



Difficulties Encountered in the Application of the Phenomenological Method in the Social Sciences

by **Amedeo Giorgi**

Abstract

While it is heartening to see that more researchers in the field of the social sciences are using some version of the phenomenological method, it is also disappointing to see that very often some of the steps employed do not follow phenomenological logic. In this paper, several dissertations are reviewed in order to point out some of the difficulties that are encountered in attempting to use some version of the phenomenological method. Difficulties encountered centred on the phenomenological reduction, the use of imaginative variation and the feedback to subjects.

Introduction

As phenomenological philosophy gets better known in general in contemporary society and there is general awareness that it has developed a method, more scientists interested in studying experiential phenomena are turning to the use of the phenomenological method. On the one hand, this awareness and usage is good, but, on the other hand, it often is not realized on the part of the users that a proper understanding of how to employ the phenomenological method in the social sciences is not something about which a consensus exists. There are several procedures being recommended presently, but not all of these are acceptable, either according to the criteria of phenomenological philosophy or in terms of sound phenomenological research strategies. In this article, I will examine six dissertations that claim to follow the phenomenological method, and I shall highlight some of the difficulties the scholars encountered and comment on whether or not the solutions to the difficulties are appropriate. The criteria to judge the solutions will be specified and will be based upon phenomenology and the logic of research, not on personal biases.

I encountered this problem because of a recent requirement imposed upon graduate students at my institute. In order to help prepare them to conduct doctoral research in a good way, they are now required to write an essay critiquing another doctoral dissertation using the same method they intend to use. Since all my students employ the phenomenological method in their research, I have been reading many other dissertations that claim to have used the phenomenological method. I was surprised to see the great variations in interpretation of the method. Consequently, I have randomly chosen six of these dissertations selected by my students to comment upon the variations and strategies employed. Three are from the field of psychology and three are from nursing.

I want to make clear that the motive for this article is not to fault the students, nor even their directors, but to try to clarify the problems encountered in this type of research and to attempt to resolve them in satisfactory ways. One of the problems is that consistent exposure to phenomenological thought is simply not easily available in the Anglo-American culture. Even when it is, it is usually philosophical phenomenology, and the issue of how to translate

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these philosophical concepts and ideas into scientific guidelines is often not addressed. Moreover, doctoral students are dependent upon the advice of their directors, and with respect to concrete research strategies most directors in this era have been trained in empirical modes of thought and often had to pick up qualitative methods on their own. Unlike certain other qualitative methods, such as grounded theory or certain narrative strategies, the phenomenological method requires a background in phenomenological philosophy which at certain times specifies criteria other than empirical ones. Phenomenology is not against empiricism, but it is broader than empirical philosophy. That is because its method interrogates phenomena which are not reducible to facts.

To argue that some variations are legitimate and others are not is not to assume or propose an orthodoxy. The phenomenological method has some flexible characteristics, especially when applied at the level of scientific analysis, but it doesn't mean that every variation can be legitimated. The variations have to be in accord with phenomenological principles or sound research practices.

Finally, I will only be considering those methods that base their legitimation in Husserl. In other words, I will be dealing with descriptive phenomenological methods rather than with interpretive ones. It would unduly increase the length of this article if every type of phenomenological method had to be considered. Now I will turn to the issue of types of problems encountered in the practice of the phenomenological method within the context of science.

Type of Phenomenological Method to be Used

It has to be borne in mind that the phenomenological method was first articulated by Husserl (1913/1983), the founder of phenomenology in the modern sense, and was intended to be a philosophical method. Unsatisfied with the progress of philosophy over the centuries, Husserl (1910-11/1965) wanted philosophy to be as rigorous as the sciences and thus he proposed a method for analyzing conscious phenomena. Surely, not all philosophies have to proclaim a method, but Husserl believed that, if there was going to be progress in philosophical knowledge, then methodological procedures would have to be followed. In brief, Husserl's philosophical method (1913/1983) stated that one should (1) adopt the phenomenological attitude (more on this below), (2) encounter an instance of the phenomenon that one is interested in studying and then use the process of free imaginative variation in order to determine the essence of the phenomenon, and finally (3) carefully describe the essence that was discovered. The above

is the articulation of Husserl's *philosophical* method. If one applied the above method directly, without modification, one would be doing philosophical analysis. Many commentators have given variations of this method, and sometimes social scientists apply the method without modification, not realizing that philosophical analyses are being conducted even if the data seem to pertain to nursing or psychology. More is required to make the method scientific. For example, Garcia (1996) followed Spiegelberg's (1960) articulation of the method, and, despite the fact that it was clearly within the context of philosophy, no modifications were introduced when she applied the method to nursing data.

Consequently, some sense of the discipline being practiced has to be added to the philosophical procedures articulated by Husserl. We (Giorgi, 1985) ourselves have proposed that a disciplinary attitude be adopted within the context of the phenomenological attitude that also has to be adopted. Thus, if one is a nurse, then a nursing attitude should be adopted and, if a psychologist, then a psychological attitude is required, and so forth. The adoption of the disciplinary attitude brings the proper sensitivity to the analysis and provides a perspective that enables the data to be manageable. The data will always be richer than the perspective brought to it, but it is the latter that makes the analysis feasible. Without the strict application of a delineated perspective one can be pulled all over the lot.

It also should be mentioned that some students seem to consider it a virtue to refer to as many phenomenologists as possible when discussing the logic and steps of the phenomenological method. However, at this stage of the development of the phenomenological method, there are as many differences among commentators as there are similarities. For example, Grant (2004), in legitimating her phenomenological method, refers to Creswell (1998), Geertz (1983), Giorgi (1985; in press), Husserl (1913/1962), Kvale (1996), May (1965), Moustakas (1994), Polkinghorne (1989), and Schütz (1932/1967). Yet, if one put all of these thinkers side by side, one would not get a harmonious integration. There are irreconcilable differences among them. Rather, the researcher has to choose one methodologist and stick with the logic proposed by the methodologist. One can certainly try to introduce variations into the method proposed by the chosen methodologist, but not primarily by quoting from a different methodologist proposing a different logic. One would have to come up with an argument that would be harmonious with the logic of the primary methodologist.

The Interpretation of the Phenomenological Reduction

If one is going to use a phenomenological method that is based upon the thought of Husserl (1913/1983), and all the selected dissertations meet this criterion either directly or indirectly, then the phenomenological reduction has to be implemented. However, it seems that few practitioners get this part of phenomenology correct.

Basically, to employ the phenomenological reduction means two things: (1) The researcher has to bracket personal past knowledge and all other theoretical knowledge not based on direct intuition, regardless of its source, so that full attention can be given to the instance of the phenomenon that is currently appearing to his or her consciousness; and (2) the researcher withholds the positing of the existence or reality of the object or state of affairs that he or she is beholding. The researcher takes the object or event to be something that is appearing or presenting itself to him or her, but does not make the claim that the object or event really exists in the way that it is appearing. It is seen to be a phenomenon.

These are attitudinal perspectives invented by Husserl in order to make the descriptions required by phenomenology more rigorous. Many experiential errors are committed when current experiences provoke associations with former experiences and then are subsumed under the latter as identical whereas they may be only similar, and the differences could be important. Thus Husserl recommends that one examine the ongoing experience very carefully before relating it to other similar experiences or to relevant knowledge gained in other ways. Similarly, one often imagines that a situation is in fact exactly the way that one experienced it only to discover later that that was not the case. So Husserl wants to limit our epistemological claim to the way that an event was experienced rather than allow for leaping to the claim that the event really was the way it was experienced. To make the latter claim is to make an existential or reality affirmation rather than staying within the confines of experience. To limit oneself to experiential claims is to stay within the phenomenal realm.

Now, as we turn to the dissertations under review, we find that the work of MacRenato (1995), Garcia (1996), and Friedeberg (2002) do not mention the phenomenological reduction at all, and Trumbull (1993) and Driscoll (2004) mention only bracketing and do not reference the withholding of the existential affirmation. Grant (2004) mentions the withholding of existential affirmation but does not explain it, nor is it

clear that she employed it. Sometimes the lack of proper application of the phenomenological reduction is due to the sources used, and sometimes it is due to how the researcher interpreted the steps of the phenomenological method. In either case, one cannot say that the phenomenological method was properly employed.

In relation to bracketing, there is an allied procedure that is often used – although, of the studies under consideration here, only Trumbull (1993) used it – and that is an attempt on the part of the researcher to list all of the assumptions he or she has with respect to the phenomenon being studied in order to avoid their having a role in the analysis. It is dubious whether this procedure works. I have known researchers who were trapped within their own listings rather than freed from them. But the major point is that the biases must be recognized in the very process of analysis. Reflecting upon potential biases before the actual analysis is no guarantee that a bias may not still be operating during the analysis. The two activities – reflecting upon one's past experiences in the search for biases, and analyzing phenomenological data currently being experienced – are too different to guarantee a bias-free attitude.

The Role of Imaginative Variation

We mentioned above that the Husserlian phenomenological method required the use of imaginative variation in order to discover essential characteristics of the phenomenon being investigated. Of the six dissertations under review, four – MacRenato (1995), Garcia (1996), Friedeberg (2002), and Driscoll (2004) – do not mention imaginative variation. If this step is missing, and if the phenomenological reduction was not properly employed, then these dissertations would have to be considered empirical rather than phenomenological studies. Indeed, Friedeberg (2002, p. 31) even states that she is looking for criteria that would satisfy an “empirical scientific method”. To satisfy that criterion would definitely put the method employed outside the range of phenomenology, and perhaps that is why she neglects to mention the use of imaginative variation. Nevertheless Friedeberg (2002) contradictorily refers to her dissertation as a “phenomenological investigation”.

If we turn to Trumbull (1993, p. 92), he states that “Phenomenological Reduction involves Bracketing the Phenomenon, Horizontalization and Delimiting in deriving textural descriptions, followed by Imaginative Variation” (capitals in original). Trumbull references Husserl's *Ideas* after that sentence (no pages given), but he cannot be right,

because, while Husserl does speak about horizontalization in other contexts, the idea of delimiting and textural descriptions are not part of Husserl's vocabulary, and horizontalization is not related to bracketing in his works. Rather, it seems to me that Trumbull has taken some of these views from Keen (1975) and has attributed them to Husserl. In any case, there is a good bit of confusion in his method section, including the understanding of bracketing, but we only want to comment here on how he employed imaginative variation.

Trumbull (1993, p. 98) utilized imaginative variation only after he went through the process of delimitation. Trumbull (1993, p. 96) states that delimitation means that he "must, from the phenomenon itself, separate out the central data from the peripheral". In other words, Trumbull is selecting what is essential and separating it from what is not, but he's doing it without the help of free imaginative variation. Then he applies imaginative variation to the delimited data. It seems to me that his use of imaginative variation comes a bit late, and it is confusing when he states that it is applied to the "delimited data, meaning units, themes and textural descriptions to determine the underlying structures and meanings ..." (Trumbull, 1993, p. 98). The steps of the method seem not to be in proper logical order.

The Question of Generalization

There seems to be much confusion about the generalizability of phenomenological data. While Grant (2004, p. 58) allows for generalizability, she also states that "It is hard to determine, however, prior to a phenomenological study whether its results can be generalized". However, that is not true. So long as one can employ the eidetic reduction, with the help of imaginative variation, one can obtain an eidetic intuition into the state of affairs and describe an essential finding that is intrinsically general. Husserl makes it clear that one can do that even with a particular experience. I may observe a specific chair. But nothing prevents me from switching attitudes and taking a more general perspective toward the particular chair and seeing it as a cultural object designed to support the human body in the posture of sitting. That more general description is as true as the particular details of the chair that is taken as an example of a particular perception. There is no way to prevent one from assuming such a more general perspective. The switch results in eidetic findings which are intrinsically general. The understanding of eidetic findings seems to be problematic when phenomenology is applied in the social sciences.

Only two other studies of the six even mentioned the

question of generalizability, and both in a way that ignored the question of eidetic results. MacRenato (1995, p. 134) wrote: "Since the purpose of the phenomenological approach is to gain rich descriptions of individual experiences or of specific human phenomena, findings from such studies should not be considered generalizable", and Trumbull (1993, p. 84) wrote:

I am not interested in nomothetic knowledge obtained from traditional scientific methods, nor am I interested in an understanding gained from observations derived from detached, "objective," measuring, testing, categorizing and classifying ways. I am not seeking knowledge of laws of generalizations, nor of averages. Instead, my search is for idiographic knowledge, or a way to understand the essences: What distinguishes this phenomenon from all others? What is its unique character?

Both of these statements confuse several issues. MacRenato does not realize that the richness of a concrete description can facilitate the discovery of an essence with the help of free imaginative variation. If one does not employ the eidetic reduction and arrive at an essence or some other type of eidetic invariant concerning the concrete, detailed description of an experienced phenomenon by one or several participants, proper phenomenological procedures have not been followed. One could then otherwise merely end up with a summary that is an empirical generalization – and such a generalization has different characteristics from phenomenologically founded eidetic results. It is certainly acceptable for Trumbull to want to have nothing to do with "laws of generalization", but an essence is precisely such a type of generalization. Husserl (1913/1983) admits that there can be an essence of an individual, but it is still a reduction of concrete data to its "essential characteristics". Yet, Trumbull investigated 14 participants and he did try to generalize the findings across the participants because he came up with composite textural and structural descriptions. Consequently, when Trumbull stated that he was interested in a "unique" phenomenon, he did not distinguish sufficiently what belonged to the phenomenon and what belonged to the individual. Since many individuals can experience the same phenomenon, albeit differently, the distinction is very important. A phenomenological analysis can deal with both issues, but they are different problems.

Finally, it is amazing that the other three dissertations did not bring up the problem of the generalizability of the findings at all. Friedeberg (2002, p. 101) mentioned that further research might further clarify

some of her findings, but she never mentioned just what the limits were with respect to generalization. She did admit that she (Friedeberg, 2002, p. 45) had biases that she did not believe she could eliminate, and nor did she want to. Rather, she thought that the better strategy was to address them, presumably in the course of the research. She is free, of course, to choose that option, but it does remove her from a genuine descriptive phenomenological method, which she claims her dissertation to be. Neither Garcia (1996) nor Driscoll (2004) raised this issue, and as a consequence it renders the interpretation of their findings more ambiguous than need be.

The Issue of the Verification of Findings

There is not space in this article to go into the whole issue of validity and reliability with respect to phenomenological results, but I do want to respond to certain practices that are being performed in some phenomenological dissertations, as well as published articles (see, for example, Arminio, 2001), regarding alleged verification of findings. The two strategies involve the use of judges in some form or other, and the presentation of the findings to the participants for them to verify. From a phenomenological perspective, both strategies are misguided. Driscoll (2004, p. 55), for example, wrote that "... three colleagues were asked to review the exhaustive descriptions: two advanced practice registered nurses in psychiatry and a psychiatrist. They agreed with the accuracy of the description based on their experience of caring for women with bipolar II disorder". At best, this procedure might result in some type of "face validity", but it could not function to ascertain genuine validity. For example, suppose an in-depth analysis of the result came up with a completely new finding. Could the experienced nurses judge that it belonged to the experience of bipolar disorder based on their past experience with such patients? The new finding is justified on the basis of all of the new data collected, not on the past experience of experts. Besides, this strategy is motivated by empirical considerations, not phenomenological ones.

However, a major step that was employed for validity was the use of informant or participant review of the results with which the researchers came up (Driscoll, 2004; Friedeberg, 2002; Garcia, 1996; MacRenato, 1995). This is a step advocated by Colaizzi (1978), and so, if a researcher adopts his method, he or she includes this step. What this step involves is that the researcher presents her findings to the participants for the purpose of verification, and, if the participant offers corrections, then, according to Colaizzi, those corrections must be accepted. However, there are several reasons why this step does not survive critical

scrutiny.

First, there are two theoretical reasons for not using this step. The participants describe their experiences from the perspective of everyday life, from the perspective of the natural attitude. The analysis is performed from the phenomenological perspective as well as from a disciplinary perspective (psychology, nursing, and so forth). The phenomenological attitude properly employed results in eidetic findings that can only be checked by phenomenological procedures. It cannot be assumed that the ordinary person is aware of those procedures, so the so-called verification by the participant has to remain dubious. Secondly, since there is a disciplinary perspective, the findings should be loaded with the discipline's orientation, which again means that some expertise is required in order to understand the results. The purpose of the research is not to clarify the experience that the individuals have for their own sake, but for the sake of the discipline. All six dissertations claim to want to understand the phenomenon that is being researched, and the discussions all relate to the research literature in the discipline. The research is undertaken in order to understand certain disciplinary phenomena in a more adequate way. Whether or not the individual participant agrees with the findings is beside the point. There is a confusion here of goals: this is knowledge for the discipline, not for the individual (more on this below). Now, to avoid misunderstanding, it does not mean that a phenomenological method cannot be used to clarify an individual's experience. But if so, then the researcher does not need multiple participants, but multiple instances of an experience from the same participant. Even then, the findings should be mediated by the researcher so that they can be made comprehensible for the participant. Also, my stance does not mean that findings cannot be shared with curious participants. They can, but not for purposes of verification or correction. But it can be a nice gesture, since participants gave of their time and energy so that the researcher could complete his or her project.

But there is still another theoretical reason for not employing the step of having participants verify phenomenological findings. The assumption behind the procedure is that the experiencer is also the best judge of the meaning of the experience. But it is not at all clear that that assumption is true, as any experienced therapist can verify. Merleau-Ponty (1964, p. 54) puts it 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). When a researcher presents phenomenological findings back

to a participant, he or she is asking them to confirm *what* was lived through. The participant may not have even thought about that issue. Participants are surely privileged when it comes to what they *experienced*, but not necessarily concerning the *meaning* of their experience. The findings, if properly obtained, are concerned with meanings of experience. Again, Merleau-Ponty (1964, p. 65) makes a critical but insightful point when he writes: “*Reflection on the meaning or essence of what we live through is neutral to the distinction between internal and external experience*” (italics in original). Consequently, there is no privilege on the part of the experiencer, and to use participants as validity checks is thus not trustworthy. Of course, it doesn’t mean that the researcher is necessarily always correct, but, given the alleged expertise in the ways of phenomenology and the amount of effort that went into the analysis of the raw data as opposed to a simple reading of the findings and a non-methodical response to them, I would bet on the researcher.

The last comment brings up a practical reason for not employing the procedure. One must remember that the application of the phenomenological method is a time consuming, painstaking procedure. If, at the end of this long process, when the final eidetic structure is shared with the participant, one allows a single reaction on his or her part to be a sufficient reason to change the structure, the results of phenomenological analysis are fragile indeed. Moreover, if such confidence is placed in the participant’s experience, then why not simply ask her what her experiences mean to her and simply jot them down? Why go through such a long procedure and possibly not get it right, when a simple word from the experiencer can presumably tell the researcher exactly what needs to be known? Yet Colaizzi (1978, p. 62) emphasizes that “*Any relevant new data that emerges from these interviews [participants’ feedback] must be worked into the final product of the research*” (italics in original). It seems to me that the reasons for not including this step are far more compelling than any possible reason for including it.

Moreover, other problems can emerge. Driscoll (2004) included the validity step in her dissertation. She (Driscoll, 2004, p. 57) states: “Colaizzi’s (1978) seven step method for data analysis was followed which included returning to the participants for a review and validation of the final exhaustive description. Seven of the 11 women responded to the exhaustive description.” Driscoll goes on to say that, on the whole, the participants approved of her “exhaustive description”, but there were suggestions made to add some clarifications and Driscoll added them. But what about the four participants who did

not respond? What are we to make of that fact? Could it be that they did not agree with the exhaustive description? Is that why they did not respond? Of course, there could be other reasons for the lack of response, but since this is such an important step for this method, how could the researcher remain content with less than 100% feedback?

There is another variation on this “feedback by participants” issue and it is confounded with another problem: the problem of whether one concentrates on the phenomenon or on the individual in the research. But first let me address how the “feedback by participants” issue comes up again. Friedeberg (2002) decided that she wanted to study the phenomenon of countertransference as experienced by therapists, and she wanted to use a qualitative method. She states that she explored Giorgi’s (1985b) method, and, while she appreciated the rigour of the steps, she did not think that she could go along with the third step. Her reasoning was that, since psychotherapy was a collaborative effort, she wanted the raw data to be collaborative rather than simply being constituted by the participants, who were three practicing therapists. Consequently, she allowed herself to be completely dialogical with respect to the obtaining of the raw data. Nevertheless, the phenomenon she was interested in studying was “psychotherapists’ experiences moving through countertransference toward empathy”. Friedeberg stressed the relationship that is required for therapy to take place and therefore argued for a collaborative approach.

Her justification for the collaboration is based in part on other situations wherein psychologists sympathetic to phenomenology used collaborative strategies. She (Friedeberg, 2002, pp. 31-32) refers to C. Fischer, who used collaborative strategies in a testing situation, and Gendlin, who also worked with therapeutic settings. The underlying commonality here is that the individual is the focus of the process rather than the phenomenon. The phenomenon is subordinated to the individual. Thus, despite what Friedeberg wrote – that the researcher is interested in the phenomenon of countertransference – the real focus of the study is on how two therapists (that is, the researcher and the participant) talk about countertransference’s effect on one of them. The raw data is no longer the experience of the participant with respect to countertransference but includes the interpretation of the researcher – a therapist’s reaction to how another therapist (the participant) is experiencing the effects of countertransference. Because of this interaction, the raw data is no longer “clean”, as would be required by rigorous research strategies. Friedeberg doesn’t seem to realize that, when an interviewer more or less effaces herself in

order to give room for the participant's experience to emerge, the situation is still dialogic. Without the questions of the researcher-interviewer, the participant would not know what to say. But, whatever the case may be in therapy, in research, even if the situation is dialogic (and how could it not be?), it does not mean that the researcher gets equal billing with the participant.

Friedeberg (2002, p. 32) admits being influenced by Moustakas (1994) in her design, and they both implicitly value the experience of the individual along with the phenomenon that is being researched. They critique Giorgi's method on the grounds that the individual disappears in the application of the phenomenological method as he utilizes it. However, neither realizes that their critique confuses the goal of research with its method. Giorgi is a basic researcher who applies the method in general for uncovering essential characteristics of a specific phenomenon regardless of whom the experiencer is. The phenomenon is what stands out, and to have a specific individual's experience submerge is part of the design. With this goal, the contributions of specific individuals are typified so that general findings can prevail. However, the integrity of the individual experience is not violated in Giorgi's method. That is because the generalization happens with the intentional relationships with the world and others remaining intact. The typical findings are expressed structurally, not isolatedly. On the other hand, should one want to study an aspect of a given individual's experience, one has only to change the goal of the research and the method is equally applicable. The goal of the research is not then the phenomenon as such, but a specific phenomenon as experienced by John Doe. But such a strategy does not usually contribute to general psychological knowledge.

This is something frequently encountered when psychologists with therapeutic interests undertake research. Their therapeutic interests are often not sufficiently bracketed and they conflate good research design principles with therapeutic practices. Thus, because the therapeutic situation is dialogic, Friedeberg (2002, p. 45) believed that her research setting had to be dialogic in the same sense, and so she made her own responses part of the raw data. The logic is not sound. She was interviewing therapists about *their* experience of countertransference with their clients, and her role should simply have been to listen and, when necessary, to evoke deeper

descriptions, but not to participate in the experiential descriptions as a psychotherapist. If her interest were truly in the *phenomenon* of countertransference, then the individual ways that her participants dealt with that phenomenon had to be typified and not heightened as idiosyncratic responses. Otherwise her data could only speak to the individual ways that her participants experienced countertransference and no generalization would be possible. One cannot follow therapeutic interests and research interests in the same study. They should be kept separate. A therapeutic attitude should be adopted for therapy and a research attitude for research, even if one is doing research on a therapeutic relationship.

Conclusion

The review of these dissertations makes it clear that scientific phenomenological research has not as yet come of age. Unfortunately, I believe that the results encountered here are typical. The basic principles of phenomenology are often cited correctly, but they are not always fully understood, and nor are they always implemented correctly. Thus, when approaching social science research with phenomenological titles, one must be wary concerning what will be found.

Given that evaluation, it is well to remind ourselves that the last word regarding this type of research has hardly been spoken. It is also good to remind ourselves that, when natural scientific psychology got started, it too groped about for a while before certain practices were refined and became acceptable. The chief drawback, as mentioned at the beginning, is the lack of proper exposure to sound phenomenology. It happens in the philosophy departments, but those lectures are either directed toward philosophical issues or are expositions of philosophical texts. That is all to the good, but what is also required are many discussions concerning how to mediate between the fundamental concepts of philosophical phenomenology and the practices of sound scientific research. This mediation will not be easy to accomplish, because the habits of thought and practices are so solidly empirical among today's social scientists that it may simply require a new generation with a new frame of mind to bring about the required transformations. But, while nothing should prevent us from continuing to try to improve the quality of scientific phenomenological research, that cannot take place unless we first acknowledge that what is being practised today needs improvement.

About the Author



Amedeo P. Giorgi graduated with a PhD in experimental psychology from Fordham University in 1958. After working as a human factors consultant to government and industry for several years, he embarked on an academic career, proceeding from Manhattan College to Duquesne University and later the University of Quebec at Montreal. Currently, he is a Research Professor at Saybrook Graduate School in San Francisco, with which institute he has been associated since 1986.

Professor Giorgi's speciality is in the area of psychological research practices, especially qualitative approaches. Being critical of the methodology of mainstream psychology, his search for alternative approaches to the study of psychological material culminated in his adopting the descriptive phenomenological approach of Edmund Husserl and Maurice Merleau-Ponty as a framework for developing a phenomenological method conducive to understanding psychological problems. In addition to having directed over 100 dissertations that have applied the method to a wide variety of topics in the psychological field, Professor Giorgi has published over 100 articles on the phenomenological approach to psychology. He has also lectured on phenomenological psychology in Europe, Asia, Latin America, Australia and South Africa. An advisor to the *Indo-Pacific Journal of Phenomenology* and its editorial board, he is the founder and original editor (for over 25 years) of the *Journal of Phenomenological Psychology* and author of the seminal and now classic text *Psychology as a Human Science: A Phenomenologically-based Approach* (New York: Harper & Row, 1970). He also edited *Phenomenology and Psychological Research* (Pittsburgh, PA: Duquesne University Press, 1985) and co-edited, with P. D. Ashworth & A. J. J. de Koning, *Qualitative Research in Psychology* (Pittsburgh, PA: Duquesne University Press, 1986).

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