Toward a Useful Mass Movement

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Psychology has failed to reach its full potential as either a science or a profession. The inability of psychologists to generate a shared, general understanding of their subject matter and fundamental differences between scientific and nonscientific views of human behavior in society at large interact to render psychology's contributions to the world's most pressing problems much less potent than might otherwise be the case. The Tree of Knowledge (ToK) System affords new opportunities both to define the discipline of psychology and to examine the epistemological interrelations between the institution of science and other societal institutions, such as law, governance, health care, the arts, and religion. In this article I articulate how the foundation can be laid for the development of a useful mass movement that could transform the discipline of psychology in a manner that unleashes its constructive potential, while at the same time it attempts to address many of the concerns about the proposal raised by the contributors to these two special issues. © 2004 Wiley Periodicals, Inc. J Clin Psychol 61: 121–139, 2005.

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From 1999 to 2003, I directed a randomized controlled clinical trial exploring the efficacy and effectiveness of a brief cognitive therapy intervention for patients presenting to an emergency department following a suicide attempt. I learned many valuable, eye-opening lessons in carrying out this work. One lesson was that the magnitude and complexity of the problems these participants faced was humbling and, at times, even demoralizing.[1] Another lesson was that the current system was failing these individuals.

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[1]Consider that, for an $N = 180$, the modal number of Axis I diagnoses was three, two thirds had a substance abuse or dependence diagnosis, the mean Beck Depression Inventory scores were in the severe range, two thirds were completely unemployed, a quarter homeless, histories of abuse were the norm, and over 70% made less than $15,000 a year.

Psychiatrists would offer diagnoses and medication management; social workers and counselors would lend a sympathetic ear and perhaps encourage patients to enter a group or music therapy program in the afternoon. With rare exceptions, psychologists were nowhere to be found. Brief hospital stays followed by missed outpatient appointments was the norm.

My sense that there were serious problems with the ecology in which these folks lived and with the health system that was providing treatment was greatly reinforced when I discovered that the degree of psychopathology exhibited by the population of suicide attempters[2] in Philadelphia had markedly increased over the past 30 years (Henriques, Brown, Berk, & Beck, 2004). In the early 1970s, Beck and his colleagues had evaluated hundreds of suicide attempters with the same basic instruments we were using in our outcome trial. I realized that this afforded us a remarkable opportunity for a cohort comparison—and when the comparison was made, the results were striking. On every substantive measure of psychopathology and distress, the present-day suicide attempters were worse. Perhaps the most striking and disconcerting finding was the difference in subsequent suicide attempts. Four times as many present-day suicide attempters made a suicide attempt in the year following the index attempt than was the case in the 1970s (40% compared with 10%).[3]

What does this have to do with my proposal for the Tree of Knowledge (ToK) System and the theoretical unification of psychology? The answer is *everything*. No current, single-school approach has the conceptual sophistication and depth necessary to address the incredibly complex and multifaceted nature of the problems associated with the suicide attempters. In contrast, I believe that the theory of theories system that I am proposing provides the conceptual tools necessary to consolidate and coordinate our knowledge in a manner that will allow us to more effectively address some of society's most pressing problems.[4]

How might this happen? My vision can be spelled out as follows: A shared, general theoretical framework for psychology will lead to a much more coherent and effective institutional system, which, in turn, will lead to our being more successful at heeding Miller's (1969) call to give psychology away. Such a vision obviously requires the coordinated efforts of large numbers of theorists, researchers, practitioners, and policy makers. That is, it requires the development of a new mass movement in psychology. In this article I explore how the foundation might be laid for the successful launching of such a movement, while at the same time attempt to address many of the concerns about my proposal raised by the contributors to these two special issues.

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[2] Some individuals have objected when I have used the term "suicide attempters," and have instead reminded me that the politically correct phraseology is "people who have made suicide attempts." This is, of course, a well-meaning argument, and it certainly is true that the "personhood" of these individuals was not defined solely by their attempt. A clinician of any depth never forgets she treats real people. But the participants *were* defined by their attempt in relationship to the research we were conducting. Although concerns about terminology certainly have their place, in this instance I found a dark sense of irony in "Ivory Tower" academics quibbling over trivial issues of terminology that were so dramatically removed from the magnitude of the problems these individuals faced. This is my justification for continuing to use the term.

[3] This difference includes the fact that half of the present-day suicide attempters received cognitive psychotherapy, which was found to have a significant and substantial impact in reducing the subsequent reattempt rate, cutting it in about half. Approximately 55% of the group who did not receive the additional intervention made a subsequent suicide attempt in the year following the index attempt. Thus, it is likely that the "true" difference between the cohorts is even greater than what was reported.

[4] This assertion is, admittedly, a conjecture, and those who disparage any conceptual analysis without immediate data collection (cf. Hayes, 2004) are, of course, free to proceed with their current methodological commitments and not concern themselves with issues of "pure" theory. But facts are theory laden, and what is lacking is not data collection efforts, but the manner in which our data are organized.

Building Bridges to Achieve a Shared, General Understanding

In closing his article, Calhoun (2004) offered some insightful comments about the nature of mass movements and wisely cautioned about their potentially destructive nature.[5] Ideologically driven mass movements have indeed caused much damage historically, and concerns about the potential of my system to produce harmful effects were evident in several of the contributions. Katzko (2004) commented on the problems that have arisen when institutions in American psychology have dictated how scientific problems are perceived and approached. Stam (2004) saw my proposal not as an act of science, but as a "disciplinary maneuver" (p. 1259). Both Yanchar (2004, p. 1279) and Viney (2004, p. 1276) worried that my proposal might place psychology in an intellectual "straight-jacket." Slife (2005) stated that if my proposal were adopted, certain psychologists would be disenfranchised by "definitional fiat" (p. 108) and with "very little intellectual justi-fication" (p. 112). The theme here is that any attempt at unification might result in a monolithic system that dogmatically stamps out pluralism, the diversity of ideas, and free scientific inquiry. This concern was perhaps the most prominent theme in the commen-tators' criticisms of my system, and thus it requires some exposition.

Like my critics, I strongly agree that if the ToK System places us in an intellectual straitjacket and dogmatically prevents divergent opinions from being explored, it will have done a tremendous disservice to scientific inquiry. As such, I am grateful for com-mentators raising serious questions about the potential problems of a unification that is ideologically driven and anti-pluralistic. Indeed, as I note in an earlier paper (2003), both psychoanalysis and radical behaviorism were guilty of dogmatism. However, as evi-denced by many of the articles in these two special issues, the ToK System seems to open, rather than close, lines of inquiry.

In common parlance, the basic question I am asking (and answering in the affirma-tive), is: Can we develop a shared, general understanding about the way the world works? I contend that the physical and biological sciences have developed in such a way that these sciences do provide clear models for what matter and life are and how they came to be. These sciences are unified because they have visions of their subject matter that are broad enough, clear enough, and verifiable enough, such that virtually all biologists and physicists can see that they are "true." Psychology and the social sciences currently lack such a vision—and this is what fundamentally separates them from the "mature" sciences (see Rand & Ilardi, this issue). The ToK System attempts to provide such a vision for mind and culture and, in so doing, provides a comprehensive vision of the sciences as a whole.

The modern evolutionary synthesis provides an excellent prototype for the kind of theoretical unification process that I am proposing. In the 1920s and 1930s, biology was a fragmented science. Fundamentally incompatible versions of evolution were being advanced (natural selection was a minority view), and there was a vitriolic split between the "naturalists" and "experimental geneticists." The text, *The Evolutionary Synthesis: Perspectives on the Unification of Biology* (Mayr & Provine, 1998), provides a wonder-fully rich and detailed history of how disputes in biology fractionated the discipline and how they were ultimately resolved with the formation of a single, coherent paradigm. Consider the following excerpt:

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[5] Dr. Lawrence Calhoun was my Master's thesis advisor at UNC-Charlotte and is a long-time mentor and friend. His articulation of my "noble quest" was deeply meaningful to me and reflects his intimate awareness of the personal and intellectual struggles associated with the development of the unified theory over the past decade. The title of this concluding article is taken from his contribution.

The crucial significance of the synthesis, then, was the fusion of the widely diverging conceptual frameworks of experimentalists and naturalists into a single one. There is every justification to designate this process a synthesis.

When I (Mayr) read what was written by both sides during the 1920s, I am appalled at the misunderstandings, the hostility, and the intolerance of the opponents. Both sides display a feeling of superiority over their opponents "who simply do not understand what the facts and the issues are." How could they have ever come together? Just as in the case of warring nations, intermediaries were needed, evolutionists who were able to remove misunderstandings and to build bridges between hierarchical levels. *These bridge builders were the real architects of the synthesis*. (italics added, p. 40)

Mayr's articulation of the state of affairs in biology in the 1920s reads with great applicability to the current state of psychology. The only difference might be that psychologists are fragmented on so many different lines (mental vs. behavioral; bottom-up vs. top-down; modern vs. postmodern; scientist vs. practitioner) that they would likely resist classification into two broad camps.[6] Nevertheless, I am proposing that what is needed in psychology is the emergence of "bridge builders."[7]

As Mayr and Provine (1998) point out, such bridge builders are not narrow specialists, but are individuals who are willing and able to learn the perspectives and new findings from areas outside their own field of specialization. Several articles in this special series epitomize this process of bridge building. Geary (this issue) cuts across evolutionary theory, neuroscience, cognitive science, and behavioral ecology in a manner that provides a comprehensive framework for understanding the evolution of mind. Shaffer (this issue) demonstrates with precision and clarity how the psychological perspective I offered on the Justification Hypothesis (JH) can be greatly enriched by linking it to sociological theory and research. Rand and Ilardi (this issue) articulate the crucial reasons to build bridges between psychology and the consilient natural sciences. Stanovich (2004) demonstrates how the two-domain model of the human mind yielded by the combination of Behavioral Investment Theory (BIT) and the JH unifies much work in cognitive science. Mayer (2004) offers an integrated model of personality that provides a new way of viewing and evaluating change techniques across the various psychotherapies, thus building bridges between personality research and psychotherapy techniques. When viewed together, these works provide an impressive early indication of the power such bridge-building enterprises can have in generating a shared, general conceptual framework.

The Tree of Knowledge System: A Scientific Humanistic Philosophy

The ToK System is, of course, not just about building bridges within psychology, but is about constructing effective interrelations between psychology and the other sciences and, at its largest scale, between the institution of science and other societal institutions, such as law, health care, governance, the arts, and religion. This level of analysis raises profound questions about the nature of knowledge itself, and several contributors posed crucial questions and offered incisive analyses about philosophical issues (e.g., Hayes, 2004; Presbury, 2004; Quackenbush, this issue; Slife, this issue; Shealy, this issue; Viney,

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[6] Of course, my lead article does argue that the science of psychology can be successfully divided into two components, psychological formalism and human psychology. But this is not the same as characterizing the current viewpoints of psychologists.

[7] There have, of course, been many wonderful exemplars of bridge building in psychology that occurred long before this special series and my proposal for the ToK System. My personal favorite is Paul Wachtel's (1977) *Psychoanalysis and Behavior Therapy: Toward an Integration*.

2004). Space limitations combine with the complicated nature of the subject matter to prevent a detailed exposition of all the relevant epistemological and ontological issues. However, there are some critical points that need to be made.

In a fascinating text, *The Quest for a Unified Theory of Information*, Haefner (1999) makes the point that most comprehensive theories of information now recognize the need for a formulation that includes both an information processor and the data being processed. Said differently, information can only be understood as the interaction or product of the data and the processor. This formulation resonates with my views regarding the nature of knowledge. Specifically, it suggests that Knowledge must be thought of as the product of the Knower (processor) and the Known (data being processed).

This basic formulation lends itself usefully to the construction of a scientific humanistic philosophy. The two components, the scientific and the humanistic, reflect two different valuations of the knower. In attempting to construct general laws that objectively describe complexity and change, the scientist works to de-value the influence of the specific knower in the knower–known interaction. In other words, the task of the basic scientist is to describe "reality" in as knower-independent terms as possible. Scientific methodology can be thought of as the tools by which this knower-independent knowledge is acquired.

However, in accordance with many of the insightful observations made by Presbury (2004), pure knower-independence (i.e., pure objectivity) is an impossible ideal (see also Shealy, this issue). Indeed, some of the most crucial developments in modern physics raised enigmatic questions about the relationship between observation, measurement, and knowledge. Consider the following analysis offered by the renowned physicist Neils Bohr (1958) on how the developments in both quantum mechanics and general relativity involve usage of symbolism that is "not directly visualizable."

> Even the formalisms, which in both theories within their scope offer adequate means of comprehending all conceivable experience, exhibit deep-going analogies. In fact, the astounding simplicity of the generalization of classical theories which are obtained by the use of multidimensional geometry and non-commutative algebra, respectively, rests in both cases essentially on the introduction of the conventional symbol i (the square root of negative one). The abstract character of the formalisms concerned is indeed, on closer examination, as typical of relativity theory as it is of quantum mechanics, and it is in this respect purely a matter of tradition if the former theory is considered as a completion of classical physics rather than as a first fundamental step in a thoroughgoing revision of our conceptual means of comparing observations, which the modern development of physics has forced upon us. (pp. 64–65)[8]

The essence of Bohr's point is that the fundamental formulas in quantum mechanics and general relativity involve the use of the imaginary number i, which raises significant

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[8] I have included this complicated quote from Bohr for several reasons. First, Haaga (2004) raises a question about the introduction of the moniker "psychological formalism" to denote the general mind sciences. I borrowed it from the physicists who use it in reference to the formal mathematics that provide the "purest" representations of their theories. Commensurate with a crucial point made by Quackenbush (this issue), the term highlights that those in the general mind sciences are freer to develop "pure" science models of mind, whereas those who deal with human psychology must contend with the cultural-person-as-a-whole and this inevitably removes some of the scientific formalism. A second reason I have included it is because it makes the fascinating point that imaginary numbers play a central role in our deepest mathematical formulations about the physical universe. Although virtually every mathematician will tell you that imaginary numbers are as "real" as real numbers, psychologists should be aware that there are interesting mental operations at work. Lakoff and Nunez's (2000) *Where Mathematics Comes From: How the Embodied Mind Brings Mathematics Into Being* offers a fascinating overview of the relevant issues. The final reason I offered this quote is that I believe the ToK System carries substantive implications for unifying quantum mechanics and general relativity.

philosophical confusions. However, in contrast to what some have claimed, these developments did *not* lead to the general conclusion that there is no such thing as objective knowledge. And, in accordance with the analysis offered by Wilson (1998), I believe that the quest for objective truth (defined as accurate models of complexity and change) should remain the idealized goal of the institution of science.

But, as Slife (this issue) points out, science is not the only way of knowing. And in the ToK System, science is seen as one particular type of justification system, which has particular strengths (accuracy) and limitations (amorality). Other justification systems (e.g., legal, religious, or political) are explicitly prescriptive, moral systems. I am not alone in isolating the language game of science from the language game of morality. Consider that the Humean is–ought distinction is legendary. The split between science and ethics is well summarized in the following quote from Pinker (1997): "Like many philosophers, I believe that science and ethics are two self-contained systems played out among the same entities in the same world, just as poker and bridge are different games played with the same fifty-two card deck" (p. 55).

It must also be recognized that de-valuing the knower and striving for knower independent knowledge is obviously, at one level, a value-laden stance. And it is here that we find the need for the humanistic side of the philosophy. In this system, the humanist values the knower and all of her or his idiosyncratic subjective elements that contribute to the uniqueness of her or his knower–known interactions. In other words, the humanist embraces knower relativism and all of the possibilities that emerge with such an embrace. In the process of valuing the uniqueness of the knower, humanism defines humans as the most valued of subjective objects and, thus, unlike the "cold" formulations of basic science, the humanist side of the equation functions as a prescriptive value system.

Furthermore, the institution of science is seen as emanating out of, but also being constrained by, humanism. At its most general and abstract level, this constraint is found in acknowledging much of what Presbury (2004) meant when he spoke about the impossibility of a "view from nowhere" (pp. 1255–1256). It is more concretely recognized when one considers ethical constraints and Internal Review Boards that (appropriately) prevent scientists from pursuing particular avenues of investigation. Despite this constraint, the humanist values scientific knowledge as essential to promoting humanity (see Quackenbush, this issue; Shealy, this issue), and is not threatened by the ever-increasing power of scientific explanations. In the end, the scientific and humanistic positions are seen as existing in dialectical tension with one another, and there is the recognition that there is value to be had in both valuing and de-valuing the knower.

As with all simple analyses of complex problems, there are, of course, many complications. I found Quackenbush (this issue) to be highly illuminating in his rich articulation of both the natural science and human constructionist viewpoints that were ultimately very compatible with my views. Human psychology does represent perhaps the fiercest intersection of science and humanism. Indeed, this intersection has clearly pulled the discipline into two cultures (see Kimble, 1984; Lilienfeld, 2004; Rand & Ilardi, this issue). I also found Shealy's (this issue) "version of reality" from the perspective of a "scientific humanist" to be both compelling and inspiring. With powerful rhetoric, Shealy convincingly argued that the JH carries with it the potential to bridge rigorous scientific formulations with the foremost concerns of the humanist. His breakdown of the justification processes underlying fundamentalist ideologies was both gripping and incisive, and demonstrated the absolutely crucial need (perhaps now more than ever) for psychological analyses of both political and religious justification systems.

It is not accidental that the first special series devoted to the unified theory is occurring in the *Journal of Clinical Psychology*. With its scientist–practitioner model and its

attempt to bridge both basic and applied domains of human psychology, clinical psychology has had to struggle mightily with scientist–humanist tensions, perhaps more than any other discipline. Quackenbush's analysis (this issue) reflects a deep appreciation of these two cultures and provides a conceptual pathway to see the viewpoints as co-existing in a peaceful dialectic, rather than as warring factions. His call for psychologists of "good will" to build bridges between disparate philosophical positions and his concluding comment that empathy is the one attitude necessary for understanding both resonated strongly with me. I believe a successful merger of the two perspectives will allow us to finally answer Carl Roger's call for an authentic human science (e.g., Rogers, 1973, p. 380).

On the Nature of Science

Slife's article (this issue) focused on testing the limits of my proposal raised crucial issues that must be addressed both within the field of psychology and between the institution of science and society at large. While acknowledging that the system I proposed offered "a comprehensive set of ideas" (Slife, this issue) that organizes much extant research and opens new lines of inquiry, Slife also pointed out that there were potential problems. In particular, he was concerned that some psychologists will feel defined outside of the field. He wrote: "Psychologists who have ideas that fall within his theoretical boundaries will feel unified and defined, but those who have ideas outside these boundaries will feel marginalized or rejected for no other reason than definitional fiat" (2005, p. 108). He proceeded to articulate how qualitative researchers and theistic psychologists are two "fragments of a fragmented psychology that *could* fall outside Henriques' disciplinary matrix" (Slife, this issue).

Slife argued that agency, meaning making, and nonobservability were fundamental assumptions of qualitative researchers that, for him, seemed to clash with my system. In a related vein, Calhoun (2004) also wondered about the extent to which my system was compatible with postmodern notions. I will offer a couple of brief thoughts in response, but refer the reader to Quackenbush's article (this issue) for a more detailed exposition of human freedom and meaning making that is commensurate with my system. In contrast to Slife's interpretations, I do not see qualitative research methods or an exploration of meaning making as being at odds with my conception of science. (Although as is evident in my comments above and in the articles by both Quackenbush and Shealy, the relationship between humanism, science, and meaning making is a complicated one. Consider, for example, Gilbert's (2004) comment that, at one level, "even the humble fruit fly is a . . . meaning-making organism" (p. 1225). Viney (2004) also seemed to interpret my articulations of the unified theory as supporting a monolithic approach to methodology. This implication is not well founded—there are many ways to test an idea, and I strongly advocate for a pluralistic methodology. Furthermore, the notion of agency defined in terms of "self-caused" behaviors is also consistent with my system. Deci and Ryan's (2002) self-determination theory, for example, is very commensurate with the view of self-causation my system suggests.[9]

Additionally, direct observability is not a commitment of my system, nor is it a commitment of science as it is generally conceived. Consider that, in physics, string theorists

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[9] I also think it is useful to note that voluntary control can be considered to be an empirical fact. For example, I predict that in the 5 seconds following the writing of this sentence I will touch my nose. Surprisingly enough it happened. I could make a 100 such predictions and be correct every time. Such behaviors contrast with the endless variety of behaviors that lie outside my control, and thus the notion of "voluntary behaviors" as distinguished from involuntary behaviors is perfectly reasonable. I share these thoughts to emphasize my belief that self-caused behaviors (i.e., level 4 behaviors on the ToK) are of keen interest to my scientist side.

consistently posit the existence of gravitons, a 2-spin boson akin to a photon that carries the gravitational force interaction (Greene, 1999). Such phenomena have never been observed, but are considered legitimate scientific constructs. The question is *not* whether you can see the phenomena; the question is whether your models of the phenomena make testable predictions, at least in theory (e.g., gravitons make predictions that are theoretically testable, but practically impossible to test because of the high energies required).

Ultimately, however, I agree with Slife that some of the ideas he referenced clearly are defined outside the boundaries of science, and thus are outside the boundaries of the science of psychology. Slife explicitly defined agency in terms of "uncaused action" (2005, p. 110). He also explicitly referenced theistic psychologists' commitment to explaining aspects of human behavior through the existence of a personal god.[10] With such explicit assertions, he has indeed found the boundaries of my system. Personal gods and uncaused action concepts do not seem to lend themselves at all to the language game of science. Consider that if basic science is defined in terms of the search for accuracy, then there is always the potential for inaccuracy. I certainly remain committed to the notion that any or all of the ideas embedded in the ToK System could be wrong (see Henriques, 2003, p. 156). Are those who promote such ideas about personal gods and uncaused actions also committed to the notion that they could be wrong? It is difficult to see how this would be the case.[11]

I also do not see how constructs that *obviate the need for causal explanatory frameworks* can be meshed with scientific justifications. Of course, such constructs may well be viable in other justification systems, such as religion or law, because it ultimately depends on the goals and the function of the system (cf. Gergen & Gergen, 1986). If the goal is to determine the moral meaning of an act or to prescribe how we should behave, don't look to basic science. Conversely, if one is interested in an explanation for why something occurred, then it makes perfect sense to look to science (uncaused action constructs fail to describe/explain anything in the scientific sense). Thus, my retort to Slife's arguments about agency as uncaused action and theism is that he is mixing justification systems. From the point of view of humanism, God and "uncaused will" could both be seen as important, viable meaning-making constructs. However, they are not scientific justifications. Ultimately then, in contrast to Slife who argued that those who subscribe to such notions are defined out of the science game with "little intellectual justification" (2005, p. 112), the fact of the matter is that such constructs are defined out because they violate virtually every tenant of scientific thinking, such as connectivity, consilience, coherence, empirical validity/falsifiability, heuristic value, functional utility, and parsimony (see Hayes, 2004; Stanovich, 2001; Wilson, 1998).

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[10] Slife spoke of theistic psychologists in the context of discussing spiritual therapy interventions. A key and potentially subtle point is that there is an important difference between theistic/spiritual therapy interventions and God as a basic scientific construct. The former is an applied side issue. Interventions that emphasize spirituality may well have important curative powers. Indeed, when I worked as a therapist with suicide attempters, in many instances I facilitated and encouraged the awakening of their religiosity. The purpose of applied side science is to engineer change toward some goal, not describe it, as is the case in basic science. So, I have no fundamental qualms with the notion of spiritual/theistic interventions per se. (Although ethical implications are raised if there is a disconnect between what is promoted in practice and what is seen as the scientific explanation, this is a separate issue).

[11] It is important to note that although disproving the existence of god is essentially an impossibility, imagining circumstances that would prove the existence of a god that defied natural law is not at all difficult. And, indeed, if there were convincing evidence for the existence of a personal god, then this evidence would have to be incorporated into our models of the world and naturalism would no longer be fully tenable. The point here is that, contrary to popular belief, scientists do not banish a priori any concepts. But they do eventually abandon concepts that cannot be supported with evidence and that fail to fit into the connective network of ideas that make up scientific knowledge.

I have articulated my views on these issues at some length because such a (hermeneutic?) dialogue is absolutely necessary to begin to address what appears to be an ever-widening gap between the scientific and nonscientific worldviews. Gallop polls consistently find that many more Americans believe in "creation science" and the literal interpretation of Genesis than in naturalistic evolution (Pigliucci, 2002; see also Shealy, this issue). The current President of the United States (George W. Bush) has called evolution an open question (Kristof, 2000). Given that this country is the most technologically sophisticated country in the world, how can it be that our leaders and the population at large fail to comprehend what is perhaps the most significant and successful scientific theory offered to date? A likely reason is that scientific worldviews are often presented in the absence of a humanistic value system, and thus many individuals outside science experience evolutionary theory as a threat to both their moral and explanatory sensibilities. Consider that John Gribbin, an excellent and popular science writer, opened his most recent text *The Scientists* (2002) with the line, "The most important thing that science has taught us about our place in the universe is that we are not special" (p. xvii). What Gribbin means is that science has repeatedly disconfirmed the many different creation myths that various religions have invoked to justify why humans are unique.[12] However, science has not and, according to my scientific humanistic philosophy, cannot prove that humans are not special. If one defines humans as special by definition, this is a value-based claim that cannot be tested for accuracy and is clearly outside the bounds of science as conceived here.

I believe that to construct the needed connections between the scientific and nonscientific viewpoints, it is incumbent upon the institution of science to deliver our ideas in a manner and context that affirms and explicitly emphasizes a moral, humanistic component. It is in that spirit that the ToK is so named, as it is a reference to the Tree of Knowledge of Good and Evil in Genesis. Of course, in contrast to the traditional interpretation of Genesis, I contend we must eat heartily from the Tree of Knowledge if we are to flourish (i.e., we must ask questions about who we are and where we have come from and be committed to developing rational answers). Nonetheless, the larger point here again pertains to bridge building—by attaching the system to biblical mythology, a connection is made between a "hard core" scientific system and the world of religion.[13]

The Unified Theory and the Institution of Psychology

Several authors raised illuminating questions regarding the relationship of my proposal to the institution of psychology. Stam (2004) argued that any attempt at unification is concerned with the institution of psychology, rather than "real" scientific problems. He argued that my comments regarding the American Psychological Association indicated that I had "confused" problems of the institution with problems of science. Katzko (2004) also emphasized the distinction between the institution of psychology and its subject matter. In a similar vein, Yanchar (2004) saw my proposal as pursing unity per se, which presumably would be about the institution, rather than truth, which would be related to the subject matter.

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[12] It is interesting to note that Pope John Paul II has considered the Big Bang to be consistent with biblical notions of a "moment of creation" (Hawking, 1997).

[13] I have found Ken Wilber's discussions on the relationship between science and religion to be a useful starting point (e.g., Wilber, 2001). In particular, his articulation of "deep religion" or "deep spirituality" with its emphasis on the achievement of inner meaning and peace (rather than on any empirical claims) is consistent with my view of the humanistic embrace of knower relativism.

Given these criticisms, it may be useful to state that my proposal is, first and foremost, about the subject matter of psychology. The specific theories that I have offered (e.g., BIT, JH) are clearly concerned with explaining animal and human behavior. They offer ways of organizing data into a coherent network of ideas that describe, explain, and predict (e.g., Calhoun, 2004; Geary, this issue; Shaffer, this issue). Contrary to Yanchar's claims, these theories are very much about the pursuit of truth. My argument is that truth is to be found in the bridges between perspectives, as was the case in the synthesis of biology referenced earlier.

However, the institution of psychology is obviously not divorced from the problems of the science (Stam, 2004). Thus, because my theory is a "sweeping, proposal that not only defines the field of psychology in a sharp way, but also suggests a unitary theoretical framework to guide research" (Yanchar, 2004, p. 1279), it does carry significant and substantial implications for psychological organizations and governance structures, educational practices and curricula, and the practice of psychology. Thus, my proposal is, and indeed has to be, secondarily about the institution of psychology.

Despite what some of my critics implied, I am quite familiar with the difference between an institutional and scientific focus.[14] For example, I am an active participant in what is a clear institutional movement for the development of Combined-Integrated doctoral training programs (Henriques & Sternberg, 2004). Such programs argue that the practice of psychology needs the clear articulation of a generalist foundation that cuts across the traditional practice areas of clinical, school, and counseling psychology. The reason is not because advocates are against specialization—quite the contrary. Instead, the argument is that the generalist orientation opens up pathways to draw from each of the three practice areas in a manner that is complementary and synergistic. This is different from the specific school approach, which exists, by definition, in contrast to the other practice areas. That is, to justify its legitimacy, clinical psychology has to be different from counseling psychology, which, in turn, has to be different from school psychology. From the generalist point of view, the focus on difference and separatism can create problematic schisms, turf wars, the magnification of minor and peripheral differences, and the proliferation of overlapping and redundant concepts. Of course, these arguments for changing aspects of the institution of psychology are intimately tied to developing a comprehensive theoretical model for the subject matter of psychology.

Several contributors questioned if my proposal would solve the major problems faced by institutional psychology, especially the strained relationship between scientists and practitioners. Although Stricker (2004) indicated a favorable attitude regarding the unified theory, he argued that my analysis did not address the crucial power issues that exist between scientists and practitioners. His perspective was that the struggle for power has resulted in a lack of respect between these two rival groups and that this is at core of the problems faced by the American Psychological Association. I strongly agree with much

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[14]Careful examination of the primary ToK System diagram (Henriques, 2003, p. 154) clearly reveals that the institution of psychology (and science in general) evolves out of culture and is quite distinct from its subject matter (mind). However, the critics are raising an important point because confusion between the institution and subject matter is substantial. Consider the following quote from Wilson (1998) on the gap between the natural and social sciences, "The question remaining is how biology and culture interact, and in particular how they interact across all societies to create the commonalities of human culture" (p. 126). The importance of Wilson's point notwithstanding, it is crucial to note that, according to the ToK System, Wilson has made a "column jump" in his analysis. Biology is a particular class of science. Culture is a dimension of complexity. Thus, Wilson is comparing apples and oranges. Substitute "life" for biology or "the social sciences" for culture in Wilson's quote and it becomes apparent that there is the potential for significant ambiguity. The point here is that there are frequent confusions between scientific institutions and the subject matters they deal with, and the ToK System can help clarify those misunderstandings.

of Stricker's analysis and have made essentially the same point elsewhere (Henriques & Sternberg, 2004). And I did not mean to imply in the target article (Henriques, 2004) that the theoretical unification of psychology would remove all such strife between academics and practitioners. At the same time, however, it is my contention that power motives come to play a much more destructive force in the absence of clear understanding. Thus, I am arguing that definitional clarity will anchor the justification systems of the various parties involved and this can serve to clarify roles and boundaries and clear up many misunderstandings that lead to tensions.

Lilienfeld (2004) similarly questioned whether my analysis would solve the great scientist–practitioner feud. He saw the conflicts as arising from differences in epistemology, and recommended the better education of clinical psychologists in scientific methodology and philosophy of science. Here again, I find myself in large agreement with aspects of this argument. Differences in epistemology do contribute to conflict. And, as just mentioned, I did not really argue that a clear definition of psychology would not solve *all* aspects of the scientist–practitioner problem. (In my more detailed exposition of the problem with Sternberg (2004), we listed the definition of psychology as but one of five elements that contribute to confusion and conflict.) And, as with other contributors like Kihlstrom (2004), I also worry about the quality of the scientific education many professional psychologists are receiving. However, I found Lilienfeld's analysis to be too one sided in its recommendations for certain clinicians to be better trained in the methods and philosophy of science. Instead, I locate the problem as much with the failure of scientific psychology to deliver an effective, comprehensive theory of human behavior (cf. Shealy, this issue), as I do in certain clinicians having misguided romantic notions.

Defining Psychology in the Service of Educating Psychologists

As the quote that opens Lilienfeld's contribution amusingly illuminates, psychology has been, historically, extremely difficult to define. Lilienfeld, however, goes on to question whether this is a really problem and makes the important point that fuzzy boundaries abound in virtually every concept examined. He then offered an authoritative (authoritarian?) quote from the Medawars (1983; who are biologists of high stature) on the fact that the boundaries between the living and the inanimate are fuzzy, and that serious biologists tend not to concern themselves with such trivial matters. Lilienfeld proceeded to raise the question that if biologists aren't concerned about fuzzy boundaries, then why should we as psychologists be so concerned?[15] The answer is actually simple. Lilienfeld has confused "fuzzy boundaries" with a shared, general conception of the subject matter. Indeed, careful examination of my proposal in general, and the ToK diagram in particular, clearly reveals some fuzziness about my "crisp" definition of psychology [see Haaga's (2004) comments about my definition of psychology]. To offer just one example, consider the question: 'When does a neural tube become a nervous system that computes and coordinates the behavior of an animal as a whole?' This boundary is undoubtedly fuzzy. And, as with the Medawars' comments about the nature of life, the fuzzy boundary is not a tremendous concern of mine.

What *is* a major concern is that, unlike biology, psychology has not resolved fundamental questions about its subject matter. The lack of a general resolution of our subject

[15] It is, of course, a highly debatable point to suggest that the only time a serious biologist might mention the definition of life is in the context of disparaging weak-minded individuals who ask such questions. Indeed, the 1947 book, *What is Life?* by the physicist Erwin Schrodinger is about this very question and is generally seen as a classic in biology (Pigliucci, 1999).

matter is why we continue to have psychology as *the* science of behavior, the science of *human* behavior, the science of mind *and* behavior, and so forth. And it is why a prominent psychologist like Kihlstrom can question whether B.F. Skinner was even a psychologist. Thus, I am not concerned about fuzzy boundaries per se, but instead I am concerned about what it is we are trying to draw the boundary around when we use the term *psychology*.

The inability to define the epicenter of psychology has resulted in many unresolved questions about the development of a general curriculum. In reviewing the struggle American psychology has had with its curriculum, Benjamin (2001) observed that "Efforts over the past century to define the core elements of education and training in psychology, that is, to identify the essence of what a psychologist is, have met with little or no success" (p. 736). He concluded, "Historically, psychologists have never been either able or willing to answer the question, 'What is a psychologist?' . . . Therefore, perhaps it should be no surprise that psychologists have had a difficult time reaching agreement in how to best educate doctoral students in the field" (p. 741). Of course, my argument is if you can define psychology, then it should become much easier to answer the question about what defines a psychologist. And this, in turn, will mean we should be able to construct a clear general psychological curriculum.

In accordance with this claim, Gilbert (2004) argued powerfully the ToK System fills a crucial gap in offering a much needed macro-level view of the field. I am especially appreciative of his claim that my analysis should not be reserved for a specialized graduate course, but instead should be center stage in how we educate students about our subject matter. Haaga (2004) also offered insightful comments regarding the need for a more intellectually satisfying conception of psychology, the potential educational value of explicitly emphasizing the correspondence between mental behavior and animal behavior, and the taxonomy of psychology provided by the ToK System. Geary (this issue) made the fascinating point that the domains of the human mind can be well organized into folk physics, folk biology, and folk psychology (at the individual and group level), which clearly demonstrate strong correspondence with the dimensions of complexity in the ToK System.[16] Several others also commented that the graphic depiction of the hierarchical arrangement of the sciences was a highly useful educational tool (e.g., Calhoun, 2004; Shealy, this issue; Viney, 2004). Ultimately, the point here is that if the ToK System is found to be a viable depiction of the evolution of complexity and the institution of science, educators will no longer need to either struggle for weeks in a futile effort to define psychology or choose to ignore the matter completely, but instead they can simply pull out a diagram and point to it on the map.

The analysis that I have offered also lends itself to the construction of a general curriculum. The system makes two crucial divisions: The first division is between the profession and the science of psychology (Henriques & Sternberg, 2004); the second division is between the general mind sciences (psychological formalism) and human psychology (Henriques, 2004). As both Kihlstrom (2004) and Haaga (2004) point out, many psychology departments (including the undergraduate department at this institution) currently divide the teaching of psychology into two broad domains: (a) psychology as a natural science and (b) psychology as a social science. Obviously, there are clear parallels between this split and the organization I am advocating. However, unlike the division between natural science and social science, which is fraught with ambiguities

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[16] My 4-year-old daughter is fond of chanting "rocks, plants, animals, people," which lends anecdotal credence to the intuitive nature of the classification system suggested by the ToK System.

(e.g., are social phenomena unnatural?), the ToK System spells out clearly and specifically why the lines are drawn where they are. And curricula can be built around the concepts. For example, the Table I offered in the target article, "Psychology Defined" (2004, p. 1215) that lists the disciplines that fall under the construct of psychological formulism could clearly be used to generate specific requirements and coursework a student would have to complete to be well-versed in the general mind sciences.

Next Steps in Evaluating and Developing the Unified Theory

Although my proposal does appear to offer a high degree of potential to inform educational curricula of psychology, I also believe strongly that more critical analyses of the ideas are required before these ideas should formally impact institutional aspects of psychological education. Contrary to Yanchar's (2004) implication that I do not see "the need for critical, reflexive examination" (p. 1280) of my work, I am, of course, strongly committed to the notion that the ideas I have offered require such evaluation. Indeed, any self-respecting scientist should be skeptical of the things she or he wants to believe, and my ideas are certainly no exception.

Although several of the full-length articles elaborated on some of the core scientific elements associated with the ToK System, (e.g., Geary; Shaffer; Shealy—all this issue), there were, in actuality, relatively few detailed critiques of the JH or BIT, and I want to encourage such critical analyses in the future. One exception was the commentary by Vazire and Robins (2004), who argued that the JH was too narrow in its focus and that it had the evolutionary sequence backwards. These contributors agree with a central tenet of the JH, which is that self-consciousness serves a social function. However, they argued that the self serves more functions than articulated by the JH and, more importantly, they contend that any access to others' thought processes inevitably requires initial access to ones' own thoughts. Because of this contention, they conclude that self-consciousness had to have emerged prior to the problem of justification, and thus the problem of justification could not be the primary explanation for the evolution of the self-consciousness system.

I have four basic responses to their critique. First, I was not convinced that self-reflective awareness is required to access the thoughts of others via language. Although they offer an interesting quote from Hobbes, Vazire and Robins do not clearly specify why the ability to *ask* the question "Why did you do that?" requires self-consciousness. It is the contention of the JH that the ability to effectively *answer* such questions does require reflective self-awareness. Second, I see the JH as a more sophisticated evolutionary analysis of self-consciousness because it specifically matches a particular and unique adaptive problem to the design features of the self-consciousness system. In contrast, Vazire and Robins simply list useful natural functions associated with their conception of the self system. Although such analyses are clearly valuable, they are not as effective as the matching process I employed in answering crucial questions, such as: Why did self-consciousness evolve in hominids but not other apes? Why does the self-consciousness system exhibit the specific design features that it does? Third, unlike Vazire and Robins' "broader theory," the JH links the self-consciousness system to language and the evolution of culture in a manner that clearly specifies their interrelations.

My final point is that it is important to recognize that there is much overlap in our analyses, and this makes it such that our differences could be unnecessarily magnified. We are in agreement that aspects of self-awareness are present in other apes (as evidenced by Mirror Self-Recognition) and thus evolved prior to the problem of justification. We also agree that the problem of justification influenced the development of the

self-consciousness system (i.e., they note it was later "co-opted" to serve as a justification filter). And we agree that self-consciousness plays a crucial role in social functioning, the development of culture, and what differentiates humans from other animals. Thus, it is ultimately the case that there is much more that is similar about our proposals than is different.

Another criticism, levied by Yanchar (2004), was that the "exclusivity and rigidity" (p. 1280) of my proposal was problematic. He argued "many psychologists with diverse perspectives–humanistic, phenomenological, existential, hermeneutic, feminist, and others—are unlikely to endorse Skinnerian, Freudian, or neurocognitive principles; and it is equally unlikely that the precepts from these diverse theoretical perspectives can be coherently subsumed within Henriques' "human psychology" (2004, p. 1207–1221) without altering them fundamentally" (p. 1280). Given that several of the contributions offered insights from phenomenological, existential, and humanistic perspectives (e.g., Quackenbush; Schaffer; Shealy—all this issue) there are good reasons to doubt Yanchar's claims.
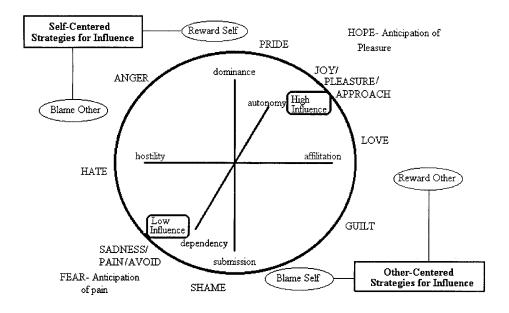
However, the feminist viewpoint was not articulated, and thus it may be useful to make a few brief comments about how this perspective is consistent with the language of my system. One of the core concerns in feminism is highlighting the manner in which political power, science, and gender are intricately intertwined. Feminists have deftly pointed out that ideas touted as scientific explanations often carry an implicit justification that legitimizes the current patriarchal power structure. Using this perspective to reflect on the current proposal, a feminist might point out that because the vast majority of contributors of the two special issues, as well as the four "fundamental" theorists I refer to (Einstein, Darwin, Skinner and Freud), are White men, there is the danger of an implicit sexism because the views offered have been filtered through the masculine lens and might tacitly justify power in this direction. Regardless of the validity of this assertion, my point here is that the process of justification is intimately associated with the logic underpinning the feminist approach. Ultimately though, and in support of Yanchar's concerns, I do concur that it is essential that my proposal be evaluated from the many diverse perspectives in psychology, and if it can be demonstrated to be incompatible with other valid perspectives, then this would clearly challenge my claims regarding the power of my proposal to unify the discipline.

Another necessary step in constructing the unified theory is to examine correspondences between my proposal and other attempts at integration. I am particularly looking forward to building bridges between my system and the psychotherapy integration movement. Mayer's (2004) articulation of a systems framework for integrating personality offers a readily accessible example of another approach to integration, and thus interesting questions arise regarding the similarities and differences in our proposals. Although our proposals appear to be quite different on the surface, I see many similarities and opportunities for drawing connections. For example, there are clear connections between BIT and Mayer's notion of "the energy lattice" (p. 1302). There are also connections between his notions of the "conscious executive" and the "social actor" with the JH. In particular, viewing the self-consciousness system as a filter that suppresses unjustifiable actions and expresses socially justifiable actions allows for a clear correspondence to arise between our two systems. The private self-conscious would represent the filtering process and would correspond to Mayer's conscious executive, and the behaviors that are expressed would represent the public self-conscious and correspond to Mayer's social actor.

One area of important difference between our systems was the location of the attachment system. Conceptualizing the social actor as arising to meet the evolutionarily recent demands of a sociolinguistic environment, my system would lead to the differentiation of

the social actor from the much phylogenetically older attachment system. I would instead characterize the attachment system as part of the behavioral investment (or energy lattice) system. This importance difference notwithstanding, I found Mayer's metaphor of an energy lattice to be remarkably congruent with an integrative model of social motivation and affect I developed called the Influence Matrix (IM; Fig. 1). Consistent with much of Geary's analysis of the evolution of social motivation, the IM is built on the assumption that high social influence was historically associated with survival and reproductive success. The IM posits the presence of three relational dimensions that underlie the computation of high social influence: (1) Power (Dominance–Submission); Love (Affiliation–Hostility); and Freedom (Autonomy–Dependence). The outer circle of the IM depicts common affects and their position in relationship to the social motives. This formulation is directly congruent with Geary's proposition that affect provides feedback regarding the effectiveness of particular behavioral strategies. Thus, whereas shame is closely associated with submission and avoidance of conflict, guilt is associated with both submissive and affiliative tendencies (see Tangney & Dearing, 2002). Consistent with the integrative theme here, the IM has been used to demonstrate connections between Beck's notions of sociotropy-autonomy, Leary's Interpersonal Circumplex, and the various personality disorders (Henriques & Beck, 2002). There are, of course, many more details about the IM that need to be articulated, but space limitations prevent a fuller presentation.

THE INFLUENCE MATRIX

LOVE   X-AXIS = INFLUENCE THROUGH
ALTRUISM/COOPERATION

POWER   Y-AXIS = INFLUENCE THROUGH
COMPETITION/CONTROL

FREEDOM   Z-AXIS = FREEDOM FROM INFLUENCE

Figure 1.

To briefly summarize, the next steps in evaluating and developing the unified theory include: (a) analyzing and critiquing the postulates and logical implications of the various theoretical formulations of my system; (b) exploring the extent to which the system is or is not commensurate with other viewpoints, and (c) building bridges between the unified theory and other integrative proposals. Of course, the other issue is whether or not the ToK System and its various propositions will be able to "deliver the goods" (Rand & Ilardi, 2005, p. 14) in terms of research programs, and, as Hayes' (2004) commentary makes clear, it will obviously be important to develop novel lines of research that examine the precision and depth of the proposal in both basic and applied domains. As a clinical psychologist, I am attuned to the need for these ideas to have practical implications and some headway has been made in this direction. I have begun to develop an integrative model of depression called the Behavioral Shutdown Model, which makes many predictions about the nature of depressed mood, its reversal, and the relationship between depressive reactions, depressive disorders, and depressive diseases (Henriques, 2000, 2002a, 2002b). Research is also currently underway examining the connections of the IM with trait theory.

Several of the current contributions spoke directly to the issue of research development. Katzko (2004) offers an important call to "elevate the 'process of justification' to the status of a phenomenon for study" through the lens of "an informed curiosity" (p. 1239). In other words, researchers are encouraged to develop methodologies that explore new frontiers in answering the question of how people justify their behavioral investments. In a related manner, Calhoun's (2004) article clearly demonstrates that the JH can be applied to specific domains of study in a manner that yields testable predictions. The latter section of Shealy's article (this issue) offers perhaps the most explicit articulation of the research methods that might be developed to explore both the process of justification and the development of justification systems. His articulation of the Beliefs, Events, and Values Inventory as an "MMPI" for justification systems seems to be a particularly promising method for developing a more fine-grained analysis of the JH. In sum, there are numerous ways in which the system can be empirically evaluated. The only cautionary note is that unintegrated lines of research have seriously hampered psychology's capacity to generate cumulative knowledge (Rand & Ilardi, this issue), and thus researchers are encouraged to use the theories offered here not only to develop new lines of research, but also to develop methods for consolidating existing ideas.

Conclusion

Stam (2004) makes the point that psychology has long been seen to be in crisis and that, in many ways, the institution of psychology is functioning adequately. I certainly agree that psychology is not in "imminent danger" of falling apart. The issue isn't so much about an institutional crisis as it is about potential—I believe strongly that the potential of our discipline to shape society in a constructive way has not been realized.

Evidence for psychology's failure to achieve its potential is seen not just when examining the chaotic state of theory, but when we look outward and see how psychology is viewed by those outside the field. When this is done, it does not take long to realize that the field has a serious image problem. Stanovich (2001, p. 194) characterized psychology as the "Rodney Dangerfield of the sciences" and illuminated how caricatures, pop psychology, and associations with pseudoscience and paranormal studies plague the popular image of our field. The causes of this image problem are undoubtedly complex and multidimensional. One element that clearly is a contributing factor is psychology's failure to define itself. As Benjamin (2001) put it, "Psychologists may have no right to

expect the public to understand their psychology when there is ample evidence that psychologists have never been able to define it for themselves" (p. 736). Of course, an implication of this claim is that if we could successfully define our field, we might be justified in expecting others to comprehend, respect, and usefully apply the knowledge we have generated. This is why I see the clear definition of psychology provided by the ToK System to be so important.

Another element that seems likely to contribute to the difficulties the field has had in reaching its potential is the conflicts between scientific and nonscientific views of human behavior. Not unlike Galileo's confirmation that the earth was not at the center of the universe, a scientifically minded approach to human behavior challenges some of the central justification systems that are operating in the population at large.[17] Indeed, it seems inevitable that to the extent the ToK System is successful in generating change, some will experience it as a significant threat to their justification systems.[18] If "true believers" (Calhoun, 2004) of the unified theory do emerge, I hope they will be empathetic to this resistance and will be keenly aware that the motivational processes underlying the justifications used to advance the unified theory will always have fundamental parallels with the motivational processes underlying the justifications used to oppose it.

In closing, I want to explicitly thank Larry Beutler for the opportunity to generate these special issues and all the contributors both for their stimulating work and for the high degree of respect evidenced throughout its development. Such sentiments are usually placed in an author's note but I wanted to include them here because I believe the empathetic, respectful understanding of different viewpoints is an essential aspect of the scientific humanistic philosophy for which I am advocating. Such humanistic patterns exist in stark contrast to the misunderstandings, the hostility, and the intolerance of opponents so often evidenced in important debates. It is through the empathetic sharing of ideas that the ultimate results will be constructive rather than destructive. And, if such a useful mass movement can be successfully constructed, we might actually be able to make a systematic difference in society that gives individuals like the suicide attempters with whom I worked a chance to reach their full potential as human beings.

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[17] When Kihlstrom (2004) critiqued the formation of the ToK, he wrote: "I would have preferred a bush instead, with branches emanating from a common root structure: one each for the several types of science, and two more for the arts and humanities, maybe even one for religion (*let's not get into that!*)" (italics added, p. 1246). I find his comment about religion to be illuminating and indicative of the deep tensions that run between religious and scientific worldviews. I believe that if psychology is really going to have a substantial impact on society at large, we *must* "get into" a dialogue about these issues.

[18] Consider that a poster on the unified theory was submitted for presentation at the APA 2004 Conference in Hawai'i and was subsequently rejected. Given the combination of the frequency with which posters tend to be accepted and the quality of ideas embedded in the ToK System as evidenced by this special issue, it seems likely that the rejection was more a consequence of threat than it was a consequence of inadequate scholarship.

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