

Ivan Gadourek's criticism to end criticism

Derek L. Phillips

I must confess to finding it difficult to know where to start in replying to Gadourek (1). But let me begin, in the spirit of Gadourek's own comments, by noting the manner in which each of us closes his paper. Gadourek, citing J. W. Goethe, notes that in „subjective periods” men lose their faith. He is correct that with regard to so-called „scientific sociology”, as conceived by positivists like himself, I have lost my faith. Perhaps, as Gadourek implies, I will write again (that is, write his kind of sociology) after I am „better” (i.e., regain my faith). In my closing remarks, on the other hand, I allude to sociologists not being alive to life and suggest that perhaps they are asleep (2). As I hope to show in the following pages, Gadourek is indeed asleep; in fact, he is in a deep slumber.

First of all, Gadourek speaks at length of my „embarrassing experience” (p. 119), my „frustrations of a personal scientific career” (p. 121), my „frustrating experience” (p. 125), the „crisis of its author” (p. 127), and „his own unfortunate research project” (p. 125). All of these remarks of Gadourek's are apparently intended to show that when I began to ask certain questions about my own research, I turned, in a fit of temper, to an attack on all social research, *generalizing and drawing inferences on the sole basis of my own „frustrating” experience*. Now Gadourek can read me that way if he chooses, and his willingness to do so perhaps reveals the extent to which he is not fully awake. But it is a bit misleading to proceed as Gadourek does.

In the „Introduction” to my book (3), I indicated *three* different reasons

* Written in response to Gadourek's, „Derek L. Phillips' research to end the research”, *Mens en Maatschappij*, 1972. My response will appear in a future issue of *Mens en Maatschappij*.

which were involved in my coming to write the book, *one* of which involved the mental health project on which Gadourek seems to be fixated. I do not recall speaking of the frustration of my personal scientific career, of my embarrassing experience, or of any crisis. I did report that I had asked myself (p. xiii) „Does my work really contribute anything to sociological knowledge?” It seemed to me a question well worth asking, and one that other sociologists might consider with regard to their own work. I also reported (p. xvii) that „I eventually abandoned a book on which I had been working for some months, in that I had acquired strong doubts about the validity of my data”. I have abandoned other books as well, and more articles than I care to remember. So this hardly represented a crisis for me. Gadourek’s insistence on ignoring my substantive criticisms of sociology by focusing, in a highly misleading manner, on the discussion in the „Introduction” to my book, is hardly consistent with his own admonitions for „objectivity”.

Furthermore, Gadourek’s comments are entirely inaccurate as to what much of the book and the article are about. He states, for instance, (p. 125) that „in fact, on the meagre evidence of his own unfortunate [sic] research-project, Dr. Phillips liquidates not only social research as such, but the whole of social science”. As a matter of fact, I do think that the evidence which I present has strong implications for the whole of social science. What I object to is the entirely *false* statement that my substantive criticisms rest entirely on the „meagre evidence of . . . [my] research-project”. In the article and the book, I refer to several dozen different studies which cast doubts upon the objectivity and validity of social research. One would never know this from Gadourek’s remarks.

Secondly, Gadourek takes-for-granted the usefulness of the various research techniques and procedures which I have tried to raise questions about, especially in my paper. He states, for instance, that (p. 122): „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 analysis of variance”. I am not sure whom Gadourek is speaking of when he says „our” studies, but since it is included in a paragraph where he tells us what „our students of sociology are taught in their courses”, perhaps he means those students whom he had taught at Groningen. If that is so, then I must say that I find very little adherence to this rule in the work of Gadourek’s ex-students with which I am familiar. Nor do I find Gadourek following his own dictates, at least not in his recent article with

Jessen (4). Nowhere in the article do I see any mention of their having obtained measures of possible sources of bias. This is surprising not only because Gadourek claims to follow this procedure, but also because „self-reports” of deviant behavior, such as those regarding drug-use in the Gadourek-Jessen research, are notoriously susceptible to bias and distortion. Although Gadourek and Jessen devote considerable space to describing their research strategy, design, and measures, absolutely nothing is said about bias and invalidity. They do say (p. 383) that „we secured the valid information from about 85 per cent of the original sample”. That is, they say it is valid, but where is their evidence? In fact, their whole article is a prime example of the kind of research that I have been criticizing in my writings. Gadourek and Jessen utilize large, carefully selected samples, numerous variables, sophisticated statistical analyses, and all the other trappings of „high science”. But if their measures are invalid, who cares about the rest? Why should we trust their results? Furthermore, they speak, in a misleading way, of „actual” drug-use even though they have no evidence from their study bearing on this matter. For example, they say that (p. 382) „knowledge of ‘drugs’ positively correlates with the attitude as well as the actual drug-use” and state that they found (p. 394) „more actual drug-use with persons from small parental families”. What they mean, although they are unwilling to say so, by „actual” drug-use is *self-reported* drug-use.

With regard to the substance of the rule cited by Gadourek, I, too, have followed exactly this procedure in my earlier work (5). But, as I indicated in the paper under discussion, I am no longer willing to assume that we *know* all the „possible sources of bias”. In addition, and again as I stated in the paper, our attempts to measure these possible sources of bias are themselves subject to possible bias. Why does Gadourek not respond to these substantive accusations instead of emphasizing the appropriateness of the very procedures which I have criticized?

Similarly, Gadourek directs us (p. 122) to „make use of the ever improving instruments of scaling”. But scaling, too, as my book indicates, is not as pure and unproblematic as Gadourek assumes. And let me register my disbelief that various scale items can be „mixed up so that it is virtually impossible for an interviewee to ‘see through’ the instrument”. Of course, it says that in the methodology textbooks and, in fact, much of the substance of Gadourek’s own remarks read just like such an introductory text, telling us the correct formulas to follow in order to guarantee success. And when he notes (p. 124) that the „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 me, he ignores the fact that I followed just this procedure again and again over the years. He further ignores my criticisms of the simple rule-of-thumb, applications of this procedure. For Gadourek (p. 124) to go on to state that either a general bias „will eventually be distributed over the interviewees and/of interviewers . . .” or the „bias (social desirability set) is systematically correlated with some known, ‘basic variables’” so that we can control or avoid it, glosses the whole point at issue. While many sociologists do blithely assume that bias is evenly distributed („simply by the law of large numbers”), the studies cited in my book and article show this to be an unfounded assumption. And while I once held the view that bias could be controlled and eliminated, I questioned this view in the paper under discussion. Again, however, Gadourek ignores the substance of my remarks. He gives no evidence of being alive to the issues which I discuss in the book and in the article.

A third element of Gadourek’s comments which betrays his deep state of slumber concerns his inability to recognize and acknowledge the very problematic nature of some of the terms which he utilizes in his remarks. On page 122, for example, he asks: „. . . why not view an interaction situation as an experimental setting in which *standardized stimuli* are administered to the subjects?” (my italics). But what does it mean to speak of „standardized stimuli”? Standardized stimuli do not just exist „out there”, as independent entities exerting force on the subject. The idea that standardized stimuli can just be taken-for-granted is for some writers a problem of inquiry in itself (6). When Gadourek states (p. 123) that „In our survey, interviewers put down *the whole setting* [italics added] of the interview . . .”, he indeed stretches our belief considerably. I cannot imagine a study where the interviewers put down „the whole setting”, but it is even more difficult for me to conceive of an „all-seeing” interviewer who could tell us what the whole setting consists of. How do we know, against what external criteria can we check, to assure that the whole setting has been taken down? Such excessive claims are a prime example of the almost total lack of self-consciousness and reflexivity among sociologists, and indicates their unwillingness to think deeply about problems of sociological investigation.

Let me now turn to several of Gadourek’s remarks which require at least brief mention:

1. On page 124, where he discusses „variance explained”, Gadourek notes

that many more possible explanatory variables are involved on the individual than on the national level. O.K., I agree. He then follows by stating that „Thus there is no necessity to ascribe the low correlations to the biased measurement alone, and reject sociology as a science”. I do not, as a reading of my book and article should reveal, ascribe low correlations to bias alone. Nor do I reject sociology as a science, although I am obviously critical of most versions of „scientific” sociology.

2. On pages 127-128, footnote 2, Gadourek tells us that he is reminded „that strong words and names are usually liked by weak persons”. Since he sees me as using strong words and names, the implication is that I must be such a „weak” person. This line of reasoning allows us to see Gadourek, the scientist, in action, measuring the intensity of words and names, and drawing the correct inference from this scientific endeavor.

3. In a similar vein, Gadourek notes (p. 125) that: (a) I wrote in my book that man today acts „because of a desire to be approved, loved, or accepted”; (b) that I believe, in Gadourek’s words, that „the young generation of American sociologists shares his [Phillips] ’unhappiness with the present state of the discipline’ ”; and that, (c) „Moreover”, the 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 informed”. Having made these three points, Gadourek then concludes the paragraph by stating (p. 126): „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 it.’ ”

Bear in mind in what follows that, the paragraph cited above, Gadourek is implying that I neglect the criteria of *logical reasoning* and *inferential truth*. Let us return to the three points made by Gadourek and see these criteria properly employed by the master himself.

a. It is true that I wrote that man acts because of a desire to be approved, loved, or accepted. But I also wrote (on the same page) that „men will sometimes forego harmony with the social environment in order to achieve or maintain inner harmony. They may act for reasons of certain internalized norms — duty or loyalty, for instance”.

b. Gadourek is mistaken with regard to my saying that „the young

generation of American sociologists” shares my unhappiness with the present state of the discipline. What I actually said was (p. xi) that „other sociologists, especially younger ones, have also expressed unhappiness with the present state of the discipline”. My use of the words „other” and „younger ones” is transformed by Gadourek into „the young generation”

c. He points out that I open and close my paper by quoting Gouldner. This is only partially correct. I quote Gouldner on the second page, and mention his name on page 115 — which could be viewed as „quoting” him, although I did not do so. He adds further that Gouldner’s views are similar to mine and that Gouldner is a senior colleague of mine here in Amsterdam. Yes, on some things Gouldner and I have similar views; on others, we do not. As for Gouldner’s being a „senior colleague” of mine, he was not at the time I wrote the article nor was he at the time Gadourek’s comments were written. But he is now. However, I must ask „so what?” Why is that relevant? Why is it important?

What Gadourek shows in his use of quotations, misquotations, misinformation, and innuendo, to imply that I neglect the criteria of logical reasoning and inferential truth, is his own inability to follow these criteria. So much for logical reasoning and inferential truth. Brave new world, indeed!

4. Gadourek (p. 124) asks: „. . . is it not the fact that only in about a third of all studies use is being made of scales developed by others due to the rapid development of scaling research?” I must confess that I do not know.

5. Gadourek states that (p. 126) „To an European reader, well versed in not only Max Scheler or Max Weber, A. Schuetz [Schutz?], H. Freyer of Frankfurter Schule but also in Husserl, Heidegger, Sartre, Merlau-Ponty, G. Marcel, the author’s suggestions in the directions of a new, alternative sociological paradigm will appear less brand-new. Phillips’ arguments only express the view that we, in Europe, always lived with. Thus it is bringing the coal to Newcastle to vent them here, in European journal”. Let me admit, first of all, that I, for one, am not similarly „well-versed”. But let me also express my skepticism as to whether „we Europeans” are either. Nor did I intend this as a brand-new paradigm. And if I carried the coals to Newcastle to vent them in an European journal, that journal need not have accepted for publication an article that apparently stated what everybody (or at least Gadourek) already knows.

Professor Gadourek is, without doubt, a gifted methodologist, whose research has received due attention. Having encountered a severe methodological criticism, Professor Gadourek turned to a variety of arguments in defending the possibility of sociology as an objective science. It was the quality of these arguments that made me accept the invitation of the editors of *Mens en Maatschappij* to write a comment on Gadourek's thought-provoking paper.

Notes

1. I. Gadourek, „Derek L. Phillips' research to end the research,” *Mens en Maatschappij*, 47, 1972.
2. Derek L. Phillips, „Some cautionary notes on sociological research: with special attention to survey studies,” *Mens en Maatschappij*, 47, 1972.
3. Derek L. Phillips, *Knowledge from What: Theories and Methods in Social Research* (Chicago: Rand McNally, 1971).
4. I. Gadourek and J. L. Jessen, „Proscription and acceptance of drugs-taking habits in the Netherlands”, *Mens en Maatschappij*, 46, 1971.
5. See, for example, Derek L. Phillips and Kevin J. Clancy, „Response biases in field studies of mental illness”, *American Sociological Review*, 35, 1970; „Some effects of 'social desirability' in survey studies”, *American Journal of Sociology*, 77, 1972.
6. For instance, I. Lakatos and A. Musgrave (eds), *Criticism and the Growth of Knowledge* (Cambridge: Cambridge University Press, 1970).