

Stigma from Psychological Science: Group Differences, Not Deficits—Introduction to Stigma Special Section

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Famed composer Judith Henderson observed that “our greatest strength as a human race is our ability to acknowledge our differences; our greatest weakness is our failure to embrace them.” As psychological scientists, we are experts at empirically identifying a vast array of distinctions among humans. But as psychological scientists, we can also fall prey to drawing invidious comparisons—contrasts and conclusions between groups of people that are made with either the explicit or unconscious goal of showing one group in a negative light (Cole & Stewart, 2001). Differences get framed as deficits, and such negativity enables stigma (Amundson, 2000).

During my term as president of the Association for Psychological Science, I used the opportunity to call attention to this risk. In a column titled “On Not Being Human,” I traced the historical practice by philosophers and theologians to deem some groups of humans as subhuman and the unfortunate contemporary practice by some psychological scientists to do the same (Gernsbacher, 2007c). In a column titled “The Eye of the Beholder,” I illustrated how a neurological marker—the relative thickness of one’s cortex, or thinness, if you prefer—has been deemed an asset or a deficit depending on which group of participants has it (Gernsbacher, 2007a).

In a column titled “Neural Diversity,” I illustrated how interpretations of brain imaging data are often susceptible to researchers’ bias (Gernsbacher, 2007b). For instance, activation in the amygdala is interpreted as reflecting the intense attachment, vigilant protectiveness, and empathy that characterize normal maternal attachment when the research participants are mothers looking at photos of their children (Leibluft, Gobbini, Harrison, & Haxby, 2004), whereas activation in the same region is interpreted as identifying the regions involved in sexual/aggressive behavior when the research participants are boyfriends listening to sentences such as “my girlfriend gave a gorgeous birthday present to her ex-boyfriend” (Takahashi et al., 2006).

In another column, titled “How to Spot Bias in Research,” I recommended a solution to such interpretative biases: Swap the participant groups’ labels (e.g., mothers vs. non-mothers or boyfriends vs. non-boyfriends) to see whether the resulting interpretation makes sense; if it doesn’t, the interpretation was most likely predisposed.

For my Presidential Symposium, I gathered four of our field’s top scholars to continue to address the problem of stigma from psychological science and to provide further solutions. Susan Fiske of Princeton University addressed the topic with regard to gender differences; Douglas Medin of Northwestern University reviewed cross-cultural research; James Jones of the University of Delaware reflected on race differences; and Gregory Herek of the University of California, Davis, discussed sexualities. I am delighted that each of these scholars agreed to document their excellent presentations in the special section that follows.

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Venus and Mars or Down to Earth: Stereotypes and Realities of Gender Differences

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Abstract

Psychological scientists, like lay people, often think in categorical dichotomies that contrast men and women and exaggerate the differences between groups. These value-laden divides tend to privilege one side over the other, often to the advantage of the scientists' own identity group. Besides balancing perspectives in the academic marketplace of ideas, scientists can recognize the complexity of stigma. Gender, like many categories, entails two fundamental dimensions that characterize intergroup stigma (and all interpersonal perception): perceived warmth and competence. These dimensions identify groups viewed with ambivalence (e.g., traditional women are stereotypically warm but incompetent, whereas professional women are allegedly competent but cold). In gender and in other areas, psychological scientists can go beyond value-laden dichotomies and consider the fundamental, continuous dimensions along which we think about stigma.

Keywords

gender, stereotypes, stigma, ambivalence, warmth, competence

I want to begin with a gender-related story about how I got here, and by “here” I mean contributing to this special issue and, more profoundly, entering the field of psychology. My father was a psychological scientist and that certainly had a huge impact on my entering the field. However, I also come from a long line of women, starting with my great grandmother, who have worked on the topic of gender. My great grandmother was a suffragist, and then my grandmother was also a suffragist, and her husband (my grandfather) was a women's suffrage advocate, and my mother worked in the League of Women Voters for most of her adult life. So, I come from a long line of people who have thought a bit about gender.

In college, when I took psychology courses, I kept noticing that, in the Personality courses in particular, all the individual difference scales had a good end and a bad end. And anyone who read the scale could almost always tell which end belonged to the person who had made up the scale. At that time, most of the people making up the scales were men. I kept reading these scales and thinking, but wait, what about the other side? Is it not okay to be that, too? Is it always a deficit?

Therefore, I realized that someone had to enter the field and point out the other perspective, and that was one of my major motivations for going into psychological science. Although I did not end up focusing that much on gender, I do have some

relevant thoughts to offer in this regard. First, I want to encourage us to think about how we frame gender differences; next, I consider some pros and cons of studying gender differences the way we currently do; and finally, I address how we can use two fundamental, continuous dimensions to think about gender.

How We Frame Gender Differences

Psychologists love dichotomies. They love to slice and dice a broader population into two categories. For example, a very popular dichotomy applied in the 1970s was termed “field dependence” (Witkins, Moore, Goodenough, & Cox, 1977). Field independent people were believed to be more autonomous and more able to cognitively restructure the tasks before them. Field dependent people were unduly influenced by the immediate situation. My eureka moment occurred when I learned about field dependence, which is the deficit women were believed to have. However, men were believed to be field independent, which was assumed to be clearly a good thing.

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It struck me that women might be viewed as field sensitive and men as field insensitive. Suddenly, the gender differences had new meaning.

Consider some other gender dichotomies: Men are perceptually thorough, mathematically self-confident, linguistically specialized, tough-minded, physically and directly assertive, and justly moral. Perhaps these are recognizable as gender differences that are often framed using different words. Seen from the other side, how about women being perceptually fast? (Does that mean men are perceptually slow?) What about women being cautious about math instead of lacking self-confidence? What about women being generally linguistically skilled (and men being linguistically overspecialized)? Are women often more agreeable and tender minded whereas men are more tough-minded? What about women being subtly, socially assertive, if they are not directly assertive? If the word *aggression* is substituted for *assertive*—men are physically aggressive and women are socially aggressive—then neither gender comes out looking so great.

All of these distinctions are actual gender differences that fall out of a collection of meta-analyses assembled by Janet Hyde (2005). It is important to put these significant distinctions in context because they form a distinct minority of all the differences examined. Of the many gender difference studies Hyde and her colleagues could find (and these are just the published ones), 78% of the studies showed very small to at the most medium effect sizes (i.e., in the .00–.35 range for Cohen's *d*). Thus, almost 80% of published gender differences are not big; the median is around .21. Moreover, in virtually all of these studies, the distribution of males and the distribution of females mostly overlap. Given this reality, why do we make decisions about people based on distributions that we assume are nonoverlapping and separate? We do so because we love dichotomies.

Some Pros and Cons of How We Study Gender Differences

If people like to talk in these kinds of dichotomies, where do the problems arise? Quite clearly, people's values and identities matter when they do this kind of science. As scientific labels illustrate, is it field dependence or field sensitivity? Researchers' own interests and proclivities determine the issues that researchers consider interesting, their methods, their analyses, and even the publications that researchers pursue. Reported findings follow from researchers' own perspectives. This is not to say that researchers are politically biased and, therefore, that their science is suspect. This is just to say that researchers, like everyone else, naturally pursue what they find interesting, and what they find interesting is informed by their values and their identities.

Because of this perfectly natural tendency, I argue that we need a variety of perspectives; we need diversity among scientists. As the people in the disability movement say, "Nothing about us, without us." Of course, there is a marketplace of ideas, and any researcher has to be up to the scientific standards

to get published. As we all know, peer review is hardly pro forma. But within these agreed upon standards, psychological science needs a variety of perspectives, because people so spontaneously look at things from their own point of view.

What is the downside of emphasizing gender or other group differences? People tend to jump from a continuum to categories. They maximize the differences between categories and minimize the differences within categories. Descriptive differences can become self-fulfilling prophecies. Or they can become a case of stereotype threat in which people worry about fulfilling the stereotype with which they have been labeled. Prescriptive differences go farther: People are enjoined to follow these differences whether they fit the distinction or not. And if they do not, there is well-documented backlash and constraints on the behavior of people to fit into these dichotomous categories (for one review, see Fiske, 1998).

What is more, people are just not "either/or." Much of my own work has focused on the ambivalence of so many of these perceptions. Given this focus on, in the case of gender, male agency and female communion, Peter Glick and I developed a theory about the different kinds of stigma that can result—the most obvious being hostility toward agentic women. But there is also a subjectively benevolent, paternalistic kind of sexism toward women who are more communal (Glick & Fiske, 1996). There is also a comparable phenomenon directed at men: There is some hostility and resentment toward male agentic dominance and some benevolence toward men's weaknesses (Glick & Fiske, 1999).

Two Fundamental, Continuous Dimensions of Gender and Stigma

The point is that these are fundamentally ambivalent forms of stigma. Often, people do not also consider the fact that even positive attributes can be damaging when they are assigned to a whole group and expected about that group. For example, by saying women are so communal and regarding it as a great virtue, there are expectations that these attributes apply to all women, which is not true. Moreover, this ambivalence turns out to be more general than just gender relations.

The communal-agency dimensions turn out to be fundamental, going across categories well beyond gender (Fiske, Cuddy, & Glick, 2007). When people encounter another person or group, the first questions people ask are "Are they on my side or not? Are they friend or foe?" And if they are a friend, then they are warm, trustworthy, and sincere. But if they are a foe, they are cold, untrustworthy, and callous. The second fundamental dimension is whether the other(s) are able to enact those intentions—if they can, then they are competent, able, skillful, and capable. These two dimensions account for as much as 85% of the variance in interpersonal impressions and intergroup impressions (see Box 1a for examples, and Fiske et al., 2007, for a review). This is an incredibly powerful principle. It is not ours alone, but we have been working on it in the intergroup relations area.

Box 1. For ease of presentation, these 2×2 tables reduce continuous variables represented by cluster analyses in a two-dimensional space.

a. Fundamental Dimensions With Illustrative Social Groups (e.g., Fiske et al., 2007)

	Low competence	High competence
High warmth	Older people, people with disabilities (mental or physical)	Middle-class people, Americans, heterosexuals
Low warmth	Homeless people, addicts, welfare recipients	Rich people, professionals, entrepreneurs

b. Fundamental Dimensions With Female Subtypes (Eckes, 2002, Study 1)

	Low competence	High competence
High warmth	Typical women, housewives, secretaries, "wallflowers"	Confident types, society ladies
Low warmth	Bourgeois, "chicks"	Feminists, career women, intellectual women

c. Fundamental Dimensions With Male Subtypes (Eckes, 2002, Study 2)

	Low competence	High competence
High warmth	Senior citizens, radicals, hippies, softies	Professors, intellectuals, confident types
Low warmth	Cads, punks, bums, rockers	Career men, managers, yuppies, typical men

d. Fundamental Dimensions With Gay Subtypes (Clausell & Fiske, 2005)

	Low competence	High competence
High warmth	Flamboyant, feminine	Artistic, in-the-closet
Low warmth	Leather/biker, cross-dressers	Hypermasculine, activists

e. Fundamental Dimensions With Mental Illness Subtypes (Moore & Fiske, 2005)

	Low competence	High competence
High warmth	Mental retardation, Down's syndrome	Attention-deficit disorder, eating disorder
Low warmth	Schizophrenia, psychopathology	Depression, bipolar disorder, obsessive-compulsive disorder

f. Fundamental Dimensions With Black Subtypes (Fiske, Bergsieker, Russell, & Williams, 2009)

	Low competence	High competence
High warmth	Disabled Blacks, Poor Blacks	Christian Blacks, Elderly Blacks, Black mothers, Middle-class Blacks
Low warmth	Uneducated Blacks, "Niggas"	Educated Blacks, Rich Blacks

g. Fundamental Dimensions With Immigrant Subtypes (Lee & Fiske, 2006)

	Low competence	High competence
High warmth	Italian immigrants, Irish immigrants	Canadian immigrants, European immigrants, documented immigrants, third-generation immigrants
Low warmth	Undocumented immigrants, Mexican immigrants, African immigrants	Japanese immigrants, Chinese immigrants, Korean immigrants

Our model captures some crucial aspects of stigma, both scientists' stigma and also lay people's. Our warmth-by-competence space does represent the most obvious diagonal, the one most often meant by stigma. Stigma usually contrasts pride in the ingroup ("we are the best") and contempt toward

the outgroup, which in this case would be low on both warmth and competence (see Box 1a). What our model adds to this standard dichotomy are the ambivalent clusters. For one, people who are stereotypically high warmth but low competence typically include older people and people with disabilities; they

all elicit pity. In contrast, people perceived to be highly competent but not so warm in the United States at this time stereotypically include people who are rich, entrepreneurs, or professionals. The data replicate across student and adult samples, across the country, and in other countries as well (Cuddy, Fiske, & Glick, 2007; Cuddy et al., 2009).

Focusing on the gender aspect of it, Thomas Eckes (2002) took this analysis of stigma one step further by looking at different kinds of female subgroups (see Box 1b). Translated from German, typical women, housewives, secretaries, and “wallflowers” fall into what might be considered “traditional” female roles; they are stereotypically warm but incompetent. In contrast to this are the feminists, lesbians, female athletes, and female professionals; they are stereotypically competent but cold. For subgroups of women, these two dimensions differentiate them in important ways. Eckes also examined different subgroups of men. Notably, the subgroups of men are more often clustered around the kinds of jobs they have (see Box 1c).

We have also found that these two fundamental dimensions appear in subgroups of gay men (Clausell & Fiske, 2005; see Box 1d). Paralleling the male and female gender subgroups, the feminine gay men are allegedly warm, but not so competent and the hypermasculine gay men are allegedly competent but not very warm. And it is apparently not good to be a leather biker—they are stereotyped as neither smart nor nice. Finally, almost no one appears to be highly warm and highly competent part of the space except perhaps artistic gay men or straight-acting ones; so, it suggests that gay men are just not seen as a likely warm and competent ingroup.

Overall, dimensions provide a more nuanced view of stigma. Groups can be stigmatized even if viewed positively on one dimension and by implication negatively on the other dimensions. Consider people with various kinds of mental illness or mental disability (Fiske, Moore, & Russell, 2006; see Box 1e). People who are viewed as mentally disabled are allegedly incompetent but nice. People with schizophrenia are viewed as neither competent nor nice. And people with bipolar disorders, as in the stereotypic mad genius, are seen as highly competent but not nice.

As for Black people, our research indicates that contemporary Americans split African Americans by social class, with poor Blacks (and poor Whites) allegedly being neither competent nor nice, but Black professionals being like other professionals: stereotypically competent but cold (Cuddy et al., 2007). Black Americans subtype themselves as well (Fiske, Bergsieker, Russell, & Williams, 2009; see Box 1f). We have also analyzed distinct subgroups of immigrants by ethnicity; some are seen as competent but not warm (e.g., Asians), some are seen as incompetent but warm (e.g., Irish), and some are seen as neither (e.g., Latinos and Africans; Lee & Fiske, 2006; see Box 1g).

These stigma dimensions matter because emotional prejudices come quickly on their tail—pity, envy, and disgust—and the discrimination directed at these different groups is quite predictable from the emotions. For example,

consider the difference between being attacked and being neglected. Both kinds of discrimination occur, but the two kinds of discrimination are often aimed toward different kinds of groups (Cuddy et al., 2007). What is more, the stigma dimensions often will force trade-offs; so an outgroup can be smart but cold or nice but stupid. These ambivalent combinations are not beneficial. And some stigmas obviously are worse than ambivalent: There are groups who are seen as having no redeeming features; some data, including neuroimaging data, indicate that such people are basically dehumanized (Harris & Fiske, 2006, 2009).

Conclusion

Stigmas differ. Scientists must go beyond viewing stigma as simply “I hate them, and I love us.” Nor is it just that “my end of the scale is good, and your end of the scale is bad.” For gender, in particular and for many other groups, the similarities are often greater than the differences between the groups. The differences divide us and oversimplify a complex, textured reality. When there is stigma, it is likely to be on multiple dimensions with ambivalent and insidious results.

Declaration of Conflicting Interests

The author declared that she had no conflicts of interest with respect to her authorship or the publication of this article.

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Sexual Orientation Differences as Deficits: Science and Stigma in the History of American Psychology

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Abstract

This article briefly describes how psychology, psychiatry, and the mental health professions (here collectively referred to as *Psychology*) treated sexual orientation differences as deficits for much of the 20th century, as well as some of the negative consequences that practice had for sexual minorities. The 1970s witnessed a remarkable turnaround when the American Psychiatric Association removed homosexuality from the *Diagnostic and Statistical Manual of Mental Disorders* and the American Psychological Association called for psychologists to work to remove the stigma historically associated with homosexuality. This history illustrates not only how cultural institutions play a central role in legitimating stigma, but also how they can recognize their own complicity in this process and work effectively to undo its harmful effects. It is argued that Psychology still has an important role to play in challenging the differences-as-deficits model in contemporary policy debates.

Keywords

history of psychology, stigma and prejudice, sexual orientation, lesbian, gay, and bisexual people, homosexuality, heterosexuality

During much of the 20th century, the sexual minority experience in the United States was significantly defined by homosexuality's official designation as a mental illness. Nonheterosexuals suffered considerable harm because Psychology¹ equated departures from heterosexuality with psychological deficits. The 1970s, however, witnessed a remarkable turnaround in the mental health professions and behavioral sciences that continues to have important ramifications for sexual minorities today. The history of Psychology's stance toward homosexuality and sexual minorities illustrates not only how cultural institutions play a central role in legitimating stigma, but also how such institutions can recognize their mistakes, reverse their policies, and become agents for societal change. The present article reviews that history as a case study in the making and unmaking of structural sexual stigma.

Stigma refers to the culturally shared knowledge that society regards the members of a particular group or category negatively and accords them inferior status in their social interactions with the nonstigmatized. It is "an undesired differentness" (Goffman, 1963, p. 5) whose significance and status are socially constructed and can change over time as norms and mores change. *Sexual stigma* is the stigma attached to any nonheterosexual behavior, identity, relationship, or community (Herek, 2007, 2009). Like other forms of stigma, it is fundamentally about power. Stigma-based differentials in power and status

are legitimated and perpetuated by society's institutions and ideological systems in the form of *structural stigma* (e.g., Link & Phelan, 2001), which "is formed by sociopolitical forces and represents the policies of private and governmental institutions that restrict the opportunities of stigmatized groups" (Corrigan et al., 2005, p. 557).

Structural sexual stigma is also referred to as *heterosexism*. As a core component of society's institutions, heterosexism ensures that nonheterosexuals have less power than heterosexuals by promoting a *heterosexual assumption*: All people are presumed to be heterosexual, and heterosexual behavior and different-sex relationships are considered normal, natural, and unproblematic (Herek, 2009). This assumption makes gay, lesbian, and bisexual people invisible in most social situations. When sexual differences become visible, homosexuality and bisexuality are problematized—that is, they are regarded as abnormal and unnatural forms of deviance that require explanation. Differences observed between sexual orientation groups are generally interpreted as indicating deficits or problems on

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the part of nonheterosexuals. In this manner, heterosexism defines sexual differences in terms that bestow greater power upon heterosexuals. By legitimating and reinforcing nonheterosexuals' undesired differentness and by according them inferior status relative to heterosexuals, heterosexism gives rise to actions against sexual minorities, including ostracism, harassment, discrimination, and violence (Herek, 2009). As summarized below, Psychology played a key role in justifying and perpetuating heterosexism in the years prior to 1973.

Sexual Orientation and Mental Health in Historical Perspective

In historical terms, sexual orientation is a fairly recent construct. Although heterosexual and homosexual desires and sexual behaviors are, and have been, ubiquitous in human societies, the meanings attached to those behaviors and attractions have varied across cultures and historical eras. It was not until 1868, for example, that the word *Homosexualität* (homosexuality) was first introduced by the Hungarian writer Karl Maria Benkert in a German-language pamphlet (Feraý & Herzer, 1990). Heterosexuality (*Heterosexualität*) came even later (Katz, 1995). Before this time, attractions, behaviors, and relationships that are now characterized as *heterosexual* or *homosexual*—and, indeed, the very concept of sexuality—were understood quite differently from today.

Early in the 19th century, marriage was regarded mainly as an institution for securing wealth and property rights rather than a companionate relationship based on emotional intimacy and romantic love. Procreative acts were authorized by heterosexual marriage, whereas nonprocreative or improperly procreative acts were considered animalistic and condemned as sodomy by religious teachings and legal statutes. The construct of sodomy encompassed not only homosexual behaviors, but also masturbation, sex with animals, pre- and extramarital heterosexual behaviors, and even sexual acts between a husband and wife that did not involve vaginal intercourse; it did not include love. Love and sexual desire were widely regarded as polar opposites, reflecting a dichotomy between the soul and the body, the spirit and the flesh. Before Freud, “[n]ineteenth-century ideologists of eros imagined a crack in the world, with love on one side, lust on the other” (Katz, 2001, p. 333). In such a world, modern notions of “the homosexual” and “the heterosexual” do not readily apply (see Chauncey, 2004; Coontz, 2005; D’Emilio & Freedman, 1988; Katz, 2001).

The belief that individuals can be meaningfully defined by their sexual attractions and behaviors began to gain widespread currency only in the latter 19th century and achieved dominance in psychiatric discourse in the early 1900s with Freud’s conceptualization of homosexuality and heterosexuality in terms of object choice (Freud, 1905/1953; see also Chauncey, 1982–1983). Love and sex came to be viewed as intimately related, and heterosexuality was understood by psychiatrists to be their mature, healthy expression. Freud did not consider homosexuality to be the optimal outcome of psychosexual development but neither did he believe it was a mental illness.

In a now-famous 1935 letter to an American who had sought advice from him about her homosexual son, Freud wrote “it is nothing to be ashamed of, no vice, no degradation, it cannot be classified as an illness” (Freud, 1951, p.787).

By the 1940s, however, American psychoanalysis—then psychiatry’s dominant theoretical framework—had broken with Freud and embraced the view that humans are naturally heterosexual (not bisexual, as Freud argued) and that homosexuality represents a phobic response to members of the other sex. According to the conventional wisdom of the day, homosexuality was a sickness (Rado, 1940; see Bayer, 1987). Thus, although Psychology played an important role in depolarizing sex and love, thereby fostering a worldview that permitted people to be categorized according to the object of their sexual and romantic attractions, it also created a new dichotomy in which heterosexuality was equated with normalcy and homosexuality was equated with disease. The language of diagnosis served to perpetuate society’s longstanding legal and religious condemnation of sodomy in general and of same-sex sexual acts and desires in particular.

When the United States entered World War II, government personnel policies incorporated the illness model. Whereas existing military regulations already prohibited sodomy (including homosexual behavior), now the armed forces sought to bar homosexual persons from their ranks (Bérubé, 1990). As part of their charge to screen potential inductees and volunteers for a variety of pathologies, military psychologists and psychiatrists had the responsibility of detecting homosexuals. The screening was superficial, however, and mental health professionals often looked the other way, allowing homosexuals to enter the armed forces during the war’s early years when personnel needs were great. As Menninger (1948) observed, “[p]robably for every homosexual who was referred or came to the Medical Department, there were five or ten who were never detected. Those men must have performed their duty satisfactorily, whether assigned to combat or to some other type of service” (Menninger, 1948, p. 227).

During the war’s waning years, however, antihomosexual policies were vigorously enforced, witch hunts occurred frequently, and many gay men and lesbians received undesirable (“blue”) discharges as sexual psychopaths. The circumstances surrounding their separation from service (or, for some, their initial rejection as inductees) were communicated to their hometown draft boards and prospective employers, effectively “outing” them to their family and community. Socially ostracized in civilian life, they were denied benefits under the GI Bill and often could not secure employment. As Bérubé (1990) noted, “Wherever blue-discharge veterans lived, employers, schools, insurance companies, veterans’ organizations, and other institutions could use their bad discharge papers to discriminate against them on the basis of their undesirable status or their homosexuality. Sometimes their lives became so unbearable as exposed homosexuals that they had to leave home or tried to kill themselves” (p. 229).

Homosexuality’s status as a mental illness also had serious consequences outside the military. Gay and lesbian civilians

risked arrest not only in gay bars and other public settings, but even at gatherings in private homes. They were routinely charged with offenses such as disorderly conduct, vagrancy, public lewdness, and solicitation. Exploiting homosexuality's diagnostic status, many states passed sexual psychopath laws that put homosexuals in the same category as rapists and child molesters and permitted their indefinite confinement in a psychiatric institution until they were declared cured (Chauncey, 1993; Freedman, 1989). Homosexuality's classification as a mental illness also was used to justify federal and state laws and regulations that barred homosexuals from employment or prohibited them from obtaining professional licensure in numerous occupations. Thousands lost their jobs as government employees, teachers, and hospital workers (D'Emilio, 1983).

During this era, many psychiatrists and physicians attempted to "cure" homosexuality—that is, they tried to change homosexuals into heterosexuals. Their techniques were overwhelmingly ineffectual (American Psychological Association, 2009; Haldeman, 1994). Large numbers of homosexual men and women spent countless hours undergoing psychotherapy in what proved to be a vain effort to change their sexual orientation (for a personal account, see Duberman, 1991). When psychotherapy did not work, many resorted to (or were coerced into) more drastic methods, including hormone treatments, aversive conditioning with nausea-inducing drugs, lobotomy, electroshock, and castration (e.g., Feldman, 1966; Max, 1935; Thompson, 1949; see American Psychological Association, 2009; Katz, 1976). Failures in these attempts led many homosexuals to suicide.

Challenges to the Illness Model

Although Psychology played a central role in legitimating and perpetuating heterosexism, not all mental health practitioners endorsed the policies and regulations that subjected homosexuals to discrimination and other forms of stigma (Bérubé, 1990; Freedman, 1989). And scientific challenges to psychiatric orthodoxy emerged in the 1940s and 1950s, even as homosexuality was listed as a mental illness in the first edition of what would eventually be called the *Diagnostic and Statistical Manual of Mental Disorders*, or *DSM* (American Psychiatric Association, 1952).

In 1948, Alfred Kinsey published his book on sexual behavior in the human male, followed in 1953 by the companion volume on female sexuality (Kinsey, Pomeroy, & Martin, 1948; Kinsey, Pomeroy, Martin, & Gebhard, 1953). Based on more than 18,000 interviews, the Kinsey team's finding that homosexual experiences were remarkably common (reported by more than one third of their male respondents) directly contradicted the heterosexual assumption. Around the same time, Ford and Beach (1951) published an extensive review of cross-cultural and cross-species studies of sexual behavior, concluding that same-sex sexual behavior occurs in many animal species and that homosexual behavior of some sort was considered normal and socially acceptable in a majority of the societies for which detailed ethnographic data were available (Ford & Beach,

1951). Like the Kinsey studies, their work revealed homosexual behavior to be common, not a rare and pathological form of sexuality.

In the mid-1950s, Evelyn Hooker conducted what would be the first published study comparing the psychological functioning of a nonclinical sample of homosexuals with comparable heterosexuals (Hooker, 1957). Hooker's research represented a dramatic break with previous work because she did not accept the conventional wisdom that homosexuality was a pathology. Instead, she used the scientific method to test this assumption. She administered projective tests to matched samples of 30 homosexual and 30 heterosexual men—none of them currently in therapy—and asked expert judges who were unaware of each subject's sexual orientation to evaluate the men's psychological functioning and to indicate whether each was homosexual or heterosexual. The judges classified most of the men—heterosexuals and homosexuals alike—in the highest categories of adjustment. Using the Rorschach, they could not distinguish the men's sexual orientation at a level better than chance. When the 60 Rorschach protocols were presented in random order, only 6 of the homosexual men and 6 of the heterosexual men were correctly identified by both judges. When the judges later attempted to identify the homosexual man in matched pairs of protocols, only 12 of the 30 pairs elicited correct responses from both judges (Hooker, 1958).² Hooker concluded that homosexuality did not constitute a clinical entity and was not inherently associated with pathology. Her findings were subsequently replicated in empirical studies of men and women (Gonsiorek, 1991; for a compelling account of Hooker's life and research, see Harrison, Haugland, & Schmiechen, 1991).

In retrospect, it can be seen that by hypothesizing that no aggregate differences in psychological distress should exist between heterosexual and homosexual samples, Hooker's research and other studies that followed it actually applied too strict a test. We know today that some stigmatized groups manifest elevated rates of psychological distress as a consequence of the stress imposed on them by stigma-related ostracism, harassment, discrimination, and violence (Herek & Garnets, 2007; Meyer, 2003). Such patterns do not mean that the characteristic defining the group is inherently pathological. Nevertheless, by demonstrating that well-adjusted homosexuals not only exist but are in fact numerous, these studies demonstrated that the illness model had no scientific basis.

How did the illness model persist so long in the absence of supporting empirical data? In a narrow sense, much of the explanation lies in poor theory and inadequate methods. Proponents of the model employed theoretical frameworks that uncritically adopted then-current cultural values surrounding sexuality. Their empirical research, insofar as they collected data at all, was seriously flawed. The samples were badly biased, typically consisting of homosexual individuals who were incarcerated, institutionalized, or undergoing psychiatric treatment. The methods for collecting data introduced additional bias. For example, information about a homosexual patient might be obtained from her or his psychoanalyst rather

than through independent observation by a third party who was unaware of the individual's sexual orientation. It would have been surprising if such practices had not confirmed a priori assumptions about the psychological functioning of nonheterosexuals.

More broadly, the basis for the illness model's persistence was the fact that sexual stigma led psychiatrists and psychologists to equate sexual difference with pathology and to interpret their observations accordingly. For example, Gonsiorek (1991) noted that when the overall family patterns observed in a homosexual sample differed from those of a heterosexual comparison group, the former's experiences were typically assumed to be indicative of pathology whereas the latter's were seen as indicating mental health. This assumption was made without independent confirmation that the homosexual sample manifested greater psychopathology than the heterosexuals. Through a circular logic, homosexuals were assumed to be mentally ill because they often reported family patterns that some psychoanalytic theories presumed were pathological, and the "proof" that the family patterns were pathological was that they were often observed among homosexuals (Gonsiorek, 1991).

Given Psychology's central role in legitimating heterosexism, it is perhaps not surprising that data such as those reported by Hooker and the researchers who followed her were not sufficient to change professional opinion. Indeed, homosexuality was still listed in the 1968 edition of the *DSM* (American Psychiatric Association, 1968). Change came only when the mental health profession was pressured by the targets of stigma themselves. In the 1970s, gay and lesbian activists directly confronted the psychiatric and psychological establishments, conducting protests at professional meetings and demanding that long-standing diagnostic assumptions be subjected to scientific scrutiny and debate.

Faced not only with the empirical evidence but also with changing cultural views of homosexuality, Psychology radically altered its stance. In 1973, the American Psychiatric Association's Board of Directors voted to remove homosexuality from the *DSM* (Bayer, 1987). The American Psychological Association endorsed the psychiatrists' actions, passing a resolution that stated, in part: "Homosexuality per se implies no impairment in judgment, stability, reliability, or general social and vocational capabilities: Further, the American Psychological Association urges all mental health professionals to take the lead in removing the stigma of mental illness that has long been associated with homosexual orientations" (Conger, 1975, p. 633).

When Psychology reached a consensus that homosexuality is not a mental illness, one of heterosexism's supporting pillars crumbled and a principal justification for sexual stigma vanished. Moreover, in the course of reversing its longstanding position on homosexuality, Psychology committed itself to undoing the harms that the illness model had inflicted on sexual minorities. The focus of clinical practice shifted from "curing" homosexuality to assisting sexual minority individuals in leading fulfilling and happy lives. Scientific research began to examine the social psychological roots of heterosexuals' prejudice against sexual minorities. Psychologists shared their

scientific and clinical expertise about sexual orientation and sexual minorities with the courts, with legislative bodies, and with the general public. This remarkable reversal has been important in influencing societal attitudes and providing a basis for reversing many of the antigay policies and laws that were enacted in the 20th century. It continues to have significant consequences (Herek, 2007).

Challenging the Sexual Differences-As-Deficits Assumption Today: Parenting

Despite Psychology's repudiation of its former legitimization of heterosexism, the differences-as-deficits model persists in American society and still warrants an ongoing response. One example is found in contemporary policy debates about the status of sexual minority adults as parents. Many gay, lesbian, and bisexual people have children, whether from past heterosexual relationships, through artificial insemination with a current partner, by adoption, or through other means (Goldberg, 2010; Patterson, 2000). Given homosexuality's historically stigmatized status, it is not surprising that sexual minority parents have frequently been the targets of hostility and discrimination. A parent's homosexuality has often been grounds for denying her or him custody in divorce proceedings. In many states, second-parent adoption rights either have not been established or have been ruled by courts not to be permitted under current law. And some state laws explicitly prohibit adoption or foster parenting by gay individuals, same-sex couples, or individuals cohabiting with a same-sex partner.³ Parenting has also been a central issue in debates about marriage equality for same-sex couples (Herek, 2006).

Science figures prominently in public discourse about sexual minority families. In addition to presenting religious and political arguments, the detractors and supporters of gay- and lesbian-parented families also routinely attempt to demonstrate that their position is supported by empirical research. Proponents of antigay family laws and policies, for example, have often cited scientific research to justify their claim that the legal recognition of marriage between partners of the same sex would jeopardize the well-being of children. The assertion by one conservative Christian organization that "study after study has found that boys and girls not raised by both of their biological parents are much more likely to, among other things, suffer abuse, perform poorly in school, abuse drugs and alcohol and wind up in trouble with the law" is typical (Focus on the Family, 2004, cited in Herek, 2006; see also National Association for Research and Therapy of Homosexuality, 2008; Stanton & Focus on the Family, 2005; Toalston, 2004).

Such claims inappropriately use studies that compare the children of intact heterosexual families with children being raised by a single parent as a consequence of divorce, separation, or the death of a spouse. Those studies generally show that, all else being equal, being raised by two parents is more beneficial for a child than having a single parent (e.g., McLanahan & Sandefur, 1994), but this body of research has not addressed questions about the parents' sexual orientation. Dozens of

published empirical studies have directly examined sexual minority parenting, most of them focusing on lesbian and bisexual mothers and their children. The studies vary in their methodological quality, but the findings to date have been remarkably consistent. Comparisons between heterosexual and sexual minority families do not reveal reliable disparities in the children's mental health or social adjustment or in the parents' fitness or parenting abilities (for reviews, see Goldberg, 2010; Herek, 2006; Patterson, 2000, 2006). Antigay activists have dismissed these findings wholesale as methodologically flawed (e.g., Bennett, 2001; Stanton & Focus on the Family, 2005).⁴

Two observations about this controversy are relevant to this article. First, the fact that advocates for each side in the debate claim support from empirical research demonstrates how the public looks to science for answers in this area and underscores the importance of scientists doing everything possible to ensure that research findings are accurately communicated to the lay public and to policy makers. Second, the heterosexual assumption greatly influences the terms of the debate. When behavioral scientists publicly discuss research in this area, we can easily find ourselves implicitly endorsing the proposition that heterosexual parenting is the gold standard to which other family forms must measure up, thereby validating the equating of sexual differences with deficits.

For example, questions about the adult sexual orientation of children raised in a sexual minority household are routinely raised in policy debates. The empirical data on this topic are limited but are consistent with the conclusion that the vast majority of those children eventually grow up to be heterosexual (Herek, 2006; Patterson, 2000). Simply reporting these findings, however, can communicate a tacit endorsement of the value assumption that it would be a negative outcome for the children of sexual minority parents to grow up to be gay themselves.

Similarly, questions are raised about the gender conformity and attitudes of children with nonheterosexual parents. The published data have not revealed consistent differences in gender role conformity between the children of lesbians and the children of heterosexual parents (Herek, 2006, Note 6; Patterson, 2000). Reporting this fact, however, might be interpreted as endorsing the differences-as-deficits model—that is, agreeing that systematic differences in gender attitudes or behavior between the children of heterosexual and nonheterosexual parents would be problematic if they were detected and could be attributed to the parents' sexual orientation. Yet it may be psychologically healthy for children to hold flexible attitudes about gender roles: for example, for girls to aspire to traditionally masculine occupations such as astronaut, doctor, lawyer, or engineer (e.g., Barrett & White, 2002). It is difficult to convey this message in a court brief or in expert testimony when the immediate goal is to avert the separation of children from their parents or to prevent sexual minorities from being further stigmatized.

Conclusion

The history of Psychology's stance toward homosexuality illustrates how scientific theories, research methods, and

clinical practices often incorporate and reproduce cultural values to the detriment of society's less powerful groups. But it also shows how institutions can change. During the past century, Psychology has transformed itself from a profession that once provided an institutional foundation for sexual stigma to one that is now dedicated to actively challenging that stigma through research, practice, teaching, and other professional work.

Nevertheless, the differences-as-deficits assumption remains pervasive in public discourse about sexual orientation and sexual minorities. In addition to discussions of sexual minority parenting, as noted above, the assumption is also evident in debates about the legal recognition of same-sex intimate relationships (in which heterosexual marital relationships are routinely treated as the gold standard), the "cause" of homosexuality (in which the origin of heterosexuality is typically not questioned), and many other issues about which Psychology has relevant expertise.

In making good on Psychology's pledge to eradicate the stigma historically associated with homosexuality, it is important that we not only challenge widespread factual misconceptions in these domains, but that we also address the deeper structures that perpetuate sexual stigma. Even as we continue to share our specific research findings and clinical insights about sexual orientation with the lay public, we should also promote a fundamental questioning of the assumption that differences between the nonstigmatized majority and a stigmatized minority group inevitably reflect the latter's deficits.

Notes

1. *Psychology* is used here as shorthand for referring not only to scientific and clinical psychology, but also to psychiatry and related disciplines, the mental health professions, and the behavioral sciences in general. This usage necessarily obscures differences among the disciplines and professions, whose explication is beyond the scope of the present article.
2. Hooker also assessed the utility of other Rorschach methods for identifying sexual orientation, including the Wheeler signs and the Schafer content themes. Although some analyses of the Wheeler signs yielded statistically significant differences between the homosexual and heterosexual men, she noted that the "very dubious validity of the individual signs" made their value questionable (Hooker, 1958, p. 51). She also concluded that the Schafer themes overall "would be of little or no value in diagnosing homosexuality in individual cases" but noted that a few themes differentiated the two groups and suggested that "further efforts to objectify and refine the scoring would . . . be warranted" (p. 47).
3. At the time of this writing, some of these laws are in the process of being challenged in state courts.
4. The exception has been an embrace of the one published empirical study that found children with lesbian or gay parents to be functioning substantially more poorly than children with married heterosexual parents (Sarantakos, 1996). This study has been promoted despite its use of a relatively small convenience sample, a characteristic that conservative activists have decried as a fatal flaw in the studies that found no differences in the adjustment of children according to the sexual orientation of their parents. In addition, its

methodological shortcomings have been ignored. For example, parental sexual orientation was confounded with divorce: Most of the children of same-sex couples had experienced parental divorce (many in the recent past) but the children of married parents apparently had not. Whereas having gay or lesbian parents has not been linked to poor adjustment or academic performance, the negative effects of divorce on children are well documented (e.g., Amato, 2001). Moreover, the children in the sample with homosexual parents faced such high levels of prejudice that some of them had to transfer to a different school or their families had to move to another town. These factors and other methodological weaknesses most likely explain the study's anomalous results. However, antigay groups have cited this article from an Australian social work journal as scientific confirmation of their claims, even as they dismiss the bulk of published research in this area (e.g., Family Research Institute, 2001; Rekers, 2005).

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I'm White and You're Not: The Value of Unraveling Ethnocentric Science

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Abstract

Whether motivated by racist intent, ethnocentric arrogance, or data analytic myopia, research that systematically stigmatizes specific human groups is destructive. The racist assumptions of the inferiority of African culture and persons of African descent have contributed substantially to stigmatizing beliefs about African Americans. This article presents historical examples of several kinds of motivated or myopic theoretical and empirical projects that stigmatize African Americans and examples of how an examination of culture and social context may alleviate these problems. A discussion of ways to diversify psychological science includes diversifying those who do it, broadening the cultural perspectives and problems from which psychological concepts emanate (particularly in the United States), and formalizing diversity science as a system of theoretical and empirical research.

Keywords

diversity science, stigma, African Americans, racism, culture

Let me begin by explaining my title. When first asked to contribute to the symposium upon which this set of articles is based, I contemplated what I wanted to say and, for some reason, I channeled Chevy Chase. In the late 1970s, Chase closed his weekend update segment on *Saturday Night Live* by concluding, “I’m Chevy Chase, and you’re not.” Chase’s witticism implicates a self-centeredness that lies behind the ethnocentric, gender-centric, culture-centric biases that inspired the symposium. Thus, I co-opted the expression.

The Problem of Darkness

In the early 1960s, Hugh Trevor-Roper, a distinguished British historian, claimed that sub-Saharan Africa had no history; indeed, “There is only the history of Europeans in Africa. The rest is darkness.” And darkness, he noted, “is not the subject of history,” which he defined as purposive movement over time. Trevor-Roper therefore concluded that Africa’s record consisted of only “the unrewarding gyrations of barbarous tribes in picturesque but irrelevant corners of the globe” (cited in Fuglestad, 2005, p. 93). I find this description of the history of a continent and a culture to which I trace my ancestry chilling and demonstrative of how deeply embedded biased thoughts are in our collective conscious and unconsciousness.

There are a number of consequences of such biases. The main one involves the imposition of values, constructs, and

perspectives from one group of people onto another. A value-laden focus on group differences almost inexorably leads to a perception of differences as deficiencies. Cultural human capital is subverted by conceptual hegemony. The range of human capabilities is truncated to a narrow culture-centric point of view. This leads to two problems: one, stigmatization through our science and our interpretations, which was the primary subject of the symposium, and two, imposed limitations on our ability to learn more about a broader range of human behaviors and human capacities. The latter problem, the limitation in understanding the broad range of human behavior and capacity, has convinced me that we need to do a different and better kind of science.

In his book, *Is America Safe for Democracy?*, William McDougall (1921), who could be considered psychology’s version of Hugh Trevor-Roper, proposed two hypotheses for how a given nation may achieve international dominance and how it might lose it: (a) an economic hypothesis, stating that superior trade routes, favorable climate, and a wealth of natural resources result in world preeminence and domination, and

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(b) an anthropological hypothesis, stating that world preeminence and domination result simply from superior genes. McDougall rejected the former in favor of the latter, noting the lack of preeminence in sub-Saharan Africa, as well as parts of South and North America: “regions which are richest in all that man needs, but which nevertheless produced hardly more than savagery or barbarism” (p. 8).

McDougall shared a worldview with Trevor-Roper regarding the darkness of African people and culture. McDougall offered his own brand of “cultural psychology,” proposing that the psychological qualities that made for superior races (here he conflates race and culture as we continue often to do) include introversion, strength of will, curiosity, intellectual capacity, self-assertion, and providence—all of which are clearly individualistic traits and person-centered. These superior qualities were characteristic of so-called Nordic “races.” The opposite qualities, which by implication were judged to be inferior, included extraversion, sociability, gullibility, being closed to experiences, unintelligence, low cognitive function, docility, externality, and impulsivity. These traits characterized Mediterranean “races,” Negroes, and to a lesser extent Alpines.

Psychological science does indeed tend to show racial differences on most of these dimensions. In fact, McDougall talked about psychological testing of race differences as mental anthropology, which in his mind, was a “marvelous invention.” Thus, the goal was to use psychometrics to illuminate these differences and to show that these groups were deficient. If the research was conducted well and the results heeded, it was believed that America could avoid the downfall experienced by other nations by somehow marginalizing and controlling the influences of these inferior groups.

The “Sketchy” Science of Group Differences

One putative disease of Blacks in the 18th and 19th centuries was described as *Drapetomania*, which, according to the physician Samuel Cartwright, was a disease common to Africans and cats and was characterized by a tendency to run away (Guthrie, 1998). The African form of the disease was named *Dyaesthesia Aethiopsis*, which Cartwright characterized as “stupidity of the mind and insensibility of the nerves, . . . [the inclination to] break, waste and destroy everything they handle . . . as if for pure mischief” (Cartwright, 1851, p. 334). This disease was much more likely to erupt, Cartwright surmised, if slaves were treated as free men (Guthrie, 1998).

Psychological testing was designed, in part, to demonstrate these social conclusions. I call it *niche testing*. In *The Psychology of the Negro* (1916), Ferguson determined that although Blacks didn’t possess much capacity for abstract thought (cf. Herrnstein, 1971, and others), they were nevertheless capable in sensorimotor powers, which were involved in manual work. He concluded that training should concentrate on those capacities.

Crane (1923), concerned with the role of inhibition in human behavior, believed that Blacks lacked inhibition in moral realms but had superior inhibition in sensorimotor systems. He tested the latter idea by rigging a guillotine test where

a big block came down toward the hand of a participant who was instructed to leave his hand there because, he was told, the guillotine will stop before it hits him. Crane measured the extent to which participants tended to react. Although they distrusted the apparatus and the experimenter, the Black participants were able to inhibit their reactions, which led Crane to conclude that “in those vocational pursuits which involve great sensory shocks and strains not unaccompanied by danger, the Black man should prove more efficient than the White” (cited in Guthrie, 1998, p. 51). Again, pseudoscientific research was used to support social engineering and maintenance of unequal social hierarchies.

The Mullato hypothesis asserted that White intelligence is diminished by the proportion of other-race blood and, conversely, that Black American and Native American intelligence is improved with the proportion of White blood. Psychologists spent many years trying to test this hypothesis by sampling different race mixings and trying to show that Negro IQ test performance decreased or increased in proportion to the amount of White blood.

Motivated by beliefs about Black and Native American inferiority, psychological research was designed to demonstrate this inferiority. When groups are alleged to be inferior, and the only opportunities in society for these putatively inferior groups are those consistent with that inferiority, then members of the group demonstrate qualities their social contexts afford. The result is a kind of a scientific self-fulfilling prophecy. This line of argument and action played a major role in sustaining apartheid in South Africa for over four decades.

The Curious Science of Group Differences

I now review a couple of research examples that illustrate various aspects of both the stigma problem (stigmatizing others through our science and our interpretations of differences) and the limited science problem (limiting our understanding of a broader range of human behavior and capacity by a deficiency approach to differences). First is a study that I conducted in the early 1970s (Jones & Hochner, 1973) to test hypothesized differences between Blacks and Whites in self-paced and reactive sports activities (cf. Worthy & Markle, 1970). A self-paced activity was defined as one in which the performer controlled the execution of his athletic task against a relatively unchanging field (e.g., playing quarterback in football, pitching in baseball, shooting free throws in basketball, bowling alone or with others, golfing,). A reactive activity was defined as executing an athletic task in a changing performance context over which one had much less direct control (e.g., playing defensive back in football, hitting in baseball, shooting field goals in basketball, boxing).

Worthy and Markle (1970) had previously forwarded the hypothesis that Blacks would be superior at reactive athletic activities, but Whites would excel at self-paced athletic activities. Worthy and Markle had tested this hypothesis by examining the percentage of pitchers and hitters in the major leagues who were Black or White. Chi-square tests confirmed

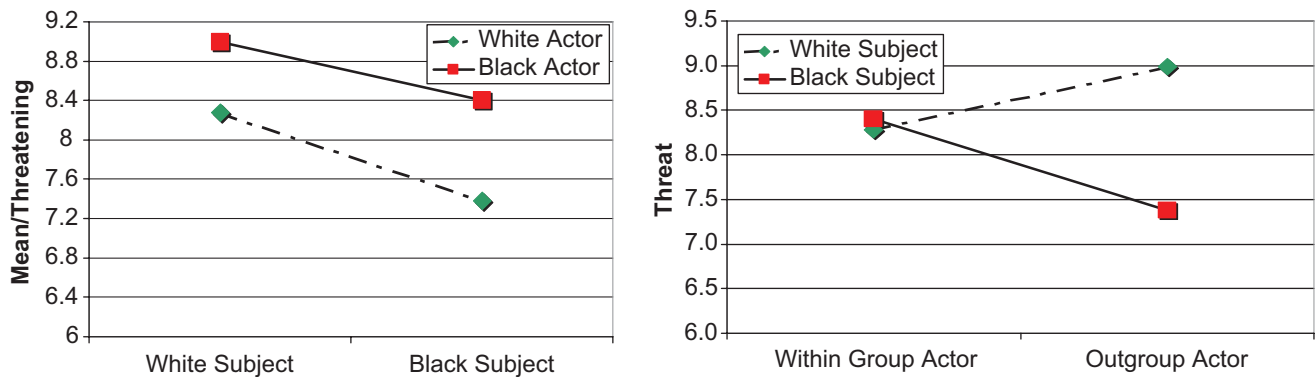


Fig. 1. Examination of fear reactions by White and Black participants to White and Black actors.

that Whites constituted a higher percentage of pitchers, whereas Blacks formed a higher percentage of hitters.

I argued that if Whites are really superior pitchers, they should perform better than Blacks at pitching and not merely be more numerous (Jones & Hochner, 1973). Using conventional statistics of pitching performance (wins–losses, earned run average, walks/strike out, and the like) it turned out that Blacks were better than Whites in pitching and hitting (higher batting average, runs batted in, etc.). So, if Whites were not better at pitching, why did they compose such a high proportion of pitchers in the major leagues? There are a number of plausible hypotheses, but the most likely is a pro-White bias for mediocre White pitchers. If a player is mediocre and he is Black, he most likely does not get the job, but if a player is mediocre and he's White, he does. The demonstration of bias in ambiguous racial situations is the hallmark of aversive racism (Gaertner & Dovidio, 1986).

We also found that Whites were, in fact, better free-throw shooters in basketball than Blacks, but Blacks were slightly, but not significantly, better field-goal shooters. How could Blacks dominate basketball but shoot free throws more poorly? Worthy and Markle (1970) offered a deficit interpretation of Black reactivity, implicating growing up in father-absent households as a precursor to reactive sport skills. This argument was taken into a genetic realm in a later publication (Worthy, 1974), in which Whites with blue eyes (Nordic) were shown to be more likely to excel at self-paced sports, such as bowling, golf, and so forth, than Whites with brown eyes (Mediterranean). In contrast, I proposed that a variety of factors including socioeconomic opportunity, cultural differences in motivation and formal instruction, and personal performance preferences broadened the discussions beyond the relatively stereotypical conflating of performance and cultural deficiency.

The next example arises from a problem of interpretation (Jones, 1983). Sagar and Schofield (1980) tested the idea that Blacks are universally perceived to be more aggressive. They asked Black and White urban middle school students to make judgments about a scenario in which an actor, who is either Black or White, interacts in an ambiguously aggressive way toward a target, who is either Black or White (the actor bumps the target, asks for his cake, pokes him in the back, or takes his

pencil). The participants rated the degree to which the actor's behavior was playful, mean, threatening, or friendly.

Sagar and Schofield (1980) reported a main effect for actor's race such that both Black and White students felt more threatened by Black actors than White actors. The left panel of Figure 1 shows this main effect. Sagar and Schofield (1980) concluded that the lack of a statistical interaction between the race of the subject and the race of the actor suggested a general bias among urban middle school students to perceive Blacks as more threatening.

However, I provided a different interpretative framework that examines both within-race and between-race judgments (see Jones, 1983). The right panel of Figure 1 rearranges the data according to whether the actor is within group (Black actors rated by Black students and White actors rated by White students) or outgroup (Black actors rated by White students and White actors rated by Black students). When the data are considered this way, it can be seen that the White and Black students rate within group actors as being equally threatening. I consider this the control condition. But White students rate Black actors as being more aggressive than White within-race actors, and Black students rate White actors as less aggressive than Black within-race actors. Thus, it was not the case that these variables did not interact, but that the interaction was different than the original researchers conceived. Given that the context for the study involved White students who were bussed into an inner city school that was predominately Black, the reinterpreted explanation is a better fit to this context than the original interpretation.

My point here is that context matters greatly. Drawing the right conclusion by using the appropriate comparison and proper behavioral evidence is often more complicated when racial, ethnic, and cultural differences are involved. I next turn to the issue of context.

The Problem of Context and the Science of Group Differences

In his best-selling book *Outliers*, Malcolm Gladwell (2008) dramatically illustrated the important influences of context. Software giants Bill Gates, Steve Jobs, and Bill Joy were born

within a few months of each other and each had extraordinary access to programming opportunities that became available to them though a variety of circumstances beyond their control. Thus, by the time the personal computer was invented in 1976, they were poised to take advantage of its enormous potential and therefore made enormous innovations in software and Internet capacities—and they accumulated great personal wealth by doing so. Had they been born a few years earlier or later, according to Gladwell, or had they not had the preparation that thousands of hours of programming opportunities provided to them, they would not be who they are today. Without doubt, these men are brilliant and possess great talent. But with all their brilliance and talent, it would not have been expressed the way it was without the benefit of the context in which they were born and grew up.

Just as context plays a great role in superachievement, it also plays a role in underachievement. Deficit models usually downplay or underutilize context in their accounts of underperformance. This is particularly true in standardized tests. But deficit models may also overinterpret context effects at times. I offer two examples: one that underutilizes context and another that overutilizes it.

An example of underutilization occurs when racial/ethnic differences in standardized tests are reported to show large deficits for minority group children. Duncan and Magnusson (2005) examined standardized test scores for kindergarteners in math and reading. Black children scored about two thirds of a standard deviation below White children in math and a little less than half of a standard deviation lower in reading. However, when socioeconomic status is taken into account, these differences nearly disappear: Blacks are then only .10 *SD* lower than Whites in math and .09 *SD* higher than Whites in reading. Similarly, reductions are found for Hispanics relative to Whites (from $-.72$ *SD* to $-.17$ *SD* on math and $-.42$ *SD* to $-.07$ *SD* on reading).

The overinterpreting context effect can be illustrated by the Black self-hatred problem. In *The Mark of Oppression*, Kardiner and Ovesey (1951) considered the Negro psyche to be wretched:

[The] Negro, in contrast to the White, is a more unhappy person; he has a harder environment to live in, and the internal stress is greater. . . . There is not one personality trait of the Negro the source of which cannot be traced to his difficult living conditions. *There are no exceptions to this rule.* The final result is a wretched internal life. (p. 81)

The assumption of the psyche's negative response to stigmatizing contexts is the basis for several decades of research based on the presumption of low Black self-esteem.

However, Twenge and Crocker's (2002) meta-analysis of published studies comparing Blacks and Whites on self-esteem shows Black self-esteem to be reliably higher than White self-esteem (moderate effect size of .2) and that this Black-White disparity has been growing since the early 1980s. What do we make of these findings in light of the self-hatred thesis? Well, I think we need to take another look at the presumed direct

relationship of a poor or toxic environment and its psychological sequelae.

For example, Crocker and Major (1989) proposed an attributional ambiguity explanation—Blacks discount negative evaluations based on the possible racial prejudice of the evaluator. Heine, Lehman, Peng, and Greenholtz (2002) proposed a reference group effect by which one compares oneself with members of one's reference group rather than with an abstract norm. Thus, relative stranding within one's group may play a bigger role in self-esteem than do comparison with broader socially stigmatizing norms (see also Cross, 1991, for an explanation of how one's racial reference group orientation can be separated from one's personal identity). Learning how one may make adaptive and resilient responses to negative, aversive, and challenging contexts is important to study as a general psychological trait. The research on stress and coping can and has benefitted from understanding how marginalized racial and ethnic groups cope with stigma.

Members of marginalized, stigmatized groups face what I call a *duality dilemma*, a double consciousness (DuBois, 1903) comprising awareness of the degraded status of one's group and desire to achieve personal goals while embracing one's group. Attempts to understand the psychological mechanisms by which one manages the challenges of psychological duality (or intersectionality, as we refer to it now) is an opportunity for psychological science to understand an important facet of diversity in America. We have made great strides but have much further to go.

Culture as the Ultimate Context for a Science of Group Differences

Returning to Gladwell's (2008) *Outliers*, the author argued that Asians are superior in math because of the exacting requirements of rice paddy farming, belief in the inevitable success of hard work, and language. He further asserted that the cultural variations in *power distance* (the cultural tendency to subordinate personal control to the power and authority of others) proposed by Hofstede (1980) combine with language to explain a rash of air crashes by Korean Airline pilots. Poor children, particularly Blacks and Hispanics, perform more poorly in school than do middle-class children, and the gap grows with increasing years. But the entire deficit can be explained by declines between the end of one school year and the beginning of the next. Middle- and upper-class "culture" provide relevant advantages for children in the summer (exposure to books, camps, vacations, travel) that lower-class children usually do not enjoy.

Recently, McGill-Franzen & Allington (2008), for each of three successive years, gave 842 randomly selected primarily African American students from 17 different high-poverty elementary schools, 12 books of their choosing to take home over the summer. In comparison with a control sample of 428 students from the same schools, the book intervention led to higher reading scores, a reduction in the "summer slide," and overall performance equivalent to going to summer school.

In short, culture truly matters (here, the books serve to reduce the culture gap), and failure to account for it leads to shortcomings in our understanding of the causes and consequences of human behavior.

Culture has had a resurgence in psychology in the last two decades. I say “resurgence” because culture was implicated more directly by McDougall’s (1921) analysis of U.S. society and by the first *A Handbook of Social Psychology* (Murchison, 1935), which devoted separate chapters to the “Negro,” “Red Man,” “White Man,” and “Yellow Man.” Advances in our thinking about culture have grown dramatically as increasingly sophisticated theory and research methods have enabled us to learn both the limits culture places on general phenomena and ways in which human behavior at times transcends cultural specification.

Cultural psychology provides a major source of diversity in our science. And it does so from a basic scientific approach involving theory specification, hypothesis formulation and testing, and experimental research designs. A wide swath has been cut through basic psychological constructs by the evidence for the relevance of culture to empirical generalizations. Prominent along this path is the research on the self (Markus & Kitayama, 1991). The interdependent self has properties that diverge from the independent self, over and above the general correlations of each with individualistic and collectivistic cultures. (See Heine, 2010, for a detailed review of cultural psychology theories and empirical findings.) For our purposes, cultural psychology diversifies our science by testing the boundaries for theoretical propositions and empirical findings, introducing new variables and theoretical formulations, and bringing more psychologists from diverse backgrounds to the scientific enterprise.

But when we consider diversifying science by expanding our research with racial and ethnic groups within the United States, the effort falls far short. The darkness problem persists. In the first edition of *Prejudice and Racism* (Jones, 1972), I framed the raging IQ debate of the time as an attempt to determine whether Blacks were inferior because of their genes or their environment—a variant on the McDougall hypothesis. For the most part, psychological science that addresses U.S. racial and ethnic groups focuses on explaining causes of or eliminating the psychological deficits of those groups. What is less frequently addressed is the possibility of expanding our scientific understanding by studying the ways in which basic psychological phenomena are cultivated and expressed within the diverse contexts associated with ethnic and racial group membership.

Space does not permit examination of all the work that has been undertaken in this regard (see issues of *Cultural Diversity & Ethnic Minority Psychology*, as well as the *Journal of Black Psychology* and the *Hispanic Journal of Behavioral Sciences*). Based on the premise that physical context, cultural dynamics, historical influences, and psychosocial marginalization create distinctive subcultural groups, I suggest that a cultural psychology approach be taken with respect to racial and ethnic groups in the United States.

Approaches to Diversifying Science

I conclude this article by proposing three ways to diversify psychological science: Diversify those who do the research; broaden the cultural perspectives and problems from which psychological concepts emanate, particularly in the United States, and formalize diversity science as a system of theoretical and empirical research.

Diversifying Those Who Do the Research

Most of the psychological concepts we explore arise from observations, experiences, and perspectives of the scientists who do the research. When I wrote the first edition of *Prejudice and Racism* (1972), it was my intention to reflect on my experience as a Black male in America and to situate the extant literature on prejudice within that perspective. I believe that infusing psychological science with ethnic, racial, and cultural diversity involves inclusion of these historically outsider perspectives. The inclusion may lead to new interpretations of old findings (for example the self-hatred thesis mentioned earlier) or constructing new concepts and variables that emanate from cultural legacy and sociocultural challenges.

In 2008, 24% of 2,837 new PhDs were Black, Hispanic, Asian American, American Indian, and multiracial, and 76% were classified as White (National Science Foundation, 2009, Table 37). Hispanics were the largest percentage (9.7%) followed by African Americans (5.8%), Asian Americans (5.2%), multiracial individuals (2.5%) and American Indians (0.5%). Of these 360 minority PhDs, 54% were in subfields traditionally related to practice (clinical, counseling, school, family), 33% were in subfields traditionally related to research (cognitive, developmental, social, industrial–organizational, experimental, behavioral neuroscience), and 13% were in general psychology.

What should these numbers be? I think census data alone are not the proper measure of expected racial or ethnic representation. Efforts to grow the pool of racial/ethnic minority doctoral students in psychological science thrived from the late 1970s through the early years of this century with support from the Minority Fellowship Program (MFP) of the American Psychological Association and with grant support from the National Institute of Mental Health (NIMH; Jones & Austin-Dailey, 2009). In 1975, a mere 5% of PhDs in psychology were granted to racial/ethnic minorities, but by 2008, the number had increased to 24%. However, in 2004, NIMH terminated the grant support for MFP, along with pipeline programs for undergraduates at Black- and Hispanic-serving colleges and universities.

Although there has been steady progress in production of new racial/ethnic psychologists over the past three decades, the number is still too small. We need to mount systematic efforts to increase the number of undergraduate majors, to provide opportunities for them to work in our research labs and thereby get excited about research, to bring them into our doctoral programs, and, ultimately, to help them build strong independent research careers. We need to do all of this while keeping open

the opportunities to examine the issues that emanate from their racial, ethnic, and cultural experiences.

The representation of racial/ethnic faculty in psychology departments is less than that of new PhDs. In a very recent survey of 1,279 faculty members in psychology graduate departments (Wicherski, Jacobsen, Pagano, & Kohout, 2010), 14.7% were Black, Hispanic, Asian, American or multiracial, and 85% were White. Having more faculty from racial/ethnic and cultural backgrounds will no doubt aid in attracting students to the major, encouraging students to academic research careers, and enabling growth in research and theory.

Diversifying Theory and Research Ideas

The explosion of theory and research in cultural psychology in the past two decades illustrates what can happen when we diversify our psychological science. Trying to understand how culture and context influence human behavior is the impetus for new theoretical challenges, methodological complexities, and practical applications. The excitement and energy directed at cultural psychology could similarly infuse our understanding of racial/ethnic and cultural diversity in the United States. These questions go beyond efforts to demonstrate or explain group deficiencies to searching for mechanisms that account for effective functioning in adverse social contexts. The possibility that at least some of these mechanisms may be different from those we already understand well is a big payoff in this endeavor.

One example of this approach is my attempt to formulate a cultural basis for patterns of coping and adaptation by African Americans. TRIOS is an acronym for five psychocultural concepts (time, rhythm, improvisation, orality, and spirituality), each of which can be traced to aspects of African culture throughout the African Diaspora (see Jones, 2003, for a fuller discussion.) A TRIOSic personal perspective confers certain forms of resilience and adaptive capability for those facing hardships and challenging contexts. Data linking TRIOS level with psychological well-being support this effect among Africans, sometimes for African Americans, and even on occasion for Whites, among whom being TRIOSic is correlated with lowered Stroop interference effects. Further, African American Head Start mothers show evidence of greater emotion regulation as their TRIOS levels increase. We can learn, perhaps, some new concepts by going beyond the traditional comparative analyses.

Saying that context matters often leads to the conclusion that if we simply fixed the environment, everything would be okay. We do need to pay more attention to the environment and social context, but we must look at how the person interacts with that environment. Plomin (1988) has shown how few personal traits or qualities are shared even among siblings in the same family. Shared environmental effects are as low as 10%–15%, compared to genetic (35%–40%) and nonshared environmental (50%–55%) effects. Wilson (1987) has shown that poor Blacks live in poorer neighborhoods than poor Whites, suggesting that equating on socioeconomic status is not that easy and that, even when we do, there are other context effects to account for. So

how does cultural capital operate across groups and contexts to confer resiliency and avoid dysfunction? We must diversify the perspectives brought to bear on psychological science if we hope to answer such complex questions.

Stan Sue (1999) wrote a wonderful paper in which he argued that we privilege internal validity over external validity in our methods and interpretations of results. But it is external validity that helps us to know how people differ, how environment matters, and even how our variables operate across contexts. Raising external validity to a more prominent place is one way to advance scientific diversity.

Develop Diversity Science

There is a growing tendency to reformulate research on race relations as diversity science (see Plaut, 2010b). The June 2010 issue of *Psychological Inquiry* is devoted to a discussion of diversity science, what it entails, and why or if it is needed. In her lead article, Plaut (2010a) defines diversity science as “the study of the interpretation and construction of human difference—of why and how difference makes a difference—within the context of existing, historically shaped cultural and structural realities” (p. 168).

In my commentary on Plaut’s article (Jones, 2010), I proposed that diversity science researchers must, in addition to addressing the sociocultural bases of racial inequalities, consider the following: (a) the duality of “belonging” (to groups, institutions, society, culture) and “uniqueness” (individuality, difference, intersectionality) and how diversity science can identify the ways positive psychological outcomes are achieved in complex multifaceted contexts and the mechanisms that produce them; (b) diversity conflicts are not always resolvable by binary either/or logic (e.g., perpetrators vs. targets) or right–wrong thinking, so a more inclusive logic should direct our formulation of research ideas and hypotheses; (c) diversity science must develop increasingly complex theories and multilevel, multidimensional analytical frameworks to deal with added complexity that considering diverse groups’ interactions necessarily entails (e.g., the multidimensional models of racial identity by Sellers, Rowley, Chavous, Shelton, & Smith, 1997, and Cross, 1991, as well as models of acculturation by Berry, Phinney, Sam, & Vedder, 2006, and LaFromboise, Coleman, & Gerton, 1993); and (d) they must understand diversity within diverse groups. This issue is best reflected by the concept of intersectionality within people (Cole, 2009) and interethnic/racial relations between and among groups.

Conclusion

An African proverb illustrates the darkness problem: “The true tale of the lion hunt will never be told as long as the hunter tells the story.” With respect to understanding racial, ethnic, and cultural groups, the scientific story is in large part a hunter’s story. The lion’s story can be better told, and in doing so, we can learn more about the hunter and the hunt. But how can we tell a different story?

First, we take seriously the hunter–lion caveat. We look for variables, mechanisms, and constructs in lives and experiences of diverse groups. We engage in inside-out theorizing—encouraging people from the group to theorize about the group or insiders to become involved in framing the research questions and conducting and interpreting the research. Everyone knows that diversity in science follows from diversity among scientists. And we study diversity itself: the factors that inhibit it, the factors that promote it, and the consequences of each for social order and psychological well-being.

And finally, we need to shine our scientific lights on the darkness, lessen the tendency to produce deficit models of others, and expand the ability to employ our science to learn more about human capacity, capability, and potential.

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Culture and the Home-Field Disadvantage

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Abstract

The *home-field disadvantage* refers to the disadvantage inherent in research that takes a particular cultural group as the starting point or standard for research, including cross-cultural research. We argue that home-field status is a serious handicap that often pushes researchers toward deficit thinking, however good the researchers' intentions may be. In this article, we aim to make this home-field bias more explicit and, in doing so, more avoidable. We discuss three often-overlooked disadvantages that result from this home-field status: the problem of marked versus unmarked culture, the problem of homogenous versus heterogeneous culture, and the problem of regression toward the mean. We also recommend four interventions researchers can apply to avoid the home-field disadvantage or, at the least, attenuate its deleterious effects.

Keywords

cross-cultural psychology, psychological distance, research bias

If cultures were not ordinarily different, one from another, we would find both the concept of culture and research about it of little interest. From what people eat (e.g., Rozin, 2007) to their marriage customs and sleeping arrangements (e.g., Shweder, Jensen, & Goldstein, 1995) to the relation of language to thought (Gentner & Goldin-Meadow, 2003), intriguing cultural differences are found. The past few decades of work have shown the folly of assuming that what undergraduates at major research universities do in experimental tasks is universal or even typical (e.g., Henrich, Heine, & Norenzayan, 2010; Medin & Atran, 2004), and there is evidence that even basic cognitive processes may differ across cultures (Nisbett, 2003).

But it is also true that few research areas are as problematic or have as jaundiced a history as the study of culture. Cultural research has been used as the instrument of colonialism to justify subjugating people whose thought processes are “more primitive.” It is easy to think of culture as something other groups have that prevents them from seeing the world objectively, like we think we do (e.g., Ross, 2004). It is also easy to interpret cultural differences in terms of a deficit model, in which the cultural group that differs from us is seen as having failed, where “failed” means not performing in accordance with our standard. For these reasons, some have argued that the very construct of culture is so susceptible to stereotyping and essentializing other groups that it should be discarded (see Brumann et al., 1999, for a review and commentaries).

For purposes of this article, we are going to assume that the most egregious and most transparent misuses of cultural

research have been called out (see Cole & Scribner, 1974, for a clear analysis and examples). Our focus will instead be on the more subtle and perhaps more pernicious forms of cultural bias that grow out of what we refer to as the home-field disadvantage.

The *home-field disadvantage* refers to the disadvantage inherent in research that takes a particular cultural group as the starting point or standard for research, including cross-cultural research. Given that psychological sciences have been dominated by research conducted in the United States, it is almost always the case that the beginning point consists of results obtained in labs at major U.S. research universities. It is not obvious that this is a disadvantage in itself, other than limiting the presumed generality of results. We argue that the home field is a serious handicap that pushes researchers toward deficit thinking (or some euphemism for deficit), however good the researchers' intentions may be. In this article, we aim to make this home-field bias more explicit and, in doing so, more avoidable.¹

Paths to the Home-Field Disadvantage

It may be useful to distinguish the contributors to home-field status. First, researchers themselves often come from only one

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of the cultural groups being compared (i.e., ingroup as home-field). Second, research design, evaluation, and analysis often originate (or originated) with only one cultural group, whether or not the researchers themselves are from that group (i.e., starting point as home-field). Third, historically, members of both ingroup and starting point populations often occupy a position of power and authority relative to other groups being compared (i.e., power as home field). The fact that White American males do a great deal of the research on culture is an example of the ingroup and power paths to home-field status. The fact that the research protocol is often designed, evaluated, and analyzed using primarily White university undergraduate students from the United States as participants is an example of the starting point and power paths to the home field.

All three paths to home-field status asymmetrically influence the psychological distance between researchers and the cultural groups being studied. *Psychological distance* refers to how subjectively close or distant an event or person is across a range of measures of distance, including time, space, and personal identity (Trope & Liberman, 2003). In this article, we are particularly concerned with the third kind of distance, specifically the degree to which research designers identify with the cultural groups they are studying. All three paths to home-field status push in the same direction such that researchers are psychologically closer to their cultural ingroup, the cultural group they take as a starting point for research design, evaluation, and analysis, and the dominant or majority culture group.²

Home-field status—with its relationship to psychological distance—in turn contributes to at least three distinct (though overlapping) disadvantages. These are the more subtle dangers that we warned about. First, home-field status affects whether a group's cultural practices and beliefs seem normal or deficient (the problem of marked vs. unmarked culture). Second, home-field status affects the degree to which cultural groups seem more uniform and easy to essentialize or diverse and multifaceted (the problem of homogenous vs. heterogeneous culture). Third, by virtue of the process by which research stimuli and methods are selected, home-field status makes it likely that cultural differences with no basis in reality will be found. For reasons that will become clear we refer to this as the problem of regression toward the mean.

Marked Versus Unmarked Culture

At a typical psychology convention, if research participants are mentioned at all, the speaker most often refers to them as “people,” because it is safe to assume that the researchers are referring to undergraduate students at an American university (and most likely a student taking a Introduction to Psychology course), as well as the additional unmentioned demographic and cultural characteristics that go along with being a student at an American university. There are many reasons that this particular cultural group is unmarked, of course, including all three paths to the home-field advantage mentioned above. One

problem with this cultural group (i.e., the standard research population) being unmarked is that its peculiarities go unnoticed. Instead, the characteristics of this group are the characteristics of “people,” implicitly taken to be representative of humankind and thus providing insight into universal, culture-free human psychology. Culture is something that members of other populations have—the lens that shapes the way they see the world.

The problem of markedness may persist among cross-cultural researchers, though in less direct ways. Although we may carefully mark the demographic characteristics of our research participants, the cultural specificity of the normally unmarked group's practices and beliefs are not so easily marked.

Consider the following example from Medin's research with Scott Atran (Atran & Medin, 2008). One of the things these investigators looked at in their cross-cultural studies concerns the so-called *diversity principle* in reasoning about categories. For example, suppose you know that Disease A affects river birch and paper birch and that Disease B affects white pines and weeping willows. Which disease do you think is more likely to affect all trees? If you give problems like these to University of Michigan undergraduates, over 90% answer Disease B, the disease that affects white pines and weeping willows (López, Atran, Coley, Medin, & Smith, 1997). If you ask them why, they say something like this: “Well, Disease A could be just a birch thing, and if it happens for trees that are this different—as different as white pines and weeping willows, it's more likely to affect all trees.” This is the *diversity principle* in categorization and, at least initially, Atran and Medin thought the diversity principle might be universal. But when they tested it with Itza' Maya agro-foresters in Guatemala they found below chance diversity responding (López et al., 1997).

In this case, the university-student population was unmarked because their understanding of the task corresponded to Atran and Medin's (2008). Therefore, the “natural” question Atran and Medin asked was, “Why do the Itza' fail to show diversity?” To Atran and Medin, diversity seemed the only reasonable strategy. As it happened, Atran and Medin were also simultaneously conducting studies involving another population consisting of U.S. adults who knew a lot about trees—people such as parks workers and landscapers (Proffitt, 2000). Like the Itza' they also showed below chance diversity responding. For example, when presented with the disease scenario outlined above, 13 out of 14 parks workers picked Disease A—the disease that affects river birch and paper birch—as more likely to affect all trees. Their typical explanations were causal and ecological. They regularly said such things as: “Well, first of all, birches are incredibly susceptible to disease. If one of them gets it, they'll all get it. Secondly, they're very widely planted as an ornamental, and they're widely dispersed naturally; so there would be plenty of opportunities for that disease to spread.”

Had Atran and Medin begun their studies with Itza' Maya or tree experts the deficit thinking would have been reversed: “Why do college undergraduates fail to show causal and

British Columbia (BC) Statistics

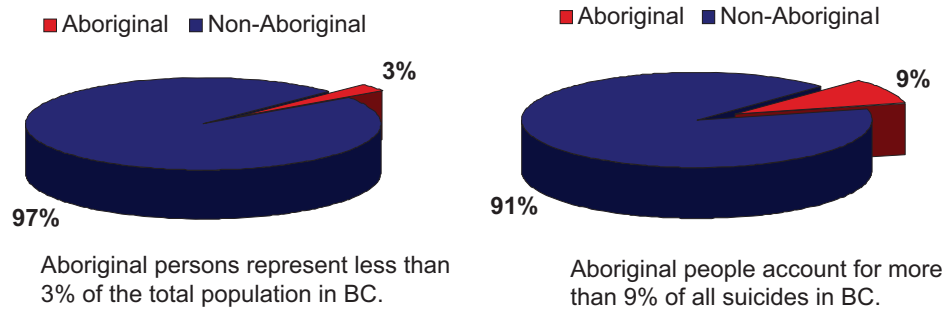


Fig. 1. Graphs showing First Nation population and suicide rate in comparison with the general population of British Columbia.

ecological inductive reasoning?” Even the notion of “expert” depends on one’s perspective. The tree experts could identify roughly 90% of the trees in the Chicago area, but where Atran and Medin work in Guatemala, nearly everyone can identify nearly all the trees. Itza’ Maya might feel that our tree experts lack expertise. Thus, whether the landscapers and park personnel are experts or whether Northwestern undergraduates have a nature-deficit disorder depends on where you start.

Homogenous Versus Heterogeneous Culture

There is extensive work in social psychology suggesting that greater psychological distance and power differentials bias people toward making dispositional rather than situational attributions (Fiske, 1993; Trope & Liberman, 2003). Galinsky, Magee, Ena Inesi, and Gruenfeld (2006) have experimentally manipulated power and found that a position of power is associated with a diminished capacity to take other people’s perspective and comprehend how they think and feel.

A related consequence of distancing may be its influence on what one takes to be the relevant unit of analysis. If outgroups are seen as homogeneous, then it will seem natural to aggregate over broader groups. Chandler and Lalonde’s (1998) research looking at suicide rates among First Nations peoples in British Columbia provides an example of how misleading results can be if the level of aggregation is too broad. First Nations or Aboriginal peoples have the highest rate of suicide of any culturally identifiable group (Kirmayer, 1994). In British Columbia, the suicide rate among First Nations people is about three times higher than it is for other Canadians (Fig. 1), and it is several times higher still for First Nations youth. Given this summary statistic, one might be tempted to organize suicide-prevention programs targeting all First Nations people.

But Chandler and Lalonde broke down the Figure 1 data by different bands or tribes (Fig. 2). Some of the more than 200 bands in their study have a suicide rate literally hundreds of

times higher than the national average, but 40% of the bands studied have a suicide rate of zero. Chandler and Lalonde then looked to see what might be correlated with these variable suicide rates. There are a number of easily imagined standard demographic factors that do not correlate, including whether the band members live in urban or rural settings, average income, population density, and rates of unemployment. Instead, they found that suicide rates were negatively correlated with efforts on the part of bands to restore and revive their threatened cultures. Using a series of measures that they label as markers of “cultural continuity” (including self-government, land claims in courts, health services, and knowledge of Aboriginal languages), Chandler and Lalonde found that the suicide rate decreased dramatically as a function of how many of these factors were in place (Fig. 3). This finding completely reverses the perspective on the meaning and policy implications of indigenous suicide rates.

Moving from a global First Nations perspective to an exploration of the differences that divide specific bands allowed the researchers to identify factors associated with resilience that would have been missed when reasoning about psychologically distant cultural groups.

Regression Toward the Mean

Another home-field disadvantage in cross-cultural research comes from knowing the “sweet spots.” Suppose we begin with some psychological phenomenon that has already been established using a North American sample, or use experimental stimuli that other people have been effectively using (with North American samples), or generate experimental stimuli or methodological intuitions already well honed within our own cultural group. If we then translate and transport these stimuli and methods for a cross-cultural comparison, chances are the phenomenon will weaken or disappear. This may well happen even when researchers work to translate the materials into

BC Youth Suicide Rate by Band (1987-2000)

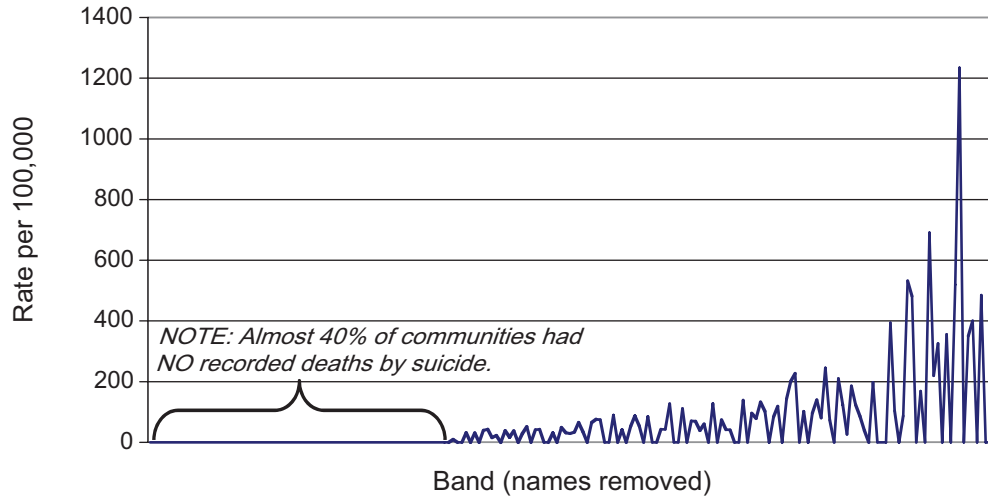


Fig. 2. Suicide rate for First Nation youths by band or tribe (1987–2000).

BC Youth Suicide Rate by Number of Factors Present in Community

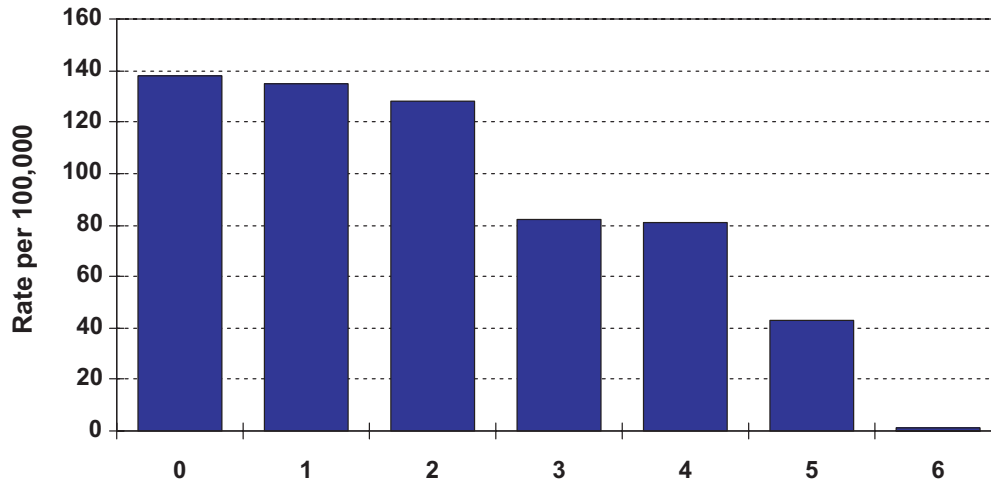


Fig. 3. Suicide rate for First Nation youths by number of markers of cultural continuity within the community.

local languages or otherwise take steps to insure that the assessment tools in use are locally meaningful and familiar. A common and tempting interpretation of this pattern of results is that we have discovered a bona fide cross-cultural difference, but such an interpretation may be problematic because of regression toward the mean.

Perhaps the easiest way to understand regression to the mean and its implications for cultural comparisons is with a concrete example. Suppose that we were studying sense of humor and developed a set of jokes in North America and then (after proper translation) tested them in another culture. We could be quite sure that people in the other culture

would not find them as funny as people in our home culture. But no reasonable person would want to conclude that people in the other culture had a worse sense of humor. We recognize that jokes have been selected for funniness and that this is determined by (variable) knowledge, values, and other sorts of individual and cultural variables. But what holds for jokes also applies to any assessment tool when it is selected and developed in one culture and applied to another.

This should not be taken as a restatement that home-field status is associated with psychological closeness or with an emic perspective, though we are making the point that home-

field status contributes to the problem. Regression toward the mean refers to an often-overlooked feature of random sampling: Items that are essentially outliers in terms of performance, such as extremely funny jokes or research methods that produce particularly impressive results, will tend to regress toward mean performance when replicated with a new sample. Home-field status—and the corresponding psychological closeness or emic perspective—contributes to one's ability to pick particularly effective stimuli (e.g., jokes or research methods), but one needs to understand the statistical phenomenon of regression toward the mean to understand why such outlier performance will usually not be repeated when applied to a new (even randomly different) sample.

Research findings are not simply measures of psychological phenomena—they result from an interaction between psychological phenomena and the stimuli designed to bring them out. Through trial and error, or active intention, research stimuli are selected according to how well they fit the participant population to produce a particular effect. For example, the most widely cited example used to show framing effects in decision making—Tversky and Kahneman's Asian disease scenario (1981)—also happens to produce larger effects than other framing scenarios having the same abstract structure (Kühberger, 1998). Looking for items that bring out some effect of interest makes perfect sense, but it has inimical consequences when used asymmetrically in cross-cultural comparisons. Because the particular stimuli have been selected to bring out particular effects with a particular (though often unmarked) cultural group, regression toward the mean predicts movement away from this exceptional performance toward the less exceptional, even if the second cultural group would display the same psychological phenomenon equally well (or better) using a different set of stimuli that had been tailored for success with the second population.

Of course, this does not always contribute to a perception that the second cultural group is deficient. The Asian Disease scenario, for example, is designed to demonstrate deficiencies (i.e., deviation from normative models of rationality). In such cases, regression toward the mean suggests that other cultural group will appear less deficient rather than more deficient. In general, the fact that research stimuli have been selected to "work" with a particular cultural group (usually American, usually university students) gives that group a privileged status that is not shared with other cultural groups, whether this privilege makes the original group look better or worse or just different.

Giving Up the Home-Field Disadvantage

Is there a cure for the home-field disadvantage? No, but we will propose four vaccines. The first is to simply try to remain aware of the three disadvantages discussed above. Actively mark the unmarked cultural group. This does not simply mean identifying the demographics of the populations and acknowledging that findings may not generalize to other groups; it means recognizing that seeing outside of our own culture-specific perspective is a considerable and ongoing challenge. Work to see cultural groups as diverse and

heterogeneous rather than homogenous. Recognize that research findings will tend to represent a relationship between a particular cultural group and the research design and that, therefore, when these same designs are applied to a different population, some kind of flattening of performance (regression toward the mean) can be expected.

Second, try to be as diverse and collaborative as possible in designing and carrying out research. This includes members of the research populations being studied. The Canadian Institute for Health Research has an outstanding set of guidelines along these lines for research involving Aboriginal peoples that might serve as a model for such an approach (Canadian Institute for Health Research, 2007).

A third approach, which is particularly challenging, is to do one's best to study the phenomenon of interest on the terms of the culture, or cultures, being studied. For example, if one were studying cultural differences in emotions, it would be a mistake to start with English emotion terms and try to identify their counterparts in another culture, as this presumes part of what one wishes to study (Boster, 2005). In other words, there is much to be gained by changing the starting point of investigation and the home-field disadvantages that come with it.

Our last piece of advice, which goes against our training as psychologists, is to substitute perspective taking for trying to be objective. The idea of trying to be objective may be like trying to draw a map without a point of view. The solution we propose to the impossibility of not taking any perspective at all, on one hand, and the pitfalls of taking a particular perspective, on the other, is to do one's best to take multiple perspectives. This entails not just taking the first two pieces of advice above, but also actively seeking to use multiple methods comprising a variety of research stimuli with a variety of cultural groups and collaborators. It also requires the humble recognition that whatever pains we might take to lose the home-field disadvantage, it may be that the best we can hope to achieve is a partial rendition of the phenomena we are seeking to understand.

Notes

1. Some related ideas about the home-field disadvantage can be found in our commentary on Henrich, Heine, and Norenzayan's excellent article (2010) on the hazards of using primarily Western, educated, industrialized, rich, and democratic (WEIRD) samples in psychology research (Bennis & Medin, 2010). Lest readers conclude that the ideas discussed in that commentary inspired the current discussion, it should be noted that the current article preceded the other commentary by more than 2 years.
2. Psychological distance is related to the *emic/etic* distinction commonly made among sociocultural anthropologists (Harris, 1976), who rightly note that one's perspective of a cultural group from either the inside as a member of that group (*emic*) or from the outside as an observer of that group (*etic*) has important implications for how one understands that group.

Declaration of Conflicting Interests

The authors declared that they had no conflicts of interest with respect to their authorship or the publication of this article.

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