

research design one uses (longitudinal, cross-sectional, experimental, quasi-experimental, etc). It does not depend on the sample (e.g., American or Nigerian). Science is characterized by testing and falsifying theories (Meehl, 1978).

In light of this philosophy, it is unclear why research on cultural context should be considered more scientifically progressive than research on basic processes. In fact, Arnett's (2008) description of cultural research raises concerns that it could actually slow progress in psychology. His vision of cultural psychology does not invoke theory or the importance of having testable hypotheses. Rather, cultural psychology appears to be exploratory and descriptive in nature. Will cultural psychology simply be an anecdotal record of cultural differences or a collection of replication studies? Will 100% of the world's population have to be studied before psychology can be considered a "complete science?" Arnett failed to provide any information about how cultural psychology will progress as a science.

From a philosophy of science perspective, Arnett's (2008) distinction between cultural context and basic processes is a false dichotomy. The problem with human psychology is not its focus on basic processes rather than cultural context; it is the lack of strong falsifiable theories (Meehl, 1978). Cultural context cannot exist in a vacuum isolated from basic processes such as cognition, perception, language, and so forth. If cultural research is to take hold in psychology, then it must be theory driven and integrated into work on basic processes. It is not enough to surmise that different cultures may lead to different outcomes. Researchers need to specify the conditions for when they would and would not expect culture to affect basic processes and behaviors.

Cultural context can serve an important purpose in psychological science: It will enable us to test hypotheses about which features of human behavior are acquired through experience and which are basic (or innate). Basic processes are mechanisms via which humans—and other animals—are able to respond adaptively to typical environments; however, these processes can be distinguished from another kind of adaptation, acquired associations or strategies (such as reading), which vary across situations and cultures. Within this framework, cultural adaptations can be thought to arise from the operation of basic processes, such as learning.<sup>4</sup> For example, at one time it was thought that language was acquired solely through imitation of and reinforcement by models within one's sociocultural context (e.g., Skinner's, 1957, *Verbal Behavior*), until Chomsky's synthesis of cross-cultural linguistic

variation revealed important similarities across cultures, suggesting that language acquisition also depends on a more basic structure or process that all humans share. Similarly, conventional wisdom suggests that abstract mathematical concepts are learned through years of formal education and training; however, studies of hunter-gatherer cultures (e.g., the Pirahã; Gordon, 2004) and even of nonhuman animals (e.g., monkeys, rats, pigeons; Gallistel & Gelman, 2000) have shown that we all share a common system for representing the abstract concept of number. In clinical psychology, many assume that eating disorders such as anorexia nervosa and bulimia nervosa share a common genetic etiology. However, recent research suggests that the genetic diathesis for bulimia nervosa may exhibit greater pathoplasticity cross-culturally than the diathesis for anorexia nervosa; this finding indicates distinct etiologies for these disorders (Keel & Klump, 2003). These examples highlight the importance of using cultural context to test theories about basic and acquired human behavior.

## Conclusion

Focusing on cultural context *rather* than basic processes is not going to advance American psychology, or psychology in general. Neither are having students travel abroad or take anthropology classes (as recommended by Arnett), in and of themselves. Rather, science will advance by developing and testing theories. We believe that psychological science can benefit most by using differences in culture and context to develop and test novel hypotheses about basic human processes.

<sup>4</sup> Note that this formulation of the purpose of cross-cultural psychology differs markedly from Arnett's (2008), which espouses cultural representativeness as a goal unto itself.

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DOI: 10.1037/a0016723

## The Neglected 95%, a Challenge to Psychology's Philosophy of Science

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My goal in writing "The Neglected 95%: Why American Psychology Needs to Become Less American" (Arnett, October 2008) was to fuel a conversation in psychology about whether American psychological research should become more reflective of how human beings in different cultures around the world experience their lives. I am pleased to see that many of my colleagues have taken up this conversation, as represented in the four comments *American Psychologist* is publishing in this issue. The four comments were well chosen in that they represent quite different reactions to my article. Two of the comments were generally in support of my thesis that American psychology is too narrow culturally, and sought to provide additional information on the issues I raised. The other two comments were in opposition to my thesis and presented the grounds for their

opposition. In this rejoinder I address the issues raised in each of the comments, first the two supporting comments and then the two opposing comments. Following this, I address the more general problem that cuts across the comments: American psychology's dominant philosophy of science.

### **Strategies—and a Caveat—for Reaching the Neglected 95%**

LoSchiavo and Shatz (2009, this issue) agreed that my analysis of articles published in APA journals shows that American researchers in psychology have focused too narrowly on Americans while neglecting the other 95% of the world's population. However, they placed the origin of the problem not mainly on a misguided philosophy of science, as I did (Arnett, 2008), but on practical issues, specifically "a lack of viable options for conducting research with international samples" (LoSchiavo & Shatz, 2009, p. 566). To remedy this problem they suggested the creation of a centralized network of multinational field sites "so that researchers can partner with international colleagues and collect data from samples that better reflect the whole of humanity" (p. 566). They also recommended increased use of Web-based research methods, which could make it possible to involve colleagues and research participants around the world without the expense and logistical trouble of international travel.

I support these suggestions. However, I would only raise the caution that multinational studies would have to be based on diverse culturally grounded theoretical perspectives and methods in order to be successful in addressing the problems I described in my article. For example, it would be a mistake to believe that taking American-based questionnaires and using them in 10 different countries would be an adequate way of representing the cultural contexts of all 10 countries. Questionnaires are laden with cultural assumptions, in the items chosen and the response options offered, so the methods used would have to be adapted to the range of cultural contexts involved, even if doing so would make it more difficult to compare the samples. Similarly, it would be pointless to use the same experimental laboratory methods in 10 different countries. If experimental laboratory methods strip away cultural context in one country, they will do so in other countries as well. What needs to change is not just the cultural range of samples used in psychology but the dominant philosophy of science.

### **Is American Psychology Already Becoming More International?**

Like LoSchiavo and Shatz (2009), Webster, Nichols, and Schember (2009, this issue) agreed with the thesis of my article. However, their appraisal of American psychology's current international representation was more favorable than mine. They conducted a journal analysis to supplement the one I presented and concluded that "substantial progress has been made over the last 30 years" (Webster et al., 2009, p. 566) in APA journals, toward representing a broader portion of humanity.

Their analysis included three journals that mine did not—*Journal of Experimental Psychology: General* (*JEP: General*), *Psychological Bulletin*, and *Psychological Review*—and they excluded two journals that had been part of my analysis, *Health Psychology* and *Family Psychology*. My analysis was over 20 years in 5-year increments; their analysis went back 30 years in 10-year increments. Furthermore, they analyzed national institutional affiliations of editors, associate editors, and consulting editors across five time points from 1980 to 2008. In contrast, my analysis of APA's editorial representation was for only one year, 2007. This is an impressive analysis that Webster et al. (2009) have conducted, with admirable swiftness (presumably in the few months since my article was published in October 2008). Perhaps only they and I can truly appreciate the tolerance for tedium that such an analysis requires.

Although I now consider Webster et al. (2009) my comrades in tedium, I do not share their sanguine interpretation of their results. As I see it, there are three problems. First, although they stated hopefully that the "glass" is now "half full" (p. 568), in my reading of their results it appears that three glasses are actually 37% (authors), 18% (consulting editors), and 21% (editors/associate editors) full, in terms of international representation in APA journals. Second, and more seriously, their analysis did not specify the proportion of non-American authors, editors, and consulting editors who were from English-speaking or European countries, as mine did. If, as in my analysis, nearly all non-American representation was from English-speaking or Western European countries, the cultural areas most similar to the United States, then the increased international representation they reported for recent decades is not as comforting as it appears. Adding English-speaking and Western European countries to the United States may raise APA journal representation to about 12% of the world's people (Population Reference Bureau, 2006), up from less than 5%

for the United States alone, but a human science that neglects 88% of the species it purports to study remains a dubious one.

Third, and most serious of all, it is not enough for a growing proportion of authors and editors to be non-American if nearly all of them, American and non-American alike, share a narrow philosophy of science that focuses mainly on basic processes and ignores or strips away cultural context. In Webster et al.'s (2009) analysis, the journal with the highest international representation was *JEP: General*, less than 50% of whose first authors in 2008 were American. However, *JEP: General* articles all use experimental methods that cast little or no light on the cultural context of the persons or the phenomena being studied. Similarly, in my analysis, the highest proportion of non-American authors and samples was for the *Journal of Personality and Social Psychology* (*JPSP*), but virtually all of the studies in that journal, no matter where they were conducted, involved samples of university students taking introductory psychology classes, and even non-American studies ignored the cultural context of the samples.

It is a dangerous illusion to assume that the mere presence of international authors, editors, or samples in APA journals would be enough to do justice to the cultural breadth and richness of the world's peoples. International representation is a necessary but not sufficient condition for a cultural perspective in American psychology. For this, a change in philosophy of science is necessary.

### **Psychological "Laws" and Theories**

Stroebe and Nijstad (2009, this issue) began their comment with a startling declaration: "It is a fundamental assumption of psychological science that, unless specified otherwise, our theories apply to all of humanity" (p. 569). This is a bold statement—and a remarkably ethnocentric one. Given human cultural diversity, how can it be justified to assume that a theory developed on the basis of research on a tiny proportion of the world's population can "apply to all of humanity"? As I noted (Arnett, 2008), this is certainly a strange way to conduct science.

Stroebe and Nijstad (2009) acknowledged that it is a scientific standard that the results of research can be generalized only to the population from which the participants were drawn. Nevertheless, they asserted that sampling from a diverse range of humanity is necessary only if the results of a study are expected to be moderated by other variables. "If no moderation is expected, any subgroup of the population will do equally well, even the often maligned

undergraduate students” (Stroebe & Nijstad, 2009, p. 569).

The problem with this position is that our theoretical expectations, and the variables we consider as possible moderators, are shaped by our cultural assumptions, whether we realize it or not. For example, a long-standing finding on parent–child relations in American research is that from middle childhood to adolescence, conflict increases and closeness decreases (Laursen, Coy, & Collins, 1998). On the basis of these findings, an American psychologist proposed a theory that “distancing” between parents and adolescents may have an evolutionary basis, in that it would be adaptive for young people to move away from closeness to their parents once they reach sexual maturity, so that they would mate and reproduce with persons outside the family (Steinberg, 1989). Yet among adolescents and parents in countries including India, Brazil, and Indonesia—all sharing the evolutionary history of *homo sapiens*—conflict does not increase and closeness does not decrease; adolescents enjoy being with their parents and feel closer to them than to their friends (French, Rianasari, Pidada, Nelwan, & Buhrmester, 2001; Larson, Verma, & Dworkin, 2003; Schlegel & Barry, 1991; Van Horn & Cunegatto Marques, 2000). Thus a researcher with knowledge of cultural variations in parent–adolescent relations would develop much different theoretical expectations, and search for much different moderators, than an American researcher who focused on the American pattern and yet assumed that a theory developed from this singular case applied to all of humanity.

Similarly, Stroebe and Nijstad (2009) argued that “a major weakness of Arnett’s (2008) argumentation is that he failed to distinguish between research that *tests general laws* of behavior and research aimed at *describing the impact of societal factors on behavior*” (p. 569, emphasis in original). As an example, they named AIDS research, arguing that for gay American men as for heterosexual African women, a theoretical model like the theory of planned behavior would apply equally well. The problem with this view is that often what psychologists proclaim as “general laws of behavior” are actually general “laws” of American behavior or, worse yet, general “laws” of American undergraduate introductory psychology student behavior (Norenzayan & Heine, 2005).

Theories are essential to good psychological research, but to be widely applicable—much less “laws”—they would have to be based on a broad knowledge of cultural practices, not just the ways of one culture. It would be a grave error to import

American-based theories and measures into cultures around the world and assume that they were based on “laws” that applied equally well to all peoples. To continue with the AIDS example Stroebe and Nijstad (2009) invoked, research has shown that a key factor in the transmission of AIDS in Africa is that men often migrate to urban areas in search of work, where they have sex with HIV-infected prostitutes before returning to their rural villages and unknowingly infecting their wives (Kalipeni, Craddock, Ghosh, & Oppong, 2008). Knowledge of this cultural pattern would be far more useful in developing interventions for HIV prevention in Africa than would applying an American theory like the theory of planned behavior advocated by Stroebe and Nijstad, which, in its American way, focuses entirely on individual-level variables of knowledge, beliefs, attitudes, and perceived behavioral control without attending sufficiently to social or cultural context.

### What Is Science? What Is Scientific Progress?

The most extensive of the four commentaries is the one offered by Haefel, Thiessen, Campbell, Kaschak, and McNeil (2009, this issue), who took the position that “Theory, Not Cultural Context, Will Advance American Psychology” (p. 570). Their main goal was to defend the value of research on basic processes (e.g., cognition, perception, learning) and question the value of culturally diverse research.

Haefel et al. (2009) are on shaky ground from the beginning. They showed the limits of their perceptions in asserting that “the problem of generalizability is often overstated” (p. 570), offering in support of this statement the assertion “Studies using one sample of humans (e.g., Americans) often generalize to other samples of humans (e.g., Spaniards)” (p. 570). Even adding Spaniards to Americans (and throwing in Canadians for good measure) still makes for less than 5% of the world’s population. Psychologists are far too quick to jump from one study of Americans and one study of Spaniards to a declaration of a universal psychological principle. It is not the problem of generalizability that is overstated but the research findings of psychologists based on a tiny and unusual segment of humanity.

There may be an effective case to be made for the value of psychological research on basic processes, but Haefel and colleagues (2009) did not make it.<sup>1</sup> They claimed that I suffer from a “fundamental misunderstanding about basic research” and that my position is “akin to asking why medical research continues to focus on

growing stem cells when there are more daunting problems such as Alzheimer’s and Parkinson’s disease” (p. 570). If only the connection between psychological research on basic processes and real-world human problems were as clear as the relation between stem cell research and diseases like Alzheimer’s and Parkinson’s! The relation between stem cell research and treatments for Alzheimer’s and Parkinson’s disease is evident even to the nonscientist. The relation between basic research in psychology and real human problems is far less clear even to a research psychologist. There may be value in psychological research on basic processes, especially when the results are linked to cultural contexts, as Haefel et al. suggested. It is just that research on basic processes alone is not enough for a science of humanity. This approach to research leaves out too much about cultural beliefs, cultural practices, and social relations.

Haefel et al. (2009) accurately identified the heart of the difference between my perspective and theirs as a divergence in views of “how to define science . . . and how to evaluate scientific progress” (p. 570). They hold to a philosophy of science they attribute to Popper (1959) and Meehl (1978): “If a theory is falsifiable, it is by definition scientific” (Haefel et al., 2009, p. 570). To some extent, I agree with this view. Certainly testing falsifiable hypotheses is one part of psychological science. However, restricting research to falsifiable theories alone is far too narrow a view of psychology as a human science. A focus on falsifiable theories narrows psychology’s

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<sup>1</sup> Haefel et al. (2009) claimed, “Basic research in psychology has clear implications for real-world issues” (p. 570), but the examples they provided fall flat. Research on information processing and behavioral activation has not “led to the creation of highly effective treatments (e.g., cognitive behavior therapy) for disorders such as depression and anxiety” (p. 570). Cognitive behavior therapy was developed in the 1950s and 1960s by Albert Ellis and Aaron Beck, and its roots are in ancient Greek philosophy, not basic research on information processing and behavioral activation. To find an example of basic research related to any of the problems I suggested that psychology should address (e.g., religious fundamentalism, terrorism, international ecological crises, war), the authors are forced to go back half a century to Milgram’s obedience studies and Zimbardo’s prison experiment. I agree about the value of the Milgram and Zimbardo studies, and I regard it as a great pity that psychological research today is rarely as creative in its methods as those studies were. As for research on “abnormal binocular experience such as esotropia” (p. 570), this seems more in the realm of optometry than psychology.

intellectual and scientific scope mainly to the laboratory, where experimental situations can be carefully controlled. The problem with this focus is that laboratory studies are often ecologically invalid and have little relation to how people actually live and how they experience their lives. There are many aspects of human development, behavior, and experience that are worth investigating even if they cannot be reduced to falsifiable theories (Rogoff, 2003). Psychology needs to get over its "physics envy" and adapt its methods and theoretical approaches to its uniquely human topic, in all its cultural complexity and diversity, rather than endlessly and fruitlessly aping the natural sciences.

### Toward a Broader Philosophy of Our Human Science

The four comments on my article (Arnett, 2008) are diverse, but together they suggest a need for a reexamination of psychology's dominant philosophy of science. Even the two comments that were sympathetic to my thesis did not fully grasp the crux of the problem. Both assumed that a cultural understanding of human psychology could be attained through cross-cultural research, not realizing how transporting American-based theories and methods to other cultures might result in missing the most distinctive and essential features of those cultures. The two opposing comments represented well the traditional approach to psychological research, with its confident assurance that progress in psychology is best served by following the model of the natural sciences, investigating basic processes in search of universal laws, with limited or no attention to that distracting variable, cultural context, that actually means the most to how people behave, how they function psychologically, and how they understand and interpret their lives.

I advocate a broader, more intellectually vibrant and inclusive philosophy of science. The goal of the human sciences should not be simply the pursuit of universal laws and the falsification of theories—no matter how dull or trivial the theory, no matter how little relation the theory has to how people experience life outside the laboratory. The goal of the human sciences should be to use the tools of the scientific method to illuminate our understanding of human behavior, human functioning, and human development. The tools of the scientific method in psychology should be construed broadly to include not just laboratory tasks but any systematic investigation of human phenomena. In this philosophy of science, the structured interview and the ethnography are no less legitimate as tools of the

scientific method than are the laboratory or the questionnaire. Many diverse methods are welcome, and all contribute valuable pieces to the mosaic that makes up a full understanding of humanity.

That mosaic is still missing many large and essential pieces, over a century after psychology was first established as a field. However, many research psychologists are working daily to fill it in, using a wide range of theories and methods (Jensen, in press). What we need now in American psychology is not a narrowing of theories and methods to those that seem best to mimic the methods of the natural sciences, but a wider range of new, creative theories and methods, synthesizing cultural perspectives from all over the world, that will broaden our understanding of the endlessly fascinating human experience.

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DOI: 10.1037/a0016593

## Teaching White Privilege to White Students Can Mean Saying Good-bye to Positive Student Evaluations

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As faculty and instructors working to reduce racism in our students and ourselves, we certainly know about the phenomenon alluded to in the title of this comment. Many of us have discussed it with our colleagues and administrators, but we lacked empirical evidence to support our views. Teaching antiracism can have a negative impact on our careers when students evaluate our teaching efforts and abilities (in fact, teaching antiracism has been called "the kiss of death," Nast, 1999, p. 105). The published literature abounds with anecdotes about negative student reactions to antiracism teaching, particularly when it involves teaching White students about White privilege (McIntosh, 1988). Some scholars have reported that their classroom teaching experiences were negatively impacted, and their professional legitimacy questioned, because they discussed racism