Facts, Concepts and Theories: The Shape of Psychology's Epistemic Triangle

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FACTS, CONCEPTS, AND THEORIES: THE SHAPE OF PSYCHOLOGY'S EPISTEMIC TRIANGLE

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ABSTRACT: In this essay we introduce the idea of an epistemic triangle, with factual, theoretical, and conceptual investigations at its vertices, and argue that whereas scientific progress requires a balance among the three types of investigations, psychology's epistemic triangle is stretched disproportionately in the direction of factual investigations. Expressed by a variety of different problems, this unbalance may be created by a main operative theme—the obsession of psychology with a narrow and mechanical view of the scientific method and a misguided aversion to conceptual inquiries. Hence, to redress psychology's epistemic triangle, a broader and more realistic conception of method is needed and, in particular, conceptual investigations must be promoted. Using examples from different research domains, we describe the nature of conceptual investigations, relate them to theoretical investigations, and illustrate their purposes, forms, and limitations.
Key words: conceptual analysis, scientific method, language, epistemic state, psychology.

Regardless of their particular philosophical penchants, most scientists would probably agree that scientific development requires at least three kinds of investigations—factual, theoretical, and conceptual. When we examine whether an 8-month-old baby will reach for a toy that was removed from its sight or whether a rat will revisit an arm of the maze after the experimenter altered the extramaze cues, we are engaging in factual investigations. When we describe mathematically how pacemakers, accumulators, and comparators account for a pigeon's ability to regulate its behavior in time or how children construct the concept of time from the progressive coordination of the concepts of sequence of events, simultaneity, and duration, we are engaging in theoretical investigations. When Catania (1975)

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examined the consistency of the concept of self-reinforcement in learning theory, or when Piaget (1986) distinguished true knowledge from necessary knowledge in his theory of human development, each author was engaged in conceptual investigations.

These three kinds of investigations are closely interrelated, and to stress this point we visualize them as the vertices of an equilateral triangle. Factual investigations yield the elementary components of functional relations and theories, which in turn may be conceived as coordinating and animating facts, as bringing them to life. Conceptual investigations, on the other hand, check the intelligibility of theories, explicate their meanings, and identify their sensible domains. Although the separation among factual, theoretical, and conceptual investigations is not as sharp as we have implied, for heuristic purposes we will continue to stress the end points of the lines (i.e., the vertices of the triangle).

The interrelation among the three kinds of investigations implies that scientific growth will be hampered whenever any one of them is atrophied—when the epistemic triangle collapses, as it were, to a line or a point. Unconstrained by facts and functional relations, for example, theoretical and conceptual investigations are mere verbiage or idle speculation. Like Bacon’s Idols of the Market Place, they “plainly do violence to the understanding and throw everything into confusion, and lead men into innumerable empty controversies and fictions” (Bacon, 1994, Book I, Aphorism 43). But if unchecked by theoretical and conceptual investigations, facts are blind, disorganized, and even meaningless. They multiply, “range widely, yet move little further forward” (Bacon, 1994, Book I, Aphorism 70). Scientific communities must therefore strive to balance the various kinds of investigations, lest the growth of their science proceed in unprofitable directions.

The purpose of the present essay is to elaborate and use the preceding ideas to understand some of the problems that afflict contemporary psychology. Specifically, we will try to show that a variety of well-known problems in psychology reflect a pattern of unhealthy growth due to a disproportionate emphasis on factual investigations at the expense of theoretical and conceptual investigations, particularly the latter. In other words, psychology’s epistemic triangle seems to have lost its harmony because it has been stretched excessively in the direction of facts. In the first part of this essay we analyze the expressions and costs of this distortion.

If our diagnosis is correct, then it follows that the harmony of the triangle must be restored before the problems can be solved. Conceptual investigations in particular must be promoted. But for that to happen we need to know first what accounts for psychology’s excessive preference for factual investigations. This issue will be addressed in the second part of the essay. To anticipate our conclusion, we identify two fundamental reasons for psychologists’ preference for factual investigations: (a) their overconfidence on the scientific method as a means of finding, almost mechanically, empirical truths and (b) their long-held suspicion of philosophical speculation.

In the third and final part of the essay we examine the interrelations between theoretical and conceptual investigations and argue that although psychology needs to promote both to balance its obsession with facts, the need for conceptual investigations is particularly severe. We document this need through a variety of examples from different research domains and in the process characterize conceptual investigations, show the forms they may take, their limitations, and how they can be at least part of the solution to the problems of psychology.

Part I: The Prominence of Factual Investigations

Since its early days as an experimental science, psychology has always had its share of discontents. At the turn of the century, William James (1892/1985) assessed the state of psychology thus:

> A string of raw facts; a little gossip and wrangle about opinions; a little classification and generalization on the mere descriptive level; a strong prejudice that we have states of mind, and that our brain conditions them: but not a single law in the sense in which physics shows us laws, not a single proposition from which any consequence can causally be deduced. We don't even know the terms between which the elementary laws would obtain if we had them. (p. 335)

However, for James, the confusion, disorientation, contradictions, and false pretenses of psychology were understandable given its young age.

Almost 60 years after James, the philosopher Wittgenstein (1958) had much the same to say about psychology. Despite the appearance of healthy growth, from numerous books and journals, meetings and symposia, to burgeoning professional associations, specialties, and schools of psychotherapy (see Burnham, 1987, p. 100), Wittgenstein was disturbed by the lack of conceptual analysis in psychology and by its nonsensical dichotomies (e.g., the inner and the outer, the mental and the behavioral). On the last page of his *Philosophical Investigations*, he identified the source of the trouble: "For in psychology there are experimental methods and *conceptual confusion*" (p. 132). He further remarked that the relatively young age of psychology had little to do with its conceptual problems and, therefore, that time in itself was no guarantee that the "hope of a science" would be fulfilled.

As time elapsed, the number of discontents with the state of psychology increased and the invoked reasons multiplied. From within, Lindsey (1977) criticized the excessive triviality and illiteracy in the publication stream of psychology; Peters and Ceci (1982) and Harcum and Rosen (1993) the poor reliability and bias in the peer review process; Staats (1991) the fragmentation of current knowledge and the disunity of the community of psychologists; Boneau (1992, p. 1596) what he construed as the "self-destructive processes that seem to plague our field" as a consequence of its fragmentation and disunity; Cohen (1994) and Meehl (1978) the overuse of the null hypothesis significance testing as a means to validate empirical propositions; Scarr (1997) what she perceived to be the intolerance of mainstream cognitive psychology toward those who are not married to the system and steeped in its fashion; Liben (1997) the tendency for psychology to collapse on the back of straw men instead of standing on the shoulders of giants; McFall (1991) the scientifically invalid, inappropriate, and unacceptable aspects of "much that goes on under the banner of clinical psychology today" (p. 79); and Bruner (1990, 1996) the meaninglessness of contemporary cognitive psychology. From without, Andreski (1972) denounced the ineffectiveness of applied educational psychology; Dineen (1998) characterized psychotherapy as the snake oil of the 90s and the psychology establishment as a big industry concerned more with business than science (see also Dawes, 1994); Horgan (1999) noticed how the investigations of the mind have failed "to generate applications that compel belief in a particular paradigm. . . . If psychoanalysis is the equivalent of phlogiston . . . so are all its would-be successors"(pp. 6-7); Ziman (1978) lamented the absence of "reliable maps" and consensual theories in the social and behavioral sciences, and the presence of much experimental irrelevance, theoretical unprovability, and categorical vagueness; on the basis of similar arguments, Bauer (1992) and Gellner (1984) concluded that, despite appearances, psychology is not a science. Going one step further, the late Sigmund Koch even wondered whether psychology, a science "created by fiat towards the end of the nineteenth century" (Koch, 1969, p. 64) could ever become a science like physics or biology (see also Gibson, 1994; Koch, 1981; Kupfersmid, 1988; Leahey, 1991; Rotter, 1990; Slife & Williams, 1997).

The feeling of malaise conveyed by the preceding remarks, of enduring visceral dissatisfaction with psychology, was voiced recently by one of its leading investigators:

> In 1964, I entered the field of psychology because I believed that within it dwelt some of the most fundamental and challenging problems of extant sciences. . . . Today, in 1996, my fascination with these problems is undiminished. But I have developed a certain angst over the intervening 30-something years—a constant, nagging feeling that our field spends a lot of time spinning its wheels without really making much progress. (Loftus, 1996, p. 161)

On less subjective grounds, the preceding remarks identify a plethora of serious problems but no clear pattern or motif. In fact, it is conceivable that there is no such pattern, that the heterogeneity of the problems expresses a variety of different motifs. However, it is also conceivable that such heterogeneity contains a leitmotif, one that may be perceived if we are willing to look at the problems from a new perspective. This latter approach will be pursued here, not because it is necessarily correct, but because it seemed more promising than the search for separate solutions to each one of the preceding problems.

Consider then an epistemic state in which factual investigations overshadow conceptual and theoretical investigations. What are some of the expressions of this unbalance? We make four guesses: (1) An excessive number of empirical articles, for facts can stand alone and therefore we may treat them as publishable on their own, whereas theoretical and conceptual analyses must relate and therefore require more systematic research. (2) An asymmetry between an advanced technology of data analysis and a rudimentary state of conceptual and theoretical frameworks, for facts ask only to be collected, sorted or lumped together, whereas concepts, functions, and theories ask to be clarified, sharpened, delimited, and coordinated. (3) Fragmentation and artificial specialization in the field, for facts are always unique, whereas concepts, functions, and theories, due to their symbolic nature, abstract, generalize, and interrelate. (4) A high frequency of distortions and mischaracterizations of other people's work, for an excessive investment of resources on learning to collect facts and conduct experiments will necessarily reduce the resources allocated to learning conceptual and theoretical analyses. And without a careful analysis of a theory, of its history and conceptual structure, distortions are likely to occur. We examine next these four expressions of a distorted epistemic triangle as well as their costs.

Excessive Number of Publications

According to *Journals in Psychology* (American Psychological Association, 1993), there are about 350 journals in the field and approximately 20 to 300 articles are published annually in each journal (the number varies greatly from journal to journal). Taking the very conservative figure of 30 articles per journal per year, we obtain a total of 10,000 published papers. Furthermore, if we extrapolate the average rejection rate of manuscripts submitted to APA journals (approximately 80%) to all other psychology journals, then we conclude that the number of manuscripts submitted for publication may exceed 50,000.[1] Add to the preceding figure the number of manuscripts that are published in proceedings or in books or orally presented in hundreds of yearly symposia, and one cannot help but be overwhelmed with the tremendous output of psychology.

At first sight, there are many reasons to celebrate when a science reports so many new findings and identifies so many new problems annually, for we are tempted to view its productivity as a token of fast progress. However, the annual productivity that leads to this avalanche of journals, books, articles, meetings, and congresses seems disproportional to the number of findings convincingly explained or of problems effectively solved—not to mention the number of theories that attain even a modicum of social consensus. According to some authors there is much "triviality, illiteracy, and dullness" in the publication stream (Lindsey, 1977, p. 579), and "too little adventure in the thinking and research" (Scarr, 1982, p. 248). The disparity between the voluminous empirical output and the advancement of the science leads to the impression that, as Loftus (1996) stated, we are getting too little in return for too much, spinning our wheels without making much progress (see also Rotter, 1990; Staats, 1991).

Robust Techniques and Fragile Theories

When a scientific community invests most of its resources in factual investigations, another asymmetry is likely to develop, that between its

[1] Non-APA journals may have a lower rejection rate, but even if one takes 60% as the average rejection value, the number of submitted manuscripts still exceeds 25,000 per year.

sophisticated technology of data gathering and analysis, on the one hand, and its primitive conceptual structures and theories, on the other. Psychologists' toolbox includes well-developed techniques of behavioral observation, coding, and measurement, a rich collection of experimental designs to test research hypotheses, and abundant repertoires of statistical techniques used to process and interpret experimental findings. This advanced technology might suggest that psychology's field of inquiry is reasonably clear, that its major areas of study have been identified on the basis of consensual lines of fracture of its subject matter, that the basic dimensions of the phenomena under scrutiny are known, and that the primitive terms of a psychological explanation have been agreed upon. Unfortunately, this is not the case. Psychological research is rich in tabular asterisks but only at the cost of theoretical risks and confusions; it uses powerful statistical techniques but has rarely delivered equally powerful concepts, functions, and theories.

The asymmetry between technical and theoretical growth rates would be far less problematic were it not accompanied by three troublesome tendencies: (a) the tendency to assess the significance of data by the means used to obtain them, (b) the tendency to eliminate the empirical-conceptual distinction and reduce all problems to empirical problems, and (c) the tendency to initiate experiments without first setting the stage by appropriate conceptual analyses. Concerning the first tendency, Kupfersmid (1988), for example, has shown that whenever a study arrives at a statistically significant result its probability of being published increases substantially. Other authors have claimed that the permanent appeal to null-hypothesis significance testing is responsible for the illusion that we have found a straightforward and noncontroversial procedure to test our experimental predictions and hypotheses (Cohen, 1994; Meehl, 1967, 1978, 1990). Still others (e.g., Michael, 1974) have explicitly noted the current conflation between technical sophistication and practical or theoretical relevance.

Smedslund's (1994) study illustrates the second tendency, to assimilate conceptual to empirical questions. The author selected, without advance knowledge of their content, five studies published in *Child Development*, a leading journal in developmental psychology. The studies were published as tests of empirical, synthetic, or contingent hypotheses, and thus amenable to falsification, but a conceptual investigation of the propositions involved revealed that in four cases the hypotheses were *a priori*, analytic, or noncontingent and hence immune to empirical test. In other words, what appeared to be the result of an empirical test of particular propositions followed necessarily from the meaning of their constituent terms. For example, one study hypothesized that "maternal employment will adversely affect the quality of care mothers provide their toddlers when it co-occurs with low role satisfaction, poor social support, low income or education, or when mothers work long hours" (Smedslund, 1994, p. 289). "Of course!" one is likely to reply, "How could it be otherwise?" And if the reader objects that the mother may be aware of the negative effects of her concerns with other matters and, as a consequence, she may try to compensate for it, then, as

Smedslund pointed out, the objection simply adds an additional factor to the main hypothesis. The latter remains insensitive to empirical test.

To illustrate the third tendency, to engage in experiments without preliminary conceptual analyses, consider Piaget's critical distinction between true and necessary knowledge, "the central problem of psychogenesis of operational structures" (Piaget, 1967, p. 391). For a child to be credited with an operational competence, he has not only to give the correct answer in a given task (i.e., true knowledge) but also to show that the answer has to be the case (i.e., necessary knowledge). However, during the last 30-odd years countless empirical studies claimed that Piaget had underestimated the young child's operational competencies because they seemed to have revealed that 4- to 6-year-old (or even younger) children were already capable of performing "well" on tasks deemed to be identical to those used by Piaget (see, e.g., Gelman & Baillargeon, 1983). The problem was that in most of these studies Piaget's conceptual distinction between truth and necessity was overlooked because their authors intentionally avoided repeated questioning (e.g., Siegal, 1997), did not ask for a child's justifications (Siegel, 1982), did not offer counter-suggestions (Rose & Blank, 1974), and did not challenge a child with perceptual seductions[2] (McGarrigle, Grieve, & Hughes, 1978). It is now widely recognized that children have been ascribed "operational" competencies that, on further analyses, were only preoperational (see Chandler & Chapman, 1991; Lourenço & Machado, 1996).

To experiment and analyze data statistically are indispensable practices in science. But when they are taken as ends rather than means; when only questions answerable by experiment are deemed worth asking; when experiments are published because they use sophisticated techniques; and when numbers are privileged regardless of whether true measurement has been achieved, then we have the signs of an epistemic state dominated disproportionately by factual inquiries.

Fragmentation and Artificial Specialization

Whenever factual research greatly exceeds theoretical and conceptual analyses and discovery prevails over the explication of meaning and the interrelation of concepts, attention centers on particulars, small differences are magnified, and large similarities are overlooked. Fascination with facts, and its corollary of technical sophistication at the cost of functions, theories, and concepts, also tends to alienate scientists from their past and lure them into a blind *fuite en avant*. Hence, two additional expressions of a distorted epistemic triangle may be the fragmentation and the artificial specialization of a scientific community. As Burnham (1987) remarked after reviewing the history of 19th- and 20th-century

[2] A child may state that there are as many marbles in one row as M&M's in another row when marbles and M&M's are aligned in a one-to-one correspondence. By changing the spatial layout of the marbles (say, putting them close together), the experimenter may perceptually seduce the child into saying, "Now there are more M&M's because they reach further than the marbles."

science, "one of the marks of the narrow technician was his or her unwillingness to go beyond facts" (p. 251).

That disunity and fragmentation characterize psychology is well known: "The present scene in psychology is one . . . of fragmentation and chaotic diversity" (Maher, 1985, p. 17; see also Horgan, 1999; Staats, 1991). In addition to the dominant approach known as cognitive psychology, one has to reckon with (in no particular order, and without being exhaustive) connectionists, radical, biological, and theoretical behaviorists, ethologists of various persuasions, evolutionary psychologists, dialecticists and postmodernists, Piagetians, neo-Piagetians, and constructivist Piagetians, Kohlbergians and Gilliganians, contextualists, bioecologists, socioculturalists, Cartesian innatists, and so forth. The disunity is equally impressive in the more applied areas. For example, in 1980 there were more than 100 schools of therapy (Marshall, 1980) and today it seems that we have many more; applied psychologists have frequently complained that basic research is divorced from practical interests and needs (Borkovec, 1997); and the large number of APA divisions (more than 50), the creation of its rival, the American Psychological Society (APS), and the existence of other societies (e.g., the Psychonomic Society, the Association for Behavior Analysis), all seem to support Geertz's (1997, p. 22) acidic remarks: "[Psychology] looks like an assortment of disparate and disconnected inquiries classed together because they all make reference in some way or other to something or other called 'mental functioning.' Dozens of characters in search of a play."

For some psychologists, however, the fragmentation of psychology is not a cause for concern. Estes (1979), for example, likens the current situation of psychology to that of physics during the early 20th century, divided as it was into the studies of mechanics, heat, optics, acoustics, electricity, and magnetism:

> The situation in which the experimental psychologists find themselves is not novel, to be sure, nor peculiar to psychology. Physics during the early twentieth century subdivided even at the level of undergraduate teaching into separate disciplines. . . . Similarly chemistry has branched out, evidently irreversibly, into inorganic, organic, physical, and biochemical specialties, among which there may be no more communication than among some of the current subdisciplines of psychology. (p. 661)

But the comparison is disingenuous to say the least, for the subareas of psychology are not characterized chiefly by the phenomena they investigate, no matter how fuzzy the boundaries between them may be. To be convinced of the difference, in case any doubt remains, it suffices to perform the following "experiment": Place a Piagetian child psychologist, a behavioral psychologist, and an information-processing cognitive psychologist at the same table, and ask them to discuss a research topic. It will soon be obvious that what distinguishes the three psychologists is less their respective object of study than their philosophical beliefs, methodological preferences, and even styles of reasoning. For the situation of psychology to be closer to that of physics, one would need to have an expert on mechanics, say, claiming that his neighbor's research on electricity and magnetism is irrelevant, his methodologies inappropriate, and his theories plainly wrong. Psychology, like all other natural science, lacks a single theory of its domain of inquiry, but unlike other natural sciences it reveals a profound disharmony and inconsistency among its various specialties.

The difference between the fragmentation of psychology and the divisions in physics, chemistry, or biology, is further highlighted by the historical dimension of the problem. Consider, for example, how many new psychological schools were born and died in the span of a single century (see, e.g., Hunt, 1993; Leahey, 1991), and how often psychologists dismiss their heritage: "Piaget? Yes, a brilliant psychologist, but a mere figure of the past" (see Cohen, 1983); "Are you referring to Skinner, the radical behaviorist? But hasn't Chomsky refuted his entire approach?" (see Richelle, 1993); "Why bother learning the research tradition on memory if Neisser in 1978 dismissed the work of the past 100 years as largely worthless?" (see Roediger, 1991). In Zeaman's (1959, p. 167) words, "in the natural sciences, each succeeding generation stands on the shoulders of those that have gone before, while in the social sciences each generation steps in the face of its predecessors."

Disunity and fragmentation beget artificial specialization, that is, specialization determined not by the lines of fracture of the object under study, or by the degree of scientific maturity attained at a particular historical moment, but by social isolation. One of the consequences of this kind of specialization is that instead of an open market where scientific ideas compete freely, we obtain a closed market where ideas remain unchallenged. As evolutionary biologist Ghiselin (1989) described it,

> each field becomes ever more specialized, inward-looking, and isolated from the rest of the intellectual economy. Communication between disciplines comes to be avoided as a matter of policy. Each develops a language intelligible only to insiders. Problems become increasingly technical, and their solution contributes less and less that might interest an outsider. The field becomes autonomous in the sense that only a professional acting in the interests of the profession plays any role in evaluating the product. Theories thus become immune to any but internal criticism. The profession ceases to recruit immigrants from other fields, relying, instead, upon those cast in the mold of its own pedagogical machinery. So it all becomes academic in the worst sense of the word—divorced from real life, especially the life of the mind. (p. 188)

Another consequence of artificial specialization was identified by Burnham (1987) who dated the onset of the process shortly after 1930: "Not only narrowness, but mediocrity. In a highly fragmented, technical system people flourished professionally who in another day would have been handicapped by insufficiency of breadth, to say nothing of their lacking the culture and calling of Victorian scientists who argued for science because it was culture" (pp. 251-252)

Distortion and Mischaracterization

Distortions are expected when factual investigations predominate over theoretical and conceptual investigations because, in this circumstance, standards of technical competence tend to substitute standards of intellectual scholarship. Given a limited budget, a scientist with a strong preference for factual discovery is likely to allocate most of his resources to planning and running new experiments, refining equipment to gather more reliable data, devising new means of data analysis, and applying for funds to run more experiments. The skills necessary to engage in conceptual analysis—to explicate the meaning of fundamental concepts, to distinguish between substantive scientific issues and vacuous verbal disputes, or to contrast different theories—will tend to be less developed. This asymmetry is harmless and perfectly legitimate at the level of individuals because, varying in their talents, scientists naturally invest more where they expect the highest returns. But the asymmetry becomes a serious problem when it characterizes a scientific community.

In what follows, we provide four *thematic* examples of distortions with two goals in mind, to call attention to a problem that, unlike the previous ones, has not been examined in detail, and to illustrate how distortions stem from insufficient conceptual analyses. The examples are based on the work of Piaget and Skinner—although other choices would certainly be possible—because both authors advanced ideas that are hard to evaluate, and hence, without careful conceptual investigations, easy to distort.

Positive false assertions. An extreme type of distortion consists of declaring that some approaches say or assume what in fact they do not say or assume. In this case, more historical investigation of an author's work would lessen the problem. To illustrate, it is often said that Piaget's theory is an extreme competence theory (Fischer, Bullock, Rotenberg, & Raya, 1993) because it states that the way a child solves an operational task depends only on his stage of cognitive development and not the features of the task. Having found that the task does affect children's performance, some authors have concluded that their findings undermined Piaget's theory: "contrary to [Piaget's] operational theory, a situation may facilitate a subject's ability to solve a problem while it may hinder another's" (Larivée, Normandeau, & Parent, 1996, p. 31). This conclusion is flawed, however, because Piaget never claimed that the child's performance on operational tasks was independent of the particulars of the task: "It is clear that in each task there intervenes a multitude of heterogeneous factors such as the words we use, the length of our instructions. . . . Therefore . . . we never attain a measure of comprehension in a pure state, but always a measure of comprehension relative to a given problem and a given material" (Piaget & Szeminska, 1980, p. 193; see also Lourenço & Machado, 1996, for more detailed analyses of this and other examples).

Negative false assertions. The converse type of distortion occurs when one states that a theory does not address a particular issue when, in fact, it does. For example, Skinner (1938) found that if a hungry rat receives a pellet of food when it presses a lever after, say, 2 min have elapsed since the last pellet, then its response rate increases with the passage of time. Anderson (1995, p. 22) comments: "Skinner was not interested in why the organism behaved in this way; he was content with knowing what kind of behavior could be expected from many different organisms (including humans) given a fixed-interval schedule." This is a gross and misleading distortion because it ignores Skinner's attempts to explain the progressive increase in response rate, the scalloped function. Together with Ferster (see Ferster & Skinner, 1957; also Skinner, 1938), Skinner advanced several explanations such as the discriminative properties of the number of responses emitted during the interval, the cuing function of the pellet itself, and the conditioning of changes in response rate (instead of single responses). Regardless of how crude they may seem today, these attempts certainly qualify as an interest in knowing "why the organism behaved in this way."

Partial interpretation. Another typical way of distorting takes a partial interpretation of a theory as the theory itself. In this case, closer analysis of the meaning of key concepts, the role they play in the theory, and how they evolved through time would avoid the distortion. For example, although Piaget explicitly recognized that his structures-of-the-whole were formal criteria used to analyze the level of organization involved in distinct forms of knowing (i.e., preoperational, operational, and formal thinking), some psychologists interpreted those structures as functional antecedents causally responsible for knowledge, and took for granted that their interpretation of the theory was the theory itself. Contrast Corrigan's (1979, p. 620) words, "the structuralist position taken by Piaget and his followers is that synchrony between domains is a fundamental principle because overall structures *explain* [similar] functioning in many different areas" (italics added), with Piaget's, "it is necessary to insist vigorously that stages [and structures-of-the-whole] do not constitute an end in themselves. I compare them to zoological and botanical *classification*, which is preliminary to analysis" (Piaget in Osterrieth, 1956, pp. 56-57; italics added). Similarly, many authors believe that Skinner denied the existence of mental states and opposed the development of theories, and that such interpretations are the only accurate representation of his viewpoints (see Modgil & Modgil, 1987; Richelle, 1993). Yet, in a substantial portion of his writings, Skinner tries to explain what mental states *are*, how humans learn to talk about them, and what their role is in an explanation of behavior (Skinner, 1945, 1974, 1978). Regarding his presumed atheoretical stance, Skinner stated numerous times that "experimental psychology is properly and inevitably committed to the construction of a theory of behavior. A theory is essential to the understanding of behavior as a subject matter" (Skinner, 1972, p. 302).

Figure-ground reversal. Another type of distortion happens when we present the peripheral concepts of a theory as its core concepts or, conversely, when we trivialize and push to the periphery concepts that are central in the theory. In this case, the distortion could be avoided by investigating how two or more concepts are interrelated and prioritized in a theory. Baillargeon's (1987) criticism of Piaget's developmental theory illustrates the former case because it takes as central the concept of age-of-acquisition, whereas for Piaget the core concept in development is the sequence-of-transformations (see Lourenço & Machado, 1996; Smith, 1991). The converse case is illustrated by a variety of criticisms of Piaget's theory (e.g., Siegal, 1997), which contend that many of his techniques (e.g., repeated questioning, verbal justifications, and counter-suggestions) should be abandoned because they entail linguistic competencies that impede the child from revealing his "true" operational knowledge. However, if we realize that Piaget's operational understanding entails more logical necessity than empirical truth, then we cease to question the role of such procedures in the assessment of children's cognitive development and even see them as central for that purpose.

Although the types and instances of distortion could be multiplied manifold (see also Rotter, 1990; Todd & Morris, 1992), a taxonomy of distortion is beyond our current goals. Instead, we stress the idea that distortions have serious consequences. First, they contribute to conceptual confusion, which in turn increases the number of otherwise unjustifiable publications. Second, they increase disunity and fragmentation in the field. Third, they become the foundation of a shared core of false beliefs about a theory, a sort of scientific folklore that is perpetuated in textbooks and classroom lectures. These three consequences are yet another cost of an epistemic state in which factual discovery eclipses theoretical and conceptual investigations.

In conclusion, we claim that a voluminous productivity should not be confused with progress, that the preference for tabular asterisks should not be confused with respect for Nature as the ultimate Court of Appeals in scientific matters, that the fragmentation of the field and the artificial specialization of its members should not be confused with the division of labor that exists in sciences such as physics and biology, and that distortion and mischaracterization should not be confused with the pluralism and diversity required for scientific progress. Instead, we see these characteristics of contemporary psychology as expressions of an epistemic state wherein factual investigations have been elevated at the cost of theoretical and conceptual ones.[3]

[3] Burnham (1987) has shown that the popularization of psychology during the 20th century derailed into trivialization, that is, into reducing the context of science to "facts", to isolated bits and pieces of information, to snippets. Judging by the current scenario, however, we believe that obsession with factual investigations and its products was not restricted to the popularizers of the new science but came to characterize also a large fraction of its practitioners. That is, the effects of the popularization spread both outwards into the public and inwards into the community of psychologists.

Part II: Psychology's Infatuation With the Scientific Method

> The existence of the experimental method makes us think we have the means of solving the problems which trouble us; though problem and method pass one another by. (Wittgenstein, 1958, p. 232)

To avoid misinterpretations, we state from the outset that our goal here is not to offer a *causal* account of the preceding problems. To do that we would need to analyze the numerous sociological, historical, economical, and psychological factors that shape the daily practices of psychologists, including the promotion criteria used by colleges and universities, the policies followed by funding institutions, the peer review process, the characteristics of the teaching of psychology and of its students, the history of psychology, and even the history of science in general (for some of these analyses, see Burnham, 1987; Ghiselin, 1989; Machado & Silva, 1998; Peters & Ceci, 1982). Instead, our goal is to identify and evaluate the fundamental assumptions underlying the shape of psychology's epistemic triangle.

Our basic hypothesis is this: The privileged status of factual investigations in psychology stems from (a) its excessive reliance on the power of rules of logical inference to discover empirical truths and (b) its enduring distrust of philosophy and all matters that appear metaphysical (see Schultz & Schultz, 1996; Stanovich, 1998). These are the two sides of the same coin; one shows psychology's obsession with what came to be known as *the* scientific method, the other its suspicion of any claims to knowledge not based on the scientific method.

The Grounds for the Hypothesis

Historians have traced psychology's infatuation with the scientific method to the last quarter of the 19th century, the very beginnings of the science (Burnham, 1987). Since then this infatuation has remained basically unchanged. In 1933, for example, Oakeshott (1995, p. 239) remarked: "What, I suppose, is generally taken to be characteristic of the emancipation of modern psychology is the predilection it has shown for the experimental method, and it is even believed that psychology has become scientific because and in so far as it has become experimental."[4] In the 1980's Koch (1981, p. 258) blamed psychology for inventing a "sacred, inviolable "self corrective" epistemology that renders all inquiry in the field a matter of application of rules which preguarantee success," and Hogan concurred (1982): "Academic psychology seems peculiarly prone to what medieval scholars called the fallacy of dogmatic *methodism* that is, when a problem is analyzed by the proper method, truth will somehow inevitably emerge" (p. 216). More recently, McCall (1994) voiced a similar idea: "We [psychologists] need to invest more

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[4] The author continues "But it is not difficult to detect here a misunderstanding. For what constitutes and defines science is never its experimental character, but is character as an attempt to elucidate experience in terms of a world of quantitative concepts." (Oakeshott, 1995, p. 239)

intellectual energy thinking, conceptualizing, and theorizing, to balance our disproportionate obsession with data collection" (p, 293). That the problem is still with us, and with the same strength that it had 120 years ago, is evinced by a recent email announcement heralding the formation of the Comparative Cognition Society (CCS). According to the Society's steering committee, the "CCS will be a nonprofit scientific society with no doctrine or philosophy except the scientific method as it is commonly understood in all natural sciences."

Other aspects show our infatuation with method. Thus, even a cursory review of introductory textbooks readily shows that the scientific method receives much more coverage in psychology than in physics, chemistry, or biology. Again, this is not a recent trend: "In the course of surveying psychology textbooks [from the 19th and 20th centuries], I ultimately stopped my sampling because I had found not a single one in which the author had omitted the exhortatory scientific methods section" (Burnham, 1987, p. 299). Similarly, the few studies on the peer review process have found that, in psychology, the most common reason for rejecting a paper is a methodological flaw, not poor conceptualization or theorizing (e.g., Peters & Ceci, 1982; see also Broad & Wade, 1982; Rosen, 1993).[5] Finally, in a study to determine the concepts psychologists judge to be of sufficient importance, that all students of psychology should know them, Boneau (1990) found that the highest rated concepts came from the Methods/Statistics subfield. (Forty-two of the "Top 100" terms came from Method and Statistics, whereas only one came from Cognition) The author concluded: "Finding 10 or more psychologists who are unanimous about anything clearly represents a major achievement, but it happened in the Statistics subfield!" (Boneau, 1990, p. 893).[6]

The history and teaching of psychology, its peer review process, and even the opinions of psychologists on what is most important in their science and its applications, all point to the central role of method. For method, it seems, stamps psychological research with the prestigious seal of scientific adequacy, and it separates psychology from superstition and faith (see Burnham, 1987; McFall, 1991; Oakeshott, 1995). Any activity that does not use the scientific method, with the exception of formal logic and mathematics, is thereby excluded from the scientific world. Inevitably then, philosophy is denigrated and more so because philosophy was also the mother from which the young child recently rebelled. As

[5] Ghiselin (1989, p. 66) comments on Peters' and Ceci's (1982) study: "The perceptive onlooker will easily see that this should be labeled not a reason, but an excuse. Science deals with uncertainties; and, its methods being fallible, any piece of research might be improved through better techniques. Since everybody thinks he knows a lot about methodology, methodology provides a cheap and easy source of justification for saying 'no'. To evaluate a work on its own merits requires much effort and at least some competence."

[6] In a similar, but less comprehensive study, we polled more than 60 prominent psychologists from all areas of psychology and asked them to answer the following question: "What is the most important subject (broadly defined, e.g., a psychological theory, a construct, a functional relation, a factual discovery) that we definitely should teach all undergraduate students of psychology, regardless of their future careers as psychologists?" Although the most frequent response mentioned issues related to Learning, the second most frequent response mentioned the study of Method and Statistics.

early as 1895, Isaac Cook portrayed psychology and philosophy "in unnatural war, like a child entering suit to overthrow its mother. In some recent presentations [of the new psychology] . . . there is this species of antagonism, with a spirit of matricide as unwise as it is unnatural, ungrateful, and unscientific" (cited in Burnham, 1987, p. 90). Almost 100 years later, similar thoughts were expressed by the founding editors of the journal *New Ideas in Psychology* (1983, p. 1): "In the over-zealous attempt to gain independence from its parent (philosophy) and attain the rigor that might bring it the respect of other disciplines, psychology has tended to expunge creative and speculative thought. . . . Data and data-gathering procedures are the modern priorities."

Given the prominence of the scientific method in the psychological discourse, one is entitled to ask what characterizes that method and how accurate that characterization is. We turn to these questions next.

The Standard Conception of the Scientific Method

Psychology's conception of the scientific method is characterized by a body of ideas in which may be distinguished a backbone and a few appendages. To describe them we will appeal to Stanovich's (1998) book "*How to think straight about psychology*" because it provides a recent, well-known, and clear account of psychologists' shared conception of method. As for the backbone of the conception, it asserts that scientists develop theories on the basis of previous research; extract from the theories (by logical deduction or otherwise) hypotheses or predictive statements about observable events; and construct experiments to test the hypotheses. If the experimental results are negative, the theories are rejected and new ones developed; if the results are positive, the theories gain plausibility and science is one step closer to the truth. Stanovich (1998, p. 15) summarizes: "Science advances by positing theories to account for particular phenomena in the world, by deriving predictions from these theories, by testing the predictions empirically, and by modifying the theories based on the tests (the sequence is typically theory > prediction > test > modification)."

As for the appendages they include (a) systematic empiricism because science is about structured or planned observation of the natural world, not the haphazard collection of facts; (b) replication of experimental findings because "replication of a finding is critical to its acceptance as an established scientific fact" (p. 28); (c) operationalism, the idea that "science advances by developing *operational definitions* of concepts [i.e., correspondence rules linking theoretical constructs to observables]" (p. 37), because without operational definitions and replication knowledge is not publicly verifiable and, therefore, not scientific; (d) the progressive accumulation of knowledge because whenever a new theory is proposed, that theory "must explain all the facts that the old theory could explain *plus* the new facts that the old theory couldn't explain" (pp. 34-35).

Although we have identified the main appendages, we have not exhausted them. It is also said that, unlike essentialists, scientists do not argue about the meaning of words (e.g., what is gravity? what is life?).[7] Also, unlike common folk, scientists do not engage in power struggles to decide between rival theories: "The humanizing force in science is that of making knowledge claims public so that conflicting ideas can be tested in a way that is acceptable to all disputants. Recall the concept of replication from Chapter 1. This allows a selection among theories to take place without a power struggle" (p. 46).

In summary, the standard conception of the method views scientists as following an "experimental logic" (p. 85), operationalizing variables, replicating findings, carefully building and impartially rejecting theories according to the evidence, slowly accumulating knowledge, and inexorably getting closer to the truth. Seductively simple, for sure, but how accurate is this conception? To answer the question we will contrast the standard conception of method with some of the current thinking in the philosophy of science, with the daily practice of scientists (psychologists included), and with the historical record.

Questioning the Standard Description

Philosophy of science. Perhaps the most misleading implication of the standard conception of method is that there is an algorithm that guarantees the discovery of empirical truths. If only scientists follow the linear sequence "theory > prediction > test," operationalize their variables, and choose appropriate designs to test their hypotheses, truth will be seized; perhaps not the full truth at once, but certainly a part of it. Yet, it is a trivial but nonetheless true remark that a study may fail even though it abided by all the textbook rules of good experimentation. For example, after 20 years of heated controversy and more than 400 experiments, it is still unclear why the results on the "transfer of memory" in planarian were sometimes positive, sometimes negative (Travis, 1981; see also Broad & Wade, 1982). Historian of science Gerald Holton (1988) citing Einstein remarks: "There is, of course, no logical way leading to the establishment of a theory but only groping constructive attempts controlled by careful consideration of factual knowledge," and then adds,

> entirely in accord with the honest self-appraisal of an original scientist, Einstein's forthright confession is yet so contrary to the widely current myths which present scientific work as the inexorable pursuit of logically sound conclusions from experimentally indubitable premises. Systematizers, axiomatizers, text writers, and others may yearn for linearized sequences both in scientific work itself and in accounts of it; the truth, alas, is different. (p. 304)

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[7] Did not Newton, for example, sidestep the question of how action at a distance could occur? And has not physics ever since Newton followed his example? And yet we find Michael Faraday, perhaps the epitome of the experimentalist, conjecturing on the nature of gravity and thinking about it in terms of lines of force: "The lines along which gravity acts between the sun and earth seem figured in his [Faraday's] mind as so many elastic strings; indeed he accepts the instantaneity of gravity as the expression of the enormous elasticity of the "lines of weight." Such views, fruitful in the case of magnetism, barren, as yet, in the case of gravity, explain his efforts to transform this latter force." (Tyndall, 1961/1868, p. 151)

It is different indeed. The empirical evidence, for example, does not always reject theories in the logical and dispassionate manner entailed by the standard account of method. In his provocative *Against Method*, Feyerabend (1993) has shown that often new theories are proposed despite available contrary evidence, and old theories are retained even when there are disconfirming experimental findings. Kuhnians also have suggested that incorrect theories vanish only when their supporters die of old age. And Holton (1988) again reminds us of some of the criteria used by scientists to select theories, criteria that typically are overlooked by the standard conception of method:

> This is the characteristic position—the crucial difference between Einstein and those who make the correspondence with experimental fact the chief deciding factor for or against a theory: even though the "experimental facts" at that time very clearly seemed to favor the theory of his opponents rather than his own, he finds the ad hoc character of their theories more significant and objectionable than an apparent disagreement between his theory and their facts. . . . Einstein more and more openly put the *consistency* of a simple and convincing theory or a *thematic conception* higher in importance than the latest news from the laboratory—and again and again he turned out to be right. (pp. 254-255, italics added)

The various appendages of the standard conception of method are dubious for similar reasons. Take systematic empiricism. The problem is not, obviously, that scientists fail to observe the world or experiment with it, but rather that no reference is made to other critical issues, such as the role of aesthetics in the acceptance or rejection of theories and so called ad-hoc hypotheses, or the role of a researcher's reputation in the weight assigned to his laboratory findings. Copernicus rejected Ptolemy's system not on the grounds of disagreement with data but chiefly because it did not please his mind. The same argument applies to operationalism. Once again the problem is not operational definitions per se but what operationalism excludes or remains silent about—the coordination of different operational definitions of the same concept, the logical difficulties with the operational definition of some concepts (e.g., dispositional concepts like "soluble," or "irritable"[8]), and, perhaps more important, the neglect of other properties of scientific concepts such as their quantification and interconnectedness. As for replication, if it were required to establish a fact as scientific, then there would be few scientific facts because rarely is an experiment exactly replicated. The reason for this is not hard to find: Scientists are rewarded for obtaining novel findings, or improving upon those already available, not for reproducing someone else's results. Hacking (1983, p. 231) remarks:

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[8] Some of these problems are discussed by Klee (1997) who comments: "Operationalism did not last long in the physical sciences; but, for reasons that continue to puzzle philosophers of science, it survives to this day with considerable influence in the social and behavioral sciences (especially psychology), where the methodological cry to 'operationalize your variables!' persists among practitioners in certain quarters despite the problems with operationalism." (pp. 53-54)

> Folklore says that experiments must be repeatable . . . roughly speaking, no one ever repeats an experiment. Typically serious repetitions of an experiment are attempts to do the same thing better—to produce a more stable, less noisy version of the phenomenon. A repetition of an experiment usually uses different equipment. There are cases from time to time when people simply do not believe an experimental result and skeptics try again.

In summary, each and every one of the core ideas of the standard conception of method has been seriously questioned. But it is also the case that no alternative formulation of method has emerged from the criticisms of the standard conception by philosophers. As Hacking (1996, p. 51) put it: "There is no longer any consensus on what the scientific method is." From the limitations of the standard conception of method and the absence of any consensual alternative, we draw two conclusions. First, there is no valid reason for holding a dogmatic view of method—in a court of law, the preceding arguments would provide sufficient grounds for reasonable doubt. Second, perhaps it is time for researchers concerned with method to assume less and look more into the actual practices and behaviors of scientists and into the historical record.

The behavior of scientists. The standard conception of method also fails to accommodate the complexity and variety of the behavior of scientists, particularly the skills involved in different types of research. Consider, for example, Blough's (1958) study on dark adaptation in pigeons, that is, on how the birds' threshold of light intensity changes with time spent in the darkness. The experiment required great skill in setting up the apparatus, training the birds to peck at one key if they saw a light projected on a stimulus display, to peck at another key if they did not see the light, adjusting the reinforcement schedules to maintain good discrimination, changing the light intensity according to the birds' most recent behavior to track the current threshold, and so on. In Blough's study there was no theory being tested, corroborated, or rejected by the experimental findings. There was only an attempt to obtain a complex functional relation between two variables.

Similarly, Piaget rarely if ever tested a theory or used experimental designs, but neither did he try to obtain functional relations. Instead, he carefully prepared a set of tasks, interpreted children's answers, and classified their distinctly different ways of thinking. He and his collaborators needed great skill to choose the tasks (to assess, e.g., the child's conception of time; see Piaget, 1969/1927) and to expose the logics of the children's justifications while not leading them on. But how do we understand Blough's or Piaget's styles of research, to use Hacking's (1996) expression, in the light of the standard description of method? Do we say that Blough used reinforcement, extinction, schedules of reinforcement, chaining, stimulus discrimination, generalization, *and* the scientific method? Or that Piaget used classification and taxonomy of distinct forms of reasoning *as well as* the scientific method? Clearly not, and yet we do not hesitate in recognizing as science what each researcher did.

The historical record. The standard account of the importance of the scientific method in promoting scientific progress also is at odds with the history of science (see, e.g., Bauer, 1992; Broad & Wade, 1982; Brush, 1974; Butterfield, 1957; Feyerabend, 1993; Gellner, 1984; Hacking, 1983, 1996; Holton, 1988; Hull, 1988; Leahey, 1991; Travis, 1981; Ziman, 1978). It is not clear, for example, that one could explain the achievements of the scientific revolution of the 15th-17th centuries by appealing mainly to better experimental procedures (think, e.g., of the principle of inertia), or that one could separate alchemy from chemistry, or psychology from parapsychology, mainly on the basis of the standard conception of the scientific method. Laudan (cited in Hacking, 1983, pp. 14-15) summarized the consensus based on historical evidence as follows:

1) Theory transitions are generally noncumulative, that is, neither the logical nor empirical content (not even the confirmed consequences) of earlier theories is wholly preserved when those theories are supplanted by newer ones.

2) Theories are generally not rejected simply because they have anomalies nor are they generally accepted simply because they are empirically confirmed.

3) Changes in, and debates about, scientific theories often turn on conceptual issues rather than on questions of empirical support.

4) The specific and "local" principles of scientific rationality which scientists utilize in evaluating theories are not permanently fixed but have altered significantly through the course of science.

5) There is a broad spectrum of cognitive stances which scientists take towards theories, including accepting, rejecting, pursuing, entertaining, etc. Any theory of rationality which discusses only the first two will be incapable of addressing itself to the vast majority of situations confronting scientists.

Each of these points undermines or seriously questions a major tenet of psychology's standard conception of method. Notice in particular Laudan's third point, the crux of our essay. The standard description of the scientific method does not recognize the role of conceptual investigations in promoting scientific progress. Hence, it is incapable of alerting us to conceptual vagueness and ambiguity or to logical and semantic pitfalls; even when these problems are identified, the method remains powerless to correct them. We will return to conceptual investigations in the last part of the essay.

The limitations of the standard conception of the scientific method in psychology may also be inferred from what otherwise would remain a puzzling observation. Individually, psychologists invent rigorous hypotheses, define precisely the variables they plan to measure, design sensitive tests, carefully observe and quantify behavior, and submit their findings to statistical analyses. Each psychologist claims, quite legitimately, that he or she is behaving scientifically. And yet, the sophistication of quantitative descriptions in psychology rarely matches the accuracy of theoretical predictions; insights, theories, and paradigms earn limited consensus; working models do not handle well empirical details as soon as the original domain of the model is extended to include new territory. In short, psychologists follow the scientific method but seem not to obtain its putative automatic results. This puzzling observation was vividly captured by Richard Feynman (1985) when he compared experiments in psychology to a cargo cult science: Having watched planes land and unload valuable merchandise during the war, islanders in the South Sea carried out exactly the same actions after the war (lit fires along the runway, put on wooden "headphones," and the like) and then waited for the planes that never came.

Psychology, Superstition, and the Scientific Method

Finally, we note another limitation of the standard conception of method, its inability to counteract prejudice and irrationality. Psychologists have often tried to correct erroneous common-sense beliefs, to fight superstition, and to expose charlatanism and fraud. In this noble struggle to educate and protect the public at large, the scientific method is often brandished as psychology's most effective weapon. Unfortunately, however, successes seem modest at best, nonexistent at worst (see Stanovich, 1998). One reason for this state of affairs is not hard to find, although it may be hard to acknowledge by psychologists: The discipline has a credibility problem because it has promised more than it has delivered. For example, "paradigmatic revolutions" in psychology have succeeded one another at a pace unheard of in any other science, but the succession of "paradigms" has not been accompanied by a similar (and, according to the standard account of method, predictable) accumulation of knowledge. Therefore, those who define science by its *accomplishments* rather than by its method have concluded that psychology simply is not a science (e.g., Andreski, 1972; Bauer, 1992; Koch, 1969; see also Horgan, 1999; Wade, 1982). To appreciate the difference between these two viewpoints, contrast the following remarks. The first is offered as a prescription for solving the problems of clinical psychology, a field where prejudice and superstition have always found fertile ground:

> [It is a] mistaken notion that science is a set of answers, rather than a set of processes or methods by which to arrive at answers. Where there are lots of unknowns—and clinical psychology certainly has more than its share—it is all the more imperative to adhere as strictly as possible to the scientific approach. (McFall, 1994, p. 77)

The second viewpoint questions the existence of *the* scientific approach—should we embrace Carnap's version of it, or Popper's? Kuhn's or Feyerabend's? Lakatos' or Bruno Latour's?—then substitutes an emphasis on accomplishments for an emphasis on method as the hallmark of science and finally reaches a disturbing conclusion:

> We shall know that the social sciences [psychology included] have become scientific when their practitioners no longer claim that they have at long last stolen the fire [the scientific method], but when others try to steal it from them; when the philosophy of social science becomes a search for an ex-post explanation of a cognitive scientific miracle, rather than for a recipe or promise for bringing it about. (Gellner, 1984, p. 585)

In our view, the cognitive miracle consists in replacing the variety of facts by the variation of functions, metaphorical statements by abstract concepts, and picturesque descriptions by quantitative laws and powerful theories. Only then can we realistically expect victory in the struggle against prejudice and irrationality, for it seems to us that in any natural domain the amount of superstition is inversely related to the amount of scientific knowledge.

Summary

We have argued that the prominence of factual investigations in psychology stems from the belief that method can accomplish a gigantic feat—attaining empirical truths alone. Add to that belief a narrow view of method, one that overlooks some of the fundamental attributes of science, and a conclusion is forced upon us: The ruling conception of method in psychology is a Cyclops. Deeply suspicious of philosophical analyses, the Cyclops believes that more data will automatically spawn new concepts and improved theories. A reader of a previous version of this essay expressed it this way "[the emphasis on data collection in psychology] is as it should be . . . [It] is no different than publishing Hubble space telescope images. The theories will not be far behind." But the Cyclops' vision is not shared by philosophers of science, does not resolve the complexity and variety of scientific research practices, and seems to be at odds with science's historical record. In the end, the view of method promoted in psychology, described in its textbooks and taught in its courses, may be no more than a chimera arrogating to itself more importance than it is entitled to.

To some psychologists, however, the preceding analogy is misguided. For them, the standard conception of method is akin not to a Cyclops but to something more familiar, a good caricature. As the caricature artist deliberately exaggerates the distinctive features of a face to achieve some practical purpose (e.g., to ridicule, to amuse), psychologists have also deliberately exaggerated the most distinctive features of the scientific method to achieve a practical purpose, namely, to educate the masses of students that nowadays flock psychology courses, students that typically have little knowledge of experimental science or mathematics and seldom intend to pursue a career in science. Moreover, they argue, the caricature is also harmless because it does not represent the practicing scientists; these have always known that textbook descriptions of method compare poorly with their scientific know-how. In sum, although the standard conception of method may be of questionable value in the eyes of the philosophers, sociologists, and historians of science, it remains a useful caricature for pedagogical purposes, a caricature that no experimental psychologist mistakes for a self-portrait. Our criticisms of the standard conception of method are therefore without merit.

We disagree. First, the Cyclopean conception of method in psychology has a long history, one that cannot be explained by the relatively recent pressures to educate large populations of students. Second, as we showed before, the Cyclopean view of method is often expressed in contexts that have little to do with teaching (e.g., in the reasons invoked to reject a manuscript or a grant proposal; in the excessive number of fact-gathering studies). Third, it is hard to believe that current researchers have simply dismissed as a caricature the conception of method to which they were exposed during their student years. In other words, it is hard to believe that the teaching practices of psychology instructors have been so ineffective. Finally, perhaps the strongest piece of evidence that the caricature is indeed a self-portrait is the conspicuous absence of conceptual issues in standard accounts of method in psychology, in the training curricula of psychology students, and in mainstream psychological research. As we show below, it seems that in psychology a methodologically sound study is readily published regardless of the ambiguity, inconsistency, and even absurdity of its conceptual content.

Part III: Conceptual Investigations: What They Are And Why We Need Them

> The increase of the conceptual clarity of a theory through careful clarifications and specifications of meaning is . . . one of the most important ways in which science progresses. (Laudan, 1977, p. 50)

In the first part of the essay we argued that a variety of well-known problems in psychology reveal an epistemic triangle in which factual investigations overshadow theoretical and conceptual investigations, particularly the latter. In the second part we argued that a narrow view of the scientific method might be the main operative force distorting psychology's epistemic triangle. In the third and final part of the essay, we elaborate on the nature of conceptual investigations, distinguish them from theoretical investigations, explain why they are important in psychology, and then illustrate the various forms they may take.

Conceptual Versus Theoretical Investigations

Conceptual investigations are closely related to theoretical investigations, but should not be confused with them. On the one hand, conceptual investigations are always relative to a particular theory. They target a theory as factual investigations target empirical problems. Hence, in the same sense that factual investigations cannot be carried out in the absence of an empirical problem, conceptual investigations cannot be carried out in the absence of a theory. On the other hand, precisely because conceptual investigations target theories as objects of analysis, they should not be confused with them. A theoretical investigation has as its object an empirical domain and, broadly speaking, the theoretician aims at developing a set of principles that will permit anyone acquainted with them to reconstruct the relevant empirical relations, understand these relations, summarize them in economical ways, and perhaps even discover new ones; in turn, a conceptual investigation has as its object the result of the theoretician's work, in particular the core concepts of the theory, their meanings, and their grammars. Newton's mathematical theory of fluxions and his theory of mechanics epitomize theoretical investigations, whereas Bishop Berkeley's criticism of Newton's concept of infinitesimal ("the ghost of departed quantities") and Leibniz's criticism of Newton's concept of absolute time in the *Principia* epitomize conceptual investigations.

Another reason to distinguish conceptual from theoretical investigations is that what counts as a theory varies widely among scientists. At one end of the spectrum, which we may label "theory in the strong sense," only a system in which fundamental and derived principles are clearly differentiated and linked by formal rules of inference, a hypothetico-deductive system in other words, qualifies as a theory. In this case, a theoretical investigation consists of constructing, refining, or testing the hypothetico-deductive system, activities masterfully illustrated by Newton's derivation of Kepler's laws of planetary motion from his fundamental principles of mechanics. At the other end of the spectrum, which we label "theory in the weak sense," any set of loosely interrelated verbal statements about an empirical domain already qualifies as a theory (think, e.g., on what is usually called "attachment theory," "reinforcement theory," "attribution theory," or "cognitive dissonance theory"). In this case, there are no clear distinctions between fundamental and derived principles, and because the theory is insufficiently elaborated, researchers do not derive predictions from the theory by using formal rules of inference (how they do it will be addressed below). It is also the case that what qualifies as a theory changes across time. But regardless of how conservative or liberal our definition of theory tends to be, it is always possible to place a theory under a conceptual microscope to probe not its empirical adequacy—the role of a theoretical investigation, but the intelligibility of its concepts—the role of a conceptual investigation.

Conceptual Investigations are Grammatical Investigations

The closer a theory is to the weak end of the spectrum described above, the greater the need for conceptual investigations of the theory. The reason for this conclusion is straightforward. Lacking quantitative concepts and explicit principles and laws, a weak theory requires the assistance of extraneous factors to regulate the use of its concepts—extraneous in the sense that they are not an explicit part of the theory. One of the most important of these factors is the pattern of use in everyday language of the concepts of the theory, what we might call following Wittgenstein their "conventional grammar." In weak theories then, conventional grammar plays the role that scientific principles and laws play in strong theories. However, this grammar is seldom analyzed, for we learn to speak and understand a language and use its concepts appropriately in a bewildering variety of contexts, but not to analyze the language's semantic patterns. This then is the major reason for a conceptual analysis in weak theories.

The importance of such analysis is illustrated in the following example. A monkey receives a piece of banana whenever it points to one of two cups. After it learns the correct choice, the experimenter replaces the banana with a piece of lettuce. The monkey shows disgust toward the lettuce and does not eat it. One account of this experiment states that during the original training the monkey formed a "banana expectation." When the rewards were changed, the expectation was violated and therefore the animal was surprised and frustrated, and refused to eat the lettuce. A conceptual analysis of the foregoing account could start with a set of questions. On what grounds does one move from the violation of an expectation to surprise and frustration, and then to a behavioral response? More generally, what justifies the way the concepts of expectation, surprise, and frustration are interrelated in this account? The account is not embedded in a theory whose explicit principles and laws provide the logic to coordinate the concepts. Instead, the coordination—in the form of a chain of inferences—is based on (*one* of) the everyday meanings of the concepts of expectation, surprise, and frustration. A conceptual investigation of this account would then attempt to explicate these meanings, distinguish them from alternative ones, and identify how the meanings are coordinated.

The preceding argument may be further clarified by contrasting the use of the concept of expectation with the concept of force. To do that, let us attempt to locate each concept on a plane defined by two orthogonal axes, one representing the empirical dimension of the concept, the other its analytical dimension.[9] The empirical dimension of the concept of force can be measured by the extension of a spring or the deflection of a needle under specific conditions. Similarly, the empirical dimension of the concept of expectation can be measured by a change in response rate, also under specific conditions. In either case, the empirical dimension can be measured by one of the operational definitions of the concept. Consider now the analytical dimension. In the case of force it is given by the mathematics of vector calculus, but in the case of expectation we are hard-pressed to find anything that fits. Obviously, the calculus of logic or mathematics does not (presently) apply to expectation. And yet we use the concept in theories and derive predictions with it. What enables us to do that? If not formal analytical ratiocination, then what? We have argued that it is the everyday grammar of the concept. Expectations are assumed when regularities occur, for instance, when pointing to a cup is regularly followed by a piece of banana; presumably they are violated when the regularity is broken without warning, and they may be positive or negative. The aim of a conceptual investigation is to make explicit the grammar of the concept.[10]

[9] We have borrowed this idea from Holton (1988).

[10] For example, suppose the monkey had eaten the lettuce. Would the experimenter then conclude that no "banana expectation" had been formed? And what would the answer to this question reveal about the grammar of the concept of expectation? A negative answer such as "No, the animal formed

Consider a second example. A researcher claims that a person attains only 85% correct responses in a temporal discrimination task because occasionally the person does not pay attention to the duration of the sample stimulus. A conceptual investigation would probe the explanation by asking, "What does it mean to pay attention to incoming temporal information, say, the duration of a stimulus?" The question does not ask for the definition of the word attention, much less for the essence of attention, but for a clarification of the grammar of the concept in this unfamiliar situation. Compare the use of the concept of attention in this case with its use in another, more familiar case, one that may actually serve as a paradigm for the use of the concept in everyday life: While juggling three balls, someone calls your name; you look aside and let one ball fall to the ground. You explain that "I let the ball drop because I was not paying attention—I did not see it falling." The conceptual investigation may reveal that the concept of attention was simply extended from this paradigmatic case to the temporal discrimination case. Contrast, "Pay attention to the ball! It's falling." or "Look at the ball! It's falling." with "Pay attention to time! It's passing." or "Look at time! It's passing." But if it is reasonably clear what it means to attend to a ball, it is unclear what it means to attend to time or duration. The issue therefore is not one of definitions but of propositions and their logic. One benefit of a conceptual investigation may be to reveal that what seemed to be a genuine explanation was only the illusion of one.

The preceding analyses carry an obvious implication for psychology: Because most psychological theories are instances of weak theories in the sense defined above, conceptual investigations are fundamental to the progress of the science. Quine (1936) expressed a similar idea when he remarked that "the less a science has advanced, the more its terminology tends to rest on an uncritical assumption of mutual understanding." (p. 90). The purpose of conceptual investigations is to challenge this uncritical assumption.

Conceptual Investigations as a Weeding Activity

The results of conceptual investigations often have a negative flavor because they are more likely to identify error, expose incoherence, or reveal nonsense, than to find the truth or suggest how it may be found. The negative flavor of conceptual investigations was stressed by Bouwsma (1982, p. 10) when he described them as "a form of purification, intellectual purification" and by Bachelard (1975, p. 18) when he described them as "intellectual and affective catharsis."

For some researchers, the negative flavor of conceptual analyses suffices to devalue them, for if nothing constructive seems to come from their exercise, then surely they must be worthless. In a related vein, conceptual investigations have also been dismissed as philosophical speculation alien or even inimical to science,

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the 'banana expectation' even though it ate the lettuce" would reveal an obvious missing link, how the expectation relates to overt behavior. An affirmative answer such as "Yes, no 'banana expectation' was formed and that's why the animal ate the lettuce." would reveal the circularity and emptiness of the concept of expectation, for in this case the concept would have added nothing to the animal's overt behavior.

as misguided attempts to circumvent empirical research, a sort of shortcut in the path to the truth, or as armchair speculation about the meaning of words. We detect in these arguments, often associated with the standard conception of method, a questionable psychological attitude and a serious equivocation. The psychological attitude may be called the Thumper complex in honor of the bunny in Walt Disney's animated cartoon, Bambi, who is often reminded by its mother that "If you cannot say anything nice, don't say anything at all." Although of some potential value in other areas of life, this attitude is counterproductive in science, for science is as much about the generation of novel ideas as about their subsequent selection. By revealing obfuscation, inconsistency, and nonsense, conceptual investigations contribute *positively* to the selection process.[11]

On more serious grounds, the preceding arguments also fail to distinguish the aims of conceptual investigations from the aims of empirical and theoretical investigations. As we said earlier, conceptual investigations probe theories and accounts not in terms of how accurately they predict empirical relations, but in terms of their overall intelligibility and coherence, the clarity of their categories, or the boundaries of their sensible domains. Conceptual investigations are not, we repeat, substitutes for empirical searches for the truth. And as for the claim that conceptual investigations are mere philosophical speculation, metaphysical verbiage perhaps, we reply that the unprejudiced reader will have to judge each case on its merits and decide whether the conceptual investigation is any more metaphysical than the theory it scrutinizes.

We mentioned that Berkeley's criticism of Newton's concept of fluxion illustrates a conceptual investigation. Note that Berkeley was not attempting to develop an alternative or improved mathematical calculus, but simply to probe the coherence of Newton's own concepts (fluxions, moments, infinitesimals, etc.). Similarly, when Catania (1975) analyzed the concept of self-reinforcement, he was not proposing an alternative theory of how, when, or where people or animals reinforce their own behavior, but simply probing the (in) consistency of the concept at the light of current reinforcement theory. Interestingly, the reasons advanced by Berkeley for his investigations are perhaps one of the best definitions we have found of the aims and contents of conceptual investigations. He said,

> Whereas then it is supposed that you [the mathematician] apprehend more distinctly, consider more closely, infer more justly, and conclude more accurately than other men, and that you are therefore less religious because more judicious, I shall claim the privilege of a Free-thinker; and take the liberty to inquire into the object, principles, and method of demonstration admitted by the mathematicians of the present age, with the same freedom that you presume to treat the principles and mysteries of Religion; to the end that all men may see what right you have to lead, or what encouragement others have to follow

[11] Horgan (1999) suggests that in sciences dealing with human nature the standards of proof should be the opposite of those found in courts of law—theories should be presumed guilty (wrong or dubious) until their correctness is established beyond a reasonable doubt. One of the criteria of correctness, we claim, is the ability of the theory to survive a conceptual investigation.

> you. . . . But whether this method [Newton's method of fluxions] be clear or obscure, consistent or repugnant, demonstrative or precarious, as I shall inquire with the utmost impartiality, so I submit my inquiry to your own judgment, and that of every candid reader. (Berkeley, 1734)

Conceptual Investigations Come in Many Guises: Two Sets of Examples

Having laid out the major features of conceptual investigations, we proceed to illustrate with two sets of examples some of the forms they may take. By the form of a conceptual investigation we mean the potential source of conceptual confusion it scrutinizes. The first set of examples illustrates the ambiguities occasioned by the excessive use of metaphors to explain behavior, and the second illustrates the problems caused by a mix-up of language games. Obviously, other sources of confusion exist, and an in-depth conceptual investigation of a specific theory is likely to examine several sources at once (e.g., Koch's 1954 review of Clark Hull's theory[12]). Hence, our two sets of examples should be taken only as illustrations. In either case we have opted to identify a number of instances in which the same problem recurs instead of analyzing in detail one single instance of the problem. This strategy also reveals another benefit of conceptual analyses, which is to identify a source of potential problems in a variety of research domains.

Example I. Metaphor: a double-edge sword? When learning is conceived as a *mapping* from examples to concepts, rules, and results; when the stages of language acquisition are conceived as stages in *deducing* a grammar; when understanding a text is conceived as *activating* effortlessly the meanings of the words of a mental dictionary composed of tens of thousands of words; when solving a problem is conceived as *following internal rules*; when the spatial abilities of rats are conceived in terms of *scanning cognitive maps*; and when concept learning in children is conceived as *formulating tentative hypotheses* about the criteria that define a category, *evaluating the hypotheses* against subsequent information, and *revising* them at the light of inconsistent information, then it is clear that in psychology metaphor reigns.

The role of metaphor in science is a complex and controversial issue and it is not our intention here to argue for or against metaphors (for some of these analyses see, e.g., Bachelard, 1968, 1975, 1984; Bunge, 1967, 1969; Cassirer, 1946; Leahey, 1991; Leary, 1990; Leatherdale, 1974; MacCormac, 1986; Soyland, 1994). Instead, we want simply to illustrate how conceptual investigations may expose the ambiguities that arise when metaphors are used without caution. When, for example, they violate a major characteristic of scientific knowledge that Ziman (1978, p. 6) called *consensible* content: "Each message should not be so obscure or

[12] Whewell's (1840/1996) classical work provides one of the best analyses of the role of conceptual issues in scientific progress. The two-volume book is also rich in historical examples from a variety of sciences—Physics, Chemistry, Astronomy, Geology, Biology, etc.

ambiguous that the recipient is unable either to give whole-hearted assent or to offer well-founded objections."

Consider the following account of memory errors: "These memory experiments suggest that script knowledge is used in understanding stories and that the activation of a script and its use to fill gaps in the story leave memory traces that can *become confused* with the memory traces for what was actually read or heard" (Stillings et al., 1987, p. 33; italics added).

Here we have two types of memory traces, those activated directly by reading or hearing the story (the traces that represent the elements of the story), and those activated indirectly as part of the activation of the entire script (the activation spills over to these traces as it were). The person's errors stem from the inability to distinguish the two types of traces. But how should the account be taken, metaphorically or literally? On one hand, it could be taken as a formal description of the person's pattern of errors, as when we substitute, in certain contexts, apples for oranges, or one person's name for another person's name. The error is explained by seeing it as an instance of a pattern of errors. On the other hand, the account could also be taken as identifying an internal cause of the memory errors: memory traces are brain entities that when active determine the person's behavior. But if the overall description suggests the internal, efficient cause interpretation, then in what sense could two memory traces be confused? We certainly understand what it means for a *person* to be confused, but confusion in memory traces is unclear.

The second example is a typical description of propositional networks as models of declarative memory:

> The basic assumption of the theory is that at each moment in time each node in a network is at some level of *activation* and that activation spreads among nodes along the links. If the level of activation reaches a high enough value in some portion of the network, that portion of the network is accessible to conscious attention. The links are therefore *associative* connections that determine the tendency of one item of information to lead to another in thought. . . . Quantitative predictions become possible when concrete assumptions are made about how rapidly activation spreads, how rapidly it decays, and so on. . . . Most cognitive psychologists would want to make a strong claim that the facilities for propositional representation and some sort of associative activation are built into the biology of cognition. (Stillings et al., 1987, pp. 26-35)

Again, we vacillate between metaphorical and literal interpretations of this passage because we do not know whether the authors conceive of the network as a representation that describes structural aspects of the person's knowledge, and that could be invoked as a formal cause of the person's behavior, or as a description of an internal brain mechanism in which activation flows in real time, at a particular velocity, and that therefore could be invoked as an efficient cause of the person's behavior.

Consider a third and more extreme example. Grobecker (1998) offers the following account of the source of mathematical learning disabilities in children:

> I have argued that, from a systems approach to development, the source of learning problems lies in the *equilibration cycle* in that the *spiral of mental activity* in LD [learning disabilities] is not as *expansive* as that of their same-aged peers as it *winds itself upward and outward* by the exercise of its forms. This qualitative difference in the *equilibration cycle* in children with LD creates a *vulnerability* in their system that could easily *magnify* and *disrupt* all developmental levels to follow. For example, their *systems* may experience an *excessive degree of agitation*, when *structures* are *dissipating* their forms of *reorganization* onto higher-order levels. As a result, cognitive systems may favor a return to their *initial state* rather than sustaining the *tension* necessary to reorganize their structures onto higher-order levels. (p. 4, italics added)

Here one is at loss to assimilate the metaphors. In what sense does mental activity expand along a spiral? We understand that the trajectory of an oscillating pendulum plotted in a velocity-position phase-space defines a spiral because the pendulum continuously loses speed due to friction and approaches an equilibrium point in which it is motionless. But how do we go from the pendulum, or any other similar system, to mental activity and its spiraling upward and outward? In the same vein, in what sense is a cognitive system vulnerable and agitated, or dissipates its form of reorganization? Again, we understand that when water is heated the degree of agitation of its molecules (their mean kinetic energy) increases, and that when the heat source is removed, the water dissipates its energy as it cools down. But how we go from boiling water to agitated cognitive structure is unclear. Having failed to understand the main elements of the account, inevitably we also failed to grasp their connections: How or why does a difference in the spiraling activity of the mind create vulnerability in the child's cognitive system? And how or why does the agitation of the child's cognitive system favor a return to an initial state? In each case the problem is the same: A literal interpretation of the concepts and expressions in the contexts where they occur yields gibberish, whereas a metaphorical interpretation yields the illusion that something profound and rigorous was said merely by using a homonym of a technical term. (See Lourenço & Machado, 1999, for an expanded commentary on this issue.)

Interestingly, these examples illustrate the same problem that 18th century physicist van Swinden found when he reviewed the dominant accounts of magnetism:

> This expression: Iron is a sponge for the magnetic fluid is therefore a *metaphor* . . . . And yet, all explanations are based on this expression used in its *literal sense*. From my part, however, I think that it is not correct to say that all Phenomena are reduced to this, that iron is a sponge for the magnetic fluid, and yet assert that this is a deceptive appearance: to think that reason shows these expressions to be misleading, but nonetheless apply them to explain the Experiments. (van Swinden, 1785, cited in Bachelard, 1975, p. 78)

Besides creating confusion, the ambiguity between the literal and the metaphorical may also cast doubts on the causal effectiveness of an account of behavior. For if we say that a rat navigates a maze efficiently because it scans with the mind's eye a stored representation of the maze, a cognitive map, and then admit that there is literally no mind's eye, literally no internal action of scanning, literally no map, at least in the sense that we usually conceive of eyes, scanning actions, and maps, then how does our account *explain* the rat's behavior? Roediger (1980) seems to have identified a similar problem when he reviewed the large number of metaphors for human memory:

> The list of putative physical processes and structures intended to correspond to mental events is discouragingly long. Many of the hypothetical processes have the unfortunate character of referring to human-like activities. Theorists repeatedly have something or someone in the mind storing, searching, matching, locating, identifying, detecting, discriminating, making decisions, and so on. (p. 243)

That this situation—wherein an account presented as only a metaphor for X is in fact the only way in which its author can talk and think about X—may be ubiquitous in some areas of psychology is another compelling reason to promote conceptual investigations.

The preceding examples indicate that metaphorical concepts in psychology often extend beyond the realm of expression and communication. Unfortunately, psychology has few natural defenses against the seductive power of metaphor, its propensity to become autonomous and enslave reason—"Metaphor belongs to the theory of manipulation, in effect to the politics of the mind" (Danto, 1992, p. 74). By contrast, in more mature sciences, formalized theories severely restrict the meaning and the extension of metaphorical concepts (cf., Ohm's law and the concept of electrical *resistance*). This then is another cogent reason for conceptual investigations; they help us clarify metaphors, eliminate ambiguities, and ultimately move from intuitions and images to abstractions. The latter in particular may be critical, for as Bachelard (1968, p. 119) observed, "intuitions are very useful: they serve to be destroyed. . . . The diagram of the atom proposed by Bohr [equating the atom to a solar system] a quarter of a century ago has, in this sense, acted as a good image: there is nothing left of it."

In summary, we do not need to impugn the use of metaphors in science to recognize the dangers that may lurk when they proliferate without control. For if an approach that approves only of literal language stultifies our imagination, an approach that lingers on metaphors gives us only superficial looks of the thing itself. In its rush to steer away from the maws of Scylla, some areas of psychology may have fallen into the whirlpool of Charybdis, from evil into worse. Careful conceptual investigations are necessary to sail past the two monsters.

Example II. The mix-up of language games. Typically when an organism's interactions with the world seem insufficient to explain its knowledge, the origins of that knowledge are searched in innate structures and computations. If language is not learned according to the known principles of learning, then it must be due to an inborn language acquisition device; if the meaning of a verbal utterance cannot be reduced to statements about behavior, context, and dispositions to behave, then it must spring from an inborn language of thought; if differences in the pattern of two stimulus arrays cannot explain habituation/dishabituation, then the 4-month-old infant must innately know a series of physical principles and infer complex propositions according to innate rules of thought. Before one can determine whether any, or all, of these statements are empirically adequate one needs to understand their meaning. Here we immediately face a problem for which conceptual inquiries are not only desirable but also indispensable.

The preceding statements (and many others could be given) share a salient feature. On the one hand, they extend to the domain of the mind/brain concepts that usually belong to the domain of people, their behavior, and its context. On the other hand, they do not explicate the intended new meaning of the constituent concepts. The latter is critical, though, for if it may make good sense to say that a person knows, believes, follows rules, infers, is right or wrong, becomes aware of, is conscious that, and so on, it makes no sense, unless an explanation is offered, to say that the mind/brain knows, believes, follows rules, infers, is right or wrong, becomes aware of, or is conscious that (Machado, 1999; also, Baker & Hacker, 1984; Button et al., 1995).

Consider the following example. A baby's response of looking is habituated by repeated presentations of a ball falling from a certain height. Then, two events follow, one possible, the other impossible. In the possible event, the ball falls and then stops when it contacts a solid surface; in the impossible event, 4-6-month-old babies see the ball passing through the solid surface (Spelke, 1991). It is found that the response of looking at the moving ball dishabituates more to the impossible event, even though apparently its visual characteristics were closer to the initial display. The baby, so goes the argument, is surprised by the violation of the "principle" of object substance and solidity. More specifically, 4-6-month-old babies compute or make inferences on the basis of input data, computations or inferences that are then compared with actual events; mismatches produce surprise and hence the greater periods of time looking at impossible events.

Given the preceding account, we are tempted to ask whether the baby's *mind* can ever go wrong when computing or drawing inferences, for this question is part of the language game that we learned to play with the concepts of "computing" and "drawing inferences". But what would count as "going wrong" in this case? Is there any evidence, independent of the baby's behavior, that a computation or inference was properly executed? Another account might say that the baby's *brain* is hardwired in such a way that an inference occurs when a set of stimulus conditions holds. But this extension to the brain of properties of a person, its behavior and its context, raises further difficulties. For example, could the baby's brain mechanism malfunction and yet give the correct inference, as when an incorrect algorithm yields the correct result because two errors cancel each other? And if the behavioral reaction is absent—in fact some children do not look more to the impossible event—do we then interpret the result as a malfunction of the mechanism or as the absence of the putative mechanism? More generally, how should the familiar concept of inference be used when it is extended to an unfamiliar object like a baby's brain?

As a second example, consider the following account of blocking:

> When the noise was followed by shock during pretraining, the occurrence of the shock would at first have been *totally surprising*, leading to an *active search* for stimuli that could have predicted it. Once subjects learned that the noise was always followed by shock, however, the shock's occurrence would no longer have been *surprising* and thus would no longer have triggered a *memory search* . . . . When the light was added to the noise on the compound trials . . . at first subjects might not *have been certain* that the shock would still follow . . . Once *subjects realized* that the noise was still a reliable predictor, however, the occurrence of the shock no longer would have been *surprising*. . . . Thus, whereas an attentional analysis attributes blocking in this case to a *failure to attend* to the light, Kamin argued that *the rats were fully aware of* the light but that, because the shock was *not surprising*, the *rats made no attempt to form an association* between the two events. (Lieberman, 1993, pp. 458-459, italics added)

Although we may have no difficulties understanding what it means for a rat to "attempt to escape a trap," without further assistance we do not how to assimilate the expression "the rat made no attempt to form an association." What would count as a criterion for the rat's *attempt* to form an association, to be "fully aware of," or "certain that?" And who or what does the memory search: the rat, its mind, or its brain? And is the assertion "Once subjects realized that the noise was still a reliable predictor, however, the occurrence of the shock no longer would have been surprising" susceptible to empirical refutation, or is it true by definition?

The reader may object that the questions raised in both cases are unwarranted, that they are like asking who is doing the pushing, and what for, when Newton's law of universal gravitation is stated. As Leatherdale (1974, p. 186) put it, "a more sophisticated answer might be, 'The word "force" means different things in different contexts. Obviously your questions don't apply in this context'." But this is precisely the point: Unless a conceptual analysis clarifies the new meaning and the new grammar of the terms that have been extended beyond their ordinary boundaries, and identifies the sorts of questions we are entitled to ask and those we are not—in a word, unless we learn how to use the new language—these accounts will remain equivocal and therefore not consensible.[13]

[13] The ambiguity of some writers on these issues is revealed also by the strategies used to avoid clarifying the sense of their concepts. For example, Posner and Raichle (1997, p. 194) describe Baillargeon's experiments thus: "research has been able to show that some brain systems in the infant do 'know' about object permanence considerably earlier than Piaget recognized. . . . Thus, in some sense these infants 'knew' that a hidden object was there and that it should have interrupted the movement of the screen." The terms in quotation marks are never explained.

These difficulties are present also when the behavior of animals is explained by isomorphisms between the animal's brain and its external world. Typically, the isomorphism is only stipulated, not shown, and for that reason one can say that:

> a rich, functioning isomorphism may exist even when the sensory mapping from the external reality to its internal representation in brain activity does not make it immediately clear what exploitable formal correspondences there might be. [In other words, an isomorphism may exist even if we do not know what it is.] Conversely a formally transparent mapping is no guarantee that the brain itself exploits a formal correspondence obvious to the experimenter [In other words, a mapping may exist even if the mapping is inconsequential.] (Gallistel, 1990, p. 24)

More generally, one wonders if a cat who waits for a mouse at the entrance of a hole is going through a process of inference, manipulating propositions and having the thought that the mouse will eventually come out or if the squirrel who buries nuts and retrieves them a few months later is solving Hume's induction problem (see e.g., Hacker, 1987, 1991; Harris, 1987; Malcolm, 1970, 1995).

The examples could be multiplied easily: To describe a synaptic change as a "memory storage," or an instance of remembering as "knowledge deposited in the brain," and yet fail to explain the sense in which changes in ionic channels, dendritic trees, neurotransmitter reuptake times, or membrane conductivities represent the experienced events, may be a crude but harmless *façon de parler*. But to account for cross-modal matching in a newborn in terms of an amodal, language-like system of representations, and yet fail to explain what this private language is about (Kaye & Bower, 1994), or to interpret the performance of a 3- or 4-year-old child in Piaget's number conservation task in terms of a chain of inferences, and yet fail to specify the meaning of an unconscious, complex, syllogistic reasoning process (Dehaene, 1997), is not a harmless *façon de parler* but a source of equivocation.

We fully recognize that as science develops, concepts inevitably change their meanings. The Newtonian concept of force differed from its Aristotelian counterparts, and the same has happened in psychology (e.g., Efron, 1977, Kagan, 1989). As Wittgenstein (1967) remarked:

> nothing is commoner than for the meaning of an expression to oscillate, for a phenomenon to be regarded sometimes as a symptom, sometimes as a criterion, of a state of affairs. And mostly in such cases the shift in meaning is not noted. In science it is usual to make phenomena that allow exact measurement into defining criteria for an expression; and then one is inclined to think that now the proper meaning has been *found*. Innumerable confusions have arisen in this way. (p. 438)

When a scientist extends an everyday concept into a new domain, it is incumbent upon him to explain the new meaning of the concept and how the concept should be used.

Summary

In one way or another conceptual investigations deal with language as it informs our intuitions, observations, theories, and approaches. They attempt to unveil the hidden, confusing, ambiguous, and even contradictory meanings of our notions and accounts. They focus on the comprehension of our concepts and how it relates to their extension. Conceptual investigations are to theory building as methodology is to data gathering. Both are fundamental in science—sound methodologies help us to reconcile things with other things and thereby prevent truth from becoming merely our convention; conceptual analyses help us to reconcile minds with other minds and thereby prevent truth from becoming merely our representation (Bachelard, 1984). Psychology seems to have learned the first but not the second lesson. As our examples document, it is still case that a methodologically sound research article is often accepted regardless of the fragility, ambiguity, or inconsistency of its conceptual scaffolding.[14]

Conclusions

This essay introduced the idea of an epistemic triangle in psychology with factual, theoretical, and conceptual investigations at its vertices. Given the interconnectedness of these three kinds of investigations, scientific progress requires a fine balance among them. Unfortunately, it seems that psychology's epistemic triangle is heavily stretched in the direction of factual inquiries. This unbalance is expressed by the excessive number of empirical publications, the asymmetry between technical sophistication and theoretical poverty, the fragmentation of the field and the artificial specialization of its members, and the frequency of distortions and mischaracterizations of the work of other researchers.

Although some of these problems have been mentioned before, the present essay attempted to understand them as different manifestations of one operative force—the obsession of psychology with an impoverished and mechanical view of the scientific method and a misguided aversion to conceptual inquiries. It may be argued that the problems of psychology are common to other sciences, even the so-called "hard sciences," if not during the 20th century, then perhaps during earlier times. In a word, psychology's problems are the natural expressions of a young science. Although this may be true, the statement seems to imply that psychologists should wait passively for the more mature stages of their science to materialize, as if by spontaneous generation. Alternatively, we believe that psychologists can shape the future of their discipline by acting now. If our thesis is correct, progress in psychology will depend on it adopting a less dogmatic, more comprehensive and realistic view of method, one in which conceptual

[14] More than 250 years ago, Castel had already summarized the spirit of our message: "The method of facts, full of authority and influence, arrogates an air of divinity that tyrannizes our beliefs, and coerces our reason. A man who reasons, or even demonstrates, takes me as a man: I reason with him; he leaves me the freedom of judgment; he does not force me, except by my own reason. The man who cries 'here is a fact,' takes me as a slave." (Castel, 1740, cited in Bachelard, 1975, p. 41)

investigations receive proper weight. This view does not underrate the importance of factual investigations, for these are part of the nourishment essential to scientific growth; nor does it relax methodological standards, for these are like hygienic rules. But in the same sense that healthy growth requires more than prophylactic measures, scientific progress requires more than an accumulation of facts and an algorithmic view of method—in particular, it requires more attention to the meaning of concepts and their grammar.

Although the specific ways to best achieve this aim requires careful study, which is well beyond the purpose of the present essay, we advance the following general suggestions. A richer conception of method may be achieved by studying actual scientific discoveries, the errors made by great minds, both in psychology and in other sciences, and their subsequent correction, as well as by studying the many styles of scientific reasoning—observing and experimenting in the laboratory; interviewing children and adults in clinic-like settings and then classifying and interpreting verbal protocols; building mathematical models, selecting and fitting data at the office desk; testing statistical hypotheses at the computer. The aim of promoting conceptual investigations may be achieved by rewarding psychologists for doing them, not just for collecting new facts, and also by teaching students how to do them. In this respect, the study of the history of science, logic, and analytical philosophy, for example, could prove more beneficial than our typical course on research methods and statistics (see Flavell & Wohlwill, 1969; Machado & Silva, 1998). For if it is certain that conceptual investigations alone will not solve the manifold problems of psychology, it is also certain that without them, no attempt at a solution can possibly succeed.

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