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Phenomenological Research v. Experimental Phenomenology

1. Introduction

Friedrich Christoph Oetinger introduced the term “phenomenology” in philosophical language in 1736, but the first use of it as “science of appearance” (*Lehre vom Schein*) was in the II Volume of Lambert’s “Neues Organon”. Here, however, I’m not dealing with the history of phenomenological research (PR). The interested reader is referred to two excellent papers, by Vicario (1993, 2008) and by Bozzi (1999). In some sense, the origins of PR can be traced back to Christian Wolff’s *Psychologia empirica* (1732 – see Kobau, 2002), or to Kant (1790 - see Bozzi, 1989).

In this paper, with PR I intend to refer to a tradition of research, mainly European, that began with Carl Stumpf and Ewald Hering in the last quarter of the 19th century and was largely developed in the following century, mainly (but not exclusively) in the tradition of Gestalt psychology. Among the main exponents of this line of research, we can cite Mach, Katz, Rubin, Michotte, and more recently Gibson, Metelli, Kanizsa, Johansson, Runeson, Bozzi, and so on; and, of course, all the proper representatives of the Gestalt school. In Italy, in particular, the PR had a particular salience, given the particular relevance that the *Gestalttheorie* had here for perceptual research (cfr. Verstegen, 2000; Zanforlin, 2006).

In previous years, the expression “experimental phenomenology” (from here on we will call it EP) denoted more often the same field of investigation; notice that, according to Verstegen (2005), EP is fully in the tradition of *Gestaltpsychologie*. Notice also that Michotte possibly created the expression “EP” in 1939 (cfr Michotte, 1952), and this expression became very popular after him. Earlier, Michotte, a former pupil of Cardinal Mercier (the most representative exponent of Neo-Thomism in psychology), of Wundt and of Külpe, had joined behaviorism (as many French-speaking psychologists before the Second World War). His ideas turned towards *Gestaltpsychologie* in the mid thirties, stimulated by the research on rhythm carried out in his laboratory by Fraisse (cfr Fraisse, 1946). According to Michotte (and Fraisse), rhythm could be considered a “temporal Gestalt”. It is interesting to note that Michotte was surprised by the fact that, in his opinion,

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this clear manifestation of Gestalten had been neglected by Gestalt psychologists, and his own studies "are the first intensive experiments on particular aspects of temporal forms". Of course, this is not true, and it is apparent that Michotte at least ignored the fundamental work by Koffka (1908-1910, see below). Fraisse (1956, p. 102) quoted it summarily, only to distinguish between acoustic and visual rhythmic structures.

In the words of Vicario (1993, p. 197), "experimental phenomenology is regarded as a true experimentation. Its experimental variables are mental contents of direct experience rather than physical stimuli or physiological processes". In Kanizsa's words (1983, p. 10), the end of the EP "is the discovery and analysis of causal connections necessary between the phenomena, the identification of the conditions that determine, help or hinder their appearance and the degree of their evidence". However, Michotte's definition of EP was substantially different (see below).

It is clear that this approach, EP, has nothing to do with the „experimental phenomenology" of Husserlian origin, for example that proposed in 1977 by Don Ihde, that attempts to apply the principles of scientific experimentation to it. This doesn't mean that the Husserlian approach is irrelevant both for PR and for EP – and that the scientific research inspired by Husserlian theorising is devoid of scientific value. In my view, the fact is that the Husserlian phenomenological approach is anti-naturalistic, in both ontological and epistemic terms (cfr. Aikin, 2006).

2. In what Sense is the EP "Experimental"? The Problem of the Intersubjective Observation

As argued earlier, 1939 marked a "turning point" in Michotte's career, with the creation of the EP. In Michotte's words, he, "still employing experimental method", confronted some problems "fundamental to phenomenology", that is, "causality, permanence, and apparent reality in our experience of the external world". According to Michotte, all these issues could be treated according to the method of "concomitant variation", proposed one century before by J. S. Mill (1843, vol. I, p. 470), stating that "whatever phenomenon varies in any manner whenever another phenomenon varies in some particular manner, is either a cause or an effect of that phenomenon, or is connected with it through some fact of causation". According to Hall (1924), this method could be considered "an efficient basis of any experimental method in psychology". (For a wider analysis of this method, see Vannini, 2006).

In an important paper, Michotte (1936 – notice that at that time he was greatly influenced by behaviorism) stated that when it is true that a true science of inner experience is impossible, impossible as it is to have reliable concomitant variations in the inner experience, nevertheless, if we don't consider the psychological situation in purely physical or physiological terms, but for their *meaning*, we can individuate that it is experienced by different observers in the same situation. The meaning is "the end result of a vast synthesis, stands only as a sign, the only effective indicator, of such a synthesis" (pp. 221-222).

Now, as Costall (1990) says, it is with the EP that Michotte found some situations in which he could operate a "remarkable reconciliation of his theoretical and methodological concerns", because here "meaning is directly experienced" (p. 9). In his more classical experimental situation, the observer sees an object moving towards another static object, and when there is collision, the second object is seen to move, as "pushed" by the first one. The "causality" in such a situation, and its meaning, is directly perceived, as well as the other kinds of causal effect that are originated by minor variations in the experimental conditions (Michotte, 1946). And, as Wagemans (2006, p. 11) noted,

> "whereas psychologists had long swung between the poles of pure introspection and anti-mental behaviorism, Michotte's work showed how phenomenal percepts could be identified, measured, and explained via careful demonstrations and experiments".

Now, we can concede that the concomitant variation is an important (perhaps the most important) aspect of the experimental method. But from Mill to now a long time has elapsed, and not only has the language of the methodology changed, but also new concepts have made their way, which have changed the criteria by which we can consider research genuinely experimental. We must stress that it is not only experimental research that is scientifically genuine, and at the same time that it is not only the research that investigates the concomitant variation of variables that is experimental. I agree substantially with the criticism raised by Pomeranz & Kubovy (1986), that in the tradition of Gestalt psychology we have many "demonstrations", when the Helmholtzian counterpart gives "experiments". Do we also in the case of EP have demonstrations, and not experiments?

When we speak of "experimental" research, we refer to a particular kind of *control* of the effect at play. Let's limit our analysis to only two characteristics of the experimental designs.

First, an experimental design is a research design in which the researcher tries to maintain control over all the factors that may affect the results of his/her research. This control could be obtained in several ways, relative to the variables and to the participants used in the research. With respect to the variables, if the independent variable is scaled on different levels, these levels must be chosen by the experimenter: for instance, if we are studying the effect of the speed of a moving object at different velocities, these different velocities must be chosen by the researcher.

Second, the participants must be chosen at random from a predefined universe. When the same observers are used for all levels of the independent variable (a practice that is better discouraged), the order of the exposition for each observer of the different levels must be determined at random, and must be different for each participant.

But why must we apply these rules, to obtain a genuine experimental design? The aim is to distribute reliably the variance obtained from the data at least in two portions, the first because of the treatment (the influence of the independent variable/s), the second because of the ever-present confusing variables, which are impossible to maintain completely under control, and which we summarize as statistical error.

Of course, this is the description of an ideal experimental design; we have different degrees of quasi-experimental designs, according to the different levels of control that we can maintain. For instance, if an independent variable is sex, it is obvious that we cannot vary the sex of the participants; if we choose males and females at random from the respective universes, the violation of the principles of the experimental design will not be serious; but if I choose my participants not at random, but with some other rule (for instance, males doing a certain job, and females doing another job), any possibility of control is lost.

When these are *grosso modo* the main features of the experimental designs, it must be clear that there are many other research designs that are fully meaningful from a scientific point of view. We can list here the case studies, the causal designs, the action research designs, the cohort designs, and so on. Anyway, these designs are not experimental, and for me it is difficult to understand why one is willing to call "experimental" a field of research in which the experimental designs are absent. So, when Zanforlin (2006) or Vicario (1993) state that to have true experiments is sufficient to assure the repeatability of the phenomena under study, they only neglect what the experimental control is.

In explaining why his phenomenological research must be considered truly "experimental", Michotte appeared to be a little simplistic (see Joynson, 1971). For example, he says that he utilizes two sources of information (Michotte, 1956, p. 11): the first is the "'experimental situation', consisting of the system of stimulation, and that can be defined in physical units, which one can measure with some instrument [...] All this evidently pertains to the 'world' of the physical science". (However, for a more articulated view of Michotte's methodological stance, cfr. Michotte, 1955).

Here, we have apparently the side of the independent variables. The dependent ones are the phenomena. Notice that Michotte denies implicitly the possibility of having phenomena as independent as well as dependent variables: that is, of having a phenomenological research *juxta propria principia*. This was contrary to

the major assumption by Bozzi (1989, 2003). In this sense, Michotte's and Bozzi's EPs are completely irreconcilable.
The second source is the language used by the participants to describe the phenomena that they observed. Here, Michotte (pp. 11-21) is rightly more analytic. First, he says that one must avoid inducing in the participants an "introspective attitude". "The instruction was 'simply to say what was in the apparatus'". The answers of the participants refer always to the physical world: this is not the world of physical science, but it is the "*world of the things as they appear to the subject*" (italics by Michotte). Second, one must carefully distinguish the world of physical science from the phenomenal world. Third, the processes that constitute the phenomenal world can be inferred only from what the participant says.

Notice that we are not saying that Michotte's methodology in this field was not experimental, in the true sense. It appeared to respect most requisites of the experimental designs at that time. We must add, however, that Michotte gives very little detail in describing his methodology, especially from the point of view of experimental control. But we are concerned here about what is considered today to be EP.

In 1989 Bozzi & Martinuzzi proposed a new methodology for the study of the EP, that they called *intersubjective observation* (IO). To be sure, most of Bozzi's research was performed with more traditional methods – see, for instance, all his work on "naïf physics" (see Bozzi, 1990). Bozzi's thinking could not be confined to IO and its implications. His theory has been extremely complex, ranging from a more or less orthodox adherence to *Gestalttheorie*, as in Bozzi (1969), to a Gibsonian approach (see Bozzi & Luccio, 1999).

According to Bozzi (1978, 1989, 2002), IO constitutes a substantial methodological innovation, a powerful tool to unveil what the phenomena really are for the observers, and was readily accepted by many researchers (cfr Savardi, 2006; Savardi & Bianchi, 2003).

Let's outline the experiment by Bozzi & Martinuzzi. They presented to two groups of participants six pairs of stimuli that consisted of achromatic circles (white, grey and black) that were at the centre of achromatic squares of different sizes and colours. The participants had to say which figure of the pair was lighter (or darker), or if their brightness was equal. The participants were divided into two groups: the first group (I) consisted of 25 participants tested individually, the second group (G) of 25 groups of three participants each that could discuss freely (also with the experimenter) what they were observing, to reach a shared final decision. They could also change places in the room, pose questions, and so on.

In the G group, compared with group I, there were fewer decisions of "equal". Generally, the decisions tended to be uniform in each subgroup. According to the authors, the difference was significant at the X^2 test. Of course, the conditions to use this test are not met here, but this is not so important, and we can safely take for granted this result. Moreover, the results of the group G confirmed the ones of the group I, but are more clear-cut.

According to Bozzi & Martinuzzi, these results cannot be attributed to a Rosenthal effect, because the experimenters had no previous experimental hypotheses. So stated, this is fun, because a few pages before (pp. 24) are listed the "expectancies" of the authors, which are exactly the results obtained. The results, however, as Bozzi & Martinuzzi recognized, go in the same direction as the ones obtained by Sheriff (1935) on autokinetic movement. In other words, there is a tendency to conform, which the method enhances.

My point, anyway, is not to criticize the IO *per se*. Sure, it is not an "experimental" method, because it completely lacks experimental control. Consider this point: in comparing G with I, each subgroup was considered to be composed by *one* participant. In terms of attribution of explained variance, it is impossible to make any meaningful statistical analysis. For this reason, the use of the X^2 test is inappropriate. The comparison of the results of individuals and of groups could be fruitfully done, in my opinion, but certainly not in experimental designs. So, why call this research EP, *experimental* phenomenology?

The kind of design that we have described above is not the only experimental design. In other experimental disciplines, like physics, chemistry, biology, more often the scientist tries to establish lawful relationships between variables: in other words, to establish a mathematical model, and to see how the data fit the model. This is seldom done in psychology, but it is possible. Consider, for example, what happens with reaction times, one very classical domain of experimental psychology.

Here, researchers have established a number of lawful relations between variables. Let's consider the cases in which the lawful relationship links an independent variable to a dependent one. For example, the classical Piéron's (1914) law links the intensity of stimulation *I* to the *RT,* according to this power function:

$$(RT - t_0) = \beta I^{-\alpha}.$$

Here, t_0, a and b are parameters: the first is temporal, the second is a sensitivity parameter and the third depends on the units of stimulation.

A typical study of the Piéron law requires: i) that one measures the RT at different levels of intensity of stimulation; ii) that on the basis of these data it is then possible to estimate the values of the parameters for the function that has the best fit with the empirical data – the techniques more frequently used are based on the principle of maximum likelihood, or Bayesian statistics; iii) that we can now calculate the values of the theoretical points, and an analysis of the distances between theoretical and empirical points allows us to determine the portion of variance that we can attribute to the relationship between independent and dependent variables (explained variance). The most widely used index of explained variance is the R^2.

A relation like Piéron's law is properly a mathematical model. Sometimes, these models are borrowed from other sciences, like physics. The introduction of non-linear dynamics in psychology has rendered this opportunity more popular. An example will illustrate this possibility, and will help also to discuss another core problem of phenomenological research: the percept-percept coupling (Epstein, 1982).

Non-Linear Dynamic Approach: an Example from Synergetics

In 1994, Kanizsa, Kruse, Luccio, & Stadler gave an experimental demonstration that is particularly useful for our argument. Look at the Fig. 1. Here, we have three dots that occupy corresponding positions on three circles, representing three circular paths, along which they move. The paths are partially overlapped, and when the dots move what we see is an elastic triangle rotating and twisting in three-dimensional space. Increasing the number of the dots moving on each path from one to five, the patterns are still invisible. In the area in which the circular paths overlap, the dots form, continuously transforming and disrupting the groups. The overall impression is of great disorder, of the Brownian movement of dots surging up from the middle of the configuration. The observer succeeds in detecting the circular motions only when there are more than six dots on each path. Note that the observer is quite aware of the existence of the three distinct circular paths: his/her task is precisely to succeed in detecting them.

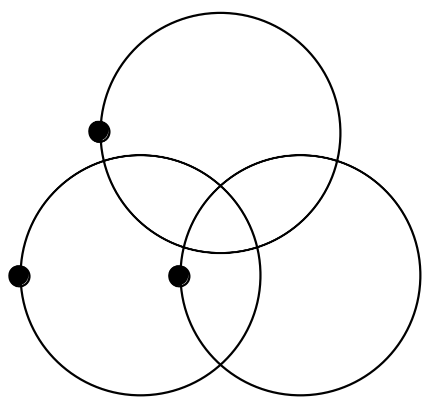

Fig. 1 When the three black dots rotate circularly along the three partially overlapping paths, one sees a virtual triangle that rotates, and it is impossible to see the three paths.

To understand what happens, it is important to distinguish between common and relative motion (Cutting & Proffitt, 1982). The first is the apparent motion of the whole configuration relative to the observer, and the second is the apparent motion of each element relative to the other ones present in the configuration. Cutting & Proffitt, however, have shown that there are two simultaneous processes that correspond to common and relative motion. The prevalence of either is a matter of which process first reaches a minimum.

Here, in the situation with 1-2 dots there is a very clear prevalence of common motion: the participants perceive the whole figure like one or two triangles, that move circularly all together. One does not see the individual circular paths, because the three dots that are the vertices of the triangles each belong to a different path, and one sees the triangles rotating as a whole. With 6 or more dots, relative motion clearly prevails. Notice that in this case relative motion and absolute motion coincide. Here, we see clearly the dots moving along circular paths. These dots, however, are seen *relative* to the other dots, forming three independent rotating circumferences. So, according to the minimum principle, one can consider the perceptual solutions for 1-2 and 6-15 dots to be „good" solutions.

When the average distance of each dot from the other dots on the same path (DS) is different, according to the class of the polygon, the average distance of each dot from the dots on the other paths (DO) is always the same, leaving constant the relative position of the three paths. If the centres of the paths are on the vertices of an equilateral triangle, DO is always the same, regardless of the number of dots per path. This suggests the hypothesis that the difference between DO and DS could be a crucial factor for the prevalence of either common or relative motion. When DO is clearly less than DS common motion prevails. The same can be obtained also varying the distance between paths, or by either reducing or increasing their radius. When DOs are smaller than DSs, it is very difficult to detect the circular paths. A perceptual organisation in which the dots are moving along these paths is possible only if the average distance between dots on a path is less than the average distance of each of them to the dots on the other paths. Thus, the proximity of dots proves itself to be a crucial factor in the perceptual organisation of phenomenal motion.

We can conclude that by increasing the number of dots in the first experiment the visual system undergoes a phase transition from common motion to relative motion, passing through an unstable situation with no clear perceptual organisation. The relevant control parameter (in terms of „synergetics"; Haken & Stadler, 1990) is the difference between the average distance from the dots on other paths (DO) and the average distance from the dots on the same path (DS). If DO is clearly smaller than DS the order parameter of common motion emerges and the system is in a stable attractor state. If DS is clearly smaller than DO the order parameter of relative motion emerges and the system is in a totally different stable attractor state.

So, the participants had to individuate the two transition points between common motion and perceptual chaos, and between perceptual chaos and relative motion. We have also a strong perceptual hysteresis, according to the direction of the movement of the paths, that is assumed to provide strong support for nonlinear dynamic models of categorical perception.

More generally, one can subsume this phenomenon under the general family of Haken, Kelso, & Bunz's (1985) model of phase transitions. The transition between the three percepts is abrupt, as in all cases of categorical perception, also when the variation of the length of the differences between DO and DS is continuous. Here, we have an evident non-linearity, with a phase transition from a first attractor (the common motion) to another attractor (the chaos) to a third attractor (the relative motion). We can describe this process in terms of direction and tilt for the potential *V*(*x*), where *x* is the perceptual form, deriving it from the ordinary motion equation:

$$\dot{x} = -\frac{dV(x)}{dx},$$

where $\dot{x}$ is the time derivative of *x*. Here, we find a fit with the following function:

$$V = k - a \cos \varphi - b \cos 2\varphi$$

This equation describes a so-called 'saddle-fork' attractor. The best fit is obtained here with $a \approx 0.5$ and $b \approx 0.25$. *k,* a monotonically increasing function of the difference between DO and DS, is the *control* parameter. When $k < 0$, the prevailing attractor corresponds to *common motion*; when $k > 0$, the prevailing attractor corresponds to *relative motion*; when $k \approx 0$, neither attractor prevails, and we have the perception of chaos. (For a deeper discussion of these models, see Kelso, 1995).

In the above-illustrated cases, we are in the presence of true experimental designs, of course different from the case of the comparison between groups. Here too we can attribute the portions of variance because of the relationship between independent and dependent variables. Moreover, we can also measure the fit of our empirical data to the theoretical function.

Field Theory

In terms of experimental control, procedures of the kind described in the previous sections offer the most reliable guarantees. Of course, the problem is not to adhere to some pre-defined theory, like synergetics, or other non-linear models.

Here we will briefly introduce other models that are particularly suited to phenomenological research.
A very important concept in Gestalttheorie is *self-organization*. Strictly linked is the concept of *field* (Köhler, 1920), often used as a vague metaphor. However, there have been several attempts to use the field concept in a more rigorous way, first in Brown & Voth's model of vector field (1937).

More recent attempts were those by Stadler, Kruse, Richter, & Pfaff (1991) and by Kruse, Luccio, Pfaff, & Stadler (1997). Stadler et al. developed a vector field model of the visual field. The experiments at the basis of this model required that the participants, presented with a sheet on which there was one dot, reproduce the position of it on a pressure pad. From the displacement of the reproduced dots, the authors calculated the vectors of translation and torsion for each position on the field. The resulting vector field was amazingly similar to Blum's grassfire field (1967), and to the path shown by the "migrating point" in Bartlett's reproduction experiment (1951). In this model, the dynamics of each point in the field were separated into two components, a *gradient field* and a *circular field* (see below). According to this representation, one can describe the field in terms of a landscape, with hills (high potential) and valleys (low potential).

So, one can describe the field $A(r)$ dividing itself into two subfields: a gradient field $G(r)$ and a circulation field $C(r)$ (details in the original paper by Stadler & coll., 1991).

$$A = G + C = -\mathrm{grad}V + \mathrm{curl}W.$$

More recently, Igel & Harvey (1993), Kruse & coll. (1996), Luccio, Mancini, & Salvadori (2005), and Harvey & Schmidt (2014) obtained similar results. A typical representation of the landscape is given in Fig. 2.

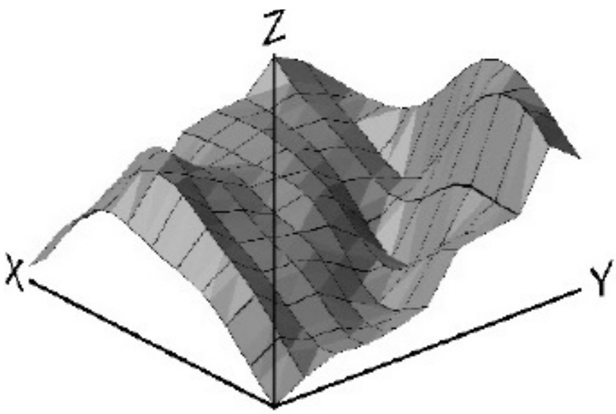

Fig. 2 A typical landscape obtained from a participant, averaging the data of the four quadrants in one quadrant (from Luccio, Mancini, & Salvaori, 2005).

Of course, here we are very far from experimental control, but this doesn't mean that these data weren't obtained via a very rigorous scientific method.

Concluding Remarks

I have confined myself to only a few respects in which phenomenological research has developed in recent years. But the perspectives that one can list are countless. Wagemans et al. (1912), in the second part of a valuable analysis of a century of Gestalt psychology and looking to the future, stress the need for a reconsideration of the conceptual foundations of the Gestalt approach. They list different domains in which the research must progress: first, what holism means today; and, after Prägnanz, in four theoretical perspectives, namely the brain as a complex adaptive system, the economy principle, the Bayesian redefinition of Prägnanz, and Structural Information Theory.

Geremek, Greenlee & Magnussen (2014) have made another valuable contribution, collecting (on the occasion of the 75^{th} birthday of Lothar Spillmann) thirteen papers on perception, devoted to today's perspectives on Gestalt psychology. The authors are all well-known researchers in the field. What is interesting here is the effort to collect side-by-side pure phenomenological and psychophysiological papers.

Last, but not least, I consider worth mentioning the computational Gestalt approach, developed by Agnes Desolneaux and her collaborators (e.g. Desolneux, Moisan & Morel, 2008). Here a central concept is "quasi-gestalt".

In conclusion, all these perspectives are of course in line with the legacy of Gestalt psychology. But I find it in some sense hard to subsume several results that we obtained with these methodologies into Gestalt, *strictu sensu*. Are the order parameters of the synergetic *Gestalten*? The different levels of potential in the visual vector field: are they *Gestalten*? What about the partial gestalts of the computational theory?

I think that in all these cases we face some cognitive structures that embrace more elements than the Gestalten that are in some sense free. I want to suggest that we appeal to a concept defined by Stumpf (1907) in a very important paper, the concept of *Gebilde*, maybe the most important antecedent of the concept of Gestalt, not in the sense of von Ehrenfels (1890), but in the sense of the Berlin school. However, there is not enough room here to analyse this point.

Summary

In this paper, I distinguish between phenomenological research, that is, the tradition of research in the field of Gestalt psychology, and experimental phenomenology, a subsector of this field, originating with Michotte and particularly developed by Bozzi. I argue that the expression "experimental phenomenology" is misleading, because, especially in

Bozzi, we are confronted with research that lacks experimental control. In my opinion, there are anyway several advanced scientific procedures that are not strictly experimental, but which are nevertheless valuable from a scientific point of view (non-linear dynamic models, vector field analysis).
Keywords: Phenomenological research, experimental phenomenology, experimental design, Gestalt psychology, intersubjective observation.

Zusammenfassung

In diesem Artikel unterscheide ich zwischen phänomenologischer Forschung, das heißt, der Forschungstradition der Gestaltpsychologie, und der experimentellen Phänomenologie, ein Subsektor dieses Feldes, mit Ursprung bei Michotte und insbesonders von Bozzi entwickelt. Ich behaupte, dass der Ausdruck „experimentelle Phänomenologie" irreführend ist, denn vor allem bei Bozzi sind wir mit einer Forschung konfrontiert, bei der die experimentelle Kontrolle fehlt. Meiner Meinung nach gibt es ohnehin einige fortgeschrittene wissenschaftliche Verfahren, die zwar nicht streng experimentell, aber dennoch aus wissenschaftlicher Sicht sehr wertvoll sind (nichtlineare dynamische Modelle, Vektorfeld-Analyse).
Schlüsselwörter: Phänomenologische Forschung, experimentelle Phänomenologie, experimentelles Design, Gestaltpsychologie, intersubjektive Beobachtung.

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