has had a stronger intellectual influence on me than Fritz Hasenöhrl, except perhaps my father.” (Schrödinger, 1989a, p. 15) To the coming theoretician, Exner’s scientific teaching was less important in retrospect. Yet the special climate within the Exner circle and the themes discussed there became a most important inspiration for Schrödinger’s general view of the world above and beyond indeterminism. At the beginning of his inaugural address to the Prussian Academy of Sciences, Schrödinger looked back to the days when he entered the University of Vienna.

> The old Vienna Institute, which had just mourned the tragic loss of Ludwig Boltzmann, the building where Fritz Hasenöhrl and Franz Exner carried on their work and where I saw many others of Boltzmann’s students coming and going, gave me a direct insight into the ideas which had been formulated by that great mind. His sphere of ideas may be called my first love in science [wissenschaftliche Jugendgeliebte]; no other has ever thus enraptured me or will ever do so again. (Schrödinger, 1929d, p. 1/xiv)

In 1910 Schrödinger obtained his Ph.D. under Exner with an entirely experimental work. His habilitation in 1914 consisted of a group of theoretical papers which were of interest to the Exner circle. Military service prevented him to accept a vacant assistantship with Hasenöhrl, such that he re-entered the University of Vienna in 1911 as an assistant to Exner. Schrödinger’s recollections of this decade interrupted by war service are of a mixed kind. He was glad to “belong to those theoreticians who know by direct observation what it means to make a measurement.” (Schrödinger, 1989a, p. 17) Yet we also find bitter criticism.

> I learned two things during these years: First, that I myself was not suited to be an experimentalist. Second, that the ground on in which I lived, and the people with whom I lived there, were no more suited than I to achieve experimental progress along great lines. This was mostly a consequence, among other things, of the tendency of the golden Viennese heart to place amiable bunglers [liebenswürdige Stümper] in key positions …, where they blocked progress. … Thus atmospheric electricity and radioactivity, which really had their beginnings in Vienna, were taken out of our hands. (Schrödinger, 1989a, p. 16f.)

From the biography of Moore one can conclude that this was alluding to the experimentalists Gustav Jäger, Exner’s successor, and Felix Ehrenhaft; but Schrödinger did not have a high opinion of his friend Hans Thirring’s work in theoretical physics either (Cf. Moore, 1989, pp. 129-131; 475).

In 1918 Schrödinger received word that he was seriously considered for an associate professorship at the University of Czernowitz.

> I made up my mind to lecture there honestly on theoretical physics, initially according to the pattern of the splendid lectures of my beloved teacher Fritz Hasenöhrl, fallen in the war, but besides to concern myself with philosophy, deeply immersed as I then was in the writings of Spinoza, Schopenhauer, Mach, Richard Semon, and Richard Avenarius. My good angel intervened, since soon Czernowitz no longer belonged to us. (Schrödinger, 1989a, p. 44)

In 1920 Schrödinger turned down an associate professorship in Vienna mainly for financial reasons and accepted an offer by Max Wien to come to Jena. He quickly climbed up the academic ladder. After a semester each at Stuttgart and Breslau in the fall of 1921 he became professor for theoretical physics at the University of Zurich, where he found in Hermann Weyl a friend and kindred spirit in mathematical physics and philosophy including the distrust of classical causality. Schrödinger remained in Zurich until he succeeded Planck in 1927. As in the days of Boltzmann (See Sect. 4.7.1.), there still existed a substantial difference in formal habits between the institutes in Vienna and Berlin. "Erwin introduced a style of informality into his lectures that was unlike anything ever seen before at the University of Berlin, where class lines were rigidly drawn." (Moore, 1989, p. 242) In 1933 he emigrated to Oxford, in 1936 he accepted a call to Graz, and in 1938 he again had to emigrate to Dublin.

It is somewhat strange that Moore finds Schrödinger's research of the early 1920s unsuitable for a genius.

He was still reacting to various concerns of the somewhat isolated Vienna school, still using his great mathematical facility to make improvements in structures built by others, although he was now thirty years old, an age by which most great theoretical physicists have been prepared to rebel against the paradigms received from their university teachers. (Ibid., p. 97)

Schrödinger himself was quite explicit that his reasons to do so consisted in his convictions about how a good theory look like.

Only very slowly did I approach the modern atomic theory. Its inherent contradictions sounded like shrieking dissonances when compared to the pure and inexorably lucid development of Boltzmann's reasoning. I even, as it were, fled from it for a while and, inspired by Franz Exner and K.W.F Kohlrausch, I took refuge in the sphere of color theory. … Only de Broglie's idea of electron waves, which I elaborated into undulatory mechanics, brought a certain relief. (Schrödinger, 1929d, p. 1/xiv)

His doubts, it seems, had not completely faded away even after his breakthrough in quantum mechanics. Outside inspirations were of great importance to Schrödinger. Compare his autobiographical note for the Nobel prize in 1933.

In my scientific work (and also in my life) I have never followed one main line, one program defining a direction for a long time. … [I]f I am to have an interest in a question, others must also have one. My word is seldom the first, but often the second, and may be inspired by a desire to contradict or to correct, even if the consequent extension may turn out to be more important than the correction, which served only as a connection. (Schrödinger, 1984, p. 362f.)

The passage continues with a brief description of his physical achievements.

The most interesting topic in physics seemed to me, strictly speaking, Boltzmann's probabilistic theory of thermodynamics, and some older works of mine … continue from there. A second group are the works on color theory which emerged from the contact with Kohlrausch and Exner and the study of Helmholtz. Of importance appears to me only the last result about the true meaning of the three and four color conception and its connection with the phylogeny of color vision. (Ibid., p. 363)

Color theory also neatly corresponded to his philosophical interests because in the tradition of Machian epistemology physiological investigations stood side by side with historico-critical analyses. Nonetheless, Moore overstates this link by calling Schrödinger's work "the most impressive example in all the scientific literature, of how the philosophy of Mach can be applied to an actual problem." (Moore, 1989, p. 127) The third group mentioned were of course his contributions to quantum mechanics up to the seminal papers of 1926 which were strongly inspired by the work of de Broglie.

6.2 Schrödinger and Philosophies: Repeated Changes or Consistent Program?

Scholars substantially disagree about content and import of Schrödinger's philosophy of physics. To some, he repeatedly changed his mind about fundamental issues such a causality and realism, to others, he tenaciously pursued a complicated philosophical program on various levels that was notoriously misunderstood by his Copenhagen opponents. Interestingly, interpreters' stand in this respect strongly depends upon which importance they assign to Schrödinger's philosophy within his overall scientific activities. To Beller, "Schrödinger was no less a philosophical 'opportunist' than his Göttingen-Copenhagen opponents" (Beller, 1999, p. 284), while Ben-Menahem (1989), Bitbol (1996), and deRegt (1997, 2001) take his philosophy very seriously and associate its core traits not only with the local Mach-Boltzmann tradition and his teacher Exner's indeterminism, but also with a long list of classical and contemporary views on causality and realism. This list includes Schopenhauer (de Regt, 2001, Bitbol 1996), neo-Kantianism (Beller, 1999), Husserlian and post-modern phenomenology (Bitbol, 1996), van Fraassen's constructive empiricism (de Regt, 1997), Putnam's internal realism, Blackburn's quasi-realism, and Sellars' new phenomenalism (all Bitbol, 1996).

The present study does not intend to amend this list by further names. Rather does it continue the local Viennese perspective by embedding Schrödinger's views into a network of references to his tradition and dialogues with opponents. Thus contextualizing Schrödinger contributes to changing the image of an idiosyncratic loner some of whose philosophical ideas presaged modern developments to the same extent as some of his interpretative proposals have meanwhile reappeared in alternative interpretations of quantum mechanics. Expectedly, there are conflicting claims also here. Whereas Beller and Cushing affiliate him with the Bohmian camp, Bitbol emphatically rejects this and associates Schrödinger, albeit with qualifications, to the modal interpretation and Everett's many-worlds-interpretation. But those classifications face a certain problem of evidence which is nicely described by Olivier Darrigol.

> Despite his dissatisfaction with the Copenhagen interpretation of quantum mechanics, Schrödinger never reached a satisfactory alternative. He just gave some hints about what to look for. The modern commentators of Schrödinger react to this tentativeness of his views in various ways. Some [Beller, for instance] see it as a sign of superiority, meaning that Schrödinger did not try to fix the interpretation of quantum mechanics prematurely. Others conclude that Schrödinger, like Einstein, was engaged in a quixotic quest.
>
> In any case, Schrödinger's reflections regarding the themes of holism, acausality and visualizability are not to be judged from their fertility in physics. Rather their usefulness should be

measured from the epistemological clarification they could bring to the field of quantum mechanics. (Darrigol, 1992, p. 275)

On Darrigol's account, Schrödinger was immersed in a foundational yet philosophical project that aspired deeper than his Copenhagen opponents who mainly sought a philosophical defense of their already established interpretation.

6.2.1 Routes to Wave Mechanics

Since Schrödinger did not tenaciously defend a well-entrenched interpretation of his own for a longer time, scholars have sought the changes in his views and tried to explain them. In Section 1.1.2.3. we have already encountered his alleged 1922 conversion to acausality under the pressure of the Weimar milieu. Interestingly, two years before the famous adaptation thesis appeared, Forman and V.V. Raman (1969) held that Schrödinger remained rather neutral until the BKS paper of 1924. But Raman and Forman already set up the simplistic dichotomy between the approaches of Bohr, on the left, and Einstein, on the right, and claim "the partisans of the left, in their effort to eliminate light quanta entirely, came, by way of virtual oscillators, to the matrix mechanics; partisans of the right, in their effort to justify light quanta, came, by way of a radical parallelism between light quanta and material particles, to the wave mechanics." (Raman/Forman, 1969, p. 299) More generally, they hold "that the anti-Copenhagen alignment usually associated with the interpretation of quantum mechanics had already formed by 1923 over the issue of quantum mechanics to be sought for." (Ibid., p. 314) Within this two-faction framework, Schrödinger commenced from an non-partisan attitude in 1922-24 until his enthusiastic appraisal of the BKS theory (Schrödinger, 1924a)

Yet in late 1924 or early 1925 Schrödinger came down off the fence, onto the right-hand side. Schrödinger's close personal relations with Willy Wien suggest that the stimulus to this shift is likely to have been as much personal and political as scientific … Thus by 1925 Schrödinger would appear to have become one of the few members of the Einstein-de Broglie faction. (Forman/Raman, 1969, p. 301)

And accordingly all his papers appeared in Wien's 'right' *Annalen* rather than in the 'left' *Zeitschrift für Physik*. To my mind, the close personal ties to the Wien brothers, however, rather reduce than increase the significance of Schrödinger's choice of journal. Just about the time when Schrödinger was brooding over his new wave mechanics in late 1925, he was involved in an affair that brought him closer to Wien but, at least in a political sense, in opposition to Einstein. German and Swiss physicists set out to perform a Michelson-Morley experiment at a high altitude in the Swiss mountains to check a positive result for the ether wind obtained by Dayton C. Miller in 1921 at the top of Mount Wilson. This alleged contradiction to special relativity was greeted by the enemies of Einstein. Although the experiment resulted in a further confirmation of relativity theory, regarding the organizational aspect of the project "Schrödinger found himself aligned with the more nationalist and antisemitic wing of the German physicists." (Moore, 1989, p. 168) Schrödinger wrote to Wien, who was involved in planning the experiment, about this issue in the same letter from Arosa of

27 December, 1925, in which he reported his breakthrough: "At the moment I am struggling with a new atomic theory. If only I knew more mathematics!" (from Moore, 1989, p. 196). Doubtlessly, when Schrödinger's papers appeared they were greeted by Planck, Wien and Einstein as the prospect of a reintegration of atomic physics into classical physics (Cf. Planck, 1928, Sect. 6.3.5.) while they were criticized by the Göttingen-Copenhagen group.[132] But it seems to me wrong to extrapolate this dichotomy backwards in time, into the dialogical phase – to use Beller's terms.

Raman and Forman primarily inquire why Schrödinger happened to develop de Broglie's ideas. To be sure, he acknowledged the stimulating function of Einstein's elaboration of de Broglie's matter waves and four years ago Schrödinger (1922b) had published similar ideas himself. But Raman and Forman find another reason more plausible. Already Martin J. Klein (1964) had argued that wave mechanics was the product of a tradition concerned with quantum statistics rather than with spectroscopy. Raman and Forman take this as justification for their dichotomic left-right scheme. Because of his stubbornly advocating a false energy level scheme, "among the central European physicists deeply involved in the problems of theoretical spectroscopy – and this was indeed the great majority of those seriously concerned with quantum theory – de Broglie must have had a very bad reputation." (Forman/Raman, 1969, p. 296) Einstein was never involved in spectroscopy and also Schrödinger strongly felt about the logical gaps in the field.

Linda Wessels rather accentuates the continuity in "Schrödinger's Route to Wave Mechanics" since the papers on gas statistics and stresses the foundational tendency of his scientific work. "[U]nderlying every step along the way was either a question about the physical significance of certain formal expressions, or a desire to capture theoretically an intuitive model of physical processes." (Wessels, 1977, p. 313) Schrödinger's "Remarks on the Statistical Definition of Entropy for an Ideal Gas" showed a way how to avoid definition of entropy from the microlevel but by a holistic approach: "the energy levels … of the gas molecules must now, of course, be derived from the energy level distribution of the body of gas as a whole, exactly the opposite of how it was previously done." (Schrödinger, 1925b, p. 439; cf. Wessels, 1977, p. 317) But only in 1926 Schrödinger discovered a holistic strategy that could guide the inference to molecular behavior and suggested to him a solution of the problem to understand the nature of molecules. His way of deriving Einstein's gas theory, so he wrote, "means nothing else than putting into effect the deBroglie-Einstein undulatory theory of moving particles according to which the particles are nothing more than a kind of 'wave crest' of a wave radiation that forms the basis of the world [Weltgrund]." (Schrödinger, 1926c, p. 95)

Darrigol distinguishes three periods in Schrödinger's work in statistical physics. Initially he favored the molecular point of view in the sense of Boltzmann's kinetic theory. Then in his work on the definition of entropy he "switched to a holistic statistical method, but remained attached to a separation of the dynamical model into individual molecules. … In the third period, Schrödinger adopted a fully holistic conception, in which the dynamical model was no longer analyzable in terms of individual molecules." (Darrigol, 1992, p. 255) "The importance of Schrödinger's statistical works was not so much their direct impact on other physicists, but their

[132] This alliance can, for instance, be clearly seen by Wien's angry reaction on Heisenberg's criticism of Schrödinger (Cf. Moore, 1989, p. 222).

determining effects on his own reflections on other subjects." (Ibid., p. 238) In a philosophical perspective and beyond the clarificatory achievements, it appears to me that Schrödinger's holism in statistical mechanics basically put the Exnerian approach to natural laws into practice because there was no longer a preferred status of explanations from the microscopic constituents which were assumed to follow deterministic laws. After 1926 Schrödinger abandoned de Broglie's treatment of particles and waves as separate entities in favor of a pure wave theory of matter which entailed an even stronger form of holism in virtue of the infinite extension of his wave functions. Borrowing an idea of Arthur I. Eddington, Schrödinger after 1937 considered "the 'particles' as proper modes of vibration of the closed universe as a whole." (Bitbol, 1996, p. 48f.) Bitbol concludes that once Schrödinger had developed a second quantized formalism, the "radical inversion of roles between the states and the bearer of the states, between the whole and the parts, had just to be transposed to the theory of atomic phenomena." (Ibid., p. 54)

Surprisingly, Schrödinger's first paper on wave mechanics made almost no reference to the wave picture so that he "urged acceptance of his wave equation solely on the basis of its success with hydrogen and the fact that these results were obtained without the artificially imposed quantum conditions of the old quantum theory." (Wessels, 1977, p. 331) Wessels offers two reasons why. Firstly, Schrödinger knew that his audience was "convinced that the key for solving the quantum problem was to surrender all hope for a theory based on physical pictures." (Ibid., p. 332) Secondly, he had not yet established the stability of the wave packets. Only in the second paper do we find the more intuitive presentation of the theory by the formal analogy between optics and mechanics. But Schrödinger quickly recognized that the wave equation could not be interpreted as a description of the original matter waves and outlined an electrodynamic interpretation. Yet both interpretations could not be maintained and Schrödinger emphatically rejected Born's interpretation of his wave function as producing the probability of a particle to be found at a certain place. Until the EPR-paper he often just rehearsed certain elements of the Copenhagen creed in a critical perspective (Schrödinger, 1929e, 1930). This has led some interpreters to the conclusion that he had willy-nilly accepted the matrix mechanical interpretation until 1935; and only in the late 1940 and in the 1950 he launched a new campaign that, to many readers, appeared as a return to his starting point of taking the wave function as the ontological basis. But this view has been contested by various authors.

Bitbol's book considers Schrödinger's interpretations of 1926 as "an early and simplistic way of coming close to the interpretation of the 1950s" and concludes from a retrospective analysis of his mature views that Schrödinger developed "by fits and starts … a coherent methodological program" (both Bitbol, 1996, p. vii) for which he also elaborated an intricate epistemological foundation. I find Bitbol's thesis about the coherence of Schrödinger's view in general quite appealing although there are several places where he associates them too quickly to quite a few modern views. Some of these links are inspiring, others suffer under an insufficient historical contextualization. The weak spot is, as we shall see, Mach's empiricism.

Yemina Ben-Menahem criticizes Raman and Forman and holds "that Schrödinger did not change his views in any substantial way with regard to causality. To the end of his life he was ready to entertain the idea that some of the fundamental laws of nature are merely statistical laws and that chance is a legitimate element of science.” (Ben-Menahem, 1989, p. 309) Although Schrödinger changed from considering acausality as more natural (1922a) to an undecided position – in his conventionalist account (1929a) –, he constantly repeated his skeptical arguments against the traditional Humean concept of causality and was “willing to accept *chance* as an irreducible element of physical reality.” (Ben-Menahem, 1989, p. 319) It is instructive that Ben-Menahem distinguishes three notions of causality current in the debates during the 1920s. Apart from the classical Humean notion of causality as regularity for which classical mechanics was the paradigmatic case, relativity theory suggested the different notion of causality as locality, and finally Bohr considered “a causal description to mean the laws of *conservation of energy and momentum.*” (Ibid., p. 313). Schrödinger (1922a) carried Boltzmann’s and Exner’s speculations about a statistical theory of gravitation (See Sect. 4.4) into relativity theory, so that randomness did not contradict relativistic causality. In none of the places where Schrödinger criticized discontinuity as the problematic feature of quantum mechanics he suggested that his aim was to restore causality. While Schrödinger kept insisting on (sufficiently continuous) space-time pictures, Bohr’s causal description was complementary to a spatio-temporal description of physical events. In these days, the notion of causality was considerably in flux.

The second main issue of Schrödinger’s philosophy of quantum mechanics is ontology, or rather what his repeated references to realism actually mean. Emphasizing Schrödinger’s indebtedness to Mach’s philosophy, Wessels stresses his adherence to the descriptivist tradition that reached back to Maxwell and Boltzmann who replaced the unsuccessful quest for explicit mechanical models by the use of consistent mechanical pictures.

> Schrödinger’s commitment to finding a coherent description of microsystems is best explained … by considering the formative role of traditional science on Schrödinger himself. Schrödinger learned his physics in Boltzmann’s Institute at Vienna. … There Schrödinger was trained to develop and use realistic intuitive and mathematical descriptions of microsystems to explain macrophenomena and to regard this as the proper approach to science – in explicit opposition to the approach urged by the other major influence in Vienna at that time, Ernst Mach. The text books and journals of the day reinforced this approach. … It was not until after the First World War that physicists would be weaned on the paradoxes of Bohr’s theory of the atom. They would learn to use a model, even several models, without worrying whether they had a complete picture, or even whether their models were entirely compatible with their theory, or with each other. Schrödinger was unwilling, probably unable, to adapt to this new way of working. Even at the time he invented and developed wave mechanics, Schrödinger’s commitment to the descriptive tradition was becoming outmoded. Yet that commitment was essential for the creative act for which he is best known. … There is an obvious irony in Schrödinger’s situation: his equation became the cornerstone of a new physics that rejected the descriptive tradition from which it sprang. (Wessels, 1983, p. 272f.)

Wessels’ account derives support from Schrödinger’s above-quoted reminiscence that the “shrieking dissonances” of atomic theory kept him off that field for a while. But the allegation of anachronism prematurely echoes the criticism of Bohr and Heisenberg, and the relation of Mach and Boltzmann was considerably more complex. Also Darrigol considers the *Bild*-conception of physical theory as the most conservative trait of a thinker who in virtue of his indeterminism and holism was anything but an adherent to conservative methodology. Darrigol rightly emphasizes that Schrödinger did not refer to the Kantian pictures of Hertz which must obey the laws of thought, but to Boltzmann’s historically acquired pictures. Nevertheless, "Boltzmann was not *practically* more inclined than a good Kantian to give up the presentation of phenomena in space and time." (Darrigol, 1929, p. 272)

Bitbol, to the contrary, calls Schrödinger's ontological attitude over-revolutionary. "Schrödinger claimed more strongly than any other physicist that the gap between quantum mechanics and classical theories hindered any partial rescue of the classical concepts" (Bitbol, 1996, p. 22) and he criticized Bohr in correspondence for not having completely abandoned the classical corpuscular concepts.

If you want to describe a system, e.g., a mass point, by giving its p and q, then you find that the description is possible only with a limited degree of exactness. This is very interesting, for it seems to me a limitation on the applicability of the *old* concepts of experience [Erfahrungsbegriffe]. But it seems to me imperative to require the introduction of *new* concepts, in which these restrictions *no longer* exist. For, what is in principle unobservable should not at all be contained in our conceptual scheme, it should not be possible to represent it within the latter. In the *adequate* conceptual scheme it must no longer appear as if our possibility of experience [Erfahrungsmöglichkeit] is restricted by unfortunate conditions. (Letter of 5 May, 1928, from Bohr, 1985, p. 464)

Bohr answered in rather Kantian terms that the "old concepts of experience appear to be indissolubly linked to the basis of the human faculty of perception." (Ibid., p. 465) Bitbol describes this difference as such:

Would it be absurd to think that, by contrast, some spurious remnants of metaphysical (macro)-realism have been the actual reason for the Göttingen-Copenhagen physicist's reluctance towards Schrödinger's light-hearted tendency to endow new theoretical constructs (ψ-functions) with the status of 'real entities'? Think for example of Born's assertion that (macroscopic) physical apparatus 'consist of bodies, not of waves': *consist,* and not *appear* to consist. … Of course, the members of the Göttingen-Copenhagen group were very far from being just naive realists, even about the macroscopic bodies. However, the type of pragmatic analysis to which they submitted the concept of 'real objects' of daily life prevented them from going very far in their ontological inquiry." (Bitbol, 1996, p. 14)

It is interesting to note that also Philipp Frank gave a pragmatic reason why concepts such as position and momentum should be maintained despite their obvious limitation (See Sect. 8.7.).

Mara Beller plays down Schrödinger's above-quoted reference to Boltzmann's evolutionary account of our laws of thought and *a fortiori* of our basic theoretical pictures. Rather does she consider it as one among Schrödinger's rhetorical strategies. "While much insight can be (and is) gained by studying Schrödinger's approach *vis-à-vis* the 'descriptive tradition' of Helmholtz, Hertz and Boltzmann, Schrödinger's 'commitment' to it should not be taken too strictly." (Beller, 1997, p. 427f.) His superb mastery of the matrix formalism and of the Copenhagen interpretation permitted him to be

skillful in positivist analysis when criticising the orthodoxy. … In such criticisms, Schrödinger often presupposed the verificationist meaning of quantum formulas: the uncertainty relations, for example are not merely limits on the possible measurement values of physical variables – the uncertainty restricts the very definability of the concepts used. … Similarly, Schrödinger used positivistic arguments when 'deconstructing' the concept of a particle. (Ibid., p. 428)

Over the years, so Beller reads Schrödinger's post-war Dublin lectures (1949-55)[133], it was "the relative weight of positivistic and model-descriptive elements in Schrödinger's argumentation that changed … according to the theoretical challenges he encountered." (Beller, 1997, p. 429 and Beller, 1999, p. 284) Although Schrödinger had his unique intellectual style, in particular, an "intense quest for comprehensibility" he did not "adhere to any consistent philosophical tradition." (both Beller, 1997, p. 429) He freely changed positivist and realist strategies, according to the local rhetorical requirements, and "employed a causal or a statistical approach. according to the specific theoretical situation he encountered." (Ibid., p. 431) But does 'addressitivity' necessarily refute the prospect of a consistent philosophical program? As Beller, Bitbol detects an "awkward reciprocity" in "the fact that almost no creator of quantum mechanics could dispense with positivist-like arguments, at one stage or another of his investigation." (Bitbol, 1996, p. 9) And assenting to Ben-Menahem he holds that Schrödinger "taught orthodoxy to his students because he had nothing better to propose, whereas he expressed more overtly his doubts and his projects when he was due to speak to more specialized audience." (Ibid., p. 82) Yet simultaneously Bitbol sets out to generalize Ben-Menahem's analysis about the continuity in the concept of causality "to the whole of Schrödinger's interpretation of quantum mechanics." (Ibid., p. 31)

Beller does not underestimates the complexity of Schrödinger's ideas about ontology.

> Yet an analysis of Schrödinger's writings reveals instead a very sophisticated position, along neo-Kantian lines: the concept of reality 'as such', as it objectively exists independent of all human observers, is indefensible, if not downright meaningless. … [Still it] is as indispensable in science as it is in practical life. (Beller, 1999, p. 282)

Reality thus appears as a relative construct, quite in the sense of the invariances of neo-Kantianism. This point is also stressed by Bitbol (See below).

In Section 2.2.2., I have already criticized that Beller's microscopic dialogical analysis leaves little space for truly philosophical motives to unfold among the founders of quantum physics. This also proves as a great hobble in assessing Schrödinger's declared philosophical ambitions. Still, they are hardly reconcilable with Beller's implicit presuppositions about the character of philosophy. While she seemingly accepts neo-Kantianism as a coherent philosophical tradition, positivism is treated as a volatile rhetorical strategem rather than a position that could be adhered to seriously. This has two negative consequences. First and more generally, the philosophical ambitions of scientists are measured against the ideal of adherence to a philosophical school. The attempts of the scientist-philosophers to develop a philosophy from amidst the single science thus do not qualify as philosophy, neither does as a consequence large part of Logical Empiricism. Second, Beller seems not at all interested in the type or historical context of positivistic or verificationist arguments. Heisenberg applied a positivist meaning criterion within a framework that was largely anti-positivist (See Sect. 2.2.2.). On the other hand, the Machian tradition – though not intended as a 'school philosophy' as neo-Kantianism – was sufficiently strong to find adherents. Thus an appropriate appraisal of what the Machian heritage

[133] Beller (1997) is an essay review of (Schrödinger, 1992).

consists in, proves to be a precondition of assessing Schrödinger's philosophy. Unfortunately, this is the point where one must diagnose substantial shortcomings in the literature.

6.2.2 Between Mach and Boltzmann: The Issue of Realism

In the case of Paul Hanle's paper, the misreading of Mach produced the wrong impression of a deep cleft between Boltzmann and Exner (See Sect. 4.4.). It also leads to the false impression of a fundamental cleft between Exner and Schrödinger. Being more open to philosophical influences than Beller or Forman, Hanle claims that "there was a fundamental difference in their approaches to physics. While Exner was a dedicated experimentalist and a positivist in his attitude toward theory, Schrödinger devoted practically all his career to erecting the theoretical structure of physics and rejected positivism as inadequate." (Hanle, 1979, p. 235) Moreover, "Schrödinger, like the Machists, was unhappy with 'metaphysical' methods in physics, but he recognized a need for some direction in research from metaphysical principles – from the Kantian scaffolding. Exner had denied that need." (Ibid., p. 261) Hanle, strangely, takes it as a metaphysical stand that "for Exner the unity of the world was more fundamental than empiricism" (Ibid., p. 239), so that Exner erroneously took Brownian motion as an instance of genuine indeterminacy. Yet this was just an instance of Occam's razor which, indeed, is more important to the empiricist than to a Kantian whose transcendental categories are laid down a priori. Schrödinger used ontological parsimony as an argument against Heisenberg's quantum jumps. As they were neither required to account for observable effects nor an integral part of the formalism, they could simply be eliminated.

Boltzmann, whom Hanle reads in a rather Kantian fashion, and also Schrödinger clearly saw the difference between indeterminism in practice and indeterminism in principle. After 1926, Schrödinger (1929a, 1932a) repeatedly stressed it in print. But to the empiricist in the local Viennese tradition this difference was negligible and, accordingly, Schrödinger's unequivocal support for indeterminism reached back to his Vienna days. Hanle is, however, fully right about Schrödinger's attitude toward Born's interpretation of his wave function.

> [S]uch an indeterminacy [in principle] was too stringent a restriction to place upon nature. Experiment had not decided whether nature is absolutely determined or is partially undetermined, whereas Born, Heisenberg, and Bohr had decided in advance. For Schrödinger, molecular indeterminacy was possible, even likely in a way, though not because of quantum mechanics. Heisenberg's uncertainty principle proved only that indeterminacy was built into quantum mechanics, not that it was built into nature. So Schrödinger abhorred the claims of closure of the theory. (Hanle, 1979, p. 268)

Two recent interpretations have tracked the Machian influence to considerable detail and related it to Boltzmann's *Bild* conception of physical theory. According to Henk de Regt,

> Schrödinger did achieve a synthesis between the ideas Mach and Boltzmann, by applying them to *different domains*: Mach's to ontology and epistemology, and Ludwig Boltzmann's to methodology of science. … Mach's ideas had a stronger influence on Schrödinger's philosophy while Boltzmann's methodology was certainly more important for his discovery of wave mechanics. However, when it

came down to questions concerning how to best interpret quantum mechanics Mach's philosophy became of equal importance. (De Regt, 2001, p. 99f.)

"Schrödinger rejected the 'strong' methodological tenets of Machian positivism, and thus was left with the 'weak empiricist methodology shared by all scientists: the precept that theories must be empirically adequate." (De Regt, 1997, p. 479) Already in the first chapter of *My View of the World* (written in 1925 but published only in 1961), Schrödinger filed his discontent with Machian descriptivism.

[C]all to mind that feeling of anxious, heart-constricting desolation and emptiness that, I daresay, has crept over everyone on first comprehending the delineation given by Kirchhoff and Mach of the task of physics (or of science altogether): a *description of the facts that is as complete and thought-economical as possible*. … Indeed, aiming at this goal *in itself* would not suffice to keep going the research work in any domain. By suspending metaphysics in actual fact, art *and* science would be exanimated into siliceous skeletons, incapable of the least further development. … But theoretical metaphysics *has* been eliminated. There is no appeal against Kant's sentence. … Metaphysics does not form part of the edifice of knowledge but is the scaffolding [Gerüst], without which further construction is impossible. Perhaps we may even be permitted to say: metaphysics turns into physics in the course of its development. (Schrödinger, 1989a, p. 48f.; 1964, p. 35 translation altered)

At first glance, Schrödinger seems to repeat Planck's classical argument about the infertility of positivism for modern physics. Yet Planck had considered the greater fertility of realism as an argument in favor of convergent realism while Schrödinger just considered metaphysics as a scaffolding that can be removed once the edifice is finished. This was the way how, according to his mind, Boltzmann's pictures acted in physics as an indispensable guide of progress. In his 1954 *Nature and the Greeks,* Schrödinger explained this Boltzmannian surplus of theory (See Sect. 3.4.) by the difference between description and understanding. He approved the positivist method of an economical description of the observed facts. But other than Mach and Kirchhoff, he believed that "even from the positivists' point of view we ought not, so I believe, to declare that science conveys no understanding." (Schrödinger, 1954, p. 89) The mechanical theory of heat or Darwin's theory of evolution simply convey true insight beyond a mere storage of facts. This shows that in science a second feature is at work. "The scientist subconsciously, almost inadvertently, simplifies the problem of understanding Nature by disregarding or cutting out of the picture to be constructed, himself, his own personality, the subject of cognizance." (Ibid., p. 90) This "momentous step" which is not without gaps and pitfalls of paradox "might be called objectivation" or "the hypothesis of the real world around us (*Hypothese der realen Aussenwelt*)" (all ibid., p. 91) The last expression was of course a shibboleth for Mach and it became Logical Empiricists' favorite example of meaningless metaphysics.

Yet the continuation of the passage shows that Schrödinger was not at all after metaphysical realism.

Well, the 'real world around us' and 'we ourselves', that is our minds, are made up of the same building material, the two consist of the same bricks, as it were, only arranged in different order – sense perceptions, memory images, imagination, thought. It needs, of course, some reflection, but one easily falls in with the fact that matter is composed of these elements and nothing else. Moreover, imagination and thought take an increasingly important part (as against crude sense perception), as science, knowledge of nature, progresses.

> What happens is this. We can think of these – let me call them *elements* – either as constituting mind, everyone's own mind, or as constituting the material world. But we cannot, or can only with great difficulty, think both things at the same time. To get from the mind-aspect to the matter-aspect or vice versa, we have, as it were, to take the elements asunder and to put them together again in an entirely different order. (Schrödinger, 1954, p. 91f.)

This sounded like return to a truly Machian stand. But then, how should we read the principle of objectivation that, to Schrödinger's mind, has originated back in antiquity? In a historical perspective, one might recall Exner's thesis that the emergence of the objective world view constituted a cultural achievement basic to modern sciences rather than a metaphysically necessary principle. The principle of objectivation, accordingly, was nothing absolute and had its gaps, but to later generations of scientists it became a subconscious precondition of science. In a systematic perspective, Henk deRegt considers it as a "pragmatic, methodological assumption" (DeRegt, 2001, p. 99) and introduces a philosophical distinction that closer corresponds to Boltzmann picture realism, but does not reflect the large historical dimension which Schrödinger had assigned to objectivation. He distinguishes the domains of ontology, epistemology, and methodology.

> Ontological realism asserts that the world (whatever its specific constitution) exists independently of our knowledge of it. Epistemological realism asserts that scientific theories aim to provide knowledge about an unobservable reality behind the phenomena. Finally, methodological realism merely declares that scientists, when working with a theory, should act as if it truly represents reality in all respects. (DeRegt, 1997, p. 463f.)

In view of Planck's inference from the success of methodological realism to epistemological realism – or empirical realism in Kant's terms – the boundaries between these domains might be more than "blurry" (Ibid., p. 463) on the general level.

The local Viennese tradition was, to be sure, more specific and concerned the relation between Mach's ontological and epistemological anti-realism and Boltzmann's *Bild*-realism and the quest for *anschaulich* theoretical pictures. "Notwithstanding his persistent use of the expression 'real', Erwin Schrödinger's interpretation was clearly inspired by his belief in *Anschaulichkeit* as a criterion for understanding, rather than by a realist epistemology." (DeRegt, 2001, p. 98) Thus *Anschaulichkeit* did not restrict "the possible character of reality, but the possible character of *understanding*." (Ibid., p. 96) Regarding the *anschaulich* description of atomic phenomena in space and time, Schrödinger wrote in his second paper on wave mechanics: "We cannot really alter our forms of thought, and what we cannot comprehend within it we cannot understand at all. There are such things – but I do not believe that atomic structure is one of them." (Schrödinger, 1984, p. 118) And he criticized the absence of *Anschaulichkeit* in Heisenberg's theory.

Looking at these arguments with the eyes of the Mach-Planck controversy, it appears that to all scientist-philosophers sharing Planck's background, and to those who overlooked Schrödinger's distinction between description and understanding, all this must have sounded as an assent to a Kantian categorical approach with its static and preconceived forms of thought and a criticism of positivism. What, however, bluntly contradicted Planck's Kantian approach was Schrödinger's insistence on *Anschaulichkeit* and space-time pictures. Stressing the historical tendency of deanthropomorphization, Planck was primarily after simple principles of great generality and a deterministic regularity at the basic level. Insistence on *Anschaulichkeit* was better reconcilable with the Machian view. (See Sect. 3.1.) The only difference was that for Mach the objects of intuition were typically not parts of a theory but basic functional dependences, such as the law of the lever. Or in de Regt's terms, Machian *Anschaulichkeit* corresponded to direct visualization, for instance, in classical mechanics, while Boltzmann's represented an indirect and deeper form of visualization that conveyed a higher degree of understanding. Quantum jumps defied spatio-temporal visualization, and the "wave-particle duality prohibited an unambiguous visualisation of radiation." (De Regt, 1997, p. 463) But non-uniqueness was as unacceptable for Boltzmannian pictures as for a physical theory. For Mach, who subsumed the use of direct pictures or indirect pictorial hypotheses under the principle of economy of thought, ambiguity was undesirable but not tantamount to inadmissibility in principle. Because of Mach's physiological approach, pictures stemming from distinct senses, e.g., vision and temperature, need not be formally consistent as long as they did not leads to conflicting predictions about the empirical facts. As regards unique and universal theoretical pictures, Schrödinger clearly followed Boltzmann's lead.

When Schrödinger first discussed the equivalence between his wave mechanics and matrix mechanics, the issue of descriptivism returned.

> Today there are not a few physicists who, like Mach and Kirchhoff, regard the task of physical theory as being merely a mathematical description (*but as economical as possible*) of the many empirical connections which exist between the observable quantities, that is, a description which reproduces the connections, as far as possible, without the intervention of unobservable elements. On this view, mathematical equivalence has almost the same meaning as physical equivalence. In the present case there might perhaps appear just to be a certain superiority in the matrix representation, because through its *Unanschaulichkeit,* it does not tempt us to form space-time pictures of atomic processes, which must then perhaps remain beyond control. (Schrödinger, 1984, p. 160)

Schrödinger subsequently rejected the identity of mathematical and physical equivalence because of differences in fertility for coupling atomic theory with neighboring theories, such as electrodynamics. Thus after his equivalence proof, the surplus of wave mechanics was not ontological but pragmatic.

6.2.3 The Ontological Conversion of Epistemology

Let me return to Bitbol's discussion of Schrödinger's over-revolutionary attitude and investigate it from the perspective of the apparently conflicting local influences. This will support Bitbol's claim that Schrödinger advocated an ontological conversion of epistemological arguments, or as he wrote in the above-quoted letter to Bohr, the search for "an adequate conceptual scheme". This was precisely how Mach attributed reality to sufficiently stable complexes of functional dependences which Schrödinger supplemented by a Boltzmannian *Bild*-realistic comprehension of these dependences and by a theory reduction to the basic universal concepts they rest upon. All this, to my mind, does not support the post-modern interpretation or the anomalous parallelism between the facts and their theoretical picture or model that Bitbol advances.

Bitbol departs from the diagnosis that, on the metaphysical level, Schrödinger closely followed Mach's anti-realism and extended it in two directions. By adding Schopenhauer's elaboration of vedantic spirituality, according to which the whole world consists just of a single mind, he arrived at a substantive idealistic monism. Yet apart from this ontological perspective, there is also an "epistemological (or methodological)" (Ibid., p. 14) one. "The two faces of Schrödinger's attitude towards the concept of 'reality' can thus be characterized as follows. Fully recognize that the 'real objects which surround us' are nothing else than constructs, but take these constructs *very* seriously, since they are a precondition for our life." (Ibid., p. 13f.) Bitbol's opposition: ontology versus epistemology and methodology, accordingly, does not agree with de Regt's distinction: ontology and epistemology versus methodology. But their disagreement is largely terminological, and I have a certain preference for Bitbol's terminology because it better conforms to the emergence of Schrödinger's ideas from within Mach's epistemology. Concerning the cat paper, Bitbol writes: "Schrödinger [1935] was here again suggesting an ontological conversion of epistemological requirements, namely a projection of the epistemic limitations onto an appropriate system of intentionally aimed at entities." (Bitbol, 1996, p. 88) According to deRegt's wording, this was plain epistemological anti-realism because the assumption of a world behind the phenomena was barred.

The main problem of Bitbol's account is, to my mind, the interpretation of the intentional motivation of the quasi-realist strategy in science. Instead of the Machian stand, which Schrödinger (1932a) advocated himself, Bitbol chooses a transcendental or phenomenological approach. For, "a quasi-realist thinker like Schrödinger is a post-modernist in philosophy." (Bitbol, 1996, p. 41) The turn from a classical to a post-modern stance took place after Schrödinger's two direct interpretations of the wave function had failed. "By 1929, he had acknowledged explicitly an irreducible distance between representation and appearances which is typical of the post-modern trend of thought." (Bitbol, 1996, p. 28) "Bohr's strategy, which involved *couples* of complementary symbolic (wave-like and corpuscle-like) pictures, was replaced by a distinction between the picture and the events, between the (wave-like) content of an *unique* continuous representation and the discontinuous observable events." (Ibid., p. 29)

Bitbol detects in Schrödinger's writings five non-metaphysical criteria for endowing the wave functions with ontological significance. First, monist and quasi-realist ontology must be "taken in a restricted (Quinean) sense of choosing an appropriate system of references, not in the sense of an act of picking out some set of intrinsically defined entities." (Ibid., p. 99) This contained the above-mentioned move of "defining a system of entities in such a way that epistemological considerations become *redundant* with respect to it." (Ibid., p. 100) Second, the "ability of the wave functions to embody the law-like connection between experimental events" was considered as an "index of their 'reality'" (both ibid., p. 100). Partaking in a stable relational structure was precisely Mach's precondition to qualify as a scientific fact. Yet Bitbol considers this as a strong neo-Kantian element because it reflects the fact that we are not only interested in laws but that they "*are* necessary for us, if we are to objectify a domain of entities beyond the level of the sequences of isolated experimental facts." (Ibid., p. 100) We shall see below that Bitbol wrongly reads Mach as someone who could deal only with collections of isolated facts. Third, "to mould an ontology out of a set of instruments of statistical predictions" (Ibid., p. 100) was admissible for Schrödinger because he did not think that there existed a crucial difference between statistical and sharp predictions. Fourth, "the project of ontologization of ψ-waves has further obstacles to overcome, once their objectivity has been recognized." (Ibid., p. 102) While Heisenberg had linked 'reality' to the Latin *res* and considered the wave function only as *potentia*, Schrödinger insisted that "virtualities manifest themselves not only through their being actualized in such-and-such an experimental event exclusive of any other event, but also by modulating *as a whole* the characteristics or distribution of events." (Ibid., p. 104) Bitbol here points to "adiabatic (or 'protective') measurements [which] are able to provide direct access to … distributional features of the wave function." (Ibid., p. 106)[134] From an experimentalist's standpoint, accordingly, "the most natural extrapolation of the macroscopic ontology into the microscopic realm, is not an ontology of bearers of sharp values but an ontology of bearers of distributional characteristics." (Ibid., p. 108) And hence we obtain as macroscopic limit of quantum theory the Gaussian distributions characteristic of all real-world measurement protocols of classical physicists. This at first glance could have been an argument to Exner's taste. But inspired by Wolfgang Köhler's gestalt theory, Schrödinger (1929b) gave it another tack. Thus, fifth, ψ-waves "are individuals by virtue of their having a *form*, namely a wave-length and a … modulation." (Bitbol, 1996, p. 108)

6.2.4 Neutral Monism and Anomalous Parallelism

Bitbol finds Schrödinger's ontology convincing up to the notorious blind spot of quantum mechanics, the measurement problem. Yet "Schrödinger's dominant attitude towards quantum mechanics [was]: push that description of the entities which can be construed both objectively and repeatably to its limit; don't bother about its connection with experimental outcomes until the *very last* stage of the description." (Ibid., p. 118) Schrödinger wavered in the hope whether a definitive solution of the measurement problem could ever be found. "But what was really needed was a full acceptance of the *parallelism* between the time-development of the holistic wave-function (object+apparatus) and the sequence of macroscopic events, rather than an new blend of the old idea of a causal interaction which takes place between objects and apparatuses in order to *produce* the events." (Ibid., p. 123) Since the early Schrödinger did not succeed in construing this parallelism as the emergence of macroscopic properties from microscopic ones, his post-modern turn led him to everyday catalogue-like structural relations between macroscopic facts – as we find them in our life-world – and the ψ-model of quantum mechanics.

Bitbol calls this position 'anomalous parallelism'. It "expresses the circumstance that the series of experimental facts is not supposed to be linked by a strict law to the time development of some overall wave function, but only through Born's probabilistic correspondence rules." (Ibid., p. 256) Moreover, "*anomalous parallelism* was not only one of the dominant components of Schrödinger's late interpretation of quantum mechanics; it also agreed more closely with the general trends of his philosophical outlook than with most alternative conceptions." (Ibid., p.

[134] See (Dickson, 1995) for more on this new developments.

261) To Bitbol's mind, Schrödinger developed anomalous parallelism because he abhorred the instrumentalist consequences of a stand like Bohr's where our position in the world remains a crucial point of reference.

Bitbol claims that anomalous parallelism was the mature end product of a continuous development of Schrödinger's epistemological thoughts. I doubt whether his account of Schrödinger's understanding of the facts of everyday life is historically accurate, in particular as far as the role of Mach is concerned. The close relation between macroscopic scientific facts and our life-world is not only a creed of phenomenology, but also part and parcel of Mach's philosophy. Adopting a less restricted view on Machian ontology would moreover yield certain convergences between Bitbol's parallels.

Turning to details, Bitbol distinguishes three different elements of Schrödinger's view about the things-of-our-environment: perspectivism, emphasis on form, and holism. First, the "object only shows us its profiles or aspects; its presentation to us is *perspectival* and *incomplete.*" Form "is what confers individuality and permanent identity on entities given to the senses. When form happens to be inaccessible at every instant such an object can be identified by relying on the continuity of its trajectory." If no elementary components of matter can be individuated, we must rely upon more complex forms. "These organized wholes, or their characteristic observable form, must be considered as the only real individuatable, and permanent objects." (all ibid., p. 160) The first feature appeared in the continuation of the above-quoted passage from *Nature and the Greeks* after Schrödinger had subscribed to Mach's neutral elements.

> For example,… my mind at this moment is constituted by all I sense around me: my own body, you all sitting in front of me and very kindly listening to me …, and, above all, the ideas I wish to explain to you, the suitable framing of them into words. But now envisage any one of the material objects around us, for example my arm and hand. As a material object it is composed, not only of my own direct sensations of it, but also of the imagined sensations I would have in turning it round, moving it, looking at it from all different angles; in addition it is composed of the perceptions I imagine you to have of it, and also, if you think of it purely scientifically, of all you could verify and would actually find, if you took it and dissected it, to convince yourself of its intrinsic nature and composition. And so on. There is no end to enumerating all the potential percepts and sensations on my and on your side that are included in my speaking of this arm as an objective feature of the 'real world around us'. (Schrödinger, 1954, p. 92f.)

How could neutral monism combine with this conception of 'everyday thing'? Bitbol commences from a very restricted view on Mach's elements. "Only sensorial experience can be ascribed, according to Mach's positivism, the status of factual material; only atomic sensations are truly *given.* The perceptual or intellectual components are only considered as an artificial structure superimposed onto the brute data, or as useful fictions which can be modified according to the needs." (Bitbol, 1996, p. 164) It is true, "Schrödinger was not so comfortable with sensualistic reductionism." (Ibid., p. 165) But as we have seen in Section 3.1., Machian elements neither corresponded to isolated perceptions of red spots on a screen, nor were they atomistic in the sense that they were indivisible. Elements were just the elementary sensational constituents of a sufficiently stable complex of elements whose further divisibility was left open. Elements could be immediate sensations, earlier sensations taken from memory, imagined sensations, and all kinds of thoughts. Moreover, all our sensations were already embedded into previous experiences and ultimately into a provisional world view. There were more complex kinds of sensations which made it possible to intuit at a single glance Stevin's thought experiment establishing the law of the inclined plane or the meaning of a Chinese character (See Mach, 1988, Sect. I.2 & IV.4). It was precisely this departure from a *tabula rasa* conception of sensual experiences which at all permitted Mach to account for the historical development of science by the combination of fantasy and the principle of economy. (Cf. Haller, 1986c). Or so he writes right at the beginning of the respective section of the *Mechanics* : "It is the object of all science to replace, or *save*, experiences, by the reproduction and anticipation of facts in thought. Memory is handier than experiences, and often answers the same purpose." (Mach, 1988, p. 494/577) Thus Bitbol is wrong in asserting that

> in Schrödinger's conception, the basic constitutive elements do not reduce to bare sensations as in Mach, or to sensations plus images of sensations as in Russell. Schrödinger's 'events' already mix up sensitive and imaginative components, into a 'complex' whose structure is likely to be of an intellectual origin. … Thus, in his version of phenomenalism, the constituents of the *real objects* are not just actual and imagined sense-data, but rather perceptual *complexes* which are so elaborate, so 'inextricable', that they make any further analysis into elementary data quite difficult and probably pointless. (Bitbol, 1996, p. 165)

Real objects are "complexes of events, which are themselves inextricable complexes of sensorial, imaginative, and intellectual components. … And the complexes eventually acquire complete *autonomy* with respect to their constituents." (Ibid., p. 166) But also Machian facts were multi-layered sufficiently stable and approximately autonomous complexes that could be intuited in a single act and be economically communicated to other humans, from craftsmen to their apprentices, or among fellow scientists. Communication was, according to Mach, one of the driving forces for the economy of science. Just this was meant, or so I believe, when the late Schrödinger described the formation of invariants which we have adopted since childhood. "It begins with the sensory complex of the individual, but very soon extends to forming mutual invariants, in common to the individuals that are in social contact." (Schrödinger, 1995, p. 146) Bitbol even arrives at an "ideal norm" (Bitbol, 1996, p. 188) of forming invariances within the constant changes of our environment, a faculty that is already acquired by a child who has thus, it seems, learned to apply neo-Kantian categories.

There is, moreover, no trace of a combination of "both experience and intuition of essences" which prompts Bitbol to hold that "Schrödinger's conception of the real object … [was] much closer to Husserl's phenomenology than to any empiricist or positivist doctrine." (Ibid., p. 166) Mach's evolutionist epistemology which assumed a far-reaching continuity between the first experiences of a child and scientific theories makes the following simply a pseudo-problem. "Are the elements then pre-given and later connected in order to make complexes, or are the complexes pre-given and later disintegrated in order to reveal elements?" (Ibid., p. 168) On Mach's account, nothing was pre-given. The problem of the empirical basis only emerged with Logical Empiricists who rigorously set apart empirical observations and formal theory.

To be sure, there existed a major difference between Schrödinger and Mach as to the status of theoretical elements within such complexes. While for Mach, hypotheses were mere placeholders for experiences still to be made – or a promise of future sensations – Schrödinger's quest for theoretical understanding followed Boltzmann's lead. The goal were universal theoretical entities to which our observations could be reduced to. But all this was far from the neo-Kantian perspective that there exist invariances in our experience which could, in the limit of the whole history of science, replace the old Kantian categories. Bitbol arrives at such a classification of Schrödinger by claiming that "[p]erceiving a real object, according to Schrödinger, is tantamount to associating unconsciously with it an indefinite number of *expectations* concerning *future* explorations or experiments." (Ibid., p. 180) Taking the infinite limit of "anticipations, or presumptive synthesis of aspects into material objects," (Ibid., p. 181) one arrives at the 'horizon' of Husserlian phenomenology. This infinite limit of autonomous complexes makes problems for Schrödinger's complexes which are at bottom only a "finite aggregate of actual and possible perceptions." (Ibid., p. 189) The solution of this problem is, to my mind, simply to regard them with Mach as approximately stable but always capable of further development. According to Bitbol, however, the problem roots much deeper and its solution ultimately requires autonomous parallelism. "Schrödinger wavered between a quasi-phenomenological attitude, which is more adapted to the case of familiar 'things', and a constructivist attitude, which fits better with the case of objects of science." (Ibid., p. 189) This yielded problems with the scientific revolution that had occurred with quantum mechanics.

> It was the kind of radical revision of beliefs which is typical of scientific revolutions and which has no manifest equivalent in our way of dealing with the view of the world presupposed by daily actions and speech. What Schrödinger insisted upon when he compared the notion of 'thing' to a scientific construct is … that this very notion of thing should not be sheltered; not any more, at any rate, than the most fundamental axioms of a (falsifiable) scientific theory. But is this possible? (Ibid., p. 196)

Bitbol views three possible attitudes. First, "*conservative monism* aims at preserving at any cost the traditional notion of material body, even in the microscopic domain." This class includes Born, with qualifications, and Bohm. Second, *loose dualism* assumes a fluctuating separation between the classical macroscopic world "wherein the traditional notion of material body operates, and a domain (the microscopic world) wherein it collapses." (Ibid., p. 197) This was Bohr's home territory. Third, *ontological parallelism* accepts the existence of two conflicting ontological attitudes and devises several strategies to reconcile them, such as Schrödinger's anomalous parallelism.

This list is not complete. It does not include Exner's synthesis of Mach's empiricism and Boltzmann's theories as pictures. To Exner's mind (Cf. Sect. 4.4.), facts grow continuously while theories might undergo radical changes. This seems to me also Schrödinger's stand in his quest for a radically new quantum ontology. I even think that owing to Mach's principle of continuity Schrödinger would have been rather critical of Kuhnian revolutions and incommensurability.

Thus far my critical discussion of the present literature. In the subsequent chapter I shall try to add a new element to it and treat Schrödinger as member of the tradition of Vienna Indeterminism and as a scientist-philosopher who further elaborated his teacher Exner's synthesis of Mach and Boltzmann. To be sure, Exner's name is mentioned by Bitbol (1996), de Regt (2001) and, of course, Hanle (1979), as the ancestor of Schrödinger's indeterminism. But interpreters hardly go beyond Schrödinger's own acknowledgments. In elaborating this historical perspective I shall focus almost exclusively on texts that appeared in *Die Naturwissenschaften* or whose philosophical aspiration is evident.

6.3 Schrödinger on Atomism and Indeterminism

From 1917 to 1935 Schrödinger published ten papers and one review in *Die Naturwissenschaften*. They reflect the full breadth of his scientific interests. In the first, the young Vienna Privatdozent reviewed new results on atomic and molecular heats (1917). Having become a recognized leading expert in color theory, Schrödinger twice wrote on this subject (1924b, 1925a), while at the same time he published his endorsement and interpretation of the BKS theory (1924a). After his breakthrough in wave mechanics, he gave an explanation of the continuous transition between micro- and macromechanics (1926a). Already in Berlin, he eventually had his Zurich inaugural address (1922a) published in 1929. In the same volume we find his exchange with Planck (1929a), a discussion of the gestalt properties of wave functions (1929b), and a review of Eddington's *The Nature of the Physical World* (1929c). The Oxford émigré still contributed a paper on the inapplicability of geometry at atomic distances (1934) and the cat paper (1935) in which he reacted to the EPR-problem. In subsequent years Schrödinger would regularly have papers in *Nature* after which Berliner's journal had originally been modeled, among them a philosophical discussion of indeterminism and free will (1936).

There is no textual basis to argue as in Section 5.4. because Schrödinger did not publish any philosophical text before the year 1929 – although he had already written (1922a) and a sketch of his world view (1989a). From 1929 on popular and philosophical papers appeared quite regularly and at different places, from the daily press to the separately published (1932a).[135] What can be said, however, is that Schrödinger's paper on the law of nature (1922a) and in particular the cat paper (1935), both published in *Die Naturwissenschaften*, have been among the most influential texts for the discussions about the interpretation of quantum mechanics. The papers (1924, 1926a, 1929b, 1934), on the other hand, must be counted as important contributions to the then current foundational discourse in physics.

6.3.1. On Boltzmann's Atomism

In his early years, Schrödinger had worked in several typically Viennese fields, both experimentally and theoretically. What Moore classifies as Schrödinger's "first outstanding paper" (1989, p. 75) harked back to Boltzmann's mathematical atomism. (Sect. 3.5) Yet instead of a constructivist argument involving humans' finitary reasoning powers, Schrödinger was searching for an explicit example where atomism and continuum physics lead to diverging scenarios. Citing Boltzmann's paper on the

[135] I have used the bibliography in (Schrödinger, 1984).

indispensability of atomism in natural science (1905, pp. 141-157), Schrödinger stated the task of the contemporary atomist.

It has often been claimed and is, so to speak, part of the atomist's creed that all the partial differential equations of mathematical physics which connect the spatial and temporal variation of some physical variables (temperature, deformation, field strength, etc.) are incorrect in a strictly mathematical sense. For, the mathematical symbol of the differential quotient describes the transition to the limit to *arbitrarily* small spatial variations, while we are in fact convinced that in forming such "physical" differential quotients we must halt at "physically infinitely small" regions, i.e., at those that still always contain very many molecules; if we were to push the limiting process further, the quotients concerned, which up to then really were proceeding nearer and nearer toward a definite limit and practically seemed to have already reached it, would again begin to oscillate very strongly and only much later they would perhaps approach a true limit. For the latter limit, however, and irrespective of our inability to ever measure it, those simple laws which are expressed in the partial differential equations and hold for the "pseudo limits" of the former type, would not be valid in the least.

If we intend to put this conception into effect, we are confronted with a double task. First, all those differential equations first derived by consideration of a continuous medium as differential equations in the strict sense, now must instead be derived in the above sense as difference equations on the basis of a model constructed of molecules. (Schrödinger, 1914, p. 916f.)

The first task is the easier one. But a successful proof only establishes the feasibility of the atomistic conception, not its necessity. "And compared with the phenomenologist" – that is, for Mach and all those whom Boltzmann called the mathematical phenomenologists – "we are always at the disadvantage that he typically reaches his goal more quickly by a more simple and plausible ansatz." (Ibid., p. 917)

Atomistics [accordingly] also calls for a second task, through whose solution it only establishes its exclusive right over the phenomenological theory. This consists in searching and predicting *such* conditions under which the differential equation based on a continuum actually leads to an incorrect result because of the truly atomistic structure of matter. (Ibid., p. 917)

As a simple model, Schrödinger compared a one-dimensional atomistic model of a string in which one elongates a finite number of isolated atoms with the familiar d'Alembert differential equation of the vibrating string. Of course, the limit obtains in the physically meaningless situation when the distance parameter *a* between the atoms converges to zero. For a constant distance parameter the problem transforms into finding those features which the system of initial values x_n^0 for the distances between the atoms or the initial elongations $\xi_n^0, \dot{\xi}_n^0$ must possess "to be somewhat similar to a continuous elastic medium." (Ibid., p. 925) The result is that the $\xi_n^0, \dot{\xi}_n^0$ must be similar to continuous functions that show significant changes only at distances which are large compared to *a.* This condition is met for functions which correspond to averages over a sufficiently large number of atoms. If one argues that only such averages correspond to the physically observable quantities, then both approaches are equivalent. But this argument is inapplicable for phenomena of thermal disturbance [Wärmestörung] which are realized in this simple model by randomly elongating the atoms in a finite segment that is large compared to *a* but leaving the chain unaltered elsewhere. Any average value is blind for these internal differences of elongation and, accordingly, cannot describe the phenomenon of thermal disturbance.

We see that the young Schrödinger was advocating Boltzmann's atomism with his already developed sharp mathematical weapons. In the debates about quantum mechanics Schrödinger would defend a continuous theory based on his wave function and, much later, a unified field theory. This was his main departure from the Boltzmann's teaching.

6.3.2. What is a Law of Nature?

When in 1929 Schrödinger published his 1922 Zurich inaugural address without changes, he was well aware that indeterminism was no longer a minority view. He added a short preface that was to secure Exner's priority for it. "The subsequent rise and development of quantum mechanics has brought Exner's sphere of ideas into the focus of scientific interest, by the way, without his name ever being mentioned." (Schrödinger, 1922a, p. 9/133) Schrödinger was fully certain about his case.

> Within the past four or five decades physical research has clearly and definitely shown that *chance* is the common root of all the strict regularity that has been observed, at least in the overwhelming majority of natural processes, the regularity and invariability of which have led to the establishment of the postulate of universal causality. (Ibid., p. 9/ 136)

The most surprising aspect in this passage is the enormously long time period as a result of which indeterminism has become inevitable. Only the final decade (1913-1922) was shaped by Bohr's theory of the atom, and another decade (1900-1912) could have been accounted to Planck's quantum theories of radiation. Schrödinger, however, ultimately reached back to the 1870s and 1880s that had, on the one hand, witnessed Boltzmann stepwise developing his statistical theory of the second law of thermodynamics. On the other hand, the five decades harked back to the year 1872 in which Mach's seminal booklet *On the History and the Root of the Theorem of the Conservation of Work* first appeared, hardly noticed by the German physics community. Mach's main achievement, further elaborated in the *Mechanics,* was a modified conception of causality and a thoroughly historical and evolutionary perspective on the principles of physical science. As we have seen in Chapters 3 and 4, Boltzmann largely, however with important qualifications, followed this program from the late 1880s on, and Exner set up a synthesis of both approaches as the basis of a genuinely indeterministic world view.

Also Schrödinger rehearsed the Machian view of the postulate – or principle – of causality according to which "every natural process be absolutely determined at least through the totality of the circumstances or physical conditions that accompany its appearance." (Schrödinger, 1922a, p. 9/135) *Causes*, according to this view, are constant and regularly occurring *conditions* of an event. Schrödinger, moreover, followed Mach's evolutionary epistemology in depicting how humans have arrived at the postulate of causality by continuous abstraction from the regularities discerned both unsystematically in their "daily struggle for life and afterwards … from systematically and rationally planned scientific experiments." (Ibid., p. 9/134) Seeking their advantage in the struggle for life, humans ultimately went beyond experience and created the idea of a necessary regularity in the course of natural phenomena. As Boltzmann, Schrödinger called the belief in causality a habit of thought [Denkgewohnheit] which humans have acquired through hundreds and thousands of years. Yet they did so “from observing … precisely *those* regularities [Gesetzmäßigkeiten] in nature which, in the light of our present knowledge, are most certainly not *causal*, or at least not directly causal, but *directly statistical* regularities.” (Ibid., p. 11/144) What we observe on the macroscopic scale, which is of primary importance to human beings, already involves such a huge number of individual events that the statistical laws appear as strict regularities. Although this guarantees a practical value for the principle of causality, the inference to a causal behavior on the molecular scale is unwarranted. This becomes particularly clear in gas theory.

In the corresponding calculations and considerations we generally assume the validity of the mechanical laws for the single event, the collision. But this is *not* at all necessary. It would be quite sufficient to assume that at each individual collision an increase in mechanical energy and mechanical momentum is just *equally probable* as a decrease, so that taking the *average of a great many* collisions, these quantities remain constant in much the same way as two dice cubes, if thrown a million times, will yield the average 7 whereas the result of each single throw is purely a matter of chance. (Ibid., p. 10/138f.)

For gas theory, which represented our most immediate daily experience with molecular phenomena, Brownian motion was the crucial experiment to establish its statistical character. Radioactivity provided another example of a random process.

More than by however many examples, our conviction of the statistical character of physical laws is strengthened by the fact that the second law of thermodynamics, or law of entropy, *which plays a role in positively every real physical process,* has clearly proved to be the *prototype* of statistical law. (Ibid., p. 10/140f)

The reason for the universality of the second law is its intimate connection with the direction of time and the tendency towards more probable states.

Thus far Schrödinger closely followed the fourth chapter of Exner’s *Lectures* (1919, 1922) titled – almost as Schrödinger’s paper – “On Laws of Nature”. And he commended his teacher for the first philosophically precise enunciation of the idea “that the assertion of determinism was certainly *possible*, yet by no means *necessary*, and when more closely examined *not at all very probable*.” (Schrödinger, 1922a, p. 10/142f.) Schrödinger was aware that this conclusion was controversial. In order to argue that determinism is unnecessary, he repeated an example of Exner and Boltzmann. “Naturally we *can* explain the theorem of energy conservation on the large scale by its already holding true in the small [that is, for the single events]. But I do not see that we *must* do so.” (Ibid., p. 10/143) And even if we postulate energy and momentum conservation for the single events, we only obtain four equations and thus fall short of the pretended goal of complete determination of microscopic events.

As did Exner, Schrödinger considered the problem of causality as empirically open. “The *possibility* [that deterministic causality] may be in reality the case must be admitted, but this duplication of natural law so closely resembles the animistic duplication of natural *objects,* that I cannot regard it as at all tenable.” (Ibid., p. 11/ 145) In virtue of Occam’s razor, “*[t]he burden of proof falls on those who champion absolute causality, and not on those who question it*.” (Ibid., p. 11/ 147) But this was not the whole argument. Notice how Schrödinger depicted this duplicity of two realms.

On the one hand we would have the intrinsic, genuine, absolute laws of the infinitesimal domain; while on the other there would be that observed regularity in the finite domain which in its most essential features is *not* due to the existence of the genuine laws but is determined rather by the concept of *pure number*, the most translucent and simple creation of the human mind. Clear and definite intelligibility in the world of outer appearances, and behind this a dark, eternally unintelligible imperative, an enigmatic "Must". (Ibid., p. 11/144f.)

Like in Boltzmann, the concept of pure number was linked to the finitary human reasoning powers. And Schrödinger's idea that all natural happenings in the finite domain are based on this concept was echoing Exner's view that the law of large number was the most basic law of science and humanistics. The (possibly infinite) rest remained behind Schopenhauer's veil of the Maya; it was not contradictory as for Boltzmann, or downright meaningless as for Logical Empiricists.

Schrödinger even defended Exner's and Boltzmann's view that the law of gravitation was of statistical origin. The number of atoms involved in gravitational phenomena is much larger than for any other process in nature, so that deviations from the strict macroscopic laws would hardly ever become noticed. Having himself worked on general relativity, Schrödinger was aware that "Einstein's theory strongly suggests the *absolute validity of the theories of energy and momentum conservation.*" (Ibid., p. 11/146) To avoid a conflict between this finding of the last decade and the tendency of the preceding four decades, Schrödinger made a surprising move and considered relativity theory as virtually irrelevant for the issue of causality.

Applied to the mass point, these principles actually involve nothing more than a *tendency towards absolute perseverance* – for the whole theory of gravitation can be considered as the reduction of *gravitation* to the *law of inertia.* That *under certain conditions nothing changes* is surely the simplest law that can be conceived, and hardly falls within the concept of causal determination. It may after all be equally reconcilable with a strictly acausal view of nature. (Ibid., p. 11/146)

This was, to be sure, not a Machian argument. Recall the passage about Galileo's intuition of the law of inertial quoted in Sect. 3.1. Still in the *Mechanics* and after discussing how free inertial motion depends on the presence of other bodies in the universe, Mach concluded "that precisely the apparently simplest mechanical principles are of a very complicated character, that these principle are founded on uncompleted experiences, nay on experiences that can never be fully completed." (Mach, 1988, p. 259f./290) Since these principles were experiences, they consisted in functional dependences among determining conditions; hence they represented cases to which Mach's concept of causality applied. Schrödinger's argument is surprisingly weak, in particular, if one thinks of the importance of relativity theory within Schlick's (1920) first theory of causality.

Moore criticizes that "Schrödinger's treatment of conservation laws was surprisingly superficial" (1989, p. 154) because he neglected their deep roots in the fundamental symmetries of space and time. By assenting to Einstein, he moreover contradicted his own (premature) dismissal of the conservation laws. The problem of Moore's argument, however, is that he presupposes that Schrödinger assumed space-time symmetries in the usual sense as prior preconditions of all physical processes. Schrödinger still stood in the tradition of Boltzmann who had even advocated an atomistic concept of time. Admittedly, this view did not go well along with relativity theory. So even between the lines Schrödinger's inaugural lecture was a confession to the Viennese tradition. He concluded with the belief "that, once we have discarded our rooted predilection for absolute causality, we shall succeed in overcoming these difficulties, rather than expect atomic theory to substantiate the dogma of causality. (Schrödinger, 1922a, p. 11/147)

6.3.3. Indeterminism circa 1924

In 1924 Niels Bohr, Hendrik A. Kramers and John C. Slater proposed a new quantum theory in which energy conservation held only on average, but not for the individual atomic processes. Schrödinger was enthusiastic and immediately wrote to Bohr explicitly citing Exner's *Lectures* and his Zurich speech.

I have just read with the greatest interest the interesting change in your ideas in the May issue of the *Phil. Mag.* I am extraordinarily sympathetically touched by this change. As a pupil of the old Franz Exner, I have long ago become accustomed to the idea that the basis of our statistics is probably not microscopic "regularity", but perhaps "pure chance" and that perhaps even the laws of energy and momentum have only statistical validity. (Letter of 24 May, 1924, in Bohr, 1984, p. 490)

In the fall of 1924, Schrödinger published a survey article in *Die Naturwissenschaften* that emphasized the theory's philosophical significance and made abundant mention of Exner's name. The conception "that the individual molecular process is not causally determined by 'laws' in a unique fashion, [here] for the first time attains a tangible form." (Schrödinger, 1924a, p. 720) If this theory were true, the theorem of energy conservation would cease to be an exact law of nature and give way to the "Exner-Bohr conception" (Ibid., p. 724) according to which it is only of statistical validity. Schrödinger's reading of the new theory corresponded to the second of the two tasks which he had laid upon the atomist a decade ago. (See Sect. 6.3.1.) A confirmation of the BKS-theory would thus put it alongside Brownian motion as a demonstration of indeterminism within a certain domain of facts. It took only a year until the crucial experiment was realized: Geiger and Bothe showed that energy was conserved in each individual process.

After two pages of summary in which Schrödinger emphasized that a main goal of BKS was to dispense with the light quantum hypothesis, he provided some rough estimates to show that the new theory did not contradict present experiences, although the changes effected by the BKS on the individual processes were considerable and the energy fluctuations were even of the size of the differences of atomic energy levels. But the transitions between the levels occurred very rapidly and became macroscopically significant only at very high temperatures, so that it would be difficult to at all isolate the considered system from its environment.

At the end of the paper, Schrödinger argued that a merely statistical validity of the theorem of energy conservation would have "much deeper theoretical consequences than in the case of the entropy theorem." (Ibid., p. 724) While in the latter case the statistical theory for a closed system approaches the exact thermodynamic laws in the limit of infinite observation time, in the BKS theory – or if the Exner-Bohr-conception holds true – a closed system exhibits an average behavior only for relatively short times.

> In the limit t→∞ its behavior becomes *completely undetermined.* … We can reduce the deviation only by increasing the *size* of the system, or by considering it as a subsystem of a more extended system ("heat bath"). The *exact* validity of thermodynamics now could perhaps be maintained at most for a system in a heat bath, to wit, for the double limit t→∞ *and* heat bath→∞. But this double limit poses much bigger conceptual difficulties than the single one.
>
> One may also say: a certain stability of the happenings of the world *sub specie aeternitatis* can only occur through the *connection* of each individual system with the rest of the world. The separated individual systems would be, from the standpoint of unity, a chaos. It requires the connection as a permanent *regulator*, without which, energetically considered, it would wander about at random. – Is it an idle speculation, to find this a similarity to social, ethical and cultural phenomena. (Ibid., p. 724)

More than speculating about another instantiation of Mach's principle of holistic interdependence, the end of Schrödinger's paper pointed back to Exner's Inaugural Address (1909), which he never quoted in his writings. One may also refer to his vedantic views according to which there existed an interconnection between all human consciousnesses.

Whereas Schrödinger's article was unanimously positive about the BKS theory, his above-quoted letter to Bohr also continued with some criticism.

> Your new account to a large extent signifies a return to the classical theory, as far as radiation is concerned. I cannot completely go along with you when you keep calling this radiation 'virtual' … For what is the 'real' radiation if it is not that which 'causes' transitions, i.e., which creates the transition probabilities? Moreover, another sort of radiation is surely not assumed. Indeed, if one adopts a purely philosophical standpoint, one might even dare to doubt which electron system has a greater reality – the 'real one' which describes the stationary trajectories or the 'virtual one' that emits virtual radiation and scatters impinging virtual radiation. (Letter of 24 May, 1924, from Bohr, 1984, p. 490)

Interpreting this passage, scholar's have largely followed Wessels' view that "Schrödinger was enthusiastic about the assumption of irreducibly statistical processes, but objected to the authors' reluctance to give a coherent physical picture for the theory." (Wessels, 1977, p. 313) De Regt even concludes that "[p]recisely because his epistemological position amounts to Machian anti-realism, Schrödinger is in a position to object to calling some terms in the theory 'virtual'. If he had adhered to a hard-headed correspondence realism [and maintained that there could be any picture faithfully describing atomic reality], he would have dismissed the BKS-theory out of hand." (DeRegt, 1997, p. 473)

It appears to me that there are three intertwined aspects in Schrödinger's criticism. First, there is the difference in the meaning of causality observed by Ben-Menahem; in Bitbol's words, "according to Schrödinger the crucial criterion for calling a theoretical entity 'real' is its being ascribed the capability of 'causing' effects (be it in a restricted probabilistic sense), and *not its being energetically homogeneous with the effects it produces*." (Bitbol, 1996, p. 36) Second, Mach's causality as functional dependences involved a holistic stance, such that no entities were designated in advance as 'real' without their standing in causal relations. Darrigol even surmises that "Schrödinger would not have dared such a loose speculation in a scientific journal [as at the end of the BKS paper] … had not he been very eager to connect two of his main favorite themes, holism and acausality, and to do so in harmony with his philosophical and political convictions." (Darrigol, 1992, p. 268)

Third, complicated and merely statistical processes in space-time corresponded to Schrödinger's idea about theoretical pictures much better than did the quantum jumps.

Earlier in 1924, Schrödinger wrote a long letter to Hans Reichenbach with whom he had often met during his Stuttgart semester in 1920/21 (Cf. Moore, 1989, p. 133). It was a comment on a paper of Reichenbach (1932a) that was only published together with Schrödinger's letter in the third volume of *Erkenntnis*. As did Reichenbach, Schrödinger addressed the relationship of causality and the problem of induction.

> To be sure, the deep *problem* of causality seems to me the following: why do we expect the *completely* identical outcome under *completely* identical circumstances? – and not only after many but already at the first repetition. … I call this the riddle of inductive inference. I do not believe that it is solvable for us in its proper sense. If one ponders about it for a longer time … some sort of intellectual rotary vertigo sets in. (Schrödinger, 1932b, p. 65)

While Reichenbach believed at the time that he had basically solved the problem of inductive inference, to Schrödinger's mind, all such attempts ended up in tautologies, and Reichenbach thus had merely buried induction in the problem of probabilistic inference. "In actual fact one does not get beyond the fact *that* we constantly infer inductively, *that* we derive the greatest profit from it, *that* all our living is based upon it." (Ibid., p. 66) Apparently, Schrödinger was a true heir of Mach's anti-methodology. I cannot enter here into the details of his criticism of Reichenbach because this would require a more detailed study of a position which I have excluded from the present study because of the close connection between causality and induction; in Schrödinger's case, as we shall see, the riddle of induction remained almost unaffected by the causality debate.

Schrödinger subsequently addressed, as it were, the inverse problem as before and argued that in a chaotic world we would never have arrived at the concept of causality. Unlike Schlick (Sect. 7.1.) he defined 'chaotic' no other than as the simple negation of the principle of causality.

> [Accordingly,] this would be the same vicious circle as above. But perhaps this shows that our idea of causality has something to do with *realism*. Only because we consider our environment as something real which has a certain persistence, we are able to arrive at attributing to this real the *property* of being causally connected. Of course, behind the idea of some "relatively persistent real" precisely that is locked which had been intended originally: why experiences made can say something about experiences to be made; to wit, now: just because of this order property of the real which has to be thought of as persistent. You may learn from my inaugural address [1922a] which is enclosed that in actual fact I do not quite believe in this order property. (Schrödinger, 1932a, p. 66f.)

This was anything but a transcendental argument according to which realism and causality were a mutual precondition. Rather did it reflect the Machian heritage according to which both causality and realism were based on functional dependences. On this account, a fact – or something real – consisted in a sufficiently stable – or relatively persistent – complex of elements which, on their part, did not represent elementary substances independent of such relations.

Schrödinger drew an interesting consequence from Reichenbach's definition of causality by the convergence of a certain series, although he believed that most interpreters would consider it as useless. It shows that "the experience available today decides with a high probability *against* causality, probably *in all cases*. It is most valuable to me that by careful and impartial analysis you have been led to this conclusion which was certainly not your intention." (Ibid., p. 69) This was the place where Exner's name had to be dropped. And in the remainder of the letter Schrödinger contemplated how an Exnerian picture of nature, "as it will probably be the case in a few decades," (Ibid., p. 69) could look like. Certainly the elements of this world would reveal a certain persistence presumably based on the conservation laws. But the riddle of inductive inference would not disappear, and the problem of equal outcome under equal circumstances would just be shifted to the atomic level.

There one will have to assume – or so I imagine – laws of the kind that *sharply* defined conditions are related to a whole continuum of possible outcomes – or perhaps with certain persistence restrictions, to *all* possible outcomes. The "riddle" will have retreated to [the position] that by repeated preparation of sharply defined initial conditions ("circumstances") the distribution of the outcomes over this continuum will be a specific one, e.g., a uniform one. – To be sure, one cannot know whether this idea (which is obviously modeled after a game of chance) will turn out to be useful. However, at some point an axiom will slip in that is no less enigmatic than causality. For problems do not resolve themselves. (Ibid., p. 70)

This was quite a successful prediction about how quantum mechanical results would look like just two years later. It is quite surprising that Reichenbach's concluding remarks (1932b) did not appraise the remarkable continuity between what Schrödinger wrote in 1924 and the quantum mechanics as of 1932. After all, Reichenbach had read Exner's *Lectures*. He held, perhaps less surprisingly, that quantum mechanics fully confirmed his own conception of causality and emphasized that while quantum mechanics in principle blocked the classical extrapolation to strictly causal laws, in the kinetic theory of gases it was only practically unfeasible – see the footnote added to (Reichenbach, 1932a, p. 63). To be sure, this was said with the benefit of hindsight, but Schrödinger's position in 1924 was precisely that such a difference – even if it had been known at the time – was of minor importance for the empiricist's decision in favor of indeterminism. Evidently, it was difficult to get the basic message of Vienna Indeterminism across according to which classical causality had essentially terminated with the second law of thermodynamics. This does not contradict the fact that after the advent of quantum mechanics Schrödinger would himself stress the difference between unfeasibility in practice and in principle.

6.3.4. Alleged Counterevidence: The 1926 Letters to Wien

The *locus classicus* for claims that Schrödinger at least temporarily changed his mind in favor of causality, is a letter he sent to Willy Wien on 25 August 1926.[136] He apparently abrogated the main thrust of his 1922 inaugural address and by stressing the problem of the energy of individual processes, he implicitly referred to the BKS-paper.

I have the feeling – to express it quite generally – that we have not yet sufficiently understood the *identity* between *energy* and *frequency* in microscopic processes. … What we call the energy of an

[136] See (Forman, 1971, p. 104) discussed in Sect. 1.2.3. and (Forman/Raman, 1969, p. 301, fn. 36) discussed in Section 6.2.1. I have used the letters and carbon copies in the Nachlaß Schrödinger, Zentralbibliothek für Physik, Wien.

individual electron is its frequency. The electron does not move with a certain speed for *the* reason that it has received a certain "shove", but because a dispersion law holds for the waves of which it consists, as a consequence of which a wave packet of *this* frequency has exactly *this* speed of propagation. What we call the energy content of a stream of electrons depends for a given frequency only upon the number of electrons and this determines how often those events that are permitted by the frequency occur in the electron stream.

But today I no longer like to assume with Born that an individual event of this kind is "absolutely random", i.e., completely undetermined. I no longer believe today that there is much to be gained from this conception (which I championed so enthusiastically four years ago). From a galley print of Born's last work in the *Zeitsch.f.Phys.* I know more or less how he thinks about this: the *waves* must be strictly causally determined through field laws, the *wavefunctions* on the other hand have only the meaning of probabilities for the *actual* motion of light- or material-particles. I believe that Born thereby overlooks that – granted that this picture be already worked through completely – it would still depend on the taste of the observer *which* he now wishes to regard as *real*, the particle or the guiding field. There exists really no philosophical criterion for reality [Realität] if one does not want to say: the *real* is only the complex of sense impressions [der sinnenfällige Komplex], all the rest are only pictures.

Bohr's standpoint that a spatio-temporal description is impossible I reject in point of principle [*a limine*]. Physics does not only consist of atomic research, science not only of physics, and life not only of science. The purpose of atomic research is to integrate our respective *experiences* into the rest of our thinking. And all our other thinking, as far as the external world is concerned, moves in space and time. (And above all: The *experiments* to be explained are entirely embedded into space and time.)[137] If it cannot be embedded into space and time, then it fails in its whole purpose, and one does not know *which* purpose it serves at all.

At face value, matters seem to stand clear. Schrödinger rejected indeterminism and Born's positivism, and advocated a spatio-temporal description instead. Such a reading would signify a complete rupture with Schrödinger's acceptance of indeterminism as a viable option before and also after 1926. Moreover, Bohr's finality claim was rejected by a classical move of Mach against Planck's exclusive focus on a physical world view (Sect. 3.9.); there has to exist a certain continuity with the facts of our macroscopic life world. Schrödinger does not charge Born of anti-realist metaphysics, because there simply is no other philosophical criterion of reality than Mach's. Nonetheless, Born's way of return to a pure Machian ontology happened in such a way that the theoretical pictures became entirely detached from any possible realities in space and time. Or in more historical terms, Born's positivism on the basis of a still classical particle ontology endangered the subtle equilibrium between the teachings of Mach and Boltzmann which Schrödinger had imbibed at the Vienna Institute of Physics and elaborated into a joint advocacy of continuous pictures and indeterminism.

Yemina Ben-Menahem was the first to give an interpretation of this kind by restoring continuity with Schrödinger's (1924a) earlier work. The important point is to recognize that "[c]ausality and continuity were independent for him." (Ben-Menahem, 1989, p. 321) Schrödinger favored the BKS theory because microprocesses were acausal and continuous. This was the content of an earlier letter to Wien written on 18 June 1926.

I cannot convey to you what extraordinary joy it has given me that personalities such as you and Geheimrat Planck share my confidence in the way I took. It appears, to be sure, that at present not all parties are convinced that the renunciation of the basic discontinuities, *if* possible, is to be absolutely

[137] This sentence is a handwritten addition to Schrödinger's own carbon copy. From the translation in Moore, 1989, p. 226 which is based on the letter in the Wien Archive, Deutsches Museum München, I conclude that it was a later addition of Schrödinger's when he filed the carbon copy.

welcomed. But I have always wholeheartedly wished that it would be possible, and would have seized the opportunity with both hands – as I did with Bohr-Kramers-Slater – even if chance had not played the first tag right into my own hands (or in regard to de Broglie I should better say: the second).

Thus, so Ben-Menahem concludes, "Schrödinger himself regarded his earlier response to the Bohr-Kramers-Slater paper as fully consistent with the views he held in 1926 when working on wave mechanics. It is therefore clear that the allegation that a radical change occurred in Schrödinger's scientific outlook between 1924 and 1926 is at odds with Schrödinger's own understanding of his position." (Ibid., p. 322) But then what was the point of the August letter? "It is the claim that there is 'not much to be gained' by a probabilistic interpretation that seems odd. But if I am right about Schrödinger's conception, this phrase makes perfect sense. In the BKS paper causality was renounced but continuity rescued. In Born's case, however, there was no such pay-off." (Ibid., p. 326) Darrigol's interpretation is similar: "one theory [wave mechanics] offered a fairly detailed space-time *picture* of radiation processes, despite the quantum jumping, while the other [matrix mechanics] explicitly denied the possibility of representing quantum processes in space and time. What Schrödinger could not accept was the mutual destruction of the claims of causality and visualizability." (Darrigol, 1992, p. 268) In one of Schrödinger's papers of 1926, Bitbol (1996, p. 17) rightly finds the same motive of lack of compensation at work against Born's probabilistic interpretation of wave mechanics.

As far as I can see, one can thus never arrive at a uni-directional (irreversible) process without an additional hypothesis about the relative probability of the various possible initial distributions of the probability amplitudes. I am flinching from this conception [Begriffsbildung], not so much on account of its complexity as on account of the fact that a theory which postulates an absolute primary probability as a law of nature should at least repay us by freeing us from the old 'ergodic difficulties' and establishing us to understand the unidirectionality of natural processes without further supplementary assumptions. (Schrödinger, 1984, p. 279)

If one adopts a fully probabilistic approach it should at least eliminate the ergodic hypothesis which arises from the combination of a deterministic theory of the microphenomena and a statistical theory at the macroscopic level. Dispensing with the notorious ergodic hypothesis was precisely the reason why Richard von Mises had elaborated an entirely probabilistic theory of Brownian motion. As we shall see in Sect. 8.2., to him, this success was a case in point for indeterminism.

In an even earlier letter to Wien of 19 March, 1926, which accompanied the submission of his paper establishing the equivalence of matrix mechanics and wave mechanics, Schrödinger emphasized the virtues of his own theory because "[p]hysically, … [it] seems considerably more satisfactory and capable of extension. There is yet another circumstance a posteriori in favor of the superiority of the standpoint of undulatory mechanics and the greater clarity which it promises also in a purely mathematical perspective. From this point of view I succeeded in unveiling the connection [between both approaches] while Weyl, with whom I cannot compare myself in the least as regards mathematical knowledge and capabilities, did not." This indicates that Schrödinger's *Bild*-realism involved the pragmatic criteria of simplicity and fertility.

The intention to reach a positive trade-off between causality and realist pictures in space-time can also be seen at the back of Schrödinger's "simultaneous rejection of hidden variable theories and of the epistemological interpretation of quantum mechanics." (Bitbol, 1996, p. 87) While Born's renunciation of causality did not lead to a coherent ontology, hidden variable theories, to the late Schrödinger's mind, amounted to a "belief in destination." (Schrödinger, 1995, p. 78) One gained apparent causality at the price of ontologically dubious additional entities, such as Bohm's quantum potential. They correspond to completing in thought the observable entities by experimentally inaccessible elements. Bitbol emphasizes Schrödinger's strictures for devising a new *Bild*-ontology.

In so far as the (proper) wave-functions can be constructed from given matrices, just as the matrices can be constructed from the (proper) wave functions, one cannot contend any longer that wave mechanics is an *ad hoc* device for creating pictures. This very lucid requirement of reciprocal equivalence shows that, unlike the later proponents of hidden variable theories, Schrödinger did not consider it satisfactory to an empirically void "clothing" to the structure of quantum mechanics just for recovering the classical ontology of for satisfying the desire for pictures. What he wished to demonstrate was rather that there exists an adequate picture and an (non-classical) ontology which arises quite naturally from unmodified quantum mechanics itself. (Bitbol, 1996, p. 68)

This argument basically rehearses Schrödinger's positivist reality criterion that had motivated his criticism of Bohr's distinction between 'real' and 'virtual' radiation. Schrödinger was a staunch advocate of ontological parsimony, in 1922 when calling the quest for determinism an animist duplication of the world, and after 1926 when he rejected a combined ontology of waves and particles in favor of a pure wave picture. Ontological parsimony also meant that an appropriate picture should not rely upon introducing explicit limits of what is knowable or speakable. (Cf. the letter to Bohr of May 1928 cited in Section 6.2) Once limits were drawn, completeness could not be positivistically rescued. And thus Bitbol rightly diagnoses a "latent complicity between Heisenberg's positivist-like statements and Einstein's program of replacing quantum theory by a complete theory of the behaviour of individual objects." (Bitbol, 1996, p. 85)

6.3.5. Continuing the Debate with Planck

In 1928, Max Planck published an emphatic review of Schrödinger's collected papers on wave mechanics (1927). The author left no doubt that he preferred wave mechanics to matrix mechanics, their formal equivalence notwithstanding.

[Schrödinger's] discovery consists, shortly speaking, in having established a deep-rooted connection of quantum mechanics with classical mechanics, a connection from which follows that both these theories are related much closer to one another than one had been inclined to believe after the latest developments of research. (Planck, 1928, p. 59)

And Planck even suggested that one arrived at Schrödinger's equation "by a certain, to some degree obvious generalization of the equations of classical mechanics." (Ibid., p. 59) But what Planck cherished the most, was of course motivated by his own futile attempts to integrate quantum theory in some way or other into classical physics. "It is the first time that the quantum of action which hitherto has strongly demurred all attempts to be comprehended from the standpoint of continuum physics, has been captured into a differential equation.” (Ibid., p. 60) Indeed, when measured against Schrödinger’s (1914) own program to corroborate the atomistic hypothesis by investigating conflicting scenarios following from differential equations and difference equations, Boltzmann’s mathematical atomism had – quite ironically – failed in its proper domain, the physics of atoms. Quantum jumps, the irreducible discontinuity in microphysics, could thus be circumvented. Of course, Planck was aware of the interpretational problems and he even commended Schrödinger for honestly mentioning all the difficulties of his approach.

Summing up, one must say that the new wave mechanics is just at the beginning of its development. In particular the physical nature of the wave function is still in great need of clarification. That this quantity cannot be intuitive [anschaulich] in the usual sense but only possesses an indirect, symbolic meaning is certain if only because matter waves do not occur in physical space but in the so-called configuration space. (Planck, 1928, p. 61)

Planck, reasonably, remained skeptical about interpreting the wave function by way of the electrical charge density. To be sure, Schrödinger would not have admitted that the wave functions were unintuitive just because they were living in configuration space.

Schrödinger did not quite accept Planck’s reading according to which he brought quantum physics back to the well-entrenched domain of classical physics. Rather than emphasizing the continuity with classical mechanics, his 1929 inauguration as a member of the Prussian Academy of Sciences continued the dialogue between Exner and Planck. Starting his short speech with the reminiscences of Vienna quoted in Section 6.1., Schrödinger asserted that the goal of theoretical physics was to subsume the manifold of phenomena under a few simple laws. Classical mechanics, which had once ideally fulfilled this goal, now only represented a first approximation.

One of the most burning questions is … whether together with classical mechanics also its method must be abandoned, the maxim that fixed laws together with random initial conditions uniquely determine the happenings in each individual case. It is the question about the purposivity [Zweckmäßigkeit] of the unswerving postulate of causality. It is true, in practice we had had to forgo causality already within the classical mechanical explanation of nature. (Schrödinger, 1929a, p. 732)

And Schrödinger recalled the inaugural address of his beloved teacher Hasenöhrl “who laid the foundation of my scientific personality.” (Ibid., p. 732) The example was due to Boltzmann and we have already (See Sect. 4.4.) encountered it in Exner’s *Lectures*.

It would not contradict the laws of nature, Hasenöhrl explained to us, if this piece of wood should lift itself into the air without any ostensible cause. According to the mechanical explanation of nature such a miracle, being a reversion of the opposite process, would not be impossible but only extremely improbable. – Yet the probabilistic conception of the laws of nature, which Hasenöhrl had in his mind when using these words, by itself does not really contradict the causal postulate. Uncertainty in this case arises only from the practical impossibility of determining the initial state of a body composed of billions of atoms. Today, however, the doubt as to whether the processes of nature are uniquely determined is of quite a different character. The difficulty of ascertaining the initial state is supposed to be not one of practice but of principle. (Ibid., p. 732/xvi)

Compare this distinction between “in practice” and “in principle” with Schrödinger’s (1922a) insistence that Brownian motion and radioactivity stood on a par as empirical demonstrations of the indeterministic character of a certain domain of phenomena. Instead of a few decades it had taken no more than a few years until the "Exnerian picture of nature" won favor – though in a way that broke the continuity with Boltzmann's kinetic theory of gases and did no longer depend upon the universality of the second law of thermodynamics.

Franz Exner (to whom I am personally indebted for his unusually great encouragement) was the first to mention the possibility and the advantages of an acausal conception of nature. … But I do not believe that in this form [this fundamental question] will ever be answered. In my opinion this question does not involve a decision as to what the real constitution of nature is, but rather as to whether the one or the other predisposition of mind be the more purposive and convenient one with which to approach nature. Henri Poincaré has illustrated that we are free to apply Euclidean or any kind of non-Euclidean geometry we like to real space, without having to fear the contradiction of facts. But the physical laws we discover are a function of the geometry which we apply, and it may be that the one geometry entails complicated laws, the other much simpler ones. In that case the former geometry is inconvenient, the latter is convenient, but the words "right" or "wrong" are unsuitable. The same probably applies to the postulate of rigid causality. One can hardly imagine empirical facts which ultimately decide on whether the natural phenomena are in reality absolutely determined or partially indetermined, but at best on whether the one or the other conception permits a simpler survey of what is observed. Even this question will probably take a long time to decide; for the question of world geometry has been rendered the more doubtful by Poincaré's having made us aware of the fact that we have the liberty of choice. (p. 732/xvii f.)

Schrödinger's (1924) optimism that a decision in favor of indeterminism was just a matter of decades has faded away although the Exnerian picture of nature had been established so rapidly. Admittedly, also Exner had remained open with respect to the alternative between determinism and indeterminism although he had preferred the former in virtue of manifold supportive evidence and because of its more unified character. Methodological purity was, of course, a pragmatic motive that could be subsumed under the heading "simplicity" as his application of Occam's razor in 1922. But Schrödinger's own works, particularly his proof of the equivalence between wave mechanics and matrix mechanics, substantially changed the nature of the alternative. There was, on the one hand, a beautifully deterministic differential equation the application or interpretation of which permitted only statistical predictions. There was, on the other hand, an abstract and openly indeterministic theory which nonetheless integrated the whole conceptual apparatus of classical mechanics in a quantized form. So ultimately, determinism and indeterminism were intermingled. What Schrödinger established with his equivalence proof corresponded to the systematic classification of all possible geometries achieved at the end of the 19th century which had constituted the basis of Poincaré's conventionalism. In contrast to a Machian view which took all theoretical descriptions just a mere economizations, conventionalist choice required a precise formal characterization of the alternatives. This might be the reason why Schrödinger mentioned the conventionalist thesis only in 1929 although there exists an approving passage in notes written in 1918 and titled "Kausalität". "There he quoted Poincaré's statement about principles: 'They are neither true nor wrong, they are expedient [commodes]' and he commented: 'This is certainly entirely true of the causality principle." (Darrigol, 1992, p. 264) It is quite interesting that Schrödinger thus arrived at a conventionalist position from considering the issue of causality as of an empirical nature. But this did not contradict the fact that on other occasions (See

Sect. 6.3.6. and 7.4.) he would stress that indeterminism was more likely or more probable.

In his response, Planck tried to drag the wavering Schrödinger onto the grounds of strict causality. Associating the possible dismissal of causality with "the present crisis of theoretical physics", Planck praised Schrödinger's "benevolent neutrality" and intended "to break a lance for a strictly causal physics even at the risk of appearing as a narrow-minded reactionary." (Planck, 1929, p. 732) Planck admitted that the question of causality was ultimately an issue of purposiveness [Zweckmäßigkeit] because any physical theory represented just a skeleton [Gerüst] devised in order to obtain a picture of nature as faithful as possible. But he was far cry off from interpreting purposiveness in a conventionalist fashion.

[T]he skeleton by all means needs a solid foundation if it is not to stand in the air; and if the postulate of rigid causality were no longer as before a suitable foundation then first of all the counterquestion suggests itself as to what foundation should then be introduced for the "acausal" physics. For no physical theory can be developed without any presupposition, unless one wants to pass off the mere registration of single observational facts as a theory. (Ibid., p. 733)

This transcendental skeleton was the principle of causality, at least until there was "any compelling reason that causal physics does not suffice to do justice to the empirical facts." (Ibid., p. 733) Since the days when he had subscribed to Boltzmann's statistical mechanics, Planck read this theory in a way consistent with the principle of causality and a universal determinism at the deepest level of reality. This also motivated his interpretation of Hasenöhrl's example.

From the standpoint of classical physics [this strange behavior] is not only not impossible, but within a sufficiently long period it has to be expected with a certain probability, and precisely the quantitative confirmation of such fluctuations by experiment, in my view, represents an excellent support in favor of the postulate of strict causality by means of which it has after all been derived. (Ibid., p. 733)

Certainly, no Vienna Indeterminist would have denied that this inference from a deterministic microdynamics was possible but, as Schrödinger had argued in 1922, it was by no means necessary. Thus, Planck's argument did not represent a confirmation of the principle of causality but merely showed that strict microcausality was consistent with the statistical macrodynamics. The really decisive evidences for Vienna Indeterminism had been the microscopic fluctuations observable in Brownian motion and the universal nature of the second law of thermodynamics.

It is interesting that by 1929 Planck had not only made his peace with the occurrence of strange events, such as Hasenöhrl's piece of wood, but in a complete turnabout accounted them to the principle of causality. Until 1914 he had insisted that their extremely low probability be turned into impossibility by the introduction of the principle of elementary disorder (Sect. 3.7 & 4.5) This principle represented a lawlike condition on the admissible initial states of a system. And again in 1929 Planck adopted a similar strategy to rescue the principle of causality in the field of quantum mechanics. It consisted in modifying the concept of initial state as inherited from classical mechanics.

[T]here is indeed one aspect in physics as hitherto which needs revision, and it is presumably this aspect which has caused all doubts about the reliability of the law of causality. We must henceforth drop the presupposition implicitly made to date, that we are able to experimentally realize those conditions that causally determine a process to an in principle infinite degree of precision. This presupposition is irreconcilable with the laws of quantum mechanics. … Accordingly, also in the physics of the future it will be important to keep apart in point of principle the question of the conditions which uniquely and causally determine the course of a natural process from the further question whether and to what extent these conditions can be experimentally realized. (Planck, 1929, p. 733)

For the empiricist however this distinction was an artificial one and only motivated by an a priori preference for the principle of causality. As long as the empiricist remained in principle open to a future causal theory – although he deemed it unlikely – rejecting Planck did not require an empiricist criterion of meaning in the strict sense.

Planck ended with what he took as the strongest argument in favor of a causal quantum theory. It was Schrödinger's own wave mechanics within which atomic processes remained deterministic "yet under the assumption that one considers matter waves as their elements instead of motions of mass points." (Ibid., p. 733) Of course, Planck was aware that the physical meaning of these matter waves was still open. The shift from mass points to matter waves – even if it did not do the job – was significant in a philosophical perspective. When changing the basic ontology to maintain strict causality and admitting that at least in principle the law of causality could be falsified, Planck implicitly accepted the core tenet of Vienna Indeterminism, the intimate relation of causality and ontology.

6.3.6. Indeterminism circa 1930

In 1932, Schrödinger assembled two lectures into a small booklet which was dedicated to the memory of Franz Exner. The Viennese heritage was most obvious for the first lecture "On Indeterminism in Physics" which had been delivered to the "Society for Philosophical Education" on 16 June, 1931. Without entering into a debate with his philosophical audience whether physicists' most recent discussions about causality actually corresponded to certain lines of the philosophical tradition, Schrödinger considered it as the main question whether "it is possible to precisely predict, at least theoretically, the *future* behavior for any given physical system if one knows its constitution and its state at one *instant of time*?" (Schrödinger, 1932a, p. 1/53) Schrödinger here for the first time provided an explicit philosophical definition of causality. Before he had followed the tradition of Exner closely associating causality with the determinism peculiar to classical mechanics and electrodynamics. In his unpublished letter to Reichenbach, Schrödinger had stated quite generally that we are expecting identical outcomes under identical circumstances. As we shall see in Sect. 7.3., Schrödinger's definition was identical to Schlick's (1931) new theory of causality published a couple of months before. Schrödinger's speech was remarkable in a second respect. Although he had always made clear his adherence to the Boltzmannian tradition of considering theories as pictures, the word "picture" never had occurred so frequently in his earlier writings.

While 1½ decades ago, so Schrödinger argued, nobody doubted the dogma of determinism, now many physicists believed that the repeated failures to understand the experimental results of the preceding 3 decades by means of deterministic pictures had lead to a dismissal of determinism in the sense of classical mechanics. To his mind, such a repeated failure could not be decisive all by itself. Although the conventionalist thesis of the 1929 inaugural address had again disappeared, Schrödinger remained open to the possibility of a causal quantum theory.

It will be difficult to ever *prove* that no determined [bestimmtes] picture can be found which equally does justice to the facts. But what makes this modern attempts to abandon determinism nonetheless very interesting is that their declarations of a lack of determination are not at all *vague and undetermined*, but entirely precise, quantitative, expressible in cm, g, sec. (Schrödinger, 1932a, p. 3/55)

And he tried to give an intuitive explanation of Heisenberg's uncertainty relation according to which it is impossible to jointly know the initial position and velocity of a mass point more precisely than a certain limit given by a universal constant of nature, Planck's constant. The idea to imagine, at least in principle, that observations could be made arbitrarily precise, had to be abandoned. According to Schrödinger, this was the most intuitive among all the statements of indeterminacy. "A comprehensive and definitive judgment about these matters *does not at all exist at the present moment.*" (Ibid., p. 7/59) Thus Schrödinger just added three "in part loosely connected" (Ibid., p. 6/59) remarks in which he nonetheless defended quite specific theses.

Surprisingly, the first of them harked back to Boltzmann's mathematical atomism. If we understand lack of determinacy as the fact that identical initial conditions only lead to an identical outcome *statistics*, the same holds true already in classical mechanics. "If one generally claims the opposite, this rests upon a knack [Kunstgriff] which we have got accustomed to for a long time, so that we regard it as self-evident." (Ibid., p. 7/60) As mechanical motions are determined by the accelerations, we got used to count the initial velocity among the initial conditions. But this is, strictly speaking, not correct because the definition of velocity by means of a differential quotient involves two moments in time of which one imagines that they can be made coincide in the limit.

[P]erhaps this mathematical limit … is inadmissible. Perhaps the thought machinery [Denkapparat] invented by Newton is *not sufficiently adapted to nature*. The modern claim, that for sharply defined position in space the concept of velocity becomes meaningless points strongly in that direction. (Ibid., p. 9/62)

Notice that this was said by the single scientist who, as Planck emphasized, had re-integrated atomic physics into the physics of differential equations. But rather than succumbing whole-heartedly to the Göttingen-Copenhagen quantum jumps, Schrödinger precisely rehearsed his 1914 concern (and Boltzmann's teaching) that the issue between determinism and indeterminism had to be decided by the more adequate mathematical description. Moreover, the "knack" and the "thought machinery" were nothing but Boltzmannian "habits of thought" which – as the principle of causality itself – had an inherent tendency to be applied also within new domains to which they were badly adapted. With all this historical baggage, Schrödinger was too modest when calling his first remark merely a benefit from hindsight. In particular because the same argument was turned against the Copenhagen extension of classical ontology, as we shall see in the third remark below.

Also the second remark pointed back at the days of Boltzmann. For more than half a century we know, Schrödinger declared, that a large number of natural laws are statistical. Since all those laws which refer to irreversible phenomena are necessarily statistical, they represent "the overwhelming majority because the course of nature is essentially irreversible, *one-sided*." (Schrödinger, 1932a, p. 11/64) "[S]trictly speaking all [laws are statistical] except for gravitation and perhaps (?) also this." (Ibid., p. 12/66)

All the "predictions" derived from these laws … only apply with very small deviations or errors that can be estimated with complete accuracy. Is this not strikingly similar to what I have talked about at the beginning? Why did one make so – relatively – little ado about this? Why did nobody say already 40 or 50 years ago that modern physics (modern as it was then) was compelled to abandon causality or determination, etc., why just 5 or 6 years ago?

Well this much is clear. *Then* the abandonment of determinacy was merely of a *practical* kind, *today* one assumes that it is theoretical. … Thus one continued to imagine a strictly causally determined happening in the realm of the single atoms and molecules, so to speak, as background or basis of the statistical mass laws only which, in actual fact, are accessible to experience. (Ibid., p. 12f./66-68)

This set the stage for the Planck-Exner debate which Schrödinger recapitulated without mentioning Exner's erstwhile combatant. But the opponent depicted in the following passage clearly was the Planck of 1914 who had rejected examples such as Hasenöhrl's because they did not occur in "our given nature".

Most physicists considered a strictly deterministic foundation of the physical world as indispensable. They believed that it was impossible to think otherwise … It was said, and sometimes it is said still today, that an exact science of nature would be entirely impossible on any other basis, that everything would get into flux, that without a strictly deterministic background our *picture* of nature would degenerate into a complete chaos and thus would not fit to our *given* nature because nature is in fact not completely chaotic. This is certainly *not* correct. (Ibid., p. 14/68)

Without contradicting our experience – so Schrödinger rehearsed the argument of his Zurich speech (1922a) – the deterministic laws of impact assumed at the molecular level could simply be replaced by an appropriate game of chance determining the further path of a molecule as long as the relevant conservation laws, e.g., for linear momentum, remained valid on average. The only new element in Schrödinger's as compared to 1922 reasoning was that the theorem of energy conservation was strictly valid for the single molecules rather than being treated as the most likely candidate to follow the second law of thermodynamics into the domain of merely statistical regularities. Energy conservation for the single processes had been established by the experiments of Bothe and Geiger, and also the new quantum mechanics assumed it strictly. "But these laws of balance [Bilanzgesetze] do not uniquely determine the outcome of the impact. Beyond this 'primary' chance could rule." (Schrödinger, 1932a, p. 14f./69) And Schrödinger returned to the days of Exner.

Back then it was simply a question of taste, or … a question of *philosophical prejudice*, whether one decided in favor of determinism or indeterminism. The age-old *habit* (or perhaps an 'a priori') spoke for determinism. In favor of indeterminism one could advance that this *habit* was evidently based upon the *factually* lawful course of nature which we observe in our surroundings. Once recognized that *almost all* these regularities – and perhaps really all – are of a *statistical* character, then they no longer provide a *rational* argument for the retention of determinism. (Ibid., p. 15/70)

There was enough – but not rigorously compelling – reason to doubt determinism even before Heisenberg's uncertainty relations. And Schrödinger recalled that his Zurich inaugural address had caused a general shaking of heads – enough reason, so it seems, to withhold its publication until 1929.

In his third remark, Schrödinger criticized the ontology of the Göttingen-Copenhagen picture by claiming "that the concepts 'position', 'trajectory' [Bahn, Bahnkurve] are exaggerated when applied to such small [atomic] spatial and temporal dimensions" (Ibid., p. 22/77) – they 'overshoot the mark', Boltzmann would have said. Schrödinger's starting point was "that the thing about which quantum mechanics speaks [in Heisenberg's indeterminacy relation] is *no longer* a 'material point' in the old sense of the word." (Ibid., p. 16/71) For such a point is situated at a precise position independently of whether this is measured or not. And a trajectory is constituted by a material point having a precise position at each time.

To speak of electrons and protons as material points but to deny nevertheless that they have definite trajectories seems to be contradictory and rather crazy. Again it should not be denied or passed over in tactful silence, as it is done in certain quarters, that in this way [when applying it to atomic dimensions] the concept material point undergoes a substantial, yet still poorly understood change. But from atomistics one can quite well understand, or at least conjecture, that the concept of *trajectory* is lost at very small dimensions. (Ibid., p 17/72)

At this point Schrödinger turned Boltzmann's atomism against the Göttingen-Copenhagen picture according to which material particles are the basis of quantum mechanical ontology without having well-defined trajectories. If we depart from how we actually observe natural phenomena, it seems to be clear that "[e]very quantitative, measuring observation is discontinuous by its very nature" (Ibid., p. 17/72) because it ultimately represents nature's answer to a finite number of yes-no question. We complete this finite raw material by interpolation and in this way arrive at a continuous trajectory, which in itself is not directly observable. This procedure, however, is admissible only if all such measurements could in principle be performed by really existing apparatus. To be sure, "we continuously have to complete what is directly observed; otherwise there would be no picture of nature but only an inextricable patchwork of individual findings [Einzelfeststellungen]." (Ibid., p. 21/76f.) By inferring from a finite set of observations to a continuum in this way, we run the risk to erroneously complete our factual observations and "mess up our picture of nature" (Ibid., p. 21/77) by employing a concept, such as 'trajectory', outside its domain of validity.

Already Boltzmann had criticized the concept of a continuous trajectory within Newtonian mechanics as a source of contradictions. (See Sect. 3.4.) In this perspective, Schrödinger argued that even after having discovered a new suitable atomic theory – which could resolve the problem of Vega in the classical limit – the inference to the continuum remained problematic in point of principle. Moreover, Bitbol cites a letter to Margenau of 12 April, 1955, in which Schrödinger himself established the historical link with Boltzmann. "The discontinuity removes the univocal identification. Would you believe it, that Boltzmann, in his *Principe der Mechanik*, right in the beginning, underlines this point in what he calls *Erstes kinematisches Grundgesetz.* This was a few years before Planck's great discovery."

(Bitbol, 1996, p. 96) Here is what Boltzmann introduced as the first basic assumption, not as a ‘law’ as Schrödinger wrote.

Let us now further develop our picture [of the motion of material bodies] by assuming certain fictitious laws for the way these material points change place with time. First assumption: we imagine that no two different material points coincide or are infinitely near to each other at any time, but that whenever at any time any material point is at any position … then also one and only one material point will be at an infinitely near position at any infinitely near time. We say the second material point is the same as the first and call this the law of continuity of motion. It alone enables us to recognize the same material point at different times. The concept [Inbegriff] of all places at which one and the same material point, and the concept of these positions that it traverses in a finite time is called the path during this time. We may alternatively formulate the law of continuity as such: to every material point which at a given time had certain coordinates, there corresponds at an infinitely near time one and only one material point with coordinates differing from the former by infinitely little, and this is called the same material point; that is, the coordinates of every material point are continuous functions of time. (Boltzmann, 1897, p. 9/230f.)

Bitbol considers completion in thought by virtual elements as a core feature of Schrödinger’s ontology (See Sect. 6.2). And indeed Schrödinger did not limit this argument to the concept of trajectory, but asserted quite generally that some of the unavoidable completions “concern what is *in principle* unobservable,” (Schrödinger, 1932a, p. 21/77) for example, the simultaneous existence of both facades of the Brandenburg gate. Yet all this could easily be easily reconciled with Mach’s account how knowledge of facts is gained and how errors are simultaneously produced by way of our mutual completion of different actual and non-actual sensations.

Most part of the third remark of 1931 can already be found in a lecture on “The Change of the Physical Concept of the World” which Schrödinger had delivered at Munich in May 1930, but which remained unpublished until 1962. Interestingly, Schrödinger then spoke of “a series of single statements [Konstatierungen]” (Schrödinger, 1930, p. 602) instead of “findings” [Feststellungen]. This had been Schlick’s terminology, e.g., in (Schlick, 1920, p. 464).

He continued the remark about our acquired habit of interpolation and the origin of continuous trajectories with a criticism of the Göttingen-Copenhagen quantum jumps.

Many interpret the failure of the deterministic picture of nature in such a way that in the course of nature there is in actual fact something discontinuous, jumpy, that instead of the old sentence: *natura non facit saltum* the direct opposite were true: *natura facit nil nisi saltus* … – But one should really be very careful with this interpretation. (Ibid., p. 606)

The reason for such caution was, of course, the discontinuous nature of our observations. But Schrödinger gave his argument a twist that would be absent in the following year. It led back to Boltzmann’s constructivist atomism.

Due to a certain finite, limited constitution of our mind [Geist], we are totally incapable to pose nature a question which admits a continuous sequence of answers. The observations, the single measurement results, are nature’s answer to our discontinuous questions. Thus they concern perhaps in a most essential way not the *object* alone, but rather the interrelation between subject and object. For the philosopher this is a truism, but perhaps it now attains again an increased significance. (Ibid., p. 607)

More specifically, both electromagnetic waves and matter waves do not represent an objective description of reality, the do not describe "nature in itself, but the *knowledge* we possess of her through our observations actually performed." (Ibid., p. 607) As these observations disturb one another in virtue of Heisenberg's uncertainty relation, we can only reach statistical predictions about future observations. We have to renounce the goal of an objective description of nature, even as an asymptotic ideal in the sense of Planck. To many, Schrödinger admitted, this seems to be "a painful reduction of their claims to truth and clarity. … But is this relation [between subject and object] not at bottom the only true reality which we know of? Does it not suffice when *it* finds a fixed, clear and completely unambiguous expression, for which indeed there is all hope!" (Ibid., p. 608)

In these passages, Schrödinger considered the picture of nature as provided by quantum mechanics as more coherent than ever before. In all other papers investigated so far, he avoided the Copenhagen talk about subject and object. Large part of what he said in the city of Sommerfeld, the teacher of Heisenberg and Pauli, might thus be read as a conversion. But it need not. Taking the relationship of subject and object as the ontological basis of science could also be seen as an instantiation of Mach's neutral monism, though with some terminological concessions to his Copenhagen-oriented audience; a staunch Machian would have avoided the metaphysical terms 'subject' and 'object' altogether. Schrödinger in particular did not defend Heisenberg's anti-Machian version of positivism that involved a conscious subject endowed with spontaneity and a prior restriction to observable entities.

According to Bitbol, the Munich speech documented that "in the process of ontological deconstruction … [Schrödinger] had landed on the surface of the bare subject-object relatedness." (Bitbol, 1996, p. 80) This surface was the dwelling of the Machian elements. But what about the Boltzmannian pictures? "The pictures have not been lost as such, according to Schrödinger, but our conception of what they represent, has changed." (Ibid., p. 79) The wave functions only describe our knowledge and "permit us to predict the results of *future* observations … with precisely that degree of uncertainty and mere probability prejudged by the observations actually performed at the respective object." (Ibid., p. 607f.) And Schrödinger even assented to Heisenberg's idea of disturbance. Yet where is this knowledge dwelling? Not in the subject itself – there was no mind to reduce wave packets – but only in the interrelation between subject and object. This was as close as Schrödinger could get from Mach to Copenhagen. But accepting the Copenhagen reading of his wave function became primarily an incitement to search for a better *Bild*-ontology, in particular, since the criticism against the very concept of trajectory figuring in the Copenhagen interpretation was another centerpiece of the same paper.

In an earlier talk, "Conceptual Models in Physics and Their Philosophical Value", delivered in December 1928, we can diagnose already a sort of therapeutic assent to Heisenberg's uncertainty relation.

This idea, which originated with Heisenberg, is satisfactory in a way, since it consoles us for the failed attempts we had made to attribute the predicate of real existence [Wirklichkeitswert] to our specific pictures by means of *virtual* (if not actual) observations. … On the other hand, however, Heisenberg's idea is profoundly disconcerting. It makes it exceedingly difficult to use all the terms and concepts we have employed hitherto. … The position and velocity of a particle cannot both be accurately indicated simultaneously. Thus, since the particle now becomes a thing which does *not* describe a definite path,

> the question as to which path it describes becomes illusory in the usual sense. … The new idea obviously prohibits the formation of pictures or models which unambiguously and continuously fill the continuum of space-time in all details and without gaps. Maybe the world that can be observed … is no *continuum* at all. Of course, when faced with the question of how to represent it *otherwise,* we are still confronted by an insoluble conundrum. I do believe that we cannot be satisfied in the long run with the answer I once received from the young genius Dirac. Beware of forming models or pictures at all! (Schrödinger, 1929e, p. 292/159f.)

For a while this passage reads like a complete surrender on Schrödinger's part. But the quest for pictures survived Schrödinger's partial assent to the semantic aspects of the Copenhagen interpretation. And further down in the text we see him turning the tables. He accepted his opponents' conviction that one has to find a suitable meaning criterion, but tried to frame it in another way. This is an example in favor of Beller's view that certain tools from the positivist's tool kit proved extremely versatile.

> One may either believe (1) that matter has *really* a wave structure. Then the uncertainty principle is an immediate consequence. Or (2) one may think that the uncertainty principle is fundamental. The wave picture then is simply an auxiliary idea [Denkbehelf] for the convenience of grasping and representing the principle. (Ibid., p. 293/161f.)

No doubt, Schrödinger opted for (1). Yet he firmly believed that the elimination of those principal gaps in our physical world picture which resulted from Heisenberg's uncertainty relations "ought to be possible without leading to the consequence, that no visualizable [*anschaulich*] scheme of the physical universe whatever will prove feasible. …[To this end] it will be necessary to acquire a definite sense of what is *irrelevant* in our new models and schemes, before we can trust to their guidance with more equanimity and confidence." (Ibid., p. 294/165)

Thus in contrast to a metaphysical realist, Schrödinger was at bottom advocating a rigorous mathematical introduction of the basic concepts of quantum theory in order to suit intuitive clarity. There was little difference between him and von Neumann, who laid the mathematical foundations of matrix quantum mechanics, that axiomatization involved explicit limits for the meaning of the basic concepts. Yet any finality claim about a theory so construed is precisely as strong as the conceptual framework itself. And here Schrödinger still believed that physicists had not reached a satisfactory framework. To be sure, I do not intend to drag Schrödinger on the grounds of Carnap's linguistic frameworks. He accepted neither of Logical Empiricists' dogmas. Yet in contrast to the Göttingen-Copenhagen group who had a prior commitment to particle concepts as an alleged bridge to macroscopic physical reality, Schrödinger could more freely adopt any kind of ontology, any conceptual framework, as long as it was empirically adequate and corresponded to his quest for pictures. No wonder that he reminded his audience: "*We must not forget that ultimately pictures and models serve no other purpose than to hang all observations on them which are possible in principle*." (Ibid., p. 294/164)

Pictures and models are thus nothing but a scaffolding for observations actually made and virtual observations. In this Machian context it is no surprise that Schrödinger's criticism of matrix mechanics dressed up in a historico-critical fashion. To discuss the real existence of electron orbits had little sense because quantum physicists were convinced that the effect through which the orbiting electron would manifest itself, in case it existed, is certainly *not* observed. Despite the *immeasurable* progress which we owe to Bohr's [older quantum] theory, I consider it very regrettable that the long and successful handling of its models has blunted our epistemological delicacy concerning such questions. We must not hesitate to sharpen it again, lest we may be too rash to content ourselves with the new theories which today have supplanted Bohr's, and believe that have reached the goal which indeed is *still* far away. (Ibid., p. 290/155)

6.3.7 Science and the Milieu

The second paper in Schrödinger's 1932 booklet bears the title "Is Science Determined by the Milieu?". It appears to aim right at the heart of the Forman thesis, and indeed Forman (1971, p. 57) read it as an assent to the Spenglerian idea of an intimate relation of science and *Lebensgefühl.* But as we shall see in the present section, the intellectual roots of Schrödinger's speech did not point to post-war Weimar but to pre-war Vienna.

It is true, Schrödinger openly declared that "we are all members of our cultural milieu." (Schrödinger, 1932a, p. 38) Yet he did not doubt the truth of experimentally corroborated scientific facts. Rather was there, on his account, a subjective element in scientists' selection of topics and in the attention they devote to certain findings.

Once for some thing the *direction of our interest* matters at all, then the milieu, the cultural sphere, the zeitgeist, or however one wants to call it, must exert its influence. On all fields of culture one will find common traits in world view, and even more frequently, common stylistic traits in politics, arts, science. If one succeeds in exhibiting them also in exact science, then one has produced a kind of circumstantial evidence for subjectivity and milieu dependence [Milieubedingtheit]. (Ibid., p. 38)[138]

Such was the program of Schrödinger's paper that expanded an address held in front of the Prussian Academy of Science. In more than one respect Schrödinger returned to Exner's conception of a general theory of culture in which a culture was associated with the ideas it contributed to the evolution of mankind. Science, on that account, was part and parcel of cultural evolution, but because of the objectivity of its results as compared to the subjectivity of what we choose to investigate, Spengler's radical cyclism and the incommensurability of different cultures could not thrive. Some passages of Schrödinger's text sounded close to cultural morphology – as did a larger part of Exner's *From Chaos to the Present.*

As did Exner's Inaugural Address, Schrödinger began with the relation of *Geisteswissenschaften* and *Naturwissenschaften*. No doubt, "[r]egardless of all scientific faithfulness and allegiance to truth, there exists a notable artistic and accordingly subjective touch" in the humanities. Within the exact sciences one everywhere hears, in contrast, the maxim to "do away with all kinds of 'anthropomorphisms'." (both ibid., p. 26) This was, of course, what Planck considered as the pivot of modern physical science. But Schrödinger expressed his misgivings about Planck's quest for the absolute. "This claim to and this longing for absoluteness are partly justified, but partly they go too far." Of course, "perfect reproducibility of experiment is the primary condition that we call something a scientific result." (both

[138] The English translation was "freely rendered by Dr. James Murphy" (p. 81), even more freely than in other cases. For this reason I have translated this paper myself and omitted references to *Science, Theory and Man.*

ibid., p. 26) And there is no source of scientific knowledge other than experiment. But the number of experiments actually made is negligibly small compared to all possible experiments. Thus at each stage of scientific research "it is necessary anew to *select* those experiments which we consider as interesting, important, informative." (Ibid., p. 27) Of course, our choice will be motivated by earlier experiments; yet, firstly, not by them alone,

> but mainly by the *thoughts* we have formed about them. Secondly, the *selection* of the preceding experiments was in turn determined by the results of even earlier ones and the thoughts linked to them. And thus forth, until this pedigree of planned experimentation arrives at the first conscious observations of nature by primitive man, which were not yet brought about by any specific organization, but determined by the biological situation, by the construction of the soma and its interaction with environment. (Ibid., p. 28)

One can hardly imagine a more faithful brief of Mach's epistemology. Not only that modern science ultimately rooted in the primeval experiences of mankind, Schrödinger also depicted the development of science as an interaction of (experimental) facts and thoughts – as did Mach in his famous slogan. Schrödinger rejected an objectivist, or determinist, reduction of our subjective interests to the "force of the facts" (Ibid., p. 28), an idea which was advocated, for instance, by Duhem (1908, Ch. 11). "The entire history of science speaks against this. Often a single thought all of a sudden stirs up the interest for investigations which seemed uninteresting and irrelevant before." (Schrödinger, 1932a, p. 28f.) It was Nernst's heat theorem which created the interest in heat capacities. On the other hand, Grimaldi's important discoveries were neglected because Newton's emission theory drove out Huygens' undulatory theory. Mach's study about the history of optics showed "how little the development of science corresponds to a logical and systematic course." (Ibid., p. 32 citing Mach, 1921, p. 204) Thus, Mach's psychology of knowledge must supplement the descriptivist maxim of Kirchhoff. Not the facts by themselves count, but the thoughts we form about these facts. Science accordingly does not deal with something objectively given. Yet even if we assumed that there exists a definite object for scientific research, such that the subjective order of investigation were irrelevant, investigators could err systematically as if one just picked the even numbers out of all natural numbers. By this re-turn to Mach, Schrödinger's thesis about the difference between corroborated facts and interest-influenced thoughts became more basic than the distinction between facts and theories by means of which Exner had criticized Spengler (See Sect. 4.4.).

After these general arguments, Schrödinger provided three historical examples of how the general milieu of a certain epoch looked like. The first was Greek antiquity.

> The clear, transparent, and rigid edifice of *Euclidean* geometry corresponds to the plain, simple and *limited* forms of the Greek temple. The whole temple is small, near at hand, completely surveyable … So, too, Greek science could not really access the infinite. … Greek drama, especially that of earlier epochs, is absolutely *static.* … So also in Greek physics there exists no dynamics [in the sense of Newton]. (Schrödinger, 1932a, p. 39)

The second example could be seen as an Exnerian elaboration of Boltzmann's conviction that the 19th century was "the century of Darwin" (Boltzmann, 1905, p. 28/15) rather than the steam engine. "There has hardly ever been a single idea which exerted a more dominant influence on all fields of life and the sciences than evolution, in its general form as well as in the special form given to it by Charles Darwin." (Schrödinger, 1932a, p. 40) "Ernst Mach has applied it to the *scientific process* itself. … In astrophysics we have learned to consider the various types of stars merely as different stages in one and the same stellar evolution." (Ibid., p. 41)

Thirdly, within contemporary physics Schrödinger spots five not mutually distinct trends caused by the milieu: *Reine Sachlichkeit*; the desire for revolution [Umsturz] and a preference for freedom and lawlessness; the ideas of relativity and invariance; the methods of mass control; statistics. Let me go through them in more detail.

First, within our present material culture "houses, furniture and all our domestic accessories are produced exclusively with regard to their designated use, their purposiveness [Zweckdienlichkeit];" (Ibid., p. 43) ornamentation and useless decoration are banished. The same tendency prevails within our physical world view. Scientists focus on the observed facts and exclude all arbitrary hypotheses, in particular everything that cannot be observed in principle. For example, in the kinetic theory of gases the specifications made for the single molecules have been constantly reduced in the course of time: from microscopic billiard balls to systems only obeying the laws of classical mechanics, and finally to the even more modest assumption of the conservation of energy and momentum as statistical average laws. In a footnote Schrödinger explicitly distinguished these statistical laws from "the tendency of modern quantum mechanics to deny exact regularity altogether." (Ibid., p. 45 fn. 1) Schrödinger's second example concerned the problem of quantum jumps which emerged in the older quantum theory. And he rehearsed the standard Copenhagen argument that the precise energy of an electron at a certain instant of time made sense only when it is measured, which however disturbs the system. Accordingly, "certain concepts are simply removed and their places left empty in contrast to the desire of completion that once prevailed." (Ibid., p. 48) Have *Reine* (or *Neue*) *Sachlichkeit* and positivist meaning criteria jointly overcome the *horror vacui*? Certainly, there was an alliance between the modernist movements in the arts and in science, for instance, between the *Bauhaus* and the Vienna Circle. The interesting point of Schrödinger's testimony is that, in stark contrast to Forman (1971), he did not present this modernist tendencies as a fringe phenomenon but as the outcome of a history that reached back to the days of Mach and Boltzmann.

Second, everywhere the authority of history is called into doubt. "One demands the right of independent examination. Everything, every institution, if it wants to persist, must legitimate itself by its rationality or by something other than its historical emergence." (Schrödinger, 1932a, p. 48f.) The desire for revolution has become a characteristic trait of modern science. The doubts of Gauß that physical geometry need not be Euclidean culminated in Poincaré's conventionalism according to which we are free to choose any geometry we consider convenient. "The revolutionary tendency of modern physics has most strikingly taken form in relativity theory and in quantum theory. The latter even casts doubts on the dogma of causality." (Ibid., p. 50) And referring to the first paper in his booklet he added an observation familiar from his dialogue with Planck (See Sect. 6.3.5), to wit, that the issues of causality and geometry were one of a kind.

It can never be decided experimentally whether causality in nature is 'valid' or 'invalid'. The relation of cause and effect, as already Hume has pointed out, is not something that we find in nature but concerns the form of our thinking about nature. We are completely free to maintain this form or to alter it according to convenience, that is, in which way the whole description of nature becomes simpler. (Ibid., p. 50f.)

Schrödinger's desire for revolution thus falls by far short of the alleged desire of the Weimar milieu "to sink the law of causality by hook or by crook;" (Forman, 1971, p. 84) in particular if one takes into account the rigorous mathematical form of this freedom as continued in the next point.

Third, "stated very generally, the kernel of the idea of relativity is this: even … to a question that apparently admits of only 'Yes' or 'No' as an answer, one has to answer sometimes: As the case may be! That depends! … What really matters now is to actually construe the 'that depends' in such a way that the conflicting experiences or thoughts which have led to the dilemma are reconciled." (Schrödinger, 1932a, p. 52) Special relativity, for instance, reconciliated the aberration of the light coming from the fixed stars and the result of Michelson's experiment by defining motion relative to a reference system. In this way, one also obtained an elimination of superfluous entities in the sense of the first observation. But are there questions which can be unambiguously answered at any time?

The complementary antithesis to the concept of relativity is *invariance.* ... Once formed, the concept [of invariant] proves so comprehensive that it appears as if all human concept formation is subject to it. … Also the thesis of the objectivity of scientific knowledge is a claim of invariance. The question is whether the propositions of natural science are invariant with respect to the cultural milieu or whether they need the latter as a reference system and, in the case of a radical change of the cultural milieu, become not false in all details but substantially change their true meaning and their interest. (Ibid., p. 54f.)

It is crucial not to misread Schrödinger's statement as an assent to Spenglerian relativism. The statement of relativity, if more than just evading the question, must be made sufficiently precise, so that we know how to transform a question from one reference system into another. Invariant questions are of particular interest, but being scientifically meaningful does not require absolute invariance. As Schlick (1920) emphasized, physical laws must not depend explicitly upon the position in the Universe, but whether events can causally influence each other depended upon their relation in space-time. Hence what Schrödinger discussed under relativity corresponded to the search for a coordinate system that was appropriate to the problem at hand and eliminated spurious degrees of freedom. Moreover, it appears to me that also Schrödinger's quest for suitable pictures in quantum mechanics was a quest for invariances in this sense. They need not be absolute invariances, in the form of Kantian categories or neo-Kantian limits, but they had to be subject to well-defined transformation rules between reference systems; the notorious collapse of the wave packet did not live up to these requirements. In an analogous way we can also partly transform the views of other cultures to our present one. As Exner had stressed, scientific facts prove to be invariants under this transformation because of their objectivity. But within certain limits we can also assess sunken cultures or their traces in present culture and compare them to our. For instance, Schrödinger added a footnote that the idea of relativity "has been familiar to the *Indian* thought from time immemorial." (Ibid., p. 51 fn.1)

Fourth, mass control means “a highly-developed technique of managing populations [Gesamtheiten] large in number whose members nonetheless require individual handling, … [such as] inhabitants, voters, taxpayers, clients, … . The means to manage them are registration, cartography, catalogues, … Also the making of laws and jurisprudence belong here.” (Ibid., p. 55) Moreover, the increased demand of goods is met most economically by factory production. However, the “most perfect instance of mass control by excellent organization and, at the same time, economy of labor by making an expenditure once and for all, we find in the methods of mathematical analysis.” (Ibid., p. 56) Differential equations permit the physicist to describe the motions of an arbitrary number of mechanical particles. The familiar tensors of general relativity represent a systematically arranged register for calculating any desired gravitational phenomenon. The art of the theoretical physicist thus consists in formulating a problem in such a way that it can be approached in an economical, factory-like fashion. “Simplifying economy is the true essence of mathematical progress, whereby a constantly growing territory becomes accessible to our quantitative thought.” (Ibid., p. 58) This came quite close to Mach’s understanding of mathematics.

Mathematics is the method of replacing in the most comprehensive and *economical* manner possible, *new* numerical operations by old ones already made, so that there is no need to repeat them. It may happen in this procedure that the results of operations are employed which were originally performed centuries ago. (Mach, 1988, p. 499/583)

To be sure, assenting to Mach’s thesis about the economy of mathematical practices did not require to accept, as Boltzmann had actually done, Mach’s empiricist foundation of mathematics as “economy of counting”.

Fifth, statistics introduced a new idea into mass control, the “prudent renouncement of detailed knowledge.” (Schrödinger, 1932a, p. 59) We have to redirect our interest in such a way as to phrase new questions that let emerge new regularities. The restriction to statistical knowledge, accordingly, does not amount to a resignation. But it is not a trivial task and as “the statistical method is a dominant feature of our times” (Ibid., p. 61) there is a lot of misuse and uncritical application. In the end, Schrödinger turned to the application of statistics in economy and sociology. As in physics, there one tries “to forecast the laws according to which the statistics will alter if the external conditions are arbitrarily changed.” (Ibid., p. 61f.) And he reminded his readers of the first lecture in which he showed that the physicist can reach precise average laws without knowing of or being able to act upon the single molecules.

Might one not perhaps see here common traits with a characteristic of our epoch that is not yet reached but nonetheless aimed at? For it appears to be the aim of a higher culture to reach the necessary order and lawfulness in the human community without an overly detailed interference into the living of the individual; rather [it aims to intervene] in such a way that one studies the average nature of man and its statistical range of variation and subsequently posits suitable motives and offers such goals to the feelings of desire that a bearable community is secured at least on average in the large. (Ibid., p. 62)

Schrödinger’s booklet thus ended with a plea for social engineering that was diametrically opposed to the end of Exner’s 1908 Inaugural Address. There Exner considered the statistical distribution of human faculties and commodities as unchangeable by any kind of political intervention, however, without restricting the behavior of the individual members of society. Culture defined itself through the emergence of this unchangeable state, at least until it degenerated into civilization. Schrödinger, in contrast, not only assumed that the overall distribution in society can be changed once we have learned the most effective mechanisms to do so. He also considered it as the aim of higher culture to improve the conditions of the community. Despite all dedication and adherence to Exner, Schrödinger thus did not follow Exner's mandarin world view. And Schrödinger's criticism against the uncritical application of statistical methods could also be turned against his beloved teacher.

7. Moritz Schlick at the Causal Turn

As early as during the first decade of the 20th century Vienna Indeterminists seriously contemplated the idea that the basic laws of nature were irreducibly indeterministic. Thus Heisenberg's (1927) pronouncement of the invalidity of the principle of causality in quantum mechanics did not find them unprepared. In contrast, Moritz Schlick frankly admitted that quantum mechanics had taken him by surprise.

> The turn that recent physics has taken on the subject of *causality* could equally [as in the case of relativity theory] not have been foreseen. For all the philosophizing that has gone on about determinism and indeterminism, about the content, validity and testing of the causal principle – nobody has lighted on precisely that possibility which quantum mechanics offers us, as the key which is to yield understanding of the type of causal order that actually prevails in reality. (Schlick, 1931, p. 145/176f.)

In the present section I shall show why Schlick had to substantially change his mind about the principle of causality after the advent of quantum mechanics. To this end, I compare his two influential papers on the subject that appeared in *Die Naturwissenschaften* in 1920 and 1931. While his "Philosophical Reflections on the Principle of Causality" were basically after a precise formulation of the principle of causality within the framework of relativistic physics and contained just a few lines on the problems of statistical physics, the 1931 article "Causality in Contemporary Physics" explicitly revoked the earlier project and, citing the lessons of quantum mechanics, considered causality as basically coextensive with successful prophecy.

In 1920 Schlick was about to become the leading philosopher of relativity theory and constantly engaged in skirmishes with those neo-Kantians who insisted on the synthetic a priori character of space and time. There exist detailed analyses of how Schlick employed certain versions of conventionalism to that end and on what grounds he rejected any relativized a priori.[139] As we shall see, the departure from Kantian territory took considerably longer for the category of causality than it did for space and time, case in which it was fully completed with the second edition of the *Erkenntnislehre* in 1925.

Schlick's drastic turn in causal matters corresponded to a change of philosophical influences. While his 1920 theory (Section 7.1.) was still strongly indebted to the Kantian conception of causality as advocated by his former teacher Planck, the 1931 theory made prominent – though not decisive – use of the Wittgensteinian distinction between an assertion and a prescription for the making of assertions (Section 7.3.). In-between there exist a few documents of transition in which a simple rehearsal of the earlier stand meshed with serious doubts about the validity of the principle of causality prompted by the developments in quantum mechanics (Section 7.2.). Although his modified theory of causality brought Schlick significantly closer to his Vienna Circle colleagues, there is clear evidence that Schlick's position remained by far more accessible to other German physicist-philosophers than Vienna Indeterminism. (Section 7.4.) But all of them had problems with his adherence to a Kriesian theory of probability. It would take another five years until the growing

[139] E.g., (Friedman, 1994) and (Howard, 1994).

'linguistic turn' on both sides of the aisle brought the Machian and the post-Kantian strands of the Vienna Circle, or more precisely: Frank and Schlick, to a far-reaching agreement (See Sect. 8.7.).

7.1 Schlick 1: Causality Modeled after General Relativity

Schlick's 1920 paper opened as such. "The causal principle is not itself a law of nature, but rather a general expression of the fact *that* everything which happens in nature is subject without exception to valid laws." (Schlick, 1920, p. 461/295) Any particular law of nature is subordinate to this principle taken in the sense of Kant's original formulation "Everything that happens …, presupposes something upon which it follows to a rule."[140] As the rules determining the sequence of natural events are the laws of nature, the principle of causality is "identical with a claim to thoroughgoing subsistence of natural laws." (Ibid., p. 461/295) Rather than expressing these laws by way of relations of cause and effect, their strictest form is achieved in the mathematical equations governing physical processes. There can be differential equations involving infinitesimals, as in classical mechanics or electrodynamics, or difference equations owing to the discontinuous constitution of matter on the atomic scale suggested by quantum theory. Restricting attention to the first case, the principle of causality in physics can be given a "by now unobjectionable and empirically testable form … on the [empirical] assumption that forces acting at a distance do not exist," so that we can isolate the events in a certain region of space-time. Accordingly, "given the 'initial conditions' and 'boundary conditions', everything that occurs in the area under consideration is univocally determined and calculable by means of the differential equations of physics." (Both ibid., p. 462/297)

As the infinitely small is unobservable in principle, those differential micro-laws cannot be observed, but are inferred from the integral macro-laws accessible to measurement or observation over a finite space-time interval.

> The differential laws prevailing in nature can … be conjectured only from the integral laws, and these inferences are never, strictly speaking, univocal, since one can always account for the observed macro-laws by various hypotheses about the underlying micro-laws. Among the various possibilities we naturally choose that marked by the greatest simplicity. It is the final aim of exact science to reduce all events to the fewest and simplest possible differential laws. (Ibid., p. 462/297)

This micro-macro distinction neatly corresponded to the set-up of Exner's (1909, 1922) argument in favor of indeterminism by virtue of greater nomological coherence. Determinists like Schlick's teacher Planck (1914) had argued, to the contrary, that even probabilistic macro-laws required strictly causal micro-laws, whereas Vienna Indeterminists saw no cogent reason to do so. Against this backdrop it is quite startling that Schlick did not altogether interpret the difference between macro-laws and micro-laws in the sense of statistical mechanics, but remained exclusively in the realms of deterministic physics and simply rehearsed Planck's (1915a) description of the goal of theoretical research.[141] To be sure, by that time simplicity had already become a major

[140] (Kant, [1781] 1990, A 189) cited in (Schlick, 1920, p. 461/295).
[141] See Sect. 4.5. for the debate between Exner and Planck, and Sect. 3.8 for the pivotal role of the Principle of Least Action within Planck's philosophy of physics.

philosophical motive within Schlick's assessment of relativity theory by means of which he counterbalanced the conventionality of space-time (See Friedman, 1994, p. 26-29), a move which testified his parting company with the neo-Kantians. But within his theory of causality, the criterion of simplicity led Schlick straight to Planck's preferred candidate for a simple universal principle, the "Principle of Least Action" or "minimal principles" in general. Yet in contrast to Planck, Schlick's conventionalism blocked attributing any ontological surplus to the integral principles. That one can describe a motion between two moments in time by specifying the initial state and the final state indicates, rather, that

> there is a great arbitrariness of view here, and each [of these descriptions] is legitimate, really, so long as it only leaves untouched the thoroughgoing perfect determinacy of the whole. This should be borne in mind, above all, when examining the difference, and the legitimacy, of causal and finalistic or teleological viewpoints; many erroneous questions in this area have arisen from lack of clarity in regard to the simple relationships we have discussed. (Schlick, 1920, p. 462/298)

Already by this move Schlick implicitly rejected Mach's rephrasing causality as functional dependences, for when considering "the thoroughgoing interconnection of processes, which finds expression in the invariable determinacy of all happenings [in nature], as a causal one" (Ibid., p. 462f./298) he added the following footnote: "In contrast to functional relation, which signifies not a real relation, but a purely conceptual and analytic one, such as exists, for example, between the number *x* and *log x*." (Ibid., p. 463/320)[142] The contrast to Mach's account was completed by Schlick's pronunciation that "we think the laws independently of whether man, in particular, knows about them or not: he does not make, but merely discovers them." (Ibid., p. 465/302) Mach and Exner, to the contrary, had held that natural laws are formulated by man.

Although Schlick's conception of causality as an atemporal and real relationship of lawfulness remained consistent with Kant's conception, he rejected the transcendental argument that only the special form of our knowledge about natural laws was rooted in the special nature of our cognitive organization so that the general principle of causality was valid in any cognizable world even if humans were unable to formulate a single universal law. Uniformity of nature is thus not only a precondition for our understanding of nature, but of her being at all causal.

In Schlick's rather intricate argument we discern both his explicit abandonment of neo-Kantianism in favor of an empirical notion of causality[143] and an early use of the verificationist criterion of meaning. This is not to say that Schlick whole-heartedly subscribed to empiricism. At the beginning, he rejected Mach's argument against the notions of cause and effect that was condensed in the pithy phrase that "nature has but an individual existence." (Mach, 1988, p. 496/580) While Mach held that like cases exist only in abstraction, Schlick emphasized that already a far-reaching resemblance sufficed for a direct experience of similarity because small differences remained below the threshold of consciousness. Although our factual knowledge of causality thus

[142] I assume that the expressions "Zusammenhang", "Abhängigkeit", and "Beziehung" are used interchangeably here.

[143] A footnote on p. 465/320 takes back his position from the *Allgemeine Erkenntnislehre* (1918, p. 322) and rejects similar ideas of Hugo Bergmann and Johannes von Kries.

depended, contingently, on uniformity, could there be, nonetheless, laws of nature in an universe without any uniformity? Schlick rephrased this question as such.

Must every law of nature be *universal,* that is apply to a number of real cases separated only by space and time – or can there also be *individual* laws, such that every process in the world follows its own special rule … ? … Nothing seems easier! We need only suppose that space and time enter explicitly into the mathematical expression of natural laws, and do so as arguments of non-periodic functions. (Schlick, 1920, p. 464/301)

In such a chaotic world, space and time would attain an absolute meaning. "A change in the frame of reference would necessitate a quite different formulation of the laws of nature: so spatial and temporal determinations would not be relative" (Ibid., p. 464/302) – which is clearly at odds with relativity theory. Before drawing philosophical consequences from this fact, Schlick addressed the more fundamental question whether such a thought experiment could at all be empirically verified. Having abandoned the categorical validity of the principle of causality, Schlick could consistently imagine that there exist

two exactly similar worlds, such that in one of them the very same processes occur by chance as go on in the other through causality. Given any seemingly lawless sequence of world-states whatsoever, we can also view it, if we please, as the obedience to law. For let the piece played upon the world-stage be as chaotic and confused as can be: … for the maddest irregularities a sufficient explanation may always be found … in the particular values of the space and time coordinates of the process in question. (Ibid., p. 465/303)

Hence the existence of a mathematical formula cannot ground the necessity of the causal link between two states.[144]

Schlick now refuted a restricted version of the prophecy approach which would become his starting point in 1931. One could, first, argue that for random events the individual laws are *established subsequently* while in causal processes the course of events can be *predicted*. "But this attempt at a distinction fails of its purpose. For in the first place, every law, every objective rule, as a purely conceptual structure, is atemporal." (Ibid., p. 466/304) Concepts exist whenever they are consistent. One could, secondly, invoke the moment in time when the law was actually formulated. Without the presupposition of a recurrence of similar events, however, "the observational discovery of the true laws of events would be wholly impossible to us. But *in the absence* of observation, the correct formula could only be *guessed.* But guesswork, of course, is equally possible in a totally lawless universe." (Ibid., p. 466/304) Third, Schlick ruled out the possibility that the distinction in question be found in psychological experience. This would lead into a dangerous anthropomorphism by confusing causal necessity [Notwendigkeit] with compulsion [Zwang]. "The contrary opposite of necessity is *contingency*, while that of compulsion is *freedom*". (Ibid., p. 467/306) If this difference is properly respected any conflict between causality and free will simply disappears.

As, accordingly, no difference can be asserted "between a universe confounded by chance and one thrown into confusion by causality," (Ibid., p. 465/303) uniformity of nature, and hence the universality of natural laws, were necessary conditions for a meaningful notion of causality, not only for their actual discovery. While practical

[144] On a similar basis Frank (1907) argued that the law of causality was merely a convention; see Sect. 8.1.

uniformity as a regulative principle would, to my mind, correspond to Mach's principle of economy, Schlick's requirement of uniformity – albeit based on a verificationist instead of a transcendental argument – came close to a Kantian 'condition of the possibility'.

It is not enough that we should be able to think at all a formula whereby the course of nature can be presented – this is possible *without exception*; the formula also has to be of a *specific kind.* Any number of cases of its application must, in fact, be possible. A law of nature is such, therefore, only if it is *general:* the concept of individual causality has led us into contradiction. (Ibid., p. 467/307)

This position prompted Schlick to reject "under all circumstances" the idea that the laws of nature could change, even if this change occurs slowly, "since such a possibility could never fall within the scope of scientific experience." (Both ibid., p. 467/307) The idea of natural laws changing on the cosmological scale was advocated by Fechner and granted by all Vienna Indeterminists; it was an unavoidable consequence of their radical empiricist stance.

Schlick's restriction on the form of causal laws involved the concepts of space and time. Here again, he took a rather Kantian tack.

[I]f like cases are to be able to exist in the course of nature, some principle of separation must be presupposed, which sees to it that occurrences can be *alike* without being *identical*. … This separatedness in nature is realized in two ways, … namely in spatial coexistence and temporal succession. … These principles of separateness, which constitute the presupposition of the concept of lawfulness and causality, have rightly been called *forms*, following Kant's terminology, precisely because the concrete [contentual] determinations [inhaltlichen Bestimmungen] of things do not depend on them. That space and time are these forms is a fact that we have to accept. We can imagine no others even if they are thinkable. Space and time could not fulfill their function as forms if they entered explicitly into the differential laws of the natural process, for in that case they would indeed have a concrete significance. (Ibid., p. 467f./307f.)

Schlick attributed this insight to Maxwell.

Space and time are hereby credited with a homogeneity which is in fact indispensable to them, if they are really *forms*, as causality requires. This homogeneity is of the most general kind (and thus consistent with spatio-temporal inhomogeneities conditioned, according to gravitational theory, *by matter*) … By now thanks to the new advances in our physical views, the relativity of space and time has been laid down in a far broader and deeper sense. It has to be asked whether this most thoroughgoing relativization likewise forms an inescapable presupposition for the possibility of causality, or whether there is no such close connection here as with that particular relativity, of which we found that without it there can be no meaningful talk of a universal lawfulness of nature. (Ibid., p. 468/308)

Schlick's argument commenced from Einstein's famous thought-experiment about two isolated fluid bodies of different shape rotating relative to one another. Ascribing the flattened shape of one of them to centrifugal forces in absolute space, Newtonian mechanics only appeared to provide a causal explanation. But there is no contradiction to the requirements of the principle of causality; "it is merely that the bounds of its applicability are more narrowly drawn there than in the general theory of relativity." (Ibid., p. 469/309) For, Newtonian mechanics did not consider shape as a *causa vera* but restricted the principle of causality to processes. Here also Schlick found that the specific formulation of causality used within a particular theory is linked to the latter's ontology.[145] But Schlick considered only one ontology as suitable to the principle of causality, to wit, processes in space-time. And his main point was that general relativity permits the reduction of further substantial properties and states to processes.

General relativity thus represents "not merely a logical simplification but an actual *advance in causal explanation*; it opens up to the concept of cause a new area beyond the boundary that had hitherto seemed to limit its sway." (Ibid., p. 470/312) It did so by the experience that the system of fixed stars possesses a privileged status as a system of reference. What Mach had merely postulated, Einstein turned into a differential equation by introducing the concept of a gravitational field and interpreting its states as processes, to wit, as motions of matter.

> From the epistemological point of view it is remarkable that the gravitational field does not represent anything perceptible in the same sense as the motions of visible bodies to one another. But if we stop short at considering the latter, we get no further than the mere postulate of the relativity of all motions … *Mach* who established the postulate, notoriously arrived only at … quite useless formulae. … The situation seems to me not without relevance to an assessment of Mach's theory of knowledge. (Ibid., p. 471/313)

As had his teacher Planck (1908a), Schlick charged Mach of infertility.

Schlick, strikingly, not only argued that by turning seemingly irreducible properties into causal processes, general relativity extended the realm of causal explanation of nature, but he also contemplated an inversion.

> If it were absolutely established that these properties of bodies have to be interpreted in this way [i.e., as processes], … then in fact the demand of causality would be satisfiable only by the general principle of relativity… [A] non-relativist view would then actually contradict the causal principle … There is no seeing, indeed, how … a rigorous proof is to be adduced for the necessity of viewing the property in question as a process. … But despite possible want of proof, the scientist – and this cannot be sufficiently stressed – has in general no firmer conviction than that of the process-character of the perceivable properties of matter. … It tells very strongly, in my view, in favor of general relativity, that the presupposing of principles so fundamental and well-established is enough to make it appear as a mere inference from the principle of causality. (Schlick, 1920, p. 471/314)

Recall that, to Schlick's mind, the principle of causality was empirical because the non-existence of action-at-a-distance represented an empirical fact, in virtue of which the principle's mathematical form had to be of a particular kind. Moreover, it is a contingent "fact that there is any such thing as natural law and causality." (Ibid., p. 473/317)

Schlick followed Kant in intimately linking the principle of causality to one particular theory of space and time. Einstein's general theory of relativity grossly extended the sway of the principle of causality by reducing properties to processes. Yet, are there "properties which we cannot understand as processes, which consequently defy causal explanation, and in which our physical knowledge therefore encounters an insuperable barrier and natural limit?" (Ibid., p. 472/315) All laws of nature contain initial and boundary conditions which cannot be reduced to the laws themselves. They "fill the empty form of natural laws with content, in that they determine the integration-constants therein." (Ibid., p. 473/316) Their values are as contingent as the form of the primeval nebula. Adopting a terminology of von Kries,

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[145] Von Mises (1922a) considered relativity theory as more restrictive than Newtonian mechanics, which following his notion of causality corresponded to a diagnosis similar to Schlick's (See Sect. 8.2.).

Schlick contrasted *nomological* and *ontological* determinations of reality, that is, dynamical processes and initial or boundary conditions.

Although general relativity does not permit any longer to separate off a unique time coordinate, one could still split space-time into a three-dimensional hypersurface – Schlick called this the basis of a deformed cylinder – and a fourth dimension – the height of the cylinder – that played the role of time and carried the nomological dynamics.

> [T]he only [thing] that matters to us here: a three-dimensional region has to be given, which is at least extended by an infinitesimal amount into the fourth dimension as well; by this, then, the immediately adjacent world-slice is also co-determined through 'causal dependency'. *Everything real is four-dimensional; three-dimensional bodies are mere abstractions, exactly as lines or planes are.* The causal determinacy of the world extends only in *one* dimension, and this we call the direction of time. Once it is chosen, what lies in the other three dimensions has to be seen as simply *contingent.* With this an insuperable barrier to the causal mode of consideration has undoubtedly been designated. The causal principle finds application only to extension in the time-direction. (Ibid., p. 474/319)

Despite this categorical wording, Schlick had qualified his claim already on the day before his article appeared in print. In a letter to Einstein written on 10 June, 1920, he admitted that he "must not object to considering the lawfulness [Gesetzlichkeit] within a time slice as causal. My reasons were only: 1) The fact that in the reality of *consciousness* time seems to play a distinctive role, and 2) that those regularities [Gesetzlichkeiten][146] must have a different character than those in temporal direction. But these are only subjective reasons which perhaps are dispelled upon closer consideration." On 7 June, 1920, Einstein had sent Schlick some critical comments on the galley version. He contemplated that "more complete laws of nature might leave a considerably restricted arbitrariness in the choice of initial conditions".[147] From the standpoint of statistical mechanics, this was a natural objection to raise; it could be seen as the call for conditions more convincing than elementary disorder.

Could ontological determinations be lawful in a non-causal sense? Or put differently, could they resist reduction to more fundamental laws? Schlick believed that if all properties are reduced to processes some factual conditions or fundamental constants in the laws would remain. This had also been Planck's position for whom the irreducible constants of nature represented something absolute (See Sect. 3.8). But Schlick's position was less committed to convergent realism. Although laws among these conditions cannot be causal, they must not attribute an absolute meaning to space and time coordinates. Schlick contemplated two possible types. First, one could consider the identity of all electrons in the world as a law. As a matter of fact, modern physics knows of two different types of indistinguishability, which separates the quantum world into bosons and fermions. Second,

[146] The translation of "Gesetzlichkeit" and "Gesetzmäßigkeit" poses some problems because they denote both certain regularities, weaker than laws of nature, and the fact that a certain domain is governed by law. In Schlick's second theory the problem is aggravated by the fact that he denied the status of "Gesetz" to statistical "Gesetzmäßigkeiten". I thus translate depending on the context as "regularity" or "lawfulness" and add the German word where necessary.

[147] Both letters from the Schlick-Nachlaß; I take the almost identical letter of 9 June simply as a draft of the letter of 10 June. I refrain from entering into Einstein's other criticisms in the letter of 7 June, in particular, because they would lead into detailed considerations of relativity theory and because in a letter of 31 June, 1920, Einstein makes a partial withdrawal.

if it were certain that everywhere in finite portions of the universe only such processes occur as are bound up with an increase of entropy (transitions, that is from states of lower to states of higher 'probability'), this would presuppose an initial state of specific lawfulness (the hypothesis of molecular disorder), which would likewise be of the type in question.

> The problem of whether there actually are such laws … is of greatest importance for the shaping of our picture of the world. Only when it is solved, perhaps, will a satisfactory logical theory of 'inductive' cognition be possible. For logical induction can be regarded as the procedure by means of which we ascertain causal connections of a merely 'contingent' kind. (Schlick, 1920, p. 474/320)

It is quite interesting that at the end of his paper, Schlick considered the problem of causality as more basic than the problem of induction. This was in stark contrast to the views Reichenbach would develop during the 1920s.

Similarly as Planck, Schlick strictly separated dynamical causality and statistical regularity, or (causal) nomological and (contingent) ontological determinations. Statistical regularity was a specific type of ontological determination that supervened on what was nomologically possible. Although Schlick used ontological determinations in a somewhat more general sense, this neatly corresponded to von Kries's (1886, 1927) *Spielraumtheorie.* Von Kries had supplemented the a priori category of causality by the equally unempirical principle of range (Prinzip der Spielräume) (Kries, 1886, p. 170) in order to make space for objective probability in a deterministic world. The range (*Spielraum*) of all nomologically possible events was determined by the laws of nature. Ontological determinations concerned circumstances for which we do not have any cogent reason to demand further explanation but simply have to "consider as something factually realized." (Kries, 1919, p. 5) In physics they were typically described as initial conditions. In contrast to Planck's (1914) intention to eliminate the highly improbable events, von Kries's theory could accommodate the highly improbable events and even justify a version of the ergodic hypothesis. (Cf. Kries, 1919, pp. 19-21) Although Schlick explicitly departed from von Kries by admitting an empirical notion of causality, he did not abandon the distinction between both kind of regularities and, consequently, the Kriesian conception of probability.[148] But he remained surprisingly vague and cautious about molecular disorder, and largely argued by conditional clause.

Four years later, Schlick would become even more hesitant about Planck's insistence on strict dynamical causality. In a review of Planck's first collection of philosophical papers *Physical Panoramas* (1922), Schlick endorsed the criticism of Mach's positivism as of inferior fertility and emphasized the value of Planck's quest for synthesis and final principles. Yet at two points the reviewer called for philosophical caution. First, when Planck believes that the distinction between reversible and irreversible processes is of so fundamental a character as to persist in any future physical world view, one has to remember "that reversible processes – as Planck himself clearly emphasizes – are only ideal limit cases; thus all *real* processes would be strictly speaking irreversible, and the distinction in question rests upon an

[148] I have excluded a separate discussion of von Kries's theory although it was outlined on the pages of *Die Naturwissenschaften* and von Kries commented upon an earlier paper of von Smoluchowski (1918). But von Kries was not only a contemporary of the 1920s. His theory of probability had been written at about the same time as Fechner's, and he never made significant modifications to it. For a succinct account of von Kries's theory and the influence of von Kries on Waismann, and thus on (Schlick, 1931), see (Heidelberger, 2001). A broader discussion is found in (Neumann, 2002), but, as far as Schlick is concerned, the author exclusively focuses on logical probability only and erroneously charges the Vienna Circle of misunderstanding von Kries's account of probability in physics. (See Section 7.3.)

artificial abstraction." (Schlick, 1924, p. 819) Thus far Schlick was in accordance with Exner's criticism of Planck's rectorial address (See 4.4.). But while Exner had accentuated the primary character of irreversibility, Schlick interpreted the kinetic theory as a proof that all irreversible macroprocesses reduce to reversible microprocesses. Ultimately the distinction between reversible and irreversible processes becomes simply a consequence of "the opposition between the statistical regularities of the constellations in nature and the strictly causal lawfulness of all processes." (Ibid., p. 820) At bottom, this interpretation was closer to Schlick's own theory of causality than to Planck's original views because a substantial part of the difference in principle between reversible and irreversible physics was linked to the difference between that part of physics that could be subsumed under a Principle of Least Action and that part that could not. Next, Schlick endorsed Planck's (1914) criticism of Exner's belief that all laws of nature are of statistical kind.

> Certainly, this careful attitude [of Planck's], which wants to sacrifice the principle of causality under no circumstances, has to be approved from the philosophical point of view. But even though the reviewer sympathizes with this, he must nevertheless emphasize that on purely logical grounds one has to admit the possibility that the final laws of nature might be of statistical, and not of causal character. (Ibid., p. 820)

Yet it is remarkable, so Schlick continued, that the stubborn defense of the principle of causality stemmed from the creator of quantum theory. Referring, finally, to Planck's diagnosis of the "gigantic breach in the present system of natural science" (Ibid., p. 822) that opens between quantum theory and relativity theory, Schlick praised his teacher's quest for a unified theory at a deeper level of reality.

7.2 Documents of Transition

At about the same time, Schlick completed his entry "Naturphilosophie" for Max Dessoir's *Lehrbuch der Philosophie.* Although Schlick basically rehearsed his 1920 theory of causality, he now seriously contemplated the possibility that the basic laws of nature could be statistical. We can find even a mention of the "newest findings which raise serious doubts as to the absolutely strict validity of the energy principle as such: it seems hardly possible to dismiss the idea that energy remains constant only on average." (Schlick, 1925a, p. 420/23) This obvious allusion to the BKS-theory[149] appeared in the context of Schlick's historical outline of the dissolution of the concepts of substance and identity of substance into mechanical motions and – after the mechanical world view had proven insufficient – into law-governed processes and constants of nature. This historical development, so Schlick contended in full accordance with Planck, was mainly driven by the motives of objectivation, abstraction and unification. Measured against these criteria, energeticism clearly signified a step backward to a set of distinct energetic substances. This was precisely the point where Boltzmann had rightly turned Mach against Ostwald. (See Sect. 3.4.)

"Once the theory of relativity is admitted … it is not just *unnecessary* … to found things on the idea of substance as the concept of the identically permanent; it

[149] This testifies that Schlick was among the latecomers mentioned in Dessoir's preface.

has become *impossible*. Only with this is the physics of substance genuinely destroyed." (Ibid., p. 425/28) Modern physics is the physics of fields, although in the realm of quantum theory it is unclear,

> whether it will be possible to sustain the conception of continuity that underlies it. … But the mode and manner in which this theory describes nature contains features so universal, that we may well believe that any future physical world-picture will have to appropriate these same features: explanation, that is, of all events in nature by pure state-quantities, and abandonment of the notion of substance in favour of that of law. (Ibid., p. 426/28f.)

Such an asymptotic extrapolation to the final theory was the basis of Planck's convergent realism. Schlick also resounded Planck's (1925) interpretation of relativity theory. "In every case there exists, for the theory, an objective world, equally real for all observers and common to all of them, which can thus even justly be called 'absolute'." (Ibid., p. 441/43) And apart from the emphasis on invariants in relativity theory, we find the core of Planck's structural realism, the Principle of Least Action – though the praise stood under a strong conventionalist proviso.

> In all the advances of physics it has turned out that, in contrast to many another law, the action principle preserves its validity unshaken; all newly discovered laws of nature, including those of relativity theory, can be regarded as consequences of a principle of least action, which thereby appears to assume the highest rank of formal generality. It is obviously capable of this, because its formulation involves the fewest assumptions about the particular type of reciprocal dependency among natural processes. In regard to these dependencies there is actually a considerable arbitrariness in our choice of views: the one is as legitimate as the other, so long as it does but conform to the idea of a thoroughgoing perfect determinacy of the whole. (Ibid., p. 433f./35)

At quite a few places of the text, Schlick leveled explicit and implicit criticism against Mach or, rather, what he took to be the Machian position; apparently Schlick was still mainly influenced by Planck's reading of Mach and he typically took his teacher's side when mentioning the Mach-Planck controversy. The ideal of describing the physical world without hypotheses, as defended by Mach and energeticism, failed miserably; physicists must form more and more abstract hypotheses "under the inescapable compulsion of the need for unity in our knowledge." (Ibid., p. 446/47) The loss of *Anschaulichkeit*, however, does not have ontological drawbacks. "Physical reality is *not* in fact made up of directly experienceable, observable data, but is merely linked to them, and the physicist has only to take care that this linkage with experience is maintained at any time for *all* observation." (Ibid., p. 446/47) Interestingly, Schlick relegated the final answer to the problem of external reality to epistemology; philosophy of nature can just show that there is no difference between the existence of electrons and the objects of our daily life.

> Any scientific view of reality – like all the rest of our practical dealing in the world – rests upon the assumption that all events in nature take place *according to law.* The claim that this assumption is satisfied constitutes the content of the *principle of causality.* ... Investigation of whether this principle is really universal or necessarily valid is a matter for the general theory of knowledge. The latter establishes that a science of reality is possible, at all events, only insofar as the principle of causality holds. … The validity of causality is thus a presupposition, not an object, of the natural sciences. … But philosophy of nature must certainly get clear as to the *content* of the causal principle. (Ibid., p. 429/31)

Largely recapitulating his 1920 stand, Schlick identified causality with the lawfulness of nature. Causality is empirical because experience alone teaches that there exists no action-at-a-distance. The differential laws of nature are inferred from the observable macro-laws, an act which often yields an ambiguous outcome. Mach's reinterpretation of causality was overtly rejected.

> Ernst Mach … has described the mystical notion, whereby the effect is ejected from the cause as if by some effort of will, as a relic of 'fetishism', and has thereupon repudiated the concept of cause altogether. Following the usage of mathematics, he wished to speak of a 'functional' dependency instead. But this is ultimately a mere change of name; there is no seeing why the real dependencies in nature, as opposed to the purely logico-conceptual dependencies of mathematics, should not be called 'causal', and it should have become clear from our discussion that the concepts of cause and effect can be defined in a manner free from fetishism. (Ibid., p. 434/36)

As in his 1920 paper, Schlick moved on to argue that "uniformity [Gleichartigkeit] of nature is a necessary condition for the concept of law." (Ibid., p. 435/36) His primary example relativity theory was now amended by some considerations about the spatial boundedness and temporal infinity of the cosmos. While considering most steps in the life of the single stars as well-confirmed, Schlick remained skeptical whether stellar and "cosmic evolution can really be thought of as a cyclical process eternally repeated in similar fashion." (Ibid., p. 452/52) Against this stands the second law of thermodynamics, which is "of an entirely new type." (Ibid., p. 452/52) Its merely statistical character also rules out the notorious heat death of the Universe which had prompted Nernst (See Sect. 5.5.2.) to invent an explicit nuclear mechanism to close the cycle of cosmic evolution. Nernst had not accepted the highly improbable strange events admitted by the second law. As Schlick had dropped Planck's stricture of molecular disorder, he could make virtue of necessity on the cosmological scale. However small the probability of such events may be,

> since the universe has any amount of time at its disposal, there will, in principle, be no state of the world that could not recur, none that could not be carried out in reverse. So should anything like a heat-death have one day come about in the universe, then, albeit after immeasurable ages, a new differentiation of the universally undifferentiated state will at length have to recur of itself (by 'chance') …; the processes of nature would then have to unfold in a reverse sequence or direction to that which we are used to in the present state of the cosmos. (Ibid., p. 454/54)

And Schlick also cited Boltzmann's intuition that "if ever in the world, or some part of it, events run counter the entropy principle, past and future have there exchanged their roles." (Ibid., p. 454/54f.)

Apparently Schlick had already arrived at Vienna, yet without ever subscribing to all elements of Vienna Indeterminism. While the latter tradition considered the second law as the primary and most basic law of nature, for Schlick, it remained foreign to reversible physics. And he also followed Planck's reading of Boltzmann according to which irreversibility was not grounded in the laws of nature but in the initial conditions. Since we can directly observe the violations of the second law at the example of Brownian motion, Schlick felt the need to give some lawlike justification to such statistical regularities. Returning to the distinction adopted at the end of his 1920 paper, Schlick concluded that they are a special kind of ontological regularity. "Causality [nomological regularity] prevails only in the temporal direction, not in those of space. There we have laws, but here, initially, only facts. The entropy theorem indicates that even in the realm of facts there are general hypotheses, that in this area, too, it is possible to conceive of general knowledge" (Ibid., p. 458/58) by relating the facts in a particular temporal slice of the world.

To a Machian, Schlick's distinction between laws and facts sounded artificial. To Schlick, it represented an even deeper justification of the fundamental difference between dynamical and statistical laws; although he assumed other ontological regularities, such as the identity of all electrons in the world, that were not of a statistical kind. Interestingly, Schlick did not openly advocate the thesis that probabilities are meaningful only once the nomological determinations are fixed, or Planck's contention that any statistical regularity needed a determinist foundation.

After Schlick discussed the dim prospects to illuminate the area of ontological regularities, "which seems to lie wholly in the twilight of contingency, by means of the concept of law" (Schlick, 1925a, p. 459/59), the thrust of the paper changed, and in the subtext we can discern the author's sudden doubts as to the limitations of the theory just outlined. Notice the terminological shift from *statistical regularity* to *probabilistic law*.

Today there is already serious consideration that even the causal laws are by no means so far-reaching as is generally assumed, and as the reader must suppose after our previous discussion. For when once the statistical approach had been introduced into physics, the idea could arise, that perhaps the *ultimate* regularity of nature is itself statistical in character, that the true micro-laws are themselves laws of probability. … [In statistical mechanics] probability related only to the contingent frequency of the initial states; but if we suppose the ultimate micro-laws to have a probability character of their own, the events themselves then become a contingent matter, they would be removed from causality and cease to be exhaustively knowable. … [In quantum theory] we have to reckon with the fact that this behaviour [of emitting and absorbing atoms] can no longer be understood, say, in an ontological fashion. (Ibid., p. 459/59)

In this way, of course, a Kriesian account of quantum probabilities was blocked. Planck's quantum theory, so Schlick continued, had extended the discontinuities, long familiar already within the ontological realm, into the domain of natural processes. Future physics would have to achieve a reconciliation or decision between continuous field physics and discontinuous quantum physics. This had also been Planck's agenda since the Leiden lecture.

The most recent development in atomic physics of the years 1924/25 propelled Schlick, or so it appears to me, to amend his "Naturphilosophie" with some timely deliberations which in tendency run counter to the overall thrust of the text. They reveal that the 'gigantic breach' as of 1920 had turned into a foundational crisis. If the newest results of quantum mechanics were true, so Schlick contended, Laplace's old dream would be finally over. "The world, in the last resort, would be handed over to chance." (Ibid., p. 460/60) He did not greet this development.

It is clear … that only in the utmost case of emergency will the scientist or philosopher decide to postulate purely statistical micro-laws, since the scope of such an assumption would be enormous: The principle of causality would be abandoned, only an approximate validity would still be retained for macro-processes, while events on the smallest scale would be subject to chance, and hence the possibility of exhaustive knowledge would have to be renounced. The adoption of such a position would not, indeed, be impossible, for a thinker schooled by the empirical sciences will hold neither the causal axiom nor the demand for nature's exhaustive comprehensibility to be absolutely necessary and irrefragable – but still, he will drop these otherwise so well-confirmed presuppositions of all inquiry only if the pressure of facts becomes unavoidable. (Ibid., p. 461/61)

In a paper written a few months later, but published only in 1929 in *Scientia*, Schlick went one step further on his way to Vienna. "Epistemology and Modern Physics" criticized three synthetic a priori assumptions of Kantian and neo-Kantian epistemology: the Euclidean nature of space and time – referring to Weyl (1924), Schlick even contemplated that space-time might have a complicated topology –, the continuity of nature that has been dismissed after Planck's quantum theory, and finally the principle of causality. And Schlick rejected any modified or relativized apriorism, as advanced by Cassirer and other neo-Kantians: "the logical *a priori* is inseparable from the psychological, if it is to characterize a particular epistemological position, namely the Kantian notion that our understanding prescribes laws to nature." (Schlick, 1929, p. 313/95) We can find a similar identification in Schlick's letters of 1920 in which he had convinced Reichenbach to abandon the distinction between two meanings of the Kantian a priori, to wit, 'being necessarily and unrevisably true' and 'being constitutive of the concept of the object of knowledge'. Reichenbach's relativized a priori upheld the second meaning but admitted that the first was untenable after relativity theory. (See Friedman, 1994, p. 23-26, and Howard, 1994) Apparently, it took Schlick five years to apply the same strategy to the category of causality; and yet he still was reluctant to sacrifice the idea that causality is constitutive for physical science. But while his textbook entry had talked about emergency measures, Schlick now grudgingly called it a great progress that science itself defines the domain of its validity.

Even if – like the author – one fails to perceive in the facts available any sufficient basis for this conclusion, it could still become perfectly legitimate if further facts were to hand, and so this case has the following lesson to teach: Although physics is well aware that the causal principle … is a presupposition for its own existence, it still by no means assumes this presupposition to be satisfied *a priori* everywhere, or even in a particular area; it ascertains for itself, rather, *using its own* methods (and with the exactitude of these methods), whether and to what extent this is the case. It establishes for itself, that is, the boundaries of its own kingdom. (Schlick, 1929, p. 313/96)

Of course, it would always be possible in principle to sustain the causal law by suitable *ad hoc* hypotheses. But doing so repeatedly and against growing evidence, science would violate the scientific method as such. Despite the unenthusiastic concessions in causal matters, Schlick's paper ended highly optimistic and much closer to a coherent empiricist position than the "Naturphilosophie".

The relation outlined between modern physics and philosophy could occasion regret that epistemology should cast the anchor of its criterion of truth into empirical science, and thereby partake of its uncertainty and mutability. But if the hope of grounding philosophy on a firmer soil than that of experience and logic must be abandoned (and it has never been more than a hope anyway), this would have to be set off in the bargain against the advantage of having obtained any objective criterion at all. … [T]he empiricist unable to join in the lament … that physics is constantly changing … He knows, rather, that no law till now, *in the sense and with the exactitude* whereby it has once been confirmed, has ever again had to be abandoned. The changeable elements in physics are not the relations of dependency [Abhängigkeitsbeziehungen], which once established, continue to find repeated confirmation, but rather the intuitive ideas which serve for interpretation and interpolation. (Ibid., p. 315/97)

The task of empiricist philosophy, so Schlick continued, is to purify the logical and empirical content of science from the ever changing pictures. For the first time, Schlick spoke friendly about ‘functional dependences’ without considering them inferior to ‘real causal relationships’. Still, Schlick had no theory of causality that could express his final turn to empiricism. And for another five years he would not present one; the very few publications written between 1925 and 1930 do not give us any clue about his changing views. His positive opinion about Herbert Feigl’s Ph.D. dissertation[150] rather suggests that Schlick was still clinging to the view that causality was a distinguished kind of lawful relationship.

7.3. Schlick’s New Theory of Causality

In 1931, Schlick explicitly withdrew his earlier theory of causality and published a long paper in *Die Naturwissenschaften* that was oriented at quantum mechanics as closely as his earlier one had followed relativity theory. Initially he distinguished the concept of causality – or natural law – from the validity of the principle of causality. “The content of the *principle* of causality obviously consists in the claim that *everything* in the world takes place according to law; it is all one, whether we maintain the validity of the principle of causality, or uphold the truth of *determinism.*” (Schlick, 1931, p. 145/177)

Referring to his earlier theory of causality, Schlick sustained that the concept of causality still required comparability of different world regions and a certain ordering of events. This order could either be spatial, in which case the events simply coexist, or temporal, that is, nomological. Although Schlick dropped the Kriesian wording ‘ontological regularities’, he maintained the rigid distinction between spatial and temporal order and a Kriesian notion of probability. But now he renounced all his former attempts to list specific criteria for laws to qualify as causal and plainly declared that “*every* ordering of events in the temporal direction, of whatever kind it may be, is to be viewed as a causal relation. Only complete chaos, an utter lawlessness, could be described as non-causal occurrence, as pure chance; every trace of order would already signify dependence, and hence causality.” (Ibid., p. 146/179) But how to draw the border line between order and chaos? It does not suffice to demand that an order be expressed by a mathematical function because any arbitrary distribution of quantities could be expressed in this way with arbitrary exactness. Upon such a wide definition of causality, the principle of causality would become a tautology.

The most natural restriction is to require that causal laws of nature be universal. Schlick (1920) had thus posed the condition that space and time coordinates must not explicitly appear in them.

The concept of law undoubtedly figures in physics only in such a way that this requirement is always satisfied; … but would it be a necessary condition? … So far as I can see, it would be imaginable, for example, that regular measurements of the elementary quantum of electricity (electron charge) would yield values for this quantity that fluctuated up and down quite uniformly by 5%, in say 7 hours, and then another 7 hours, and then 10 hours, without our being able to find even the slightest ‘cause’ for

[150] The dissertation has recently been published in (Haller/Binder, 1999) and the opinion can be found in the university archive at the University of Vienna.

this; and perhaps there would be another fluctuation on top of this, for which an absolute change of the earth's position in space would be held responsible. The Maxwellian condition would then no longer be satisfied, yet we would certainly not find the world disordered, but could formulate its laws [Gesetzmäßigkeit] and make predictions by means of them. (Schlick, 1931, p. 147f./181f.)

While this example sounded quite Fechnerian in spirit, Schlick subsequently pointed to the purely random or 'uncaused' quantum jumps in Bohr's atomic theory. "As soon as the slightest *simple* claim could be established about these jumps, if the temporal intervals, for example, were to become larger, then it would straightaway appear to us as a regularity, even though time explicitly entered into the formula." (Ibid., p. 148/182) As the criterion of simplicity, accordingly, remains applicable in cases where the Maxwellian condition is violated, it appears to be a better candidate to single out the causal laws among all orderings of events. "But simplicity is a concept half pragmatic and half aesthetic." (Ibid., p. 148/182) In particular, simplicity is conventional, admits degrees, and a less simple individual law might lead to a more simple system of all natural laws (or vice versa).

At this point, Schlick called for a fresh start that departed from the verificationist criterion of meaning in its full-fledged form.

Our mistake hitherto has been not to adhere with sufficient exactness to the actual procedure whereby we actually test, in science, whether processes are dependent on one another or not, whether a law, a causal process, is or is not given. So far we have merely investigated the way in which a law is *established*; but in order to ascertain its true meaning, we must see how it is *tested.* It is quite generally the case that the meaning of a proposition is always revealed to us only through the manner of its verification. (Ibid., p. 149/185)

If we have found, in whatever way, a formula describing our observations, we investigate "whether the formula obtained now also presents correctly those observations which we have *not yet used* in obtaining it. … In other words, the true criterion of lawfulness [Gesetzmäßigkeit], the essential mark of causality, is the *fulfilment of predictions*." (Ibid., p. 149f./185) Although, trivially, the act of verification comes after the establishment of the law, "past and future data have entirely equal rights …; the criterion of causality is not that it holds in the future, but that it holds at all." (Ibid., p. 150/185)

Schlick considered his new criterion as necessary and sufficient. "The confirmation of predictions is thus the *sole* criterion of causality; only by means of it does reality speak to us; the establishing of laws and formulae is purely the work of man." (Ibid., p. 150/186f.) Schlick (1920) had, in contrast, maintained that laws of nature were not made by man, but that man only discovered them. His respective shift in favor of the view endorsed by all Vienna Indeterminists was not mainly a product of radical empiricism but of his move to pragmatics by virtue of which causal laws drastically changed their status. And here the influence of Wittgenstein came into play.

However often a law of nature successfully passes an empirical test, "confirmation of a prediction never ultimately *proves* the presence of causality, but always makes it probable, merely. … From this we gather that a causal claim [Kausalbehauptung] by no means has the logical character of an *assertion* [Aussage], for a genuine assertion must ultimately allow of verification." (Schlick, 1931, p. 150/187) And Schlick credits Wittgenstein for the idea "that at bottom a law of nature does not even have the logical status of an 'assertion', but represents, rather, a 'prescription for the making of assertions'."(Ibid., p. 151/188) Thus causality cannot be defined in such a way that it applies to any given course of events. "Only by reference to the *single case*, the single verification, can we say: it behaves as causality requires." (Ibid., p. 151/188)

> Verification as such, the fulfilment of a prediction, confirmation in experience, is therefore the criterion of causality *per se,* and this is the practical sense in which alone it is possible to speak of the testing of a law. But in this sense, the question concerning the existence of causality *can* be tested. It can hardly be sufficiently emphasized that confirmation by experience, the fulfilment of a prophecy, is an ultimate, incapable of further analysis. It is utterly unstateable, in principles of any kind, just when it must occur; we simply have to wait and see whether it occurs or not. (Ibid., p. 151/188)

The crucial empirical test for causality in these days was quantum mechanics, or more precisely, Heisenberg's uncertainty relation. And Schlick attempted to give common interpretative wisdom – which was oriented at rejecting the Laplacian ideal – a new twist.

> The fact that position and velocity of an electron cannot both be measured with complete accuracy is commonly expressed by saying that it is impossible to detail the state of the system completely at a particular point in time, and hence that the principle of causality becomes inapplicable. For since the said principle claims that the future states of the system are determined by its initial state, and assumes, therefore, that the initial state can in principle be stated exactly, the principle of causality thereupon collapses, since this assumption is simply not fulfilled. I would not wish to declare this formulation as false, but yet it seems to me inappropriate [unzweckmäßig], since it does not clearly express the most essential point. What matters is to realize that the indeterminacy referred to in the Heisenberg relation is in fact an indeterminacy of *prediction.* (Schlick, 1931, p. 152/190)

As Bohr and Heisenberg, and contrary to Schrödinger, Schlick openly assumed that the concepts of macroscopic physics remain applicable in the atomic domain, yet without giving much of a justification.

> Even in present-day physics, it is certainly permissible, with restrictions [set by the Heisenberg relations] …, to say, as a *façon de parler*, that every physical system is to be viewed as a system of protons and electrons, and that its state is perfectly determined by the fact that at every moment the position and momentum of all the particles is known. (Ibid., p. 151f./189)

What Schlick more clearly associated with the Copenhagen interpretation was his belief that Heisenberg's uncertainty relation introduced a limit of principle to quantum mechanical explanation.

> The new contribution made by present physics to the causality problem does not consist in contesting the validity of the principle of causality as such, nor in describing, say, the microstructure of nature by statistical rather than causal relationships, nor in the recognition of a purely probabilistic validity of natural laws having replaced belief in their absolute validity. All these ideas have already been expounded earlier, some of them a long time ago. The novelty, rather, consists in the discovery, never previously anticipated, that a limit of principle is set to the exactness of prediction by the laws of nature themselves. … In earlier times, it was always bound to seem as though the question of determinism would have, in principle, to remain undecided. (Ibid., p. 153/191)

To Schlick's mind, it represented "a great achievement of modern physics to have shown that a theory of such a structure [i.e., a theory that contains its own limits] is at all possible in the description of nature." (Ibid., p. 153/191) As the decision against determinism has been reached by a particular law of nature, it entirely depends upon the validity of the latter. This conditional refutation seems to suggest that the principle of causality is false – as Heisenberg (1927) had explicitly enounced, to Schlick's mind. But in actual fact, laws of nature and the principle of causality do not represent assertions but prescriptions for the making of assertions.[151] So the principle of causality can be neither empirically true nor false. It is not tautological either because physicists constantly discuss its empirical validity. It rather "represents a demand or prescription to seek regularity [Regelmäßigkeit] and to describe events by means of laws. Such a directive is not true or false, but good or bad, useful or idle." (Schlick, 1931, p. 155/196) Quantum mechanics teaches us that the principle of causality is bad within the boundaries set by Heisenberg's uncertainty relation. And as this theory itself is only judged as good or bad according to its current predictive score, "there always remains the hope that with future progress in knowledge the principle of causality may be able to triumph again." (Ibid., p. 156/197) Thus, ultimately, Schlick's farewell to causality was considerably less rigid than the Göttingen-Copenhagen finality creed.

The empirical foundation of the principle of causality also shows that this prescription "is not a *postulate*, in the sense in which this concept figures among earlier philosophers, for there it means a rule to which we must adhere *under all circumstances*." (Ibid., p. 155/196) Schlick here rejected the neo-Kantian strategy to consider the principle of causality as a regulative principle. And he contended that "the so-called problem of 'induction' is also rendered vacuous." (Ibid., p. 156/197) General propositions cannot be justified because they are simply not genuine propositions.

As radical as Schlick's departure from his 1920 theory of causality appeared on the epistemological level, several core features of his analysis of statistical laws persisted with qualifications owing to the changed framework. While Schlick's verificationist theory of causality in itself, at least to my mind, would have been reconcilable with Vienna Indeterminism, the remaining elements of a Kriesian theory of probability and the strong emphasis on the criterion of simplicity prompted Schlick – or so I shall argue in the remainder of this section – to reject the idea of statistical laws and to stick to his Planckian heritage.

Although simplicity was unsuitable for the definition of the concept of causality, Schlick considered it probable that a formula fulfilling the Maxwellian condition and the more general criterion of simplicity

> really expresses a law, an order that actually exists, and hence that it will be *confirmed*. If it is confirmed, we again think it probable that it will also be further confirmed (meaning by this, without the introduction of new hypotheses). … The term 'probability', that we use here, means something quite different … from the concept that is dealt with in the probability calculus and occurs in statistical physics. (Schlick, 1931, p. 151/187f.)

This distinction between logical and physical probability was, to be sure, in stark contrast to Reichenbach's advocating a unified notion of probability by means of which – or so he claimed to the displeasure of his Vienna colleagues – the problem of induction was solved rather than, as Schlick held, dissolved.

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[151] Cf. (Schlick, 1931, p. 155/195); where "Aussage" is now translated as statement.

Due to the intimate connection of simplicity and (logical) probability of confirmation,

[simplicity] *de facto* coincides with the true criterion [of causality], that of *confirmation.* It obviously represents, in fact, the special prescription, effective in our world, whereby the general directive of the principle of causality, to seek regularity, is supplemented. …

Once the purely practical character of the principle of simplicity is recognized …, we are also in the position to understand why 'simplicity' cannot be strictly defined and yet that the vagueness here does no harm at all. If we were to try to draw the simplest curve through the points whereby, in some experiments, the data of quantum processes (electron jumps in the atom, for example) are represented, this would be of no use at all in making any predictions. *And since we also know no other rule* by which we could achieve this purpose, we just say that the processes follow *no* law, but are *random.* So *de facto* there is actually a clear agreement between simplicity and lawfulness, between chance and complexity. (Ibid., p. 156/197f.)

This distinction, in effect, led back to the Kriesian distinction between nomological and ontological regularity that figured so prominently in Schlick's 1920 paper. There are many cases in science where our predictions are constantly confirmed in, say, 99% of all cases while, despite the best means available, we cannot give any cause for the deviant 1%. Such a world, it seems, it nicely ordered and provides a simple way of obtaining predictions that are much more reliable than those of medicine and meteorology. Since natural laws are nothing but prescriptions for the formation of predictions, Schlick cannot deny these statistical regularities the status of 'law'. Still, they represent a "causality of an imperfect kind [unvollkommene Kausalität]." (Ibid., p. 157/198)

It is important to note that such a [statistical] law, wherever we encounter it in science, is regarded, so to speak, as the resultant of two components, in that the imperfect or statistical causality is dissected into a strict regularity [Gesetzmäßigkeit] and an element of pure chance, which overlap. … In the kinetic theory of gases, the laws whereby each individual particle moves are assumed to be totally rigorous; but the distribution of individual particles and their states is presumed at any given moment to be entirely 'lawless'. Combination of the two assumptions yields both the macroscopic gas laws (for example, the Van der Waals equation of state) and the imperfect regularity [Regelmäßigkeit] of the Brownian motion. …

There is another example obviously to hand in Schrödinger's quantum mechanics (as interpreted by Born). There, too, the description of processes is likewise split into two parts: into the lawful propagation of ψ-waves, and into the occurrence of a particle or quantum, which is absolutely random, within the limits of the 'probability' determined by the ψ-value at the point in question. (That is, the value of ψ tells us, for example, that at a particular point, *on average,* 1000 quanta per second are arriving. But in themselves, these 1000 display a wholly irregular distribution.) (Ibid., p. 157/198f.)

Schlick, it becomes clear, was far from granting Brownian motion the status of a genuine lawlike process and, accordingly, he had to deny that it served as decisive evidence in favor of indeterminism – as Vienna Indeterminists did. Moreover, Schlick strictly separated deterministic micro-laws and thermodynamic laws of state, on the one hand, from the lawless initial conditions and fluctuations, on the other hand. No wonder that he also denied a primary character to the second law of thermodynamics and the phenomenon of irreversibility. It is true, the concept of entropy served to "*distinguish* the direction past-future from its opposite; but that the actual course of events *proceeds* in the first direction, and not in the opposite one, is in no way assertible and no law of nature can express it." (Ibid., p. 160/205) Thus he could still maintain Boltzmann's intuition that the direction of time could differ in distinct regions of the universe. Reichenbach, accordingly, was wrong to assume an irreducible temporal asymmetry of the causal relation; this is just "feigned by circumstances connected with the entropy theorem." (Ibid., p. 160/205) This was not the only criticism against Reichenbach.[152] I cannot enter here into further analysis because it would lead deeply into Reichenbach's own theory of causality, the problems of error and induction.

More relevant to the present context is Schlick's attack against von Mises's frequentist interpretation of probability. To Schlick's mind, "the so-called probability distribution is simply the *definition* of total disorder, of pure chance." (Ibid., p. 157/200) But this definition would be correct only for an infinitely large number of observations.

> Since this is impossible, of course, in reality, it remains strictly speaking undecidable in principle whether ultimate disorder is present in any given case or not. … We here have the same difficulty of principle which makes it impossible to define the probability of any events in nature through the relative frequency of their occurrence; for in order to arrive at correct statements [Ansätze], such as are presupposed for the mathematical treatment (the *calculus* of probability), it would everywhere be necessary to proceed to the limit for infinitely many cases – which for the empirical world is naturally a senseless requirement. This is often insufficiently attended to [here Schlick cites (Mises, 1928)]. The only usable method for defining probabilities is, in fact, that which utilizes logical ranges (Bolzano, von Kries, Wittgenstein, Waismann). (Ibid., p. 158/200f.)[153]

We shall in Section 8.2. that this was quite a standard criticism which von Mises typically rebutted by radical empiricism, that is, by considering probability calculus as an axiom system on a par with Newtonian mechanics the point particles of which cannot be found in nature either. This answer was unacceptable for someone, such as Reichenbach, defending a single objective notion of probability that embraced statistical physics and human judgment (probabilistic induction). Schlick, on his part, explicitly separated probability of judgment from physical probability and considered, as did von Mises, the application problem (Anwendungsproblem) as meaningless. Why then did he reject von Mises's frequentism so bluntly?

That the frequentist definition of probability invoked an infinite sequence events was not only a foundational problem but also a very concrete obstacle for a theory of causality and randomness that was based on the single act of verification. It was not a primitive but part of a larger mathematical structure. Von Kries's theory, on the other hand, attributed a range of nomologically possible behavior to the single events, e.g., the individual molecules in a gas. But how could a new version of the distinction between nomological and ontological determinations be reached? Recall Schlick's above-quoted reading of Heisenberg's uncertainty relation. It demonstrated, as an empirically corroborated law of nature, that there were "circumstances which in point of principle could never be observed." (Kries, 1919, p. 6) But this was precisely the situation in games of chance where we could only use probabilities to assess what

[152] Schlick also rejected Reichenbach's approach to problems (as the one above) in which a high percentage of our predictions is confirmed. In those cases, Reichenbach invoked a 'principle of probable distribution' [Prinzip der wahrscheinlichkeitsgemäßen Verteilung] alongside the principle of causality.

[153] Heidelberger (2001, p. 184) rightly cites this passage as circumstantial evidence for the close relationship between the theories of von Kries and Wittgenstein.

was equally possible under the laws of nature.[154] On this basis, Schlick simply rejected the idea that there exist laws of chance. Laws of probability do not represent 'laws' but merely a definition of 'chance', that is, "we are not content with a statistical law …, but frame it as a mixture of *strict* regularity and *total* lawlessness." (Ibid., p. 157/199) With respect to natural laws, it seems to me, this distinction is more basic and more rigid than the former distinction between ontological and nomological regularities. While Schlick (1920, 1925a) had granted the possibility of lawlike ontological regularities, such as the identity of all electrons, Schlick (1931) opposed nomological causal regularity to a lawless kind of ontological coexistence, chance. Notice that although Schlick's (1931) concept of causality was much wider in a theoretical respect, all the restricting conditions of Schlick (1920), simplicity foremost, remained untouched as practical maxims. Thus Schlick's empiricist turn in epistemology, in effect, did not bring his philosophy of nature any closer to Vienna Indeterminism. In particular, Exner's contention that exact laws are the ideal limit of probabilistic laws simply changed from being treated as a violation of the most basic presupposition of scientific research – in terms of (Schlick, 1920) – to an outright category mistake.

It is true, Schlick new theory could easily count all predictive successes of quantum mechanics on a par with other instances of lawfulness – within the limits set by Heisenberg's uncertainty relation. But the idea of strict lawfulness as opposed to pure chance remained as unassailable as Planck had taught. In Schlick's above-quoted description of Born's interpretation the determinist evolution of the wave function is simply contrasted with its non-deterministic interpretation. Within Schlick's conception, the scientist could content himself with this situation as long as successful predictions were possible and once the separation into causal laws and pure randomness had been achieved. Schlick's whole theory thus, at bottom, crucially depended upon the verificationist criterion of meaning. Discussing Heisenberg's disturbance argument (the Heisenberg microscope), Schlick accordingly continued as such.

> But I should prefer to put it more strongly, in complete agreement with what I take to be the incontestable basic viewpoint of Bohr and Heisenberg themselves. If a statement about an electron's position is not verifiable within atomic dimensions, we can attach no meaning to it; it becomes impossible to speak of the 'path' of a particle between two points at which it has been observed. … This can be regarded as a sharpened form of a principle of the general theory of relativity: just as no physical meaning can there be attached to those transformations which leave all point-coincidences – or intersection-points of world-lines – unchanged, so here we may say that it has no meaning whatever to attribute physical reality to the world-line segments between the points of intersection. (Ibid., p. 159/203)

It is true, Schlick applies the verificationist criterion of meaning on an empiricist basis while Heisenberg (See Sect. 2.2.2.) did so on the basis of quite a few presuppositions that would have appeared metaphysical to Logical Empiricists.

If we try to locate Schlick within the dispute on quantum mechanical ontology, we see an interesting division. While Göttingen-Copenhagen insisted on the use of

[154] Neumann, in the same vein, argues that "the development of quantum mechanics up to Heisenberg's uncertainty relation of 1927 could be seen as a paradigm example of the theory of ranges." (2002, p. 328) Unfortunately he wrongly believes that Schlick considered "statistical laws as something final" (p. 376) and thus fails to see that Schlick had basically done, though in a different wording, what a Kriesian turned empiricist, to Neumann's mind, should have done.

classical concepts, Schrödinger called for an entirely new and consistent quantum ontology. In this, Schrödinger not only followed Boltzmann's picture realism but also availed himself of the large freedom in the choice of ontology characteristic of Vienna Indeterminism; it resulted from the close connection between causality and ontology within the context of a given scientific theory. Thus empiricists could accommodate physical ontology to a new law of nature. Schlick's verificationist account of causality, on the other hand, provided a strategy to circumvent such changes in ontology. To search for a universally applicable ontology was less important once natural laws ceased to be assertions. Verificationism provided the means to live quite well with a theory that explicitly described the limits of its own validity – a prospect unacceptable to Schrödinger. This limit permitted Schlick to maintain a distinction in principle between causal laws and randomness; apparent statistical laws resulted from their combination. Moreover, Schlick could easily accept the Copenhagen contention that the ontology of classical physics was indispensable as a basis for quantum mechanical ontology – it was just a *façon de parler* that was admissible if it provided successful prediction. Thus although close allies on the general level, verificationism and radical empiricism pulled into different directions if applied to quantum mechanics. But this division was not the only aspect that made Schlick's position difficult reading for his contemporaries. In the next section we shall see that many physicists were wondering about his treatment of statistical law.

7.4. Reactions and Dialogues

Schlick entertained close connections to many leading physicists of the day. This was not just a consequence that he had obtained his Ph.D. under Planck and was on close terms with Einstein. It also expressed the prominent role the German physics community of the 1920s and 1930s attributed to the philosophical foundations of their field. For those participating in the discourse among the scientist-philosophers, Schlick was an eminent frontman within academic philosophy. In a letter written to Schlick on 11 June, 1919, in which he expressed his fascination about the *Allgemeine Erkenntnislehre,* Max Born went so far as to write: "We [the new generation of physicists] are now forming a community [*Gemeinde*] which has found its prophet – I hope that you will accept this honorable position. After all, it entails no burdens other than those which your philosophical profession anyhow shifts onto you, to wit, to continue the research into the purification and clarification of knowledge." As outlined in Section 5.2, Schlick had become the main philosophical defender of relativity theory in the critical years between 1917-1922. When Schlick published a new theory of causality in *Die Naturwissenschaften* in 1931, this, accordingly, represented an authoritative voice.

In the present section, I shall discuss the reactions to Schlick's new theory by six physicist-philosophers involved in the interpretation of quantum mechanics: Einstein, Heisenberg, Pauli, Born, Sommerfeld, and Schrödinger. The first three had received a carbon copy of the manuscript. At surface value one should expect that assent and criticism to a paper defending – at least at surface value – the Göttingen-Copenhagen position, would divide according to the already established interpretative front lines. Looking through the correspondence one sees that, to a certain extent, this is indeed the case. While Born, Heisenberg, and Pauli – for his standards at least – reacted positively to Schlick, Einstein, Sommerfeld, and Schrödinger expressed their doubts. But this is not the only line of division. We will also find many remnants of the causality debate between Vienna and Berlin which reveal that Schlick was indeed considered as being much closer to the German philosophical context than his Vienna Circle colleague Frank. Most explicit was Schrödinger who, defending Exner's priority, wondered why Schlick had identified such a radical change in matters of determinism. In one respect, however, Schlick unanimously received negative responses, to wit, that he had not fully accepted statistical laws and the relative frequency interpretation.

On 28 November, 1930, Einstein wrote to Schlick:[155]

> I have immediately read your paper and found it essentially correct. Roughly speaking I may put it as such:
>
> Science searches for universal relational assertions [allgemeine Relationsaussagen], which connect possible sense experiences [Sinneserlebnisse] in such a way that these assertions can turn out correct or incorrect, or respectively, are suitable for correct prediction in empiry.
>
> I essentially disagree with you on the following points.
>
> 1. Even quantum theory knows such relational assertions which are not of a statistical nature, but applied to a single instance yield something entirely definite (e.g., energy-momentum theorem applied to an elementary process).
> 2. I do not believe that 'statistical law' is a contradictory concept. It is, to be sure, a limiting assertion which refers to frequent repetition of a set-up which is defined in a very particular way. Whether one wants to call such laws as deterministic or not, is a question of nomenclature. It is common to call them non-deterministic.
> 3. Temporal assertions, to the extent that they can be conceived of as relational assertions, which directly refer to sense experiences are not to play a *special role* superior to relational assertions. (I agree to the criticism against Reichenbach with respect to the non-invertibility of temporal direction.)
>
> From a general perspective, your presentation does not agree with my point of view, inasmuch as I consider your whole conception, as it were, too positivistic. Physics, to be sure, *supplies* relations between sense experiences, but only indirectly. For me *its essence* is by no means exhaustively characterized by this statement. I frankly tell you: Physics is an attempt at conceptual construction of a model of the *real world* and its nomic structure. However, it must represent exactly the empirical relations among those sense experiences accessible to us; but only *thus* it is chained to the latter.
>
> I also admire the achievements of quantum mechanics as coined by Schrödinger-Heisenberg-Dirac, but I firmly believe that one will not want and will not be able to make do with this mode of consideration. For this theory does not furnish a model of the real world. (The elements functionally connected in it do not represent the real world, but only probabilities which refer to experiences [Erlebnisse].)[156] In brief, I suffer from the unclear separation between experiential reality [Erlebnisrealität] and ontic reality [Seinsrealität]. Moreover, I am firmly convinced that one fine day the statistical law as a *basis* of the physical expression of law will be overcome. As already said, I do not share your opinion that the 'statist. law' is no law at all.
>
> You will be astonished about the 'metaphysician' Einstein, but every four-legged and two-legged animal is *de facto* a metaphysician in this sense.

It is remarkable to what extent Einstein was able to enter into Schlick's philosophical train of thought; this was certainly a product of their intense intellectual exchange at

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[155] All letters can be found in the Schlick-Nachlaß; my translation.

[156] "Erlebnis" in another term difficult to translate. I translate it here as "experience" to set it apart from "sensations" which would give it a Machian bias and "observation" which would lead to some confusions in the context of quantum mechanics. This admittedly wipes out to difference between elemental "Erlebnis" and "Erfahrung" in general which is less important for the objective of the present study.

the beginning of the 1920s. Don Howard has studied in great detail how Schlick's (and also Reichenbach's) "development of a new form of empiricism that would force the issue with the neo-Kantians, … chiefly to save relativity … from the clutches of the neo-Kantians" (Howard, 1994, p. 74) led to a parting of philosophical ways with Einstein in the late 1920s. Einstein's general criticism of positivism and his ironical self-description as a metaphysician have been rightly seen as a case in point (Cf. ibid., p. 94). Howard's main evidence for Schlick's change is a detailed comparison of both editions of the *Allgemeine Erkenntnislehre* (1918, 1925b). Yet the drastic changes necessitated by quantum mechanics came after the second edition, and the Schlick of the years 1924-1925 was still wavering – and even published an account (Schlick, 1925a) that came close to a hybrid of two positions – without having a satisfactory theory of causality.

In this perspective, Schlick's (1931) turn must have appeared to Einstein as once again radicalizing a 'positivist' trend without compelling new reasons, or rather, for reasons which Einstein openly rejected because, to him, the present state of quantum mechanics was transitory only. Yet if we take his well-known quest for a realist and not merely statistical theory of quantum mechanics, it becomes even more striking that Einstein, twice, rejected Schlick's contention that statistical laws are not laws at all. Here, so I take it, one of the former core players in statistical mechanics charges Schlick not only of overshooting the mark in the defense of Göttingen-Copenhagen, but also – compare point 2. – of an inadequate notion of physical probability; as most physicists in those days Einstein was quite happy with statistical law referring to the limit of relative frequencies.

Also Heisenberg had received a carbon copy of Schlick's manuscript. In a letter of 27 December, 1930, he emphasized that "he extraordinarily liked its tendency," in particular the distinction between the three possible meanings of causality, that is, whether it is a tautology, an empirical assertion, or just a practical prescription. The first of Heisenberg's critical remarks indicates that Schlick's understanding of statistical law was strange for Heisenberg, too. "The distinction between order, lawfulness [Gesetzmäßigkeit], and 'statistical regularity' [statistische Gesetzmäßigkeit] has not become totally clear to me."

But what means absolutely random within the limits of 'probability'? I cannot see any difference between your 'statistical regularities' and those which we know of in atomic theory. Neither do I see how anything could be still possible in-between 'complete' causality and disorder plus probability 'laws'. What Einstein wants, is, e.g., complete causality in the strictest sense, that is, Einstein hopes that some day it will be possible to predict the moment in time of a 'transition' in the atom on the basis of previous experiments.

I am also a bit unhappy to be always quoted for the phrase of the 'invalidity of the causal principle' [Kausalsatz], as if I would be in opposition to the views of Born. I have given much thought to the word 'invalidity' and wanted to express by it two things: First, that the principle of causality no longer has a domain of *validity* in physics (in the sense as, say, the stamps of 1912 are no longer 'valid') – which is not the same as the claim 'it is false'; second, that a principle that has no domain of validity also cannot be at all interesting. The word 'invalid' seemed to me just to stand in the right middle between 'false' and 'inapplicable', but unfortunately is has always been identified with false. To be sure, for the then common unclear form of the causal principle the word 'false' is not entirely justified. In the meantime, however, Planck has asserted a form of the causal principle which is surely 'false'. Planck thinks[157] that Schrödinger's ψ-function determines a process or a system; it can indeed

[157] For instance in (Planck, 1929b), his response to Schrödinger's inaugural address.

be taken with arbitrary precision from experiment and its future follows from a differential equation. But Planck here completely overlooks that the physical *behavior* of the system does *not* uniquely follow from precisely this ψ-function (or at least not in general). Thus he claims: 'If one knows all determining instances [Bestimmungsstücke] of the system at a certain moment in time, then one can calculate the future.' The antecedent can now be satisfied. The consequent is false if one intends the physical future; it is only correct if one speaks about the ψ-function.

I completely agree to your rejection of Bergmann's [1929] writing and, more generally, of the possibility, believed in by physicists time and again, of synthetic a priori judgments. Planck, von Laue, Kellner, and others still cling to the authority of the good old Königsbergian who would have understood the whole situation long before had be been accustomed with non-Euclidean geometry. This much is correct of these judgments a priori, that it remains possible to make postulates, and, at bottom, Kant has in part thought of these postulates (your 'case III'). But, it is true, these postulates often prove inappropriate [unzweckmäßig] if applied to new domains. …

Here Heisenberg clearly misread Schlick's concept of postulate that was explicitly intended not to represent a Kantian regulative principle. Reinterpreting the disreputed categories as regulative principles was a widespread neo-Kantian defense strategy. Yet it was rather the influence of Wittgenstein's concept of rule that loomed in causality as prescription, not as postulate.

As had Einstein, Heisenberg considered the laws of quantum mechanics simply as probabilistic laws and opposed Einstein's belief in a future deterministic theory. Unlike Einstein, however, he did not see that Schlick in effect was denying matrix mechanics the status of law altogether. And his interpretation of Planck's futile return to determinism took just the form of the standard argument about Laplacian determinism that Schlick had considered as missing the essential point of prediction (Cf. Schlick, 1931, p. 152/190). To Heisenberg, the lesson of Planck's obtaining a false consequent from a true antecedent was that the antecedent had to be denied.

Heisenberg openly expressed his embarrassment about Schlick's procedure of separating complete causality and disorder. In his response of 2 January, 1931, Schlick admitted that the expression "random within the limits of 'probability'" was unclear and pointed to a parenthesis added in the printed version (Cf. ibid., p. 157/198f.). However, the parenthesis just rehearsed Born's interpretation of the wave function.

I was believing that also in all other cases arising in physics such a division of the description into *strict* lawfulness and *pure* chance is carried out or is at least aimed at. … Perhaps I may express my view that has been badly formulated in the paper as such: If it is observed as a final, not further reducible, fact that under certain circumstances an event occurs in a given percentage of cases more frequently that the other observed events, then a 'statistical law' is present. Upper limit: this percentage is 100 (complete causality). Lower limit: this percentage is the same for all possible sequences of events, none of them is distinguished (complete disorder, lawlessness, probability distribution). In the latter case I would consider it misleading to speak of probability 'laws'.

Though perhaps more pointedly, Schlick simply recapitulated his original position and insisted on the division into complete lawfulness and complete lawlessness. Schlick did not hold that strict causality (100%) was the limit of statistical laws; it resulted from the strict lawfulness meshed with randomness once the latter had been tamed or reduced, e.g., when cosmic radiation is screened off in an experiment. Schlick's distinction did not perform well as regards the distinction between atomic processes and macroscopic physics that entered only by way of Heisenberg's uncertainty relation because he in effect understood it solely as a limit of the applicability of concepts. Thus, Schlick had not much to say about the issue that was plaguing quantum physicists, whether the basic laws of nature were deterministic or not. His theory of causality went equally well with Planck's and with Heisenberg's ideas; quantum mechanics could not provide final laws because it did not provide laws at all – here even Planck could agree –, but this was no hobble against its validity because ultimately it were the predictions which counted.

On 5 February, 1931, Wolfgang Pauli wrote a long letter to Schlick which expressed similar misgivings.

I may interpret everything what you say in such a way that I can agree to it. Yet one might also interpret much in such a way that I would have to protest; in short, I think, that you have expressed yourself not precisely and clearly enough on all issues. First of all, I believe, one has to keep apart two problems more clearly as has been done in your manuscript.

A) How does one establish a causal connection between observational data and in which sense does such a connection exist according to the conception of quantum theory?

B) How does one test a definite theory by experiment?

The main difference between the cases A) and B), according to Pauli, was that a causal connection in the sense of A) might be established without knowing the definite form of the law of nature in the sense required for B). Thus Schlick's main criterion for successful prediction, to wit, that the law of nature was applied to predict new data, already required B) instead of just A). Pauli insisted on distinguishing two types of prediction. "'Whenever A, then the same B' is something else than a special functional form 'B=f(A)'. The 'test of a law' is something else than the question whether there exist at all unique connections between certain experimental results." Pauli's criticism made it clear that his notion of causality was very different from that of his godfather Ernst Mach. Nine years ago, the 22-year old Pauli had written to Schlick in a different spirit. "I have thoroughly reconsidered your objections against [Mach's] positivism and I am *no longer* able to accept them as valid. I now consider positivism as an entirely perfect and coherent world view. Of course, it is not the only possible one." (quoted from Meyenn, 1994, p. 41)

Pauli fully accepted the coherence of statistical law. He moreover emphasized that such laws directly emerged from the mutual exclusiveness of certain experimental set-ups and because each measurement introduced disturbances part of which remains undetermined in point of principle. This was nothing but the standard Copenhagen argument.

As regards the characteristics of 'statistical law', I would also want to say that, to me, the partition of such a 'law' into strict lawfulness and complete lawlessness appears neither feasible in general nor advisable [zweckmäßig]. Moreover, for the physicist there cannot exist any definition of probability other than through the number of favorable cases divided by the number of possible cases, *despite* its inherent ambiguity in principle due to the transition to the limit of an infinite number of cases.

Apart from Planck, by 1930 practically every physicist-philosopher had made his peace with the frequency interpretation, setting foundational problems aside. And Pauli even expressed a more fundamental dissatisfaction with Schlick's whole approach of logical analysis.

The difference between 'useless prescription' and 'incorrect assertion' has remained unclear to me. I cannot reconcile myself with the whole direction Wittgenstein (transfer of the logical methods of Hilbert and others from mathematics to the natural sciences); I cannot see the fertility of this method.

This passage, it is true, was not a model of careful and clear distinction.[158]

A transfer of mathematical methods to physics was, of course, no annoyance for Born who had studied with Hilbert at Göttingen. Moreover, the axiomatic method represented the main philosophical instrument of the Hilbert school. On 8 March, 1931, Born wrote to Schlick.

I would like to say to you that your article on causality has given much pleasure to me. Also Hilbert with whom I have spoken yesterday was very satisfied with your paper. He and I are in general appalled by what the philosophers of today are saying and writing – each time when incidentally a philosophical writing comes into our hands, we have the feeling to look into an alien world, farther than the farthermost fixed star. The more we are pleased that you are close and comprehensible to us, even more that you are capable of formulating so crystal-clear those thoughts which in us act more unconsciously underneath the surface of our creative production. Your way of philosophizing is a supplement, yes the crowning of the special research and a guide for life – With other members of the Viennese school of philosophy I do not agree so completely. Let alone the presumptuous tone of some writings, I have many objections in substance, e.g., against that form of positivism which Philipp Frank [1929] has recently advocated in the *Naturwissenschaften.*

Beyond the narrower context of Hilbert's foundational program, Born's short letter clearly demonstrates that German scientist-philosophers felt much closer to Schlick's conceptual framework and to the tendency of his philosophizing than to the Viennese tradition rooted in Mach. *Pace* Wittgenstein, this shows to what extent the Mach-Planck controversy, or the opposition between radical empiricism and post-Kantian criticism, was still shaping the protagonist's philosophical identities.

In the above-mentioned (Sect. 2.1.2.2.) short correspondence with Schlick, Sommerfeld had charged positivism of infertility in science and pointed to Frank. In the same letter of 17 October, 1932, we can also find his criticism of Mach's empiricist conception of causality.

I am not a dogmatist in a religious sense, but I am a dogmatist on the issue of natural laws. I cannot tolerate Mach's 'principle of sloppy laws of nature' (Prinzip der schlampigen Naturgesetze) despite the uncertainty relation.

Schlick responded with some delay on 18 December 1932.

Personally also I do not want to have anything to do at all with the 'principle of sloppy laws of nature', moreover I firmly believe in the objective lawfulness of nature, and presumably the uncertainty relation in principle does not require any change in this respect even though the laws are then of a different kind. In this point there seems to be complete agreement [between us].

While Mach's principle of sloppy laws of nature was, when supplemented with the frequentist interpretation of probability, fully consistent with the notion of probabilistic law, Schlick in his 1931 theory had reserved the term 'law' to strict regularities. And in his rejection of 'sloppy laws', that is when pure chance had not yet been separated off, he was in full agreement with Planck and Sommerfeld who, however, remained skeptical that quantum mechanics was the final word.

[158] Schlick was not to only one who had to stand Pauli's bitter criticism of Hilbert's axiomatic method. Born reports that Pauli called his interpretation of Heisenberg's uncertainty relation as matrix multiplication "useless mathematics" (Born, 1975, p. 300) that was about to destroy Heisenberg's physical ideas.

Schrödinger shared this skepticism, but for different reasons. As we have seen in Sect. 6.3., they concerned ontology rather than the statistical character of natural law. In a letter to Schlick of 25 February, 1931, Schrödinger defended the tradition of Vienna Indeterminism against Schlick's negligence.

I hope you won't get me wrong if I write to you about a little thing, that is certainly not the most important in your article, but that has made me a bit sad. … The turn that recent physics has taken on the subject of causality *has* been foreseen. To be more precise, in Franz Exner's Lectures published in 1919 at Deuticke. Franz Exner has not gone by the cross-roads [of the old and new ideas] without noticing it. He was not at all in possession of those quantum theoretical conceptions which have been created in 1925/26 by Heisenberg-Born, on the one side, and by de Broglie and myself, on the other side, and which we have got used to within the last five years.

With respect to causality, he nevertheless made already precisely that conjecture which today is advocated by the quantum theoreticians and which – perhaps – will be confirmed. It is true, a quantitative version, say, of the kind of the uncertainty relation cannot be found in him. But instead an extraordinarily more profound investigation of the question of acausality from a purely philosophical standpoint.

Whether the new conception of natural law [Naturgesetzlichkeit] can be made probable by very general arguments (as Exner had done) or whether it becomes imperative *only* by the discoveries of quantum mechanics, to my mind, is a question of far greater importance than all petty priority disputes. …

And Schrödinger emphasized that he would not mind if Schlick frankly admitted not to have read Exner's book or regarded his own inaugural address (Schrödinger, 1922a) as mere "popular prattle". He nonetheless attached an offprint of it marking off the initial popular prattle from the core of Exner's thought by a red line.

Personally, the general philosophical reasons for acausality, which were first advanced by Exner and which I have merely repeated in that paper, are almost closer to my heart than the particular ones from quantum theory. For the former are almost infinitely portative [tragfähig] while the latter are not. I even can tell you why not. They rest upon what today is called the 'interpretation' of quantum mechanics. And this interpretation suffers from the disastrous defect that it is hardly reconcilable with the special theory of relativity. I thus expect that it will undergo quite a radical modification some day or another.

We see again how deep-seated Schrödinger's adherence to Vienna Indeterminism was. Even a modified quantum mechanics that would conform to his quest for new universal concepts and coherent pictures, could not lead to a simple return of determinism.

Schlick answered only a fey days later[159] that he had indeed never read Exner's book because he felt fully informed about it through Schrödinger's paper.

It is true, I should perhaps have mentioned Exner, of course particularly since I entirely admire the boldness of his thesis – yet the reasons why I did not do so, come out in my paper with full clarity. … It simply appeared to me – rightly or not – as most characteristic of the present situation that assuming the correctness of the relevant statements of quantum theory "a limit of principle is set to the exactness of prediction by the laws of nature themselves." *Only* this idea I had in mind when asserting that the present turn has not been foreseen by anyone. As far as I know, this idea is not found in Exner.

That modern physics takes acausality into consideration and is inclined to believe into the statistical character of the ultimate microlaws, did not appear to me, frankly speaking, so surprising

[159] The carbon copy bears no date, and I have not found the letter in the Schrödinger-Nachlaß. But Schrödinger already responded to Schlick on 13 March, 1931.

and exceptional. For even though determinism has long been regarded, as it were, as a dogma in physics, nevertheless, the likes of us are well accustomed to doubts against causality from the history of philosophy.

And Schlick mentioned Epicurus' oft-quoted indeterminist atomism. Since Hume, it had become "one of the most familiar insights of empiricism that acausality is not unthinkable at all". After emphasizing again that Exner's "philosophical candor commands respect", Schlick filed a logician's caveat against the thesis that the primary laws of nature could be of a statistical character; six years before, he had expressed in a similar wording his doubts about Planck's uncompromising insistence on determinism, when reviewing Planck's rebuttal of Exner (See the end of Sect. 7.1.).

As a careful logician I am unable to make his [Exner's] reasons for this view [of genuine indeterminism] my own. – Although we know that precisely the natural [natürlichen] laws of macroscopic events which served as a model of causality, in actual fact are of a directly statistical character, it nonetheless does not seem to me legitimate to say that it is really improbable that the microlaws carry a different, namely, a strictly causal character, and that it is much more probable that there do not exist two kinds of laws (for finite size and for the infinitely small). To me it seems that from the structure of the macrolaws one can initially draw no conclusion about the structure of the mircolaws to be assumed; and success alone can show how one has to imagine the latter. It seems that one can equally well assent to Poincaré who states at some point (albeit in a different context) that it is an unjustified prejudice to assume that the microproperties must be somehow similar to the macroproperties. If one looks closer at how statistics emerges in the gas laws, one finds, I think, that there is no reason to assume that it is *more probable* that strictly causal laws do not exist at all. Einstein, as is well-known, considers complete causality still as 'more probable' even under the present circumstances. I only want to say that general philosophical deliberations neither speak for nor against the statistical character of the microlaws, that accordingly only factual confirmation can decide here.

These remarks should only justify why I cannot settle for your position, namely, to attribute almost a higher dignity to the general philosophical reasons than to the empirical ones.

Schrödinger responded promptly on 13 March, 1931. "I have just read your kind letter and reread the last paragraph of § 8, which you indicated to me. And from both together I recognize a much deeper divergence of our opinions than I have assumed." Admitting that a quantitative result is of course more valuable than a merely qualitative one, Schrödinger affirmed that Exner's result was in fact no less empirically testable than Heisenberg's uncertainty relation. And indeed (Cf. 3.4., 4.4., 6.3.2., 8.1., 8.2.), Schrödinger and the Vienna Indeterminists took Brownian motion and radioactive decay precisely as empirical corroborations of their core indeterminacy thesis. Schlick's suspicion that Schrödinger put philosophy above empirical science was hence unjustified.

Schrödinger brushed aside Schlick's reference to Epicurus by pointing to the many absurd theses advanced by the philosophers of that epoch and that Epicurus' ideas were considered as entirely absurd for a long time.

Well, *Hume's* deed is in fact generally acknowledged. But after Hume came Kant and his *tremendous* influence on all philosophy and natural science. Even if you tell me that for a certain group of philosophers since Hume it has remained one of the most familiar insights that acausality was not unthinkable, this insight nevertheless remained without the slightest influence on anyone's conviction that physics was doing right to follow the words of the superior pope [Oberpapstes] on this issue and to reject indeterminism. I do not believe than any among those philosophers has seriously opposed Laplace's claim that from a precise knowledge of the initial state of the physical world it would be possible to calculate in advance the state at every later instant in time. I am not even entirely convinced that Hume himself would have doubted this.

Exner defended a correct thesis that stood against the general opinion of the philosophers of the day. Ending thus the question of priority, Schrödinger expressed his embarrassment about Schlick's rejection of his proof for the probability of Exner's indeterminist thesis as presented in the inaugural lecture (Schrödinger, 1922a).

You are committing the *petitio principii* that I would be talking about *two* things: 1) the macrolaws, 2) the microlaws. ('Law' here is intended in the most general sense; perhaps 'state of affairs' [Sachverhalt], 'form of the events', or something like that would be better.) Moreover, you impute to me the following absurd conclusion: macroscopically we find *regularity* [Regelmäßigkeit], but recognize its merely *statistical* character. Hence it is probable that *the same* state of affairs prevails microscopically too.

The same state of affairs would mean: likewise regularity, likewise in actual fact statistically conditioned, for instance, by a huge number of collisions of extremely small ultra-atoms, say, of the ether.

This is diametrically opposed to the views of Exner and myself. It would instead be the view of some person who, *although* he has encountered a certain variability [Schwankungsbreite] of determinacy in the microscopic realm, nevertheless *à tout prix* would stick to determinacy and to this end invent a strictly causal ultra-mechanism which would account for this variability in a way similar to the molecular mechanism of Boltzmann have accounted for the thermodynamic fluctuations.

But instead we [Exner and Schrödinger] actually claim, precisely *in accordance with* Poincaré's statement, which I know very well, that is it highly improbable that upon sufficiently precise observation in the microscopic realm one would find the same again as in the macroscopic, to wit, regularity. More precisely, not statistical regularity justified by ultramechanics because this would be too naive following Poincaré's aperçu. Not 'true' determinacy either because it would be equally naive to hope that nature had done us the kindness to run a theater play within a domain into which we have not entered so far, a play to which a certain instrument of thought by chance happens to fit just well which he have made up – notoriously wrong – in an entirely different domain. Only not to be forced to put into the corner as useless our cozy little causality [unser liebes herziges Kausalitätchen] and say that nothing came out of it, only that we continue to have some use for it, nature should have done us this favor. But that is really totally obvious. It is almost as naive as if an explorer having initially drawn an extremely inaccurate map of a newly discovered island, tried to find his way around the *next* island he discovers by means of this map. This has been my train of thoughts when considering the 'duplicity' of natural laws as extremely improbable.

As Boltzmann and Exner, Schrödinger rejected the primary character of distinction between macrolaws and microlaws because its only emerged – as a contingent fact about nature – from the large number of particles present in our every-day environment which makes the statistical laws become exact causal laws in the limit. Thus Schrödinger basically rehearsed the classical Viennese criticism of causality as a habit of thought that was used beyond the domain of its validity; in Boltzmann's words, causality thus overshot the mark (See Sect. 3.3.). And he accused Schlick of simply defending the Planckian position – leveled against Exner in 1914 (Sec. 4.4.) – according to which any statistical law required a deterministic foundation, e.g., by atomic collisions or even more microscopic ultramechanism. Schrödinger concluded with some remarks about the relation of entropy and the predictions about a systems future and past behavior that are closely related to his paper on the inversion of natural laws (Schrödinger, 1931).

Schlick responded promptly on 19 March, 1931, being sorry about the many misunderstandings. In particular he never assumed that Schrödinger committed the above-mentioned absurd conclusion.

I was rather thinking of an analogy in the *form of the laws,* which in both cases are based on a different state of affairs. In actual fact, e.g., the macroscopic gas laws are statistically explained by the assumption of lawlessness in spatial direction (hypothesis of molecular disorder), while maintaining causality in the temporal direction; however the quantum laws cannot be reduced any longer, in them causality is dropped. Perhaps one might say that one gets from the state of affairs prevailing in gas theory to the one in quantum theory by transferring the idea of disorder from the spatial to the temporal direction. Excuse my still defective way of expression.

Now as before I do however not believe that on the basis of general considerations one could meaningfully conclude that strict causality is 'rather improbable' and that one can instead only say: from the outset we do not know anything *at all* about the microscopic regularity.

What about if, e.g., the certainly possible case occurs (which Einstein, Planck and others indeed hope to be real) that by new discoveries modern quantum theory together with the uncertainty relation would be overcome by a strictly causal field physics that successfully describes all observations with arbitrary exactness? As your argument for the improbability of finally arriving at strict causality is to be valid with complete generality, you would have to consequently say also in this case: this field theory, however beautiful its differential equations may appear, is definitively not the last word and we must continue research until we arrive at acausality because it is far too improbable that this should not be the case! To me this position would appear unjustified and to the physicists, I am sure, entirely infertile. In brief: we must not be surprised about causality or acausality. Your comparison with newly discovered islands appears to me inappropriate because in this case it would well be meaningful, so I believe, to speak about a probability of whether the islands are similar or not.

Schlick apparently did not realize how radical Schrödinger's indeterminism in effect was; exact laws just emerge from chaos. Notice that in the correspondence with Schlick, Schrödinger defended the original Exnerian stand without referring to the conventionalist argument ending his Berlin inaugural address (Schrödinger, 1929a).

Schlick argued that Schrödinger did not possess any basis for a meaningful probability judgment. Hence the question as to the characteristics of the true microlevel had to be left open. It is true, such probability could not be put on a frequentist basis – let alone on a Kriesian –, and Schlick was right to question Schrödinger's conclusion. Yet paradoxically it was Schlick himself who had insisted on the intimate connection between simplicity and logical probability. Although it was irrelevant for the justification of the concept of causality – albeit crucial for each application of causal laws –, Schlick could have understood that Schrödinger was not only speaking about an ill-defined range of possible islands for explorers but that in effect he was also making a claim about ontological simplicity when rejecting the duplicity of natural laws.

Schrödinger had, of course, to grant that a successful deterministic theory of atomic physics was provisional with respect to the issue of causality; and indeed all Vienna Indeterminists were empiricist enough to do so. Strangely, Schlick even imputed to Schrödinger an a priori preference for acausality; this was an open misunderstanding.

Schrödinger was wrong to assume that Schlick was simply defending Planck's criticism of Exner. Yet something remained of Planck's contention that probabilistic laws required a determinist foundation. This was the Kriesian approach to probability, or rather the more fundamental distinction between ontological and nomological determinations which yielded the rigid distinction between disorder in temporal and spatial dimension by way of which Schlick set apart what all Vienna Indeterminists saw as the very same philosophical problem, namely, Boltzmann's statistical mechanics and quantum theory. This was not to say that quantum theory did not introduce a novel feature: now there was a principal limit on determinism emerging from within the theory itself. In this respect, Schrödinger could well agree with Schlick and accordingly he did not contest the respective clarification in Schlick's first letter. Unlike later interpreters Schlick was careful enough not to wrongly associate Schrödinger's ideas about a better quantum theory simply with the position of Einstein.

After responding to Schrödinger's remarks about the concept of entropy by quoting the respective passages from his "Naturphilosophie", Schlick concluded their correspondence as such.

> At present I do have the time so say and ask more; perhaps at some point there will be an occasion to speak about a few things. When this occasion comes I hope to learn also whether you agree at least to basic conception of causality as advocated in my article and thus assent to those eminent physicists who have already expressed to me, in part with great warmth, that this conception completely coincides with their own. Of course, it would be of great importance for me to know also your opinion about this.

This passage not only shows Schlick's frustration about the negative reaction on Schrödinger's part, but also to what extent Schlick was still better fitting into the German conceptual coordinate system than into the local Viennese tradition in the philosophy of physics. As we shall see in the next chapter, communication with his Vienna Circle colleagues was much easier despite basic differences as regards the issue of causality and indeterminism. The reason, or so I shall argue, was the linguistic turn or rather that the lesson of quantum mechanics was one about the limits of meaningful statements. This was a move Schrödinger would never make.

8. Frank and von Mises: Frequentism and Statistical Coordination

Let me finally come to the second strand of the second phase of Vienna Indeterminism. I shall discuss the positions of Frank and von Mises together because of their common intellectual origin and the many affirmative cross-references in their relevant writings. This intimate connection justifies enrolling the Berlin professor of applied mathematics von Mises – at least for the scope of the present book – in the Vienna Circle. This is not to say that both did not advocate rather diverging ideas about culture and politics: von Mises was politically rather a conservative while Frank was a socialist.

While Erwin Schrödinger entered the University of Vienna in the semester after Boltzmann's tragic death, Frank had still started his Ph.D. thesis under Boltzmann. Yet the opinion for Frank's thesis had to be written by Exner and co-signed by the other experimentalist of the University, Viktor von Lang. Frank's dissertation focused on a purely mathematical topic concerning the Principle of Least Action that was – as he repeatedly stressed – typically absent from the standard treatises of mechanics including Boltzmann's (1897, 1904): sufficient conditions for a minimum of the action integral or the theory of the second variation. Yet both were research fields of Gustav von Escherich and the young Privatdozent Hans Hahn who would become his philosophical ally in the years to come. Frank broadened his mathematical knowledge in Göttingen during the summer semester of 1906 where he studied among others with Hilbert, Klein and Zermelo.[160]

The first pages of Frank's thesis already reveal the direction his philosophical convictions were going to take. Discussing Jacobi's (1866) work on variational calculus, he turned to the notorious issue of the philosophical interpretation of the Principle of Least Action.

After Jakobi [sic!] has stated the principle in a form more precise than ever done before him, he says:

"It is difficult to find a metaphysical cause for the Principle of Least Action, if it is expressed in this true form, as is necessary. There exist minima of an entirely different type, from which one can also derive the differential equations of motion, which in this respect are much more appealing."

One can give the theorem an even more ametaphysical form than Jakobi's by saying: A material point moves according to the Lagrange equations appertaining to the variational problem $J = \int_a^b \sqrt{h - V}\, ds$. This casts off the last remnant of minimum-romance. (Frank, 1906, p. 2)

This was an early statement of Frank's anti-metaphysical attitude. It followed Machian footsteps not only as to the general philosophical outlook, but also by casting Mach's reluctance to admit any philosophical surplus of the Principle of Least Action in a more precise mathematical form. As a student of Boltzmann and working on the topic of sufficient conditions, Frank knew of course very well that Mach's identification of this principle and the conservation of energy was untenable (See Sect. 3.2.). Thus Frank had already found his stand within the coming controversy between Mach and

[160] See his Curriculum Vitae in the *Habilitationsakt* at the Archive of the University of Vienna. More on Frank's early work in (Stöltzner, 2003b).

Planck. As we shall see in Section 8.1., Frank was at pains to strengthen Mach's case by openly admitting the inadequacy of Mach's anti-atomism.

Frank's first paper on causality, however, was written from a truly conventionalist point of view. Inverting a neo-Kantian analysis of Hans Driesch, Frank (1907) considered the general law of causality as a mere convention. This position was clearly at odds with Mach's radical empiricism and, as he later admitted under the influence of von Mises, not in line with a genuinely probabilistic conception. After some publications on variational calculus and two philosophical papers, Frank turned to the special theory of relativity. In 1912, this early work earned him a professorship at the University of Prague as Einstein's successor. The conceptual progress in statistical mechanics led Frank to partly change his view in 1919 and attribute an empirical meaning to the law of causality. Yet he still remained neutral as to whether a deterministic explanation of phenomena such as Brownian motion could still be obtained.

Richard von Mises studied mechanical engineering at the Technical University of Vienna until 1906, obtained his habilitation at Brno in 1908, and became extraordinary professor at the University of Strasbourg in 1909. His early research concerned various areas of mechanics and hydrodynamics, in particular water wheels and the theory of flight. Already at the Technical University of Vienna, von Mises became accustomed with questions of probability calculus through Emanuel Czuber.[161] His first contribution to the field (Mises, 1912) consisted in an elegant reformulation of Fechner's theory of collectives. Right after the war he published his seminal theory of probability (Mises, 1919b) where he defined the concept of collective simply by two conditions and rigorously developed the relative frequency interpretation. Later he gave a broader outline and philosophical defense of his interpretation in the booklet *Probability, Statistics, and Truth* (Mises, 1928a, 1936). In later years, von Mises's interpretation was superseded by the axiomatic theory of Andrej N. Kol'mogorov (1933) which took probability as an implicitly defined basic concept instead of reducing it to the concept of collective. Yet as regards the application of probability calculus to "real events" in the empirical world, Kol'mogorov "to a high degree" (1933, p. 3 fn. 1) endorsed von Mises's position.[162]

From 1920 until his forced emigration to Turkey in 1933, von Mises held the chair of applied mathematics at the University of Berlin. His most important Berlin contribution to the tradition of Vienna Indeterminism was the 1922 paper "On the Present Crisis of Mechanics" (Section 8.2.). The crisis was not pessimistically gazed at as a symptom of coming decline, as Forman reads this article. Rather did Mises propose a statistical approach in order to obtain solutions where classical mechanics failed miserably. Moreover, he explicitly criticized Boltzmann's formulation of the second law as a blend of microdeterminism and macroprobabilism and advocated a purely probabilistic approach instead. In this respect, von Mises's version of Vienna Indeterminism was more radical than Exner's openness in principle or the conventionality of determinism that Schrödinger advocated at places. Exner's and Schrödinger's argument that indeterminism was more probable than determinism was unacceptable for von Mises because he regarded such talk about probability as simply

[161] On the conceptual development from Fechner to Mises in general, see (Heidelberger, 1993, pp. 370-389).
[162] On the development of Kol'mogorov's concept of probability from the context of Hilbert's axiomatic method, see (Hochkirchen, 1999).

meaningless. As we have seen in Section 7.4, Schlick shared von Mises's criticism but he admitted a logical probability of judgments based on simplicity.

On von Mises's account, dynamical and statistical laws did not compete with one another; they simply concerned different observational facts. Moreover, the law of causality obtained empirical content only once it had been specified by means of certain axioms, the differential equations of Newtonian mechanics, the force functions, and the appropriate boundary conditions. Just as the Newtonian dynamical laws govern the motions of point particles, statistical laws deal with mass phenomena which are represented by statistical collectives.

In September 1929 the German physicists and mathematicians gathered for their biennial meeting at the University of Prague. Being president of the organizing committee, Frank had not only arranged that the first public meeting of the Vienna Circle was affiliated to the congress but also that two of three plenary lectures in the opening session were dedicated to the new scientific philosophy. Section 8.3. discusses Frank's (1929) and Mises's (1930a) speeches which later appeared in *Die Naturwissenschaften.* It is completed by von Mises's 1930 rectorial address at the University of Berlin. The three papers show to what extent Frank and von Mises considered the shift to statistical regularity as the most important lesson of the physics of the day.

In the year before the Prague meeting, Frank had taken a stand against the uncritical demand for *Anschaulichkeit,* which, to his mind, rooted in nothing but the adherence to a school philosophy that, in turn, rested upon the outdated mechanistic world view. In both relativity theory and atomic physics physicists had only seen more clearly than ever before that the method of measurement was a relevant problem and that the realistic interpretation of auxiliary concepts involved certain dangers. (Section 8.4.)

In his 1932 book *The Law of Causality and its Limits,* Frank largely revoked his 1907 position as one-sided. (Section 8.5.) He emphasized that his change of mind was caused by quantum theory and by von Mises's conception of statistical laws. Frank investigated in great detail the conditions under which the general law of causality could attain an empirical content, but he came up with a negative result. Yet causality's close vicinity to tautology stood in stark contrast to our continuous and successful application of specific forms of the causal principle. This apparent antinomy, however, dissolved as long as one did not assume the existence of a true world behind the phenomena. Frank's view thus approached Schlick's (1931) new understanding of causality as successful predictions. But there were important differences. Most importantly, Frank remained committed to a single type of lawfulness and thus considered statistical laws as genuine laws. Taking collectives as a possible ontology and holding that all concepts in physical theories are coordinated to specific observations or measurements, Frank could simply argue that the only new feature of quantum mechanics, as compared to Boltzmann's kinetic theory, was the statistical character of this coordination.

The final two sections of the book are dedicated to the rapprochement between both strands of the causality debate. Large part of this development, so I shall argue, was the sharpening Viennese focus on linguistic analysis of scientific theory. Schrödinger who remained committed to Boltzmann's conception of theories as pictures thus, in one of the final dialogues in Berliner's *Naturwissenschaften,* found himself aligned with von Laue and against von Mises (Section 8.6.). On the 1936 Copenhagen Congress for the Unity of Science, Bohr's concept of complementarity provided the basis for a far-reaching rapprochement between Frank and Schlick (Section 8.7.). The reason was not only that Frank had meanwhile adopted the physicalist ideas of his Vienna Circle colleagues, but also that quantum mechanics had to be defended against a flood of metaphysical misinterpretations. (Section 8.8.)

8.1. Frank's Early Views on Causality and Statistics

One of the first philosophical papers of a later member of the Vienna Circle was Frank's "On the Law of Causality and Experience". He argued that "the law of causality, the foundation of every theoretical science, can neither be confirmed nor disproved by experience; not, however, because it is an a priori true necessity of thought, but because it is a purely conventional definition." (Frank, 1907, p. 444/63) In order to prove this pointed thesis, Frank adapted an argument which the biologist-philosopher Hans Driesch had devised to establish the a priori character of the law of energy conservation. This already suggests that Frank to a certain extent discussed the issue of causality within a Kantian frame of thinking in which the a priori category, however, had been replaced by a convention. And, indeed, Frank contended that

> the latest philosophy of nature revives in a striking way the basic idea of critical idealism, that experience only serves to fill a framework which man brings along with him as a part of his nature. The difference is that the old philosophers considered this framework a necessary outgrowth of human organization, whereas we see in it a free creation of human arbitrariness. (Ibid., p. 447f./66)

Mach and Boltzmann, who are not mentioned in Frank's paper, had been considerably more empiricist. Frank's conventionalism replaced their naturalist epistemology according to which our common habits of thought were simply a product of man's successful adaptation to nature. As had Kant's, Frank's theory of causality remained on the conceptual level. The tacit dismissal of any Darwinist or Lamarckian justification of epistemology would remain one of those tenets by which Logical Empiricists severed the ties with the local Viennese tradition.

Frank discussed the law of causality in the following form: "If, in the course of time, a state of the universe *A* is once followed by the state *B*, then whenever *A* occurs *B* will follow it." (Ibid., p. 444/63) In this form, strictly speaking, it was applicable only to the Universe as a whole. "In finite systems, the law of causality is the more nearly valid the larger the system. In the application of the law to a finite system, the answer to the question whether the system is large enough depends on the degree of accuracy required for the occurrence of the predicted effect." (Ibid., p. 444f./63) Interestingly, Frank held that "the so-called inductive method has been developed" (Ibid., p. 445/64) to reach a fallible and provisional decision as to whether a substitute of the general law of causality can be applied. But even for systems, e.g., in astronomy, where it can be applied, a more basic problem arises. The crucial point in each form of the law of causality is the arbitrariness in the definition of 'state'. "If the law of causality is not valid according to one definition of the state, we redefine the state simply in such a way that the law is valid. If that is the case, however, the law, which appeared to be stating a fact, is transformed into a mere definition of the word ‘state’.” (Ibid., p. 446f./65) Hence the general law of causality can always be fulfilled and becomes a mere convention.

If I wish, I can provide all bodies with state variables that are all qualitatively different, in order to fulfill the law of causality. I can regard heat, electricity, magnetism, as properties of bodies, essentially different from one another, just as is done in modern energetics, and as Driesch does. On the other hand, if I wish, I can get along with less qualitatively different properties. For example, I can introduce only the motion of masses; but then, in order to obtain the necessary diversity, I must take refuge in uncontrollable hidden motions. This leads to the purely mechanical world view, which Democritus dimly conceived as an ideal, and which occurs mostly in the form of atomism. (Ibid., p. 448/66f.)

Arguments similar to Frank’s correspond to adding so-called hidden variables in order to retain a completely determined – though unpredictable or even chaotic – behavior of the quantum particles at the price of adding in-principle unmeasurable quantities. Frank, on the contrary, was at pains to avoid any ontological commitment – either to Boltzmann’s property ascription to universal theoretical entities or to Mach’s universe of relatively stable facts. Although he remarked that some world view would be more simple than another, he opted neither for Boltzmann’s ontological simplicity nor for Mach’s principle of economy.

The important role of Mach within Frank’s early thinking only emerged in later publications. In 1910, he reviewed Planck’s (1908a) Leiden lecture for the *Monatshefte für Mathematik und Physik*[163]. Frank held that Planck’s attack essentially arose from various misunderstandings. In particular, one could well maintain simultaneously that our world view is an arbitrary creation *and* that this, nonetheless, reflects natural processes independent from us. The real conflict, according to Frank, was that Planck – in contrast to Mach – assumed that our present physical world view possesses some lasting traits, which are counted as real. Frank also rejected introducing metaphysical realism as a guiding postulate; “it is even less admissible to repeat now what had happened with God, freedom, and immortality in favor of atoms and electrons.” (Frank, 1910, p. 47) And he held that even Mach would have considered energy conservation as real, once the quantities of all the single energies had been specified. As a general law, however, it was merely a convention whereas Planck considered it as the most important guiding principle of scientific research.

In a review of the third edition of Planck’s historical and systematic study on *The Principle of Energy Conservation* (Planck, 1908b), Frank was even more direct. To an argument of Planck in favor of the empirical character of the general law he retorted: “There is still a breach through which the skillfully expelled ‘conventionalism’ can intrude into this [general] form of the energy law and this lies in the concept of ‘the same state’.” (Frank, 1916, p. 18) This was precisely the point by which Frank (1907) had justified the conventionality of the law of causality. In the interview with Kuhn, he recalled the story as such:

Yes I knew Planck a little. Concerning science, the most I ever had to do with him related to this book which he wrote about the first law of thermodynamics. [Planck, 1908b] … His main point was to refute Poincaré and similar authors who had held that the first law was tautological. He formulated it in such a way that it was not, and I rejected this. His main argument was, and this is often repeated,

[163] The *Monatshefte für Mathematik und Physik* was the house organ of the Institute of Mathematics of the University of Vienna. So it is quite natural that Frank and Hahn published there several “Literaturberichte” (reviews), but apparently Uebel (2000) was the first to notice this.

that the first law has to be formulated in such a way that if you have a cyclic process, then by a cyclic process no weight can be lifted. And I said this is not sufficient, because what is a cyclic process? It is one which will restore the same state. And what is the same state? Something remains the same if no weight has been lifted. And this is also a kind of tautology. (from Blackmore, 2001, p. 65)

Frank was, of course, wrong to believe that Planck's target was Poincaré's conventionalist interpretation. The first edition of Planck's book had appeared in 1887 and thus more than a decade before Poincaré started his philosophical works. To be sure, Planck's principle-oriented view at the laws of thermodynamics was in contradiction to the views of Poincaré as it was to those of Mach. (See Sect. 3.6.) Frank's conventionalist interpretation of the law of energy conservation was fully in line with his interpretation of the more fundamental law of causality. But it was at odds with the views of both Mach and Boltzmann. After a short correspondence in October 1893, both had agreed that the law of energy conservation "has no other evidence than an empirical law." (Höflechner, 1994, p. II 204; see Sect. 3.3)

Both reviews of Planck formed the basis of Frank's commemorative article on "The Importance for Our Times of Ernst Mach's Philosophy of Science" which he viewed in Mach's having adapted the great project of Enlightenment to the present. As a main tenet of Enlightenment philosophy Frank considered "the protest against the misuse of merely auxiliary concepts" (Frank, 1917, p. 70/80) as an absolute foundation of physics and philosophy, because this bore the danger of conceiving any change in the foundations of physical theory as a bankruptcy of the scientific world conception as a whole. Of course, each epoch creates its own auxiliary concepts which may in turn transcend their own domain of definition. "The work of Mach is therefore not essentially destructive, … but on the contrary it is an attempt to create an unassailable position for physics" (Ibid., p. 68/75) despite the constant change of theories. Not entire parts of theories, as Planck held, will become lasting truths, but only the functional dependences between the phenomena will remain.

The known connections among phenomena form a network; the theory seeks to pass a continuous surface through the knots and threads of the net. Naturally, the smaller the meshes, the more closely is the surface fixed by the net. Hence, as our experience progresses the surface is permitted less and less play, without ever being unequivocally determined by the net. (Ibid., p. 66/72)

To this network of interconnections are the point at which the later Vienna Circle would apply their logical analysis of scientific theory. A theory corresponded to a network of logical relations the knots of which were coordinated to possible observations.

Frank did not posit any analog of Mach's principle of uniqueness in order to guarantee the integrity of the facts constituted by this network. Instead, he affirmed that all our theories were empirically underdetermined and contained an irreducible conventional element. Frank also took a stand on atomism. Emphasizing that Mach, above all, strove after concepts that were applicable in all sciences, he concluded as such.

I will not deny that Mach allowed himself to be misled by this argument into attacking the use of atomistics in physics more sharply than can be justified. After all, the usefulness of the atomic theories in this limited realm is certainly indisputable. His followers, as is generally the case, often saw in this weakness of the master his greatest strength. … I believe that one can completely free the nucleus of

Mach's teachings from this historically and individually conditioned aversion to atomistics. (Ibid., p. 68f./77f.)

Frank's paper was not the official obituary for Mach published by *Die Naturwissenschaften.* One may rather assume that it represented a Viennese attempt to correct the picture that had been drawn in the year before by the Jena physicist Felix Auerbach. "Ernst Mach's Lifework" mainly criticized Mach's anti-metaphysics as an unfulfillable promise. Mach's "fear of metaphysics" (Auerbach, 1916, p. 181) prevented an appropriate reception of his metaphysical thinking that was superior to other types of metaphysics. According to Auerbach, Mach's philosophical "system" rested upon two pillars: the principle of economy and that 'the given' consisted of elementary sensations. As the motivation of the principle of economy, Auerbach apparently regarded the Principle of Least Action – which he called "principle of the least quantity of force". This showed, to his mind, that great care was required when applying the concept of economy, even more when extending it to intellectual processes. Summarily, he assented to Wundt's classification of Mach as an "inverse Kant" (Ibid., p. 181).

While Kant however at least attempted to deduce the categories from general functions of thought, in Mach the principle comes like a shot, almost as a teleological maxim; and similarly as Kant … Mach comes from the a priori given to experience. And this is surely metaphysics, even though critical metaphysics. (Ibid., p. 179)

Planck, too, had charged the principle of economy of being metaphysical in kind (See Sect. 3.7.). Although Auerbach, in contrast, properly appraised Mach's neutral monism, using a Kantian perspective as comparison rendered Mach, at bottom, a sensualist according to whom sensations were the "final elements" (Ibid., p. 181). This interpretation missed the flexibility present in Mach's notion of element and the subtle interrelation between facts, or stable complexes, and the elements they consist of (See Sect. 3.1.). Hence in effect Auerbach came rather close to Planck's criticism. But concerning the classical polemics between Planck and Mach he took an intermediate position and rejected Planck's convergent realism in favor of the idea of theories as pictures. Auerbach wondered whether Planck was not aware himself

that his electrodynamical-thermodynamical theory including its quantum consequence … is, on the one hand, too special and, on the other hand, too complicated to count as an ultimately satisfactory system. … Doesn't he, who has grown up under the wings of Helmholtz and Kirchhoff, and who has acted aside Heinrich Hertz know best himself that all that are just pictures which we make of the world … in order to arrive at coherent knowledge? (Ibid., p. 181)

And Auerbach continued to wonder how Planck could have condemned wholesale Mach's *Theory of Heat,* "a book from which so many mature scholars and thousands of maturing younger scholars have learnt infinitely much." (Ibid., p. 181) On the field of historico-critical-epistemological investigations Mach was "the qualified if not the only existing guide". (Ibid., p. 182)

Frank's 1919 paper on "The statistical approach in physics" published in *Die Naturwissenschaften* marked a first change of his view on causality owing to the new statistical setting. He commenced from the same definition of the principle of causality as in 1907 and again approached the problem by an analysis of the notion of state.

The present state of a closed system of bodies uniquely determines its future state, that is, whenever the system reaches the state A, a particular state B follows. … If one understands by state the sum of all *physically measurable* properties of the system, the law of causality has no validity. In the sense of molecular theory one must rather add to the description of the state also the positions and velocities of all molecules, by means of which the law of causality is saved, but its actual application becomes impossible. (Frank, 1919, p. 727)

Frank's main justification for the factual invalidity of the law of causality, revoking thus his earlier radical conventionalist thesis, came from Brownian motion. In the theory of gases the number of molecules is typically so large that highly improbable events, such as a spontaneous departure from equilibrium, practically never occur. While for gases the invalidity of the law of causality is only inferred theoretically, in Brownian motion we observe these spontaneous density fluctuations. Thus, "in the realm of the empirical-physical, the experimentally measurable quantities, … there exists no causality." (Ibid., p. 728) It remains, however, possible to establish an average law, Smoluchowski's law of diffusion for the Brownian particle. Brownian motion had been also Boltzmann's and Exner's case in point for a genuinely indeterministic physical world picture. But like von Smoluchowski (1918), Frank did not fully subscribe to indeterminism yet; although empirically irrelevant, it remained possible to formulate the notion of state in "a molecular-deterministic fashion," (Frank, 1919, p. 728) so that the law of causality was upheld. The same was true in history where a similar macro-micro distinction presents itself.

The law of causality [in history] does not require at all the existence of historical laws. It might well be that the properties by which the historian describes groups of nations do not suffice in principle to fulfill the law of causality, and that there exists no historical, but only an individual psychological causality. (Ibid., p. 728)

Summing up, Frank's outline of the "statistical conception of nature" (Ibid., p. 701) shared the core tenet of Vienna Indeterminism, the separation of causality and ontology. Moreover, Brownian motion is understood as empirical evidence that the reduction of statistical laws to dynamical laws is impossible. This had been precisely Exner's empiricist rationale against Planck. (Sect. 4.5.) Exner had, more generally, considered the very fact that all natural processes are directed as the starting point of our understanding of nature and thus the second law of thermodynamics as the basic law of nature. Right at the beginning of his paper, Frank characterized "the tendency of assimilating all distinctions" as the most characteristic trait of natural processes and as a "brazen law" (Ibid., p. 701) that originates in a game of chance. For this result he credited Boltzmann and von Smoluchowski. Despite his far-reaching assent to Vienna Indeterminism, Frank did not subscribe to so radical a probabilism as Exner. And although he applied the frequency interpretation when explaining the concept of probability, he did not yet derive clear-cut ontological consequences from it. He would do so only under the influence of von Mises (see Sect. 8.2.).

How does Frank's early thinking about causality relate to Schlick's? First of all, Frank discussed statistical mechanics while Schlick's (1920) theory was exclusively oriented at relativity theory. And in contrast to Frank, Schlick remained committed to the idea that only dynamic laws were genuine laws. Second, Frank did not address the question as to which conditions a law must fulfill to count as causal and he based his considerations solely on the concept of prediction. Thus, Frank's (1907) starting point was the same as that of Schlick's (1931) second theory. Frank (1932) would broadly study the conditions under which the law of causality became empirically meaningful.

8.2. Von Mises on Probability and the Crisis of Mechanics

Von Mises's 1922 article "On the Present Crises of Mechanics" set out by distinguishing two strands of mechanics that differed in content, not just in method. 'Bound mechanics' contains all mechanical problems in the classical physical sense that can be subsumed under a single variational principle. 'Free mechanics' denotes all theories that are consistent with the wider framework of Newton's axioms and that are specified by arbitrary force functions. Also general relativity, von Mises continued, seems to be expressible as a part of free mechanics, but the force functions admitted are of a highly restricted type.

> It seems to us that the mechanics of relativity is much more absolute or 'absolutistic' than the usual one, 'more bound' in our words. … Perhaps here one finds part of the reasons which have induced *Ernst Mach* in his posthumous 'Optics' to reject relativity theory so firmly from the standpoint of experience (Mises, 1922a, p. 26).

Here von Mises touched upon the absolute character of the metric which for Planck signified the main virtue of relativity theory. (Sect. 3.7.) Taking 'bound mechanics' as the paradigm of causal explanation, von Mises's diagnosis that general relativity was more 'bound', corresponds to Schlick's view that Einstein's theory extended to domain of causality (Sect. 7.1) Although von Mises associated 'bound' mechanics with a variational principle, or rather with forces for which such a principle could be formulated, his distinction did not agree with Planck's more global distinction between reversible physics based upon a variational principle and irreversible physics. To the Viennese von Mises, this distinction was not of a fundamental kind and thus 'free' mechanics was embracing both reversible and irreversible phenomena.

Granting Planck that general relativity was also more 'absolute' but remaining simultaneously committed to Mach's epistemology, von Mises had to find a way to deal with the notorious preface of Mach's *Optics* (1921) that had appeared just in the year before. Apparently, he did not possess a better argument than to call for mitigating circumstances. Other than Frank (1917) who had openly admitted the failure of Mach's anti-atomism and tried to detach Mach's epistemology from it, von Mises's wording revealed surprisingly persistent misgivings against atoms. "I want to clearly emphasize that I do not think of hypothetical molecules, electrons, α-particles and the like, but that I have in mind only phenomena of motion and equilibrium at sensorily perceptible masses." (Mises, 1922a, p. 28) Indeed, von Mises studied the main question of his article, to wit, whether the framework of 'free mechanics' suffices to explain all observable phenomena of motion and equilibrium, by purely classical examples. Both turbulence phenomena in liquid media and Brownian motion taught us that no satisfactory result was obtained unless one resorted to statistical methods which, in the first case, yield a phenomenological theory whose degenerate system of equations "provides the welcome opportunity to adapt the theory to observations." (Ibid., p. 27) While in this case von Mises argued almost like an engineer, in the case of Brownian motion he took the position of a methodological purist and charged the theories of Einstein and Smoluchowski of

> the intolerable contradiction that the course of events at one time was considered as uniquely determined by physical or mechanical laws, while one subsequently believed to be able to reach results about this course from a completely different angle. This contradiction particularly comes to light in Boltzmann's version of the kinetic theory of gases (which, however, deals with the hypothetical molecules and not with observable masses, so that it can serve only as an analogy here) where one calculates first the velocity changes according to the laws of elastic scattering and then thwarts these calculations by purely statistical considerations. (Ibid., p. 29)

In kinetic gas theory this connection between the deterministic microlevel and the probabilistic macrolevel was established by "the notorious ergodic hypothesis." (Ibid., p. 29) It is instead more coherent to pursue a thoroughly probabilistic approach. As in Exner's case, the frequentist account of probability permitted von Mises to furnish the probabilistic laws with a suitable ontology, to wit, mass phenomena which become an independent object of physical theorizing in the same vein as Newtonian point particles. "Probability calculus is part of *theoretical physics* in the same way as classical mechanics or optics, it is an entirely self-contained theory of certain phenomena, the so-called mass phenomena, irrespective of whether they are of mechanical, electric or other nature." (Ibid., p. 28) This calculus maps initial probabilities, which play the combined role of the force functions and initial values of 'free mechanics', into other probabilities without ever yielding deterministic results about single processes. The burden of finding and verifying these probability distributions remains with the empirical sciences. Thus, statistical physics "never directly competes with a result of mechanics or of the rest of deterministic physics."(Ibid., p. 28)

By attributing to probability calculus its own domain of facts, mass phenomena, von Mises could maintain the strict separation between deterministic physics and statistical physics. This avoided Exner's radical outlook that, most likely, all deterministic laws were, in actual fact, indeterministic. This compartimentalization of physical ontology according to the type of law seemed to be the price paid for von Mises's staunch rejection of atomism and his continuous criticism of Boltzmann's reductionism. Thus, von Mises read Boltzmann rather in the Berlin fashion as an opponent of Mach. This made him overlook many Machian traits in Boltzmann's and Exner's radical probabilism; and it was, perhaps, a consequence of not being a student of the Vienna Institute of Physics.

Nevertheless, von Mises endorsed core tenets pertinent to Vienna Indeterminism: firm empiricism and the rejection of any a priori category of causality foremost. And he considered Brownian motion as a decisive case in point for indeterminism.

> It is entirely irrelevant whether we stick to the assumption that the orbits [of gas particles] *would be* determined if we knew the exact initial conditions and all influences; since we have no prospect of ever achieving this knowledge, this is an assumption of which it can never be decided whether it is true or not, hence an unscientific one. (Ibid., p. 29)

Mises quite generally believed that such unanswerable questions could be excluded in the course of scientific progress.

A major driving force of both Exner and von Mises was their unequivocal endorsement of the relative frequency interpretation. Besides this, von Mises arrived at the core convictions of Vienna Indeterminists on a slightly different route. Without referring to Exner's synthesis, he directly went back to Mach's radical empiricism turning it, as Exner, into an argument in favor of genuine indeterminism. Yet he phrased the problem of equipping statistical laws with a suitable ontology in an entirely different way. Exner and Schrödinger had believed that indeterminism became preferable – or more probable –, in virtue of the theoretical universality of atomism – taken as a Boltzmannian picture – and the macro-micro distinction – understood as a distinctive characteristic of physical phenomena. Yet the Machian von Mises was reluctant to count on an unobservable microworld even if this chaotic world was not bound by physical law. In his inaugural address, Schrödinger (1922a) argued against ontological duplicity and for conceptual purity. If one understood this as an argument for ontological parsimony, von Mises could well agree, and in later years he would explicitly stress this point (Mises, 1930b; see Sect. 8.3.).

Other than Boltzmann, Exner, and Schrödinger, von Mises was not after universal pictures but rather a satisfactory mathematical framework that could be coordinated to observation. And thus he followed the core step of Logical Empiricism: rigidly distinguishing mathematical theory and empirical observations. In this way, he could hope to avoid the usual ontological quibbles altogether. This was an important move that introduced a new dimension into the tradition of Vienna Indeterminism. It was not decisive in itself because, for instance, Schlick's advocacy of it did not propel him to accept genuine indeterminism; rather did he use the strict separation between two languages to simultaneously accept the Copenhagen interpretation of quantum mechanics and cling to the idea that only deterministic laws were genuine laws.

A comparison of "The Crisis of Mechanics" with his first paper about the philosophical consequences of his relative frequency interpretation of probability teaches us. This shows that a substantial part of the motivation for indeterminism consisted in his successful formulation of a genuinely indeterminist theory of Brownian motion (Mises, 1920b).[164] In the year before, von Mises (1919a) had still been more reluctant to take a stand on the issue of indeterminism in physics. The aim of his first paper in *Die Naturwissenschaften* was to refute the philosopher Karl Marbe's claim that probabilistically distributed events harbored an inherent tendency of equilibration. He commenced by distinguishing concept formation in philosophy, which starts out from everyday language, and in the sciences, which rest upon exact but arbitrary definitions within a partially or fully axiomatized theory. Thus 'probability' in everyday parlance, our subjective degree of certainty, is sharply distinct from its mathematical homonym.

Von Mises' definition of probability as the limit of the relative frequency of a property within an infinite series presupposed that this series forms a collective. There are two conditions for a collective: (i) The relative frequencies of the occurrence of the property converge to a limit. (ii) "If out of the whole series of elements one forms a subseries without using the differences between the properties in the subseries to be selected, then within this subseries the relative frequencies for the occurrence of the

[164] For a detailed analysis of von Mises's theory of Brownian motion, see (Hochkirchen, 1999).

properties possess the same limits as for the whole series." (Mises, 1919a, p. 171; italics dropped) This second condition is called 'irregularity of coordination' or 'impossibility of a gambling system'.

According to von Mises, the first condition is based on our manifold experience that in lotteries, birth rates, etc. the relative frequencies become more and more stable as the observed series gets longer. In those days empirical investigations into such simple phenomena were still very common; Frank (1919, p. 704f.), for instance, reported in detail a statistical investigation of the number of pedestrians occupying a small strip of the sidewalk. Since Poisson, the empirical fact of the convergence of relative frequencies was often called the law of large numbers. But, as von Mises demonstrated in a later paper that is largely identical with a part in *Probability, Statistics and Truth*[165], this terminology was ambiguous because Poisson also used it for a particular mathematical theorem that generalized a result of Jacob Bernoulli. It states that the probability p for an experiment repeated n times to lie within $[pn\text{-}\varepsilon n, pn+\varepsilon n]$ (ε a small positive number) converges to 1 as $n\rightarrow\infty$. Von Mises showed that if one stays within the realm of classical a priori probabilities, this theorem is of purely algebraical nature and does not permit any conclusion about actual experiments. Adopting the frequency interpretation, however, it yields a valuable statement about "the *order* of the experimental results" (Mises, 1927a, p. 501) or "about the course of the phenomena" (Ibid., p. 502) that transcends the empirical law of large numbers which had only concerned the existence of the limit. In order to derive Poisson's theorem, one has to assume the irregularity condition. While in his 1919 paper he had argued that his second condition was hardly accessible to direct empirical observations – but derives its empirical support mainly from the manifold experimental corroborations of the multiplication rule of probability which can be derived from it – he now provided an analogy from physics. "As modern physics has deduced from the failed attempts to construe for centuries a perpetuum mobile the valuable energy law or the principle of the excluded perpetuum mobile, so we have to avail ourselves of the experiences of the system players in the casinos." (Mises, 1927a, p. 501)[166] The analogy presupposed a Machian reading of the principle of energy conservation and contradicted Frank's 1907 conventionalist account. And combined with von Mises's mathematical results, it provided a justification for a core theme of Exner's inaugural address, to wit, that the law of large numbers was the empirical meta-law basic to all science. Condition (i) alone did not suffice.

While in the "Crises of Mechanics" von Mises (1922a) considered probability theory on a par with mechanics and attributed to it its own domain of facts, in 1919 he was still holding that in the application to theoretical physics "the connection between probability theory and reality is not so immediate [as in games of chance or population statistics] because theories of physical nature lie between them." (Mises, 1919a, p. 173) Instead he compared probability calculus to geometry because probabilities are calculated from given probabilities; but "the determination of the initial collectives of the calculation does not belong to the tasks of probability calculus in the narrow sense." (Ibid., p. 175) Similarly the procedures of determining the base length and the angles of the triangles do not belong to geodesy itself. Pure geometry, he continued, corresponds to the games of chance.

[165] To wit, the first half of the fourth lecture in the German original (Mises, 1936, pp. 129-143).
[166] This analogy also appears in (Mises, 1930a, p. 148).

Von Mises's second analogy was less far-fetched and could count on an important voice. Hilbert's sixth problem (1900, p. 272f.) had declared geometry as the pattern of the axiomatization of empirical theory. Hilbert's agenda contained the axiomatization of probability theory in order to reach a rigorous formulation of the theory of gases. As for both Mach and Hilbert geometry was undoubtedly an empirical science, it was only a short step for von Mises to subsequently consider mass phenomena as the ontology suitable not only for societal, but also for physical probabilities. His 1919 paper still envisaged probability theory predominantly from the mathematical side and left the specification of the probability distributions and statistical collectives to the empirical sciences. But in 1922 he found that this question was decisive for the scientific import of probability calculus and for an ontology suitable to statistical physics – even more after quantum mechanics had won favor by the end of the decade.

Von Mises's analogy between geometry and probability was repeatedly criticized by other Logical Empiricists. On the first meeting on "Epistemology of the Exact Sciences" co-organized by the Vienna Circle and the Berlin Society for Scientific Philosophy in 1929, there was an entire section and a broad discussion about probability that was later documented in the first number of *Erkenntnis.* During the discussion, Reichenbach argued that

> in the coordination of a physical body to a mathematical theory the notion of approximation occurs which contains the concept of probability…: within certain limits these physical objects correspond *with high probability* to the mathematical axioms. Thus, the problem of coordination itself contains the concept of probability. It is true, in geometry one is allowed to separate the coordination problem from the mathematical theory because the coordination problem does not contain any *geometrical* concept; in probability theory however the concept constituted by this theory enters itself into the coordination problem: this is the logical particularity of the problem of probability. (*Erkenntnis* 1, p. 275)

Von Mises, to the contrary, considered the statistical collective no longer as an empirical but as an ideal concept. Asked about this shift by Zilsel (Ibid., p. 271), he emphasized that the question "whether an empirically given series represents a collective … [accordingly] does not constitute a problem within probability calculus." (Ibid., p. 272) He insisted "that approximation and statistics are not to be confused with one another." (Ibid., p. 280) Strictly in line with Hilbert's axiomatic method, he even contemplated that by "modifying the axiom of irregularity … one can obtain another probability calculus in the same sense as there is an Euclidean and a non-Euclidean geometry." (Ibid., p. 280) As a matter of fact, von Mises's relation to Hilbert's program was a complex one, and von Mises's axiomatization of probability was not Hilbertian insofar as probability was not a primitive and implicitly defined concept but rested upon the more basic concept of collective.[167]

The disagreement between von Mises and Reichenbach rooted in his firm empiricism owing to which there could not be any difference between the observed

[167] Interestingly, von Mises (1939) was very critical about Hilbert's axiomatic method. In a recent paper (2002a) I have argued that Hahn and Frank identified Hilbert's axiomatization program as professing the faith of a Leibnizian pre-established harmony between mathematics and the empirical sciences, a bridge which contradicted their rigid separation between analytical and empirical statements. To my mind, a similar case can be made with respect to von Mises. Recent work on Hilbert (Corry 1997, Majer 2002) shows that such an account misrepresents Hilbert's intentions. On the axiomatic formulation of probability theory, see (Hochkirchen, 1999).

and the existing that would require a probabilistic theory of approximate correspondence. On the ontological side, Mach, Boltzmann, and Exner's insistence upon the individual existence of the world and, accordingly, the rejection of possible-world arguments blocked – or at least made very unattractive to frequentists such as Frank and von Mises – Reichenbach's probabilistic reasoning concerning coordination in the framework of classical physics.

It is beyond the scope of the present investigation to study all concepts of probability advocated within Logical Empiricism. As the 1929 discussion showed, there was a large spectrum of opinions which is not exhausted by von Mises's frequentism, the Kriesian theory of Waismann and Schlick, and Reichenbach's extended use of the concept of probability. Suffice it to say, that von Mises's position corresponded to that shared by most physicists and that, on the 1929 meeting, his friend Frank had taken measures that it was most prominently placed.

8.3. The Prague Meeting

In September, 1929, the biennial meeting of German physicists and mathematicians took place at the German University of Prague. Frank seized the opportunity to publicize the new scientific world conception, not only by attaching the above-mentioned meeting to the congress and the distribution of the famous manifesto (Hahn, Neurath & Carnap, 1929)In these days it was not uncommon for a meeting of the German Physical Society to accept philosophically-oriented papers,[168] but an entire opening plenary session with two philosophical talks, was indeed a novelty. After Frank (1929) and Mises (1930a), the session was completed by Arnold Sommerfeld's "Some principal remarks concerning wave mechanics". Frank later recalled the event as such.

> I had prepared an elaborate paper that was intended to give the scientists a kind of preview of our ideas and to prove that the new line in philosophy is the necessary result of the new trends in physics, particularly the theory of relativity and the quantum theory. …
>
> Some friends cautioned me not to speak too bluntly. The audience, which consisted mostly of German scientists, knew little about philosophy, except that they had some sentimental ties to Kantianism. … My wife said to me after the lecture: 'It was weird to listen. It seemed to me as if the words fell into the audience like drops into a well so deep that one cannot hear the drops striking bottom. Everything seemed to vanish without a trace.'
>
> There is no doubt that quite a few people in the audience were shocked by my blunt statements that modern science is incompatible with the traditional systems of philosophy. Probably, most of the scientists had not been accustomed to thinking of philosophy and science as a coherent system of thought. …
>
> After the meeting, however, our committee received a great many letters from scientists who expressed their great satisfaction that an attempt has been made toward a coherent world conception without contradictions between science and philosophy. (Frank, 1961, p. 49f.)

While Frank and von Mises interacted perfectly in their plea for a new epistemology, Sommerfeld "strongly dissented in the appraisal of the philosophical background" (1929, p. 866) of the recent achievements of theoretical physics. He spoke out against Mach, monism, conventionalism, pragmatism, and praised the harmony of natural laws

[168] See (Stöltzner, 1995).

independent of man. This was Planck's program which Sommerfeld amended by interpreting Bohr's concept of duality, or complementarity, as corresponding to the metaphysical problem of the relation of mind and body.

In contrast to Sommerfeld, many physicists of the younger generation by 1929 were convinced that quantum mechanics required a final farewell to well-entrenched methodological convictions. To them Frank wanted to offer an entirely new perspective. His paper "What do the present physical theories imply for general theory of knowledge?" thus contrasted the emerging scientific world conception as continued application of scientific methods in the philosophical analysis of science with the outdated, paradox-laden school philosophy which scientists naively adhered to when going beyond their narrow domain of expertise.[169] School philosophy falsely pretended the existence of a separate domain of philosophical truths investigated by genuinely philosophical methods. Constant philosophical reflection, so Frank contended, could only come from the sciences themselves. But it was indispensable for scientific progress. Insisting on a 'purely physical point of view' was the best guarantee of tacitly rehearsing "a philosophy that contains a fossilization of the earlier physical theories." (Frank, 1929, p. 991/119)

> Experience has taught that those physicists who declared, for instance, that the relativity theory was nonsense often spoke in the name of 'pure, empirical science, free from speculation', but chiefly took their arguments from the school philosophy, not from empiricism [Empirie]. It need not be supposed that one has to make any philosophical studies to be acquainted with this world conception. In all knowledge that has come to us from the elementary school, in all metaphors of our language, it is implicitly contained. … Hence it is no wonder that it is just the physicist opposed to speculation who is easily inclined to the *ignorabimus* of Du Bois-Reymond, with his surrender of the scientific conception of nature. (Ibid., p. 974/102)

For a whole generation, du Bois Reymond's categorical pessimism to ever reach an understanding about the true essence of matter and force was the most famous quotation from an academic speech. Many developed strategies to get around it. Hilbert's (1900) battle cry was that in mathematics there was no *ignorabimus* because all correctly formulated problems were solvable. Others emphasized that the *Ignorabimus* was indissolubly linked to the exalted optimism of mechanical reduction embodied by Laplace's demon. Frank, finally, charged a naive correspondence theory of truth, in particular, the idea that there exist truths independently of any possible experience of them, such as the 'real length' of a body or 'real rest'.

> One who considers it obvious that an electron must have at every instant a definite position and velocity – though the measurement of them may be impossible – … is forced to interpret the quantum-mechanical calculations, which he uses nevertheless, in such a way that these definite positions and velocities of the electron do not determine the future. Since, on the other hand, the doctrines of the school philosophy in the field of mechanical phenomena require strict determinism, one is forced to assume for the motion of the electron some mystical vital causes, similar to organic life. (Ibid., p. 973/102)

[169] In (Frank, 1961) the paper is translated as "Physical Theories if the Twentieth Century and School Philosophy".

The only solution is to abandon the idea of a correspondence between our thoughts and the real world altogether. "The edifice of science must be built up out of our experiences and out of them only." (Ibid., p. 104/974)

Of course, Frank cited the local Prague hero of this program in which the *ignorabimus* loses its justification. After a short description of Mach's fundamental insights he concluded not without a critical distance.

Neither Mach himself nor his immediate students have systematically carried further his point of view. … On the contrary, Mach's teaching, through many presentations, has been washed out into something indefinite rather than built up to a consistent scientific conception of the world. It has even been interpreted again in line with the school philosophy. (Ibid., p. 975/105)

And similarly as the Vienna Circle *Manifesto,* Frank sketched the ancestry of the scientific world conception: the conventionalism of Duhem and Poincaré, the pragmatism of William James, the logical works of Schröder, Frege, Hilbert, Russell, and Wittgenstein. Among their positive results most relevant to the appraisal of scientific progress was Schlick's dissolution of the correspondence theory of truth into the uniqueness of coordination [Zuordnung]. "Every verification of a physical theory consists in the test of whether the symbols coordinated[170] to the theory are unique." (Ibid., p. 987/111) And thus he summarized the new optimistic program.

The task of physics is only to find symbols among which there exist rigorously valid relations, and which can be coordinated uniquely to our experiences. This coordination between experiences and symbols may be more or less detailed. If the symbols conform to the experiences in a very detailed manner we speak of causal laws; if the coordination is of a broader sort we call the laws statistical. I do not believe that a more exact analysis will establish a definite distinction here. We know today that with the help of positions and velocities we cannot set up any causal laws for single electrons. This does not exclude the possibility, however, that we shall perhaps some day find a set of quantities with the help of which it will be possible to describe the behavior of these particles in greater detail than by means of the wave function, the frequencies.[171] (Ibid., p. 992f./123)

Let me elaborate on five aspects of Frank's stand. First, what conclusion can we draw from the fact that the values of Planck's constant *h* observed in black-body radiation and in atomic spectra agree? To Planck's mind (See Section 3.8.), such agreement was a trustworthy sign that we had successfully moved up one step in the ladder, from the relative to the absolute, because, after we had given up simultaneously precise positions and momenta, we gained a new absolute constant. On Frank's account, agreement of various determinations of *h* did not warrant the inference to its real existence, as pretended by school philosophy. Notice that Frank did not criticize Planck by name, but example and interpretation were clue enough to spot the addressee. "The theory in which *h* plays a role then asserts that all the various groups of experiences, which are qualitatively so different from one another, nevertheless should give the same numerical value of *h.* It is therefore only a question of comparing experiences [Erlebnisse] with one another." (Ibid., p. 976/107) And as the true

[170] To restore terminological continuity with other Logical Empiricists of the day, here and in the subsequent passages I have changed Frank's own English translation. Frank wrote "assigned" and "correspondence" for "zugeordnet" and "Zuordnung". In view of Frank's post-war writings, I suspect that this was done deliberately to distance himself from a position he now longer advocated.

[171] In the English translation, Frank used the word "probability" instead. To a frequentist this was of course the same, at least in the present context.

existence of a physical quantity is only uniquely defined through the agreement of all experiences involving it, “the concept of a really existing quantum of action is only an abbreviation for the group of experiences which yield one and the same numerical value for *h*.” (Ibid., p. 989/114)

Second, correspondence between a measuring rod and a measured body is the core of measurement. In formulating the law of causality according to Laplace’s demon, classical physics contained the idea that exact knowledge of the initial state of a system was effectively attainable by a precision of measurement that could be increased at will. But if we stay on the level of possible experiences, each measurement of length ultimately reaches into atomic dimensions and becomes a coincidence of electrons measured by light. Arbitrary precision in such a measurement requires radiation of arbitrarily small wavelength, hence arbitrarily high frequency and energy. This is not only unrealistic, but also disturbs the measured object by Compton scattering. And Frank compared Heisenberg’s disturbance argument with electrodynamics in which we cannot effectively use of test bodies with infinitely small charge either.

Third, that Frank approached quantum mechanics from this point of view might suggest that he simply adhered to the Copenhagen creed. To a certain extent, this was indeed the case. But Frank’s reading of Born’s interpretation of the wave function and the dismissal of causality was embedded into a different context. Moreover, Frank remained open to future deterministic modifications of quantum mechanics and thus, as a positivist, he rejected the notorious finality thesis But as the deliberations about measurement had shown, so he held, setting up causal mechanical equations did not amount to actual experiences. Experiments of electron diffraction at a lattice, to Frank’s mind, demonstrated that the law of causality was invalid for our experiences of the positions and velocities of electrons.

> It is often concluded that electrons follow absolute chance in their choice of direction. … This follows, however, only if one starts from the picture given by school philosophy, according to which every electron has a definite position and velocity, which nevertheless do not determine its future.
>
> From the standpoint of a purely scientific conception, on the other hand, one will say that there are no individual experiences involving positions and velocities of electrons from which the future of the latter can be predicted univocally. Instead it appears that the probability that an electron will be deflected in a definite direction can be predicted from the experience of the initial experimental arrangement. For these frequencies (the squares of the absolute values of the wave functions) Schrödinger in his wave mechanics, sets up rigorous causal laws. To the frequencies that occur in these laws and define the state of the system one can therefore coordinate definite experiences. This theory is called statistical. The statistical element here consist in the manner of coordination of experiences to symbols. Thus to certain symbols, the squares of the absolute values of the wave functions, there are coordinated, not individual experiences, but numbers which are obtained by averaging from a great many individual experiences. (Frank 1929, p. 992/122)

Frank thus, fourth, rejected the notion of absolute chance not because it is impossible to prove that given events are absolutely random, but because it was a metaphysical concept. And thus he implicitly criticized Exner’s exalted declarations that chance was the root of all natural processes. Yet Exner and Schrödinger constantly maintained the empiricist’s openness on the issue of determinism and indeterminism. Moreover, in the above-quoted passage on the task of physics Frank took Exner’s stand against Planck and Schlick by asserting that the distinction between deterministic and statistical laws was at best a gradual one. The reason why Frank arrived close to Copenhagen and far from Schrödinger's Vienna, to my mind, is found in the strict division between theory and experiences which by then had become the core tenet of Logical Empiricism.

Roughly their account of experience and theory can be pictured like a commuting diagram in geometry between symbols at t_0 and t_1 and the respective experiences e_0 and e_1 (figure 6). This suggests that statistical features enter in two places that are, it is true, strongly correlated: in coordination (or assignment) and in the law. Pointing to the existence of a causal equation for the frequencies or probabilities, Frank could simply relegate the probabilistic element into the correspondence, such that the e_i were replaced by statistical collectives E_i.

$$\begin{tikzcd} S(t_0) \arrow[r] \arrow[d, leftrightarrow] & S(t_1) \arrow[d, leftrightarrow] \\ e_0 \arrow[r] & e_1 \end{tikzcd}$$

Fig. 6: The coordination of experiences to theoretical symbols

Advocating a form of Boltzmann's *Bild*-realism, Schrödinger rejected to leave such a prominent part to coordination only. To him, laws could well be of a statistical character, but theory should as much as possible minimize the arbitrariness of coordination. Schlick, instead, demanded a separation of strict causality and pure randomness already on the level of theory. As randomness was ascertained by the complete impossibility of prediction, to my mind, it cannot be interpreted as a definite experience in Frank's sense. Also their interpretation of Heisenberg's uncertainty relation diverged. To Schlick's mind they represented an internal limitation of quantum theory, to Frank they restricted coordination or better: measurement. Yet on the level of experiences or predictions, Schlick and Frank were in complete agreement.

Fifth, when it comes to ontology, Frank followed Mises lead and considered statistical collectives as entities that could figure in strict laws. They were ideal objects, but within Frank's conception all theoretical concepts represented abstract entities that were coordinated to experiences by certain definitions or correspondence rules. When contemplating possible further specifications of the quantum mechanical states, he continued (the above passage).

When we determine a number through a so-called single observation, we really observe even in this case only a mean value; 'point experiences' are never recorded. The coordination of symbols to experiences always contains then, strictly speaking, a statistical or, if we like, a collective element. Thus it is always a matter of making the coordination so as to go into detail to a greater or lesser degree. (Ibid., p. 993/123)

Thus collectives (or, more precisely, objects derived from them) can be coordinated to single observations and, accordingly, represent a possible ontology for physical laws that map probabilities into probabilities. And thus he reformulated the problem of causality as such: "What is the character of the coordination between our experiences and the quantities describing the state of the system, which are subject to rigorous laws?" (Ibid., p. 993/123) If this coordination involves individual events, then causality holds.

Following Frank's opening, von Mises (1930a) explained to the Prague congress that the recent changes towards a statistical viewpoint were rooted in a modified attitude to causality. By postulating hidden causes, it is rather easy to rephrase any statistical law in such a manner that it conforms to both of Kant's very general definitions of causality. In this way, of course, the principle completely loses its value for science. In the limit, one would arrive at Schlick's (1920) example of a completely chaotic world with specific laws for each space-time point. Yet von Mises's intention was not to search criteria to protect the principle of causality from emptiness.

As it can always be trivially fulfilled, so he held, the principle of causality is not a necessity of thought, "but *changeable*, and it will *subordinate itself to the demands of physics.*" (Mises, 1930a, p. 146) For this reason causality does not provide an adequate basis to assess the more relevant distinction between determinism and indeterminism, or between the description of nature by means of differential equations and by means of probability distributions. But as had Frank, von Mises remained reluctant to a final decision because absolute chance did not make sense to a Vienna Indeterminist. "The systematic theory, as I have pursued it for more than a decade, has never known of any failure of deterministic physics other than that it becomes *idle* in certain cases." (Ibid., p. 152) In this respect, quantum mechanics did not represent a fundamental breach in modern physics, and with minor qualifications von Mises could return to the hydrodynamic examples from "The Crisis of Mechanics" and stress the continuity between quantum mechanics and pre-quantum indeterminism.

"Laplace's demon, the executive officer of determinism" (Ibid., p. 146) can fulfill his duty only as long as the force laws are not too complex. "Newtonian mechanics only provides a useful means of causal explanation of nature as long as *relatively simple force laws entail more complex motions.* ... Explanation just means reducing to something more simple." (Ibid., p. 146) Otherwise, Mach's principle of economy would be violated.

> The deterministic approaches of classical physics can be maintained *formally*, or better: *ideally*, in the entire realm of directly observable phenomena, but in many cases … they become *idle*, they lose *the character of a causal explanation,* they do not contribute to our knowledge, to describing or predicting the course of phenomena. … Who views ponderomotoric forces, densities, and dielectric constants as things enjoying an existence independently of the task of describing nature, will consider determinism as in principle preserved but practically excluded. For those who comprehend these concepts [occurring in physical theories] only as means introduced in the approaches based on differential equations in order to jointly enable an orientation in the phenomenal world, the limits of applicability and the limits of determinism itself coincide. (Ibid., p. 147)

Once again we find Mach's empiricism at the roots of preferring an indeterministic approach. More precisely than in his earlier papers, von Mises studied the difference between the macro and the micro level. Hydrodynamics, Brownian motion and Boltzmann's various attempts to provide a mechanical foundation of the kinetic theory all show that "[t]he transition between the physics of the single elementary body, atom, proton, electron, etc., to the macroscopic phenomena is simply *obtained only by statistics*." (Ibid., p. 148) If one consequently adopts a purely statistical approach the notorious ergodic hypothesis becomes a solvable mathematical problem. Although the time evolutions of Brownian particles themselves do not form a collective, and, accordingly, the original concept of probability cannot be carried over to them, the law of large numbers (in the general sense) can be applied to the time evolutions.

Giving a succinct presentation of the frequentist approach, von Mises stressed that the statistical collective represented an ideal object similar to a sphere in geometry and that the irregularity condition corresponded to what physicists commonly described as 'molecular disorder'. As probability calculus merely was a theory mapping probabilities into probabilities, von Mises could tacitly relegate the problem of the status of this assumption to his physicist colleagues. This in effect turned Planck's (1914) use of molecular disorder upside down: from a supplementary condition rescuing determinist causality despite the use of probability in the second law of thermodynamics into a justification of an indeterminist theory.

Von Mises followed Schlick's (1920) contention that the verification of the law of causality required a suitable notion of when identical conditions recur. As a Machian, he had to stress the uniqueness of the course of physical events. Yet, the statistical approach – as any scientific investigation, to the empiricist's mind – tried "to find out observable processes which are limited in space and time and which reoccur to a reasonable approximation. Only *approximately repeatable processes* are the object of physical considerations." (Mises, 1930a, p. 151) Any observation or measurement yields only a decimal number with finitely many digits. While Schrödinger (1932) had emphasized that the determinist insisting on differential equations in actual fact assumed the feasibility of a continuous interpolation between the measured values, von Mises held that those who equate the idea of causality to naive determinism, are extrapolating their results beyond possible experiences by assuming that the precision of measurements could be increased beyond any limit. But this, so von Mises contended, contradicts the atomistic hypothesis. So ultimately he had made peace with atomism. And citing the idea of disturbance, he concluded that the efficiency of determinism is limited to sufficiently coarse measurements. The statistical point of view was thus superior to determinism. But von Mises did not conceive of any "*contradiction* between a series of observations and classical theory, we are never forced to say that a law of classical physics is *violated* in any single process." (Ibid., p. 152) Only the BKS-theory was based on this assumption, but it was quickly abandoned.

Quantum mechanics blocked the possibility to support determinism by reduction to atomic processes. "One has recognized that the elementary processes themselves do not admit a causal description. This was an immediate consequence of the requirement that a theory must be considered only together with the experiments serving its verification." (Ibid., p. 153) And now von Mises repeated the standard Copenhagen arguments up to Heisenberg's microscope. But there was a difference of philosophical interpretation. In von Mises's treatment of Born's interpretation of Schrödinger's wave function there was little evidence of a quantum revolution. Just "the same interpretation we have to give to any result of the physics of differential equations in the macroscopic realm if we restrict ourselves to assertions about what is actually observable." (Ibid., p. 153) According to Heisenberg's theory of measurement, "also in microphysics the concrete measurement process does *not represent an elementary process, but a statistical event.*" (Ibid., p. 153) But already when testing a deterministic theory, we have to presuppose the notions ‘collective’, ‘distribution’, ‘expectation value of a distribution’ because there is no other way to speak of the true value of a measurement other than defining it as the expectation value of the collective belonging to it.

During the academic year 1929/30 von Mises was Rector of the University of Berlin. On 27 July, 1930, he delivered the annual address commemorating the founder of the University, King Friedrich Wilhelm III. As Planck had done 16 years before, von Mises began with some words about the historical moment, the “fortunate settlement of one of the most unfortunate consequences of the lost war,” (Mises, 1930b, p. 885) the liberation of the Rhineland. “We conceive in the stage now reached a first indispensable precondition for the recovery and restrengthening of the nation which can come only from within; which can only grow from the three pillars of our own strength, work, character, and insight.” (Ibid., p. 885) Planck could not have better described the duties of the university as the Berlin academic elite understood them.

As regards philosophical content, “On the scientific world picture of the present” initially targeted the same opponent as Frank’s (1929) opening address: du Bois-Reymond’s *Ignorabimus.* Von Mises’s main theme was to compare the physical world picture of the 1870s with the present one. Of du Bois-Reymond’s in principle completed mechanical world practically nothing had remained. The convulsion began at the apparently safest place, in Euclidean geometry. It continued with the principle of causality in atomic physics and the principle of the excluded middle in mathematics. All these developments contradicted the intuitive idea of a reduction to elementary motions of atoms, which represented “wishful thinking reaching back to antiquity and closely linked to primitive habits of thought.” (Ibid., p. 887) Atomism gave birth to Laplace’s demon, but the first consistent implementation of this reductionist program by Boltzmann’s kinetic theory of gases unearthed the concept of probability foreign to the determinism of the mechanical world view. Ultimately, quantum mechanics dashed all hopes for a future return of determinism and demonstrated the “essentially statistical character of all physical assertions.” (Ibid., p. 890) But already before determinism or the physics of differential equations hit many obstacles, the impossibility to find simple and universal force laws for many phenomena of our immediate life-world and the fluctuations present in all physical measurements that ultimately reach atomic dimensions. In the second half of the speech, von Mises put these arguments familiar from his earlier writings into a general philosophical context and approached his colleagues from the humanities.

Many new developments of physics, so von Mises continued, blatantly contradicted our common intuitions. But we have to view them in the same vein as the people of the 16th or 17th century wondered about the rapidly moving earth.

> In actual fact this is nothing but a process of habituation, an adaptation of our faculties of thought and imagination to certain claims which, in actual fact, are completely unintuitive and definitely contradict the naive conception and all doctrines handed down to us before. *In every epoch of truly creative progress in natural science, there has to occur such an essentially voluntary process of assimilation of thought.* (Ibid., p. 890)

Von Mises's conclusion closely followed Mach's lead except for the voluntary character of the adaptive process. Mach had instead assumed also an instinctive process. This shift again documents Logical Empiricists' demise of biologism.

What forced the physicist "to develop theories which make such high demands to our faculty of intuition?" (Ibid., p. 891) The outcome of an experiment alone does not suffice. Although there can be crucial experiments within a well-defined setting, in the large experiments can be accommodated into many different theories. Although von Mises thus accepted Duhemian underdetermination, he rejected Poincaré's view that physical theories represent mere conventions agreed upon to suit a given purpose. "This conventionalism which seems to be a counterpart to the *contrat social* and similar extreme views in other domains, no doubt, contains a perfectly true core, but it envisages only *one* side of the matter. Already the far-reaching unanimity of physicists in rejecting and accepting theories speaks against the existence of free conventions." (Ibid., p. 891)

Although von Mises, accordingly, admitted that theories change while experimental results essentially remain correct within their observational limits, he clearly distanced himself from the far-reaching conventionalism still prevailing among Logical Empiricists. Nevertheless, the actual decisions scientists make about any given theory followed the usual pragmatic criteria of theory choice: "the simplicity, plausibility, decency [das Unanstößige] of a theory" (Ibid., p. 891). And von Mises cited a whole history of such criteria ranging from Occam's razor "until the lucid yet not always properly understood principle of 'economy of thought' which we owe to Ernst Mach [all of which express] the guiding principle of scientific theory formation: *Among all assumptions consistent with our present stock of experimental knowledge, we choose the one which in the smoothest way and with the least resistance adapts to our previous ideas, the one which imposes the least constraints to our previous habits of thought*." (Ibid., p. 891) Coming back to the comparison of the world picture of du Bois-Reymond's epoch and the present one, von Mises emphasized than above and beyond the extraordinary increase in scientific knowledge, "also our *epistemological attitude* has become more comprehensive, more profound, and richer in insight. Between those days … and our present lies above all the great clarificatory work [Aufklärungsarbeit] of the physicist and philosopher Ernst Mach which – after a short period of misunderstanding – now begins to have the widest consequences." (Ibid., p. 892) And apart from relativity theory, von Mises even praised the value of Mach's ideas for the restriction of quantum mechanical concepts to what is actually observable. It is quite interesting to note that while on this basis von Mises largely assented to the Copenhagen interpretation, Schrödinger turned the same criticism of the overestimation of auxiliary concepts against Copenhagen's particle trajectory (Sect. 6.3.6.).

In the end, von Mises returned to du Bois-Reymond's *Ignorabimus* and its persistent effect on the autonomy claims of the *Geisteswissenschaften.*

Still today for many representatives of the *Geisteswissenschaften* the program of a naive atomistic explanation of nature represents the basis of their attitude towards the natural sciences. Extensive theories about the 'limits of concept formation in the natural sciences' or the '*geisteswissenschaftliche* method' and the like are erected thereupon and try to elaborate the alleged contrast between two types of viewing world in a programmatic fashion. But who looks at present-day natural science as it really is, must understand that it does not close its eyes to any method suitable to impart knowledge

[Erkenntnis] and that it does not possess any limits other than those altogether set to human cognition [Wissen], that is, communicable knowledge [Erkennen]. (Ibid., p. 892)

Emphasizing the communicative basis of science was indeed a truly Machian perspective (See Sect. 3.1.). Yet von Mises was not after the continuity between craftsmanship and the experimental method. Almost verbally quoting Mach he instead praised it as "the highest philosophy of the scientific investigator is to tolerate an incomplete world view" (Ibid., p. 892)[172] and thus emphasized the openness in principle and the universality of the empirical method. "As to aim, content, and method, there exists only a *single* science, the imitation of the world by concepts; the bipartition in *Geistes*- and *Naturwissenschaften* has only a practical and provisional significance, it is not systematically necessary or final." (Ibid., p. 892) This was a creed, Vienna Indeterminists defended from Mach and Exner until Logical Empiricists' unified science. But more than the latter von Mises admitted that while the *Naturwissenschaften* set out from simple problems, the *Geisteswissenschaften* addressed the "more lifelike and vital and, above all, the more complicated problems." (Ibid., p. 892) This implied a difference of perspective on conceptual changes in both realms.

While in the latter one attributes high significance to a new discovery, to a new basic idea only if it actually permeates large part of the problem in question, if it quantitatively affects a wide field, the most important physical theories often exert only little influence at a remote place, at least at the beginning of their development. (Ibid., p. 890f.)

And von Mises rightly held that missing this difference and the difference between constant facts and ever changing theories was the basis of many misunderstandings of modern physics among people with a *geisteswissenschaftlichen* background. The failure to recognize these differences, so one might add, nourished a substantial part of the sentiments of a hopeless foundational crisis of science widespread in the general cultural milieu. Moreover, this distinction gives some clue that when assenting to Spengler in the early 1920s, von Mises was talking about a different type of crises. (Cf. Sect. 1.1.2.2.) As Exner but with a markedly different orientation towards modern art, von Mises thus simultaneously lived the life of a scientific modernist and a *Bildungsbürger*. This was an important difference to his friend Frank.

8.4. Logical Empiricists' Anschaulichkeit

Perhaps the most ideology-laden paper of Frank, still more outspoken than the Prague opening speech, was his 1928 criticism of the debates about *Anschaulichkeit*. It amounted to a radical criticism of the milieu's demands characterized by Forman and tried to track down their metaphysical roots. In "On the '*Anschaulichkeit'* of physical theories", Frank initially assented to Heisenberg's redefinition of the term. A theory was *anschaulich* if it was possible to think all experimental consequences in a consistent qualitative fashion. This, however, "is a demand made on every physical theory." (Frank, 1928, p. 121) Since the demand for *Anschaulichkeit* that was typically

[172] This famous quotation appeared in the *Mechanics* (Mach, 1988, p. 479; 559). See also Sect. 1.1.2.1.

made went further and was much more specific, Frank set out to investigate its precise meaning.

> [At bottom, one only finds] a certain desire for convenience [or simplicity] and an unconscious adherence to some traditional philosophical systems. One might perhaps immediately object that that the latter claim is untenable because precisely those physicists have the desire for *Anschaulichkeit* who think in the most concrete fashion and are averse to any speculation. Against this one can only say that who is averse to any speculation simply takes over at face value the speculations of earlier generations. (Ibid., p. 121)

This inescapability to critically discuss anew the foundational questions of every scientific theory is a figure that returns over an over again in Frank's writing. (See Sect. 8.3. & 8.5.) Otherwise scientists typically relapse into school philosophy (*Schulphilosophie*). The Vienna Circle reserved this tag for those classical philosophies which considered themselves prior to empirical sciences and strove for scholastic coherence. In the case of *Anschaulichkeit*, the philosophical convictions taken over uncritically consisted in two characteristic metaphysical creeds.

> First, the materialistic world conception according to which all events can eventually be reduced to the motion of absolutely inelastic little particles in vacuum. … Second, idealistic philosophy with the special status of the enigmatic triad of space, time, causality (or space, time, matter), where by means of the, or so it seems to me, absurd concept of "pure" intuition a daring bridge is thrown that leads from mystical intuition to a real optical experience of viewing. (Ibid., p. 124)

The unnamed party guilty of absurdity and the enigmatic triad was Kant's transcendental philosophy. For the Logical Empiricists, general relativity required a radical departure from the a priori categories of space and time that Kant had grounded in pure intuition. And indeed one of the goals of Kant's transcendental philosophy was to embody the core concepts of Newtonian physics in a systematic fashion.

Frank extended this model of the dismissal of the synthetic a priori and a return to pure empiricism to the issue of causality by implicitly identifying materialism and deterministic causality. This is also the reason why he believed, but did not really prove, that both metaphysical positions "presuppose one another." (Ibid., p. 124) Yet one may well ask whether this identification is necessary for a Kantian standpoint, in particular if one takes Kant's original formula for causality: "Everything that happens, that is, begins to be, presupposes something upon which it follows to a rule." (Kant, [1781] 1990, A 189) As a matter of fact, there were neo-Kantians who treated the issues of space-time and causality on different grounds. And Ernst Cassirer (1910) even viewed the joint development of modern science and modern philosophy as the transition from the concept of substance to the concept of function.

There were, it is true, less sophisticated minds. In Lenard's attacks against Einstein, Frank rightly detected a materialist origin, in particular the conviction that all inanimate processes in nature are "merely translocations of a substance [*Stoff*] given once and for all." (according to Frank, 1928, p. 126) Apart from these metaphysical dispositions, the preferred status of the Newtonian world view of particles mutually interacting through forces was just a contingent fact of the history of physics which people simply imbibed and got used to as centuries went by. But those who "establish a vague connection between the lack of *Anschaulichkeit* in Einstein's theory and the lack of a causal explanation" (Ibid., p. 121) simply forgot that at the time of Newton, forces at a distance were totally unintuitive and – so one might add – Newton had to admit that his mechanics could not explain the almost circular nature of the planetary orbits. Similarly, the electrons orbiting around the nucleus in Bohr's early atomic theory were intuitive only because they reminded us of the solar system. But this intuition proved illusionary once we try to actually look at [*anschauen*] these atoms. 'Looking at' means, according to the empiricist, "observing and experiencing a reaction of this body to light waves." (Ibid., p. 123) Frank's argument thus developed into Heisenberg's famous thought experiment about the determination of momentum and position of a quantum particle under the microscope.

If as a result one believes that nothing is said about the positions and velocities of the electrons themselves, but only about the possibilities of their precise measurement, the proper response is that one has to distinguish between the position coordinates as mathematical concepts and the position coordinates as physical experiences. As the latter, the electrons are experienced in the properties of dispersed light; as regards the former, quantum mechanics shows that coordinate triples of points are inadequate quantities to represent radiation phenomena. But there is nothing "intuitive" in these material or electrical points. (Ibid., p. 124)

This strict division between the mathematical and the empirical level of science was another core tenet of Logical Empiricism that had been developed by interpreting the relativity theories. This division represented their most important departure from Mach's empiricism and was a precondition, or so they held, to avail themselves of modern logic in the analysis of science. Any idealistic metaphysics, Kant's synthetic a priori foremost, amounted to a border violation that was meaningless, at least under a strict reading of the verificationist doctrine. The relation between the analytic and the synthetic realm was established by suitably chosen coordinative definitions. In physics they corresponded, at bottom, to a theory of measurement. This two-tired ontology permitted Frank and his colleagues to reject Planck's (1908a) allegations that positivism led to a return of anthropomorphic and subjective elements into science.

The new theories of physics, so Frank continued, did not "introduce the observer as a causally determinative factor" (Frank, 1928, p. 123) let alone a subjectivity of sense perception because any observer could be replaced by a measuring device. "One may express the basic idea of relativity theory nearly as such: the registering devices and the processes of their construction and calibration belong as essential parts to the system whose regularities we want to describe." (Ibid., p. 124) The same holds true in quantum mechanics, as the Heisenberg microscope argument demonstrated.

Frank at this point continued the Mach-Planck debate. While relativity theory at first look favors positivism, the successes of atomistics suggest that physical theory could reveal "the innermost nature of matter." (Ibid., p. 125). But there existed Planck's realist interpretation of general relativity according to which the metric was "moving the absolute more backward…[by] welding space and time by means of the velocity of light into a uniform continuum." (Planck, 1925, p. 154) In contrast, Frank believed that quantum mechanics – both in Schrödinger's and Heisenberg's versions – continued the epistemological changes of relativity theory and corroborated a major insight of Mach, namely, the criticism of a realistic interpretation of auxiliary concepts.

> The concept of position, the precisely defined locality in space turns out not to be applicable for all phenomena. The final fixed point is seen in the immediately experiencable reaction of an atom to light rays. Wholly in line with Mach's conception of physics … the auxiliary concepts "position of the electron", etc., turn out not to be applicable in general, adhering to them even hampers the understanding of radiation phenomena. (Ibid., p. 125)

In these lines, Frank appears closer to a staunch Machian position than he would do in later writings having taken a linguistic turn. (See Section 8.7.) The auxiliary concept Mach was most reluctant to accept were Boltzmann's atoms. Frank, as it were, made virtue of necessity and praised Mach's early insight that "one need not imagine the chemical elements to exist in a space of three dimensions", (Mach, 1909, p. 55, quoted by Frank, 1928, p. 126f.) of course without suggesting that Mach presaged configuration space. Still, this sounded apologetic. Referring to his obituary (1917), Frank conceded that Mach was wrong to expect that "the most important advancements would be stimulated by sense physiology," (1928, p. 125) but he remained committed to a strict reading of the empiricist criterion of meaning. General relativity and quantum mechanics teach us not only that the measuring devices are part of the system, but also "that all physical statements can be reduced to statements about the relations of the readings of measuring devices," (Ibid., p. 125) readings which replace the enigmatic triad of space, time, and matter. And these readings were construed in parallel to the classical Machian elements. To give a simple example first, Frank argued that neither the heliocentric nor the geocentric picture of the planetary system had any similarity with the experience of twinkling points; "the only similarity is that the pictures can be generated by a unique rule from the phenomenon and vice versa." (Ibid., p. 122) The same occurred in Heisenberg's quantum mechanics.

> [I]n the positivist conception of nature we have gained [here] a fixed point. On this account, every theory is justified and indicates an understanding of natural phenomena which uniquely connects the sense impressions. In the fate of the materialistic world view we clearly see the consequences of an idolization of auxiliary concepts. (Ibid., p. 125).

But this uniqueness did not at all imply that there is only one true theory because there can and typically will be different rules that are preferred or rejected according to pragmatic criteria among them simplicity and fertility. Mach's positivism was thus highly anti-reductionist.

In the final pages of his paper, Frank rejected Vladimir I. Lenin's criticism of Mach by observing that the basic idea of the old materialism consisted in the "mathematical description of all natural events" (Ibid., p. 127) which was not tied to a primitive mechanicism. In the emphasis of mechanicism, the materialist of Lenin's brand and the vitalist meet though with directly opposing aspirations in the description of animate nature. Some pages before, Frank had leveled the same criticism against that type of physics which Lenard had found so intuitively appealing. Hence one might conclude by transitivity that, to Frank's mind, there was a certain agreement between Lenin and Lenard. They shared the will to return to the classical physics from which their metaphysical world view had once emerged.

8.5. The Law of Causality and Its Limits

In the Introduction to his 1932 book, Frank recalled that his earlier views (See Sect. 8.1.) had been shaped by his failed attempt to posit causality as an axiom of physics in the style of Hilbert's *Foundations of Geometry*. As I have argued elsewhere (Stöltzner, 2002a), Hilbert's Viennese students Frank and Hahn conceived the axiomatic method through the glasses of conventionalism, thus rejecting Hilbert's repeated talk about a non-Leibnizian pre-established harmony between mathematics and physics.

Two developments of recent physics prompted Frank to return to the causality theme. "On the one hand it was the conception of statistical laws [Gesetzmäßigkeit] and their relation to dynamic laws which has been developed in several publications by Richard von Mises; on the other hand the new formulation of the law of causality in quantum mechanics." (Frank, [1932] 1988, p. 24/12) In contrast to Schlick (1931), quantum mechanics accordingly did not strike Frank as a bolt from the blue because substantial modifications of the concept of causality familiar from classical physics were already called for by the statistical physics of the 1910s. Frank explicitly cited (Mises, 1922a) and acknowledged useful suggestions from Einstein, von Mises, and Schrödinger.[173]

We can find many affirmative references to the tradition of Vienna Indeterminism. First of all Frank acknowledged Exner's priority.

Franz Exner has already drawn attention to the possibility that elementary processes do not follow the pattern of celestial mechanics with their Laplacian causality but that perhaps for an individual event, for example the collision of two molecules, no causal law can be established at all, and that nevertheless, with the formation of averages, laws can be derived by which some causal determination is expressed. (Frank, [1932] 1988, p. 90/70f.)

And citing the *Lectures* (Exner, 1919) in the notes, Frank continued that the "significance of Exner's thoughts for our time is very correctly characterised by Erwin Schrödinger [1922a]." (Frank, [1932] 1988, p. 338/284) Hence Frank was well aware of the philosophical tradition of the old Vienna Institute even though he had not explicitly defended these views when he was working there before 1912.

Frank cited his 1919 paper, that represented a transitory position, only in the notes as a presentation of gas theory. Although he now called his juvenile work (Frank, 1907) one-sided, what remained in 1932 was the insight that very general theorems, among them energy conservation and the law of causality, constantly were close to meaningless tautologies. This insight motivated the strategy of Frank's book, to search for conditions under which the law of causality had a well-defined empirical meaning, that is, was a statement about the real world [Wirklichkeitssatz]. In the end, Frank arrived at a negative conclusion.

From our experiences [Erlebnissen] no proof can be derived for or against the validity, or even probability, of the law of causality in nature, nor can we conclude anything about observable events from the validity of the law of causality.

On the other hand, our whole science, even our whole practical life is apparently based on the continual application of the law of causality. Our whole life is built upon confidence in this law; each

[173] (Cf. Frank, [1932] 1988, p. 28/15).

manipulation is accompanied by the expectation of definite results, an expectation that we can draw only from the belief that equal initial conditions will always be followed by the same.

Both conceptions are correct and therefore cannot be in real opposition. The appearance of such a contradiction comes about because we often have an unclear notion of the connection of the 'real' world with the world of our experiences, that an old tradition has taught us to look for a sharply designed world of 'real' things behind the living, but vague, world of our experiences, a notion that seems to us as obvious as in fact it is misleading and obstructive to understanding the more delicate features of science. (Frank, [1932] 1988, p. 286f./238f.)

Frank treated this apparent contradiction between the theoretical and the practical content of the law of causality almost like a Kantian antinomy. The contradiction dissolved once untenable metaphysical presuppositions were dropped. Indeed, there was hardly a better Kantian example for a transcendent concept – at least in the phenomenal realm – than the 'real' world. No wonder that at the end of Chapter X, Frank praised neo-Kantianism as the most progressive direction within "the process of decomposition [Zersetzungsprozeß] of school philosophy." (Ibid., p. 320/268)

Frank's philosophical conclusion was more radical and more specific than just rejecting metaphysical realism. Manifold historical investigations demonstrated that "obviously the general law of causality was not a great discovery. Only special causal laws were, for example the discovery by Galileo and Newton that all motions can be predicted from the positions and velocities at a moment in time." (Ibid., p. 328/274f.) Just the form of these special laws underwent drastic changes. More precisely than in 1929, Frank elucidated his commuting diagram between theoretical symbols and empirical observations (or possible experiences). Ultimately, this distinction permitted Frank to overcome – and partially maintain – his 1907 position; recall that the definition of state had been crucial for establishing the conventionality of causality.

Full precision is, in principle, not possible at all in the world of sense experience, because equality of states is not defined in it; it is only defined in the mathematical scheme with which theoretical physics represents our experiences.

The scheme itself always connects the present values of certain magnitudes with their future values in an unambiguous way, and is therefore, seen as a formal scheme, always purely causal. Depending on the manner in which the mathematical magnitudes are connected with the observational sense experiences, the systematic summary of a scheme and rules of coordination [Zuordnungsregeln] can form a causal or a noncausal theory. The latter is true when the mathematical magnitudes are coordinated not to individual experiences, but, as happens in modern wave mechanics, to a whole group of experiences, which results from a series of experiments made under certain conditions. …

This [new development] however makes a change only in the physical theory, that is in the totality of scheme and coordination, compared to classical physics. The subdivision is different now; the summary [pauschal] nature of the prediction of experiences [Erlebnisse] is already inherent in the rules of coordination, whereas formerly we retained the unambiguous nature of the relation between mathematical magnitudes and experience, and took the summary nature of the observed connection between future and present experiences into account by regarding the scheme used as altogether too simple for a faithful representation of the experiences.

This difference is therefore not a difference in the statements about experiences, for in any case there are only summary predictions here; but the deviation exists only in the theory of how these summary predictions can be made. … What the new physics teaches us is an advance in the analysis of uncertainty, since the theory now also predicts this spread [Streuung][174], whereas formerly the spread, the uncertainty, was simply thrown into the residue which could not be understood theoretically. (Ibid., p. 333f./279f.)

[174] The English translation here reads "scatter". But I found a term from statistics closer to Frank's intentions.

At bottom, Frank took it as the main lesson of quantum mechanics that coordination of theoretical symbols and experiences was no longer one-to-one but statistical. This move simply integrated the errors present in every real-world measurement into the theory itself. It is quite interesting that in this passage at the end of the book Frank made reference only to Schrödinger's wave mechanics and remained silent about what Schlick (1931) had taken to be the pivotal lesson of quantum physics, to wit, that quantum theory contained an absolute limit of language.

Frank, however, openly approved Schlick's emphasis on prediction. Discussing the identification of the law of causality with predetermination by a world formula, Frank concluded "that pre*determination* of the future has scientific meaning only if we connect it with the question of scientific pre*diction*. Schlick [1931] is justified in regarding this as the proper scientific meaning of the law of causality." (Frank, [1932] 1988, p. 51/37) And when explaining that before coordination to certain experiences all mathematical formulas of theoretical physics are neither true nor false, Frank added the note that "Schlick, M., following Wittgenstein, says that the laws of nature themselves are not assertions about the real world, but only directions for the formation of such assertions." (Ibid., p. 335/281) To Frank's mind, rules of coordination were not only appropriately chosen conventions but "the reason why in exact science, however exact it may seem, there are remnants of uncertainty." (Ibid., 31/19) Quantum mechanics only integrated this uncertainty into the theory itself.

But there is another important difference here above and beyond the strong emphasis on limits of the speakable that resulted from Schlick's adherence to a rigid verificationist criterion of meaning. In Sect. 7.3. we have seen that he used the pragmatic Wittgensteinian twist to maintain a difference between strict dynamical laws and statistical regularities which still had to be divided into laws and pure chance. Frank instead remained committed to the Viennese tradition and used only a single type of lawfulness [Gesetzmäßigkeit] thus treating statistical and dynamical laws on a par. In this perspective it is very instructive to follow Frank's attempts to assign empirical meaning to the general law of causality – albeit unsatisfactory at the end of the day – because he provided a variety of very general yet still specific versions of the law of causality, some of which were historically influential enough that philosophers equated them to the general law of causality itself. As demonstrated in Section 7.1., Schlick (1920) coined his theory of causality after the general theory of relativity while quantum mechanics would prompt the complete dismissal of this conception in 1931.

Frank's historico-critical investigation commenced from Laplace's demon, "probably the most incisive and definite [formulation the law of causality] has ever received." (Frank, [1932] 1988, p. 60/44) That there exists a unique determination of the whole course of the world from a given state, however, does not suffice to escape tautology because this demand "has nothing to do with causality." (Ibid., p. 66/50) While Mach had taken unique determination as a very weak ontological principle and thus criticized the notion of independent (and repeatable) causes and effects (See Sect. 3.2.), Frank considered this kind of uniqueness as a plain tautology, probably because by way of the concept of coordination, Mach's criticism was already incorporated into epistemology; the mathematical symbols were just the abstractions Mach had called for. Symbolism and coordination had to reflect this uniqueness in the sense that they must not lead to an ambiguous relation between experiences, but there could well exist different symbolic representations – this was Duhemian underdetermination – and the coordinations could be statistical.

> It is clear that for the world there is only *one* course of events that really happens. If there is an all-embracing intelligence that knows this course in advance it can also predict it. Every other being also knows that there can be only one course of events for the world; only he does not know in detail what it will be like. If the proposition 'everything is predetermined' is meant to say that in reality there is only one course of events, it is a tautology; for 'to be predetermined' is then only another expression for 'to exist'. … Our statement about the predetermination is no tautology only insofar as it claims the existence of an all-embracing intelligence; by this it states something about the real world if what this intelligence knows of the future also becomes noticeable in our experiences. (Ibid., p. 49f./35)

More specifically, Laplace's superhuman intelligence that is able to calculate the future of the world from its present state, again runs the risk of tautology because anything can be expressed through an arbitrarily complicated world formula. This corresponded to Schlick's (1920) insight that a world governed by arbitrarily complex laws was empirically indistinguishable from a chaotic world. Interestingly, Frank left open the theological alternative that such a superhuman world formula became meaningful by the assuming the empirical existence of a superhuman intelligence cognizant of it. "If however we want to avoid both the introduction of this spirit and also the decline into the tautological, we have to introduce the assumption that the world formula is of a special kind, for example given through Einstein's differential equations of the general field theory, or a similar system of equations." (Frank [1932] 1988, p. 50/36)

The paradigmatic example were Newton's laws of the mechanical motions of mass points, that is, second order differential equations of the form $m\ddot{x}(t) = X(t)$, where X denotes the relevant force function.

> Laplace's spirit must have three achievements, helped by his superhuman capabilities: he must know all initial positions of all mass-points of the world; he must know the forms of all functions *X,Y,Z* for all masses, and finally he must be able, from knowledge of the initial conditions and of the functions *X,Y,Z*, to calculate the positions at any time whatsoever, that is he must be able to integrate Newton's equations of motion for any initial conditions and any 'law of force'. (Ibid., p. 64/48)

It is empirically meaningful to assert the existence of a superhuman spirit so construed. "In order to make Laplace's demand meaningful for a human mind, the arbitrariness of the functions *X,Y,Z* has to be reduced," (Ibid., p. 66/50) they must be simple enough to make possible successful predictions. "The significance of Newton's discovery is precisely this: that the laws of force can be expressed by simpler functions than those that would be needed to describe the processes of motion if we wanted to specify the shape of the trajectories directly." (Ibid., p. 67/50) And Frank explicitly endorsed von Mises (1930a) contention that the limits of applicability of mechanical concepts and the limits of mechanical determinism itself coincide. (See Sect. 8.3.)

Frank accordingly drew the limits of mechanics just in the same way as his friend had done in the "Crisis" paper (Mises, 1922a). Celestial mechanics represented the ideal and yet the only case where the Laplacian program could actually be carried out. "The suggestive power exerted by the celestial mechanics of Newton and Laplace was so strong that for a long time the law of causality could be understood in no other way." (Frank, [1932] 1988, p. 68/51) In actual fact, however, already bodies of finite dimension did not fit into this scheme because not all their properties could be reduced to properties of elementary mass points. Treating solid bodies as continuous media instead – thus as part of 'free mechanics', in von Mises's terminology – already transcended the Laplacian framework. The equations of hydrodynamics or elasticity theory "in no way allow us to calculate the future states of particles from their initial positions and velocities. For the state of the system is described by magnitudes that result from forming averages of positions and velocities, for example density or the shape of the surface." (Ibid., p. 70/53) Accordingly, von Mises (1922a) advocated a genuinely statistical approach to hydrodynamics which Frank approvingly discussed at length in Section 7 of the third chapter "Currents of Thought Hostile to Causality".

Other than von Mises, Frank did not argue by methodological purity in the first place but interpreted the whole of Boltzmann's statistical mechanics as an "Attempt to Rescue Mechanical Causality with Statistical Ideas."[175] But the validity of Newtonian mechanics for atomic mass points alone was insufficient to obtain phenomenological thermodynamics.

> Rather, certain additional assumptions about average behavior had to be made which have been called the 'assumption of disorder', assumption of a number of collisions' [Stoßzahlansatz], 'the ergodic hypothesis', etc. It is in no way sufficient to make such an assumption just for the initial state; the assumptions are rather for the whole course of the movements, in addition to the Newtonian laws, and we cannot even show that these assumptions are compatible with the equations of motion. (Frank, [1932] 1988, p. 89/70)

Other than Planck (1914), Frank did not treat these assumptions as supplementary laws. And he precisely invoked Exner's reasoning against the inescapability of a deterministic microstructure. (See Sect. 4.4.) "[I]f the Newtonian laws of motion were not sufficient to derive laws for averages, … perhaps it did not matter at all that the individual laws from which the averages were formed were in fact Newtonian laws with their dynamic palpable causality in the Laplacian spirit." (Ibid., p. 90/70) Moreover, he read Exner's reasoning – too conventionalistic, to my mind – as pointing to the tautological character of "the proposition that mechanical causality exists for each particle of arbitrarily small size." (Ibid., p. 92/72) This reading, to be sure, corresponded to Schrödinger's (1929a) reference to Poincaré at the end of his Berlin Inaugural Address; but in the Zurich speech and in the correspondence with Schlick, Schrödinger basically rehearsed Exner's original argument that indeterminism had become more probable than determinism. (See Sections 6.3.2., 6.3.5., 7.4.) From the above-quoted negative conclusion of Frank's we can see that he considered, in contrast to Schlick and von Mises, talk about probability as legitimate in this case but nevertheless as inconclusive, now in full agreement with them.

Frank also criticized another mediator between the mircrolevel and the macrolevel, Maxwell's demon. The fact that once again one "has to take recourse to the kingdom of spirits [Geisterreich] at the very point where the law of causality has to be pronounced concretely, in full generality, for a system of mass points" indicated more than was commonly acknowledged "an essential difficulty in the formulation of the general concept of mechanical causality." (Ibid., p. 72/92)

[175] So reads the title of the respective section III. 4. of (Frank, [1932] 1988, p. 89/69).

All these sceptical views about the general applicability of the law of causality in physics were however only a weak prelude to the conception that is often argued in quantum mechanics today. (Ibid., p. 94/74) …

[C]lassical mechanics was convinced that it might well be possible, in the end with sufficient refinement of technical tools, to define the state of the individual mass-points so precisely that, some time, the future of the gas could be exactly predicted from the observations of the present moment. The introduction of more or less superhuman intelligences by Laplace and Maxwell, however, demonstrated that very vague hopes for improvement of human capacities are hidden in this opinion. The new quantum mechanics however is convinced from the outset that there is no possibility in principle of ascertaining the initial values of the wave function with precision. This anticausal standpoint has sharpened our eyes to look back at the whole difficulty of formulating the law of causality, even in classical physics. (Ibid., p. 95f./75)

Consequently, in 1926 there occurred no quantum revolution but rather a great advancement in the continuous process of clarifying the concept of causality. In this vein, Frank discussed the emergence of the field concept within electrodynamics and the various attempts to uphold the Laplacian conception in field theory although determination of the future state of a system required the knowledge of the state of the whole space. Among them were the ether, which at bottom was defined only through its electrodynamic effects, and the introduction of differential equations of higher order, which pushed the law of causality closer to the tautological statement that all observations can be described by analytic functions.

The significance of the reduction of all natural phenomena to motions was so great that it was later practically identified with the understandability of natural phenomena. … As a consequence there was strong resistance to acknowledging that not everything can be reduced to motion. The abstract conception of a physical field seemed to be a relapse to the half mystical assumption of occult qualities [residing in empty space]. (Ibid., p. 78/59f.)

The main problem was to give an appropriate definition of field densities with their characteristic fluctuations from point to point resulting from the atomic constitution of matter.

If we want to give a meaning to the law of causality that does not sink into the tautological, we have to say: By the introduction of a limited, manageable number of state variables alone, if their initial distribution is not over-complicated, we can bring it about that after the return of the same state, also the whole sequence which followed it the first time will always return. We see also that this formulation has to make use of the not very precise concepts, 'limited', 'complicated', and the like. (Ibid., p. 82/63)

Field theory, accordingly, deprived the law of causality of the simple and intuitively clear meaning it had enjoyed within the Laplacian world view.

Could there be manifest gaps in the laws of nature? Since within Frank's conception statistical laws represented genuine laws for collectives, no such gap existed in statistical theories. It did so for chaotic systems – in today's terminology – which exhibit a 'sensitive dependence from initial conditions': minute changes of the initial conditions entail a drastically different future behavior. "In this sense we can say that the world of mechanical laws, if we want to pursue it into its finest detains, has 'gaps like a sieve'." (Ibid., p. 103/81)

At the classical problem of miracles as purported gaps in the lawfulness of nature, Frank devised an argumentative strategy that he also applied against independent teleological explanations and vitalism. Diagnosing a miracle is a non-tautological statement about the real world only if it is turned into a positive statement about the plan of a superior intelligence. Then, "mechanical laws are replaced by a different kind of law which has to do with psychological states of a higher intelligence instead of with mass-points. If causality is understood to be only the permanent links between events, the belief in miracles is as compatible with this as the belief in the general validity of the strictest laws of mechanics." (Ibid., p. 105/82)

Thus we find Mach's liberal notion of causality as functional dependence at the bottom of the alternative which Frank posed to the critics of causality. Claims about miracles, vitalistic factors of goal-directedness in nature are non-tautological only if they are positively expressed either in terms of psychological laws about superior intelligences or in terms of empirical laws that are of no other kind than those governing physical phenomena. The Machian conception of lawfulness – so Frank believed in accordance with the Viennese tradition – was wide enough to incorporate all positive claims of the life sciences and the so-called *Geisteswissenschaften*. It was, one might add, so wide not to provide a satisfactory criterion to exclude natural theology at least if theologians set themselves the, probably insurmountable, task to list in gruesome detail the purposes of a highest being.

Unity and openness of the scientific method was also Frank's main gear to reject the vitalist claim that the phenomenon of life was inexplicable from the laws of physics. Such a negative statement was defective because it crucially, and in effect exclusively, depended upon the present imperfect state of physical science. As to the positive claims of vitalism, Frank took them seriously enough to devote an entire chapter to their criticism. Most prominently and respectfully he treated the biologist-philosopher Hans Driesch who had introduced entelechy as a quantity that measured goal-directedness and was irreducible to basic physical quantities. It could be seen in analogy with material constants in physics. But, so Frank concluded, the more the formal analogy with physics was sought in order to produce a scientific theory alongside with and independent of physics, the more the autonomy of vitalistic quantities disappeared. What remained was a proto-scientific theory in the spirit of animism. "The more scientific vitalism wants to be, the more it retreats from true science." (Ibid., p. 120/94)

Also Ludwig von Bertalanffy's attempts "to formulate vitalism positivistically" by distinguishing causality and finality according to the temporal order of the functional dependences faced the same dilemma. For "in physics each dependence between two events can equally well be formulated as a dependence of the later event on the preceding one or vice versa, and that therefore nothing is stated about the events themselves and even less on what is characteristic for biological processes. By positivist purification, the concept of finality loses absolutely everything that constitutes its attraction." (Ibid., p. 148/118) Even the concept of system – which would become the core notion of Bertalanffy's later thinking – was no peculiar characteristic of living organisms. "We cannot solve the most primitive mechanical problem, the path of a particle on which no forces have an effect, without knowing its velocity with reference to the whole galaxy." (Ibid., p. 150/120)[176] This was nothing else but Mach's principle of the relativity of motion.

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[176] For a detailed assessment of Frank's criticism, see (Hofer, 1996).

Those seeking organismic traits in the indeterminism characteristic of atomic physics, in those days, could point to Sommerfeld's (1929) Prague address which elaborated the idea that teleology played a certain role in atomic physics because the transition between two energy levels in the atom depended both on the initial and the final state. Warning against the dangers of physicists' relapse into school philosophy, Frank tried to eliminate the problem through a consistently statistical approach based on collectives as the basic theoretical entity.

If initially the electron circulates on the orbit with energy E_3 the quantum theory allows us only to predict what will happen on the average if we examine a very great number of atoms with the same initial state. Thus in this theory the proportion of the number of jumps to the orbit with energy E_2 to the number of jumps to the orbit with energy E_1 is unambiguously determined. … But if, in the case of an individual experiment, the initial and the final state are known, then the frequency can be predicted also for each individual atomic experiment. (Frank, [1932] 1988, p. 166/134)

Yet this situation was no different from already knowing the outcome of a throw of dice which followed purely mechanical laws. The new feature of quantum mechanics was thus not the intrusion of teleological elements into physics but "that from the initial state only statistical statements can be made about the final states." (Ibid., p. 167/134)

Rather in passing, Frank also turned to the teleological connotations of the Principle of Least Action. Repeating an example from his dissertation where the minimality of the action integral did not correspond to the physical solution, he concluded that

[i]t is not at all characteristic for the orbit a point-mass follows that along that orbit any magnitude assumes its smallest value. If the orbital curves satisfied another law … there would always be a magnitude that depends on the velocity (or acceleration) and which is smaller for the orbital curves than for any other curve. Just this magnitude would then be regarded as a measure of the action of nature. We should therefore be able to prove why a definite magnitude signifies the action of nature. … This would mean a return to pure anthropomorphism, to the animistic world-conception of the pre-scientific age. (Ibid., p. 115/91f.)

Similar as Mach, Frank held that "[o]nly a certain mathematical simplification is hidden in the minimal principles of mechanics. With its help the laws of the orbital curves can be expressed in fewer variables." (Ibid. p. 116/92)

It was not so much the specter of metaphysical realism implicit in Planck's veneration of the Principle of Least Action, which prevented Frank to assign any greater significance to it. Rather was he at pains to seal any door trough which anthropomorphic design arguments could intrude into physics. In the criticism of this "widely spread manner of treating natural phenomena by analogy to human emotional life." (Ibid., p. 114/90), Frank's book strengthened the tendency of de-anthropomorphization revealed by Mach's historico-critical studies. By recognizing additionally those abstract theories, such as atomic physics, which Mach had downgraded to mere economizations, as theoretical structures of their own right, Frank in effect pushed this tendency of objectivation equally far as Planck, albeit without any realist aspirations. Moreover, he implicitly shared Planck's dislike of the strongly Darwinist and Lamarckian elements in Mach's epistemology. Yet instead of advocating convergent realism, Frank replaced the biological corroboration of scientific knowledge by its social corroboration. Already in the Introduction, Frank formulated his mature stand with respect to the classical Mach-Planck controversy. In his Leiden speech, Planck (1908a) had compared

> the *positivist* and the *metaphysical* conceptions of science. He characterized them perfectly correctly in this way: according to the metaphysical conception, the aim of science is the discovery of an existing 'true' world, while according to the positivist, however, it is the construction of a system of statements with the help of which we can find our way in the world of our experiences. Planck finds fault with the latter conception: the passion and readiness for sacrifice with which men like Galileo have fought for their convictions could not be understood if the matter had been merely purposeful [zweckmäßige] constructions and not the discovery of truth.
>
> However these passions and this fighting spirit are facts that are as empirical as those of physics. … Planck may be right insofar as the establishment of theories by the positivist wing has often been made all too much in empty space, without regarding its connections with the total activity of mankind. … The events around Galileo make it clear that the passionate conflicts connected with a physical theory have nothing to do with its suitability to represent natural processes but much more with their relationships to the political and social events of the time. Therefore there is no need to amplify the positivist conception of science by a metaphysical concept of truth but only by a more comprehensive study of the connections that exist between the activity of the invention of theories and the other normal human activities. (Frank [1932] 1988, p. 26f./14)

Consequently Frank's criticism of vitalism and wholeness made ample reference to the political and societal motivations of these conceptions.

Let me turn to the general problem of randomness and the frequentist interpretation of probability. To Frank's mind, any event could be called accidental or random only with reference to a definite set of causal laws that had hitherto failed to predict it. In virtue of the openness of scientific progress, such a negative definition of randomness never yielded a positive statement about experiences. Consequently, absolute randomness in the emphatic style of Exner (1909) was not a meaningful concept. The goal to formulate positive empiricist conditions of randomness directly led Frank to the frequentist conception of probability. "If we have conditions of experiment such that from them only the frequency [Häufigkeit] can be predicted with which each possible individual result appears among a large series of experiments, the result of an individual experiment is called 'random'." (Ibid., p. 194/158) This definition is not a negative one but "states positively that there is a lawful dependence of the collective experiment upon the conditions of the experiment. Random events are those that are not identified by a definite causal law, but yet are members of a collective experiment whose average results can be considered as given by a causal dependence upon initial conditions." (Ibid., p. 194/159)

How strongly Frank was now indebted to von Mises's frequentism and the Viennese tradition can be seen in the eighth chapter titled "Causality, chance or plan in the development of the world?" After a criticism of the concept of equiprobability and a detailed discussion of the kinetic theory of gases, Frank concluded that

> statements that discuss the probability of a definite state of the world have a meaning only if the world is conceived as a mechanical system that passes through the same states again and again with a certain frequency. And only under this assumption does the concept of entropy of a state as derived from its probability have a concrete meaning. … If on the other hand we did not conceive the world as a system that passes through all states again and again with a certain frequency, the concept of probability of state would lose its sense altogether, and so would the relation between entropy and probability. Also the proposition that the world tends toward its most probable state, would thereby become meaningless. (Ibid., p. 253f./210)

This assumption was nothing but the ergodic hypothesis. In contrast to von Mises, Frank did not dismiss it in virtue of the apparent difficulties of obtaining a mathematically rigorous and physically satisfactory formulation. What prevented him to advocate a unified statistical approach was, to my mind, that Frank wanted to retain the micro-macro distinction as a framework to discuss the issue of historical laws. And indeed while chapter VII had ended with rejecting the then increasingly popular associations of quantum mechanics and free will, chapter VIII set out with the question as to whether there are "strictly causal historical or sociological regularities [Gesetzmäßigkeiten]." (Ibid., p. 239/198) Yet even if the microlaws provided by individual psychology were strict, one would not obtain the existence of strict sociological laws because all measurable sociological quantities referred to macroscopic quantities for which only the average behavior could be predicted.

The various theories of historical and sociological laws are therefore distinguished, essentially, by using different macroscopic state variables. The materialist conception of history, for instance, assumes that knowledge of the present economic conditions is sufficient to predict the future sociological development in its essentials. (Ibid., p. 240/198)

And Frank held that by using more and more state variables the macroscopic level and the psychology of the individuals could be decoupled such that the notorious polemics surrounding materialist historiography, whether great men or the social conditions determine the course of the world, became a metaphysical pseudo-problem.

If we confront Exner's physicalist theory of culture (See Sect. 4.6.) with Frank's criteria of meaning, it becomes clear that it contained empirically meaningless elements even though by postulating a certain repetition of cultural phenomena it met Frank's standards to meaningfully claim a transition to more probable states. No surprise because Exner's theory had emerged from a frequentist approach and abundantly utilized the micro-macro distinction. Supporting his friend Neurath's views about social engineering, Frank had to reject the end of Exner's (1909) Inaugural Address that the distribution of wealth and commodities was given by nature.

The main problem of Exner's application of probability was to find the right state variables and the measure of order. Frank addressed the interest dependence of order at a classical example of natural teleology and its modern probabilistic version. "If in a desert, we were to come across an accumulation of sand that has the form of a regular pentagon, we will hardly believe that this accumulation is the result of the action of the wind and the mutual impacts of sand particles." (Frank, [1932] 1988, p. 254/210) At least since Vitruvius, such example counted as strong evidence for human or divine design. However, the regular pentagon appears so extremely improbable to us "only by allowing no difference between the individual irregular figures. … The whole improbability of the regular figure consists in the fact that we direct our special interest specifically to it individually, whereas the deviations of the irregular figures from each other do not interest us." (Ibid., p. 255/211) Still probability is not well-defined here. The frequentist can assign a probability to the regular pentagon only after all possible shapes of sand heaps have actually formed and each shape has occurred sufficiently often.

If we think this to be possible, we have to divide the time during which the regular pentagon exists without being destroyed by the wind by the whole time past. Our result is the probability for the formation of a regular pentagon by chance. For such a probability to have any meaning at all, the possibility of the formation of the pentagon must already be presupposed, indeed its frequent formation. (Ibid., p. 258/213f.)

While this presupposition poses no problem for sand heaps in the desert, frequent formation in the time available proves problematic in other cases, in particular for the emergence of the universe or the origin of life. Within his frequentist conception Frank had simply to deny a well-defined empirical meaning to the idea of

tracing the origin of organisms back not to macroscopic physical laws but to deviations from these laws, which come about because many microstate correspond to one macrostate. The organisms are formed – according to this conception – by extremely rare microstates during ordinary macrostates.

The question whether the probability of such formations is big or small can be answered only under the assumption that this formation took place within a closed system, in which it was repeated again and again, though after very long intervals. Since however there is no question of such a closed system, as we do not know anything about a closed cycle of events in the known or hypothetically assumed universe, there is not the slightest chance of making an estimate of the probability for the formation of an organic substance. (Ibid., p. 259/214f.)

Frank was of course right to criticize the unwarranted probabilistic arguments for divine design or the presence of teleological factors. But also his frequentist strictures were too narrow. Given that our present theories of evolutionary biology and the origin of the universe admit far too less time for all possibilities to be tried out repeatedly in the actual world, one wonders whether given such a reasonable corroborated basis theory, talk about probabilities should be excluded beforehand. The theory of von Kries, on the other hand, permitted one to speak meaningfully about the probability of a single event. Quantum mechanics has since revealed other problems of a purely frequentist account. Let these few remarks from the present perspective suffice to show how strong Frank was committed to the frequentist interpretation of probability.

Let me close this section with some remarks about Frank's specific treatment of causality and quantum mechanics. It is the hypothesis of (Laplacian) determinism that by the "progress in our technical capacities we can in time get closer and closer to the goal where empirically equal conditions of experiment bring equal results." (Ibid., p. 196/160) The claim that such refinement is possible represents an empirical statement about the results of future physical experiments. The opposite hypothesis – which Frank did not call 'indeterminism' – is only meaningful if the limit of future refinement can be "expressed numerically; the numbers occurring would have to be so-called 'universal constants', that is they would have to be founded within the empirical structure of the real world, as for example the velocity of light, the elementary unit of electric charge, or Planck's quantum of action." (Ibid., p. 196/160) It was the last of these universal constants which set a definite limit to quantum mechanical predictions from a given initial state.

We can describe this fact, the scattering of the final states, if the initial state is given, in two different ways.
First: there are exact causal laws. A definite final state corresponds to every initial state. But very different real states are hidden behind each measured state. The scattering of the final states originates in the uncertainty of the measurement of the initial states.
Second: No exact causal laws exist, but we can state that from a definite initial state a definite final state follows with a certain relative frequency, so that with a given initial state and many experiments, a scattering of the final states becomes noticeable.

… [T]hese two propositions are only different formulations of the same factual situation; they are not two hypotheses, one of which maintains the strict validity, the other the non-validity of the causal law, if we understand this law to be a proposition about actual experiences. (Ibid., p. 200/164)

Restriction to the actual experiences thus, so Frank believed, effectively eliminated the core problem of the philosophical interpretation of quantum mechanics. Either the uncertainty was already in the coordination of the initial state to experiences, i.e., measurement results, or the statistical law only emerged after a sufficiently large number of trials. While the first alternative corresponded to Heisenberg's claim that in quantum mechanics precisely the antecedent of the determinist argument fails, the second alternative could be considered as an early version of the statistical interpretation. The only difference was that Heisenberg had held that the law of causality was invalid in any case.

Not only in this respect, Frank remained more open-minded to future modifications of quantum theory than most Göttingen-Copenhagen protagonists. Regarding the fact that quantum theory makes predictions about 'collective experiments' only, he remarked:

Here, in a certain sense, there is really a gap. It could be filled only if we might find laws that determine the fate of the individual particle. Claims like: the future of the individual particle is perhaps not determined by the laws of physics, but is still somehow determined, are either tautological … or theological. (Ibid., p. 234/193).

This alternative neatly corresponded to his above-discussed analysis of gaps in the nomological structure of the world. Frank was also rather careful about the problem of duality between the wave and the particle picture.

This 'dualism' is in no way mystical, but only characteristic of present theoretical physics, perhaps an imperfection which will one day disappear, or perhaps a permanent disagreeable quality. There has been a similar dualism in [classical] field theory. Already Lorentz's theory of electrons consists of the equations for the electromagnetic field in the 'ether' on the one hand, and on the other, equations of motion of the electrons. … The same dualism was preserved at first in all field theories, as for example in the general relativity theory of Einstein where field equations of gravity and the equations of motion of mass-points (geodetic lines) confronted each other. (Ibid., p. 232/192)

Although Frank could report substantial progress on the understanding of this duality, in a certain sense the duality between field and particles is still with present-day general relativists. Thus once again, Frank applied his usual strategy of integrating an astonishing feature of modern physics into the continuous historical development. The predecessors of quantum mechanical duality emerged only more sharply if analyzed against the backdrop of the new atomic physics. Such a Machian approach to the history of physics was at odds with Copenhagen's absolutistic finality claims and Heisenberg's notion of a closed theory. Of course, Frank held that the mathematical symbolism of quantum theory contained an absolute limit of the speakable. "Since scientific statements deal only with symbols, the question what happens if one always gives the particles the same initial position and velocity, cannot be formulated in wave mechanics" (Ibid., p. 223/184) because it cannot be coordinated to any concrete experience. The main lesson of quantum mechanics consisted in the statistical character of coordination.

That Frank called the duality a temporary imperfection or a permanent disagreeable quality shows, to my mind, that Boltzmann‘s teaching was still alive. Admittedly he did not go nearly as far as Schrödinger who took Boltzmann‘s *Bild*-realism as a requirement for a consistent physical theory. But also for Frank the universality of basic concepts remained a desirable goal. This was, to my mind, one reason why he still avoided any talk about Bohr‘s complementarity, a concept which, in effect, represented and an enormous extension of this disagreeable duality.

Reviewing his friend Frank’s book on causality (1932) for *Die Naturwissenschaften*, von Mises (1932b) praised the general criticism of school philosophy but considered less harsh a tone to be more fruitful. He also assented to Frank’s strategy not to start with a simple general definition of the concept of causality but to start from a detailed analysis of its various applications. This was indeed a major achievement of the book.

8.6. Von Mises Versus Laue and Schrödinger

In 1934 a continuation of the debate between Vienna and Berlin took place between three former Berlin colleagues. It was documented in *Die Naturwissenschaften* still under Berliner’s directorship. Schrödinger supported von Laue’s criticism against Copenhagen’s finality claims and in principle limits of causal description, while von Mises charged von Laue of disrespect for statistical laws. The short debate shows that front lines had changed because strategic alliance were now established predominantly for motives other than indeterminism. From the perspective of whether quantum mechanics in its Göttingen-Copenhagen form was a satisfactory theory, Logical Empiricist’s logic-oriented view at physical theory seemed to conform to Copenhagen’s finality claims while Schrödinger’s *Bild*-realism and his (apparently deterministic) equation associated him with those who were searching for a deterministic theory.

In his contribution to the *Festschrift* for Berliner (See Sect. 5.1.1.), von Laue basically defended the Planckian standpoint by warning against a precipitous dismissal of the principle of causality.

> Certain purely physical concepts, which were based upon experience, have failed in the face of newer experiences; for the time being better concepts are missing. This situation is not uncommon in the natural sciences and predates every larger progress. But these difficulties cannot force anyone to change his epistemological point of view however it may be; although they indicate – as every deep physical question – the importance of epistemological considerations. (Laue, 1932, p. 916, italics of the original removed)

While Planck’s response to Schrödinger’s inaugural address (Sect. 6.3.5.) had still placed his ontological stakes on the wave function itself, von Laue considered the problem as to the nature of material bodies, the quantum enigma, as fundamentally open, even regarding the direction in which progress could be made. To von Laue, Bohr’s duality between wave and particle picture was rather a wish for synthesis than a solution. Moreover, he shared Planck’s and Schrödinger’s criticism of the concept of mass point. In a footnote he approved Schrödinger’s (1932a) claim that the uncertainty relations contain an internal contradiction. “According to its definition, a mass point is a mechanical object defined by the specification of its position, velocity, and mass. Denying the specifiability of position and velocity (momentum) nullifies the concept." (Laue, 1932, p. 916) Thus there is no need to rescue the mass point by renouncing the description of individual events – thus sacrificing the principle of causality – and restricting oneself to statistical regularities. "We do not want to criticize this procedure; *at present* it is probably the best way out." (Ibid., p. 916)

Von Laue assumed a far-reaching neutrality of the mathematical formalism. It was legitimate to proceed on the path of statistics as long as one advanced on it. On the other hand, "often mathematical methods outlive the ideas on which they are based." (Ibid., p. 916) All this does not justify an epistemological sacrifice and a prohibition against the investigation of single processes. "Who wants to presage that *never* anything comes of it." (Ibid., p. 916)

Von Laue's criticism of Bohr's complementarity – though avoiding, as Schrödinger, the term itself –, his rejection of the particle concept, and his assent to Schrödinger's criticism clearly mark him, in the light of Beller's (1999) criteria, as a critic of the Copenhagen interpretation. In contrast to Schrödinger, concerning causality itself he followed Planck by putting general epistemology above the physics accepted at a certain time. Thus it is surprising that to make his case von Laue availed himself of positivist arguments that were closer to the original Machian stand than Heisenberg's alleged positivism.

The unique determination of position and momentum was lying at the very heart of Newtonian mechanics. It was shattered by quantum mechanics more radically than by relativity theory.

> Upon all this [astonishment about Heisenberg's uncertainty relation] one had typically forgotten that [Newtonian mechanics] is nothing but a physical theory, that as any such theory it contains hypothetical elements which transcend the experience it is based upon, and that accordingly – as any physical theory – in contains the germ of death in it *from the very beginning*. One of the few who remained conscious of this was Ernst Mach; but his historico-critical presentation of mechanics never received due attention. In point of fact, mechanics enjoyed a special status in the physicists' consciousness which was psychologically understandable as a consequence of the great experience which everyone has when entering through it as the entrance portal of physics. De facto mechanics held this special status, but not de jure. (Ibid., p. 915)

Von Laue's argument precisely echoed Mach's distinction between direct and indirect descriptions. In contrast to Heisenberg, he argued that there is no need to a priori restrict physical theory to observable entities only. And indeed Mach allowed scientists to temporarily introduce hypotheses because the restriction to direct descriptions was not of a metaphysical kind. Nor did the positivist Mach consider methodological and epistemological strictures as absolutely compelling.

> Already before physics has ignored an epistemological postulate and it did so for a long time; and nonetheless today we consider this postulate to be almost as important as the principle of causality with which it is, incidentally, intimately linked. We are speaking about the principle of local action and Newton's law of attraction which, as is well known, expressed an action at a distance. Newton himself sharply expressed his dissatisfaction with this deficiency; nevertheless theoretical astronomy emerged from his law. As today general relativity has finally instated the principle [of local action] in its right, we know, firstly, that Newton's law will remain an excellent and for all times indispensable approximation and, secondly, that progress beyond it was impossible in Newton's days. (Ibid., p. 916)

Von Laue thus pondered that the principle of causality could well be suspended for decades before a new theory, as deep as relativity theory, would reinstate it. All this did not justify abandoning the principle of causality which set the distant goal at the horizon of the history of physics.

Interestingly, von Laue's strategy to point to the continuity with historical predecessors had been used over and over again by Frank and von Mises. Frank was convinced that the essential characteristics of physical science had not changed since Newton's axioms; quantum physics required no revolution in epistemology.

> The laws of physics consist of mathematical relations between quantities, as well as of directions on how these quantities can be related to actual observations, and in this respect nothing has changed even in the twentieth century. The equations have changed, the quantities are different, and the directions, too, are therefore no longer the same; but the general scheme according to which a physical theory is constructed still has the same fundamental character today as it had in Newton's time. (Frank, 1935, p. 198/128f.)

In 1934, von Laue restated his view about the inapplicability of mechanical concepts in the atomic domain and amended it by a criticism of Bohr's and Heisenberg's disturbance argument. Although he accepted their claim that the feedback of a test body on the measured object put a bound on the precision of measurements in the atomic domain, he objected to considering this as an in principle insurmountable limit. "For such a conclusion is based on the tacit assumption: 'Opening up new possibilities of measurement *necessarily* requires new experimental means.' Only by this assumption can one conclude: 'Since now we have arrived at the finest means, the atoms themselves, we can never get any further." (1934, p. 439) But, so von Laue continued, neither Hertz nor Nernst invented any new means prior to their discoveries, but their progress resulted "from a genial experimental *train of thoughts*" (Ibid., p. 440) using the already available experimental means in a new way.

Hence it would be premature to draw far-reaching epistemological conclusions from the present state of atomic physics and "to arrive at an 'Ignorabimus' *in principle* because present atomic physics and its splendid formalism renounce to answer certain questions." (Ibid., p 441) As von Mises, von Laue attributed the '*Ignorabimus'* to the failed optimism about import and range of mechanical explanation. That atomic physics had established the naiveté of mechanicism did not justify that one, instead, relapsed into an equally uncritical pessimism and considered the task of physics as irresolvable. "Despite all physical pseudo-reasons put forward for it, that pessimism is only the physical consequence of the widespread and deep-seated cultural pessimism which represents a general tone of our times. To deal with this, is no longer the province of the natural scientist; his science stands *above* all human temperament." (Ibid., p. 441) This distinction between science and temperament entirely corresponded to Planck's Kantian separation between the realms of necessity and freedom.

Von Laue's resistance against pessimism, at first glance, supports Forman's later thesis on the continued abandonment of causality after 1927 under the pressure of the milieu (Sect. 1.3.). Von Laue also contradicted the milieu's demand for *Anschaulichkeit.* "There is, it seems to me, and entirely objective measure of progress [in atomic theory] …; this lies in its often criticized unintuitiveness [Unanschaulichkeit]. What one considers as intuitive it a matter of the time." (Ibid., p.

440) This diagnosis was fully in line with Frank's (Sect. 8.4.) And von Laue criticized that in atomic physics the traditional intuitive idea that matter is filling space badly coexisted with the existence of smallest particles. Instead of limits to knowability as such, "[t]he uncertainty relations [Ungenauigkeitsrelationen] – this is my view – pose a limit to any corpuscular mechanics but not to any physical knowledge." (Ibid., p. 441, original in italics) This precisely amounted to Schrödinger's interpretation of them. Continuing this passage, von Laue charged the Göttingen-Copenhagen physicists of shifting the burden of proof in causal matters. "In fact when should causality be counted as 'empirically proven'? Perhaps when the last riddle of natural science has been completely resolved? This stage will probably never be reached." (Ibid., p. 441) This rhetorical question, of course, served to suggest that causality represented a necessary presupposition of natural science that could be temporarily suspended but never abandoned. In correspondence (Sect. 7.4.) Heisenberg had agreed with Schlick that 'invalid' was not tantamount to 'empirically false'. Thus, at bottom, von Laue's argument rather targeted Exner's move against Planck (Sect. 4.4.) that the burden of proof rested with the determinist, because his claims were stronger than those of the indeterminist. Exner nonetheless believed that the question was empirically open. Schrödinger largely followed the Viennese tradition although he viewed the prospects for a decision to be as dim as von Laue did. But, at those places, Schrödinger stressed the conventional character of determinism while von Laue turned to a relativized and temporarily suspendible a priori. Accordingly, this part of the front line between Vienna and Berlin was still intact. Schrödinger's reaction on von Laue's paper, however, shows that the emphasis of the causality debate had shifted to the issue of finality claims.

Only five weeks later, Schrödinger published a short paper in *Die Naturwissenschaften* in which he agreed to von Laue's (1932, 1934) papers "from the bottom of his heart. The aim [of the paper] is to sing the same song, so that it sounded louder and in multiple voices." (Schrödinger, 1934, p. 518) He assented to von Laue's analysis of the disturbance or feedback argument and contemplated that a measuring apparatus shows certain results even without an interaction, for instance, when a target is not hit.[177] Yet Schrödinger's main point was to intensify his earlier criticism that the concepts of classical point mechanics were still applied albeit with absolute limits of precision. (Cf. Sect. 6.3.6.) "The *concepts* must be abandoned, not their sharp definitiveness. One tries to get around the monstrosity of unsharply defined concepts by hundred thought experiments." (Schrödinger, 1934, p. 519) "Among the concepts to be abandoned is also position. But this means: *geometry.*" (Ibid., p. 519) The reason was that geometry was based on congruence the empirical realization of which presupposed the existence of rigid bodies. According to Schrödinger, the application of geometry to real objects represented a gedanken experiment which had to be consistent with the laws of nature. The classical solution to approximate rigid connections by potentials was impossible due to the finite distance between energy levels. Thus there could be only approximately rigid bodies. Schrödinger concluded that "the spatial structure derived from the group of translations fit to nature only approximately – and not merely that there do not exist sufficiently precise material measuring rods to measure it. The true geometry of physics is … the four-dimensional

[177] As a matter of fact, 'interaction-free measurement' is today among the most-discussed topics in the foundations of quantum mechanics.

one of relativity theory. … The difficulty to adapt to the requirement of relativity is a well-known crux of quantum mechanics." (Ibid., p. 520) In short, geometry was inapplicable to small distances. And as he held at least since 1931 (See the letter to Schlick in Sect. 7.4.), quantum mechanics was at odds with relativity theory.

Von Laue's paper (and consequently also Schrödinger's second voice to it) was criticized by von Mises in a letter to the editor. Initially, von Mises assented to von Laue's criticism of the feedback argument.

> But von Laue advocates … the view that one could remain loyal to the determinist conception of physics in spite of the indeterminacy relations. … On this I would just like to remark that, to my mind, one can *at all* assert Heisenberg's indeterminacy relations *only as a proposition of statistical physics,* which within the framework of causal physics does not find a place and is not possible. (Mises, 1934a, p. 822)

Within von Mises's purely probabilistic approach, "it can be left entirely open whether in the cases intended by the uncertainty relations such a 'true' value exists or not." (Ibid., p. 822) And he repeated his argument that speaking of a 'true' value already presupposed the concept of collective. (Cf. Sect. 8.3.)

> If one abandons the point of view that the measurements form a collective, then there exist no distribution, no divergence, and thus no uncertainty relation. I agree with pleasure to von Laue's rejection of the 'Ignorabimus'. But I believe that one has to get used to the fact that the assumption of statistical instead of causal explanations does not signify a renunciation of knowledge but only another and perhaps more advanced form of knowledge. (Ibid., p. 822)

Although von Mises's plea for granting equal rights to statistical explanations represented an important lesson of the whole historical development from Boltzmann to quantum mechanics, within his frequentism it required ontological commitments neither the determinist von Laue not the indeterminist Schrödinger was willing to accept. On the statistical level the interpretational problems of quantum mechanics dissolved trivially. If any ontology was fine that permitted successful predictions, the philosophical problems dissolved as well. But almost every quantum physicist-philosopher wanted more than this minimalistic ontology of mass phenomena. Von Laue wanted concepts that allowed for causal explanation, Schrödinger wanted consistent Boltzmannian pictures, Copenhagen wanted classical concepts to connect quantum mechanics with the macroscopic world, and even his friend Frank wanted a certain linguistic continuity between quantum concepts and the concepts of everyday life. Or so he expressed himself at the 1936 Copenhagen Congress. Already in 1928, von Mises had stressed that his did not heed such aims. His review of the second edition of von Kries's book, which was just a photomechanic reprint with a new introduction concluded as such.

> Only in one respect von Kries's introduction [to the new edition] aptly characterizes the mutual relation of our views. The assumption of a "regularity directly related to and expressed in mass phenomena" indeed appears to me to serve our intellectual need to the same extent as the assumption of a "general lawfulness of nature". I cannot blame anybody for his intellectual needs reaching deeper or wider, but I believe that the whole development of our knowledge about nature points into a direction that requires from us the indicated modesty. (Mises, 1928b, p. 1030)

8.7. Reconciliation and Strategic Alliances: Copenhagen 1936

From 21-26 June 1936, the Second International Congress for the Unity of Science took place in Copenhagen. Its main topic was the problem of causality with special regard to physics and biology. The congress was opened in Bohr's house, and Bohr delivered a lecture on "Causality and Complementarity" that was followed by Frank's "Philosophic Interpretations and Misinterpretations of Quantum Theory" and Schlick's "Quantum Theory and the Knowability of Nature". Schlick did not participate in the meeting because – as he wrote to Neurath on 2 June, 1936 – due to new university regulations it was impossible to obtain leave of absence during the exam week at the end of the summer term. Thus he suggested that, if desired, someone else read his paper to the congress. On 17 June, 1936, Frank sent a postcard to Schlick.

> Thank you very much for sending me your manuscript. I will arrange everything according to your wishes. With its content I agree completely. In its tendency it perfectly coincides with the paper that I have prepared for Copenhagen. I believe that they go together very well and exactly complement one another. I am departing tomorrow.

On the second day of the congress, on 22 June, 1936, Schlick was killed by a former student on the stairs of the University. (See Stadler, 2001) Frank delivered a short obituary still during the congress.

In this section I analyze the contributions of Bohr, Frank, and Schlick and argue that Bohr's notion of complementarity provided the background of a far-reaching agreement between Frank and Schlick because it permitted them to treat the problem of quantum mechanical causality as a problem of theoretical language and experimental arrangements. Within the more general context of complementarity, their original differences – that had, to be sure, never been pronounced explicitly – lost their importance in comparison to the common goal of combating the then sprouting metaphysical and spiritualistic misinterpretations of quantum theory. This was yet another example of how Bohr succeeded in providing common philosophical ground (Cf. Section 2.2.). Against the background of how the interpretative debates in quantum mechanics had developed since 1926, it is clear that Frank's open endorsement of the concept of complementarity – which had still been absent from his book (Frank, 1932) – signified his parting company with Schrödinger to whom the term had become the shibboleth of the Copenhagen interpretation. Thus a main question of the present section will be what motivated Frank and Schlick to adopt the concept of complementarity.

Bohr's talk essentially contained a summary of his earlier papers in *Die Naturwissenschaften* and his rejoinder to the EPR-paper[178] which consisted in a refined and broadened version of the concept of complementarity. The impossibility to sharply distinguish between the autonomous behavior of an atomic object and its interaction with the measuring device, so Bohr set out, "forces us to replace the ideal of causality by a more general viewpoint usually termed 'complementarity'. The apparently incompatible sorts of information about the behavior of the object under examination which we get by experimental arrangements can clearly not be brought into connection with each other in the usual way, but may … be regarded as complementary." (Bohr,

[178] (Bohr, 1928, 1929, 1930) and (Bohr, 1935) answering (Einstein, Podolsky & Rosen, 1935).

1937, p. 295/291) Thus the quantum of action introduced a certain individualization of the single atomic processes.

Bohr, interestingly, compared the principle of equivalence in relativity theory with the idea that "results obtained by different measuring arrangements apparently contradictory because of the finite size of the quantum of action, are logically compatible." (Ibid., p. 295/291) This was, to his mind, the deeper logical significance of Heisenberg's uncertainty relations. Quantum mechanical objects no longer possessed autonomous and inherent attributes as the particles of classical mechanics. Of course, the abstract logical connections of the new theory required a farewell to the usual quest for *Anschaulichkeit;* one of the motivations of an artificial term like complementarity was to avoid any intuitive associations.

We thus see that the impossibility of carrying through a causal representation of quantum phenomena is directly connected with the assumptions underlying the use of the most elementary concepts which come into consideration for the description of experience [position and momentum]. In this connection the view has been expressed from various sides that some future more radical departure in our mode of description from the concepts adapted to our daily experience would perhaps make it possible to preserve the ideal of causality also in the field of atomic physics. Such an opinion would, however, seem to be due to a misapprehension of the situation. For the requirement of communicability of the circumstances and results of experiments implies that we can speak of well defined experiences only within the framework of ordinary concepts. In particular it should not be forgotten that the concept of causality underlies the very interpretation of each result of experiment, and that even in the coordination of experience one can never, in the nature of things, have to do with well-defined breaks in the causal chain. (Ibid., p. 297f./ 293)

Bohr's criticism targeted primarily Einstein's program of a causal completion of quantum mechanics. Schrödinger, instead, combined his call to expel the traditional concepts of classical physics from the atomic domain with a solid indeterminism in the Viennese tradition. (See Sect. 6.3.). In contrast to Einstein, he never looked for "some causal mechanism underlying the atomic phenomena and hitherto inaccessible to observation." (Ibid., p. 298/294)

Bohr's argument for the indispensability of classical concepts took, at first, a Machian tack: all scientific results must be communicated or, at bottom, all science was communicable knowledge. But while von Mises (1930b) (1930b) had used precisely this move to justify the universality and adaptability of the scientific method, Bohr advocated a rather limited notion of communicability. Only the physical concepts corresponding to our classical life-world qualified for communicating experimental results. Against this von Mises could have reasonably argued that results about mass phenomena that are no further analyzable into individual events, can be communicated perfectly well; and in many cases of hydrodynamics they were everything communicable. Moreover, von Mises and Frank repeatedly emphasized that the concepts of Newtonian mechanics had become the basis of daily talk only because this theory, rather than Aristotelian physics, was part of the school curricula. Bohr's final argument came close to Frank's (1932 [1988]) insight that causality – or rather specific causal laws – still represented as pragmatic presupposition of our daily life. We could not act properly if there were gaps in the causal chain; the good news of Frank's book had been that finding a gap represented a merely negative result that was provisional or even idle until, positively, definite limits of knowledge were empirically established.

Present quantum mechanics, so Bohr held, was just the first step from classical causality to complementarity. Thus it could not provide any solution to philosophical questions such as ‘mechanism or vitalism’, ‘free will and causal necessity’.

> Just the fact that the paradoxes of atomic physics could be solved not by a one sided attitude towards the old problem of ‘determinism or indeterminism’, but only by examining the possibilities of observation and definition, should rather stimulate us to a renewed examination of the position in this respect in the biological and psychological problems at issue. (Bohr, 1937, p. 299/295)

And in the remainder of his paper, Bohr provided some sketches of how complementarity could help to avoid futile metaphysical controversies about living organisms. In particular, “every experimental arrangement suitable for following the behavior of the atoms constituting an organism … would be incompatible with the maintaining of the life of the organism.” (Ibid., p. 300/296) The phenomenon of life and Planck’s constant shared the property of being elementary facts about nature. “Thus the existence of life itself would have to be regarded in biology, both as regards the possibilities of observation and of definition, as no more subject to analysis than the existence of the quantum of action in atomic physics.” (Ibid., p. 301/296) Although Bohr rejected “every compromise with any anti-rationalistic vitalism” (Ibid., p. 301/296) as well as the mechanist world-view, such a categorical limit of further analysis was not without problems for Frank who had rejected vitalism not the least by emphasizing the openness of science (See Sect. 8.5.).

Bohr was confident that complementarity would also clarify the relation between the activities of life and the second law of thermodynamics. As Frank had done, Bohr objected to any association between quantum physics and spiritualism, but he nonetheless believed that complementarity “would rather seem suited to put the old problem of psycho-physical parallelism in a new light.” (Ibid., p. 302/297) Moreover, he hoped “that the epistemological attitude which had led to the clarification of the much simpler physical problems could prove itself also helpful in the discussion of psychological questions. … Above all, just the impossibility in introspection of sharply distinguishing between subject and object as is essential to the idea of causality would seem to provide the natural play for the feeling of free will.” (Ibid., p. 302f./297) Although he thus pronounced one of the most forbidden words of Logical Empiricists, Bohr concluded his speech with the hope to have “to some extent succeeded in giving you the impression that my attitude is in no way in conflict with our common endeavors to arrive at as great a unification of knowledge as possible by the combating of prejudices in every field of research.” (Ibid., p. 303/298)

A principal goal of the papers of Frank and Schlick was to put Bohr’s approach into an empiricist perspective. As Schlick put it, their task was “one of interpretation, not of correction.” (Ibid., p. 318/483) Yet although they, accordingly, readily adopted Bohr’s notion of complementarity, both were at pains to pinpoint its precise empirical content where possible and to properly distinguish it from metaphysical readings given to it by other scientists and philosophers. It is another matter whether their interpretation really was the most adequate one.

To Frank, metaphysical misinterpretations were endemic to scientific progress. “As soon as any new physical theory appears, it is used to contribute something toward settling the controversial questions of philosophy, the questions on which philosophers have been working for centuries without coming a single step closer to their solution." (Frank, 1937, p. 303/158) Their favorite controversy between materialism and spiritualism Frank had studied at length in his book (See Sect. 8.5.). It manifested itself in the persistent resurgence of animistic conceptions that laymen and scientists had imbibed through the general world view. "Every crisis [Wendung] in the history of physical theories is associated with a certain lack of clarity in their formulations, and this unfulfilled longing [for a return of the anthropomorphic conception of nature] bursts forth with great strength from the unconscious." (Ibid., p. 304/169) As outlined in Section 8.5., Frank was well aware of the influence of the social embedding of science. Yet in his Copenhagen address, Frank took a more linguistic tack which made possible, so I shall argue, a rapprochement with Schlick.

"[E]ven the slightest similarity in the wording is enough to induce the physicist to offer a proposition of his science as support for the idealistic philosophy." (Ibid., p. 305/160) These misinterpretations ran in two steps. "First, physical propositions that are really statements about observable processes are regarded as statements about a real, metaphysical world" that are, of course, scientifically meaningless. Second, such "proposition, by means of a rather small change in wording, goes over into a proposition which again has a meaning, but is no longer in the realm of physics; it now expresses a wish that people should behave in a certain way." (Both ibid., p. 306/160) Misinterpretations were thus produced by mismatches between the empirically meaningful realms of scientific propositions and ethical imperatives that resulted from the transition through the meaningless real, metaphysical world. Frank called for a "direct short circuit between the physical principle and the moral principle. This can be done, for example, through the consistent use of the 'physicalist language', which Carnap and Neurath have suggested as the universal language of science." (Ibid., p. 306f./161)

The Vienna Circle's physicalist program, the outcome of the so-called protocol sentence debate, motivated Frank's analysis of the concept of complementarity and it provided a new context for Bohr's thesis of the indispensability of classical concepts, a thesis which Schrödinger had criticized so heavily. There are clear indications that Frank followed Neurath's brand of physicalist language that was oriented at everyday expressions about events in space and time rather than Carnap's that took the statements of physical science as the basic statements. Compare Frank's concluding judgment.

> The great importance of Bohr's complementarity theory for all branches of science, especially for the logic of science, seems to me that it starts out with a language that is generally understood and accepted, the language used to describe the gross mechanical [grobmechanischen] processes of motion. Its significance lies in the fact that in its use all men are in harmony. In physics this language is used in such expressions as 'position of a particle', in the sense of gross mechanics. Atomic processes, however, cannot be described in this language, as the new physics has shown. Bohr has demonstrated in a careful analysis of modern physics that certain parts of the language of everyday life can nevertheless be retained for certain experimental arrangements in the field of atomic phenomena, although different parts are required for different experimental arrangements. The language of daily life thus possesses complementary constituents which can be employed in the description of complementary experimental arrangements. (Ibid., p. 316/170)

And Frank contemplated an application of Bohr's complementarity, now reinterpreted in the Vienna Circle setting, to problems of psychology. One might always start with everyday language and amend it, if limits of applicability arise, by the protocol language of Carnap and Neurath, the symbol language of psychoanalysis, and the phenomenal language of Carnap's *Aufbau*, each of which could be seen "as a constituent of a general language in the sense of Bohr's conception." (Ibid., p. 317/170) This pluralist conception of language came rather close to Neurath's intuition that physicalistically purified everyday language should be the embedding of and connection between the languages of the single sciences, the special languages. The end product of this train of thought was what Neurath later came to call the 'universal jargon'; it was substantially different from Carnap's conception most pointedly expressed, still later, in his linguistic frameworks.[179]

This background elucidates why Frank counted it as an advantage that everyday gross mechanical language remained applicable within certain limits and preferred this view to Schrödinger's search for a better, consistent and unambiguous, conceptual framework for quantum theory. However this search came out, its product ultimately would have to be related to the language – not to the 'things' (See Sect. 6.2.) – of everyday life. As for a Machian any scientific theory was provisional, this was motivation enough for Frank to side with Bohr's complementarity against ideals of completeness. But Frank's linguistic approach to classical concepts substantially differed from the much stronger status which Bohr and Heisenberg ascribed to classical physics as such in the theory of measurement. Physicalism had more conceptual leeway than a theory crucially dependent on measurement apparatus.

In this vein, Frank proposed the following formulation of Bohr's notion of complementarity which did not contain any metaphysical enunciations about constituents and cognizability or even the indefiniteness of the 'real' world.

> The language in which occur statements like 'The particle is at this place and has this velocity' is suited to experiences involving gross mechanical processes and cannot be employed satisfactorily for the description of atomic processes. However, one can give a group of experimental arrangements for the atomic domain in the description of which the expression 'position of a particle' can be used. In the description of these experiments – and in this consists the idea of Bohr – the expression 'velocity of a particle' can *not* be used. In the atomic domain, therefore, certain parts of the language of gross mechanics can be used. The experimental arrangements, however, in the description of which these parts can be used, exclude each other. (Ibid., p. 310/164)

This formulation, so Frank held, blocked three widespread misinterpretations. Here is the first one. "'It is *impossible to measure the position and the velocity of a particle simultaneously*.' The world, therefore, just as it is according to classical mechanics, is filled with particles having definite positions and velocities; unfortunately we can never attain a knowledge of them." (Ibid., p. 307/162) This view produced the same pseudoproblems as Kant's thing-in-itself because it introduced in principle unknowable objects. The second misinterpretation came close to Schrödinger's criticism of the particle concept and Frank remained open to redefinitions.

> [P]articles 'in general *do not possess definite positions and velocities* simultaneously.' … [But] the combination of words 'particle with an indefinite position of velocity' transgresses the syntactic rules according to which the words 'particle', 'position', and 'indefinite' are ordinarily used in physics and everyday life. Of course, there would be no objection if a new syntax were introduced for these words

[179] Admittedly, I am too brief here, but placing Frank in this context would be a topic of its own; in particular because the context itself is still under discussion. See (Uebel, 1992) for a broader discussion of the protocol sentence debate and physicalism.

for the purposes of quantum mechanics. In that case, expressions like ‘particle with an indefinite position’ could be employed inside of physics without any danger. And there exist many correct works on the quantum theory in which this is the case. (Ibid., p. 308/162)

A case in point, one might surmise, was quantum logic. Yet the problem of such a special language was, to Frank’s mind, that the connection to other domains of knowledge would typically be established through metaphysical talk about particles as constituents of the ‘real’ world.

A third temptation to metaphysics was the complementarity between space-time and causal descriptions. Frank was well aware that Bohr‘s notion of causality was a very particular one.[180]

In this way the fact is often hidden that this again only means the complementarity of position and momentum or time and energy. By ‘causal description’ we understand here only the description by means of the principles of conservation of energy and of momentum, which does not quite agree with what is usually understood by causality. (Frank, 1937, p. 309/163)

At the end of his paper, Frank discussed the misuse of complementarity as an argument for vitalism and free will. This was certainly the point where the consistent empiricism of the Viennese – Bohr’s declared rejection of vitalism and spiritualism notwithstanding – had qualms with Bohr himself, not only with his philosophical interpreters. Thus Frank filed his “objection to the use of the words ‘free will’ for the description of certain situations, corresponding to the experimental arrangements in physics.” (Ibid., p. 312/166) The problem of Bohr’s analogy was that in contrast to ‘position of a particle’, the term ‘free will’ was of a metaphysical origin and had no meaning in the language of everyday life, where ‘freedom’ merely denoted the absence of certain external coercions.

Concerning the complementarity between observation in atomic detail and persistence of a living organism, Frank emphasized the distinct experimental arrangements used to investigate an organism as a physical system and in its vital functions. Yet although Frank judged Bohr’s way of putting the matter as tenable, he raised doubts as to its usefulness. At bottom, Bohr had established an analogy between the transition from classical to quantum physics and the transition from quantum physics to the science of animate bodies. But the difference was that in the first case complementarity emerged from a positive law of nature, Heisenberg’s uncertainty relations, while in the second case it was just based on a negative assertion about contemporary physics. Thus the analogy was strictly valid only if empirical evidence was presented

that the exact physical observation of the atoms of a living body is incompatible with the known empirical laws for the behavior of living bodies and with the physical hypothesis about their atomistic structure. As long as this evidence has not been submitted, it follows only from Bohr’s train of thought that in biology, in the present state of our knowledge, the complementarity mode of expression is *possible* and perhaps even *desirable*. In contrast, for the transition from classical physics to quantum mechanics one can conclude that in atomic physics the complementarity mode of expression is *necessary*. (Ibid., p. 315/169)

[180] Cf. the distinction drawn by (Ben-Menahem, 1989) and discussed in Section 6.2.

Schlick, in the paper read by Frank, was even more critical about this application of complementarity. What Frank had called an analogy, Schlick took as the psychological stimulus [Anregung] "that the situation which led to the establishment of the quantum theory might possibly (or as Bohr even thinks, probably) repeat itself elsewhere." (Schlick, 1937, p. 325/488) Quantum concepts could well fail in biology as classical concepts did in atomic physics. Accordingly, "organic regularities [Gesetzmäßigkeiten] … will perhaps have to be formulated in specific concepts, differing from the known physical concepts in the same way as quantum concepts differ from the classical." (Ibid., p. 325/489) But some enunciations of Bohr could "be expounded as though we were bound to believe that in knowledge of the organic we are confronted with a problem essentially insoluble." (Ibid., p. 325/489) This rocked the foundations of Logical Empiricist's creeds: unity of science and epistemological optimism – or the rejection of the *Ignorabimus* that already Frank (1929) and von Mises (1930b) had taken as the core of the modern scientific world view.

The method of knowledge in biology cannot be different in principle from that of physics. All observations that can be made of organisms can likewise be described in classical terms, and the task of science consists in finding a formalism that permits us, from the observed behaviour of an organism, to predict its future behaviour as exactly as possible (the latter in turn being naturally described in classical terms only). Either such a formalism exists – in which case it is discoverable after the fashion of all empirical inquiry, namely by inductive conjecture; or it does not exist – and this would mean that no law is present. (Schlick, 1937, p. 326/489)

Schlick's conception of causality was still liberal enough to be optimistic about obtaining biological regularities in a suitable scientific language. And by way of the conception of coordinative definitions, Schlick's insistence on classical concepts as the basic language of all scientific results, that is, in terms of "observation-protocols [that] ultimately describe events in the ordinary space and time of everyday life" (Ibid., p. 320/484f.), was less of an ontological commitment to classical physics than was Bohr's theory of quantum mechanical measurement. On the other hand, all this liberal epistemological and nomological structure was set up to draw all the more rigid another border line against metaphysics and absolutely unsolvable problems. This was Schlick's final conclusion.

The whole question furnishes a fine example of an important principle of consistent empiricism, as upheld, for instance, by the Vienna School; the principle that nothing in the world is *intrinsically* unknowable. There are many questions, to be sure, which for practical or technical reasons will never be answered, but a question is intrinsically insoluble only in the one case where it is no question at all, and we are dealing, therefore, with a problem wrongly put. The limit of knowability lies only at the point where there is nothing further to which knowledge could address itself. Where the quantum theory sets a limit to causal knowledge [Kausalerkenntnis], where it tells us to abandon the search for further causes, this does not mean that the additional laws [Gesetzmäßigkeiten] still at work must remain unknown to us; it means, rather, that additional laws do not exist and cannot be propounded, since the question about them would make no sense. (Ibid., p. 326/489f.)

Echoing this 'important principle' Schlick's paper opened with a succinct description of his views about quantum mechanics. Since "to know nature is to establish natural laws", where "by natural law we mean a formula which permits us to predict events", the limit drawn by quantum mechanics reached deeper than possible differences on causal matters.

> [H]owever we may chose to formulate the conclusions which quantum mechanics yields for the causal principle, it is nonetheless certain that this theory restricts in quite specific fashion the possibility of predicting physical processes. … It therefore sets an insuperable limit to the knowability of nature. It is in fact a limit to the possibility of prior causal determination. (All ibid., p. 317/482)

While Frank had been more careful and mentioned possible imperfections, Schlick was clearly willing to accept Copenhagen's finality claim. Everybody admitted that quantum mechanics, trivially and like any scientific theory, could turn out to be false at the end. In contrast to Frank, Schlick did not advocate a pluralistic conception of different languages embedded into and mediated by the physicalist language of everyday life. The limit of language was the limit of knowability; quantum mechanics was the pivotal example of an empirically corroborated theory that explicitly stated its limits.

Notice that Schlick's motivation to subscribe to the finality claim was neither Bohr's authority – repeatedly stressed by Beller (See Sect. 2.2.) – nor any ontological predilections. Rather did he repeat a move made already in the early 1920s to block neo-Kantian interpretations of general relativity by a strict reading of the verificationist criterion of meaning and by taking physical theory as a rigid system of axioms. Rather than the synthetic a priori, the metaphysical idea to be excluded this time were the Kantian 'things-in-themselves' and the belief that the question as to their nature represented "a meaningful problem, whose solution, indeed, could in principle be found by beings organized different from ourselves" (Ibid., p. 318/483) and endowed with intellectual intuition. Equipped with this philosophical background, so Schlick held in full accordance with Frank, many "authors have welcomed the gaps in causality disclosed by recent physics" as a "scope for certain pet metaphysical ideas." (Ibid., p. 317/482) This shows that as regards the opponent, Schlick's criticism of Kant was quite in line with his Viennese colleagues. The main difference, however, was that Frank's radical empiricism was more flexible to possible modifications of quantum theory than Schlick's verificationism.

In his last paper, Schlick had not changed his mind on matters of causality as compared to 1931. And as regards probability, he remained on Kriesian grounds. In quantum mechanics, so he wrote,

> it is not possible to make this connection [between experimental conditions and experimental results] in a wholly unambiguous way, and this is just what we refer to in speaking of an abandonment of strict causality. The uncertainty relations establish for the experimental results, and thus for the values of the measured quantities, a quite specific range [Spielraum], having objective significance; they do not refer to any subjective ignorance on our part. (Schlick, 1937, p. 320/485)

Accordingly, Heisenberg's uncertainty relations affected the range of possible outcomes on which quantum probabilities were defined. Or put differently, coordination between symbols and experiences yielded objective possibilities. This marked a substantial difference to Frank's relative frequency approach within which the basic object of quantum mechanics was given directly by statistical collectives because coordination itself was of a statistical nature. From the standpoint of Schick's (1931) theory of causality, such an approach suffered from an imperfect separation between law and disorder.

No wonder that Schlick emphasized the historical cleft between the kinetic theory of gases and quantum theory which for Vienna Indeterminists just represented an important step within a rather continuous scientific development.

> [T]he concept of probability plays an altogether different role in modern physics from that assigned to it, for example, in the kinetic theory of gases. In the latter, the description of nature with the help of statistical mean values is introduced *faute de mieux*, as it were, because we are not in a position to track down the elementary processes in detail …; we therefore renounce insight into the minuter molecular processes, though without of course doubting their existence. In quantum theory, by contrast, the probability viewpoint is not introduced in consequence of any such renunciation, for here it is the appropriate mode of description. Apart from it there are no further independent laws of elementary processes, which have remained hidden from us. The quantum laws lay claim to being a complete and exhaustive description of nature. (Ibid., p. 319/484)

Setting aside their markedly different accounts of the history of probability in physics, the notion of complementarity provided a territory for compromise. Recall that Bohr had introduced complementarity as a generalization of causality – not only of his own rather narrow concept of causality. It was mainly Frank who changed his position, as it were, by a linguistic turn and adopted the physicalist language of Neurath and Carnap. Thus both could approach complementarity as a problem about the validity of concepts.

Still, there were important divergences of opinion. While to Frank different experimental arrangements corresponded to different statistical collectives, to Schlick they corresponded to different ranges. And Schlick remained critical about indeterminism because, as in 1931, he still accepted only a negative concept of disorder, or randomness, relative to certain laws, while Frank (See Sect. 8.5.) considered statistical laws as a positive description of randomness. As the causality debate had, ultimately, been absorbed by the notion of complementarity, Schlick's dislike of the adjective 'undetermined' found another object: quantum logic.

> There are philosophers who maintain that it is possible to speak of an indeterminacy of reality, in the sense that a meaningful proposition about it has to be answered, not with 'yes' or 'no', but at most with a statement of probability. This would mean that there were meaningful propositions about reality which are neither true nor false: such a view would thus contradict the principle of excluded middle, and it is to be rejected as utterly absurd. To declare a real situation to be in a certain respect objectively 'undetermined', can in fact only mean that certain propositions about it are neither true nor false, but *meaningless*. (Ibid., p. 322/486)

One of these philosophers was Reichenbach. But we have seen above that also Frank thought it a viable possibility to give a scientific meaning for the assertion 'the particle has an indefinite position'. He rejected it because of the greater advantages of using a language that was close to everyday physicalist language. Schlick's open rejection of quantum logic reached again back to the 'important principle of consistent empiricism': there cannot exist something in principle unknowable; theoretical description is complete. While Schlick's principle pushed him very close to the finality thesis, it estranged him, on the other hand, from a research program launched from within the Göttingen-Copenhagen group by von Neumann's *Mathematical Foundations* (1932).[181]

[181] To be sure, the definitive formulation of quantum logic came a few years later.

Moreover, Schlick also discarded Heisenberg's and von Neumann's allegedly positivist talk about quantum mechanical measurement. He was convinced "that the question of the psycho-physical relation has been wrongly dragged into the debate, and in any case has nothing to do with the relation of observer and observed, in the sense that matters in quantum theory." (Schlick, 1937, p. 318/483) And discussing the second misreading of quantum mechanics mentioned by Frank, Schlick discarded the widespread answer

> that in itself the electron has neither a quite specific position nor a definite velocity, but that by the act of measurement – e.g., by means of very short-wave light – a specific position is nonetheless *given* to it; or more generally, that by means of a particular experimental arrangement it is compelled, as it were, to confess to a specific state. Yet even this mode of expression seems to me inadequate. (Ibid. p. 321/485f.)

The interpretation of a blackened spot on a photographic plate to denote that the electron was located at a specific position is already based on a theory of measurement that comes afterwards. Thus Schlick also rejected certain Copenhagen-style formulations according to which the measurement result emerged or even was created by the interaction. Despite ambiguous wording at places, such views were not really defended by the leading physicists of the day, but they played a major role for the return of spiritualism, idealism, vitalism, and the like, against which the papers of Frank and Schlick intended to fortify a united front.

8.8. The Debate Ends

In the end, there were three reasons why about 1936 we can, on the one hand, witness a rapprochement between Frank and von Mises, the Vienna Indeterminists turned Logical Empiricists, and Schlick, who came to this movement from neo-Kantianism and the philosophical thoughts of Planck and von Kries, while, on the other hand, the apparent distance between the former and Schrödinger increased. First, the Copenhagen interpretation increasingly won the community's acceptance over the factual competitors. Logical Empiricists primarily analyzed given scientific theories. When an empirically corroborated scientific theory contradicted some methodological or philosophical principles, the latter had to go. For really existing scientific theories, conceptual gaps and historical contingencies abounded, so that an improvement or completion of a scientific theory in the far future was beyond their scope of analysis. As a physicist-philosopher, Schrödinger had also other goals than scientific philosophy.

Second, the Vienna Circle continuously developed the instruments of logical and linguistic analysis of science. After his book had appeared in 1932, Frank adopted Neurath's and Carnap's physicalism. This made possible that Bohr's notion of complementarity provided a domain of compromise, other differences notwithstanding. Frank's linguistic turn estranged him from Schrödinger's unchanged commitment to Boltzmann's *Bild*-realism. The common Machian basis lost its importance as an identifying factor in the causality debate because the Copenhagen interpretation declared itself a positivist tradition. The doubts which Schrödinger partly shared came from the declared anti-positivists. Thus, in a certain sense, Schrödinger had become homeless and the common heritage of the Viennese reading of Boltzmann had ceased to be a cohesive factor.

Third, after 1933 the political situation and the intellectual milieu of the German speaking countries underwent drastic changes. In Germany and Austria fascist governments were in helm, socialists and Jews were forced to emigrate. The general intellectual milieu again favored – more powerful than in the turmoil after 1918 and with organized oppression – irrationalism, *Anschaulichkeit*, and individuality in the sense of genius and leadership. Nobody among Logical Empiricists' contemplated adaptation. Nor did Schrödinger, even though he was far from associating the scientific world conception with politics as most Vienna Circle members did. Frank and Schlick had been actively involved in the defense of relativity theory during the early 1920. And in particular Frank drew intimate connections between relativity theory and quantum mechanics as far as their philosophical import was concerned. In the changed political context of the mid 1930s, the newly emerging misinterpretations of quantum mechanics appeared to them as a dangerous *déjà vu* that required forceful opposition. Schlick's and Frank's goals to render the principles of modern physics unassailable by its critics thus suggested a strategic alliance with Copenhagen's efforts at an interpretative fortification. As we have seen, this was not a finality claim about quantum mechanics.

Two years after the Copenhagen congress, all living protagonists of the present story but Planck had emigrated and left the Continent. Thus the tradition of Vienna Indeterminism ended together with the European phase of Logical Empiricism. The causality debate investigated in this book was no longer alive because its protagonists did no longer communicate on this subject matter. Moreover, the interpretative debates about quantum mechanics largely faded away. Apart from isolated events they would not resume until Bohm's (1952) papers.

In emigration, Logical Empiricists did little to get the history of their movement straight. The devastating effects of the so-called received view on debates about positivism are today well-known. While Logical Empiricists' papers from the European days were little read, their intellectual ancestry, Mach, Boltzmann, Planck, Exner, was not read at all. What remained for the context of quantum philosophy was their endorsement of complementarity and the vague association of their positivism with Heisenberg's. It neatly combined with interpreting the staunch Machian Schrödinger as a realist.

But apart from getting the historical record straight, I have promised a second lesson. In the causality debate analyzed here, scientist-philosophers and scientific philosophers interacted in various dialogues which involved certain types of philosophical arguments and were indebted to traditions centering around them. Pragmatic criteria of theory choice and well-entrenched philosophical principles competed in the interpretation of empirical results and theoretical foundations. Such an interaction was probably a unique feature of the German speaking physics community of the day, but it provided that continuous encounter between science and philosophy which modern philosophy of science aspires at. This interesting object, I believe, should not be minimized by stockpicking in order to maximize the score of a beloved interpretation of quantum mechanics.

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My quotations are based on German originals and translations are usually mine. But where translations exist or even have been published during the author's lifetime, I have tried to follow them as long as there were no relevant departures from the originals. I mainly interfered in order to restore a terminological continuity between various authors existing in German which permitted to spot allusions or criticism. For these reasons citations contain both the page number of the German original followed (after a slash) by the page number of the indicated English translations.

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