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Implicit assumptions about the
object of research in social
research methods *)

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IMPLICIT ASSUMPTIONS ABOUT THE OBJECT OF RESEARCH IN SOCIAL RESEARCH METHODS

1. About the terms 'objectivity' and 'object-adequacy'

The need for objectivity is widely accepted. What is the use, for example, of a theory about the psycho-social causes of heart-diseases, if it is not objective, that means in common sense, if we cannot trust its statements because they do not rest on empirical data that have been collected in a methodologically sound way? This common sense interpretation obviously identifies objectivity with truth in the 'old-fashioned' sense; or, in psychological methodology, with at least 'internal validity'.

In the course of time the theory of science developed several models of truth (GROEBEN/WESTMEYER). According to 'correspondence theory' truth is the correspondence between a statement and the facts of reality. According to 'coherence theory' truth is determined by the coherence of the one tested statement with other relevant and accepted statements. According to 'consensus theory' truth is identified by the consensus of competent or 'expert' researchers. All of these models of truth are loaded with problems.

Social science methodologists, following POPPER, combined the consensus theory of truth with the correspondence theory. The existence of a fact, or - more precise - the truth of a simple statement, a so-called 'protocol statement' about a fact is verified, if no competent researcher disagrees about the truth of this protocol statement. This is the conception of objectivity as 'inter-subjectivity'. An example: if all middle-class psychologists agree that a working-class child is maladjusted, this child is objectively (in POPPER's opinion) maladjusted. No need to care any more about researchers' class-biases, like middle-class value system, current life situation, future life perspective, etc, which might all prove to be quite different for the working-class child. The child is maladjusted - and will probably remain so.

While, for POPPER, a fact is objective via the researchers' inter-subjectivity, the truth - or better: non-truth - of a theoretical statement is determined by a confrontation of this statement

with the facts: if facts and theoretical statement agree, nothing is gained; if they disagree, you have falsified the theoretical statement and science flourishes. This paradigm of scientific progress has been severely criticized by various theorists of science, like for example HOLZKAMP, KUHN, FEYER-ABEND, LAKATOS, STEGMÜLLER.

Let's return to objectivity. Objectivity in the old sense referred to both the truth of a protocol statement and the truth of a theoretical statement, that explains this - and other - protocol statements, by connecting them with constructs and certain relations (laws) between these constructs. But if objectivity just refers to protocol statements, much of the old content is lost, and you have to recover it by introducing the concepts of confirmation, verification, falsification, etc, of theoretical statements. Besides, as indicated, the criterion for the objectivity of protocol statements - namely: inter-subjectivity - is full of problems.

In the theory of measurement, objectivity is used only in this reduced sense, as inter-subjective agreement about the result of a measurement. But the theory of measurement provides other concepts - namely: reliability and validity - which are more apt to recover the old sense of truth. I refer here especially to internal and external validity (CAMPBELL/STANLEY; GADENNE), that means, the validity not of measurements, but of research results themselves. - Internal validity is the correspondence between the researcher's causal model and the real causation of his observed results. This obviously is a kind of truth of a complicated theoretical statement. The common criterion for this internal validity is replicability of the empirical result - which, of course, is a weak criterion, because even with a replicable result, you do not know for sure, how it came about. So replicability is combined with control as a necessary prerequisite for internal validity; control means, that those and only those causal variables influenced the result (in a differential manner for the experimental and the control group) that are included in the causal model. Successful control, of course, would strengthen your opinion, that your result is internally valid - but, as many researchers agree, control cannot be achieved, it is 'just a myth' (FRIEDMAN). - External validity, on the other hand, does not tell you, whether your

empirical result is correctly analyzed, but whether you can use it to explain anything in different situations. For example: you may have an internally valid result, that in a strictly controlled situation the probability of a heart-infarction is only dependent on the amount of smoking; but still it is true, that in normal, non-controlled situations the influence of smoking upon heart-infarction is weak, compared to the combined impact of other stressors. So you can have internally valid results, which cannot be generalized, and are therefore without any practical relevance. So, external validity indicates some kind of a 'practical truth', a truth that is measured by generalizability and applicability. - Internal and external validity are - as CAMPBELL/STANLEY showed - in a given situation inversely related to each other: either you strengthen control, at the expense of generalizability and applicability - or you strengthen representativeness, at the expense of theoretical rigour. This relation between internal and external validity is also the main demarcation line between 'tough-minded' and 'tender-minded' methodologists.

By referring to generalizability and applicability of a result, you become aware of another problem. Your result may not tell you something about the object, but only about the method, which you study it with. For example: if you want to study cognitive processes in a baby, and therefore interview the baby, the fact that the baby does not answer the questions cannot be interpreted as an indication, that the baby does not have cognitive processes, but as an indication, that your method to comprehend such cognitive processes is inadequate. So your result, that the baby does not have cognitive processes, might be a 'methodical artifact'. This raises the problem of the object-adequacy of methods. Reflecting on this you assume, that the object has certain characteristics, which cannot be comprehended, portrayed by all methods, because methods themselves have characteristics that interact with the object. So methods can adequately portray an object - or they can distort it. This is sometimes a very obvious matter: you know that it is useless to test a Hopi-Indian, observe a murder, or do orthodox experimentation about large-scale social problems, like the increase in heart-diseases.

But in other instances things are not so obvious, and the development of methods that are adequate to the object of research can become a difficult problem. For example: does a traditional experiment allow the subject to use the full scope of his insight, involvement, and activity? An interviewer presupposes, that the isolated respondent is characterized by thoroughly reflected, mutually consistent, and lasting attitudes, ready for recall - is it like that? The observer tries to be as invisible as possible - is that necessary?

The scientific reflection on object-adequacy gathers all the available kinds of knowledge about the object of research - not only the 'hard facts' of the 'tough-minded' scientist, but also (following CRONBACH, CAMPBELL, FEYERABEND, DICK, LEITHÄUSER, and many others) all the obvious or vague results, hints, and cues that science and every-day knowledge can carry together. In this way you can construct a model of the object, that neither presupposes in this object any 'simple' or 'linear' structure, nor independence of 'time and space' - which means independence of evolution/history and situation.

Instead you ought to construct - as do for example LEONTJEW and HOLZKAMP - a model of the object, according to which this object (here: the human being) is characterized by complexity, individuality, insight, activity, self-determination, and development (IWANOWA/ASSEJEW). If you compare this model of the object with the traditional models, you raise the question of object-adequacy.

As we will see, the assumptions of these various models are expressed in the nature and structure itself of your research methods.

2. About the methodological discussion in empirical social research

Methods are neutral 'unbiased' instruments for the generating and testing of theories. Methods themselves do not predetermine any result, but leave the object unchanged in its quality, and only express and validate objective laws, which exist independent of researcher and research. - This is the naive methodological conception of social research, the way it is practiced.

The traditional theory of methods (methodology) is much more sophisticated.- With reference to POPPER it points to 'background theories' which, via construction, are implemented in the methods, and predetermine to some extent the result or at least the potential scope of results. An example: in an interview setting it is reasonable to assume, that the frankness of responses is raised, if the social climate in the interview is made friendly - but not too friendly!- The problem here is that research results are not only influenced by the nature of the research object, but also by the nature of the research method. At this point methodologists have argued, that since the constructor and user of methods has many 'degrees of freedom' in his decision, the solution lies in reducing this freedom in favour of some sort of professional 'precept of method' ('Kunstlehre'; SCHEUCH). This 'precept of method' is rather intuitively founded - on a common sense 'model of the human being' ('Menschenbild') -, but is supposed to improve the results toward an adequacy to theories and objects; criteria for this adequacy are not given.

Other methodologists (KÖNIG, SCHEUCH) show a lot of methodical determinants of social-research results, which are empirically tested. This empirical research on methods - or in the worst case: empirical research on artifacts (MERTENS) - demonstrated a clear dependency of results from methods, and developed correspondingly methodological 'error-theories'. But until now this research could not state any criteria for the adequacy of methods, either.

In an already existing 'modern' discussion about methods it is argued, that some implications of methods and methodology block the approach to certain areas of knowledge. It is argued, for example, that in the experiment, the subject is hindered to develop insight, self-determination, and activity: HOLZKAMP (1972) calls this an 'organismic anthropology'. It is shown by the 'social psychology of the experiment' (ROSENTHAL, ROSNOW, ARGYRIS, JOURARD, MERTENS, and many others) that the experimental result does not reflect so much 'objective laws', but the hypotheses of experimenter and subject about what these 'objective laws' should be. As to speak 'social psychology of the interview' (BERGER) demonstrates the dependency of interview results on the social situation, the person of the interviewer, his power, etc. The criticism of the 'conditionalistic methodology' (DICK, MASCHESKY) tries to show, that traditional methodology disregards the 'systemic aspect' of social phenomena, which means their interrelatedness, hierarchic, causal, and genetic structures, the clustering in specific configurations, etc. The criticism of the 'anti-subjectivism' of traditional methodology (HOLZKAMP (1977b), FIEDLER/HÖRMANN, SCHNEIDER) refers especially to its neglect of the capacities of the human being, to analyze and change reality, and in this process to develop himself.

Accordingly these methodologies severely doubt - partially or totally - the epistemological value of the traditional social-empirical methods, that means: observation, interview, experiment, test. Instead they turn to alternative techniques and strategies: from 'participant observation' (FRIEDRICHS/LÜDTKE), 'group discussion' (MANGOLD), 'qualitative interview', and 'quasi-experiment' (CAMPBELL/STANLEY); to 'contradiction-experiment' ('Widerspruchsexperiment'; HOLZKAMP, 1973), 'crisis-experiment' (GARFINKEL), 'ethological experiment' (HOLZKAMP, 1973), data-gathering methods in therapy (HARTIG), like 'role-play' and 'narrative interview' (LEITHÄUSER), and the technique of 'industrial reports' (WALLRAFF); finally to 'case-study' (PETERMANN), 'process-research' (CRONBACH, 1972), 'evaluation-research' (WOLLMANN/HELLSTERN), and 'action-research' (FIEDLER/HÖRMANN, SCHNEIDER).

The epistemological value of these alternative (and partially 'tender-minded') techniques and strategies is itself still in doubt. But reasons for dealing with them are found in

- firstly the failure of the traditional methods to fulfill their own criteria, especially control, replicability, and generalizability
- secondly in their neglect of a number of evident characteristics of the research object of psychology: like subjectivity, development, and complexity
- finally the interest in alternative methods is justified by the higher profundity, relevance, and applicability of their results.

3. About implicit object-assumptions in the experiment

HOLZKAMP (1972) explicated several implicit object-assumptions in the experiment; he called them the 'hidden organismic anthropology' of the experiment.

HOLZKAMP emphasizes the conception of the 'normal subject' ('Norm-Vp.'). This normal subject can be characterized as follows:

- (1) he equals the experimenter in normal life; but in the experiment he puts himself - or allows himself to be put - in a subordinate position
- (2) he promises to follow strictly the experimenter's orders
- (3) he promises to remain 'naive', that means: to react 'naturally' and not to think about the experiment in a way, the experimenter does not want
- (4) he promises to react only in those dimensions and in those categories, which the experimenter opens up for him.

In the experimental situation this idealized normal subject voluntarily renounces his capacity for reflection and active coping with a given situation: - an ideal example of 'alienated behavior'.

This 'organismic anthropology' is implemented by putting the subject in a laboratory and in a given experimental setting, by reducing his information or even intentionally deceiving him (according to DICK the experimenter regards the subjectivity of his subject as a nuisance-variable, which should be blocked out!), by the instruction, by the experimenter's behavior, and finally by the techniques of 'reduction' ('Reduzierung'), 'parcelling-out' ('Parzellierung'), and 'labilization' ('Labilisierung').

'Reduction' means that the experimenter tries to neutralize (by standardization and control) many components of the complex causal net, which determine the ongoing process - but, as FRIEDMAN and many others say: "control is a myth!" - 'Parcelling-out' means that the experimenter dissects and splits up his original variables, in order to get at the 'real cause'. - 'Labilization' means that the experimenter shields off those determinants, which

strongly determine - and stabilize - the ongoing process, but in which he is not interested. As a consequence the subject will react even to the meekest influence - which then, as the experimenter will demonstrate, has a 'significant effect'.

While HOLZKAMP doubts the object-adequacy of experimental research principally, the empirical research known as the 'social psychology of the experiment' (MERTENS) shows, that the experimental subject cannot be pressed in the scheme of a 'normal subject' or a 'reaction-automaton'.

The psychic processes and structures, which determine the experimental behavior, are differentiated in:

- (1) the perception of implicit 'demand characteristics' (or 'role cues', 'performance cues') in instruction, setting, experimenter behavior, etc, which signalize the subject, 'what should come out'
- (2) corresponding interpretations and hypotheses of the subject
- (3) generalized or specific attitudes of the subject, which determine, whether he will give in to the experimenter's expectations. Here one can differentiate different role-conceptions by the subject: 'good subject', 'apprehensive subject', 'faithful subject', 'negativistic subject'.

An example: attitude-changes are ambivalent under the evaluative aspect. Fast attitude-changes can be interpreted as flexibility, teachability, intelligence, but also as lability; slow attitude-changes can be interpreted as behavior-consistency, stability, but also as rigidity. The experimenter cannot exclude such evaluations - even if he wants to. It can be shown, that research results about the speed of attitude-changes can be made to vary with the manipulation of the experimental subjects' interpretations, what such attitude-changes indicate about the subjects' personality (MERTENS).

Result. - The experiment tries to put the subject in an unnatural state, which makes impossible reflection and active coping (in directions, the experimenter is not interested in). But the subject can and does in an uncontrolled way resist and sabotage the experimenter's intentions, and brings his subjectivity and activity into play.

4. About implicit object-assumptions in observation

Traditional social-empirical methodology did not develop a theory of observation. This was done instead by ethnological methodology, which, combined with the theory of communication - especially: symbolic interactionism - led to 'ethnomethodology' (CICOUREL, ARBEITSGRUPPE BIELEFELDER SOZIOLOGEN, LEIT-HÄUSER). And, with regard to the contents of observation, another theory of observation was developed by the 'psychology of interpersonal perception' (LAUCKEN), which deals with 'personal constructs', 'implicit personality-theories', etc.

Looked at from traditional social research, observation can have mainly two functions:

- (1) identifying the 'real', the 'factual' behavior. For example: what do people really do at home, after work? How is a certain work task really done?
- (2) making accessible interior psychic states, processes, and dispositions. For example: what does a certain work task demand of the working individual? How does it come about, that a certain person always draws aggressions on himself?

If observation is meant to identify the 'real' behavior, the assumption is implied, that the awareness of being observed changes behavior; more precisely: the observed person feels socially controlled, and behaves 'unnaturally', 'unnormally'. Obviously then there must exist a 'natural', 'normal' behavior, which is disturbed if I feel observed. Thus the process of observing becomes a nuisance-variable.

But in my opinion, the implied concept of normality is falsely used. - On the one hand: if social control disturbs behavior, you should expect, that the behavior of socially isolated individuals is completely normal. But: social isolation does not exclude social control, since that can be exerted by objects, too. And, more important, the socially isolated individual is in a totally unnatural situation, since it is not social control, but the lack of it that is unnatural (in the statistical as well as in the social-psychological sense!) Here you recognize an individualistic and atomistic view of social phenomena. - On the other hand, talking about 'normal' behavior implies,

that you regard certain determinants of behavior as adequate, others as inadequate. But if you take seriously the concept of socialisation, any behavior and any underlying psychic processes can be modelled, at least in the long run. This is limited only by constitutional premises and fixations of socialisation-results. So talking about 'normal' behavior neglects all the teachings of 'interactionism' and 'situationism', and instead regards the individual as sort of a 'constancy-phenomenon'. 'Normality' of behavior is more a normative, than a descriptive term.

If observation is meant to make accessible interior psychic states, processes, and dispositions, you run into the following problems:

- (1) you have to formulate assumptions about the inner processes and structures
- (2) you have to evaluate, whether there is a correspondence between behavior and such inner phenomena
- (3) you have to evaluate, whether you can draw unequivocal conclusions from behavior to inner phenomena
- (4) you have to regard methodologically, whether participation or non-participation, maximum, medium, or minimum distance is optimal, whether 'Verstehen' is necessary, etc.

More concretely, you have, for example, to formulate assumptions about:

- whether the observed behavior is determined by 'personality' or by 'situation'
- whether the 'personality' is exerting its influence through 'states' or through 'traits'
- how stable the observed behavior is
- whether you can generalize the observed behavior
- what can be concluded from the observed behavior about 'personality'
- how you can identify homogenous behavioral acts
- on which level of differentiation you analyse behavior
- whether you need to interpret behavior, and if so, from which distance you gain an optimal understanding.

Here behavioral analyses can differ widely, as, for example, the 'psychology of interpersonal perception' shows. No common

set of assumptions exists, though, as I tried to show, nevertheless many assumptions are made, usually more implicitly than explicitly. It is my own conviction, that these assumptions should always be made as explicit as possible.

Result. - In observation it is assumed, that unobserved behavior is more 'natural', more 'normal' than behavior, when the observed person is aware of being observed. Thus for unobserved behavior there is falsely implied, that it is free of social control. Besides, this view paradoxically neglects the situational determination of behavior, which is only regarded as a disturbance. The attribution of behavior to inner processes and structures is based on a variety of theoretical and methodical assumptions, that differ according to problem, focus, theoretical viewpoint, etc. The 'common-sense theory of behavior' or 'psychology of interpersonal perception' demonstrates this variety.

5. About implicit object-assumptions in the interview

BERGER gives an instructive example: JAIDE made a study of young female workers in Western Germany. He was interested in political attitudes, historical knowledge, life perspective, etc. I quote BERGER (in my translation): "this study ... finds, that the respondents are responding in an exceptionally obstinate and vague manner. Half of them does not answer the question: 'what do you think about the constitutional form here in the federal republic?' ... A scale for measuring liberal vs. conservative attitudes, consisting of very global questions, was mostly not answered ... The undecided and unclear response-style would suggest, to regard the methodical approach of standardized interview and the kind of questions as inadequate ... (But JAIDE; W.M.) concludes from the answers, that these female workers generally show a lack of intelligence, a lack of intellectual contact, a lack of interest, altogether 'subnormality'". (BERGER, p.92/93) This example demonstrates, I think, not so much lack of intelligence in workers, but a lack of methodological reflection in researchers.

BERGER shows, that the research-interview has a lot of deficiencies:

- (1) the specific social relation in the interview (between interviewer and respondent) influences the answers. Here all the results of the 'social-psychology of the experiment', the 'error-theories' of the interview ('response-style', 'social desirability', 'halo-effect', etc) are applicable. Nobody dares to take the view, that these influences can be neglected; nobody knows, how they - singly and in interrelation - can be safely neutralized.
So, either you have to admit, that there is no question, that guarantees an objective answer - or you have to give up the idea, that there is something like a constant 'real' attitude, experience, etc, and have to retreat again to 'interactionism'
- (2) the isolation of the respondent in the interview presupposes, that there exists something like attitudes, which are ready for recall any time. But empirical evidence points rather

to the opposite: attitudes are formed in interaction, and vary with interaction - that is why the respondent so often is eager to find out and learn, what the interviewer thinks about the 'whole thing'. But the method, by construction, forces an individual attitude upon the respondent. If he resists, he is regarded as just obstinate and stupid

- (3) the social situation in the interview is asymmetric. The interviewer has all the authority of science, his social class, etc, as a background. Often he is potentially a danger for the respondent, as, for example, a social scientist, who studies job-satisfaction and work-motivation in a firm, that might dismiss workers. A 'power-free dialogue' ('herrschaftsfreier Dialog; HABERMAS) is thus not possible; the respondent, who is revealing his 'true' opinion, is either fool-hardy or a potential suicider.

This problem is, of course, noticed by the 'error-theory': the interviewer promises anonymity, he 'seduces' the respondent to critical statements, etc. But again there are no criteria to evaluate the response

- (4) the style of language of the interview strongly influences the responses. Terms are not understood; terms that are used, do not express the connotations, the respondent means; interpretation and connotation of terms vary between groups; etc. Language obviously is not a 'value-free' ('wertfrei') and not a 'theory-free' ('theoriefrei') instrument to comprehend meanings.

Result. - The interview assumes stable attitudes in the respondent, which are independent of the situation, and remain unchanged - though their expression can be distorted by language, social situation, **the** interviewer, etc. Empirical evidence does not confirm this view, but instead indicates, that attitudes fluctuate with the situation and are primarily formed in interaction.

6. About some methodological consequences

I hope that I could show, that the traditional social-empirical methods imply certain models of their object (here: the human being), which, by application, they try to enforce upon that object. This enforcement is realized

- (1) by shaping the research-situation in a certain manner, which is meant to prohibit the individual to bring to bear its insight, self-determination, and activity
- (2) by concentrating upon only a few chosen and fixed behavioral dimensions, thus neglecting the complexity of behavior
- (3) by disregarding 'unsuitable' behavioral responses, thus negating the individuality of behavior
- (4) by cutting away all the remaining bulk of information, that does not fit into explanatory schemes of a 'nomothetic', 'simple-structured', preferably 'linear' kind, and which operate primarily with constant phenomena.

An example: laboratory-research on the effects of work-stress puts the subject into a prefixed situation, like subtracting numbers under various noise-conditions, and then looks at output-rates, psychophysical indicators, and the marks on a multiple-choice list about 'how I felt during the experiment'. But: the subject is forbidden to modify the whole setting - like changing roles with the experimenter; subjects, who make fun of the experiment, and the like, are at least excluded from interpretation; effective coping-strategies, like switching off the noise, are just 'unheard of'; the decision of the subject, never again to participate in that sort of research, does not interest the experimenter; and afterwards the researcher is drawing regression-lines and is carefully labeling the subjects according to 'stress-tolerance'.

In my opinion - and not only mine; see CAMPBELL, CRONBACH, LEVI, LAZARUS, and many others - this sort of research is not very effective, to say the least. In the worst case it distorts the picture of the object, and produces artifacts, like: the 'inferior intelligence' of maladjusted persons, women's 'appreciation' of monotonous work; etc. But - perversely enough - from this point of view the 'Hawthorne-effect' (ROETHLISBERGER/DICKSON), the 'Pygmalion-effect' (ROSENTHAL), 'suicidal' and 'self-fulfilling prophecies' are regarded as artifacts.

well, to criticize traditional social research is easy enough - but: is there anything better to offer? What is the alternative? As I indicated, there exists an enormous variety of alternative techniques - from group-discussion, quasi-experiment, participant observation, etc, to crisis-experiment, role-play, reportage-techniques -, which can be combined, under inclusion of traditional techniques, to research-strategies like case-studies, field-research, evaluation-research, action-research, etc. This is eclectic, of course, a methodical mix, or even 'methodological opportunism' - all true!

But nevertheless, these new methods have a lot of advantages:

- (1) they do not emphasize so much standardization, control, and internal validity, but emphasize instead flexibility, adequacy, and external validity
- (2) they do not repress obvious capacities of the human subject, like: communication, reflection, self-determination, and development
- (3) they are apt to comprehend and analyse processes themselves, not only their results
- (4) they are apt to comprehend complexity and inconsistency - as a motor of development -, and do not need to enforce a simple structure
- (5) they do not sacrifice qualitative abundancy to quantitative precision, which many methodologists' premature obsession with statistics leads into
- (6) they have a higher practical applicability and thus relevance, because they operate nearer to those social phenomena, which research claims to analyse.

An example: you want to analyse, why a certain worker got a heart-infarction. If you just look at his work, you may observe time-pressure, work-overload, role-ambiguity, conflicting demands from superiors and colleagues, the danger, that his job or at least his qualification fall victim to technological progress, etc. But as his colleagues did not get an infarction that cannot be the whole answer. You test him, to find out about his physiological and psychological make-up. But here, too, he does not differ so much from his colleagues; and anyway, incidentally, he tells you, that in the past he could better tolerate stress,

was not so nervous, smoked less, and was not so anxious about his job. So you interview him about the way, he perceived his job. But he says, that the job was okay, the superiors and colleagues were okay, he did not have shift work, and anyway a job has to be done, one should not complain too much. Now you notice a difference: his colleagues do complain about their job, the firm, the superiors, the colleagues, etc. So, trained in 'tough-minded' methodology, you conclude: the less stressed the worker is, the higher the probability of a heart disease; or vice versa: the more stress he has, the healthier the worker is. - Here the 'tender-minded' methodologist intervenes: could it not be, that the typical heart-patient is just repressing conflicts, that he is sort of an 'emotional illiterate', and the like - all guesses, which cannot be tested by the interview! So he suggests a qualitative interview (not only with the patient, but also with his wife), role-play, group-discussion, tests of the inclination to suppress hostility and conflict, a stress-interview, (in which the interviewer always contradicts the respondent), a biographical account of the changes in life-perspective and behavior, etc. As a result he finds, that the heart-patient obviously has a different way to cope with stress: he sees no possibility to reduce stress, therefore internalizes the demands from outside, tries to fulfill them by all means (ROSENMAN/FRIEDMAN), represses his aversion (THEORELL), and consequently does not attribute his difficulties to his job, but to his own incompetence - which his pride forbids him to admit. Thus you get a different picture of the development of a heart-infarction - which, in my opinion, is probably much more adequate and instructive.

Conclusion.- I tried to show, on the one hand, that there exists a great variety of 'tender-minded' or alternative techniques and strategies for research, that parallel their 'tough-minded' counterparts (see diagram on p.18!); and that, on the other hand, these alternative techniques and strategies are, in my opinion, in many instances more adequate to the object of research. But this does not imply, that these alternative methods are generally superior. Their disadvantage is the lack of precision, the lack of methodical standards - which might change in the near future -, and the danger, that their results are strongly influenced by the subjectivity of the researcher - even stronger than in the 'tough-minded' methods. So I think, any methodological narrow-mindedness is at the moment unjustified and ineffective.

Diagram

'tough-minded' methodologies

'tender-minded' methodologies

experiment

field-experiment
quasi-experiment
crisis-experiment
ethological experiment
contradiction-experiment

standardized interview

qualitative interview
stress-interview
group-discussion
narrative interview
reportage-technique

standardized, non-participant
observation

participant observation
open observation
free observation
role-play

n = 1-studies

case-studies

laboratory-research

field-research
action-research

input-output-analysis
product-research

process-research

fundamental research

applied research
evaluation-research

Literature

- ARBEITSGRUPPE BIELEFELDER : Alltagswissen, Interaktion und ge-
SOZIOLOGEN gesellschaftliche Wirklichkeit/Ham-
burg, 1973
- ARGYRIS, C. : Some unintended consequences of
rigorous research/Psych. Bull.,
1968, 70
- BERGER, H. : Untersuchungsmethode und soziale
Wirklichkeit/Frankfurt/M., 1974
- CAMPBELL, D. : Qualitative knowing in action re-
search/Kurt Lewin Award Adress,
1974
- CAMPBELL, D./ : Experimental and quasi-experimen-
STANLEY, J. tal designs for research/Chicago,
1966
- CICOUREL, A. : Methode und Messung in der Sozio-
logie/Frankfurt/M., 1974
- CRONBACH, L. : Evaluation zur Verbesserung von
Curricula/in C.Wulf (Ed.): Eva-
luation/München, 1972
- DICK, F. : Kritik der bürgerlichen Sozial-
wissenschaften/Heidelberg, 1974
- FEYERABEND, P. : Wider den Methodenzwang/Frank-
furt/M., 1976
- FIEDLER, P./ : Aktionsforschung in Psychologie
HÖRMANN, G. und Pädagogik/Darmstadt, 1978
- FRIEMAN, M./ : Der A-Typ und der B-Typ/Hamburg,
ROSENMAN, R. 1975
- FRIEDMAN, N. : The social nature of psychological
research/New York, 1967
- FRIEDRICHS, J./ : Teilnehmende Beobachtung/Wein-
LÜDTKE, H. heim, 1973
- GADENNE, V. : Die Gültigkeit psychologischer
Untersuchungen/Stuttgart, 1976
- GARFINKEL, H. : Common sense knowledge of social
structures/Vortrag auf dem 4. Welt-
kongreß für Soziologie, Mailand,
1959
- GROEBEN, N./ : Kriterien psychologischer Forsch-
WESTMEYER, H. ung/München, 1975

- HABERMAS, J. : Zur Logik der Sozialwissenschaften/Frankfurt/M., 1970
- HARTIG, M. : Probleme und Methoden der Psychotherapieforschung/München, 1975
- HOLZKAMP, K. : Kritische Psychologie/Hamburg, 1972
- HOLZKAMP, K. : Sinnliche Erkenntnis/Frankfurt/M., 1973
- HOLZKAMP, K. : Kann es im Rahmen der marxistischen Theorie eine Kritische Psychologie geben?/in K.Braun/K.Holz-kamp (Eds.): Kritische Psychologie. Bericht über den 1. Internationalen Kongreß. Band 1/Köln, 1977a
- HOLZKAMP, K. : Die Überwindbarkeit der wissenschaftlichen Beliebigkeit psychologischer Grundbegriffe durch die Kritische Psychologie/Z.f.Sozialpsychol., 1977
- IWANOWA, I./ASSEJEW, K. : Methodologie und Methoden der psychologischen Forschung/in W.Schorochowa (Ed.): Methodologische und theoretische Probleme der Psychologie/Berlin (GDR), 1974
- JOURARD, S. : Experimenter-subject-dialogue: a paradigm for a humanistic science of psychology/in B.Bugental (Ed.): Challenges of humanistic psychology/New York, 1967
- KÖNIG, R. : Die Beobachtung/in 'Handbuch der empirischen Sozialforschung', Band 2/Stuttgart, 1967
- KUHN, T. : Die Struktur wissenschaftlicher Revolutionen/Frankfurt/M., 1973
- LAKATOS, I. : Falsifikation und die Methodologie wissenschaftlicher Forschungsprogramme/in I.Lakatos/M.Musgrave (Eds.): Kritik und Erkenntnisfortschritt/Braunschweig, 1974

- LAUCKEN, U. : Naive Verhaltenstheorie/Stuttgart, 1973
- LAZARUS, R./LANNIER, R. : Stress-related transactions between person and environment/in L.Pervin/M.Lewis (Eds.): Perspectives in interactional psychology/New York, 1978 (in print)
- LEITHÄUSER, T., et al. : Entwurf zu einer Empirie des Alltagsbewußtseins/Frankfurt/M., 1977
- LEONTJEW, H. : Probleme der Entwicklung des Psychischen/Berlin (GDR), 1971
- LEVI, L. : Emotional stress/Basel/New York, 1967
- MANGOLD, W. : Gruppendiskussionen/in 'Handbuch der empirischen Sozialforschung', Band 2/Stuttgart, 1967
- MASCHEWSKY, W. : Das Experiment in der Psychologie/Frankfurt/M., 1977
- MASCHEWSKY, W. : Methodologische Überlegungen zur Bedingungskontrolle/Bielefelder Arbeiten zur Sozialpsychologie, 42, 1978
- MERTENS, W. : Sozialpsychologie des Experiments/Hamburg, 1975
- PETERMANN, F. : Methodische Grundlagen klinischer Psychologie/Weinheim, 1977
- POPPER, K. : Logik der Forschung/Tübingen, 1971
- ROETHLISBERGER, F./DICKSON, M. : Management and the worker/Cambridge (Mass.), 1939
- ROSENTHAL, R. : Experimenter effects in behavioral research/New York, 1966
- ROSNOW, R., et al. : Volunteer effects on experimental outcomes/Educ. and Psychol. Measurement, 29, 1969
- SCHEUCH, E. : Das Interview in der Sozialforschung/in 'Handbuch der empirischen Sozialforschung', Band 2/Stuttgart, 1967
- SCHNEIDER, U. : Handlungsforschung - eine Alternative?/voraussichtlich 1979

STEGMÜLLER, W.

: Probleme und Resultate der Wissenschaftstheorie und Analytischen Philosophie/West-Berlin, 1974

THEORELL, T.

: Life-events before and after the onset of premature infarction/in B.Dohrenwend/P.Dohrenwend (Eds.): Stressful life-events/New York, 1974

WALLRAFF, G.

: Industriereportagen. Als Arbeiter in deutschen Großbetrieben/Hamburg, 1970

WOLLMANN, H./

HELLSTERN, G.

: Sanierungsmaßnahmen. Städtebauliche und stadtstrukturelle Wirkungen/Schriftenreihe 'Stadtentwicklung' des Bundesministeriums für Raumordnung, Bauwesen und Städtebau/Bonn, 1978.

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