
Psychology's Dilemma: An Institutional Neurosis?



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The term *psychology* refers both to an institutional discipline and to a subject matter. Henriques, in his article "Psychology Defined" (this issue, pp. 1207-1221), emphasizes the second reference, and its focus can be sharpened by taking into account the first reference. On the one hand, epistemic progress in science is a dynamic process, which, as often as not, cuts across institutional divisions. However, on the other hand there are some problems of disunity that solely concern the institution. That the latter falls within the scope of the Tree of Knowledge is illustrated in how Henriques' "Justification Hypothesis" sheds light on the nature of institutional disunity. © 2004 Wiley Periodicals, Inc. *J Clin Psychol* 60: 1237-1241, 2004.

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Henriques (this issue, pp. 1207-1221) suggests that "many of the problems that plague psychology are epistemological in nature" (this issue, p. 1209). Perhaps, but I wonder if the problems so identified are the epistemological ones that the Tree of Knowledge (ToK) System hopes to solve. There are a number of problems in psychology and efforts toward disciplinary unification have not necessarily diagnosed all of their proper causes (Katzko, 2002). Auguste Comte developed a similar ToK and is credited with "inventing" sociology as a discipline, although he neither invented nor discovered the related phenomena. In contrast, one meaning of the term *psychology* is a social organization that already exists. Henriques recognizes this in the first paragraph of his article, and would do well to follow up on some of the implications of this fact. At minimum, a better delineation between psychology as an *institution* and psychology as a *subject matter* is required (Richards, 2002). The consequence of not recognizing the explicit social structure of scientific activity will confound problems concerning institutional functioning with problems concerning epistemological content—the "subject matter" under investigation.

The discipline of psychology has been seriously constrained in its activities by an unrealistic picture of how science works in other disciplines. This is manifest in how

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problems are perceived and defined. The following two claims deserve scrutiny from alternative perspectives: first, that the discipline of psychology has a unique problem of fragmentation; second, that the solution to such a problem is necessarily a unifying theory. Consider the first assertion. What evidence is used to illustrate that psychology is disorganized? Granting that psychology can be described as a "federation of subdisciplines," does this fact adequately constitute a problem of disunity where this federation might "ultimately fragment into a multitude of smaller, more specialized fields" (Henriques, this issue, p. 1207)? Have a look at the journals in any science and you will likely discover a "federation of subdisciplines." As knowledge increases, so does academic diversity. The emergence of institutional designations can be a function of the scope of the phenomenon, the scope of intellectual interest, and the presence of wider social or economic applications (Pickstone, 2000). This can be viewed as an institutional self-organizing process (Wagner-Döbler, 1997) reflecting "the inherent richness of the phenomena, a practical need for division of labor, or simply different interests among investigators" (Katzko, 2002). Thus, one must distinguish diversity from disorganization, and then factor diversity out of the analysis.

There is indeed a problem of disorganization but one that requires a different sort of evidence. Scientific psychology developed out of the philosophical study of mind in the 19th century. The historical record of 20th century American psychology illustrates how the discipline has been driven by ideologies of a methodological or theoretical character. In this regard, the deliberate institutional implementation of a quasi-positivist ideology in psychology has been a methodological disaster on several fronts (Chow, 1996; Danziger, 1990; MacMartin & Winston, 2000; Michell, 1999; Morrison & Henkel, 1970; Stam, Radtke, & Lubek, 2000). In this context, I have argued (Katzko, 2002) that another kind of disunity has been *internally generated* by an inappropriate application of this ideology, and the evidence for it is the rhetorical structure of research articles.

Second, does psychology need theoretical unification? Unification is an ongoing project in science, representing a goal and not a precondition for progress (Katzko, 2002). If such a theory is to be a "fix" for a problem, it depends on the sort of problem at issue. To the extent there is a social/institutional problem, the fix lies elsewhere than in finding some sort of theoretical unity. But, be that as it may, when you examine the process of theoretical advance in science you will immediately notice that it is a synthetic process. Consider a breakthrough in the earth sciences: the theory of plate tectonics (Hallam, 1973). The relations between studies of the micro-phenomena (such as geological morphology, distributions of volcanoes, etc.) and the macro-phenomena that the theory identified (i.e., the "plates") did not involve the replacement of the former by the latter. The micro-phenomena were not explained away. Through the identification of macroscopic structures and processes the distribution of the micro-phenomena served both as evidence for the macro-structures even while the macro-theory was "explaining" the distribution and even the dynamics of the micro-phenomena.

What does plate tectonics have to do with the problems faced by psychologists in general or clinicians in particular? I want to draw attention to the dynamic character of scientific advance that transcends merely institutional designations. The effort to pre-define theoretical unity goes against the natural current of scientific progress. Scientific advance is not adequately portrayed in the supposition that any unifying theory is predicated on an antecedent phase of fragmentation or disorganization. An example of theoretical unification arising out of a precondition of relative theoretical disorder might be Newton's *Principia* or Mendeleev's Periodic Table. Yet even here, especially in the latter case, the theoretical advance required a solid base of accurate and systematic observation. More often, a synthetic process builds on empirical regularities and theories of more

limited scope, and is predicated on the existence of a diverse, hierarchically structured reality. A psychologist would do well to understand this process. For example, one empirical precursor to the theory of plate tectonics took place in the 19th century and was not theory driven at all. Rather, it reflected the nuts-and-bolts empiricism of descriptive generalization, specifically regarding the phenomenon of stratification (Pickstone, 2000, p. 123). Progress in stratigraphy (an Earth Science) developed in parallel with paleontology (a Life Science), built on correlations between fossil distributions and the geological strata. The process was synergistic. Furthermore, both developments provided key chapters in Darwin's argument in *The Origin of Species*. Even with the emergence of such a theory, the influential disciplines continue to function within their own scope of applicability. Indeed, breakthrough theories generally increase scientific diversification by identifying new phenomenal structures and processes, and new linkages among known phenomena.

The practical aspect of unification resides in the willingness of practitioners to both seek out relationships among diverse phenomena and to discuss freely with other scientists from diverse backgrounds. Reality is complex and phenomena are multifaceted. The most effective course of action in other sciences is to be phenomenon-driven. Researchers worry less about boundaries and niches, and use whatever is required to better understand whatever interests them. Rozin (2001) compared psychology's obsession with hypothesis testing procedures with the style of investigation in biology, which he suggested is guided by an "informed curiosity" on the part of the investigator. Similarly, Sternberg and Grigorenko (2001) made a plea for phenomenon-driven inquiry. This creates a fluidity or even arbitrariness in *institutional* designations that reflect a relative emphasis on the facets of the phenomena under scrutiny. Facts and methods of geology are central to archaeology and paleontology. The details of chemistry used by a vulcanologist are different from those used in physiology. And a chemist or geologist need know nothing at all about archaeology, paleontology, or physiology.

This might be reading as a harsh interpretation of Henriques' article. In fact, it is not. However, rather than applying the ToK to solve an epistemological problem, my analysis represents an application of what is termed the "Justification Hypothesis"—a real gem of the exposition—to solve an institutional/organizational problem. Where I might differ with Henriques is that I prefer not to discuss the topic as a set of postulates. The procedural "Achilles' heel" of psychology has been to too quickly translate the presumably elusive phenomena into theoretical presuppositions. Such presuppositions and postulates effectively remove the phenomena into a dry and abstract background. I would rather elevate the "process of justification" to the status of a phenomenon for study.

Given this analysis, psychology as institution can be situated at the fourth level of complexity of the ToK, to be further examined as a rhetorical process of justification. Maxwell (2002) asked the question, "Is science neurotic?" and defined a *rationalistic neurosis* as occurring in situations where any entity capable of pursuing aims and can represent such aims, on occasion misrepresents the aims it is pursuing. As applied to a scientific institution this arises when, for example, an "official" philosophy of science is adopted that is either untenable or does not correspond to how that science actually works. Maxwell observed that the social sciences, in particular, were vulnerable to this rationalistic neurosis. And so it has been with the institutional side of American psychology. Inquiry has been primarily driven by ideological justifications—methodological, theoretical, or both—and resulted in various types of disunity described above (Katzko, 2002).

Here is another application: substitute the target question, "What is Psychology?" with the phenomenon-oriented question, "What are you (the reader) interested in?" If you

can provide an answer, then at one level the epistemological problem is solved. However, when a wider social environment expects justification for your answer, this is where the problem becomes institutional. One can still agree that psychology has been preoccupied with a problem unique to the discipline. However, the problem becomes one where it is, to a certain degree, *believed* there is an epistemological problem. An explanation for this degree of belief can be found within the form of justification used within the institution to define its subject matter. Of course, Henriques is clear in his strong belief that psychology has a problem with its subject matter, and this is crucial to his justification for the ToK System. But what might not be recognized is that the system has its own social-historical bias, apparently developing partly out of the American behaviorist tradition, if only as a response to that tradition. This influences where key boundaries are drawn and how key figures are identified, such as the choice of Skinner. The point here is that Henriques' "justifications" for believing that psychology has a problem with its subject matter is, in part, a product of his particular viewpoint, which is situated in the current sociopolitical climate of American psychology. Yet, there are many historical routes to be followed through the epistemic terrain of psychology. Some of these routes can bypass entirely the specific highlights picked out in Henriques' "Psychology Defined."

But this is less a matter of right, wrong, or incomplete, and more a matter of relevance and applicability. One task for the future development of the ToK is a better specification of and justification for the choices of boundaries and key figures. In this way, the instrumental or heuristic value for those sharing the intellectual tradition implicit in the ToK can be better highlighted. This observation places the functional value of any ToK less on the specific content details, which will always to an extent be personal and historically bounded, and more on the willingness of a practitioner to engage in such an enterprise at all. To the extent that such activity reflects an open mind intent on discovering relationships, then this is what ought to be nurtured and encouraged in the academic community.

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