

## **On the Question of Validation in Qualitative Methodology**

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### **Introduction**

Over the last decade or so some sociologists, going by the names of Ethnomethodologists and Phenomenologists, have been professing a new methodology for research in social sciences. They trace their roots back to the phenomenology of Husserl and the works of Weber on methodology. These sociologists emphasize the qualitative aspects of social data and as such identify themselves as qualitative sociologists and their way of doing research is the qualitative methodology. This paper deals with the issue of validation in qualitative methodology. How successful is this new methodology in demonstrating proofs of and validating their findings? And also to what extent their claims to a new methodology are acceptable.

### **Qualitative vs Quantitative Methodology**

The principal argument presented in favour of the new methodology is that it offers an understanding of the social phenomena. It claims that such an understanding is not possible through the presently dominant methodology of the social sciences. Indeed, the proponents of this view maintain that not only does their methodology differ fundamentally from the currently practiced method of research, but is also superior to it in that it can effectively deal with the subjective elements, in human behaviour. The basis of their rejection of the widely used methodology of the social sciences is that it was developed for the needs of the natural sciences and hence unfit to explain social phenomena. Some even go to the extent of claiming that the social scientists continue to use the method of the natural sciences because of "ego fulfillment; the achievement of scientific respectability; the quest for social status on a par with that of the natural scientists; and grantsmanship..." (7; pp. 3-4).

The question of superiority or indeed the effectiveness of any method depends on a number of questions. One such question surrounds the issue of validation. Since science is based on generalizations, it is imperative that the findings on which such generalizations depend be valid in themselves. Thus, before this new method of inquiry, the qualitative methodology, can make any claims to superiority or even its suitability in dealing with empirical data, it must prove that findings obtained through such method can also be validated with certainty.

Validation falls in the realm of proof. Scientists making assertion about empirical reality must offer its conclusive demonstrations. "Validity addresses itself to the truth of an assertion that is made about some thing in the empirical world" (5 ; p. 202). Therefore, the methodology that allows one to make claims about the empirical world should equip the researcher with a technique for the demonstration of that claim.

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Closely connected with the question of validation is the issue of reliability. Reliability "concentrates on the degree of consistency in the observation obtained from the devices we employ... (5; p.202). This tells us whether the techniques used to obtain data are useful and reliable. We bring up the issue of reliability in our discussion of validation not only because the reliability of a technique is often confused with the validity of the claims but is also taken for granted. Consistency of measurement with any device does not necessarily imply validity. That is, if interviews reflect similar data on two occasions, it only proves that interviews can be used to obtain such data. It does not say anything whether the data explain the phenomena they are meant to. That is the issue addressed by the question of validity. It seeks to know whether the instrument (Interviews, for instance) measured what it was supposed to measure.

But it appears that qualitative sociologists seem to neglect this issue. Fiestad thinks that "when qualitative methodological procedures are employed the problem of validity is considerably lessened", (7 ; p.6). He would rather be concerned with the question of reliability while at the same time he criticizes the concern of quantitative methodology with the question of validation. Fiestad goes on to say that *a priori* knowledge with which the quantitative sociologist approaches the phenomena poses a serious problem of validation for the latter. Deutscher goes a step further by discrediting the efforts of the quantitative sociologist to attain validity by saying that they operationalize the phenomena to be identical to the instrument that measures it so that there remains no problem of validation, (5; p.203) since it measures what it is made to.

This is only an evasion of the problem. However, it is comforting to know that other qualitative sociologists who are more sensitive to the problem, like Becker, understand the complexity and recognize the inability of the qualitative methodology to effectively deal with the issue (1; p.190). But he also fails to offer any fruitful ways of validation. However, the realization leads him to offer an important suggestion to the researcher in the field. Quoting Lazarsfeld, he says that qualitative sociologists will do well to follow the method of data analysis followed by the quantitative sociologists (1; p.198). Zelditch also offers a similar solution in his discussion of the problem of validation in qualitative methods. He thinks that since phenomena observed in the field are recurring in nature they should be documented quantitatively and be subjected to the canons by which other quantitative data are evaluated (11 ; p. 227).

But why is this reliance on another "methodology" against which a large number of qualitative sociologists are so vocal? To answer this question we shall have to raise another question – that is, question the very validity of qualitative methodology and thereby explain its inability to deal with such problems and indeed challenge its claim to be, if at all, an effective methodology.

If qualitative sociologists accept that sociology is a science, they should also recognize that science has one and only one method that classifies such disciplines as physics, chemistry and biology as sciences. When economics or political science make this claim as sciences they do it on the basis of this unity of method. If sociology claims to be a science it must be on the basis of the same method of inquiry. When we talk of a different methodology we are talking of a different class of disciplines, as would be the case with literature (which also seeks to understand social phenomena and succeeds remarkably well). We are then no more in the domain of science. A different methodology

means a different approach to knowledge and hence is necessarily a distinctly different class of disciplines (10; p. 5).

Therefore, those who claim a different methodology for sociology are, by implication, talking about a different class of disciplines not of the sciences. This contradicts their own recognition of sociology as a science. But of course, if by qualitative methodology they mean to identify some 'new' techniques set within the bounds of scientific norms, then their arguments can be further looked into. Without this acknowledgement there is no issue. This also cuts to the root of the unnecessary distinction made between the qualitative and the quantitative. I shall show later, that such distinctions are not only trivial but unqualified as well. However, for the purpose of carrying on this argument we shall continue to allude to the two methodology concept.

Let us then assume that when one refers to the 'qualitative' one is only speaking about some techniques, whether those techniques are effective instruments of analysis and demonstration of assertions made on the basis of data used is at issue here. But before we go on further, let us emphasize that a methodology must be a self contained system, which not only allows formulations of ideas into concepts which can later be built into theories about reality, but also leaves room for the collection and analysis of data and offer means by which the theories, or for that matter, any assertion made about reality, can be validated and be open to tests by others. Thus a methodology is a combination of various techniques that are used by researchers at different stages of their work to make assertions about reality and demonstrate it. And this explains why any one or even a combination of a few techniques is not necessarily sufficient to deal with reality scientifically. The techniques must be so arranged that they cut through the whole process of theory building including the demonstration of proof.

Central to the qualitative methodology is phenomenology. Phenomenologists claim that they are concerned with the understanding of the empirical world from the actor's own point of view. They see the world as the subjects see it (2 ; pp. 2-3 ). They view human behaviour as the "product of how people interpret their world" (2; p.13). The phenomenologists feel that their task is to capture this process of interpretation. This requires, what they inherit from Weber, an understanding of the phenomena. Therefore the qualitative methodology is geared to achieving an understanding of this process of construction of social reality by the members of a society.

This is where the problems begin. The phenomenologists in general, and especially the qualitative sociologists of today, agree that since man is not an inert object, or for that matter, slaves of instincts, as the animals are, the method used to study the natural world are not applicable in the study of man. Man is moved by his beliefs, sentiments, values, which need to be understood. The scientific methodology used by the natural sciences is not effective in dealing with these subjective elements. Fiestad points out that "the canons of scientific method are not enough; sociologists need intersubjective and trans-objective understanding of their data" (7 ; p. 7). And as if this is not enough to reject the scientific method, he argues further that he can find no "methodological or epistemological justification" for using the "natural science model" in the analysis of the social phenomena (6 ; p. 6). Scientific method then is not enough. To understand the empirical social reality we need to use other methods. This is the method of understanding – the qualitative method. Note that the key word is "understanding". As if other scientists (social or natural) do not understand.

Let's look at Fiestad again. He says in the introduction to his *Qualitative Methodology* that this methodology is being developed by sociologists like Ball, Becker, Cavan, Davis, Glaser, Polsky, and Straus. "who are genuinely concerned with understanding human behaviour" (7 ; p. 6). Does he mean to claim that other sociologists, or for that matter of fact the other social scientists, are not concerned with understanding human behaviour? If we are to accede to such claims, I am afraid, we shall have to assume that what sociologists have been doing for the past hundred and fifty years is meaningless.

However, at the same time let us not avoid the question of understanding either. It is needless to say that the very aim of science is to understand phenomena (be they social or natural). And this understanding is attained at all stages of theory building. The quantitative man, or as some see him, the positivist (2 ; ), or the empiricist (4 ; ), or the macro analyst (6 ; ), has an equal grasp of the situation – of the phenomena he is concerned with. He understands how man conceives of reality, when he builds hypothesis about it; he understands the everyday world (as much as qualitative methodologist does), when he collects data, conducts interviews or when he is drawing up questionnaires, and when he is coding or analysing data. He is as sensitive to the social reality as anyone else. He has to be, or how else would he know what hypothesis to draw, what questions to ask, what conclusions to draw from the data – whether they make sense in the real situation and so on and so forth ?

Douglas (6 : ) takes great pains to explain that the macro analyst (meaning quantitative sociologists) adheres to the norms of understanding all through his process of theory building. Indeed, he thinks that the macroanalyst could not validly infer anything without making use of "his or someone's understanding of every day life" ( 6 : p. 6). He shows how at every stage of his research the macroanalyst resorts to the method of understanding.

Thus, once it is recognized that understanding is not the monopoly of anyone group of thinkers, we can set our minds on other aspects of the problem. Because of the assumption that method of the natural sciences is not adequate for understanding, the qualitative methodologists offered their own. In fact it is not their own at all. All the techniques that are currently labeled as parts of qualitative methodology have been previously used in other areas of knowledge and in sociology as well. Participation observation, the central and most widely used of the techniques has been used with varying degrees of success by functionalists in sociology and in cultural anthropology. The appeal of this technique and others lies in the assumption that an understanding of the social life can be achieved only through reliving it. Coupled with this is the phenomenological requirement of approaching reality with an open mind, without *a priori* knowledge. This leads them to reject the formulation of hypothesis before one goes to the field.

In drawing comparisons between the empiricist and the phenomenologist Bruyn ( 4 ; ) lists these rules of procedure clearly. First the phenomenologist seeks to investigate phenomena without *a priori* knowledge, second, he observe "phenomena that appears symbolically" in his 'consciousness' and third, he intuits "essence and essential relations existing in the symbolic data" ( 4 ; p. 285). The researcher must also be open "to all stimuli that impinge upon his consciousness during his investigation" (4 ; p. 284).

This rejection of *a priori* knowledge and hypothesis and emphasis on the concern with all conceivable knowledge is what Hempel identifies as the "narrow inductivist conception of scientific knowledge" (9 ; p. 11). Yet, most qualitative sociologists adhere to its precepts in one variety or the other. Hempel points out that all data concerning any phenomenon could never be collected. At best it may be argued that the researcher would collect all relevant data, but how would he know what is relevant and relevant to what? This brings in the role of the hypothesis to which the data must be referred to in order to make them relevant.

In sum, the maxim that data should be collected without guidance by antecedent hypothesis (and such *a priori* knowledge) about the connections among the facts under study is self defeating, and it is certainly not followed in scientific inquiry" (9 ; p. 13).

Hempel feels that with such inductive assumptions, the research would not even get off the ground. So it is, with most of the qualitative works. All they end up with is a description of what went on in the field. Such descriptions can at best be qualified as systematic journalism. And this includes works such as those of Goffman (6 ; p. 29).

Even these qualitative sociologists who allow a limited amount of *a priori* knowledge, or some preconceived notions of relation among data just short of a hypothesis, continue to assert openness to all stimuli. This necessarily implies an ever-existing lack of closure on the data. Where should the researcher stop to draw his conclusions then? Or is his report to be presented as an unending series of field notes?

This point of closure is the strongest argument against quantitative methodology. It is said that the quantitative researcher is bound by such closure and restricts himself within this closure (4 ; p. 285). He is seen as closing all doors to further influence of new stimuli on the phenomena. Any one familiar with the process of theory building or research procedure is well aware that no research is ever complete and that the researcher never locks himself into his data. But then he must stop at some point to analyse them and draw conclusions thereby. Without this closure, pursuit of knowledge will seem like chasing a mirage.

Before we proceed further, let's recapitulate what has been said till now. We have shown that the question of validity and reliability are intimately connected and argued that reliability of techniques are not only being confused with validity but is given priority at the neglect of the more vital question of validity. Then we demonstrated that what is today identified as a new methodology is only a combination of a number of techniques borrowed from other disciplines like cultural anthropology. We also examined at length the validity of the claim on the monopoly of understanding, as is done by the qualitative sociologists. And finally we have seen that the phenomenological approach essentially amounts to be the narrow inductivists' mode of inquiry and that the researcher must have prior knowledge and at some point draw a closure on his data to effectively generalize on the basis of his findings.

## **Is Separation of the Two Methods Meaningful ?**

Now it remains to establish where all these arguments are leading to. Becker (1 ;) showed that the field researcher (qualitative sociologist) works in four stages. The first of these is the selection and definition of problems, concepts and indices. The second is to check on the frequency and distribution of phenomena and third, the incorporation of these into a model. The fourth and the final is the stage of analysis and presentation of evidence and proof (1; pp. 190-191). We have already noted that while commenting on the question of validation, both Becker and Zelditch voted in favour of following closely the model of quantitative analysis. Becker also noted with concern the difficulty faced by the field researcher in presenting convincing evidence to the scientific community about his finding. He also recognizes that there is a great weakness in the method and is also aware that no one makes any serious effort to improve it either. The vastness of data is so intimidating that the researcher can hardly present a portion of his findings while the proof of such findings and the process that led to it can hardly find space in such discussions. Becker is also aware that the reader, in the absence of such evidence, is forced to "rely on his faith in the researcher" (1 ; p. 180). That is, validation of the knowledge obtained through intuition is now addressed to faith. Scientific research, thus, comes to be dependent on faith and intuition. If faith and intuition become the basis of knowledge, then what is left of science?

Now let's assume that the first three stages named by Becker are required before one takes up analysis and looks for proof, and fit this into a different scheme of research. In this new scheme we place these in a time sequence. That is, the sociologist who comes to work with any problem has been undergoing a process of conceptualization and definition of the phenomena. From his own understanding and experience with reality, being, and not only acting the part of, a participant observer all along his life. He has observed, often with intense curiosity, the recurrence and distribution of certain phenomena and, as is true of any other individual who has to go about in the society, has definitely drawn up models in his own mind of the inter connections among such and other phenomena. Then, let's assume further that he wants to learn whether his assumptions, regarding any one or more of those phenomena are correct. At this point he feels he needs to know what others think about them and consults knowledgeable people and literature. On the basis of these he gets a more sophisticated version of his model. He then looks back at the social life and makes a simple test of his hypotheses to refine them further whence in the presence of contrary data he makes necessary changes. He is not yet prepared to undertake the major study but is, however, ready with the instruments. He then works out a 'full dress' rehearsal in the form of pretests etc. and makes a few last minute changes if necessary. And now he "stops" for a while or, as some would say, 'restricts' himself within what has been covered so far and collects data, sufficient to be representative of the phenomena.

By now it must be clear that what we have been describing and what follows after the collection of data, is the quantitative sociologist's way of doing research. However, one little comment is in order before we explain as to why we are offering this simplistic description of a well-known process. The researcher had to stop at one point in time to collect data and analyse them but that does not indicate that this was the total closure of his analysis. He is well aware that even before he is through with the collection of data, a lot may have changed, some of which may be significant towards his study. But for

practical purposes some closure is required or the process will become an end less pursuit. And this analysis, as is understood by all, is not the end of data collection, and assumes further research.

This simplistic model, however, demonstrates one thing of great importance to this study. It shows that what the participant observer achieves in the field is nothing unique. Not only the qualitative researcher but all members of any given society have done it in the past, are doing it today, and may be seen doing it in the future. But, of course, the qualitative sociologist will argue that there are situations and phenomena about which we do not know enough and as such participation observation would be necessary there. But is there any situation or phenomenon about which we know enough or may claim to know enough even after the participation observation process ends? Careful thinkers like Becker, qualify this by saying that the researcher may assume that he does not know enough. But how much is enough and how little is not enough is normally left ambiguous. Though Becker would again rescue the qualitative sociologist by saying that the limit is that the researcher knows is little less than being able to form hypothesis when he goes to the field. But how does the researcher know that his knowledge is not enough to draw hypothesis without knowing what amounts to knowing enough? The qualitative researcher does not only over-emphasize the role of participation observation but tends to forget that this is the normal process of life in the everyday world, where all individuals must necessarily perform the same operations.

We do not, therefore, see the quantitative sociologist doing less or, at some stages of his research, any thing different from the qualitative sociologist. The latter develops his hypothesis after he has achieved an intuitive understanding from the phenomena that he has observed in the field so does the former, with one difference, the quantitative researcher only begins at that point. While for the qualitative sociologist this is almost the end of his research. He would hardly venture further, and even if he does, his endeavor will remain far from conforming to the norms of scientific requirements. He will simply end up describing a few more such findings without really ever explaining any thing. Commenting, on the ethnomethodology of Garfinkel, Gellner (8 ; p. 432) shows that "description and not explanation" is the way of phenomenological research. But needless to say that the quantitative sociologist goes far beyond this from where he can claim a level of validity that far exceeds the limits of qualitative sociologists. Becker mournfully admits that qualitative methodology "has not done well with this problem" of validity (1 ; p. 190).

Qualitative Sociologist could have done better if he recognised that his conclusions are essentially quantitative in nature (I; p. 194). But by not wanting to contaminate his study with number or any thing that he has come to level as quantitative, he lets the opportunity of completing the study slip out of his hand; he does not allow the study to follow its logical course of action. However, a few cautious phenomenologists, like Becker and Zelditch, who recognise this, advise the use of 'quasi' statistical' reasonings. The inductive aspect of his reasoning leads the qualitative sociologist to a place from which he should start the deductive process of reasoning, which is necessarily identical to what they label as quantitative methodology. Hypothesis testing clearly is not the business of the qualitative sociology.

Thus, when the two processes are combined we find that the qualitative forms a part of the larger method of theory building, but of course it has a logical primacy, as

Brown (3 ; p. 23) suggests, over the quantitative, in the sense that it comes earlier in the process of reasoning. The quantitative sociologist, as has been argued above, fulfills all the requirements of the qualitative methodology. His only fault is that he does not record it, since that is not the end but remains a means for a much larger goal. It is needless to say, and is always implicit, that he acquired his initial knowledge and his hypotheses through the same process of intuitive understanding that the qualitative sociologist claims to be his specialty. While on the other hand, the qualitative always remains an incomplete methodology and because of this, fails to offer means of conclusive evidence for its works.

This can be avoided by recognising that it forms a part of the general method of theory construction which is here identified as the quantitative methodology. But, even without such incorporation the latter method is not the loser since the process of discovery is built in within the method. And over and above that it is more suited as a method to offer the context of validation, which is a far cry for the qualitative researcher. However, I intend to reaffirm that the separation of the two "methods" is not only unwarranted, it is meaningless too.

Brown in his discussion of the distinctions between the phenomenological sociology and functionalism notes that the former is hard to distinguish from the latter. He argues that many phenomenological theorist, armed with the techniques of participant observation and member test verification go on to develop descriptive-explanatory models which, in terms of their logical structure, are hard to distinguish from traditional functionalism ( 3 ; p. 26). Bruyn (4 ; 1) also notes that empiricism and phenomenology were not considered by Husserl to be opposite of each other. All that Husserl argued was that phenomenology was more fundamental. In fact, at one point, he is reported to have said that phenomenology is "true empiricism" (4. p. 215).

But why is then, so much ado about nothing ? Gellner (8 ; 1) has some answers to that. He argues that ethnomethodologists in particular and the phenomenological sociologists in general, are a result of the revolution of the 1960's. But as with other spheres of the revolution, they ended up assimilating with the same forces, they raised their voices against. "These guys", he points out, "are one might say, the romantic reaction to Parsons' classicism, but within the same language" (8 ; p. 435).

Compare this statement with that of Brown above. They point to the inadequacy of the techniques of qualitative sociology such as participation observation, since these failed to dismount sociology from its saddle. Does not this indicate that there is probably some thing wrong in the assumption that these techniques and others used by qualitative sociology may be at fault? Is it not also true that these techniques are almost taken for granted to be effective indicators of the subjective element of human behaviour (or action, if that is preferred)? What is argued here is that even if it is accepted that the study of subjectivity requires a different, but, of course, an efficient technique, the qualitative sociologists have not found one. These techniques may have been found reliable to some extent in anthropology, but this reliability has been confused as the validity of the techniques in answering questions in sociology. It should, however be noted that when anthropology attempted replication (which is one good way of validating a finding) of the earlier studies, they were embarrassed by total failures. The study of Redfield and Lewis on Tepoztlan and two more studies on negro slums have been sighted as good examples of that failure by Douglas (6 ; p. 27). What is needed, before we examine the question of validation of data in qualitative sociology, is to ask if the techniques are valid.

Zelditch (11 ; p. 227) pointed out that if a reliable account is available, participation observation is not required. Garfinkle's 'commonsense' methodology may be interesting in itself as would be ethnobiology or "ethnomedicine" but lacks the sophistication required by scientific norms as would be recognized by a biologist about 'ethnobiology' and an M.D. about 'ethnomedicine'.

## **Conclusions**

We, therefore, conclude that before we embark upon validating the findings of qualitative methodologists let's see if it is, a valid "method" and whether its techniques measure what they are assumed to, and scientifically. Our position is that neither the techniques nor the method of arguments are either scientific or valid ways of building theories, And despite all their disregard for the quantitative sociology, the qualitative sociologists have not provided any thing that may be a replacement of or even an improvement upon, the former.

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