Theoretische beschouwingen

Derek L. Phillips’ research to end the research

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Dr. Phillips is, without doubts, a gifted medical sociologist whose research in the field of social psychiatry received due attention. Having encountered a severe methodological problem in the assessment of the prevalence of mental illness by means of survey-methods utilizing various inventories of symptoms, Dr. Phillips turned to the methodology as his proper field of interest. His Knowledge from What? (Rand McNally and Co., Chicago, 1971) examines the process of data-collection in sociology in the light of the author’s embarrassing experience; his recent article in this review goes even further by questioning the possibility of sociology as an objective science. It was this utmost scepticism of the author that made me accept the invitation of the editors of Mens en Maatschappij to write a comment on Phillips’ thought provoking paper.

Before entering into the argument, let me emphasize the points of agreement and of appreciation. With Phillips, I share the view that, at present, sociological methodology progresses along the line of data-analysis and evaluation rather than that of data-collection and validation. Thus I highly appreciate his work as a valuable contribution to the neglected field of the methodology of our discipline. Both in summarizing the relevant literature on ethnomethodology, on sociology of everyday-life, on interpersonal communication, and in his own research on the desirability-set, Phillips adds to our knowledge of the process of data-collection. I also share his criticism of those candid research-workers who try to use

personal interviews to collect data about non-verbal behaviour without paying attention to various forms of bias. In fact, any criticism and any scepticism is welcome to me if it is empirically founded and/or is based on sound inference. And, of course, there is no use for scepticism if it does not mobilize all our inner resources to overcome it. It is on these two points that I may differ from Phillips, to some extent.

His scepticism is of a more recent time. While in his Knowledge from What? Phillips earnestly deals with the ways of how to improve the methodology of sociology, in his recent paper he seems to have abandoned any hope for a scientific sociology, whatsoever. Yet, his book also abounds in many sceptical statements, while the Introduction forms a real „document humain“ of a modern sociologist, in its subjective, touchingly human and personal aspect perhaps only equalled by professor Gunnar Boalt in his The Sociology of Research (South Illinois University Press, 1969). Of course, introductions usually are written when the books are finished, though the reader starts with them, as a rule.

Phillips first confesses „an early sin“ of proudly exposing the resemblance of sociology to the exact sciences. His crisis is of a comparatively recent date: „About three years ago, I began to ask myself the question: does my work really contribute anything to sociological knowledge?“ . . . „For, as I came to see it, my published work in no way „added up“ with the results of other investigators to become part of the accumulation of sociological knowledge — correcting, extending, and refining what was previously known. This was especially distressing to me in that I had long ago acknowledged the failure of sociology to have any relevance for practical, real world concerns. Thus, I now came to believe that sociology not only fails to affect the world around us, but that, furthermore, sociology in no way represents some codified, cumulative body of knowledge“ (Knowledge from What, XIII-XIV). „Having reached this conclusion, I began writing this book. In so doing, my initial concern was with improving the quality of sociological data — especially those data obtained through survey interviews. Or, as my colleague Alan Blum somewhat harshly put it to me, my interest was in finding ways to „repair the data“ “ (Ibid. XVIII). Even this positive attitude is abandoned: „For, at the present time, it seems to me, that by paying closer attention to the methodological and theoretical issues involved in the actual carrying out of sociological research we may begin to widen the gap between what sociologists know and what everybody else knows“ (ibid.). This was the author’s conclusion in about December, 1970. A year later, in the above mentioned paper in this review, his final standpoint
Thus the answer — if there is an answer — lies with new theories and new conceptual schemes, with radical conceptual breaks or discontinuities. But in reaching this conclusion I have reformulated both the notion of „data” that I have used throughout this paper, and the idea of „theory” as it is usually thought of in sociology. With regard to data, I do not mean that we must look more closely at only (or necessarily) the kinds of data that are purposively collected in empirical investigations employing interviews and questionnaires, or other research „techniques”. Rather, I mean that we must try to look at the world through our own eyes and not through our „scientific” instruments that frequently cloud our vision or blind us entirely. By theory I do not mean the activities engaged in by writers like Parsons, Merton, Homans or Zetterberg, but, rather, the primordial conception of theorizing that originated with the pre-Socratics: the idea of theorizing as a self-conscious reflexive activity”.

This is a new note; in addition to the refrain „back to the thinking of the man-in-the-street” there comes a new devise: „back to the pre-Socratics”. In other words: let us forget everything mankind has found out in the course of two and a half millennia of its cultural history and start freshly anew (1). Little could be said against this standpoint if it only expressed a reaction to frustrations of a personal scientific career. Phillips, however, considers it as his task to „awaken” his colleagues and wishes to generalize his standpoint as „a king’s way” of the future sociologists. Those who do not follow this way are „the kingpins of our discipline, those whom Nicolaus terms „intellectual valets” and secular priests” (Knowledge from What, p. XX). He also speaks with disdain of sociologists who shy speculation or argue „that their assumptions are supported by empirical evidence”. His judgment is no less negative: „In my view, this is hogwash. Little of what we know about the nature of man and society is based on empirical evidence” (Ibid. 53)(2). Where so much is demanded of the reader as to abandon the sociology as an independent science or to skip over the entire cultural heritage and to return to the pre-Socratics, it may be worthwhile to briefly examine the arguments on which this advice is based.

There is, to begin with, the social desirability set. As mentioned above, it is very laudable that Phillips assessed its influence upon the results of mental health inventories and that he tries to draw the attention of the research-workers to this important source of bias. Yet it is strange that he fails to delineate the limits of the bias or to suggest the techniques for its efficient control.

Though he is right in pointing out that social desirability is especially a
nuisance when we deal with the problems of validity of verbal behaviour as sought along the line of the non-verbal behaviour as a criterium, he fails to mention that a good deal of human conduct consists of verbal communication, while a conduct devoid of its verbal elements of meaning is rather an exception or an abstraction. Attitudes, norms, and values prevalently are expressed in verbal behaviour only; why not view an interview situation as an experimental setting in which standardized stimuli are administered to the subjects?

As a matter of fact this is exactly the rule our students of sociology are taught in their courses: never take the response at its face-value but only just as a response to an artificial stimulus. Thus we seldom consider the respondents as sources of knowledge about objects of investigation; the respondents and their conduct are object of study, as such. Having taken the above-mentioned rule as the obvious starting-point of any scientifically conducted survey, a question may be asked: what techniques, if any, are there to reduce, control or eliminate the social desirability bias? Phillips suggests at least one: an inclusion of desirability-items into any interview-schedule. We should be thankful to him, for this useful advice. On the other hand, there is, in our opinion, little need for despair and for the conclusion that social research is ultimately impossible, since the social desirability set accounts for a larger proportion of variance than the independent variables used. In our studies we add the possible sources of bias (e.g. the agreement set, the evasive response set) to the list of explanatory variables and control their influence by computing corresponding partials or by means of the analysis of variance. Why not mention this possibility to the reader and why preach the impossibility of scientific sociology, instead?

Another obvious device is to use schedules and questionnaires that are not based on open questions or single items only, but make use of the ever improving instrument of scaling. Various scale items are then mixed up so that it is virtually impossible for an interviewee to „see through“ the instrument. Especially when the survey is carefully planned and covers more fields of interest, the danger of desirability bias can be reduced, to a large extent. In „cheap“, commercial or public opinion polls, where the intention of the interviewer is easily identifiable, the impact of desirability may be much greater.

One may again ask: is it admissible and/or possible to conceal the real intention of the interview? What kind of social encounter is the interview then going to be? I am not blind to the virtual limitations of the interview technique due either to the psychological factors or to the „lack of common
language”. (Years ago I wrote an article on the fact that most interviews as carried out in individual households are, in fact, group-interviews; the family-circumstances play an important role (3)). Our experience, however, has taught me that much of the real-life situation of the interview can be saved by a carefully planned and tried-out design: alternate use of open and structured questions, inviting conversation in certain phases, revealing the real intent of the interview or the standpoint of the interviewer at the end of the process, etc.

Though containing a new source of bias (the invisible or present spouse as influencing factor) these „home-interviews” open, on the other hand, a large opportunity for observation. Phillips seems to overlook the fact that surveys, from their very beginnings, were based on observation together with questioning: prisons, slums — all these were measured, described. In our survey, interviewers put down the whole setting of the interview, register the interaction between spouses, pay attention to the furniture, clothing, physical aspects of persons, etc. It is just the combination of visual and auditive perception, that makes of survey-interview a potent instrument. In addition, one also can resort to paper-and-pencil techniques or make respondents sort cards on which statements are written thus exploiting the findings of scaling-research and the techniques of test-construction. All these possibilities, when judiciously used, help to limit the various sources of bias or to measure them and keep them under control. We can hardly expect much resort to these more sophisticated techniques by a writer who complains of „an ever-increasing infatuation with mathematics and statistics” which he calls „a monstrous waste of time and effort” (Mens en Maatschappij, 47). This would not be objectionable, if he abstained of judgments in this field. Yet, in his book, Phillips employs the argument of low proportion of variance „explained”, and, in his article, of the large proportion „explained” by the bias. The latter we dealt with when mentioning the control by one of the multivariate techniques. The former is curious, as it suggests a reification of the concept of variance. It might make a difference whether we study a sample of cities, of states in the U.S., or take a time-sample (some macro-sociological trend as measured in successive years for the entire nation). All of these studies when employing the same variables will show (progressively) higher proportion of „explained variance” than a survey of, let us say, 3,000 individual citizens. Many more possible explanatory variables are involved obviously on the individual level than on the „national” level. Thus there is no necessity to ascribe the low correlations to the biased measurement alone, and reject sociology as a science.
There is more inconsistency in the arguments. There is, for instance, the lack of replication in the field of scaling. I partly share this objection; yet, is not the fact that only in about a third of all studies use is being made of scales as developed by others due to the rapid development of scaling research? Did Phillips’ assessment of desirability-set not make previous instruments (without built-in „desirability items”) obsolete and useless? Can we use pre-war scales that did not pay attention to the dimensionality? Obviously, research-workers wish to be up-to-date and will also include new findings with regard to conjunct measurement and recent mathematical scaling models into their arsenal of techniques in search for new instruments that might circumvent some shortcomings that Phillips highlights.

It strikes me, that in spite of his attempts to introduce experimental designs into the social survey (the Graeco-Latin square design as described in his book, p. 121), Phillips actually fails to perceive the real nature of controls in non-experimental designs. From his paper, I get the impression that a possibility of drawing a large random or probability sample from the population and of the control through multi-variate analysis techniques did not occur to him. He lists only two ways of control (homogeneous sample and „holding certain variables constant”) and by refuting them, concludes, that controlled procedures of data-collection are not possible in social research. But there either is a general bias that will evenly be distributed over the interviewees and/or interviewers; then it can be ruled out by the probabilistic nature of the sample (not by randomization in the sense an experimenter uses the term, but simply by the law of the large number). Or the bias (social desirability set) is systematically correlated with some known, „basic” variables; more research into these correlations also will result in an ultimate control of the bias. We shall become aware of its influence and will either control it by partialization or will avoid the „infected” items in designing our projects. Again, there is, in my opinion, no reason for the defaitism that we come so often across in Phillips’ publications.

These may seem technical niceties. Methodological aspects are, indeed, more important. It surprises me that a writer who advocates „personal knowledge” in his book and „knowledge of everyday-life”, thus shunning all-embracing theories, himself becomes a victim of an unwarranted inference-drawing and of sweeping generalizations. His whole book as well as the paper is based on a systematic application of the logical fallacy pars pro toto. Examples: „my particular survey project was biased, hence all survey results are biased”; or: „interview is a biased tool; interview is an
important tool of social research; *hence*, all social research is biased and should be abandoned"; ,,social research is biased; no social theory and science without unbiased research; *hence*, virtually no social science under present condition is possible, new ways should be sought”.

These three wrong inferences are linked with each other in Phillips’ view; in fact, on the meagre evidence of his own unfortunate research-project, Dr. Phillips liquidates not only social research as such, but the whole social science. In this sense his mental health project gets a really historical importance: it was a piece of research to end the research, to bring the whole sociological enterprise after one and a half century to a not so very glorious end.

Once more: nobody would object against Dr. Phillips drawing this personal conclusion from his frustrating experience and turning to philosophy, instead of science. What is objectionable is his intention to impose this conclusion upon others, who, ,,the valets and secular priests” accepting ,,the rhetoric of the discipline” still ,,should be awaken” to accept his standpoint. Against his generalizations, I venture to suggest, that not all interviews or surveys are necessarily biased (having pointed out of how to keep bias under control); that the class of field techniques in sociology is larger than the class of survey-interviews that collect factual knowledge about non-verbal or ,,real” conduct through naive questioning; that social research is, once more, a much larger class of activities than field-projects alone. Finally, sociological theory does not quite coincide with social research; study of secondary, historic or anthropological sources also offers opportunity for theory-construction.

One only can guess what factors made the author overlook the inconsistency in his inference. Perhaps an explanation can be found in his own philosophy. In his book (p. 60) Phillips writes as follows: ,,... I feel that man today (at least in American society) acts less because of convictions concerning what he thinks is right or wrong, or because of other intrinsic rewards, than because of a desire to be approved, loved, or accepted”. Thus not the criteria of logical cogency or empirical truth are decisive for the action (and thinking) of (American) man, but what he thinks others would like and approve of. This statement, though written in a factual form, is explicitly put down as expressing the author’s views on the basic nature of man (Actor). Phillips also is quite clear about his belief that the young generation of American sociologists shares his ,,unhappiness with the present state of the discipline”. Moreover, his paper opens and closes by quoting Gouldner, whose views on the subject, we are told, are similar to
those of Phillips and who is a senior colleague of Phillips in Amsterdam, if I am well informed. All this is understandable, though I might ask a little question: Should we so much become other-directed as to neglect the criteria of logical reasoning and inferential truth? "O brave new world, that has such people in’t!"

There is one appeal to Gouldner, that especially asks for a comment in this context: "American sociology today, is for all practical purposes the model of Academic Sociology throughout the world". In regard to the empirical sociology, this statement, in my opinion, is true. Authoritarian regimes, military occupation suppressed empirical sociology, a sociology striving towards objectivity, throughout the world outside the United States. On the other hand, I would hesitate to underwrite Gouldner’s statement with regard to Phillips’ own views, with regard to his sceptsis of the sociological frame-of-reference. To a European reader, well versed in not only Max Scheler or Max Weber, A. Schuetz, H. Freyer or Frankfurt Schule but also in Husserl, Heidegger, Sartre, Merleau-Ponty, G. Marcel, the author’s suggestions in the direction of a new, alternative sociological paradigm will appear less brand-new. Phillips’ arguments only express the views that we, in Europe, long lived with. Thus it is bringing the coal to Newcastle to vent them here, in a European journal. One can read the existentialist philosophy to learn that the "return to prescientific perception" is a very familiar concept in those circles, indeed. Not all existentialists mention pre-Socratics, and, in this respect, I am curious to see how Phillips will salvage our discipline by the reference to Thales, Heraclitus, Anaxagoras (or will it be Pythagoras, after all?). Yet the idea of common-day experience is inherent in the mainstream of European phenomenology and was not invented by Goffman, Cicourel, Garfinkel or another thinker, perhaps of ethnomethodological brand. For us, in Europe, it is an interesting phenomenon to watch the impact which the translation of French or German writing thinkers has on the American intellectual scene. After an "eclipse" of decades, when a writer falls in oblivion in Europe, his star starts rising in the United States. Often, one rediscovers truths over-there which long ago have been accepted as truisms here, in Europe. Without wishing to be malicious, one example could be drawn from Phillips’ paper: his advice that "sociologists should give more serious consideration to those data which they have gathered "in their heads" as direct participants in a great variety of social activities and situations" (Knowledge from What?, p. 157). Almost a quarter of a century ago, when I came to study in Holland, this was exactly the advice professor Van Heek
used to give his students. He also advised us to read Hans Freyer’s *Soziologie als Wirklichkeitswissenschaft*, whose by Hegel inspired author perceived the task of the sociologists to function as a mirror of the contemporary social trends, to help a society to self-awareness in the sense of „becoming conscious of itself”. In this sense, at least, there is not so much news under the sun. Yet, Van Heek formulated his advice very carefully: it is his problem that a sociologist derives from introspection; its elaboration is done along the way of empirical research. „Arm-chair sociologists” were not very popular in the Netherlands of those days and we learnt quickly to take over the empirical methods from the United States...

This brings me to the final remark on the work of Phillips: its egocentrism and subjectivism. Clearly, it forms a symptom of a crisis. A crisis of its author, so poignantly described in the introduction to his work. A crisis, too, of the sociology, that became self-conscious, putting all kinds of soul-searching questions, driving around in a circle of not so very fruitful „sociology of sociology” or of „research on research”. These may have a positive function, but only as a transient phase to an improved, re-built sociology. For all time bestowed on subjectivist soul-searching is withdrawn from mitigating the evils besetting our societies, from an intellectual effort to cope with our major social and macro-sociological issues. It was Goethe, if I remember it well, who dubbed the great subjective periods in the history of mankind as periods of crisis and illness (romanticism was only beginning at his time). In such periods, priests lose faith in their religion, poets lose faith in their message. I am astonished to witness that scientists may lose faith in their science. Asked what to do about an ill poet, Goethe observed: let him cure himself; he might write again later on, after he is better.

But let us watch what will come out of the pre-Socratics, first.

**Notes**

1. A similar advice is often given to creative minds in the field of philosophy, where reassessment of basic postulates or a construction of an original (speculative) system is aimed at. It is absurd in the field of science, which is supposed to grow cumulatively. Phillips’ standpoint may thus be given its right interpretation: that its author turns his back on social science while preferring (social?) philosophy instead. Yet even philosophy becomes rather a nuisance without a common universe of discourse.

2. R. Kipling pointed out that strong words and names are usually liked by weak persons. It strikes me that in presenting his „background assumptions” (*ibid.* pp. 52-61) much pain is taken to support his philosophical-anthropological creeds by empirical evidence, the very
"hogwash" Phillips censures. Why quote Homans, Blau, Lenski, Goffman, Etzioni and not, for instance, religious thinkers, instead?