ON EXPERIMENTAL PHENOMENOLOGY

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ABSTRACT

Through an analysis of the literature in the field and a discussion of facts, a tentative definition of experimental phenomenology is proposed. Experimental phenomenology is regarded as true experimentation. Its experimental variables are mental contents of direct experience rather than physical stimuli or physiological processes. Two limits of the phenomenological approach are pointed out, namely, the occurrence of mental facts that do not belong to the phenomenal scene (habits, forgetting) and the actual impossibility of distinguishing which aspects of a mental fact, such as percept, play the role of causes and which those of effects. Despite these limits, experimental phenomenology is regarded as the proper method for psychological research.

In these years there is renewed interest in phenomenological psychology and in experimental phenomenology (Bozzi 1989; Thinès 1977; Thinès, Costall, & Butterworth, 1991). In a sense, the phenomenological point of view has never been effaced from psychology, especially in those areas—such as personality, motivation, or psychodynamics—where no other research method is capable of depicting a sensible and consistent map of psychological facts. What is new, however, is that in the circle of hard experimental supporters a creeping reassessment of phenomenological aspects and of their scientific use has begun.

PHENOMENOLOGY

The term "phenomenology" was coined by Johann Heinrich Lambert in 1764, and the corresponding concept has been developed by Kant, Hegel, Husserl, and Merleau-Ponty—just to mention the main philosophers who contributed to our present way of intending this term. In the words of Husserl, the slogan of phenomenology is: "Zurück zu den Sachen selbst!" ("Back to the things themselves!"). Translated into the psychological "dialect," this means that if we want to understand mental facts we must cease "wasting time" in "daydreaming" about neurophysiological or informational models, and rather face the contents of immediate experience. Husserl was not only a philosopher, he also practiced scientific psychology (with Brentano), at least until he convinced himself that it could not help him investigate the concept of number.

In psychology, the meaning of "phenomenology" has been discussed by Gestalt psychologists, for example in the third chapter of Wolfgang Köhler's (1947) *Gestalt Psychology*. In his 1935 *Principles of Gestalt Psychology*, Kurt Koffka says: "For us phenomenology means as naïve and full a description of direct experience as possible" (p. 73). In his 1963 *Psychologie*, Wolfgang Metzger is even more eloquent when trying to define the phenomenological attitude:

> To simply accept the facing thing as it is, even if it appears unusual, unexpected, illogical or senseless, and even if it goes against undoubted axioms or familiar ways of thinking. To let the thing speak for its own, without indulging in what we know, or we previously learned, or in what is obvious, in the knowledge of subject, in logical demands, in linguistic prejudices, or in the insufficiency of our vocabulary. To stand before the thing with reverence and love, if anything reserving our doubt and mistrust for the premises and concepts we so far used to understand the world of data (p. 12).

WHY PHENOMENOLOGY?

The admonition to use the phenomenological attitude in considering mental facts must not be undervalued. The developing of neurophysiological, informational, or mathematical models, although commendable for their contribution to the progress of science and necessary for their help in the treatment of cognitive diseases, progressively distances us from the facts of immediate experience, which are the proper subject matter of the psychologist. The result is that our arguments are centered on methods

and models, while the factual basis tends to be forgotten, so that at the end we have some difficulty in saying what we are speaking of.

As an example, let us briefly consider psychophysical research in the field of optical-geometrical illusions. I think that very few research subjects in psychology enjoyed so sophisticated methodological techniques for data collection and so clever explanatory models[1] as those used in research on illusions. In spite of this huge amount of work, which began at least 130 years ago, we have not even come close to the explanation or the *understanding* of any illusion. I had the direct experience of this fact in a recent research on an unknown illusion by Delboeuf (1865; Vicario, Vidotto, & Zambianchi, 1992) when, after having psychophysically explored the conditions of the illusory effect, and having tried to link the illusion to some better known effect and related explanatory model, I found nothing best than to propose again Delboeuf's description and to resort to the figure-ground phenomenon for the explanation—this phenomenon being an "explanatory tool" not less puzzling than the illusion itself.

As another example, consider the fine structure of psychological time. A long list of phenomena, encompassing the so-called temporal acuity, the brightness of very brief flashes, reaction times, perceptual thresholds of succession and of the order in a succession, tapping rhythms, and so on, led theoreticians to speak in favor of a quantization of psychological time, leaving undetermined just the duration of the quanta (Stroud, 1955, Kristofferson, 1980). Several models of internal clocks or cyclic timing processes have been proposed (see Patterson's, 1990, for a brief review). However, apart from the fact that models so far proposed fit only a small part of the overall findings, the evidence is that there is a clear contradiction between the asserted discrete nature of psychological time and the smoothness of the experience of becoming—that is, of the transition from the future to the present and from the present to the past. In my opinion, the contradiction arises from the confusion between *physical time of physiological processes* and *psychological time*. The hypothesized underlying processes may undoubtedly be cyclic, but their period cannot be regarded as a "perceptual moment:" they have no phenomenal counterpart. The only way to avoid the contradiction is to turn back to immediate experience and to put it under the light of phenomenological analysis. We will then discover that, concerning the fine structure of psychological time, we are still at the point of the unsurpassed statement of James (1890, p. 609) about the "specious present" as a saddleback from where we look

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[1] Field models. Diffraction, retinal induction, lateral inhibition, filter, cortical satiation, and ocular-movement physiological models. Assimilation, confusion, inappropriate constancy scaling, multifactorial, and developmental cognitive models.

in two directions into time, which is a masterpiece of phenomenological analysis. The same can be said for the "traveling moment hypothesis" due to Allport (1968), who refers to the actual experience of looking to the landscape from the window of a moving train. In a sense, phenomenological analysis is more sensible and "explanatory" than a sophisticated psychophysical measurement.

Another source of subjective discomfort is the mechanism itself of psychophysical research. Each time we "measure sensations" by means of the classical indirect methods we face the fact that we come to know a lot of things about peripheral transducers and nothing about sensations. For example, to ascertain that there are auditory events only when the frequency of the pressure wave is between 20 Hz and 20 kHz tells us very much about the ear but nothing about sounds. If by any chance we had found that sensitivity of the ear is between 30 Hz and 30 kHz, this finding would not have any consequence on our auditory experience: the reasons of tonal qualities are not in the values actually found, while the actual values find their functional explanation in the biological ecosystem. In other words, every psychophysical function—whatever its beauty and precision—is degraded to a mere recipe of stimuli necessary for eliciting a certain sensation and, far from *explaining* anything, tells us nothing about the experienced qualities of this sensation. I see the reason of this in the fact that stimulus dimensions (for acoustics: frequency, amplitude, wave form, and envelope) do not have a biunique correspondence with the dimensions of sensation (for audition: pitch, loudness, timbre, attack, but even volume, brightness, density, consonance, and so on).

Very close to the aforesaid complaint is the recognition that each time we perform experiments where the free observation of some perceptual phenomenon is limited by physical constraints that exclude parasite effects, we come to know many things about these effects and almost nothing about the phenomenon under study. Consider for example those researches in vision where the head or the chin of the subject are immobilized by an appropriate device. Any difference between the results we obtain by means of this procedure, and the results we obtain by means of free observation, far from enlightening the content of visual experience, illustrate the weight of anomalous conditions in the building up of the phenomenon, namely, the undoubted importance of proprioceptive reafferences. One can find another example in dichotic listening: apart from the fact that this procedure may give raise to phenomena that have no counterpart in everyday experience, the point is that their recognition turns into a better knowledge of the peripheral treatment of acoustic stimulus in the nervous system, not into a deeper understanding of auditory events. In addition, let us consider the fact that knowledge of loci and ways of neurological

treatment of acoustic stimuli do not improve our intelligence of tonal qualities: had we found that the VIII nerve is addressed to a cortical area different from the one we know, our tonal experience would not have a different quality. Obviously, the intention of depriving these investigations of any scientific meaning is very far from me. Instead, what I desolately feel is that any intervention on the physical and physiological sides of perceptual phenomena is useless since it does not contribute to a better knowledge of the phenomena themselves.

To sum up, phenomenology is necessary: as we use physical tools to test physical phenomena so we must use phenomenological tools in order to investigate experienced phenomena. I reassert that psychophysics and psychophysiology are commendable and in some cases even necessary, but I maintain that they are not true psychologies. If the job of the psychologist is to explain mental facts, we should not "immolate" them to physiological or mathematical models, or force them into experimental paradigms that are foreign to their nature. The reason is quite simple: we shall never grasp the very nature of mental facts in examining their physical and physiological counterparts. If we do so, we fall in the error called "violation of the rules of categorical analysis" by Lorenz (1973), that is, the explanation of facts at a certain level of complexity (e.g., mental facts) with facts at a lower level of complexity (e.g., physical and physiological facts).

SOME HISTORICAL FACTS

Many contemporary students believe that the phenomenological method is a rather questionable innovation brought into psychology by the so-called "Berlin school" (Wertheimer, Köhler, and Koffka) and carried out by those who refer to the *Gestalttheorie*.

Things are otherwise. As anyone knows, psychology as a science was born in Germany, and its establishment is commonly attributed to Wilhelm Wundt, who made two choices: one in favor of explaining mental facts in terms of the physiology of the central nervous system, and the other in favor of the experimental method (Boring, 1950, ch. 17; see also Thinès et al., 1991). Many of our colleagues are yet inclined to forget that at almost the same time a parallel movement was initiated in Germany by Franz Brentano. He made two other choices: one in favor of the autonomy of mental facts (that is, of their irreducibility to physical stimuli or to related physiological processes), the other in favor of the "empirical" method of *demonstration* (we will consider it later).

Everyone knows how things developed. Wundt's choices progressively won. For example, Titchener and Hall exported them in the USA, where they persisted even in the form of behaviorism and cognitivism; Kiesov and Gemelli imposed them in Italy (Turin and Milan, respectively). On the contrary, Brentano's ideas were accepted by a minority. They were developed only by Husserl (in philosophy) and by Stumpf and Meinong (in psychology). Among the pupils of Stumpf were Köhler and Koffka (and thereafter the Berlin school and the *Gestalttheorie*); among the pupils of Meinong was Benussi (and thereafter Musatti, Metelli, and Kanizsa).

This shows that the phenomenological point of view is not a foreign body in the trunk of scientific psychology, thrust in it at a certain stage of its development by the fancy of some successful researchers; rather it is a way of thinking and a methodological choice that were active from the beginning of the psychological enterprise.

This clarification may perhaps help to realize that the current way for explaining mental states and behavior only in terms of processes in the nervous system is not an unquestionable truth, but just one of the two historically grounded ways for the solution of the problem of psychism (alias, the mind/body problem). In other words, underneath every follower of Wundt you will probably find a philosophical monist, and underneath every follower of Brentano you will probably find a philosophical dualist. Whether you chose the Wundt's or the Brentano's side is not a matter of science, but of a philosophical conviction. A similar argument could be upheld for the experimental method in psychological research: its adoption is a matter of choice, not a must.

EXPERIMENTATION AND DEMONSTRATION

I will now point out a singular aspect of phenomenological inquiry that hardly can be traced back to the experimental paradigm. In so doing I will raise the suspicion that the phenomenological method cannot be labeled as "experimental" and "scientific." This singular aspect of phenomenology is briefly summarized in what Gaetano Kanizsa used to say in his lectures, namely, that he performed his own experiments on the pages of his books. I refer to those pictures that "prove" or "disprove" under the eyes of the reader certain hypotheses on perceptual mechanisms.

In my opinion, a big problem is concealed under the paradoxical tenet of Kanizsa. The canonical experimental procedure—with the independent and dependent variables, the control group, the statistical analysis

of results, and so on—is foreign to phenomenological inquiry. In fact, Boring (1950) wrote:

> Thus Brentano, in argumenting about the optical illusions, was quite ready to draw new forms of old illusions and so pictorially to submit his case on the printed page to the experience of the reader: this is the empirical method in concrete form, the *experimentum crucis*. But Brentano never undertook to measure the amount of illusions under different conditions by the psychophysical methods; this course would have been the experimental method and would have yielded more precise information about the points in question. The *experimentum crucis* belongs in an argument and is thus apt to be part of the empirical method. Systematic experimentation yields precise description and is the *sine qua non* of the experimental method (p. 360).

A few lines before, Boring annotated: "Brentano had respect for the results of experiment, but he believed that all this stressing of experimentation led to an overemphasis upon method and blindness for the main issue."

The conclusion of Boring is therefore that Brentano's psychology was empirical but not experimental. We had to follow this conclusion in saying that the phenomenological method is undoubtedly empirical, but that it cannot be credited with the attribute of "experimental."

At this point, it is perhaps useful to report an enlightening comparison carried out by Pomerantz and Kubovy (1986) among the four main theories of perception: structuralism, Helmholtzean (including cognitivist and neuropsychological theories), Gestalt, and Gibsonean. They say correctly that while the method of structuralists is *introspection* and that of Helmholtzeans *experiment*, the method of Gestalt psychologists (that is, of phenomenologists) is *demonstration*. Pomerantz and Kubovy (1986) say that

> In [the phenomenological] method the observer... [is] asked to view a stimulus and to describe its apparent organization. These stimulus patterns... [are] designed so that, in principle, a number of different and distinct organizations were possible... To the extent that different observers agree on the organization they report perceiving, we have evidence for rules of perceptual organization, rules that are claimed to produce the simplest possible organization of the stimulus (p. 36-39).

In other words, one first notes the multiplicity of the logically possible perceptual results (this is the so-called *plurivocity* of the stimulus; Metzger, 1963), and then "demonstrates" that just one of them comes into reality, thus validating the argument about the processes of perception, of which the empirical observation is an integrating part.

EXPERIMENTAL PHENOMENOLOGY

In spite of the many and manifest discrepancies between what we call "experimental" and what we call "phenomenological," in the realm of psychology there is place even for such a thing as "experimental phenomenology." What does it mean?

The above mentioned Carl Stumpf is credited to have started the discipline (Boring, 1950, p. 369), because of the way he treated auditory sensations in his celebrated *Tonpsychologie* (1890). According to Thinès (1977, pp. 69 and 135), his experimental work deserves being called "phenomenological" in that for Stumpf the description of tonal qualities was more important than the refinement of laboratory techniques. In line with Stumpf is David Katz who ameliorated our knowledge about color and active touch by means of true experiments suggested by penetrating analyses of the mode of appearance of perceptual objects (Katz, 1911, 1925). Yet, experimental phenomenology gained a recognized international status only after the work of Albert Michotte (1946), who was able to treat experimentally some perceptions of high complexity, such as the impressions of "launch" and "transport" in the interaction of movements of small objects. Indeed, a masterpiece of phenomenological cleverness are his investigations in phenomenal permanence. As to Gestalt psychology, we can say that each representative of this school showed a similar ability in joining observational refinement with experimental rigor. Leaving out the founders—Wertheimer, Koffka, and Köhler—we face famous works like the *Visuelle wahrgenommene Figuren* by Edgar Rubin (1911), or that monument to the science of vision represented by Wolfgang Metzger's (1975) book *Gesetze des Sehens*, where the phenomenological attitude bears its best fruit: the discovery of new perceived things.

Yet we lack a comfortable definition of experimental phenomenology, notwithstanding a superabundant literature full of subtle discussions. Perhaps phenomenologists think that their work—like every other object—is speaking by itself. Neither Michotte (maybe with Metzger the most philosophically oriented of the phenomenologists) defines his method by means of structural terms. If I correctly interpret the point of his pupil Thinès (1977, p. 139), Michotte found no conflict in applying experimental methods to phenomenal experience simply because he thought that, in inspecting the stimulus situation, the observer casts a look into reality—a sort of Gibsonean attitude *ante litteram*. What most resembles a definition is, in my opinion, the following series of statements by Kanizsa (1984):

> The aim of experimental phenomenology in vision is not different from that of other research fields of psychology: the discovery and the analysis of necessary functional connections among visual phenomena, detecting the conditions that favor or prevent their appearance and their degree of evidence; in other words, establishing the laws that govern the phenomenal field. All this without going out of the phenomenal domain, that is, without any reference to the underlying neurophysiological processes (unknown, for the most part) or to psychic non-visual concurrent activities (logic, memory, emotional, and so on, which are no less enigmatic than vision)... The experimental phenomenology of vision does not deal with the brain but with seeing which is the result of the activity of our brain. It is not a makeshift choice, justified by the too slow progress of neurosciences and related uncertainty of prospects: it is a methodological option imposed by definite epistemological reasons. Mainly, by the firm belief that phenomenal reality cannot be confronted—and less than ever explained—by a neuroreductionist approach, since we deal with a level of reality having its own peculiarity, a reality that demands and legitimates a kind of analysis adequate to its peculiarities (pp. 38-39).

I agree with this definition. So I conclude that *"to practice experimental phenomenology"* stands for *"to manipulate phenomena."* In other words, the experimental phenomenologist, when trying to understand his objects of inquiry, does not merely act by means of systematic, blind manipulation of the stimuli but is satisfied with what he observes at the level of phenomena. The independent and dependent variables to take into account are those of the phenomena and not those of the stimuli that produce the phenomena. When one wants to fix the conditions that lead to a mental fact—e.g., a perception—one has to forbear from the bare analysis of the stimulus situation and from a blind variation of the stimulus parameters. Instead, one has to proceed to a careful inspection of the phenomenon, to discover its peculiar dimensions by means of the phenomenological analysis, and to manipulate only those stimuli that produce variations in the chosen dimensions of the phenomenon. On these dimensions one has to act, and the fact that one can manipulate phenomena only through the manipulation of physical stimuli is in principle irrelevant for the understanding of these same phenomena.

No example of this procedure is better than the famous Wertheimer's (1923) investigations of the mutual segregation of objects in the visual field—those that led to the "principles of unification." In considering the conditions for the formation of perceptual units out of the sensory mosaic, he moved black dots across a white surface, not the ink heaplets on that cellulose support which is the sheet of paper. Wertheimer's *proximity* has nothing to do with the millimeters of physical space that separates the ink heaplets on the sheet of paper (or with the excited receptive fields on the retina), but is a relational property that *we see* between a

couple of them. When Wertheimer moved dots on a paper to see what happened, he moved *phenomenal dots* in *phenomenal space*, not ink heaplets across a cellulose support.

We can perhaps find a more convincing argument in the auditory field. There we speak of tones, noises, and phones as they were "stimuli," and we are unaware that they are true *phenomena* which come out from the processing of a physical signal, continuous and unidimensional in amplitude, which already underwent a transformation into perceptual events. For example, in the acoustic stimulus that gives raise to the perception of a major chord, tones do not exist at all: there is just a pressure wave characterized by a rather complex course in time. The tone that gives the chord its peculiar appearance or feeling (that is, the tone that detaches itself by a major third from the lowest tone, the "fundamental") does not exist in the pressure wave. It is the outcome of a process that has been done that time, and that has already discriminated in total wave—in presenting them on the phenomenal scene—a fundamental tone, a tone at the major third and another tone at the fifth. When we say that, in substituting the major third interval with a minor third interval, we modify the appearance of the chord (that from major becomes minor), we do not refer to something that is present in the stimulus (the pressure wave), but to something that is already present at the phenomenal level (the middle tone of the chord). Therefore, when we shift the middle tone downward by a semitone, we do not act on stimuli but on phenomena: we do not go in for psychoacoustics, but for experimental phenomenology of audition. In specifying the values of the signal for each instant of the pressure variation, we do not care for the waveform: our play is *object oriented*, in the sense that we operate on an object (the middle tone) that is already the outcome of processes exerted by the perceptual system on that acoustic wave. As Kanizsa (1984) says, these processes are the job of the neurophysiologist, not of the psychologist.

To sum up, the expression "experimental phenomenology" is justified because it refers to a manipulation of variables like in any other domain of natural sciences. The distinguishing difference is in the nature of variables: in experimental phenomenology these variables are mental, not physical.

ONE LIMIT OF EXPERIMENTAL PHENOMENOLOGY

As is well known, the main justification for the use of experimental phenomenology in psychological inquiry is that experienced facts have to be explained only in terms of other experienced facts. This axiom has its

basis on the famous paper by Köhler (1913) against the *unbemerkte Empfindungen* (unnoticed sensations), in which he demolished—or tried to demolish—the Helmholtzean hypothesis of the machinery in the brain. The existence of unconscious ratiomorphic processes, on which cognitivists—the late epigones of Helmholtz—base their "fortunes," has been repeatedly challenged by means of various arguments.

The first is that if the conscious reasoning or computing had to be the model of ratiomorphic processes, we would be hopelessly lost. In fact, consider the many faults (inaccuracies, oversights, paralogisms, and so on) of actual thinking (Bozzi, 1989). The second argument has already been mentioned in this essay: if we explain mental facts by means of supposed underlying processes (in terms of neurophysiological events or computational or mathematical models), we shift toward a lower level of explanation, violating the law that imposes every fact to be analyzed at its proper level of systemic complexity (Lorenz, 1973). The third is that this way of arguing justifies whatever fancy—if only ingenious and expressed in formal terms—provided that the supposed lower-level mechanisms are saved from any control, and that to each mental fact may correspond a countless number of equally plausible underlying mechanisms (see Piccinini, 1993, and Uttal's, 1990, discussion of the second Moore theorem). In a way, this argument reminds us of the strong behavioristic attack to the fancies of introspectionism, and I think that cognitivists have been no less imaginative than introspectionists in transforming their subjective experience in hypothesized underlying mechanisms (for example, see Neisser, 1964, on visual search). The fourth argument is that every sort of brain machinery requiring measurable quantities of physical time is in contradiction with the everyday experience: when we open our eyes, for example, we see an extremely complex visual scene that is suddenly given with no delay at all.

If these four arguments are well grounded and crucial, one has to conclude that psychology cannot escape from phenomenologically describable facts, that its sole proper method should be experimental phenomenology, and that every assumption on the machinery of the brain, although fruitful for the study of the central nervous system and necessary in the treatment of cognitive diseases, would be devoid of any true psychological meaning. Unfortunately, this conclusion turns out to be partially misleading: reality is more complex than our expectations. Once again, we must credit Köhler for having made the point with an extreme lucidity and disarming frankness.

In his paper on the mind/body problem, Köhler (1960) maintains that the psychologist cannot limit himself to the study of the "phenomenal scene:" he is concerned also with facts that are undoubtedly "mental" but

at the same time are not functional parts of the phenomenal world. Köhler refers to memory, and points out that while memory retrieval has phenomenal evidence, the retention or memory loss have no phenomenal counterpart. The same can be said about habits, and about motives and emotions: we are aware of their phenomenal and behavioral effects but we cannot inspect their causes or their mechanisms. In a sense, Köhler restated in a better way something that was well known: in 1901 Marbe was amazed by the fact that while a subject is perfectly able to judge which of two compared objects is heavier, and to report a lot of sensations and associations accompanying the comparison task, the very act of judgment is saved from any form of introspection (this is the so-called *imageless thought*, a field of researches of the Würzburg school).

Now, since we cannot deny that memory, habits, motives, and so on, pertain to psychology, and since all these facts are inaccessible to the phenomenological method, we have to conclude that this method cannot cover all psychological objects of study. Because of this, it cannot be the only method of psychological inquiry.

This unavoidable conclusion is especially unpleasant for those who firmly trusts that only the phenomenological method can deal with the psychism—i.e., the immediate experience—and that the psychological research based on neuropsychological processes turns into a more and more precise knowledge of the machinery in the brain, leaving at all unsolved the problem of the direct experience itself. Only a naive identification of mental facts with the corresponding brain processes can carelessly take these facts at one level for a way of describing those at the other level. This jump would never be made by a philosophically shrewd researcher.

Yet we must surrender to reality, that is, to the antinomy in the conclusion that neither phenomenological nor neurophysiological descriptions can cover the totality of psychological facts, and that there is no way to put them at the same level. The Eccles (1990) attempt to bridge the gap between the opposite banks of mental life is ingenious, but fated to displease both phenomenologists and neurophysiologists. In any case, the phenomenological method has at least the aforesaid limit.

In my opinion, if we want at all costs find a coherent frame of reference for immediate experience and neurophysiological research, we can resort to Jackendoff's (1987) theorization. He makes a distinction between the *phenomenological mind* and the *computational mind*: the first should be characterized by consciousness, the second by those processes that, although unobservable, could have effects on phenomenally observable mental contents. According to Jackendoff, the phenomenological mind should be a *projection* of a subset of the computational mind, something like a light spot on a scene for the rest plunged in the darkness. I am

aware that Jackendoff's effort leaves unsolved the problem of why a subset of the computational mind should come into the light of the phenomenological mind. Yet I find that this theorization can at least legitimize the duplicity of psychological research: phenomenological analysis for conscious contents, neurological experimentation for silent processes.

ANOTHER LIMIT OF EXPERIMENTAL PHENOMENOLOGY

As I said before, in working with the method of experimental phenomenology, we use the axiom that phenomenal facts have to be explained only with other phenomenal facts. This means that there are facts that assume the role of causes and others that of effects. In fact, this is the practical significance of the term "explanation" in every natural science, from mechanics to biology. Unfortunately, this is not the case in psychology. We are taught by experience that, whenever in a global phenomenon we try to identify those aspects that could take up the role of causes and those other aspects that could take up the role of effects, we enter a vicious circle: we can no longer distinguish what comes first.

For example, let us consider the case of perceptual transparency (Kanizsa, 1955; Metelli, 1974). In order to see a surface as transparent, several conditions must be met. Some are physical—relative reflectances of the areas in the display—and some figural—continuity of contours, topological relations among surfaces, and so on. Well, when figural conditions are manipulated to distinguish basic from accessory conditions, we conclude that a surface is seen as transparent only when object stratification takes place. But this stratification is at work only when an object is seen as transparent. In the same way, transparency is the feature of a surface that, even if made up of different regions, is perceived as unitary. But the possibility to see multiple adjacent regions as an unitary surface is grounded on the possibility to see this surface as transparent, entirely or in part.

I have the impression that this state of affairs is the same for every perceptual phenomenon, although only phenomenological psychologists have stressed it. The amodal completion of an object (Burke, 1952; Kanizsa, 1980; Michotte & Burke, 1951) is realized just when a part of the visual field is perceived as a screen (occlusion), but that part of the visual field is seen as a screen just when there is an object to complete. An object can be seen as big only if it is perceived as far, but it can be seen as far only when it is perceived as big; a movement can be perceived as "passive" just only if in the field there is another movement that takes upon the "active" feature. In stroboscopic motion, the displacement of the first

light occurs as soon as the second light occurs in the field, so we are led to the conclusion that the member of the kinetic structure that comes after is in some sense the "cause" of the displacement of the first. What is puzzling in this case is that we see the first light in motion before we see the point of arrival—so that we have a phenomenal before/after that is not congruent with the physical before/after. (For other examples of this sort see Vicario, 1989).

In conclusion, at least as regards perceptual phenomena, we have some difficulty in identifying causes and effects, especially because we cannot establish the temporal sequence that characterizes the status of the elements in a configuration. It goes without saying that the differentiation before/after is the cue for the distinction cause/effect. In static displays, all elements are present simultaneously, so that they can mutually exchange the roles of cause and effect; in kinetic displays the physical temporal order is reversed in the phenomenal datum.

Now, I do not know whether we can call "experimental" a method that involves so great a confusion about the things to which we should univocally apply the concepts of cause and effect. In manipulating a variable, we do not know what we are really doing. In this sense, I see a limit in labeling phenomenology as "experimental."

In fact, I fear that the effort of some of us to make phenomenology experimental, is influenced by some paradigms—like mechanics or chemistry—that are by no means representative of all natural sciences. Bozzi (1989, p. 39) stressed that even natural sciences deal with field phenomena (e.g., magnetic phenomena), whose final configuration is determined by the simultaneous presence of multiple events to which it is impossible to ascribe the role of a cause or effect—because of their simultaneity. In my opinion, we can apply the paradigm of mechanics in very few natural sciences, since biological and chaotic phenomena suffer the same vicious circle that we have in perceptual phenomena. Academic psychology seems bounded to a representation of the physical and biological worlds that is no longer in use in these sciences: the simple reading of the popular book by Nicolis and Prigogine (1987) can give us the warning. To be sure, Köhler (1947) had an unending roll of physical, chemical, and biological phenomena, in order to justify his own refutation of any "mechanical" theory of perception. In his proposal of a "dynamic" theory, his examples were drawn from electrostatics and fluid dynamics. Even in chemistry, the so-called Belousov-Zhabotinski reaction shows astonishing selforganizing properties, just like those properties that some psychologists deny to perceptual phenomena.

CONCLUDING REMARKS

To sum up, I think that psychological research cannot avoid using the phenomenological method, since descriptions or explanations of direct experience in terms of physical stimuli or of physiological processes turn out to be, in the long run, better restatements of the physical or physiological conditions of direct experience, never a sensible explanation of this experience itself. If psychologists' job is to investigate mental facts, let them manipulate what appears on the phenomenal scene, and not what is under or behind it. I agree that there are phenomena—like habits or forgetting—that challenge phenomenological procedures, but the adoption of the phenomenological method at least avoids the grave mistake of confounding direct experience with physical stimuli or with physiological processes (*stimulus and experience errors*).

I think that experimental phenomenology is a sensible research approach, even if the proper tool of the phenomenological method seems to be demonstration instead of experimentation. After all, there are sciences that are not experimental: for example, astronomy or geology. What makes the difference between the classic experimental and phenomenological models, is that in the former we manipulate physical variables and in the latter phenomenal variables. Sure, mental phenomena exhibit the uncomfortable peculiarity of being constituted of parts to which we cannot apply with certainty the labels of causes and of effects. However, today physicists and chemists are grappling with phenomena of the same sort without being embarrassed: they make a virtue of necessity.

Perhaps, the reader will find a certain theoretical laxism in my arguments, in the sense that I make the point but I do not exclude alternative solutions to the posed problems. In this way, I am a true follower of the phenomenological attitude: we face a complex and puzzling reality, and we cannot force it in our logic categories. If mental facts exhibit properties irreducible to those we are acquainted with, we must use proper methods of investigation instead of exclusively using those from the other sciences.

DISCUSSION

Giorgio Vallortigara (*Institute of Philosophy, Pedagogics, and Teaching of Modern Languages, University of Udine, Udine, Italy*) and **Mario Zanforlin** (*Department of General Psychology, University of Padua, Padua, Italy*): We believe that science (all science) arises not from *a priori* definitions of its objects, but rather from problems. Let us therefore

consider an example of the sort of problems facing experimental phenomenology.

Place a pile of coins on a table so that the perceived height of the pile corresponds to the size (diameter) of a single coin. Then, put another coin upright and compare it with the height of the pile. The pile will appear lower than the coin. We are thus faced with a problem: the height of the pile is first the same and then not the same as the diameter of the coin. Are we to say that the correct measurement of the diameter of the coin is given by the height of the pile or by the height of the vertical coin? Which of the two appearances is "illusory?"

Faced with these phenomena, the experimental phenomenologist, like any other scientist, will try to check whether a general law of nature has been found. Indeed, this can easily be demonstrated in that there seems to be a relation of functional dependence between two phenomenal variables which is always valid for human perceivers, that is, perceived length depends on perceived orientation. In principle we could be satisfied with this state of affairs. We have found a law of nature, we have described it. However, scientists are usually not entirely satisfied with this. They want to know "why" things are as they are.

At this point we disagree with Vicario's proposals because, in our opinion, there is no way in which this "why" question can be answered without resorting to *logical constructs* that are not (and cannot be) part of our phenomenal experience.

It is obvious to us that the answer to the "why" question resides in the perceptual system, in its make up, in the way it works, and in the purpose for which it has developed in the course of biological evolution. Of course, the notion of "perceptual system," and for that matter even the notion of "biological evolution," are "mental constructs" or "logical inventions," not experienced phenomena in themselves.

Do we need constructs which are outside our phenomenal experience in order to explain phenomenal experiences? We believe that the answer is definitively positive. The alternative view, championed by Vicario, who claims that experienced facts must be explained only in terms of other experienced facts appears untenable to us. For, if all these facts were phenomenally experienced, that is, if we were consciously aware of them, we would not seek an explanation. We would be already conscious of the explanation because, by definition, this explanation is part of our phenomenal experience. There would simple be no need for academic psychology, because people would experience the "why" of their phenomenal experiences; they would be aware of the answers.

One could maintain that the explanation of experienced facts lies in the relationship between experienced facts. However, is this "relationship" directly experienced? In other words, do we *directly perceive* that perceived length depends on perceived orientation, or do we need to use "standardized" measures to *deduce* it? It is clear that we simply experience the *results* of this functional relationship, and that we experience these results as a puzzle, as a problem. Furthermore, the relationships between phenomenal variables are exactly what we want to explain, and it is therefore logically absurd to maintain that they could be explanations. Discovering a functional relationship between phenomena is the first step in doing science. The next step is to ask *why* such a relationship exists.

Vicario seems to be disappointed because in our mental life there are facts (actually, the majority of facts) that are not part of our phenomenal world. Things are indeed even worse than Vicario would admit: we are never consciously aware of why our phenomenal experience is as it is. It does not really matter if we speak of memory instead of perception. When we perceive a red triangle, there is no phenomenal experience of why we see a red triangle instead of a green circle, except the naive attitude... "because *there is* a red triangle *there*."

If we take Vicario's proposal seriously, contemplation seems to be the only activity left for "pure" phenomenologists. We disagree. We believe that in perceptual science, as well as in any other realm of psychology, we need *hypotheses* and *models*. This is not a peculiarity of psychology. The same is true for all natural sciences. The concept of "force" in physics is just as unexperienced as that of "anisotropy of the visual field" in psychology. What we experience are the results, the *phenomenally testable* effects of these hypothetical entities.

Experimental phenomenology. We claim that the "phenomenological attitude" is not an alternative to developing explanations based on models of underlying processes or "silent processes," but it is simply the initial and mandatory starting point for any scientific enterprise. A naive and exhaustive as possible *description* of the phenomena is in fact a first step common to all sciences.

In this chapter, Vicario sets the method of experimentation against that of demonstration. We think that this dichotomy is only apparent. Experimental phenomenologists also perform experiments. They simply do so *after* an initial description of the phenomena. Usually, phenomenologists start with a description of a certain phenomenal experience, and then modify it systematically (through manipulations of the *stimulus conditions*) until a certain phenomenon is apparent with the greatest vividness. Then, further manipulation of the stimulus conditions are used to test hypotheses about the why things appear as they do.

It is really true that phenomenologists do not employ the traditional parametric methods of experimentation? Obviously not. For, if we want to

use a visual display as a sort of "rhetorical trick" to sustain our point, we have first of all to check for the *better* conditions producing the phenomenon. Thus, experiments by Gestalt psychologists seem to be mere demonstrations without canonical parametric studies only because these parametric studies have been performed *before* the presentation of the final "case in point."

Admittedly, there are two important differences with respect to other psychological traditions, such as behaviorism or cognitivism. First, the latter traditions have tended to neglect the initial phase of the description of the phenomena—this has in fact been Konrad Lorenz's major complaint about the work of behaviorists in animal behavior (Lorenz, 1965). Second, typical cognitive experiments yield their results *indirectly*. That is, instead of describing and measuring the direct experiences under investigation, they take some indirect measures (e.g., reaction times) as "indices" that the direct experience has occurred. We think that the phenomenological attitude is preferable, particularly in the preliminary phases of a psychological research because, as Vicario recognizes, it is better for discovery of new perceived things. This is probably because phenomenological observations are not (or only to a limited extent) constrained into a theory that has been built up *before* looking at the phenomena and which may have to disregard some aspects of the phenomenon because they are "irrelevant" with respect to that particular theory.

Direct experience, logical constructs, and levels of explanation. Vicario asserts that if we explain mental facts in terms of supposed underlying processes we shift toward a lower level of explanation, violating the law that every fact should be analyzed at its proper level of complexity (Lorenz, 1973). There seems to be some confusion here.

Sensory experiences are the origin of all factual knowledge. They are the starting point of any scientific enterprise. We have two languages, that of sensory experiences and that of scientific constructs. Scientific constructs are not part of our phenomenal experience. However, they can be verified or falsified by translating them as logical tenets of the type "if... then" in the language of phenomenal experiences

If, as Vicario claims, direct experience must be explained in the language of sensory experience without any sort of logical constructs, then it follows that all sciences are performing a sort of "categorical mistake" (admittedly, with the only exception of the empirical phenomenology proposed by Vicario). Yet, even in the field of the phenomenology of vision there are logical constructs which are not "phenomenally given," since we experience them in terms of their effects. For instance, we do not experience "the rules of perceptual organization." Instead, we infer their

existence on the basis of their phenomenal effects. How can the rule of proximity be demonstrated? Well, we have to translate the construct in a proposition of the ordinary language, for example "if I move these points so and so, then I shall see such and such." We do exactly what physicists do. In physics, the concept of "force" is a construct the existence of which can be verified in terms of propositions in the language of sensory data. If we do this and this... then we *see* this and this.

Levels of explanation thus refer to the *logical constructs* of the various sciences, and not to the phenomenal experiences to which *all* sciences should refer as the source of factual knowledge. Of course, logical constructs of the various sciences have a certain independence, in the sense that each science tends to verify its constructs using quite different aspects of sensory experience. Neurophysiologists verify their constructs translating them into the language of those sensory experiences that could be obtained when one looks at the nervous system activity (if I put my microelectrode in that way, then I shall *see* nerve cells responding in such and such a way). Psychologists verify their constructs translating them into the language of those sensory experiences that could be obtained when one looks at human behavior, including verbal behavior (if I put this display in front of the subject in such and such a way, then I shall *hear* my subject saying so and so). Eventually, the two sorts of translation could converge. Constructs originally validated in their own proper field may turn out to be useful for predictions in the other field. The reason (or hope) why this may occur is that we are describing the same thing from different viewpoints.

We disagree with the idea that when studying "silent processes" we psychologists are doing "neurological research" (and we suspect that neurologists would also disagree). We are doing psychology simply because we are studying psychological problems, that is problems that have arisen from careful phenomenological analysis of an organism's behavior, not that of its nerve cells.

Vicario: I never claimed that logical constructs are an integrating part of phenomenal experience. I just stated that the terms by means of which we describe mental facts, and which we use to build logical constructs, have to be derived from phenomenal experience. I agree with Vallortigara and Zanforlin in considering logical constructs necessary for the explanation of phenomenal experience, as a mere contemplation of reality is not an "explanation." Perhaps, I failed to make clear that if there is the need for logical constructs in explaining phenomenal experience then the terms connected to form logical constructs also have to be phenomenal. This condition is acknowledged in other sciences, and I cannot see why it

should be refused in psychology. For example, let us consider the explanation of planetary motion in physical terms. We form our logical constructs by connecting physical terms, such as places, times, masses, accelerations, and so on. To be sure, we do not use terms foreign to physics, as in the past, when planetary motion was attributed to the thrusts exerted by gods or angels to celestial spheres. By analogy, I maintain that in explaining mental facts the terms used in the formation of logical constructs must be phenomenal. Perceptual transparency is explained by connecting terms like "continuity," "unitariness," "stratification," and so on, which refer to perceived features. There is no use in arranging the reflectances of surfaces into formulas, since these formulas specify only the conditions for reproducing the perceptual effect.

I never stated that "in our mental life there are facts... that are not part of our phenomenal world" (p. 213). This is a contradiction in terms. I just affirmed that we face facts—like the process of forgetting—to which we cannot deny a psychological status, even if they are inaccessible to the phenomenological method. Consequently, I pointed out the special limitation of the method, which seems to mirror the symmetrical inability of the experimental method to escape from mere psychophysics or from neuronal circuitry to get at the very substance of everyday experience. Certainly, I am disappointed by the fact that the method I prefer is affected by a severe limitation of its application. However, I am at least aware of this, so I can consider the matter as a problem to be investigated.

I agree with Vallortigara and Zanforlin in considering the Boring (1950) distinction between experimentation and demonstration to be rather specious. This is the reason why I pointed out that some celebrated sciences, like astronomy and geology, are not experimental at all. What Vallortigara and Zanforlin maintain, namely that there is no distinction between experimentation and demonstration since the phenomenologist performs his parametric studies *before* the presentation of the "case in point," is exactly what I also stated, namely that psychophysics provides only the recipes for obtaining phenomena, without really dealing with the subject under discussion.

I cannot reply to the last remarks of Vallortigara and Zanforlin because they attribute to me opinions that I do not have—as I tried to clarify at the beginning of this reply. The plain truth is that they are Wundt's followers, and therefore they seize any opportunity to claim that in comparing mental facts with physiological processes "we are describing the same thing from different viewpoints" (p. 215). On the contrary, I am a follower of Brentano and thus persist in asserting the uselessness of describing and explaining mental facts with something other than phenomenal experience itself.

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