

Is Psychological Science A-Cultural?

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The history of psychological science, as it has intersected with ethnoracial, cultural, and other marginalized domains of group difference, is replete with disinterest, dismissal, or denigration of these diverse forms of psychological experience. This has led some to wonder whether psychological science is *a-cultural*, or even *anti-cultural* in orientation. Assessment of this provocative proposition first requires exploration of three composite questions: (1) What is culture?, (2) What is science?, and (3) What is psychological science? Based on brief consideration of these composite questions—which are remarkably complex in their own right—I argue that psychological science is not, has never been, and indeed cannot in principle be *a-cultural*. Instead, like all forms of knowing, psychological science emerges at particular historical moments to achieve particular goals that are motivated by particular interests. Throughout much of the history of psychological science, these goals and interests were tied to ideologically suspect agendas that contemporary psychologists are right to repudiate. The interesting question becomes whether psychology’s knowledge practices can be disentangled from this earlier ideological contamination to furnish the discipline with viable methods. I propose that psychological science can in fact be so disentangled; nevertheless, the resulting methods are never adopted or deployed outside of culturally constituted interests, objectives, and motivations, thereby requiring ongoing critical engagement with the subtexts of disciplinary knowledge production. In fact, there seem to be important ways in which psychology’s scientific aspirations hobble disciplinary inquiry into the human condition that has motivated multicultural psychologists to consider alternative paradigms of inquiry.

Keywords: scientific methods, philosophy of psychology, multiculturalism, evidence-based practice, American Indians

In this article I address the question, *Is psychological science a-cultural?* My response will comprise four constituent tasks. First, I will motivate the question relative to the presumed interests of multicultural psychology. Second, I will clarify the question by elucidating certain key terms of the question. Third, I will answer the question by tracing the relationships between culture, science, and psychology. Finally, I will expound on the question by considering the benefits and limitations of psychological science for multicultural psychology.

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Motivating the Question

In 2007, I was elected to the board of directors of the First Nations Behavioral Health Association. The Association was created less than 10 years ago with support and encouragement from the Center for Mental Health Services in the Substance Abuse and Mental Health Services Administration (SAMHSA). The Association undertakes contractual work for SAMHSA and helps to informally represent the perspectives of the Association’s constituency. At the time that I first joined the Association’s board, SAMHSA then exhibited a clear preference in awarding some of its services grants to community applicants that had adopted the agency’s officially designated “model” intervention programs for the prevention and treatment of behavioral health problems. These model programs were selected on the basis of favorable outcome evidence from research studies; that is, these were evidence-based interventions that had withstood at least some degree of scientific evaluation in terms of their therapeutic efficacy. Relative to the Association’s interests, the preference for grantee adoption of these evidence-based model programs was controversial in Native American (or “First Nations”) communities. Specifically, less than a handful of the officially designated interventions that SAMHSA desired to fund were actually developed for or evaluated with Native people. As a result, the SAMHSA commitment to privileging evidence-based interventions created a serious dilemma for Native communities. On one hand, the resources available in most of these settings for addressing mental health needs are severely limited; on the other hand, there is widespread suspicion of and

objection to the adoption of “mainstream” behavioral health interventions on the grounds of cultural incommensurability.

Of interest, one of the most commonplace assertions that I have repeatedly come across in tribal communities with reference to evidence-based practice is that locally familiar therapeutic alternatives (e.g., traditional ceremonies) would be better suited for remedying the mental health problems of Native people. This might indeed be true for some on cultural grounds, but what about questions of therapeutic efficacy? Not surprisingly, such advocates are equally fervent on this question, proclaiming, “We already know what works in our communities!” For them, any dedication of already scarce resources to scientific outcome evaluation squanders an additional opportunity to provide even more people with therapeutic benefit. According to this view, restrictive funding priorities, such as those previously adopted by SAMHSA, merely reflect ongoing processes of colonization in need of urgent remedy through political critique, advocacy, and reform.¹ All of this points to the centrality of culture, knowledge, and power in American Indian behavioral health services. More broadly, it speaks to widespread concerns about the “monocultural” flavor of evidence-based practice in psychology and the mental health fields, and it engenders suspicions about the ideology of science as this might pertain to psychosocial intervention (where *ideology* refers to masked or hidden agendas enforced by those in power). Thus, as a concrete instance of science-based government policy pertaining to behavioral health issues, SAMHSA’s designation of model programs affords a compelling window through which to view a much larger set of pressing issues of both philosophical and pragmatic import.

For multicultural psychologists—and many others who share their interests in the psychological aspects of culture, race, ethnicity, gender, sexual orientation, socioeconomic class, and other aspects of group-based human diversity—such science-based policies and practices might lead one to wonder (and to worry): Is there something inherently *a-cultural* (or even *anti-cultural*) about science, especially as it is applied to the study of human mind and behavior?

Clarifying the Question

Prior to addressing this provocative question, I want to ensure that my audience shares my understanding of the question. As a result, I believe it helpful to briefly elucidate three key terms that constitute the question, namely *culture*, *science*, and *psychological science*. Note that I purposefully aim to elucidate rather than to define these terms simply because truly consensual definitions for these remain elusive.

Culture

Regarding key terms, let us commence with what must seem a deceptively simple question: What is culture? Anthropologists have struggled with this question for as long as there has been cultural anthropology. For example, in 1952, Kroeber and Kluckhohn reviewed 164 definitions of culture in their influential monograph devoted to the question. I thus acknowledge that I will not here make appreciable headway in remedying this problem of definition. Nevertheless, for the purposes of this discussion, culture may be understood to be shared, patterned, and historically

reproduced symbolic practices that both facilitate and constrain meaningful human existence (Gone & Kirmayer, 2010). I should emphasize that *practices* are key to this conceptualization of culture, with specific reference to routinized actions (whether by thought, deed, or word) that people undertake in their lives. Culture is *shared* because the practices of interest are learned from others—there can be no culture of one (although it is also true that not everyone shares equally in knowledge of, access to, or facility with such practices). Culture is *patterned* because the practices of interest are organized and utilized systematically in order to be intelligible to others—they are not randomly recreated with each usage. Culture is *historically reproduced* in that subsequent generations are socialized into facility with the practices of their communities—and yet, despite the intergenerational durability of such practices, younger generations might innovate over time allowing for cultural dynamism as well. Finally, cultural practices are *symbolic* in that they allow for the ascription and communication of meaning or intelligibility to others—language is an obvious instance in this regard.

In distilling these qualities to conceptual shorthand, culture may thus be understood as *communal patterns of activity, interaction, and interpretation* (or perhaps even more simply as *shared beliefs and practices*). Classic examples of cultural description from early ethnographies in anthropology include attention to language use, ceremony and ritual, family organization and kinship relations, economic activities, and so forth. What is important to recognize is that many fundamental cultural practices are virtually invisible to the thoroughly enculturated. Like a fish in water who cannot conceive of wetness, that which is cultural can instead seem natural to competent practitioners. And, of course, what seems natural also seems moral, giving rise to ethnocentrism—how could life be any other way, and even if it could, who would desire such a life? As a consequence, it can be difficult for psychologists to recognize the cultural constitution, for example, of human emotional experience, communicative norms, or orientation to time. Given that many psychologists are routinely engaged in prescriptive mental health activities for their livelihood, intimate familiarity with the varieties of cultural experience cannot be optional.

There is one complication I wish to acknowledge before moving on: just as disciplinary psychology was finally embracing culture, some in the academic discipline that studies culture—namely, anthropology—had gone postcultural. For example, in her influential 1991 essay, anthropologist Lila Abu-Lughod concluded that it was time for scholars to “write against culture” in their work. In essence, she argued that use of the term *culture* cannot help but to promote essentialism (or simplistic stereotypes) and to reinforce Orientalism (or exotic “other-ing”). More specifically, she asserted that the identification or description of “a culture” freezes difference by implying discreteness between cultures (instead of diffu-

¹ Incidentally, SAMHSA no longer evaluates its awards in this manner, allowing instead for greater flexibility in what constitutes evidence in support of therapeutic efficacy (e.g., longstanding community recognition). In my view, this shift was brought about precisely through skillful advocacy by a number of constituencies, including the First Nations Behavioral Health Association and its sibling organizations known collectively as the National Alliance of Multi-Ethnic Behavioral Health Associations.

sion & overlap), coherence within cultures (instead of diversity), and timelessness of cultures (instead of change). By way of brief example, I will observe that there are perhaps 4,000 people in the world who share my ethnicity. I do not here mean my race, which even in America's ever-changing classification remains American Indian, but my ethnicity (or tribal identity): we are *A'aninan* or Gros Ventre. When my people talk of Gros Ventre culture, we cannot help but to promote: (a) a marked Gros Ventre distinctiveness from our coresident Assiniboine neighbors or Euro Americans despite over a century of interaction and intermarriage (i.e., an emphasis on discreteness); (b) a general uniformity in Gros Ventre perspective, activity, and outlook despite rather terrific community variations in these according to gender, generation, religious persuasion, education, occupation, and so forth (i.e., an emphasis on coherence); and (c) an implicit reference to our historical 19th-century buffalo-hunting way of life even though we were equestrians of that sort for only 150 years and have not lived like that since the mid-1880s (i.e., an emphasis on timelessness).

Abu-Lughod's point is that each of these attributions trades in oversimplifications that ultimately serve dominant interests (that perhaps view themselves as cultivating civilization while "others" merely retain their cultures). What is the lesson? It is simply that psychologists should adopt the term *culture* with much greater reflexivity and conscientiousness, and (in the absence of "scare" quotes) should even consider restricting use of the word to its form as an adjective rather than as a noun (e.g., cultural processes or practices, rather than cultures).

Science

Returning again to key terms, let us now consider a second question: What is science? One problem in defining science is that it is difficult to generalize from paleontology to particle physics or from astronomy to economics in order to distill any common activities out of the variety of recognized sciences. In fact, philosophers of science have demonstrated that there is no method or set of procedures shared by all sciences or scientists; some have even decried the emphasis on method itself as constraining to scientific progress (Feyerabend, 2010). Nevertheless, for the purposes of this discussion *science* may be understood to entail the precise, observer-independent measurement of phenomena that are progressively or cumulatively employed to evaluate falsifiable theoretical explications of the phenomena. By observer-independent measurement, I simply mean that any competent specialist should be able to reproduce these observations, that such observations are therefore not dependent on which specialist observes them. Scientific inquiry is thus premised on a distinctive synthesis of at least three Western philosophical traditions: rationalism (dependent on logic and argumentation), empiricism (dependent on systematic observation of the world), and skepticism (acknowledgment that personally compelling inferences can be erroneous). The dominant philosophy of science for the first half of the 20th century was logical positivism (or logical empiricism) as developed by the Vienna Circle of philosophers beginning in the 1920s.

Associated with several prominent philosophers such as Moritz Schlick, Rudolf Carnap, and Otto Neurath, logical positivism expressed the view that truth statements about the world were meaningless unless they were specified in extremely precise, empirically verifiable terms. As a result, a central exercise of logical

positivism was to formulate technical statements about the world that would ensure elimination of any "surplus meaning" beyond what might be observed under controlled test conditions. In psychology, the purest expression of these ideals was Skinner's neobehaviorism (Smith, 1997). Although widely influential, logical positivism did not endure as a viable philosophy of science for very long, falling prey to a series of formidable philosophical critiques. For example, Karl Popper (1959) showed that verifiability for scientific statements was ever elusive (owing to the impossibility of observing every existing case) and instead asserted that falsifiability was the better criterion for evaluating scientific explanations. Hilary Putnam (2004) demonstrated that the strict distinctions made in logical positivism between theory and observation are not truly distinct after all, as all "data" require a set of background assumptions about what counts as meaningful observation. Thomas Kuhn (1996) revealed that scientific progress followed less from the gradual accretion of evidence and more from dramatic paradigm shifts that displaced older scientific endeavors with new ones. The result of these and myriad other critiques ensured that logical positivism was recognizably defunct by the 1970s. In its place, most scientists today are scientific realists. That is, in light of the above critiques (and others), realists acknowledge that scientific truths are only approximations of reality, though in clearly established instances they believe that these truths can be taken at face value as legitimate statements about the external world (Leplin, 1984; for realism in the social sciences, see Bunge, 1993).

Once again, I wish to briefly entertain a complication. Over the years, some have asked me whether science is not, after all, merely a consolidation of folk knowledge, a cumulative body of observed regularities about the world passed down from generation to generation in all human societies. One case in point, based in North America, is frequently labeled "Native" or "indigenous" science (Cajete, 2000). At the outset, I must confess that I am frequently unclear whether discussions of indigenous science intend to distinguish between it and what we might refer to as Western science or Enlightenment science that specifically emerged out of 17th century Europe. If there is not a distinction, then why qualify the science in question as "indigenous" as opposed to simply calling it science? If there is a distinction, then it makes sense to chart the parameters of this distinction. From what I can tell, little that resembles the Enlightenment project now called science was to emerge in Native North America. This is not to deny that, in every human community since prehistory, careful observation of the world and advances born of trial and error have profoundly shaped human experience (e.g., in agriculture and animal husbandry; hunting, trapping, and gathering; oceanic navigation; astronomy and calendrics). But what does seem to distinguish Enlightenment science is the technical adoption of systematic methods to understand and explain far-reaching phenomena in terms of abstract theorizations grounded in strictly materialist assumptions for purposes of future prediction and control of the "natural" world.

Note that I have accentuated the importance of abstract theorizations, materialist assumptions, and prediction and control of nature in this characterization of Enlightenment science. With respect to these attributes, I am simply unaware of precontact Native peoples adopting scientific inquiry of this sort. One reason for this may be that Enlightenment science presumes the progressive development of human mastery over the world and proceeds

on the assumption that nature is properly understood and explained in materialist terms (yielding descriptions of the causal laws of nature, and so on). In contrast, at least for the American Indian peoples with which I am familiar, Native societies embraced a relational cosmology based on animism as opposed to a mechanistic cosmology based on materialism. Rather than human mastery over nature, indigenous societies instead pursued harmonious relationships with a variety of nonhuman persons (both seen and unseen) who animate the so-called natural world (Gone, 2008). Thus, even when Native peoples studied the habits of game for the purposes of hunting, they still understood that these animals were persons who had to be respected (ritually and otherwise), lest they take offense and become reluctant to give themselves to humans for food (indeed, among some peoples, hunting was seen as the seduction of game by the hunter, an obvious form of interpersonal interaction [Brightman, 1973]).

And so, science as a form of inquiry and knowledge production appears to be a historical product of the seismic cultural shifts wrought by the Western Enlightenment.

Psychological Science

Building on the preceding, let us come to a final key term and ask: What is psychological science? Elucidation of this term is not nearly as intimidating as consideration of its constituents, but it nevertheless remains more complicated than one might expect. The simple answer is that psychological science is the application of methods originally developed for the natural sciences to the study of what later came to be known as *mind* and *behavior*. In this sense, psychological science represents the realization of Comte's positivism of the mid-1800s (not to be confused with logical positivism proper) that sought to bring human nature, human experience, and human affairs into the realm of natural science inquiry. It is thus important to recognize that, along with the other social sciences, psychology was born of modernity—it both shaped and was shaped by a dramatic societal shift in interest away from older religious notions like soul, virtue, and conduct toward novel naturalistic terms like mind, behavior, and personality. Psychological science has thus drawn on natural science practices such as measurement, quantification, and control in the pursuit of causal laws of behavior (Smith, 1997). As a quick aside on these matters, I invite psychologists to keep our terminology straight. The word *empirical* does not mean experimental or even scientific; rather, it refers to the practice of observing phenomena out in the world. Thus, interpretive inquiry—for example, anthropological-style ethnography—is just as empirical as more disciplinarily familiar variable-analytic inquiry. Moreover, with regard to disciplinary methodology, the quantitative/qualitative dimension is logically independent of the experimental/naturalistic dimension. Thus, it is at least conceivable that experimental results might remain interpretable in the absence of inferential statistics (cf. Asch [1955] and Milgram [1963], though both present counts and percentages [as do many studies that employ qualitative analysis]).

The need to clarify these terms is reflective of another interesting attribute of psychological science. Specifically, more so than most sciences, psychology stands out for its preoccupation with method. This is reflected in Cronbach's (1957) classic article on the two disciplines of scientific psy-

chology, in which the distinction drawn between experimental and correlational research was seen to be more definitive than any specific body of content or collection of findings. Cronbach's distinction reflects two early disciplinary legacies: laboratory studies (e.g., the study of psychophysics, reaction times, perception) and field-based studies (e.g., intelligence testing, child development), though it is useful to note that both traditions have come to depend on statistical analysis of observations. Nevertheless, in contrast to conventional histories of the discipline, psychology has been an incredibly multifaceted field of inquiry from its inception. As Roger Smith (1997) notes in his mammoth *Norton History of the Human Sciences*: "Psychology has no 'origin': its identity in the 20th century is fragmented and its roots are diverse" (p. 493). For example, Wilhelm Wundt, usually credited with creating the first psychology laboratory, was dedicated to several distinct intellectual programs throughout his career, including scientific experimental psychology, humanistic cultural psychology, and the philosophical problem of the nature of mental content (which motivated his experimental work). Wundt never identified as a psychologist and remained a philosopher until his death.

Psychology today appears to be less unified as an intellectual pursuit than ever before, subdivided into numerous "areas" (e.g., clinical, cognitive, developmental) and interfacing with numerous other disciplines (e.g., education, business, medicine). Noteworthy, perhaps, is the curious fact that perhaps the only courses required of nearly all psychologists during their graduate training are statistics and research methods (Benjamin, 2001). I shall revisit this rampant heterogeneity later, but for now I propose that psychological science is the disciplinary project that applies what I have already described as the scientific approach to the production of knowledge about the regularities of human mind and behavior.

Answering the Question

At last I return to the question that this article proposes to address: Is psychological science a-cultural? I fear that my answer to this question must seem rather anticlimactic, in part because I imagine that almost anyone interested in this question already knew the answer prior to attending to these words: No, psychological science is not a-cultural. This is because, as a collective disciplinary endeavor, its origins can be traced from the Western Enlightenment through a complex sequence of historical events that demonstrate its consolidation over time into concrete practices (such as statistical training), concrete products (such as journal articles), and concrete institutions (such as academic departments). Indeed, because no collective human activity can exist outside of its constituting cultural context, it remains impossible in principle for psychological science to be a-cultural. There! We have come to it at last. But I suspect that multicultural psychologists who posit this question also already know that psychological science was not and cannot be a-cultural. Thus, they must have something else in mind when raising this question for shared consideration. It is to this "something else" that I wish to devote the remainder of this article.

Expounding on the Question

The Implicit Problem

My sense is that the posing of this particular question by multicultural psychologists is meant to indirectly reflect some deep misgivings about psychological science as it has unfolded over the past century. For example, most psychologists are readily familiar with the following trends in the history of the discipline. Knowledge about psychology has been produced as if culturally WEIRD people were representative of humanity (where WEIRD refers to samples from western, educated, industrialized, rich, and democratic societies [Henrich, Heine, & Norenzayan, 2010]). Culture, race, ethnicity, gender, sexual orientation, socioeconomic class, and other domains of human diversity have been curiously absent from the psychological record for too much of the discipline's history (Hall & Maramba, 2001; Nezu, 2011). When these differences have been addressed, this has been done in a manner that frequently denigrates or disparages already marginalized populations (Caplan & Nelson, 1973). Moreover, research that has attended to the historical and political disadvantages of group-based psychological difference has been less likely to be valued through publication, funding, or honors. Finally, interpretive or qualitative methods employed to foster understanding of psychological experiences of difference have long been dismissed as inappropriate for disciplinary inquiry (Rennie, Watson, & Monteiro, 2002). Indeed, familiarity with these trends in our field led Guthrie (1998) to lament that, in reference to behaviorist psychology, "even the rat was white."

Perhaps the most egregious instance of disciplinary folly along these lines can be illustrated by select moments in the long history of intelligence testing by psychologists. Smith (1997) has traced the roots of intelligence testing to the innovative work of Francis Galton in the mid-1860s. Galton (1869/2009) brought together an unwavering belief in the dominant influence of heredity over environment with the novel idea that inheritance of individual attributes could be studied through analysis of the distribution of variation within a population. His research purported to demonstrate that upper class elites in England occupied their privileged stations because they in fact possessed the greatest abilities. On the basis of this research, he suggested a concrete application: eugenics. Tracing this history further along, by the turn of the 20th century, Binet and Simon in France were grappling with how to identify "mental subnormals" in state-funded education systems. With further development in the United States by Lewis Terman and Robert Yerkes, it was soon the case that the "new" psychology was assessing the intelligence of 1.75 million draftees during the first World War, although initial pronouncement that the average adult conscript exhibited a mental age of just 13 begged credulity. In short, the establishment of psychology as a distinctive expertise depended on the mental testing movement.

Owing to the hereditarian ethos of the age, the results of intelligence tests during this period were interpreted to mean that men were innately smarter than women, Whites were innately smarter than nonWhites, and American-born Whites were innately smarter than immigrant Whites. It is encouraging to note, however, that there were detractors of these interpretations even in the early days, and that cultural mores subsequently shifted after World War I—owing in part to developments in cultural anthropology—to a

greater emphasis on the environmental determinants of ability (Smith, 1997). Nevertheless, as contemporary psychologists well know, alarming assertions about innate group-based differences in intelligence have resurfaced from time to time, first in 1969 with Jensen's controversial paper on race and IQ in the *Harvard Educational Review*, and yet again in 1994 with the publication of Herrnstein and Murray's *The Bell Curve*. Through the case of intelligence testing (and other historical lessons), it seems indisputable that the specific projects of psychological science emerge at particular historical moments to achieve particular goals as motivated by particular interests. Throughout too much of the history of psychological science, these goals and interests were tied to ideologically suspect agendas that contemporary psychologists are right to repudiate.

All of this has led present-day multicultural psychologists to wonder: Is there something inherent to psychological science that predisposes disciplinary inquiry toward ethnocentric, racist, classist, misogynist, and heteronormative conclusions? To refine the question a bit further: Is the conduct of psychological science intrinsically contaminated by the oppressive ideologies that arose hand-in-hand with the discipline (just as the sciences more generally arose in the context of European empire and Western colonization)? Or to reframe the question somewhat more positively: Can psychology's knowledge practices ever be disentangled from this earlier ideological contamination to furnish the discipline with viable methods? In short: What ultimately is the promise of psychological science?

The Benefits of Psychological Science

Prior to offering a response to these compelling questions, I confess that a more sophisticated and definitive answer might emerge from someone with greater familiarity with philosophy of science than I currently possess. Nevertheless, I assert that psychological science—much like science more generally—can in fact be disentangled from its history of ideological contamination to furnish viable methods to address at least some of the questions that psychologists aspire to answer. But how could science—and by extension, psychological science—escape its own contaminated history? Earlier I emphasized the discontinuity of science with folk knowledge, but for this discussion I will accentuate a certain kind of continuity with such knowledge. Specifically, I propose that scientific knowing represents a (nontrivial, original, distinctive, and modernist) elaboration of universal epistemological processes. As I have already allowed, it is difficult to conceive of any human society that did without some form of empiricism (by which I do not mean the Western tradition of philosophical empiricism, but rather commonplace observations of the world used to draw conclusions about predictable regularities, such as trial-and-error learning). In this regard, scientific knowing represents an extension of these fundamental processes to problems of greater complexity and nuance that enables humans to better overcome the innate limitations of unaided cognition.

In essence, then, I characterize the main contribution of scientific knowing as the extension of rationality into domains otherwise beyond the limits of human reason (limits that psychology itself has productively charted [Kahneman, Slovic, & Tversky, 1982]). Thus, scientific knowing may be taken up as a cognitive prosthesis of sorts. That is, as a collection of inferential practices

for deriving certain forms of knowledge, facility with scientific knowing remains just another implement in the conceptual toolbox. By providing humans with a set of inferential tools (as opposed to a totalizing ideology), science has proven its utility and harbors great potential to contribute to the common good (as indeed it has in so many domains of life). Of importance, it is not that the epistemological traditions that have come to be designated as science have somehow transcended culture to become a-cultural; rather, it is that their evident benefits have led to widespread adoption across multiple cultural contexts. In this sense, science itself has become multicultural. It should come as no surprise, then, that psychologists can imagine plentiful instances in which it would appear helpful to reach beyond the capacity of unaided cognition to explore particular psychological phenomena. In fact, it seems perfectly plausible that even the aspiration for prediction and control of such phenomena might be productively harnessed to a range of agendas, including the progressive and emancipatory projects promoted by multicultural psychologists.

And so, to return to the issue of American Indian behavioral health that I introduced earlier, two points seem warranted. First, as a proponent of psychological science I confess to being skeptical of routine claims tendered by Native behavioral health advocates that “we already know what works in our community,” particularly when this assertion is offered as an argument for dismissing empirically supported interventions (e.g., motivational interviewing for treating substance abuse problems) in favor of culturally local interventions that have never been systematically evaluated (e.g., sweat lodge ceremonies for treating substance abuse problems). My reasons for this skepticism are twofold. For one, I am convinced on the basis of scientific evidence that reliable causal inferences are notoriously difficult to draw “off the top of our heads” in the realm of psychosocial intervention (for an elaboration in the context of Native mental health interventions, see Gone & Alcántara, 2007). For another, I recognize that routine claims of this sort permeate everyday life more generally, and in the absence of systematic evaluation it is not always possible to determine which are accurate and which are erroneous (Dawes, 2002). Note, however, that my position is not the same as denying that sweat lodge participation is effective for ameliorating American Indian substance abuse. In fact, I would not be at all surprised if the sweat lodge proved to be efficacious in treating substance abuse problems for many Native people. Moreover, I can posit a rationale (or causal model) for why the sweat lodge might in fact be therapeutic in this way. Furthermore, I believe it desirable to demonstrate, if possible, that the sweat lodge is an efficacious intervention for Native substance abuse. But, for now, in the absence of any systematic outcome evidence, I do not know (and cannot accept on faith) that such claims warrant professional acceptance and endorsement.

Second, despite my skepticism toward such claims, as a proponent of psychological science I accept that it is possible in principle to evaluate many such claims by using a range of scientific methods to discriminate between competing causal explanations. That is, I believe that the experimental method employed by psychological scientists—characterized by random assignment of participants to treatment and control groups with an eye toward assessing differential outcomes—is the best means that humanity possesses to afford reliable causal inference under conditions akin to typical instances of psychosocial intervention. What is crucial to

note is that my faith in this method does not constrain the kinds of interventions that I might in principle evaluate in this way (whether motivational interviewing or sweat lodge ceremonies; for a fledgling evaluation of the therapeutic benefits of the sweat lodge, see Gossage et al., 2003). Because I think there are persuasive reasons why many American Indians reject mainstream behavioral health interventions, I support the provisional exploration of culturally local alternatives for managing debilitating distress even (or especially) when such practices contrast markedly with psychotherapy (Gone, 2010). It is crucial to add, though, that I aspire to actually evaluate specific indigenous cultural practices in terms of their therapeutic benefits for Native people. My point is that there is nothing intrinsic to psychological science that restricts the pursuit of “pro-Native” evaluation projects of this kind.²

In sum, psychological science equips the discipline with methodological tools that can be applied to an incredibly wide range of questions in a wide range of contexts. None of this is to naively claim, however, the psychological science is the royal road to truth. In fact, the limitations of psychological science can be quite formidable, and in my opinion warrant a remarkable degree of humility. Thus, it is to the limitations of psychological science that I will briefly turn in the final section of this article.

The Limitations of Psychological Science

The preceding must make me out to be not just scientific, but rather scientific. And yet, those who are familiar with my work know that this is far from true. In fact, there are numerous reasons for maintaining a critical vigilance when it comes to the claims of psychological science. Most of these stem from the fact that psychological science—like science more generally—works better in theory than in practice, such that scientific findings have to be assessed in light of the actual routines of scientific conduct. Regrettably, there is only space to briefly canvass four such limitations. The first is straightforward for multicultural psychologists: as I have already reviewed, psychological science is never adopted or deployed outside of culturally constituted interests, objectives, and motivations, thereby requiring ongoing critical engagement with the overt and covert agendas served by its findings. When it comes to matters of group-based psychological differences, multicultural psychology remains at the forefront of such critical engagement.

A second limitation of psychological science stems from the role that investigator bias plays in the production of scientific knowledge. More specifically, the biases inherent to scientific practice favor the finding and publishing of positive (and especially groundbreaking) results that in a surprising number of cases can be shown to be erroneous. Consider the currently celebrated work of John Ioannidis, mainly with reference to the health sciences (but with clear implications for the social sciences, including psychology). Ioannidis has published a series of very high-impact analyses during the past several years that have called into ques-

² I have deliberately qualified these statements about the applicability of scientific evaluation to Native claims with the term *in principle* because there remain many under-explored practical limitations to such evaluations in *actual application*. These range from conceptual difficulties in specifying local claims to design challenges with respect to random assignment and sample sizes that collectively render these efforts challenging indeed.

tion even the most rigorous research findings in medicine. In an important conceptual paper, he reviewed six corollaries in support of his contention that “research findings may often be simply accurate measures of the prevailing bias” (Ioannidis, 2005b, p. 0699). For example, in his consideration of positive predictive value (or the probability of a false positive report), Ioannidis deduced that small sample sizes, small effect sizes, lower selectivity of tested relationships, and greater flexibility in research designs all lower the probability that affirmative research findings are in fact true. Beyond this conceptual exercise, Ioannidis (2005a) also examined highly cited positive health intervention outcomes—mostly obtained from randomized clinical trials—that were reported in high-impact medical journals between 1990 and 2003 and that had been cited at least one thousand times each. He found that, even for these highly influential and scientifically rigorous health intervention studies, subsequent attenuation or even refutation of results was not uncommon. Specifically, for 45 index studies, only 44% of reported results held firm on subsequent replication, while 16% were subsequently contradicted and 16% evidenced reduced effects on replication (24% had not been replicated at all by the time of his analysis). Lest the implications of these analyses seem ambiguous, Ioannidis (2005b) firmly asserted that “it can be proven that most claimed research findings are false” (p. 0696).

A third limitation of psychological science is that—like all sciences—the discipline depends on faith in its methods for confidence in its findings. Once findings extend too far beyond preconceived notions of truth, however, psychologists—again, like scientists more generally—will dismiss such findings on the presumption of faulty methods and often declare such work as beyond the bounds of disciplinary inquiry. For just this reason, Roger Smith (1997) in his *History of the Human Sciences* devoted a section to the emergence of parapsychology as a helpful means for illuminating the emergence of psychology proper. Obviously, parapsychology’s historical bid for legitimacy as a social science has failed, but there remain intriguing eruptions of parapsychological research into the psychological record from time to time. For example, I well remember from my graduate student years an exchange in the revered *Psychological Bulletin* about the existence of psi (or ESP). Specifically, Bem and Honorton (1994) reviewed 11 studies that employed the rigorous autoganzfeld experimental technique involving sequestered participants in which one attempts to communicate with the other by telepathy. The overall results of these studies suggested a small but significant effect in support of such anomalous communication (with artistically gifted participants faring better on such mind-reading tasks).

In early 2011, there was national attention to a current example in which Bem (in press) summarized positive results from eight experimental studies resulting in support for the existence of psi-like processes such as precognition and premonition—this article is currently forthcoming in the prestigious *Journal of Personality and Social Psychology*. Of course, almost no psychological scientists will accept these findings as evidence of the actual existence of ESP. They will instead observe that such findings regularly seem to fall off on further attempts at replication and argue that the implausibility of ESP as an explanation for these results requires various refinements in methodology. The problem, of course, is that research findings in parapsychology are not the only kind to “fall off” over time (Lehrer, 2010), and—in the

absence of considerable replication—motivated psychologists can always find methodological grounds for disputing or dismissing research findings. Such instances help to illustrate a certain insularity in the orthodoxies of psychological science and the potential for circularity in scientific reasoning between expectations, results, and interpretations. As Smith (1997) observed, “There is irony here since many of ESP’s critics who were experimental psychologists . . . had themselves tried to claim authority for their subject as science by the development of rigorous methods rather than explanatory intelligibility” (p. 679).

A final limitation of psychological science remains a function of the positivist endeavor. Throughout the history of psychology, there has been ongoing debate about whether human mind and behavior are truly or properly knowable by methods borrowed from the natural sciences. The critique is obviously not that psychologists are unable to apply natural science methods to the study of psychological phenomena, but rather that doing so tends to lead to sterile results or impoverished knowledge (e.g., when psychology’s theories are regularly mismatched to its rules of evidence; Danziger, 1985). These critics press the field to identify any truly noteworthy causal laws of behavior discovered by psychologists during the past century. Of course, in the wake of the demise of logical positivism, enthusiasm for the pursuit of the causal laws of behavior has declined, even as ascendant forms of sociocultural contextualism have attained greater influence in the discipline than ever before (including for experimental research programs; see Kitayama & Cohen, 2010). Nevertheless, psychology remains a field that is heavily dominated—and, some would argue, even defined—by scientific inquiry. And yet, one alternative to Comtean positivism is interpretivism (Richardson & Fowers, 1998). In this regard, a time-honored way of contrasting inquiry in psychology construed as a natural science with psychology construed as a human science is to distinguish the difference in goals between them as *explanation* versus *understanding*. By revisiting the incredible diversity that has characterized psychology since its emergence at the end of the 19th century, it is easy to see that both projects have been present throughout its history (Kimble, 1984; Smith, 1997).

Despite this evidence for the longstanding existence of both kinds of projects, however, it remains indisputable that the natural science quest for psychological explanation based on measurement, control, and quantification has readily dominated the human science quest for psychological understanding based on description, meaning, and interpretation. And yet, inquiry advancing this “second psychology” devoted to the study of subjectivity in context continues to find disciplinary purchase (Bruner, 1990; Cahan & White, 1992; Gergen, 1985; Packer, 1985). For example, although many psychologists seek explanations for the causal origins of distinctive forms of psychopathology using scientific methods, a few also pursue (empirical) interpretive understandings of broader cultural meanings that such conditions instantiate in people’s lives during particular historical moments (Baldwin, Williams, & Houts, 2004; Choudhury & Kirmayer, 2009; Gone & Kirmayer, 2010). Such studies can even illuminate pressing questions surrounding the meanings of psychosocial wellness, clinical intervention, and evidence-based practice (Gone, 2009). In sum, the methodological constraints of psychological science have motivated enthusiasm among an increasing number of psychologists for various

alternative paradigms of inquiry. Such alternatives include (among others): phenomenological psychology, hermeneutic psychology, discursive psychology, dialogical psychology, constructionist psychology, and indeed, multicultural psychology. It is interesting that several of these have recently been classed under the label *constitutive sociocultural psychology* (in opposition to the old cross-cultural psychology and the current experimental cultural psychology; Kirschner & Martin, 2010).

Owing to these (and other) limitations, there remains ample justification for undertaking psychological inquiry beyond the limited bounds of measurement, control, and quantification, even while retaining (qualified) faith in the methods of psychological science to address certain kinds of pressing questions. As a result, alongside the evident advantages of scientific inquiry for extending human rationality, I believe that psychology would also benefit from greater acceptance of and support for a robust methodological pluralism. Instead of concern that multicultural psychology may have strayed too far from science, I express a desire for brilliant and rigorous scholarship addressed to a wide variety of research questions espoused by multicultural psychology regardless of the kind of empirical methods used.

Conclusion

In this article, I have endeavored to: complicate the understanding of terms such as culture, science, and psychological science (e.g., science cannot be accurately characterized as a unitary concept with defining methods or common procedures); argue that psychological science is not and cannot in principle become a-cultural (e.g., there is no meaningful human activity that occurs outside of “culture”); offer a rationale for the relevance of scientific inquiry for multicultural psychology (e.g., controlled experiments may be the best epistemic tools humans possess to infer certain kinds of causal relationships); and identify key limitations of scientific inquiry that support alternative approaches for multicultural psychology (e.g., the value of attending to meaning and subjectivity in the investigation of psychological phenomena). It is my hope that this effort to appraise the relationship between culture and science within the discipline emboldens all psychologists—including multicultural psychologists—to undertake methodologically diverse research on group-based psychological differences that will result in rigorous knowledge production and enhanced human flourishing.

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