THE PHENOMENOLOGY OF REPLICATION

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> A flower is relatively small. Everyone has many associations with a flower—the idea of flowers. You put out your hand to touch the flower—lean forward to smell it—maybe touch it with your lips almost without thinking—or give it to someone to please them. Still—in a way—nobody sees a flower—really—it is so small—we haven't time—and to see takes time, like to have a friend takes time . . .
>
> Well—I made you take time to look at what I saw and when you took time to really notice my flower you hung all your own associations with flowers on my flower and you write about my flower as if I think and see what you think and see of the flower—and I don't.
>
> —Georgia O'Keeffe

Introduction

Psychical research, in its pursuit of scientific rigor, has tried to follow in the footsteps of natural science. To our forefathers in the 19th century, there was only one science—that pioneered by physics. But the objects of study within physics did not and do not, generally, encompass acts of consciousness. Therefore, it has not been necessary for natural science to consider this level of functioning.

As parapsychology now moves into its second century, however, we must ask ourselves, how useful are the methods of natural science for psi research? Do they allow us greater understanding of psi, or do they force the phenomena into a mold cast by the natural sciences? The focus of this paper is to examine such questions, utilizing a phenomenological perspective to look specifically at the "problem" of replication.

In our discussion, we will proceed in three primary steps. First, we will concentrate on the method of phenomenology, for it offers us both a criticism and an alternative to natural science methods. With this perspective in hand, we will move on to our discussion of

replication. Here, we will be concerned with the basic assumptions which underlie our studies of psi. A final step will be to illustrate the usefulness of phenomenology in yielding more comprehensive and appropriate data for psi research.

Phenomenology

A wealth of material has been written on the topic of phenomenology (see Douglas, 1970; Kockelmans, 1967, 1968; Severin, 1973; Turner, 1965). Dr. Gerda Walther, in her 1953 presentation to the Parapsychology Foundation, made a plea for the introduction of phenomenological method in the study of psi phenomena. The origins of phenomenology, however, can largely be traced to the writings of Edmund Husserl, a German philosopher of the early 20th century. Historically a reaction against the limitations of both empiricism and rationalism, Husserl's epistemology takes as its departure point the "essence" or "pure phenomena" (i.e., an individual's act of perception, judgment, experience and intention). It rejects the notion that the phenomena of life can or should be studied in terms of formal, clear and distinct categories. Rather, Husserl suggested a new foundation for scientific inquiry, one which necessitates a return to phenomena as they are directly experienced in consciousness. As stated by Husserl: "Merely empirical, descriptively classificatory (inductive) knowledge is not yet science in the full sense of the word. It merely furnishes relative truth, tied to specific situations. Philosophy, genuine science, aims at absolute, ultimately valid truths, which transcend all relativity. Such truth defines what exists, as it exists in itself. The world of perception and prescientific experience reveals itself, of course, as a really existent world in spite of its relativity, but its properties, true in themselves, transcend naive experience" (Chisholm edition, 1969, p. 131).

According to Husserl, our minds are so filled with preconceived notions about the way things should be, that we rarely experience them for what they really are. It is this common-sense, everyday interaction with the world, that Husserl (1972 translation) refers to as the "natural stance." He describes it in the following way: "I find continually present and standing over against me the one spatio temporal fact world to which I myself belong, as do all other men found in it and related in the same way to it. This 'fact world' as the world really tells us, I find *to be out there,* and also *take it just as it gives itself to me as something that exists out there.* All doubting and rejecting of the data of the natural world leaves standing the general

thesis of the natural standpoint. The world is a fact-world always there; at the most it is at odd points 'other' than I supposed, this or that under such names as 'illusion,' 'hallucination,' and the like, must be struck out of it, so to speak; but the 'it' remains ever, in the sense of the general thesis; a world that has its being out there" (p. 96).

Husserl rejects the ultimate value of this perspective—describing the "objects" or "things" which are "out there" as the products of consciousness—not pure consciousness. As such, the focus of scientific philosophy should be a suspension of metaphysical, methodological and teleological presuppositions. An investigator may then deal with a more complete phenomenon because he lets it emerge as it is, rather than restricting it to those aspects that he wishes to see or manipulate.

While phenomenology is concerned with subjective experience, it is not just concerned with any individual human consciousness. Rather, in adopting a phenomenological attitude, we are concerned with the "essence" of consciousness—experienced by all people. Phenomenology seeks to understand "what essential conditions must be fulfilled if any consciousness 'whatever' is to experience a particular kind of phenomenon as adequately as possible. And there you find that *each kind of phenomenon corresponds to a particular kind of conscious presentation*" (Walther, 1953, p. 4). We are interested in mapping the various levels of consciousness, in order to better understand pure consciousness. Unlike empiricism, which shows difficulty in dealing with general, structural, contextual behaviors (e.g., creativity, innovation and psi), phenomenology attends to the varying degrees of *attention* we give to our experience and which alters its qualities (Locke, 1981).

Method

The foundation of phenomenological method is what Husserl refers to as the "phenomenological suspension" or "epoche." Here, the goal is to identify and "bracket" presuppositions which bias our understanding of the true "essence." A useful description of the phenomenological suspension has been provided by Natanson (1962), in which he states: "Phenomenological suspension or, to use Husserl's term, 'epoche,' consists in making explicit to consciousness the thesis which unconsciously underlies every individual judgment made within ordinary life about reality. Suspension means first of all coming into awareness of the very meaning of the natural attitude itself. Negatively put, suspension of the General Thesis of the natural standpoint most

certainly does not include or signify a denial of the reality of the external world or of the validity of our ordinary experience within it. Rather, as phenomenologist I place in phenomenological doubt (which is not psychological doubt) my traditional common-sense taking for granted of the very reality of the world within which things and events are noted and appraised. Suspension, then, involves a shift in modes of attention. The same reality I took for granted in typical fashion in naive attitude I now re-view in phenomenological attitude. The real world, everyday existence, etc., do not mysteriously vanish under 'epoche'; they are merely seen in terms of a perspective hitherto unimagined and even unimaginable in common-sense terms . . .

" 'Epoche' is the necessary condition to all other phenomenological procedures, for it guarantees the freedom of a starting point which refuses to remain within the metaphysical orientation of common-sense. And further, 'epoche' is the clue to phenomenological method to the extent that it points to the kind of descriptive neutrality phenomenology encourages" (p. 12–13).

There are four important rules for carrying out the phenomenological suspension (Locke, 1981). First, we must attend to the phenomena of experience as they appear—describing rather than explaining the basic essence. Secondly, a domain for inclusion and exclusion is important for describing a phenomenon—although this itself is subject to bracketing. Thirdly, all phenomena immediately perceived must be equalized or horizontalized. In other words, we must suspend our notions of "realness" and "hierarchies of reality." While there is no possibility of absolutely canceling out assumptions inherent in the acts of perception, these assumptions are made, as much as is logically possible, into data and resources in our assessment of how perception is constructed and reported. There is a reflexive quality inherent in the phenomenological reduction ('epoche'). While we can never detach ourselves from the "natural stance," we must reflect on our presuppositions in the course of observing phenomena. In this manner, we are active participants rather than the passive observers of the "natural stance." Finally, and only after careful suspension, can we begin to look for invariant or structural features—as they emerge, rather than as we shape them.

Here, it is vital that we retain the integrity of the phenomena—avoiding the risk of false leads. The methods must be defined by the phenomena and not vice versa. This means most clearly that the phenomenon to be investigated must be the phenomenon as experienced, not that which is created by experimental manipulation. This

is contrary to the classical scientific approach, which assumes only one general set of criteria for establishing scientific validity—that founded in the natural sciences. This does not eliminate the usefulness of empiricism; rather, it calls for a truer rendition of empiricism in which experimental efforts are secondary to an understanding of the essence of the phenomenon as identified.

Phenomenology then, offers a qualitative method for understanding and eventually organizing experience. It is not, however, to be confused with introspection for, as pointed out by McLeod (1964), it differs in two important ways. "First, the introspectionist makes the initial assumption that experience is reducible to a finite number of conscious elements and attributes; this is a bias which phenomenologists attempt to bracket. Secondly, and perhaps more important, there is no place in introspection analysis for meaning, except insofar as meaning can be reduced to elements and their attributes. For the phenomenologist, meaning is central and inescapable. To try to abstract or extract meaning from the phenomenal world is futile; all we achieve is a change in meaning" (p. 54–55). A third distinction, not made by McLeod, involves the role of the researcher. Within our investigation, we must remember that our language, orientation and biases are vital properties within the process of data collection.

Although brief, we now have a framework for understanding the method of phenomenology. The major points are (a) the identification and suspension of presuppositions which bias our understanding of true phenomena and (b) the philosophy that phenomena should shape method—rather than allowing it to be shaped by method. We are now prepared to proceed with our discussion of psi and replication.

The Problem of Replication

Replication is a cornerstone of scientific method. For parapsychology, we see great emphasis on the need for replicable experiments—the focus of this conference (i.e., the "problem of replication), clearly illustrating this. But why are we so concerned about replication? What is its value for psi research?

Personally, I am doubtful that psi is, or will be, replicable—not in the sense of the natural sciences. Certainly, it is not replicable in the sense of electricity or gravity, at least not that we can measure. A more appropriate sphere of analogies can be drawn from such phenomena as creativity, imagination or dreams. While transitive, such states are vital to an understanding of human consciousness. They are not replicable upon command and, each time they are

experienced, the content and context have changed. Not one of us can guarantee the successful appearance of psi, any more than we can predict a creative moment. But because such states are transitive that should not exclude them from the scope of scientific inquiry. They are vital components of human consciousness and a true science must address them. But perhaps it is better to meet them on their own terms, rather than on the terms established by physics and chemistry. Perhaps this way we might learn more about the essence of the phenomena.

Phenomenology suggests that natural science methods are neither appropriate nor fruitful for the study of consciousness. If the last 100 years are any indication, it is clear that such methods have shed little light on the essence of the psi process, or other aspects of consciousness. Can we hope to achieve replication of phenomena of which we have little understanding? As often as not we, as psi researchers, cannot even agree on what constitutes a successful demonstration of psi.

We are not, however, alone in our problem. As pointed out by Robert Rosenthal (1966), unsuccessful replication is so common that behavioral scientists hardly know what it means when one experimenter's data fail to conform to another's. Epstein (1980) has gone so far as to say that we in behavioral sciences are rapidly approaching a crisis, due to lack of experimental replication. Perhaps the crisis lies, not so much in the outcome of our experimental efforts, as in our clinging devotion to the methods of natural science. While we are so concerned with the problem of replication, my question is why?

There are a number of reasons for our commitment. To address the first, we turn to an historical setting (see Beloff, 1977; Mauskopf and McVaugh, 1980). We find psychical research developing in the midst of 19th century intellectual enlightenment. Science is making great strides and the break with dogma is more than a crack. Psychical researchers, distinguished scholars and scientists of the time, are attempting to detach themselves from the mysticism surrounding mesmerism, spiritualism and other occult practices. Particularly within the Rhine era, beginning in the early to mid-20th century, we see parapsychology emerging, firmly grounded within a laboratory framework. Experimental procedures, including statistical methods, have been employed and researchers can assure themselves and others, that their studies are clearly divorced from religious or magical beliefs. Parapsychology, by utilizing scientific method, has been carved out as a distinct discipline of study, a branch of science and not of the occult.

A second reason, emerging from the first, is that psi researchers hoped, and continue to hope, that the methods of natural science will eventually prove fruitful. We have been handed a tradition, a scientific heritage. This heritage believes replication, so vital a component of natural science methods, is useful for distinguishing the genuine from the spurious. Parapsychologists hope that a replicable experiment will establish the appearance of a "real" phenomenon. There are problems, however, with this natural stance perspective. Historians, philosophers and sociologists alike, have shown that there is simply no agreed upon criterion of replication in science. According to Braude (1979), Collins (1978), Kuhn (1975), Travis (1980) and others, replication is a notion attributed to what are *considered* genuine phenomena. It does not work as a criterion for distinguishing the real from the spurious, especially not in controversial areas of research.

Numerous examples can be used to illustrate this point. For instance, Travis (1980; 1981) has insightfully examined a controversial area of research known as memory transfer, hoping to shed light on the formation of "scientific truth." For those unfamiliar with this work, begun in the early 50s, it involved the ostensible transfer of learned behavior from trained animals to untrained animals. In one approach (McConnell, 1962), trained planaria were ground up and fed to their experimental counterparts. Results consistently indicated that untrained animals displayed the same behavior as that conditioned into the initial group. In other words, memory was ostensibly transferred via body parts to the untrained worms. In fact, out of an estimated 400 systematic replications, two-thirds have shown positive support for the memory transfer hypothesis. However, in spite of the impressive replication rate, scientific acceptance of memory transfer has not arrived. Numerous counter-explanations have been developed to account for the phenomenon. A common sense perspective prevents us from accepting the idea that memory may be stored in other bodily parts than the brain. According to Travis, none of the counter-explanations totally account for the observed phenomenon. It appears that replicability follows as much from a phenomenon's acceptability, as its acceptability follows from its replicability. "Replicability and acceptability are inextricably interwoven" (Travis, 1981, p. 26). Preconceived notions have a strong influence upon our interpretation of experimental results—there is no common criterion for assessing replication in science.[1]

We can illustrate our point within the parapsychological literature by turning to the continuing dispute over Ganzfeld replications. Here we find Charles Honorton (1977; 1982) and Ray Hyman (1983),

both capable individuals and dedicated scientists, reviewing the same body of material, yet reaching vastly different conclusions. Each views the criterion of replication from a different perspective, each with a different set of presuppositions. Each tries to communicate this difference to the scientific community, through an "objective" review of the data.

It has been hoped that statistical techniques, based on mathematical probability, would provide some absolute criterion for demonstrating the existence and replicability of psi phenomena. However, as pointed out by Henretta (1979), this too is problematic. "Despite its apparently scientific character, quantification does not necessarily yield an objective view of human reality" (p. 1315). As we have shown in the memory transfer work, there is no clear or unambiguous criterion of replication and statistical procedures are no guarantee that replication results will be accepted by the scientific community. Science is a social process, made up of human actors who define the sphere of activity. Not only is replication not absolute, but it is subjective and vitally dependent upon various social factors.

Even if there were some universal value attached to statistical techniques, how appropriate are they for our studies? While quantitative analysis is fruitful for understanding the world of material objects, it is limited in its value for understanding qualitative properties of human experience. Little, if anything, is understood about such abstract, yet vital, qualities as imagination, motivation or emotion. Attempts have been made to objectively study such states, to quantify them. Although overly simplified, we might, for instance, have a client in our laboratory. We attach electrodes to his scalp and monitor his brain wave activity by means of an EEG. We may well obtain a specific reading for this individual, which we then correlate with his feelings at the time. Asked for his subjective response, our client reports feeling great, happy and healthy. During the next week, we invite our friend back into the laboratory, hoping to replicate our earlier finding. We note a very similar brain wave activity. Aha, we think, our client must be happy. Asked how he feels, however, he expresses depression (Heseltine, 1982). If we give up easily, we can conclude that such methods of measurement are inadequate for evaluating internal states. If we wish to persist in our reductionistic bent, we might try other forms of measurement, hoping to find the answer. But will we ever succeed in quantifying the essence of human experience? For, to quantify man is to objectify him, thus reducing him to parts which are admissible to study. As pointed out by Husserl, the reliance on natural science methods has a tendency to

force phenomena into a mold. In other words, by straining phenomena through a tight methodological filter, what was once rich and robust is now reduced to what which can be described and explained exactly by means of natural science. A more reasonable approach might be to allow the methods of study to emerge from the phenomena, rather than restricting them to those aspects that we wish to see, manipulate or measure. The reliance on statistical techniques for assessing psi really tells us little about the actual psi experience. It tells us little about how we might learn more about the organization of psi in consciousness.

To shed light on the problem of replication in science, we can again turn to the sociologists. Collins (1974) has proposed two models for understanding the rules of scientific knowledge and its transfer. The first, algorithmical, is analogous to the program of a digital computer. Here, knowledge, like the computer program, is reducible to a number of specific elements. "Thus it is implied that there is a finite series of unambiguous instructions which can be formulated, transferred, and when correctly followed, will enable a scientist to copy another's work exactly" (p. 206).

In this "official view" of information transfer, a limited range of questions is pursued and a certain range of results is expected. A body of preconceived explanations exist to account for replication failures. In the case of a high school chemistry class, for example, a failure to replicate is not attributed to anomalous results, but to a failure to follow the correct algorithm (Rao, 1980). As noted by Thomas Kuhn (1975), the project whose outcome does not fall in that narrow range of expected results is usually just a research failure, one which reflects not on nature, but on the scientist.

Within the algorithmic model there are problems, however, which led Collins to formulate the enculturational model of knowledge transfer. Because a criterion of replication is not absolute, it becomes necessary to understand replication in terms of a shared culture. According to Collins (1975): "it is the transmission of a culture which legitimates and limits the parameters requiring control in the experimental situation, without necessarily formulating, enumerating or understanding them, and which ipso-facto generates the set of anomalous experiments (failures which can't be explained by uncontrolled legitimate parameters)" (p. 207). Polanyi (1958) describes such aspects as "tacit knowledge," assumptions which are taken for granted, but which are often difficult, if not impossible, to articulate.

To illustrate the point, we may refer to Collins' (1974) study involving laser research. Here, he attempted to perform a kind of

phenomenological bracketing on the production of scientific truth. He found, through examination of letters, conference presentations and conversations, that the algorithmic model was not appropriate, even for the building of lasers. It was found, for instance, that a scientist wishing to build a laser nearly always needed to spend time with a researcher who had already built one. Neither written reports nor conversations with secondary sources, were enough to allow an adequate transmission of laser building knowledge. Even an extended period of personal interaction was not enough to guarantee success. There were instances where one well-meaning and experienced scientist tried to communicate his knowledge of laser building to another scientist—but the apparently isomorphous model was found to be an inexplicable failure.

Collins concluded, after much research, that there is apparently no absolute way of knowing whether the necessary culture has been transmitted—short of successful replication. In such a case, the failure to replicate does not reflect on the validity or "realness" of the initial laser. Rather, failure to replicate was a failure to communicate the necessary ingredients. There will always be some room for discussions as to what counts as the sameness of experiments.

There is a third reason for our clinging devotion to natural science methods, complementary to those already mentioned. For various reasons, we need to belong to the accepted scientific community. To best understand this point, it is useful to explore the scientific process within the framework of ritual. As pointed out by Goffman (1959; 1967; 1971), ritual behavior is useful in asserting and maintaining an individual's position within society. To perform properly within a given social system, the actor must be prepared to participate in the appropriate rituals (e.g., experimental manipulation, statistical assessment and, finally, replication). Frameworks of expectation are developed through which participants can anticipate and respond to specific types of behavioral situations (Douglas, 1966). In this way, certain ritual behaviors may be recognized by the social actors as "our" or "their" custom. Cooperative interactions are maintained according to preestablished, formal procedures. While not rigidly fixed, the behavior of participants is channeled into repeatable patterns of action that serve the needs of their particular kind of interaction (Smith, 1979). Ritual may also serve as a mediator, to ease the problems of socialization which "persuade or coerce each member of the society to accept the group-cognized environment and to follow the behavioral dicta implicit in that reality model" (McManus, 1979, p. 229).

To view science as ritual has several benefits in our discussion of why psi researchers have tried to adopt the methods of natural science, even when they are not totally adequate. At the most fundamental level, Western man has adopted natural science as a successful cosmology for controlling and predicting the physical world. As such, it has profound social value. It is integrated and supported by social, political and economic spheres of Western society. Scientists are regarded with high esteem.

In our attempts to gain access to the scientific community, psi researchers (and social and behavioral scientists in general) have attempted to consolidate the goals, methods and values developed in the natural sciences. The scientific ritual is sought to relieve the socialization pressures of funding, social recognition and acceptance. While these are important factors, it is vital that we recognize the value and limitations of our involvement in the ritual process. As pointed out by McManus et al. (1979), recognition of science as ritual is important because: "the ritual itself may be consciously modified to optimize the recognition of discrepant input and minimize the over defense of models. In other words, the tendency toward premature closure of models and resultant faulty assimilation may be avoided" (p. 354). We must remember that, while the history of science reveals times when methodological weakness has permitted erroneous speculation, there have also been times when the pursuit of scientific rigor has become so intense as to eliminate problems for which existing methods do not apply (MacLeod, 1964).

Thus far, I have tried to show that (a) psi is not replicable in the sense of the natural sciences, (b) that replication does not have the same value for psi research as it does for natural science and (c) that our reasons for adopting natural science methods (i.e., the separation of psi research from occult practices; the genuine belief among researchers that natural science methods are useful, and the need and desire for membership within the scientific community) do not lead us to a greater understanding of psi phenomena.

While I reject the idea that natural science methods are the most fruitful path for gaining control over the psi process, I do not reject the idea that psi may, in fact, be replicable within a transitive framework. Rather, I am suggesting that a phenomenological method may be more appropriate for mapping the levels of consciousness making up the psi process. Phenomenology demands that we let the phenomena shape the method, rather than trying to force the phenomena into preconceived molds. We must be ready to accept the fact that psi is not replicable in the fashion of the natural sciences.

To understand does not guarantee unlimited control over a phenomenon. Perhaps psi is, by nature, nonreplicable.

What I am suggesting is an ecological approach, through which the patterns of psi might better emerge. While it is futile to call for a total abandonment of natural science methods, it is meaningful to assess their value. Phenomenology might best be regarded as a foundation or, perhaps, adjunct to our experimental efforts. We may now illustrate this point by referring to a phenomenological approach, taken within the framework of experimental parapsychology. Here, we have the best of both worlds.

Phenomenology of Psi

To accomplish this goal, we turn to a brief example, taken from an informal remote viewing experiment (Locke and Schlitz, 1982).[2] The aim of this effort is not to discuss remote viewing methodology,[3] but to examine the role phenomenology might play in the psi elicitation process. The focus of the material to follow concerns the interactions between Audry Plaxton, our remote viewer, Dr. Ralph Locke and myself. The interview took place after completion of a single remote viewing trial.

Prior to the session, it was agreed that Audry, at a prearranged time, would relax in a semi-darkened room and attempt to visualize (or otherwise describe) the geographical site which I was visiting. She was aware of the standard remote viewing protocol (see Schlitz and Gruber, 1980) and was shown examples from other experimental sessions. In particular, Audry was shown notations and drawings which were described as typical remote viewing responses. She was encouraged to do the same, making sketches and recording her impressions on cassette tape.

In describing her impressions to the two experimenters, Audry began by reporting her initial experience as visual images which appeared in different degrees of clarity, intensity and recognizability. Following this brief flurry, there was a period in which she reported "seeing" nothing. Following this was a period in which she "knew something was about to happen." She "could not see anything" until suddenly a bright spot of color expanded in the center of her field of vision. She then reported "seeing something burst out of this patch that seemed like a locomotive—just like in one of those old movie sequences." She completed her description by noting that she knew it was not a train, but an antique car. In addition, she described the site as an old farm, complete with log cabin, lanterns and a wood

burning cook stove—which she noticed was cold. In fact, the site had been a log cabin, which was cold because it had been vacant during the previous night. She also described two dogs, one copper color and one black and white. The site was the home of two cats, one copper color and one black and white.

Now, this seems fairly straightforward—except that it is clearly incomplete as a record of Audry's experiences during the experimental period. In gaining a more complete picture of the situation, let us deal with one small part of the reported episode. Audry stated that she saw "nothing" preceding the sighting and description of an antique car. She was then asked what she did see that amounted to "nothing." Her reply, after brief reflection, was that she saw "grayness and flecks of light." She was then asked how she knew that something was about to happen. Again she hesitated, saying that she was uncertain. Asked how she felt, again hesitation. Then how her body had felt at the time. To this she quickly and animatedly reported a feeling of tenseness (hands clenched, legs tense) and feelings like butterflies in her stomach. She made the analogy of taking an exam and the anxious arousal that accompanies it.

What can we say about this experience which gives some flavor of a phenomenological perspective? In the first place, we must recall the deceptively simple dictum, "Don't think, but look"! This translates into a more complex approach to human experience and consciousness which emphasizes perception through the suspension of assumptions. Where the perceptual field is concerned, we are clearly interested in what is focal and what is subsidiary and why. In other words, what kinds of implicit structuring or editing occur which may reveal:

1. Culture specific categories and language habits (descriptive protocols).
2. Context specific categories and language habits contained within a situation—subtended by 1.
3. The composition of the natural attitude which is the basic matrix within which perception and action (including language acts) occur.
4. The idiosyncratic organization of experience which reflects the biography of subjects and their situational adaptation, and the way in which these intersect with the previous three points and the situation being investigated.

In gleaning the meaning of a given phenomenon, the phenomenologist is sensitive to the cultural-environmental context in which it occurs. This ecological perspective implies that a human phenomenon

cannot be abstracted from the context in which it appears and still maintain its original nature. To understand the true phenomenon under study, we must view it as it is rather than as we wish it to be. "Phenomenology deals with human beings on a one-to-one basis. The subject is not thought of as a static personality structure but as a spontaneous interacting individual who influences others and is, in turn, influenced by them" (Koestenbaun, 1966, in Severin, 1973).

In Audry's case, the experiment had been introduced with a pronounced visual orientation. In other words, the implicit assumption negotiated in the experiment was the over-riding importance of information (impressions) gained from a visual mode. As a subject, she was encouraged by the experimenters to attend to the task in a specific fashion. This attitude on the part of the experimenters was based on certain assumptions coming from a cultural, contextual orientation. So, Audry's reportage of focal visual phenomena was partly subtended by the construction of the experimental situation in terms of what counted as "relevant data." Implicitly, she was oriented away from a range of experiences, other than visual, while performing as she believed "an adequate and competent subject" would perform. The datum, taken from the phenomenological point of view, is not the primarily reported visual field content and changes which may or may not match with the actual target site. Rather, it is the from-to relationship which is expressed by the focal visual experience (the vehicle perception) to which the rest of the visual, kinaesthetic and other modalities—in this case expressed symptoms of anxiety—were subsidiary.

Now, this is not to say that the anxiety-state of embodiment caused the visual phenomena or that it was necessary to it—these issues can be dealt with separately—but that: "What must be borne in mind is that the main function of a phenomenological description is to serve as a reliable guide to actual or potential experience of the phenomena. It is in this sense never more than ostensive, or better, directive. Its essential function is to provide unmistakable guideposts to the phenomena themselves" (Speigelberg, 1970).

Phenomenology, then, is a kind of uncovering process. In the case of Audry, the definition of "something significant occurring" included the context of psychophysiological events which were "sign posts" and part of the field in which visual events were the meaningful figure. So, part of the uncovering process is an examination of part-whole relations in language and perception.

In order to complete the phenomenological picture, it would be useful to include all participants in the interviewing process. By

recognizing our own role in the psi process—by exploring it from the first person perspective—we may gain even greater insights into the working of psi. As a Curandero so clearly expressed: "Many of these things you must experience, before you understand them. When you have experienced and understood them, you either will not need to ask questions, or your questions will be the kind that I can answer" (Trotter, et al., 1981, p. 11).

By focusing on the phenomenology of remote viewing, I am not restricting the usefulness of such an approach from other areas of research. REG-PK research, for instance, could benefit from a phenomenological mapping as well. Take Schmidt's work, where the experimenter is his best subject. By utilizing a systematic phenomenological approach, we might better understand why this is so. By making tacit knowledge explicit, we might help others in replicating our results. The success of certain individuals, of certain experimenters, in the bio-PK research, Ganzfeld, forced-choice and other areas, all indicate the potential value of phenomenology in shedding light on the "how" and "why." We need not give up our scientific ritual in order to benefit from a phenomenological attitude. We must simply remember that natural science methods have their limitations. If the essence of psi is our goal, we must expand our horizons, both intellectually and methodologically.

Conclusion

In conclusion, this paper has attempted to illustrate the usefulness of phenomenology in (a) identifying assumptions concerning the "problem of replication" in psi research and (b) gaining a better understanding of the psi process. While offering both a rigorous and disciplined method, phenomenology seeks to understand phenomena as they are—rather than as a product of natural science methods. The emphasis shifts from a quantitative to a qualitative approach. By mapping the levels of consciousness involved in the psi process, the phenomenon itself may provide us with guideposts to more reliable psi.

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NOTES

1. For other examples of replication, and its shortcomings, see Collins, 1981; Pickering, 1980 and Pinch, 1981.

2. A version of this section was presented to the 1982 Society for Psychical Research and Parapsychological Association Centenary-Jubilee Conference, Cambridge, England.

3. For examples of remote viewing methodology, see Schlitz and Gruber, 1980 and Tart, Targ and Puthoff, 1979.

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DISCUSSION

BALLARD: I think it is important to note that among phenomenologists and phenomenological psychologists there is a disagreement as to whether or not the scientific method is adequate. The school that I have been identified with says that we do not have to abandon the scientific method and that where we have gone wrong is that we have confused theory with method as we have done in other areas

of psychology. The problem is not that we have inherited bad methods from natural science, but rather that we have inherited a meta-theoretical baggage that is not germane to the phenomena that we are studying. In other words, to put it in Aristotelean terms, natural science as a theory is essentially efficient and material causality based, whereas for the type of phenomena that we are dealing and the type of phenomena we deal with, for example, in quantum physics, we need formal and final causalities, patterns and intentions. So there is much to be offered by phenomenology and phenomenological approaches, but I think the real benefit that we have to gain in this area is in the theoretical side as opposed to the method.

BELOFF: I listened to your paper, Marilyn, with special interest because of the reputation you have of being an outstandingly successful experimenter and I was hoping that you would be able to pass on to the rest of us something of your secret. I was, therefore, I must confess, very disappointed to find you joining in the chorus of those who want to jettison the methodology of the natural sciences in favor of something that you are calling phenomenology. Now, this term phenomenology is one of these currently okay expressions, but when you really get down to it, it is simply the old method of introspection. I know the counter arguments. Obviously, it is not the kind of introspection that was practiced by Titchener or Wundt. But, nevertheless, it is subjects reporting on their experiences. Something as simple as that. Now, obviously, there are certain phenomena where introspection is of prime importance. For example, if I am studying the nature of imagery I obviously have to rely predominantly on introspections. But, surely, one of the baffling things about the psi process is how much of it is unconscious. When you are trying to question your subject about a process which he or she is unaware of, you are not going to get very far with introspection. It seems to me that the examples that you gave to support your thesis really turn against it. You mention, for example, this remote viewing experiment where you got very full descriptions from your subject Audry about the kinds of imagery she was experiencing. But it seems to me that we wouldn't be particularly interested in her images of antique cars or grey patches which she talks about if we hadn't, by the accepted methods of the natural sciences, established that there was an above chance correspondence between the sort of images she is having and something that is objective, that is out there. For example, Sue Blackmore takes a phenomenological approach to the out-of-body experience and that is perfectly in order once you have put psi behind you and are not therefore pleading that there is some special

process going on that needs explanation. But once you take psi seriously, I can't see that phenomenology is going to get you very far. Now, I go along with you in so far as this paper could be taken as a plea that we ought to understand our subjects when we use them, that we ought to know their attitudes, their beliefs, their past experiences, because all of this is important for whether or not we might be able to get good results from them. But couching it in this phenomenological terminology deriving from Husserl and other people who, I happen to think, are great obfuscators of the truth, I really feel this defeats your aim.

SCHLITZ: You said you had heard my paper and were looking for insights about me and then you went on to say that you don't see the usefulness of introspection. Well, I would say, as a subject and as an experimenter, that there are things which go on that are not explicit unless you look. Within a given experimental framework, because of allocated time, because of the set up we just don't look. I think that if you want to understand how I perform well in a given experimental setting, then I had better look so that I can tell you. There are things that I could tell you about my own experience, personally. Those things come from introspection. Now, phenomenology isn't introspection. For one thing, we are interested in a greater whole. We are interested in generalizing. Secondly, we are interested in ourselves within the process. We are not a detached experimenter, a psychologist, as it were, in introspection, noting the experience of our subjects. What we are doing is looking at ourselves as experimenters, within the psi elicitation process to find out what it is about us, what it is about our instructions to the subject, that either helps him or hinders him in the psi process. I might have an idea in my head, so I tell the participant to look for it. It doesn't help him at all; yet I never realized that that was what I was doing. So in that sense I think that it is vital that we examine psi.

The third thing I want to say, about this objective validation idea, is that what I did in this paper was suggest an interface. Personally, I am an experimentalist, I believe in the experimental enterprise and I am interested in it and it excites me. But I think that we as parapsychologists tend to place our emphasis on the statistical outcome—what was your p value? And we tend not to look at the experience that we had in gaining this result. We certainly don't take time to do that and, more important, we don't take time to do it in a specific way, in any kind of rigorous fashion that might help us to communicate our results to other people.

STANFORD: Well, I would be the last to be opposed to anything

that could help us to enhance the magnitude of our observations or that would allow more frequent occurrence of psi phenomena in the laboratory. I would be surprised if some of the phenomenological approaches you suggest should not be useful. Now, there are things which you said that concern me a good bit, and I have heard at least some reflections of these from other sources. One of them that really gets under my skin a bit is the imputation of the reasons why we are interested in replication and the methods of natural science. Nor, personally, do I like the distinction between natural science and other science. Science is a method. Is there an unnatural science or a perverted science? But I think the reason that we have an allegiance to such science is that it has been eminently successful in so many areas. J. B. Rhine is perhaps a prime example of a person who had an incredible faith in the scientific method, enough to think that it really could tackle psi phenomena. I do not think there is something antagonistic between the use of phenomenological information and other types, such as behavioral information, that we might encounter in an experiment. There is no reason to think that those are intrinsically opposed to each other and that if we have one we just can't have the other. That kind of argument was dismissed long ago on very solid grounds in the history of psychology. We adopt these methods because they have been successful in leading us to positive knowledge and in helping us, partly through the process of attempted replication, to separate the balderdash from the useful data.

You said that we are somehow reducing psi phenomena by experimenting with it in our manipulated studies. I am just very much impressed by some of the things I have seen in some people's experiments—some of the things to come out of Chuck Honorton's lab and William Braud's lab and occasionally my own lab. I don't see that some of the hits we get in our neat, tight experiments are any less impressive than the results that come from Mrs. Leonard in a séance. I think they are on a par in almost any way you want to name. Finally, I don't think we have given the standard methods nearly the chance that we should, to see if they are really going to put psi phenomena on the map scientifically. I don't think we have been nearly rigorous enough, systematic enough in trying to apply those methods.

SCHLITZ: I don't disagree with you in terms of the impressiveness or even the usefulness of the methodology that we are using. I am interested in the Ganzfeld, I am interested in remote viewing methodology; I think that they are useful. But I think that we will get more out of our experimental efforts if we look a little harder at

a broader range of experience. We throw out so much data, it is unbelievable. We get rid of the stream of consciousness that is the subject's response to the experimental situation. What we reduce it to is how well, in a probability of one in four, did it relate. Although maintaining the true phenomenological attitude which I like, I do believe that the methods of natural science have been very fruitful for discussing properties of the physical world. I don't think that experiences in shifts of consciousness can be dealt with quantitatively, at least not now. And, finally, phenomenology is not anti-empirical. What it is is *pre*-empirical. What it says is that, before we try to slice things up, we should look at the whole so that we can then know what kinds of variables to manipulate better.

STANFORD: I like the last remark very much. Indeed, you said something that I intended to say, but didn't. A challenge that your remarks put before us is how indeed we do reduce the rich phenomenology of the subject to something that will lead us into those hypotheses that we can then test in a more standard fashion. Then, perhaps, we can ultimately quantify our data or treat them in a way on which conclusions can be based, rather than hunches and intuitions about the way things are happening.

BLACKMORE: I am very much confused by your use of the term "phenomena," because you talk about things like mapping levels of consciousness which make up the psi elicitation processes. Now, a lot of what you said about phenomenology seems fine when you are talking about levels of consciousness, shifts of consciousness and all the things you mentioned just now. Wonderful. But by the definitions of psi which you used yourself this morning you can't do that kind of thing with it. I don't see that anything that you have said about phenomena or phenomenology is actually addressing the phenomenon of psi. You are addressing other things, such as levels of consciousness, which may or may not be connected with psi. I happen to think they are not. Maybe they are, but you have not shown me that any of the methods that you are using can actually address directly the phenomenon of psi. Now, if you are just talking about phenomenology as a very helpful adjunct, I applaud you absolutely. I think you have given us some very useful directions and that is fine. But I think that distinction needs to be brought up more clearly.

SCHLITZ: Phenomenology and consciousness are loaded terms. Husserl would say that any experience we have is conscious, in that we are individuals experiencing this thing. So whatever psi is, psi may end up being a force like gravity, but we as individuals are experiencing that. I am not saying that psi *is* a force like gravity, but

if it should be it still involves our actions in the experimental process. And so we as experimenters, we as subjects, we as experiential beings have something to gain by looking at these processes.

BLACKMORE: Perhaps I am just being blind here, because, never having had an experience of psi, I was unable to appreciate this point.

HONORTON: It seems to me, Marilyn, that rather than abandoning reductionism what you are calling for is better reductionism, more complete reductionism, because the long laundry list of things that we should be using, the information that we should be getting from subjects has to be reduced to a form that we can deal with. I agree with you completely about the need to get much more detailed information about what is going on experientially and physiologically with the participants, what is going on with the experimenters also, if it is possible to do that. But what you are calling for is not moving away from the standard reductionistic approach, it is broadening it to include a much wider range of information, it seems to me. I want to say just one other thing in terms of the value of replication. I think that the value of replication as a demarcation criterion can vary and we might be able to learn from prior experience in other fields. There are several areas of research in parapsychology now that have been around for a long enough period of time—say ten years or more—so that we ought by this time be able to establish certain minimal reporting standards that would include uniform information, available in all reports that would be acceptable for publication in a given area. If this were done over a period of time, then we would have a lot more information from both the successes and the failures.

SCHLITZ: I agree that it is important to do this for both successes and failures and that the relevance of it isn't just when the responses match the actual target site, but it is very valuable to look at in general. In terms of why I am using that grandiose word "phenomenology" all the time, it is the idea of the suspension of assumptions. In looking at parapsychology we have adopted a methodology that has already been developed, already been established and we have tried to take what we are studying and plug it in. And so we are assuming that these methods are going to be applicable to that phenomenon that we are trying to study and I think that that is a big assumption. It may or may not be true. I think it is just useful that at all times we maintain the idea that psi may not be replicable. It may not. Does that mean it may not be worth studying? You asked what if it winds up being a red herring? There is still the experience

of it. There are still a lot of people, like Susan Blackmore, who are looking at the experiential side of it. There are all kinds of different dimensions and a richness of it that can be looked at.

BRAUD: I think this suspension of assumptions idea is a very useful way of illustrating a case in which the phenomenological method might be useful to us. This also addresses the question Dr. Rao asked earlier about a new method that we might develop. We now look at response protocols in a filtered way. We look specifically for those aspects that happen to match the target in question and we emphasize those aspects of the data. We tend to ignore the rest. We treat it as noise. Now, I think if we were to be true to the phenomenological approach we would value the various parts of the protocol equally. We would, perhaps, pay more attention to bits of that protocol that we would ordinarily ignore. If we weigh it sufficiently highly we might begin to find commonalities across people which have no association at all with the target in question in any obvious way, but have tremendous consistency. We could, in fact, ask the psi process to begin speaking to us in its own voice. I think that that is something that doesn't come as clearly out of non-phenomenological approaches. Along that line, what would the phenomenological method say about searching for commonalities and combining individual reports to try to make some more general statements? Is there a unique method or does it make use of older familiar methods?

SCHLITZ: I think that is absolutely right. The reason that I am interested in phenomenology as a way of looking at the data, is that anybody who has done any kind of free-response experiment or even PK experiments, where you have tried at all to correlate your state of mind or imagery strategy to the experimental situation, you end up with all this material that you don't know what to do with. It's the stuff we throw out because we don't really know what else to do with it. What we need is a systematic way of using this vast body of material—because there is really a lot to it—and finding organization, finding order to it so that in fact we can begin to look for commonality—all the threads that run through the material that we hadn't previously thought of. I think that phenomenologists do have a methodology, so that they can say "This is the method we use" and they can begin to make comparisons with each other.

BRAUD: What are some of these methods? You said that phenomenologists have some unique methods. Can we learn anything from those methods?

SCHLITZ: Well, I tried to illustrate that in terms of the subject-experimenter profile. Now, one way of getting at it is to look at

some of the existing standard psychological data with a new light in mind, in that we are not looking for just the end result, we are looking for information within that. Age, sex, religious beliefs, body concept, mind-body concept, space-time concepts, those are the kinds of matters that a phenomenologist would address and follow up in a systematic way, letting the subject himself lead the interviewing process.

HONORTON: One fairly simple way of doing this in the context of a modern computer RNG type of experiment, would be to have subjects generate their own list of phenomenological descriptors after going through a preliminary series of RNG trials, so that they could make what we used to call state reports. But the state reports would be much more fine-grained and would involve whatever categories seem to be associated with success in the initial practice trials.

BRAUD: We are falling into the same old rut again, because we are looking only at those state report elements that are correlated with success, as traditionally defined. What I am suggesting is that we forget about that for a while and look for emergent consistencies that may or may not have anything to do with the "success" of that experiment.

Let's say we have people work with an RNG. They list their categories, we then see which of those categories correlate with hitting, which with missing—I think that is what you are suggesting? Well, let's forget about that and let's look at what kinds of commonalities exist among the people making the ratings. Let's look at the interesting "misses" that are common to a lot of people and that are connected with the target in question, with the goal of the experiment, in an unusual way. Let's let the data speak to us, rather than using a filter through which we look only at data which bear on the correlation that we are after.

HALL: I am always bothered by this phenomenological approach, which is strange because I spend a great deal of my waking life dealing with dream interpretation and a great deal of my non-waking life dreaming myself. It bothers me because it seems to me absolutely impossible to bracket one's tacit assumptions. Michael Polanyi has made so much sense to me in that regard that it always worries me about just moving from one tacit set of assumptions to another. You didn't seem to me to pick up that very interesting "greyness with flakes in it" prior to an image that was evidential of psi. As William Braud was saying, it doesn't matter if we find what goes with success or what goes with failure as long as we are still finding something.

In that woman's dream material or in her imaginal material, what other kinds of things cohere with that flecked greyness? Could you find other spontaneously arising images which would make sense out of her particular ability to hit or miss?

SCHLITZ: The work done with Audry spanned only a very brief time. To answer your questions would require a longer period of interaction. This practical consideration of time is a good argument for experimenters serving as their own subjects—since there is more opportunity to observe and study the subtle phenomena in question.