

Comment

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Romantic Versus Realistic Views of Psychology

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Sternberg and Grigorenko's (December 2001) "Unified Psychology" offered a pleasing romantic notion of psychology, along with a series of recommendations to elevate its quality and social significance. Unfortunately, in developing this conception, they ignored fundamental methodological differences within the discipline. In addition, their conception of a unified psychology was achieved by extending the meanings of *converging operations* (Garner, Hake, & Eriksen, 1956) and *paradigm* (Kuhn, 1970) beyond their original intent.

Sternberg and Grigorenko (2001) flagrantly ignored striking differences among contemporary psychologists' criteria for truth or verisimilitude. To clarify this conflict, it becomes necessary to distinguish between the psychological process of understanding and the epistemological standards of explanation. Understanding is a personal psychological phenomenon that refers to the criteria that truth seekers use when they report that they understand. Explanation, in contrast, requires a public (objective) criterion consisting of epistemological rules that must be approximated for understanding to be achieved. Understanding is personal, explanation is social, and, in a fundamental sense, they are separable. The justification for this distinc-

tion between understanding and explanation is that attention is shifted away from the quixotic search for the true definition of truth to the reasonable task of characterizing different kinds of truth that people use when interpreting their world.

Do contemporary psychologists share a common conception of psychological truth? The simplest answer recognizes that some psychologists consider the methods of the natural sciences appropriate for determining psychological truth. Others insist that psychology is not a natural science but instead is some form of a human science that uses completely different criteria for interpreting human phenomena. In addition, psychologists are in complete disagreement about whether the discipline of psychology can discover moral principles that are right for humankind, with human-science types tending to support this idea and natural-science psychologists tending to reject it (Kendler, 1981, 2000). Although one cannot dictate the epistemological standards that the discipline of psychology should adopt, it is difficult to understand how the proponents of a so-called unified psychology can ignore basic and irreconcilable methodological conflicts that permeate contemporary psychology. A unified discipline cannot emerge from conflicting methodologies any more than the games of bridge and poker can be played simultaneously with the same deck of cards.

The modest aim of converging operations (Garner et al., 1956) was to assign theoretical meaning beyond a particular operation. I am confident that heat and length have reality status because their determination is not limited to a single set of operations but instead extends to a variety of converging operations. Although converging operations have obvious advantages over a concept that is limited to a single set of operations, this superiority does not logically lead to the prescription that the determination of converging operations should become the first order of business in ongoing research. Rather than prematurely seeking converging operations, one could decide that the best strategy would be to persist in wringing out more information from a specific experimental operation.

Science cannot and should not be re-

duced to a set of cookbook recipes for how research and theorizing should be conducted. By so doing, the potential productivity derived from a variety of approaches can be compromised. One gets the impression that Sternberg and Grigorenko (2001), in addition to giving a blanket endorsement of converging operations, are also encouraging psychologists to create new paradigms to add to psychology's knowledge base. Kuhn (1962) originally suggested that a paradigm consists of a "strong network of commitments—conceptual, theoretical, instrumental, and methodological and quasi-metaphysical" (p. 42) while acknowledging that the concept is incapable of any complete formulation. This open-ended conception has generated much confusion, particularly in relation to the popular concept of *paradigm shift*, a concept whose applicability to the social sciences Kuhn rejected. What is gained from increasing the number of paradigms in the face of persistent disagreements about what constitutes a paradigm shift in psychology? To be specific, consider the meaning of the cognitive revolution, which acquired a theoretical sanctity during its early days that persists up to the present. Now questions are being raised about whether it was truly a revolution (Leahy, 1992) or whether the so-called cognitive revolution has been preserved or destroyed in the new field of cognitive science (Johnson & Erneling, 1997).

Instead of viewing psychology within the context of converging operations and paradigms and holding the amorphous conception that psychologists "need adhere to no particular set of methods, to no particular field, and to no particular paradigm" (Sternberg & Grigorenko, 2001, p. 1078), psychologists should consider an alternative, more direct approach. Psychologists should face up to the task of interpreting psychological events by explicitly stating the nature of such phenomena and their intended mode of explanation.

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Postmodernism and the Values of Science

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Sternberg and Grigorenko (December 2001) elucidated an important challenge to psychology by redirecting the signposts of the science and profession. Conceptualizing the current status as one of fragmentation, Sternberg and Grigorenko argued for a unified psychology, in the form of a reorganization that endorses “converging operations” (p. 1071). The divisions within professional and academic institutions of psychology and psychology’s dominant methodologies are evidence of the influence of logical positivist and empirical reductionist philosophies on the discipline. However, Sternberg and Grigorenko argued that it is through the integrative power of a multimethodological, multidisciplinary, and multiparadigmatic unification that psychologists can productively make sense of their discipline.

The call for a unified discipline is a welcome one, and I wish to draw a parallel to recent discussions on the intersections of psychology and postmodernism (Gergen, 2001) and to highlight the importance of examining issues concerning the values and assumptions of psychology’s scientific endeavors. Postmodernism’s “intrusion” into psychology has been heatedly debated in recent years

(see Gergen, 1994; Smith, 1994), and much of the objection to its philosophy stems from accusations of antiscientific relativism (Smith, 1994). The convergent themes that emerge from examining psychology from a unified and postmodern perspective will, however, soothe the tensions that are created from these debates and, at the same time, strengthen the call for a unified psychology.

Gergen’s (1994, 2001) optimistic evaluation of the possibility of a postmodern psychology solicited several important changes to the philosophy and practice of psychology. Instead of the possibility of a world that is objectively accessible, postmodern psychology subscribes to a world that is socially constructed through language and social systems. Gergen (1994) believes that postmodernism is defined by the “abandonment of the traditional commitment to *representationalism* . . . [in which one holds the] assumption that there is (or can be) a determinant (fixed or intrinsic) relationship between words and world” (p. 412, italics in original). This sheds light on Sternberg and Grigorenko’s (2001) proposal for a phenomenon-based psychology. Whereas a fragmented psychology presupposes the accumulation of knowledge in a piecemeal fashion (as evidenced by the discipline’s subdivisions), a unified (and postmodern) psychology recognizes that psychologists’ fragmentation of psychology does not exist a priori to the conceptualizing of it as such. In other words, psychology has to organize around psychological phenomena that have existing social meaning and not let divisions of the discipline (that are arguably arbitrary) define what is studied and how it is studied (Sternberg & Grigorenko, 2001).

Gergen (2001) also addressed the promise of a proliferation of methodologies and paradigms. If scientific theories and methodologies provide only a partial and always incomplete picture of reality, then the adoption of multiple theoretical perspectives and methodologies will serve only to enrich the understanding of psychological phenomena. This sentiment is echoed in the call for a unified and pluralistic psychology. In organizing psychology around recognized phenomena, the obligation of psychologists is to understand and solve the problems associated with them from a multidisciplinary perspective. Studying discrimination, for example, would entail not only knowledge from a social psychological paradigm but also knowledge from other disciplines, such as cognitive behavioralism and anthropology.

Under a unified psychology, the process of paradigm choice is to be guided by an attendance to utility. Gergen (2001) argued that with the freeing of psychology from the role of truth revealer, it is open to the possibility of formulating transformative knowledge that is, by some measurement, useful.

Hoshmand and Polkinghorne (1992), in a postmodern critique of the rift between science and practice, asserted that

the test of knowledge is not whether it corresponds exactly to reality, as it is impossible to ascertain whether there is such a direct correspondence. Instead, the test for knowledge is whether it serves to guide human action to attain goals. In other words, the test is pragmatic. (p. 58)

Whereas Gergen’s understanding of what is useful is localized and communalized, Sternberg and Grigorenko (2001) fell short of elaborating how this is to be conceptualized under a unified psychology. The fact that psychology, like any other science, is never value free does not escape the two converging perspectives. However, the need to be explicit about values (Howard, 1985) within the vision of a unified discipline is one that was understated by Sternberg and Grigorenko.

If the subject of study under a unified discipline is one that has communal and social meaning, then the course of paradigm choice and direction in research has to be guided by considerations of values, assumptions, and ethics (Prilleltensky, 1997). In other words, in liberating psychology from the arbitrary confines of its current fragmentation, a unified (and a postmodern) science would merely have replaced one meaningless configuration with another if concerns for social values and scientific assumptions are ignored. Although the mechanisms and even plausibility of such considerations are debatable (Kendler, 2000; Prilleltensky, 1997), they are imperative if unified psychology is to maintain a meaningful role in society.

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As Defined, Unification Is Inevitable

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Theorists have attempted to unify psychology by integrating various theories or by viewing paradigms as different levels of explanation. Sternberg and Grigorenko (December 2001) presented a unique perspective by redefining unification and identifying "bad habits" (p. 1069) that thwart its coming. They operationalize unification as "the multiparadigmatic, multidisciplinary, and integrated study of psychological phenomena through converging operations" (p. 1069). This view might be better clarified as complementing the differences within psychology as opposed to unification, yet I agree with their general perspective. The shortcoming of the article lies in the authors' supposition that scientists studying psychological phenomena need to be proactive in the process of unification. Unification already occurs in multiple ways and will continue to do so as it is described. Putting "vintage old wine in a new and better bottle" (Sternberg & Grigorenko, 2001, p. 1078) is a continuous and unavoidable progression as new psychologists enter the field and reexamine psychology's constructs and methods.

Converging Operations

Examining phenomena using converging operations is an approach that has value in all fields of science, but even more so in a field that sits on the fence between hard science and social science. Sternberg and Grigorenko (2001) implied that most psychological researchers do not use converging operations. However, I would contend that it is the foundation of the field, exemplified through programmatic research. Programmatic research, defined as a "series of research experiments that deal with a related topic or question" (Smith & Davis, 1997, p. 325), is a form of convergence. Using this technique, psychologists attempt to understand phenomena by conducting analogous experiments with incremental and purposeful modifications. Thus, scientists change the definitions, sample characteristics, assessment procedure, and so on throughout the program of study.

As such, this core approach to the study of phenomena provides psychology with evidence from multiple standpoints.

Multidisciplinary Approach

The multidisciplinary approach to psychology, suggested by Sternberg and Grigorenko (2001), is a prescription for psychology that focuses on a construct as opposed to a subdiscipline. The authors presented the view that many psychological journals are narrowly focused, creating a schism of ideas and perspectives. This causes fragmentation in the field and limits understanding of phenomena. Yet, editorial boards from newer journals do accommodate varying perspectives and paradigms, and some of these journals are considered to be the premiere forums in which to present ideas, particularly in burgeoning areas. For example, the *Journal of Gambling Studies* identifies itself as an interdisciplinary journal accepting manuscripts from many fields and publishing articles that cover all aspects of gambling: social, problem, and pathological gambling as well as personality, genetic, psychoanalytic, and behavioral approaches to understanding gambling. This is not an isolated finding. Recently established mainstream journals commonly accept manuscripts from various subdisciplines, including several published by the American Psychological Association: *Emotion*, *Health Psychology*, and *Journal of Occupational Health Psychology*. Thus, these journals have already begun what Sternberg and Grigorenko prescribed.

Multiple Paradigms

Sternberg and Grigorenko (2001) suggested that psychologists, in general, focus on a single approach to knowing rather than drawing on multiple paradigms. This would imply that psychology students are taught a one-sided approach (i.e., the professor's specialty area). Perusal of any modern introduction to psychology textbook or content-oriented psychology text invariably reveals that multiple paradigms are presented within the first few chapters. An introduction to psychology textbook provides an in-depth analysis of the contrasting views of alternative models. A social psychology textbook will cover psychoanalysis, behaviorism, cognition, ethnology, and so on. These observations indicate that although senior scholars and educators may continue to hold one view, they train students to have varied perspectives and to use convergent methods of understanding.

A changing of the guard is imminent as new scholars examine previous paradigms and respect all theoretical perspectives. Psychology's brief history shows a fierce rivalry between psychoanalyst and behaviorist as

well as between behaviorist and cognitivist. The established regime may still evaluate ideas in that same fashion. Yet, from my experience, newer scholars exercise a myriad of views in psychology. For example, as a clinical scientist, I focus on trying to understand the processes that influence pathological gambling. To do this, I review articles that examine pathological gambling and social gambling as well as those that focus on genetics and neurochemical effects. As Sternberg and Grigorenko (2001) implied, it would be ignorant of me to disregard other systems that may shed light on the research problem.

Conclusion

Sternberg and Grigorenko (2001) presented steps to achieve unification that are unnecessary. For some time now, psychologists have examined convergent evidence to understand psychological phenomena through programmatic research. Journals already recognize interdisciplinary perspectives and accept manuscripts from various subdisciplines. This trend will likely continue. As newer, less entrenched scholars enter the field, they will unify psychology by embracing all paradigms and methods. In summary, unification is coming whether it is prescribed or proscribed.

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Theory Knitting Reconsidered

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Sternberg and Grigorenko (December 2001) presented a compelling case for establishing a unified psychology. However, they may have relied too heavily on "theory knitting" as a panacea and the critique of previous proposals regarding the solutions to the unification of psychology.

An example of theory knitting is Sternberg's (1985) triarchic theory. As an integrative approach to theory development, theory knitting usually guides the theorist away from

the segregative effects of masking the new theory—the failure to explicitly identify the theoretical construct that underlies the new theory or phenomena (Kalmar & Sternberg, 1988). Underlying all the reasons for psychologists to change their approaches toward studying phenomena is the part–whole relationship. According to Sternberg and Grigorenko (2001), two solutions address this confusion for psychologists: (a) get rid of bad habits by training in a broader set of methodologies, that is, multimethodologies, or (b) work in teams having members with various kinds of expertise, that is, a multidisciplinary team. They preferred the training construct and made a strong case for it while merely alluding to the second one as simply “work in teams” (Sternberg & Grigorenko, 2001, p. 1073).

However, to suggest that a team of members with various fields of expertise is a solution says very little about how its features are associated with an integrated theoretical approach, because there are so many different kinds of teams organized in different ways to perform different functions (Zeiss & Steffen, 1996). To illustrate, compare a multidisciplinary team, which I believe is what Sternberg and Grigorenko (2001) are suggesting, with an interdisciplinary team. On the one hand, in this context, a multidisciplinary team is one that is composed of members from several disciplines working independently of each other. On the other hand, in the interdisciplinary team structure, “Leadership functions are shared among members . . . everyone must be equally committed to . . . work together” (Zeiss & Steffen, 1996, p. 427) as a unit and thereby significantly enhance social integration among team members. Given the nature of this approach together with attention focused on the psychologist’s traditional role, the source of professional identity is suspended, and so is the need to give up a well-honed methodology. In considering work in teams having members with various kinds of expertise, each psychologist would still have, in a less intrusive way, access to others’ disciplines and converging operations.

Also, within this interdisciplinary team solution, the presumed confusion associated with the part–whole relationships and exemplified by the parable of the six blind men and the elephant would no longer require the psychologist to be trained in a variety of other fields “to understand the whole phenomenon” (Sternberg & Grigorenko, 2001, p. 1074). Sternberg and Grigorenko’s approach not only masks an underlying theory but also fails to explicitly identify the theoretical constructs that constitute a new construct—one that is integrative in the nature of an interdisciplinary team. Given the differences in the

underlying structure among these teams, are Sternberg and Grigorenko failing in the process of integrating theory development by not attempting to uncover those guiding assumptions and integrate them into a new framework?

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Seeing the Forest and Seeing the Trees in Psychology

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My critical appreciation of Sternberg and Grigorenko’s (December 2001) charming article has three points: (a) appreciation of its elegant clarity, (b) my uneasiness at the dilemma it creates, and (c) my proposal to take it as a call, not a discipline.

First, Sternberg and Grigorenko’s (2001) article is a marvelous model of psychological prose, charming readers with elegant clarity, ready explanations, and apt examples. I emulate them in my critical appreciation here.

Sternberg and Grigorenko (2001) warned that psychology today is fragmented into specialties, each locked in its own presupposition, paradigm, and methodology and taken as ultimate, complete, and comprehensive. Such a *pars pro toto* approach—taking parts for the whole—misses the forest (i.e., psychological phenomenon, psychology) for the trees (i.e., aspects, specialties). This approach distorts the whole picture (i.e., psychological phenomenon) and fragments the whole discipline (i.e., psychology).

Sternberg and Grigorenko (2001) proposed an alternative. Psychologists must go

not for splintered specialties or for a general psychology that randomly collates specialties but for a multidisciplinary unified psychology with multiple converging operations and various paradigms “integrated in a unified way” (Sternberg & Grigorenko, 2001, p. 1077).

Second, I am uneasy about the dilemma created by Sternberg and Grigorenko’s (2001) article. Their unified psychology looks persuasive until more closely scrutinized. Is it one discipline or many? Obviously it is one because it is unified (although it implies many by emphasizing cross-disciplinary studies on psychological phenomena). However, the unified one requires a particular paradigm, for example, a Hegelian synthesis (Sternberg & Grigorenko, 2001, p. 1075), and this paradigm would have its own axe to grind and would be one among many paradigms (e.g., Staats’s, 1991, paradigm). In fact, Sternberg and Grigorenko admitted, “In this article, we propose one such form that the unification of psychology might take, which we refer to here as *unified psychology*” (p. 1071). Is it, then, one more specialty among others?

Sternberg and Grigorenko (2001) urged psychologists to study “a basic psychological phenomenon” (p. 1075) with many paradigms (evolutionary, biological, cognitive, behavioral, psychoanalytical, genetic–epistemological, etc.), but these paradigms and perspectives conflict with one another. “There is no one correct perspective. Each perspective presents a different way of understanding the problem of learning” (Sternberg & Grigorenko, 2001, p. 1075). How can psychologists integrate those conflicted perspectives and theories on one psychological phenomenon? It is well-nigh impossible because to be versed in one specialty alone requires so much investment of time, labor, and cost, leaving little else to integrate various specialties (see Sternberg & Grigorenko, 2001, p. 1072). However, if integration among specialties, theories, and perspectives is not found, psychology will remain fragmented as it is now.

Even if psychologists study phenomena, not fields or disciplines, as long as conflicts among perspectives and specialties remain, two scenarios would result. First, unified psychology would become another specialty among many. Unified psychologists would specialize in converging many methods or disciplines and would develop technical language that is specific and exclusive to unified psychology. Second, unified psychology would study phenomena without integrating diverse specialties, and many specialties would remain fragmented.

Now what has happened here? I see a usual third-man dilemma. One divides a matter into unified versus specialized, one versus many. Where does the person doing this di-

viding belong? Sternberg and Grigorenko (2001) fragmented psychology into unified versus specialized, one versus many, each with its rationales, and now where do the authors belong?

Concretely, Sternberg and Grigorenko (2001) saw the problem of pluralistic fragmentation. The alternatives are to be general or integrated. The authors rejected general and random collation in favor of integration. This choice implicates one discipline—unified psychology—and psychologists raise their eyebrows and ask, how? They then take it to mean unified, and psychologists' raised eyebrows begin to knit as they ask, is it now one among many? Psychologists then identify unified psychology as a synthesis that would indeed risk becoming another specialty among many, thus furthering the fragmentation that Sternberg and Grigorenko were trying to dispel.

Third, I propose that psychologists take unified psychology to be a call, not a discipline. This approach offers unified psychology a way out of its third-man dilemma. Replace *general* with *open-minded* and *integrated* with *collaboratively integrated* (i.e., *open-minded* and *interlearning*), and one can see how legitimate, relevant, crucial, and free of the above theoretical dilemma unified psychology turns out to be. In this way, unified psychology can issue a clarion call to open today's hermetically sealed specialization and fragmentation and to promote interlearning and cooperation in tackling common, basic, and actual psychological phenomena, each interrelated with the others. However, a call to open the door is just that, a call, and not another discipline or specialty beside others, such as Staats's "unified positivism and unification psychology" (see Sternberg & Grigorenko, 2001, pp. 1069–1070).

I clinch my critical appreciation of Sternberg and Grigorenko's (2001) article by following their charming concluding metaphor, "vintage old wine in a new and better bottle" (p. 1078). I urge them to keep the vintage wine out of the bottle (i.e., a labeled specific discipline) and to force none into a particular paradigm of integration, such as the so-called Hegelian synthesis in unified psychology.

Specialization has been inevitable, with legitimate causes. Besides, who would not wish to be open to novel directions, paradigms, and approaches? The call to open specialty doors must be issued, but the call itself should not be another specialty (e.g., unified psychology). Let the wine of openness be imbibed everywhere to inspire everyone, but the wine should not be bottled up, for then a specialty bottleneck will emerge to choke the flow of wine and inspiration. Let the spirit of openness forever pervasively intoxicate all specialists in psychology so that they sponta-

neously accept, learn from, and enrich one another.

The call (or wine) bespeaks a milieu where everyone grows. Scheidlinger (1999) mentioned a *mother-group therapy* where everyone mother-supports everyone else—to heal. The mother group is the mother milieu that consolidates diverse integrities (i.e., the integrities of various specialties) to enliven diversity. Diversifying specialties invigorates thorough research; sealed-off specialization chokes all to death. Diversity is possible only in the motherly milieu of concerted and supportive openness among various specialties. Here sealed-off specialization fades and specialties' diversity grows into both one and many (i.e., a psychology with many interconnected subdisciplines). Here diverse specialties facilitate enrichment of one another and move toward an unbiased, holistic, and integrated approach to every basic psychological phenomenon. Let such a clarion call and wine of motherly milieu intoxicate, habituate, and inhabit all psychologists in their various specialties, resulting in an abiding attitude of being forever open one to one another.

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E Pluribus Unum

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In Sternberg and Grigorenko (December 2001), we argued for a perspective on psychology that we called *unified psychology*. This perspective is characterized by the use of converging operations, a focus on contextualized psychological phenomena rather than methods or paradigms, and the encouragement of a diversity of views about what to study and how to study it. Our critics, to

whom we are grateful for taking the time to respond to us, take issue with various aspects of our proposal.

First, we should note that our critics disagree at least as much among themselves as they do with us. Kassinoe believes that "unification is inevitable" (p. 1127), and Lau (2002, this issue) referred to the call for the unification as "welcome" (p. 1126). Kendler stated that in developing unified psychology, we "ignored fundamental methodological differences within the discipline" (p. 1125), and Chao (2002, this issue) believes that "it is well-nigh impossible because to be versed in one specialty alone requires so much investment of time, labor, and cost, leaving little else to integrate various specialties" (p. 1128). So either unification is inevitable or it is impossible. One thing is clear: Our critics are not unified!

We respect all of the opinions offered here and would just like to make a few statements about each. With regard to Kassinoe (2002), we doubt that "a changing of the guard is imminent as new scholars examine previous paradigms and respect all theoretical perspectives" (p. 1127). Every generation thinks it is the changing of the guard—the one that will cure past ills. None quite does. The junior scholars of today will, sooner or later, be the senior scholars of tomorrow, and they will be as susceptible to the entrenchment that expertise can bring as are today's senior scholars (see Sternberg & Lubart, 1995). Everyone can fight this kind of tunnel vision. A few scholars do (Sternberg, 2002); many do not. They pay for their expertise by narrowing their focus. We also disagree with Kassinoe that "programmatic research . . . is a form of convergence" (p. 1127). Research can be programmatic and use only one type of operation. For example, someone might have a program of research using just factor analysis, or functional magnetic resonance imaging, or case studies, or any other methodology. The fact that research is programmatic does not mean it uses converging operations.

With regard to Lau (2002), we are not experts on postmodernism, but we appreciate the parallel Lau drew in his comment. We agree that science is never wholly value free. At the same time, we think it is dangerous to lapse into believing, as do some people (although not necessarily Lau), that because all of science is value laden, it is all relative anyway. Science is unique among approaches to knowledge in that it is self-correcting: Through strong empirical research, good theories gain support and bad theories sow the seeds of their own destruction. Other approaches to knowledge seem generally not to possess this self-correcting feature.

Contrary to Chao (2002), we do not see unified psychology becoming "another spe-

cialty among many” (p. 1128). Unified psychology cannot become a specialty because it does not have specialized content. Rather, unified psychology is an umbrella framework within which people can specialize in particular domains of inquiry, such as the emotions, or consciousness, or memory, or altruism, or any of a number of other psychological phenomena.

Chovan (2002) suggested that one needs to distinguish between teams that are simply collections of individuals working relatively independently and teams that are working together. Our use of the term *team* simply followed the dictionary definition of “a group of people working together in a coordinated effort” (Neufeldt & Guralnik, 1997, p. 1373). Thus, we were referring to individuals working in a coordinated rather than an independent way.

Finally, we do not agree with Kendler (2002) that our plea for unification “ignored fundamental methodological differences within the discipline” (p. 1125). On the contrary, we embrace methodological differences. Our whole point is that different methods should converge on common understandings of psychological phenomena—that as a field, psychologists should focus on phenomena, not methods. Contemporary psychologists may indeed have different criteria for truth, as Kendler argued. But a wonderful characteristic of science is that it does not care what the criteria are. Ultimately, because science is self-correcting, the truth will out. Science, as a way of knowing, ultimately transcends the imperfections of its individual practitioners. There are many approaches to science, but ultimately they will come together. *E pluribus unum*.

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Swinging Pendulums

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The article summarizing the findings and recommendations of the Task Force on the Status of Women in Academe (Kite et al., December 2001) represents a considerable effort to address perceived gender injustices in academic psychology. Because of the amount of work involved and because the authors obviously were writing with only the best intentions, it seems churlish to criticize it. However, the task force report represents only one viewpoint, a particularly politicized viewpoint at that, and one that is not shared by all women (or men) in psychology. Further, the report conveys the unfortunate and dangerous impression that women are not able to succeed in academia unless they are awarded special treatment. In particular, I take issue with the following themes that permeate the task force report.

Differences in Outcomes Necessarily Mean Gender Bias

Data provided in Table 1 of the Kite et al. (2001) article show that women's base salaries are only 84% of men's, and their total earned income is only 75% that of men's (p. 1082). Raw differences in means do indeed look suspicious, but the authors' conclusions can be debated on two points. First, their data are rather out of date, drawn from a 1993 survey. Salaries for 2000–2001 are available on the American Psychological Association's (APA's) Web site, broken down by gender and time in rank (Wicherski, Pate, & Kohout, 2001, Table 30). These data show no gender gap; across the nine reported levels of time in rank, women's salaries averaged 100.1% of the corresponding men's salaries for that level. At advanced levels, women's salaries actually exceeded men's by considerable amounts. For example, female full professors with 12 or more

years in rank earned an average of \$93,330, compared with \$89,373 earned by males at the same level.

Second, raw differences in mean income—regardless of which sex they favor—cannot be unambiguously offered as evidence of sexism. Before one can conclude that unfairness is taking place, one needs to take into account other factors. Salaries are determined by a number of variables, not the least of which are years of postdoctoral experience and merit. Analyses that take into account such relevant variables as years since the doctorate was earned and time in rank generally show no significant difference in men's and women's salaries (Lamb & Moates, 1999).

Women Need Special Nurturing to Succeed in Academia

The Kite et al. (2001) article conveys the disturbing message that women need special nurturing or allowances to succeed in academia. For example, take the section on women in leadership. The authors devoted a full page to the “physical and emotional toll” (p. 1085) inherent in being a female administrator and noted that “it is important for a woman administrator to maintain her physical and mental health by understanding the political environment, guarding her mental health, and using support groups and mentors” (Kite et al., 2001, p. 1085). They argued also that others around the female administrator should “provide her active support by taking action and speaking out to counter attempts to sabotage or undermine her leadership” (p. 1085). This argument unfortunately conveys the sentiment that women are so fragile they cannot hack it as an administrator unless somebody helps them. More seriously, this message could contribute to stereotype threat processes (Steele, 1997), whereby women become aware of the stereotype or belief that they need special nurturing to succeed and consequently perform less well. Past research on self-induced dependence similarly shows that helping somebody unnecessarily on a task can undermine subsequent performance.

Women Should Receive Preferences in Hiring

The second recommendation in Appendix A, Part III, Section A, of the Kite et al. (2001) article reads, “Offer incentives . . . to departments now hiring and promoting women and ethnic minorities at levels equal to or better than those represented in the employment pool” (p. 1096). “Better than,” of course, is impossible to accomplish without granting gender preferences in hiring and promotion.

Leave aside for now the fact that this recommendation violates federal law forbidding discrimination on the basis of gender or race. Consider instead the long-term damage to the field of psychology of making hiring and promotion decisions on any basis other than merit. If over the years what matters more is what one's race or gender is than how good one's research is, then psychology may be costing itself the contributions of talented psychologists who had the bad luck to be white and male.

Many would argue that gender preferences are an unfortunate necessity to counteract the sexism that has plagued American society for so many years. And yes, for many years sexism existed at universities, as seen in Ellen Berscheid's compelling description of the discrimination faced by women with doctorates in the 1960s, such as job announcements that specified "male" as the only qualification (Berscheid, 1992). If women are doing well today in psychology, it is because of the efforts of pioneering female faculty like her.

But such sexism does not exist today, and it would be wrong to claim that it does. The more recent salary data described earlier (Wicherski et al., 2001) indicates there is equity in compensation. More women than men are now receiving doctorates in psychology; for example, women accounted for 69% of new doctorates in 1997 as opposed to only 33% in 1976 (Kohout & Williams, 1999). Women appear to receive equitable, if not preferential, treatment in clinical internship placements, as shown in an APA report indicating that 87% of female internship applicants received a placement on the uniform notification date, compared with only 71% of the male students seeking internships (Pate, 2001). Little empirical data exist on the extent of possible sex discrimination in faculty hiring, although one longitudinal study that compared male and female candidates for a tenure-track position revealed no sex difference in how they were portrayed in their letters of recommendation (Bronstein, Black, Pfennig, & White, 1986). This study also found that more men than women applied for the position, raising the possibility that women are self-selecting out of academic careers. Although a follow-up revealed that male applicants were more likely to obtain higher status jobs, the authors acknowledged that this could be accounted for by the greater degree of postdoctoral experience held by the men (Bronstein et al., 1986), echoing the point made earlier that raw differences in outcomes cannot be unambiguously interpreted and that other factors such as experience must be taken into account.

In short, the available data suggest that women are doing very well in psychology. I would argue, in fact, that the pendulum has

swung nearly as far in the opposite direction. No job notices today read "White males only." But when gender and race preferences are extreme, and they sometimes are, it subverts the noble goals of faculty diversity and is ultimately self-defeating.

Women Should Not Be Expected to Achieve International Reputations

The task force recommendation I found most disturbing was the last one listed in Appendix A, Part V: Enhancing the Environment for Women as Researchers (Kite et al., 2001), which reads,

In evaluating faculty on the basis of their national and international reputation, recognize that some highly productive women doing high quality work have had limited opportunities to develop such reputations. Reasons may include limitations on their ability to travel due to the lack of access to child care and the need to meet family responsibilities. (p. 1097)

No evidence was offered to support the basic premise underlying their recommendation, that is, that national and international reputations require travel to develop. I would argue that the strength of a scholar's reputation should be and largely is a function of the quality of that scholar's research. People doing good work that is published in the top journals of the field are generally recognized as the eminent researchers they are. But let us grant, for the sake of argument, that this premise is true. The task force recommendation implies strongly that women should be evaluated using lower standards and that administrators should not penalize women for their inability to develop a strong reputation.

I can think of few courses of action ultimately more self-defeating to the status of women in academia than to adopt dual standards of evaluation. If a national reputation is considered by an institution to be an important criterion for tenure or promotion, then that criterion should be applied equally to all candidates, male or female, Black or White, gendered or transgendered. To do anything else subverts what a promotion signifies. And it is unfair to the institution, the institution's students, and any man who gets turned down on the basis of an inadequate reputation.

Of course, an easy solution to this latter unfairness (as well as the problem of potential litigation by said turned-down man) is to lower the standards for everybody. This is a more fair but still ultimately self-defeating solution. Yes, more women would be tenured or promoted if administrators simply dumbed down the criteria for promotions. But would they be doing their students, their colleagues, or the field of psychology any favors? I do not think so. All would do well to heed the lessons of researchers in industri-

al and organizational psychology who show that ignoring ability or merit in employment decisions is counterproductive and produces severe performance decrements and economic losses (Hunter & Schmidt, 1996).

Closing Thoughts: Beware of Swinging Pendulums

Because the task force report (Kite et al., 2001) went beyond merely summarizing the status of women to making recommendations (81 total), and because these recommendations carry the imprimatur of APA and thus the remote possibility they might actually affect some institution's policies, they warrant special scrutiny. Most of the recommendations reflect common sense and are uncontroversial, such as providing new faculty with reduced teaching loads and start-up packages. But the overall tone of the report and its recommendations imply that women deserve and need special treatment or favors, and as such it hints of benevolent sexism, the notion that women are fragile objects to place on pedestals and who need to be sheltered and protected. Special treatment or discrimination in favor of women is still discrimination. Discrimination was not fair in the 1960s and it is not fair today. As a female psychologist, I do not need—nor do I want—special favors.

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Neglected Aspects and Unsupported Claims

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We resonate with Kite et al.'s (December 2001) concern for women in academe and applaud them for detailing women's many advances over the past quarter century. As they documented, differences in the activities of men and women in the academy persist. It is not clear, however, that "inequities persist" (p. 1080). Kite et al. interpreted the observed differences in outcomes between men and women as self-evident indicators of remaining bias and discrimination. There are two problems with this interpretation. First, the authors neglected important variables that suggest alternative interpretations of these differences. Second, the authors made unsupported claims about the existence of bias and discrimination against women. Below, we document specific cases of these errors in reasoning.

Neglected Aspects

Kite et al. (2001) noted that disparities persist between the sexes in rates of tenure and annual salary. They neglected to emphasize at least two variables critical in explaining these differences: professional age and number of hours worked. In the National Research Council's (NRC, 2001) study on gender differences in the careers of doctoral scientists and engineers, for example, controlling for number of years since receiving one's doctoral degree reduced considerably the gap between the sexes in faculty rank and annual salary. In the life sciences, the gender gap in rates of tenure was reversed when career age was controlled (NRC, 2001, pp. 165–170). Recently, Benbow, Lubinski, Shea, and Eftekhari-Sanjani (2000) illustrat-

ed why full-time work should not be conceptualized as a categorical variable. In their study, intellectually talented men and women differed in the number of hours they preferred to and actually did work. Benbow et al. found that sex differences in earnings (within specialized area) were nonsignificant after controlling for the number of hours worked.

Kite et al. (2001) reported on advances women have made in number of publications relative to men, emphasizing publication quantity as a measure of female progress. They neglected to comment on publication quality, which has been examined systematically for two decades. Across both scientific and non-scientific domains, men and women manifest comparable citation rates per publication (Cole & Zuckerman, 1984; Persell, 1983). This finding argues against Kite et al.'s conjecture that women's contributions have been consistently devalued relative to men's.

Kite et al. (2001) described Park's (1996) suggestion that "if service activities are viewed as 'women's work,' they are typically devalued; in contrast, service activities viewed as 'men's work' are seen as more complex and difficult and, consequently, of higher status and value" (p. 1083). The authors therefore proposed that certain activities are devalued precisely because they are performed by women. This reasoning fails to take into account actual task complexity. The authors' own findings indicated that men in administrative positions more often serve as department chairs, whereas women more often serve as program heads (p. 1082). Following Park's logic, Kite et al. seem to imply that the position of program head is less valuable than that of department chair simply because it is more often occupied by women. But isn't the position of department chair in fact more complex and demanding than that of program head?

Kite et al. (2001) argued that "feminist scholarship has reduced the rampant mother-blaming" (p. 1087) for the development of disorders such as schizophrenia and autism in their offspring and has expanded the social network of blame for these maladies to "include fathers, peers, the schools, and the media" (p. 1087). Kite et al. neglected to mention that behavioral genetics research has repeatedly documented evidence of a strong genetic influence in both of these disorders. Furthermore, recent molecular genetics studies have shown promising results in the identification of specific genes responsible for autism (Rutter, 2000). The contributions of behavioral and molecular genetics have arguably been much more instrumental in reducing mother blaming than have the contributions of feminist psychology. For Kite et al. to emphasize social explanations to the

exclusion of compelling biological advances constitutes a serious error of omission.

Unsupported Claims

Kite et al. (2001) stated that "sexism is still a deterrent for women leaders" (p. 1085) but provided no evidence to substantiate their claim. They further cautioned women who are considering administrative goals that "hiring bodies may hold them to a higher standard" (p. 1084); again, they offered no evidence. To our knowledge, no evidence exists. But we agree that it is important to collect evidence to ascertain whether standards have been raised—or lowered—for different groups.

Similarly, Kite et al. (2001) discussed the relentless challenge women face in dispelling stereotype threat, and they stated that women "encounter many barriers that their male colleagues never have to confront" (p. 1091). Yet, experimental demonstrations of stereotype threat have not consistently replicated across samples and laboratories. Moreover, its external validity has not been established (Sackett, Schmitt, Ellingson, & Kabin, 2001, pp. 309–310); in fact, failures to replicate it in common real-life testing situations have threatened its tenability (Stricker & Ward, 1998).

Finally, Kite et al. (2001) reviewed several studies documenting differences in students' evaluations of the effectiveness of male and female teachers, and they concluded that these differences reflect gender bias. This interpretation is flawed: The mere observation of a group difference does not imply an actual bias (Sackett et al., 2001); it could simply reflect a real difference between the groups on the attribute in question. To gain evidence of a bias in students' evaluations, one must eliminate this latter alternative by comparing evaluations to some objective measure of actual instructor performance. If comparable differences in instructor performance are not observed, then Kite et al.'s case for bias in student evaluations becomes possible.

Conclusion

Kite et al. (2001) presented differences in outcomes between men and women as self-evident indicators of bias and discrimination, yet decades of empirical work have demonstrated that merely documenting group differences on a measure or outcome does not imply bias. We maintain that the observed differences between men and women might be partly a reflection of other (neglected) personological variables on which the sexes overlap considerably but differ on average (e.g., status seeking, interest in people versus things, prioritization of work and family). Just as differential outcomes do not imply differential opportunities, equal opportunities do not necessarily produce equal outcomes.

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Women in Academe: Is the Glass Completely Full?

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Harris (2002, this issue) and Bleske-Rechek and Webb (2002, this issue) have questioned some of our conclusions and recommendations (Kite et al., December 2001) regarding the status of women psychologists in academe. Here we expand on some of the points made in our 2001 article, many of which were discussed in greater detail in a full-length report (Task Force on Women in Academe, 2000). We address four primary issues: equity in compensation, the question of special treatment, the importance of continuing to transform the academy, and the need for continued vigilance and monitoring to ensure that advances in gender equity do not prove to be just a passing phase.

In regard to equity in compensation, as we emphasized in our article (Kite et al., 2001), “salary differentials have received substantial attention in the past decades, and women have used salary equity surveys to good effect in arguing for equity adjustments” (p. 1081). Harris (2002) concluded that there is no significant gender gap in academic psychologists’ salaries, a conclusion based on 2000–2001 salary data available on the American Psychological Association’s (APA’s) Web site. Space does not permit us to provide a detailed analysis of the data sets used in our article compared with hers. Three points are crucial, however.

First, although Harris (2002) claimed that the APA data set is superior because it is more recent, in fact the data set is limited to graduate departments of psychology and to only those graduate departments that responded to the APA survey. The National Center for Education Statistics (NCES, 1993) data set that we used, albeit older, was based on a stratified sampling of faculty (not departments) and thus included faculty in four-year and two-year schools, in addition to faculty in schools granting graduate degrees. Differences in outcomes between the two data sets may therefore not represent trends over time but instead may represent differences between the selective sample of departments represented in the APA data set versus the more comprehensive set of departments represented in the NCES data. Furthermore, the APA data are more likely to include departments that have been held up to the lens of APA

accreditation scrutiny, which includes an examination of gender disparities in treatment of faculty and students.

Second, a focus on base salary misses the point. The pressing concern is disparity in total compensation. Our analyses found a relatively small discrepancy between women and men in base salary (Kite et al., 2001). The larger discrepancy was in the category of *other income*—other income provided by the university, such as summer salary, and income from outside sources, such as consulting. The APA salary data did not address the issue of total compensation.

Third, even if we assume for the moment that the gender gap in base salary is small—say 5%—that small gap can mask considerable variation from one department to another, with some manifesting perfect gender equity in policy and practice and others displaying marked gender bias. If you are a woman who is seriously underpaid, it is cold comfort to learn that, on average, base salaries aggregated across all colleges and universities in the United States show only small gender discrepancies. Although we celebrate the signs of progress, we also emphasize the dangers of naiveté and complacency. We applaud the departments that are fair in their compensation of women and men. Yet even a well-intentioned department cannot know whether compensation is equitable without conducting the necessary analyses. We urge departments and universities to conduct those analyses and to correct salaries when necessary. We were unable to locate statistics on the number of colleges and universities that have done an actual gender-equity study of faculty pay.

Regarding the question of special treatment, our concern is that advocating programs to address inequities will be misinterpreted as asking for special treatment and dual standards of evaluation in which women are held to a lower standard than men. Yet a so-called gender-blind approach is unsatisfactory because the division of labor by gender is not equal. The issue, as we see it, is not special treatment for women, but rather how the playing field can be leveled for academic women, given the pervasiveness of the unequal division of labor in the family. Moreover, programs that may superficially seem to provide special treatment for women typically, in the end, benefit both women and men. Examples include on-site child care and parental leave policies. In our experience, programs that prove to provide effective support for women faculty often are later expanded to include men, and everyone benefits.

Eagly and Wood (1999) argued that the division of labor by gender is an enormous and crucial force, driving many gender-

differentiated behaviors and outcomes. The largest of these inequities in the division of labor lies in the realm of child care, for which women bear a disproportionate responsibility. The situation is particularly acute for women faculty, who work to earn tenure during their childbearing years and who may have disproportionate responsibility for care of their children. Their ability to travel nationally, much less internationally, may well be limited. Academics attend professional meetings for many reasons, one of them being to increase their own visibility and the visibility of their work, especially to the high-profile gatekeepers who will write the letters evaluating them for tenure.

One might say, reflecting the individualism of American culture, that bearing and rearing children is a woman's individual choice, and she should face the consequences of that choice. Alternatively, one might argue (a) that academic institutions should adopt a humane and wise approach by providing supports, such as on-site child care and parental leave policies, that level the playing field for women and (b) that academic institutions lose valuable contributions to research and teaching if they do not provide such supports, either because women leave unsupportive schools and move to more hospitable ones or because women waste time and intellectual energy solving problems, such as finding child care, that could be solved by the institution.

All too often, women and men are asked to take on different faculty responsibilities. Female faculty, for example, often are asked to perform more teaching and service than men. The Task Force on Women in Academe (2000) urged both women faculty and those who evaluate them to be cognizant of these demands, especially because time spent in service is time taken away from research, and research drives the outcome of the tenure and promotion decisions in many institutions.

We also emphasize the importance of continuing to transform the academy. The report of the Task Force on Women in Academe (2000) summarized the meta-analytic evidence supporting the hypothesis that women leaders are judged by different standards than are men. Women especially are judged negatively if their behavior violates gender-role norms. The bottom line is that in all likelihood, sexism in the evaluation of women's work still exists in some departments. It is just more subtle today than it was in the past. Moreover, the NCES (1993) data, cited in the task force report, indicated that some women feel they are not treated fairly. Overt discrimination has now been replaced with "microinequities" and "unintentional slights" (Task Force on Women in Academe, 2000, p. 14). The academy provides a much-improved environment for women faculty now compared with 30 years ago, but much can still be improved, and aggregate trends showing improvement may mask little or no improvement in individual departments.

Finally, we emphasize the need for continued vigilance on issues of gender equity. As Appendix A in the report of the Task Force on Women in Academe (2000) detailed, periods of progress for women in psychology may be followed by periods of backsliding or backlash, a pattern that is widespread in the larger society (Faludi, 1991). For example, although we optimistically reported in our article (Kite et al., 2001) that in 2000, 32.2% of the editors of APA journals were women, in 2001 that figure had fallen to 25.8%. As another example, we reported that women won 29% of the APA awards for distinguished scientific contributions between 1990 and 1997 and 31% of the early career awards for the same period. Yet in 2001 all three distinguished scientific contribution awards went to men, and only one of seven early career awards (14%) went to a woman.

The Task Force on Women in Aca-

deme ultimately looks to educational institutions to ensure gender equity. Data must be collected, recommendations for change must be provided, and accountability must be in place in order to produce positive change. In short, although we wish that the glass were completely full for women in academe, it is only partly full—or partly empty, depending on one's viewpoint. We urge departments, colleges, universities, and APA to continue to press forward on these issues. Without forward movement, women and the institutions in which they work are likely to slide backward.

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