Phenomenology and the Practice of Science

Amedeo Giorgi

Abstract

Phenomenology is a relatively new philosophy with some distinguishable features and when social scientists base their research methods on that perspective sometimes variations in methods take place that are attributed to phenomenology but the variations are not necessarily consistent with the phenomenological perspective nor with good scientific practices. When the modifications are not consistent it is usually because the claim is based on the failure to distinguish between phenomenology as a philosophy and phenomenology as a theory of science. Such ambiguities can arise because there is not as yet a fully developed phenomenological theory of science even though its articulation is sorely needed. In this article, some examples of the types of methodical errors practiced by promoters of the phenomenological approach that exist in the literature are examined. The conclusion reached is that the methodical errors are due to violations of principles of good science and the modifications are not consistent with the phenomenological perspective.

Introduction

Phenomenology is a philosophy that was initiated by Edmund Husserl (1970) in 1900 and it grew and expanded throughout his life and even more so after his death. It is important to realize that phenomenology is primarily a philosophy and it has philosophical origins and goals. As a philosophy, phenomenology participates in a certain style of scholarship, follows a philosophical method and accepts certain types of arguments as potentially valid. Philosophy is a distinctive scholarly discipline with strong traditions and practices.

The scholarly institution known as science also has a certain style with its own traditions, methods and practices. While the practices of science may have a philosophical basis, the practices themselves are not identical to those of philosophy. Consequently, if one is to borrow a practice from philosophy to apply to science one cannot assume that it can be applied without modification. The context of scholarly work within each type of discipline influences the practices, and if the contexts differ, then the transfer of a practice from philosophy to science cannot be done directly. Normally, some type of mediation is required.
I mention this issue because it seems that many researchers who attempt to apply a phenomenological method to the various social sciences are neglectful of this point. For example, to apply the phenomenological method exactly as Husserl (1983) described it would be to practice philosophy. The knowledge interests of philosophy and the social sciences are not identical. In addition almost all of the social sciences of our era are based on some form of empiricism and phenomenological philosophy is not identical to empiricism. There is some overlap, of course, but there are also some significant differences between them. This problem is exacerbated by the fact that the scientific practices and procedures of a science based upon phenomenology are not yet systematized or securely established. To be sure, there are expositions that show how phenomenology may lead to sound scientific work (e.g., Harvey, 1989; Hardy and Embree, 1992; Ströker, 1997) but a tradition of established, concrete procedures acceptable to all sympathetic researchers does not yet exist. As a result, what often happens is that researchers who choose to use a phenomenological method often use empirical criteria to attempt to resolve problems concerning research designs and the proper use of methods and these empirical criteria may or may not be consistent with phenomenological scientific research criteria. This means that many examples of phenomenological research become ambiguous, and perhaps even faulty, because the scientific procedures and criteria followed were not consistent with phenomenological criteria. There is a gap between phenomenological philosophy and scientific research practices which requires the articulation of a phenomenological theory of science.

Everyone should be aware today that how phenomenological research is being practiced in the social and health sciences often leaves a lot to be desired because phenomenology is not well understood by the scientific researchers (Harder and Norlyk, In Press). However, I will not emphasize that issue in this article although I may touch upon it in certain sections. What I want to concentrate on here is the fact that often the understanding of scientific procedures and criteria by some researchers who use the phenomenological method is deficient. Thus, some phenomenological research is criticized because the accusation is made that phenomenological research is not rigorous enough whereas frequently it is pure and simply poor science.

**Examples From Promoters of Phenomenological Research**

In England, Jonathan Smith (Smith and Osborn, 2008; Eatough and Smith, 2008) has developed a method he calls Interpretive Phenomenological Analysis (IPA), but unfortunately, the theory and practice he recommends has little to do with continental philosophical phenomenology. However, definitions are free, and if the promoters of IPA wish to include the term
“phenomenology” in the description of their method because “it involves detailed examination of the participant’s lifeworld; (and because) it attempts to explore personal experience and is concerned with an individual’s personal perception or account of an object or an event, as opposed to an attempt to produce an objective statement of the object or event itself” (Smith and Osborn, 2003, p. 51), they are free to do so. Definitions of phenomenology along similar lines, i.e., independently of continental philosophy, have been done before. Snygg (1941, p. 406) defined phenomenology as studying phenomena “from the viewpoint of the behaving organism itself” and Rogers (1964, p. 115) also did not refer to philosophical phenomenology and he interpreted phenomenology to be “interpersonal knowing”. These definitions, including Smith and Osborn’s (2003), are mostly “content definitions” of phenomenology. Phenomenology is defined as the study of a certain realm, the study of the experiential world of an individual.

MacLeod (1947), another early advocate of a phenomenological approach to psychology, at least was aware of phenomenological philosophy and he knew that there was a relationship between phenomenology as applied to psychology and phenomenological philosophy. He (MacLeod, 1947, p. 194) explicitly differentiated psychological phenomenology from philosophical phenomenology and stated that the use of the phenomenological method in psychology is “the systematic attempt to observe and describe in all its essential characteristics the world of phenomena as it is presented to us”. The importance of MacLeod’s (1947, p. 194) approach can be seen from the fact that he knew that developments in philosophical phenomenology would influence how phenomenology would be applied in psychology. He understood that psychological phenomenology had its historical roots in philosophical phenomenology but he also knew that it had to function differently within the context of science. In addition, MacLeod (1947) at least included some methodical characteristics and types of results sought when he spoke of a “systematic attempt” to seek the “essential characteristics” of phenomena. He also correctly stated the attitudinal modification that phenomenology required when he (MacLeod, 1947, p. 150) wrote that it involved “an attitude of disciplined naivete”. The qualifications he introduced indicate some sense of methodical discipline, some sense of how the experiential world of the other was to be understood. Speaking to how a phenomenological analysis might be done showed that he knew that phenomenological analyses could be scientific. My point is that a simple “content” definition of phenomenology is not sufficient for an authentic science of psychology, especially if one is advocating a method of analysis. Some sense of theoretical justification of how the method is to be utilized is required, not a mere description of how it is done. Philosophical phenomenology has been with us for over a
century now and if no reference to that philosophy is intended when designing a new method (since the philosophy itself includes a method), then a new justification for the use of the term should accompany its usage. The originators of IPA have given no indication as to how their method is related to the method of philosophical phenomenology. It would have been a lot clearer if the originators of IPA had termed the method Interpretive Experiential Analysis (IEA).

However, in this article, I do not want primarily to stress the phenomenological aspect of the method called IPA. I am more concerned about its scientific status. It seems to me that many of the practices being advocated by those recommending IPA are not scientifically sound. For example, in articulating the method of IPA Smith and Osborn (2008, 54-55) write:

At the same time, it should be recognized that, as is generally the case with qualitative research, there is no single definitive way to do IPA. We are offering suggestions, ways we have found that have worked for us. We hope these will be useful in helping the newcomer to come to IPA to get under-way, but remember that, as you proceed, you may find yourself adapting the method to your own particular way of working the particular topic you are investigating.

Further on, they (Smith and Osborn, 2008, 67) write:

This is not a prescriptive methodology. It is a way of doing IPA that has worked for us and our students, but it is there to be adapted by researchers, who will have their own personal way of working. It is also important to remember that qualitative analysis is inevitably a personal process, and the analysis itself is the interpretive work which the investigator does at each of the stages.

Finally, in speaking about the first step of IPA analysis, Smith and Osborn (2008, 67) state: “This is close to being a free textual analysis. There are no rules about what is commented upon, and there is no requirement, for example, to divide the text into meaning units and assign a comment for each unit …. Some of the comments are attempts at summarizing or paraphrasing, some will be associations or connections that come to mind, and others may be preliminary interpretations.”

Several criticisms can be directed toward these comments from the perspective of good scientific practices. First of all, to state that a method is not a “prescriptive method” is an oxymoron since within science (including human science), all methods are meant to be intersubjective. In addition, it is well established in science that results are correlated with methods. That is why the method employed by a researcher is always so carefully articulated in all research reports. A critical other needs to know exactly how the research was conducted not only to evaluate the adequacy
of the method employed but also so that he or she can check the original researchers’ results or even replicate the study if so desired. But if the method is not prescriptive, and if a second researcher “can adapt the method to her own particular way of working”, and if this particular way of working is not specified in the method section of the report (since no rules guide the analysis), how can an interested researcher check the findings or replicate the study? The ability to check the results of a study or to replicate it is a scientific criterion, and phenomenologically grounded science accepts that criterion. Why shouldn’t a fellow phenomenological researcher want to check the results of a scientific phenomenological study whose results she finds intriguing? Thus, to sponsor a “non-prescriptive method” is an example of poor science and goes against what a phenomenological scientific perspective would advocate, but often critics who do not understand phenomenology blame phenomenology for this misconceived idea.

Then, there is also the question of whether the promoters of the method actually do not have prescriptive intentions as they claim. After all, articles are being written which demonstrate how those who prefer to use IPA actually employ the method. The authors (Smith and Osborn, 2008, 54-55), as noted above, stated that “We are offering suggestions, ways we have found that have worked for us. We hope these will be useful in helping the newcomer to IPA to get underway…” In addition, Eatough and Smith (2008, 187) present a table in their article detailing the “methodological practice of IPA” even as in their text they state, “… although IPA wants to retain its flexible, non-prescriptive stance with respect to methodological issues such as sample size and strategy, form of data collection and so on…”. To offer “suggestions” and to hope that the procedures that the authors are using will be helpful to the newcomer, it seems to me, are prescriptions. To detail the steps for the practice of IPA also appears to be a prescription even if the authors deny such prescriptions in their text. They seem to desire to have the better of two contradictory worlds. Presumably, what the authors really mean is that they are not using fixed procedures that allow for no modifications in their analyses. But science demands that the degree of latitude allowed should be spoken to, otherwise, it is imaginable that without any direction the modification could be so large that it becomes a deviation and an entirely different method is being created. It is, perhaps, desirable not to be rigid, but to be completely prescriptionless is as problematic as being excessively rigid. It is the excess in each case that violates the sense of good scientific practice.

The developers of IPA seem to be attempting to straddle a contradiction. They want to develop a strategy for conducting qualitative research and they simultaneously want to give any potential follower of their strategy total freedom to deviate from what they are doing. Such a strategy seems to confuse scientific rigor with the freedom of a researcher to choose her own
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method of research. Whoever develops a flexible method also has the responsibility to specify the range of possible modifications as well as the logic behind the acceptable ways. Examples of deviations that are so large that they violate the intent of the method should also be provided and the logic that is violated should also be presented. Anything less simply cannot meet the criteria of good science.

The authors (Smith and Osborn, 2008, 54) also wrote that “as is generally the case with qualitative research, there is no single definitive way to do IPA.” While it is true that there is often “looseness” in qualitative research, that is something to be corrected rather than fostered. As many diverse methods there are in science, once a method is accepted, it is to be strictly followed. Moreover, there are such methods in qualitative research. For example, grounded theory purports to be methodical since Charmaz (2008, 84) writes, “Grounded theory contains both positivistic and interpretive elements. Its emphasis on using systematic techniques to study an external world remains consistent with positivism. Its stress on how people construct actions meanings and intentions is in keeping with interpretive traditions.” The phenomenological method as presented by Giorgi and Giorgi (2008) also accepts methodical demands. They make it clear that the steps they describe must be conducted as described or else the method would not be correctly applied. Thus, it is not true that all qualitative procedures lack definitive procedures or systematic steps, as even the promoters of IPA demonstrate despite their rhetoric. Scientific knowledge is meant to be intersubjective knowledge and in order to achieve true intersubjectivity other researchers have to be able to utilize the methods employed by original researchers in the same way because the knowledge obtained is correlated with the methods used.

Furthermore, as seen in the above statements, Smith and Osborn (2008, 55;67) encourage individual adaptability in employing the method. In science, however, we learn that individual variations are meant to be effaced as much as possible, and if they are not, they are considered to be random errors that will hopefully cancel each other out. This idea of methodical adaptability probably flows from the assumption that Smith and Osborn (2008, 67) uphold when they say that “It is always important to remember that qualitative analysis is inevitably a personal process….” Of course, that is equally true of quantitative procedures even if performed by calculators and computers because ultimately a human person has to program the machine and interpret the results. However, a scientist does not try to negate the personal, but rather, he or she seeks an interpersonal level of performance. An interpersonal level of performance only attempts to counteract the individuality of the researcher, not her personhood. It must be remembered that the human being who is the researcher must assume the “role of a researcher” and part of that requirement means to assume an intersubjective attitude. That is, the assumption must be made
that any other competent researcher should, in principle, be able to “see” or “discover” whatever the principal researcher saw or discovered. But if the sense of a personal attitude is advocated rather than an interpersonal or intersubjective one, and if exactly what the original researcher did is not reported --- and often even if it is, it may be so unique as not to be able to be duplicated --- then the performance of a replication will not be possible. Thus, one of the cardinal rules of science would be violated. But it is not phenomenology that is dictating the course of action that the authors are following. It is simply a failure to adhere to one of the major criteria of scientific practices.

Finally, as noted above, the authors (Smith and Osborn, 2008, 67) state that “there are no rules about what is commented upon” in the first step of the analysis. Nor is there any instruction that the researcher has to comment upon all of the data. Allow me to present a brief segment of the data analysis that Smith and Osborn (2008, 68) include in their chapter where they give an example of IPA analysis. Below, the right side presents the actual data and the left side presents the comments made by the researcher(s).

<table>
<thead>
<tr>
<th>Researcher Themes</th>
<th>Participant’s Description</th>
</tr>
</thead>
<tbody>
<tr>
<td>Me vs. Nice</td>
<td>I was to describe myself like you said. I’m a nice person, but then I’m not, am I, there’s other stuff, stuff I haven’t told you, if you knew you’d be disgusted. I just get so hateful.</td>
</tr>
<tr>
<td>Shame, if you knew –disgust</td>
<td></td>
</tr>
<tr>
<td>Fear of being known</td>
<td></td>
</tr>
</tbody>
</table>

Now, we must recall that the authors of this procedure state that there are no rules regulating the researcher’s analysis. They (Smith and Osborn, 2008, 67) state that some of the researcher’s comments are “attempts at summarizing or paraphrasing, some will be associations or connections that come to mind and others may be preliminary interpretations.” We see therefore that the researcher’s attitude is completely open-ended. So into what category do the researcher’s comments fall? Are paraphrasings the same as interpretations? Are associations the same as summaries? Some of these categories reflect the data. Others are indicators of what is initiated in the researcher’s consciousness. They do not have the same analytic value. Moreover, have all of the important statements been noted? For example, the participant says that he “just gets so hateful.” Does that comment manifest some important psychological meaning? It would seem so, yet the researcher(s) did not note it nor was “hatefulness” included in the “Clustering of Themes” table presented on P. 71. No justification is given for not responding to all of the data. Surely scientists would want all of the collected data to be taken into account.
Furthermore, there is no attempt to justify the selectivity and anytime selectivity is present without justification you can be sure that biases are operating. There are qualitative methods that claim that 100% of the data has to be analyzed and accounted for in the analysis whether the data appear to be relevant or not upon initial inspection (e.g. Giorgi and Giorgi, 2008). In the latter method, selectivity is based upon post-analytic discriminations because the discriminations can then be justified with supporting evidence. While the authors (Smith and Osborn, 2008, 67) dismiss the idea of a comprehensive, methodical approach to all of the raw data in the first step of the analysis, such a procedure guarantees that nothing important will be missed and it also more clearly guides the critic who wants to review the analysis. In the example presented above, to what part of the data does “Me vs. Nice” refer? Is it related to the first 2 lines or does it refer to the whole paragraph? Moreover, as expressed, does it capture the dilemma posed by the participant precisely enough? It seems to me that the data suggest that his attitude toward himself is more complex. He can acknowledge a “nice me” before others but also a “not so nice me” to himself. Have those dynamics been captured by the researcher?

Of course, I’m speaking about a partial analysis here and there are indications that the authors do, at the end, pick up the dynamics to which I am referring. But do they do so because they reconsidered that part or because the theme is repeated by the participant? We cannot be sure because the method does not systematically analyze the data and the free implementation of the method does not leave a track record. In brief, a strong limitation of this method from a scientific perspective is that the authors give no rules to guide the conscious processes of the researcher. The objects of the analyses are spoken to but the conscious activities on the part of the researcher are left free to be implemented as the researcher desires. Our critique here is that the best interests of science are not served by such looseness. It is true that no one can get into another person’s head, but rules and directions can help secure better intersubjective understanding. Considering the lack of directions and constraints given to individual researchers with this method – no instructions concerning how to make careful descriptions, no hermeneutic principles to follow, no comments about a perspective to be assumed – it would be even more accurate to call this method “Individualistic Experiential Analysis” because it is the unspecified individual perspective that dominates.

It is equally important to note that Smith and Osborn (2008, 53) nevertheless write that “IPA is therefore intellectually connected to hermeneutics and theories of interpretation” and as support for this claim they refer to Packer and Addison (1989) and Palmer (1969). Eatough and Smith (2008, 179) write that “IPA is an approach to research guided by a particular world-view and epistemology. It is not simply a methodology as such … (and) it connects with much larger intellectual currents in
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phenomenology and hermeneutics and with a quiet and neglected concern in psychology with subjective experience and personal accounts (James, 1890; Allport, 1953).” However, their references to these sources are merely verbal and when one consults these sources the type of relation IPA might have with them is ambiguous at best. For example, Packer and Addison (1989, 3) prescribe certain rules for conducting hermeneutic research. They (Packer and Addison, 1989, 3) write:

*The chapters of this book are arranged in a manner that reflects three phases that can be distinguished in interpretive inquiry. The first phase is that of entering the hermeneutic circle the right way: discovering an appropriate workable perspective from which interpretation can proceed. The second is the conduct of inquiry within that perspective. The third phase is one of critical reflection upon and evaluation of the interpretive account that is the outcome of inquiry.*

To say that one must enter the hermeneutic circle the right way implies that there are wrong ways to do it which implies that certain rules must be followed. To indicate that an appropriate perspective must be discovered implies that certain demands are placed upon the researcher by hermeneutic theory --- one cannot be entirely free. Finally, a critical reflection is called for and a critique also calls for certain rules, such as fairness and relevancy. These rules are quite different from what the practitioners of IPA advocate and so one wonders in what sense it follows in this tradition. A critical perspective towards their own analyses seems to lacking by the users of IPA.

The reference to Palmer (1969) for support for the position of IPA is equally ambiguous because he writes of two traditions that battle each other. Palmer (1969, 46) says: “There is the tradition of Schleiermacher and Dilthey, whose adherents look to hermeneutics as a general body of methodological principles which underlie interpretation. And there are the followers of Heidegger who see hermeneutics as a philosophical exploration of the character and requisite conditions for all understanding.” Later, Palmer (1969, 47) embellishes this point by adding “It would appear that Betti and Hirsch are assailing the whole Heideggerian version of hermeneutics and the New hermeneutics…. Gadamer states that he is simply engaged in describing what is, in every act of understanding; he is doing ontology not methodology” (Italics in original). Thus, to refer to Palmer as support for a hermeneutic tradition raises the question: Which tradition? Presumably the promoters mean the Heideggerian tradition for it purports not to be concerned about methodology. But then, Heidegger claims to be making philosophical explorations of understanding and Gadamer says that he is doing ontology. How then do Smith and his collaborators manage to work scientifically and psychologically? It does
not automatically flow from what Heidegger and Gadamer write. Their philosophical ideas would have to be mediated in order to make them scientifically relevant and there is no evidence of such work by the supporters of IPA.

If we turn to the phenomenological tradition for support of IPA it is equally difficult to find support because both Husserl (1983) and Merleau-Ponty (1962) advocate a method that requires specific steps and rules even for a philosophical level of analysis. But such rules are precisely what followers of IPA refuse to prescribe. But it is possible for psychologists to apply the phenomenological method with rules in a scientific way with appropriate modifications (see Giorgi and Giorgi, 2008).

Finally, even the appeal to James and Allport, who certainly were interested in studying subjectivity, is not without ambiguity. James was concrete, detailed and stylistic in his own descriptions and he analyzed the highly personal, religious descriptions provided by others. Still, in his (James, 1902) *Varieties of Religious Experience*, he enunciates several principles that guided his approach to such experiences. Gordon Allport also preferred more humanistic and descriptive types of analysis of psychological phenomena but he (Allport, 1942) wrote a book dedicated to the purpose of demonstrating that the use of personal documents could satisfy scientific criteria. Thus, neither James nor Allport tried to do qualitative research without respecting certain scientific demands.

In summary, then, there are four features of IPA analysis that fail to meet some basic rules of science. Perhaps there is justification for the different strategies that the IPA promoters use but they do not provide any. The scientific problem areas are: (1) the authors state that the method is not prescriptive and that different users of the method can arbitrarily modify the demonstrated steps. However, the fact that demonstrated steps are exhibited contradicts the rhetoric of prescriptionlessness. The first claim (method is not prescriptive) is contradictory to the idea of a scientific method and the second point (arbitrary modification of steps) violates the very logic of what a method means; (2) the authors claim that IPA, as a qualitative method, requires a personal way of working whereas science demands interpersonal or intersubjective steps; (3) the authors claim that there are no rules regulating the method and prescribe an individually based, subjective mode of interpretive analysis. Only philosophical hermeneuts operate in this fashion. All human scientists who adopt a hermeneutical perspective attempt to specify some rules to help them in their analyses; and (4) For IPA workers, there is not even a rule that all of the raw data has to be accounted for. This license fosters an unaccountable selectivity and heightens the chances of biased reporting of the results and such a procedure goes against what scientific results try to achieve.

Colaizzi (1973;1978) is another psychologist who advocates a phenomenological method for conducting research. In his approach,
Colaizzi (1978) includes a validating step whereby he recommends returning to the participants and presenting the researcher’s findings to him or her so that the participant can compare the researcher’s analysis of the data with his or her experience. Colaizzi (1978, 62) emphasizes that “any relevant new data that emerges from these interviews must be worked into the final product of the research” (italics in original). Superficially this recommendation seems like a reasonable step, but if one thinks through its implications, I think that it is theoretically unjustifiable. I (Giorgi, 2006a; 2006b) have argued this point many times so I will be brief here. There are two reasons why such a “validating step” should not be performed and reported in a scientific article. Firstly, the researcher is interested in the psychological meaning (or other disciplinary meaning) of the experience and not in the general meaning as such. Seeking a disciplinary meaning introduces two differences: the disciplinary perspective is quite other than the ordinary lifeworld perspective and, of course, there is a difference between the raw experience itself and its meaning.

With respect to the first point, if the participant is a layperson, as he or she ordinarily is in psychological research, then why would a psychologist expect her psychological sensitivity to be better than that of a psychologist? The assumption here is that the one who is living through the experience is the best determiner of the meaning of the experience. However, Merleau-Ponty (1964) argues rather forcefully that that is not the case. Merleau-Ponty (1964, 54) states his position this way: “The insight into essences rests simply on the fact that in our experience we can distinguish the fact that we are living through something from what it is we are living through in this fact” (Italics in original). Moreover, Merleau-Ponty (1964, 65) observes that “Reflection on the meaning or the essence of what we live through is neutral to the distinction between internal and external experience” (Italics in original). This means that the meaning of an experience, once it has been expressed, is as accessible to the other (researcher) as it is to oneself. There is no privilege with respect to meaning for the experiencer herself. The data base for scientific phenomenological research is a careful description of what the participant has lived through, not its meaning. Certainly, every experienced therapist is familiar with this phenomenon. Sometimes the other can comprehend the meaning of one’s expression better than the individual who is having the experience.

This state of affairs becomes more complicated when the disciplinary perspective is brought into play and that is our second point. A disciplinary perspective (psychology, education, nursing, etc.) is not the same as an everyday perspective. There is no reason to automatically assume that a layperson’s insight into a disciplinary meaning is equal to, or better than, that of a researcher who works within the discipline. It is obviously acceptable to give participants feedback concerning the study in which
they participated, but not for purposes of validation. The seeking of validation seems to be motivated by, or imitative of, the practice in therapy whereby a therapist presents an interpretation to a client and then she awaits the evaluation of the interpretation on the part of the client. I agree that such a step is critical for therapy because what is at stake is the quality of the client’s life and the client should be involved in decisions concerning his or her life. However, what is sought in research is typically general knowledge about a phenomenon, not an enhancement of an individual’s life.

Finally, there is a practical perspective. If, after laboring intensively on a description in order to come up with implicit and explicit disciplinary meanings, the researcher, after presenting his findings to the experiencer, simply accepts the experiencer’s corrections, as was recommended by Colaizzi, one wonders why the researcher has labored as hard as she has. If the participant’s interpretation is granted such authority, why not simply obtain both descriptions and interpretations from the participant? It would save a lot of time and energy. However, I think that few researchers would readily accept such findings. One only has to look at the criticisms that scientists direct to each other after the publication of studies that have been controlled and systematically conducted. The participant’s reflections on findings do not go through established critical procedures. To be clear, there is no challenge to the experiencer with respect to the articulation of his experience. One simply accepts what has been reported unless there is some internal evidence emerging from the data that suggests otherwise. The determination of the disciplinary meaning of the experience is best left to the researcher. To deviate from that perspective is really to depart from sound scientific principles.

**Some Issues That Arise From Doctoral Dissertations**

I am reluctant to use doctoral dissertations as examples for research flaws because at the time of the publication of the dissertation it is not clear where the responsibility for the research design lies. Because the director of the dissertation has approval power over what is submitted, it may well be that the research design is basically a reflection of the director’s decision and not at all what the student wanted to do. It’s also possible that the design represents a compromise that the student was forced to accept. Unfortunately, however, it is the student’s name that gets attached to the work. It also has to be admitted that we live in an era in which very few professors have been trained in qualitative research. Most research professors today have had adequate quantitative training but they often find themselves thrust into situations where qualitative expertise is needed and they either grope for proper qualitative strategies or else fall back on quantitative procedures to guide qualitative strategies. Thus, it is quite
possible that a professor is actually being kind to a student by stretching into an area where his expertise is thin but he is doing so in order to enable a student to use a qualitative method. In this era of transition one can expect many flaws for numerous reasons so I want to make clear here that my intent is not to blame the researchers but simply to try to correct what I think are erroneous practices from a scientific perspective. If qualitative or phenomenological research strategies are to advance then detected weaknesses in design or application have to be criticized and corrected. Consequently, the names of the researchers who have conducted the following studies are used reluctantly and only because scholarship demands it so that critical others can check and evaluate what I am saying. Perhaps this is also the place to note that the pioneers mentioned above whose work I criticized --- Smith, Colaizzi, Osborn, and Eatough --- ought to be praised as well for venturing into a new field and attempting to create a new paradigm for psychological science. That the work of the beginners of a tradition has limitations should surprise no one.

Friedeberg (2002), for her doctoral dissertation, researched the experience of “countertransference” on the part of experienced therapists. She decided to follow Giorgi’s method, except that she thought that she could improve upon it. She changed the third step in order to make it more collaborative. She (2002, 34) writes: “…. I elaborated Giorgi’s method to include active collaboration with the research participants… Collaboration between researcher and research participant in a phenomenological research method is also grounded in an understanding of the intersubjective nature of relationship.” Friedeberg fails to acknowledge that interviewing a participant in order to gain a concrete description of an experience is also collaborative and intersubjective, but under different rules. When seeking a description of an experience phenomenological researchers minimize their presence because it is the experience of the other that is being investigated, not their own. Yet, by allowing her own views and interpretations to become part of the data, Friedeberg slipped into a dual role --- researcher and participant. Such an approach confounds the data. In addition, she (2002, 33-34) quoted Moustakas approvingly when she wrote: “In phenomenological studies, ‘the investigator abstains from making suppositions, focuses on a specific topic freshly and naively (not from within the natural attitude)….’” Friedeberg, however, violates these very guidelines when she “collaborates” with her participants. A second justification she used was a comment by Wertz (1983) who argued that the phenomenological method is not rigid, but can be flexible according to the phenomenon being researched. However, Wertz (1983, 197) also wrote: “We feel that Giorgi makes explicit, orders essentially, and thereby formalizes the necessary constituents of qualitative analysis so that we can proceed rigorously and systematically knowing at each step of the way what advance is being made.” Friedeberg ignored that sentence and what
she did was to radically transform a step of the method rather than to make
a minor variation of it. I seem to run into this problem fairly frequently
with therapists who conduct research. They do not bracket their therapeutic
presuppositions radically enough and they sometimes let therapeutic
guidelines slide into scientific practices.

Friedeberg is a good example of a therapist letting therapeutic concerns
influence how research is conducted. She (2002, 34-35) states that the
therapeutic relationship has certain similarities to the research relationship
(presumably when research interviews are used). Her argument is that
there is a shared speech that helps develop meanings. But there are also
many differences between therapy and research, even with respect to
“shared speech” and these are not at all addressed by Friedeberg. One big
difference is that in research one is trying to gain knowledge about an
experiential phenomenon and in therapy one is trying to help another
person with a problem in living. In the former case, one wants an account
of the lived experience that is as precise as possible in order to clarify it
and discover its essence. In the latter case, one needs personal data in order
to understand the idiographic experiential mode of living of the individual
in order to help him overcome obstacles. With such a purpose, interpretive
guesses about what is going on could be helpful, but in the former case
such prompts could redirect what the person was initially going to say and
thus his original perspective will become confounded.

Basically, Friedeberg is trying to argue for a heavy involvement of the
researcher in the process of research just as the therapist is deeply engaged
in the process of therapy. The way a therapist relates to her client is her
model. She writes: “….it seems that phenomenological research might
involve collaborations with the research participants in various stages of
the research process, from interview to the final product.” She (2002, 36)
acknowledges further that her “interview itself was openly collaborative in
that there was a free exchange of ideas between me as the researcher and
the research participant.” The participants also commented upon her
psychological interpretations and on the structures she discovered.

We have to remember here that her interest was in how psychotherapists
experience themselves moving through countertransference towards
empathy. In her (Friedeberg, 2002, 15) own words, she was seeking to
discover an answer to the following question: “When a psychotherapist
experiences thoughts and feelings toward a patient that may be potentially
disruptive to the process of therapy…, how might a therapist … use those
thoughts and feelings to come to a better understanding of the patient…..”
(the dots in the question indicate elaborations that the author included but
they do not change the question). However, her heavy involvement in
collaboration makes it doubtful whether her data consists of relatively
pristine descriptions of the experiences as experienced by her research
participants. If her purpose were to investigate “dialogues among therapists
regarding the use of feelings of countertransference”, then her collaborative method would have been appropriate. However, since her purpose and her method do not match, the dissertation is not an example of good qualitative research. Unfortunately, she erroneously states that the motive for her modification is phenomenology but it is really a case of a flawed scientific practice.

Because Friedeberg did include her raw data and the analyses in her dissertation, for which she should receive credit, we can get some sense of what “collaboration” meant for her. In one place, with her first participant, Friedeberg (2002, 118) says “Uh-hum, so there was a little bit of a sense that you were looking at yourself, feeling a little bit insecure, wondering if you were competent enough – that was a little part of what you were experiencing as well, it sounds like.” However, in the raw data (too long to reproduce here), the participant also said that he had a “sense of confidence” and that he was concerned about being “optimally effective” which is not the same as “if you were competent enough”. And after the researcher’s comment the transcript (p. 118) shows that the participant said that he was not “professionally insecure.” In this collaborative dialogue the researcher put forth an interpretation rather than simply repeating what the participant said, and the participant corrected her. Of course, not all exchanges were like this, but enough were to cast some doubt on the analysis. Good scientific analyses require training and discipline that is different from therapeutic training and discipline. This fact is often overlooked by therapists who conduct qualitative research.

Also, what is overlooked is the fact that when a researcher assumes the dual role of researcher and participant her engagement in the research process is not the same as that of the participants. In Friedeberg’s research, her participants selected specific instances of dialogues with their clients and they first described what happened in the therapy session and then they described how they dealt with their feelings. The researcher herself, however, never provided any data about her own work as a therapist, but only her interpretations. A critical other has no way of determining to what extent her interpretations are in line with her data because the data were never presented. She was drawing freely from her own personal experience but those experiences were not revealed and shared with either her participants or the readers. From a scientific perspective, either one should refrain from interpretations at least in the data gathering phase or else, if the researcher is going to be a participant, she should do what all the other participants are required to do --- provide the data upon which the interpretations are based.

Another example of a dissertation that has some phenomenological flaws but implies scientific deficiencies is one conducted by Streseman (2007). She claimed to be following Giorgi’s method but gives no indication that she employed the phenomenological reduction. However,
no claim for phenomenological status can be made if some legitimate type of reduction is not used. Again, the point here is that if one does not follow methodical procedures well, then the work cannot be considered good science. Giorgi’s method is based upon continental phenomenological philosophy and so that method requires that the phenomenological reduction be employed. In this case, the scientific flaw is that the researcher did not follow the proper procedures.

Streseman (2007) also makes an error that I find to be common among many doctoral dissertations. She starts out following one method and then at some critical step decides to substitute a step from another method. Unlike Friedeberg, who invented her own step, Streseman began by following Giorgi’s method and then in the third step she substituted a step advocated by Morse and Field (1995). Now Streseman is certainly free to follow Morse and Field’s description of the phenomenological method but she is not free to begin by following the steps of one method and then continue by following the steps of another method midstream. There is a logic to following a method and it cannot be assumed that the procedures of another method follow the same logic as the initial one. Methodical steps are not transposable if the logic behind them differs. Streseman (2007, 107-108) writes that “In so doing the researcher was able to weed out content that was ultimately irrelevant.” However, in Giorgi’s method no material is eliminated. Thus by trying to integrate two differently conceived methods the author came up with a conflict of strategies without ever realizing it. The conflict makes the analysis suspect. Even though phenomenology was involved here, the basic error is a violation of good scientific practices. One does not mix up methods.

A similar error was made by Snodgrass (2002). She studied the experiences of grandmothers who served as surrogate parents and claimed to have conducted phenomenological research. She (Snodgrass, 2002, 29-32) discusses the nature and value of qualitative research citing 8 different authors without ever giving an indication whether all of the sources could be integrated. She (Snodgrass, 2002, 32) states that phenomenological research begins inductively, which is not correct since it is intuitional; she (2002, 36) calls the phenomenological reduction a methodology rather than a step in a method and doesn’t explain it in any way; she (2002, 36) describes a step in the analysis based on Denzin as requiring a “list of non-repetitive, non-overlapping statements”, but such a procedure does not follow the phenomenological claim to be faithful to the experienced phenomenon because if there are repetitions in an experience then they belong there and probably have a disciplinary meaning. Finally, she quotes and follows Straus and Corbin (1990) with respect to coding data, but such procedures belong to a grounded theory approach and not to phenomenology. These basic errors involve phenomenology but they
indicate even more strongly that the scientific criterion of a reflective, consistent understanding and following of procedures was violated.

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I think that I have established my main point. Many more examples could be given but they would simply be variations about the themes already discussed. Very often, the phenomenological perspective is faulted for certain research deficiencies but in fact a genuine understanding of phenomenology would not permit some of the strategies that are committed in its name. To do proper phenomenological research requires a sound understanding of continental phenomenological philosophy as well as an excellent understanding of human scientific research practices and whether or not they can be practiced consistent with phenomenological guidelines.

I mentioned above that we were in an era of transition and this transition phase causes some unique problems. Basically psychologists get good training in empirical, quantitative research methods but most of them show little interest in qualitative methods. The sheer fact that a qualitative approach to a phenomenon is being sought is enough for them to reject pursuing the same goal. They are the ones who are methodologically skilled but few of them take up the task of inventing adequate qualitative research methods. On the other hand, it is usually the practitioners who perceive the need for qualitative data but they are usually not so skilled in their research strategies. They have other skills and they fall back upon these other skills to guide them in their research but they are often not transposable. Thus, it is necessary to fill this gap and it cannot be filled by a survey course on qualitative research methods. Most qualitative methods are rather laborious and take time and it takes practice and more practice to become proficient in using them. Because qualitative analyses are relatively intelligible there is the mistaken conviction that they can be easily learned. Such an assumption is erroneous. It takes as long to be proficient in qualitative methods as it does quantitative ones even if they are easier to understand. Only when graduate schools begin to give a sequence of qualitative research courses just as they do quantitative courses will we begin to fill this gap.

The above comments are especially pertinent for phenomenology because to apply the phenomenological method correctly one has to have at least a minimum understanding of phenomenological philosophy. Almost all quantitative methods are based upon some form of empiricism as are several qualitative methods (e.g. grounded theory, many content analyses). Phenomenological philosophy differs from empiricism in certain critical areas and so one cannot transfer some well-known empirical solutions to phenomenologically guided research. This is a huge problem that cannot be discussed here but I at least wanted to mention it.
Amedeo Giorgi received his Ph.D. in psychology from Fordham University in 1958. He was trained in experimental psychology and he pursued a career in academic psychology. He found that the standard experimental and quantitative procedures being pursued by mainstream psychology missed the genuine psychological questions that ought to have been asked. After a long search he turned to phenomenological philosophy as the basis for a more adequate methodology for psychology as well as the basis for a non-reductionistic philosophical anthropology. He is the author of Psychology as a Human Science, The Descriptive Phenomenological Method in Psychology and was the founder of the Journal of Phenomenological Psychology and its first editor for 25 years. He is currently at Saybrook Graduate School, San Francisco, California primarily teaching courses in phenomenological methodology and phenomenological psychology.

References


Amedeo Giorgi


