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Civilizational analysis and the problem of contingency

COMCAD Arbeitspapiere - Working Papers

General Editor: Thomas Faist

No. 82, 2010

2010

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Bielefeld: COMCAD, 2010

(General Editor: Thomas Faist; Working Papers – Centre on Migration, Citizenship and Development; 82)

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In the field of the comparative study of civilizations especially authors of the so-called California School, notably and most convincingly Kenneth Pomeranz, have argued that the English industrial revolution and – as a consequence – the rise of the West and the falling back of China were the results of rather contingent processes. The industrial revolution in England, in this view, was not some kind of a logical end-result of historical processes that have started many centuries before 1760 as authors from Max Weber up to the majority of current Western sociologists will want to make us believe by pointing to typical Western societal features like the Roman law tradition, a particular work ethic or a particular form of the state-system. Pomeranz and others, in contrast, have forcefully argued that only easily accessible mineral resources like coal and a particular type of colonies made it possible that England and not China had the chance of an early industrial take-off. But it could have happened otherwise! China could have been the first industrial giant, if – so Pomeranz’s argument goes – only some contingent conditions would have been different.

I will not discuss the spectacular and certainly not uncontested arguments of Kenneth Pomeranz and those scholars who have followed his lead. My point is rather a methodological one since I believe that the role of contingency in the field of macro-sociology in general and in civilizational analysis in particular has been underestimated or misunderstood. My claim is that most or even all research which seriously tries to trace social and historical processes in some respect deals with contingency. Many of the great books of the social sciences implicitly or explicitly use some kind of a path-dependent argument in which some contingent initial conditions have led regions, countries or civilizations towards very different outcomes. Whether it was – in the case of Max Weber – the role of the Protestant Reformation which laid the tracks for modern capitalism, whether it was – in the case of Barrington Moore – the solution of the agrarian question which led either to democratic or to totalitarian political systems or whether it was – in the case of Shmuel Eisenstadt – the axial age which led to certain types of social and political conflicts and particular societal dynamics: In all those examples a contingent founding moment is at the centre of the story: A contingent event (or “turning point”) initiates a path-dependent, often highly deterministic development. In some respect all these authors use arguments of path dependence and thus – quoting James Mahoney – must deal with the following problem: certainly one of the most sophisticated authors within the debate on path-dependence: “The identification of path dependence (...) involves

both tracing a given outcome back to a particular set of historical events, and showing how these events are themselves contingent occurrences that cannot be explained on the basis of prior historical conditions. Because the presence or absence of contingency cannot be established independent of theory, the specification of path dependence is always a theory-laden process.” (Mahoney 2000, 507/08) Note, that Mahoney is not making a claim about the contingent or determined character of reality. His claim is not an ontological one. His argument is rather that social scientists do construct theories with which some facts – the contingent event which triggers the development – cannot be explained by the theory being used in explaining the path-dependent process. It is not the event in itself which is contingent; it is the event being theorized within a particular paradigm which appears to be contingent. Thus, a contingent event or a “turning point” is created by the theoretical paradigm being used. Or, as I would put it: The talk of “contingent events”, of “path-dependent developments”, or “turning points” is just part or element of a (theoretical) narrative, of a “theoretical” story. If that is true, then the specific features of narratives and stories have to be taken seriously while doing macro-sociological research on civilizations. To formulate it in a more critical way: Only very few authors acknowledge the methodological challenges connected with the phenomenon of contingency. Contingency – so my argument goes – forces scholars especially within the field of civilizational analysis follow at least four methodological postulates that are closely connected however.

1. Every narrative strategy requires the selection of events and structures for setting up a convincing “plot”. And these events and structures have to be brought into a temporal order so that the reader – as David Maines (1993, 21) has pointed out – gets some insight into the “tempo, duration, and pace” of the process under consideration. This seems to be trivial, but does in fact have enormous methodological consequences which are often neglected. Since the contingent event, the founding moment, the turning point is often considered as the most important element of a (theoretical) story, scholars often implicitly ascribe these events and moments some kind of a „genetic code“ as Michel Dobry has put it: The contingent event or turning point is exclusively considered from the perspective of the end result of the path-dependent process so as if the contingent event already contains the seeds of the developments still to come. Thus a strong teleological element enters the whole theoretical construction. When this is the case then it oftentimes doesn’t seem necessary any more to describe the sequence of the events in detail. Since the “plot” is exclusively created from the point of the end-result of a particular process, some kind of a deterministic development is implied making a serious analysis of “tempo (...) and pace“ superfluous.

Such teleological and deterministic assumptions can be found in Eisenstadt’s work when he tries to describe the trajectories of civilizations after the Axial Age: After the triggering event of the invention of transcendence the reader gets the idea that the cultural tracks of a particu-

lar civilization are laid down once and forever so that the outcomes of civilizational processes are somewhat predetermined. Such a problematic impression is supported not the least by Eisenstadt's own talk of "cultural programs" (kulturelle Programme) that supposedly shape civilizations (cf. Eisenstadt 2000, 15ff.).

With respect to this particular problem my argument would be as follows: In order to avoid teleological constructions, a detailed analysis of the tempo and pace of a path-dependent process is absolutely necessary. However, can this be really successfully done within civilizational analysis that often covers hundreds or even thousands of years? Is it really possible to demonstrate that there is a kind of (cultural) logic that propels the dynamics of civilizations from the very beginning of their existence? I have some doubts about that. The shorter the time span of a story, the easier it is to give the details of the tempo and pace of the path-dependent process. Therefore I would argue that one should be sceptical towards constructions which try to bridge hundreds or even thousands of years between the beginning and the end of a process. I don't think that one can really convincingly demonstrate the continuation of mechanisms that last such a long time-period. It is simply questionable whether the claims being made in such huge stories can really be demonstrated in a non-speculative way.

2. Path-dependent analyses often focus rather exhaustively on the starting point of a development. There are some good reasons for such a focus, of course, but it must not be forgotten that also the end of such a development has to be justified by theoretical means. The end of a path-dependent process – like its beginning – cannot be found in reality but is also a part of a narrative construction. Only a clear statement in regard of the explanandum of the research question makes it possible to give plausible reasons concerning the endpoint of a social process. Now, this also sounds rather trivial.

However, one has to be aware that a majority of path-dependent analyses often only regard two "turning points", one, with which the path begins, and one with which it ends and a new and different path might emerge. All too rarely the question is asked whether in between these two "turning points" there might be other events of interest, even other possible turning points which could make it possible to tell a completely different theoretical story, to see a completely different path. And – in addition – all too rarely still another question is really dealt with, namely whether the originally discovered "turning" and "end-points" might be considered as merely intermediate stops of a development which in fact bridges a much larger time period as the originally created path-dependent story was intended to do (Haydu 1998, 353): Thus, what right now might look like some kind of a starting point of a development could be – using another theoretical framework – also be regarded as the end of a path or its continuation. If that is true, then it is some kind of a methodological necessity to question ones

own narrative constructions by continuously reconsidering alternative plots concerning different paths.

This is particularly relevant for civilizational analysis since in a very odd way this type of research sometimes has a kind of a-historical touch – despite all historical ambitions! It seems too tempting to construct the “problématique” of the Axial Age (800-200 B.C.) from the standpoint of the present period, to contrast – let’s say – Chinese and Western (Christian) civilizations as seen in the year 2010. But would such a reconstruction have been different if done in the 1980s? I think, it would! And this is so not because we nowadays know much more about historical details. Today (2010) it is simply not convincing any longer to reconstruct the contours of the Axial Age in China in the same way as that was done by Eisenstadt in the 1980s. Too much has changed concerning our picture of the possible dynamics within Chinese culture and civilization – which leaves only one conclusion: One should either avoid to bridge such long historical periods (that was my first point) and/or one should at least think about various (i.e. more than two) turning points in history in order not to fall into teleological traps.

3. My discussion concerning the theoretical justification of starting- and end-points of developments immediately brings up the question of the truth content of path-dependent analyses. Within the institutionalist debate of the last decade it has been criticized that most authors have assumed some kind of a hyper stability of a path once taken. For example, it was assumed that the structures of the so-called Rhenish Capitalism in Germany have been fixed in such a rigid way that a real change seems impossible. Yet, change has come – as Wolfgang Streeck and Kathleen Thelen in the field of political economy have argued – even if only by incremental steps. Thus assumptions concerning stability of paths have turned out to be highly problematic. And I would like to suggest that this must have consequences for civilizational analysis as well. Thus we are forced to answer the question how and by what means it is possible to substantiate the truth content of narratives.

Narratives cannot produce their own truths; they can only be made plausible by being supported by good arguments (Büthe 2002, 488). As should be clear, this doesn’t invalidate narratives as a methodological principle. Narratives – since they have to use good arguments – are certainly not arbitrary and thus unscientific. What has to be done, however, in a very serious way is to demonstrate that the story or plot being offered is better and more convincing than alternative plots and stories. In this context the application of „counterfactuals“ could be helpful. Although I know that counterfactuals are criticized within the social sciences for some good reasons, one should not neglect the usefulness of such constructions since they might help in the process of justifying theoretical stories (Fox Gotham/Staples 1996, 484). „What would have happened if“-questions are useful either to find theoretically ambitious questions or to refute theoretical schemes which are often quite handy for social scientists keen to

bridge long time periods in a story of path dependence. As Richard Ned Lebow has convincingly argued, the value of counterfactuals“ is based on their tendency to qualify deterministic assumptions concerning past causalities (Lebow 2000, 557). Especially if one takes Michel Dobry’s advice seriously, namely to interpret contingent events not exclusively by the end-results of a process, then counterfactuals can help to find out whether the results could have emerged by other events as well, by a different path. Could the end-result of a process not have emerged without the originally discovered contingent event which at first looked like the decisive precondition of the process? It goes without saying that counterfactual narratives have to be used carefully, that they are only useful if developed out of research which tried to look deep into historical details and if they are able to name a mechanism which makes it plausible that the later result at least could have been caused by it. If these two conditions are not fulfilled, then counterfactuals are quite useless, arbitrary and even misleading.

Since the usefulness of counterfactuals is highly contested in the social sciences, I would like to reformulate my argument with the help of late British philosopher Bernard Williams. Williams has answered the question whether fictions might be useful for explanations by pointing to Robert Nozick’s distinction between potential explanations with a defect concerning the causal law on the one side and those with a defect concerning the facts on the other. Williams’s point is that some potential explanations with some factual deficits, but with a correct law, are useful exactly because they show that a certain kind of process at least could have been possible (Williams 2003, 54). Such explanations in which some facts are problematic (because they are, for example, not in the historical record), but with a clear causal mechanism, Williams called „imaginary genealogies“. They help the researcher to find a functional connection and even to formulate a functional explanation with the help of motives the historical actors might have had (Williams 2003: 58). This is not an argument for an all-encompassing explanatory functionalism in the social sciences – as Williams makes clear. It is just a means to think about the relationship between historical facts and history on the one side and theoretical abstractions on the other (Williams 2003: 59f.) in order to immunize oneself against one-dimensional narratives so common within civilizational analysis and research based on the idea of path-dependency. Counterfactuals and imaginary genealogies – to stress it again – can in fact be useful in criticizing teleological assumptions.

4. If – as emphasized by Paul Ricoeur – narratives are the means of coming to terms with human temporality, then it is clear that such temporality must not be neglected within social research. But – as especially Andrew Abbott has forcefully argued – this is exactly the case in much of statistical research in the social sciences since it is supposed that variables do have the same causal effect regardless of social context and timing (Abbott 2001a, 44; Isaac 1997, 8). Sequencing effects are all too rarely taken into consideration by statistical work. But this is certainly not a problem of quantitative research alone. I would argue that

works within historical sociology and civilizational analysis do implicitly agree with the premise of causal constancy since it is assumed that certain patterns of social reality are reproduced by the same causal mechanisms in different social and temporal contexts. The use of Mill's methods of difference and agreement, for example, is the very attempt to isolate context-neutral variables and mechanisms. But it is highly questionable whether such a research strategy really is an adequate method in dealing with the highly contingent temporal ordering of events so typical of social reality. As William H. Sewell has argued: „A fully eventful conception of temporality must also deny the assumption that causal structures are uniform through time. Events must be assumed to be capable of changing not only the balance of causal forces operating but the very logic by which consequences follow from occurrences or circumstances. A fully eventful account of the fate of nobles in the French Revolution, for example, would have to argue that nobles lost power not only because the loss of some of their assets – land, tax privileges, feudal dues, offices – reduced their resources relative to those of other classes but also because the rules of the social and political game were radically redefined, making what had previously been a prime asset – their noble status – into a powerful liability by the time of the Terror [...] Because the causalities that operate in social relations depend at least in part on the contents and relations of cultural categories, events have the power to transform social causality.” (1996, 263)

If one could agree with Sewell's statement, then one has to be extremely careful in trying to generalize mechanisms which one has found in a particular research project, then one should at least be sceptical concerning the hope that such a certain mechanism could be found in completely different contexts. Although I would not deny that some kind of knowledge accumulation is possible within comparative historical research especially if it turns out that some story elements could be convincingly found in various contexts, one has to be aware of the following point: Every theoretical narrative by setting some contingent starting and endpoints of a development commits some kind of a „selection bias“ – to use the terminology of quantitative researchers. Thus, the question will always be whether mechanisms being found can really be generalized (Büthe 2002, 488), especially if it is true, that it is the context which determines the causal validity of events and thus the temporal dynamics of processes. Therefore we should remain sceptical towards daring generalizations and – instead – should be open towards qualifying our own theoretical plots; we should be prepared to see altogether completely different explanatory stories as those we originally have found. To be more specific: I do not think that systematic comparisons between civilizations can really help us in generating theoretical generalizations. The comparative method in the field of civilizational analysis – so my argument would be – does only make sense if it is pursued with the intent of individualizing the cases analysed.

Let me briefly conclude: Whoever acknowledges the validity of the four points just mentioned should be extremely careful in doing civilizational analysis. Having said that, I certainly do not want to suggest that civilizational analysis is a fruitless endeavour. On the contrary: Within macrosociological research of the last 30 years this approach is certainly one of the most promising. However, it should also be recognized that sweeping generalizations are not to be expected and they shouldn't even be tried because otherwise the danger of violating sound methodological principles is simply too high. At the moment my advice for those doing civilizational research would be the following: Since the concept of "civilization" is highly contested a wise methodological strategy still is: (methodological) triangulation: Arguments on particular civilizations, let's say on US-American civilization, should be supplemented and corrected by alternative approaches: Arguments coming out of the fields of Global history, Atlantic history, Hemispheric History etc. are certainly able to undermine the somewhat teleological bias of civilizational analysis.