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On Babies and Bathwater: A Call for Diversification and Diagnosis

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Henry (this issue) has provided a critical reminder that social psychologists' overreliance on university samples restricts our ability to test universal hypotheses regarding all of our topic areas, including prejudice. The first purpose of my commentary is to applaud his efforts and to urge our colleagues to heed his advice because social psychologists' preference (prejudice, if you will) for internal over external validity has created serious image problems for the field. The people in Washington responsible for funding decisions too often point to convenience samples as an excuse to trivialize our discipline. As many of our esteemed colleagues have recently discussed in public forums, social psychology suffers from a branding problem. It is necessary for us to "think outside the lab" and beyond college student samples to improve our standing in the eyes of our fellow scientists and the public at large. Although it is true that social psychologists have taken advantage of the Internet to access diverse samples since Sears (1986) highlighted our blind spot, Henry's overview of our journal publications suggests we remain entrenched in a methodological rut. The situation is self-perpetuating: The more we rely on college students, the lower our research funds; the lower our funds, the more we are forced to gather data on the cheap. Henry's argument that we need to expand our methodologies is unimpeachable if, for no other reason, to improve our status as a discipline.

My second purpose is to encourage prejudice researchers to take stock of the extent to which college students yield different patterns of data, compared with general adults. That is, I do not question Henry's concern, but we need to know how serious the problem actually is. My first thought was to turn to meta-analyses of the prejudice literature to learn whether sampling characteristics, such as age or level or education, function as moderator variables. My search revealed only three cases in which the authors investigated the issue, two of which involved age as a moderator of the contact hypothesis—a topic that has admirably been investigated using heterogeneous samples. In the first case, a meta-analysis revealed a significantly smaller negative link between contact and prejudice for older adults, compared with college students (Pettigrew & Tropp, 2006). The authors reasonably suggested that perhaps mature adults are generally less likely to be affected by new experiences than college students. However, the difference in effect sizes ($rs = -.20$ and $-.23$ for older and younger samples, respectively) was undeniably small.

In the second case, researchers hypothesized that perceived importance of contact would mediate the relationship between contact and prejudice (van Dick et al., 2004). Their numerous samples included high school students from Germany and Costa Rica, undergraduate and graduate students from multiple disciplines, young men completing a civil service obligation, and older German adults. In contrast to Pettigrew and Tropp (2006), they did not find reliable differences as a function of participants' demographics. In the third case, a meta-analysis of simulated hiring decisions showed that young people (aged 17–29) were likely to discriminate against older job applicants, whereas older adults (aged 30–60) eschewed ageism (Finkelstein, Burke, & Nambury, 1995).

The fact that these meta-analyses returned different answers is not surprising, but it is remarkable that so few authors have even considered sample characteristics as a moderator variable. More often, authors tested for differences between experiments and surveys, but these results are uninformative for the question at hand because we have no guarantee that the latter included more diverse respondents. Surveys and experiments alike may have been primarily conducted using college students.

How Large Are the Differences Between Students and General Adults?

Unable to rely on meta-analytic findings for information, I turned to computing effect sizes (Cohen's d) for the data that Henry (this issue) provides. Henry observed significantly different scores for college students and general adults on a range of prejudice measures, but what is the magnitude of these effects? By convention, small, moderate, and large effect sizes correspond to .20, .50, and .80 (Cohen, 1988). Table 1 provides the results. I computed the effect sizes such that a negative sign reflects higher scores for older adults, compared with college students. As seen in column 1, the effects are generally small for the overall sample, suggesting that data from university students and general adults were reasonably similar. In particular, the modest size of the group difference effect for the feeling thermometers and symbolic racism ($Md = .27$)

Table 1. *Effect Sizes (Cohen's d) Comparing University and General Adult Samples*

	Overall Sample[a]	Black Participants[b]	Latino Participants[c]	Female Participants[d]
Feeling Thermometers				
Blacks	.27	.52	.11	.18
Latinos	.27			
Women	.23			
Symbolic Racism	–.29			
Conservatism	–.36			
Diversity Support	.44	.40	.96	
Inequality Support				
Less Equal Treatment	–.08	–.34	–.12	–.10
Equality Gone Too Far	–.30	–.49	–.53	–.35

Note. Data were taken from Henry's (this issue) Tables 3 and 4. Negative effect sizes indicate higher scores for general adult compared with university samples.
[a]Student $n = 277$, Adult $n = 430$. [b]Student $n = 28$, Adult $n = 124$. [c]Student $n = 39$, Adult $n = 54$. [d]$n = 376$ (separate ns not provided for women, but Henry [this issue] reports they did not differ by sample).

does not strongly support Henry's concern that "students [were] more adept at covering their underlying prejudices" (p. 55).

Perhaps the most unexpected finding in column 1 is that general adults were only somewhat more conservative than college students ($d = -.36$). If students are steeped in a "major bastion of liberalism" as Henry (this issue) states (p. 60), we might expect this difference to be larger. But I would caution us not to buy wholesale the stereotype that universities breed radical liberalism, perpetuated most recently by David Horowitz's campaign for an "academic bill of rights" and the related legislative initiatives it has provoked. Surveys reveal that although social science professors tend to skew liberal, particularly at elite institutions, professors as a whole reflect a broad political spectrum and the major trend over time has been an increase toward centrism, even in the social sciences (Gross & Simmons, 2007; Zipp & Fenwick, 2006). Moreover, on a variety of social issues (e.g., pertaining to equal opportunity), professors respond either similarly or more conservatively, compared with the general population (Gross & Simmons, 2007).

In contrast to the modest effects revealed by the overall sample, Table 1's column 2 shows differences between student and nonstudent Blacks that hover around the moderate range. Blacks in the general adult sample were more prejudiced than college student counterparts and showed less support for diversity in education. The comparable difference in support for diversity in education for student versus nonstudent Latinos yields the only large effect size in Table 1 ($d = .96$). Of course, education for students is a salient feature of their lives, and it is clear from the low numbers of minority students sampled that they are underrepresented at their institutions, which could account for their support for diversity in the schools. (A more telling item would have assessed support for diversity broadly writ.) Finally, the last column in Table 1 shows that female students and general adult women did not greatly differ in their responses; the largest effect size is nonetheless small ($d = -.35$).

In sum, the story that Table 1 tells is that students and general adults did not dramatically differ in their expressions of prejudice. The most compelling case stems from the data for ethnic minorities, for whom we continue to have scant information. One of the largest gaps in the prejudice literature concerns the relative paucity of information about minority members' prejudices and stereotypes due to our heavy reliance on (largely White) university samples (Judd, Park, Ryan, Brauer, & Kraus, 1995; Stanley, 1999). Investigations of ethnic minority members have predominately focused on the effects of being the target of prejudice rather than their biases. Although this situation has improved somewhat in recent years, it remains an embarrassment that should, on its own, spur us beyond college student samples.

Perhaps Henry's (this issue) most persuasive data stem from his Table 6, suggesting that university students justify their opposition to affirmative action using a variety of beliefs more so than general adults. However, computation of effect sizes again showed that these differences were generally small. For the overall sample, the ds ranged from .14 (for "conservatism") to .30 (for "unfairness beliefs"), yielding a small average effect size ($d = 21$). Women echoed these results, for whom the range in ds was .14 (for "conservatism") to .34 (for "unfairness beliefs"), again yielding a small average effect size ($d = .23$). For Blacks, the ds ranged from .24 (for "reverse racism") to .52 (for "unfairness beliefs"), but the average effect remained small ($d = .32$). Although some of these results are nontrivial, they do not condemn the use of student samples.

Nonetheless, who could argue against Henry's (this issue) contention that, at the very least, we should not *assume* that university samples reflect the general public? The greater argument he presents is that prejudice

researchers have largely neglected external validity in favor of tightly controlled experimental designs involving readily available student samples. Absent a wealth of meta-analytic data, we simply do not know whether this trade-off has compromised our theories, but we need to find out.

Explicit and Implicit Prejudice

Reactivity is an obstacle to studying prejudice that extends beyond students to include the general public. Henry (this issue) cites political correctness on campus as a reactivity trigger more pertinent to students, but the more robust cause is the illegality of discrimination. McConahay (1986) argued the need for the Modern Racism Scale (MRS) by noting that, following the passage of the Civil Rights Act of 1964, direct questions about race were often met with overt hostility by the general public and, as a result, interviewers "either openly refused to ask many of the old racism questions of anyone or somehow 'forgot' to ask them" (p. 94). The shift in attitudes toward prejudice, now judged as immoral, has presented a conundrum for researchers. According to general population surveys, prejudice against Blacks has become increasingly outdated (Judd et al., 1995; Schuman, Steeh, Bobo, & Krysan, 1997), yet behavioral data tell a different story, as does society at large (Crosby, Bromley, & Saxe, 1980; Eberhardt & Fiske, 1998; Gaertner & Dovidio, 2000; Saucier, Miller, & Doucet, 2005). The quest for trustworthy responses led to the bogus pipeline procedure and, much more popularly, to subtle instruments like the MRS. Yet McConahay recognized that the MRS was time stamped because people would begin to see through its subtlety as they became more sensitized to racism—a prophecy that soon came true (Fazio, Jackson, Dunton, & Williams, 1995). Measures such as the updated Symbolic Racism Scale are no doubt an improvement (Henry & Sears, 2002), but for how long? The lesson from the MRS suggests that even subtle self-report measures lose their advantage over time.

In my opinion, social psychologists met the challenge of resistance to reporting prejudice with extraordinary creativity. The best tools we have for prejudice assessment involve the response latency paradigm, which severely undermines people's ability to control their responses. And although there is healthy debate about what implicit measures assess, a meta-analysis of the Implicit Association Test (IAT) literature shows that implicit biases predict discriminatory behaviors better than self-report measures (Greenwald, Poehlman, Uhlmann, & Banaji, in press). Although the majority of this evidence came from student samples, recent studies have found the same pattern for adult samples. For example, Swedish employers' anti-Muslim IAT scores predicted their willingness to discriminate against Muslim job applicants (Agerström, Carlsson, & Rooth, 2007); American doctors completing their residencies at several hospitals made discriminatory health care recommendations to the extent that they favored Whites on the Black–White IAT (Green et al., 2007); and Australian nurses who were implicitly prejudiced against drug users were more willing to abdicate caring for them (von Hippel, Brener, & von Hippel, in press). In each investigation, self-report measures were ineffective as predictors of behavior. Henry (this issue) states that "the further removed from direct, negative attitude expression a construct is . . . the more powerful the construct may be in predicting intergroup attitudes for a university sample" (p. 60). Evidence from the IAT strongly supports that contention and extends it to the general adult population.

Or does it? Do implicit prejudice scores differ as a function of sample characteristics, including age, ethnicity, and political orientation—factors likely to distinguish students from general adults? To find out, I used data provided by Nosek et al. (2007) in their summary of more than 2.5 million IATs completed at the Project Implicit Web site (https://implicit.harvard.edu/implicit/) between 2000 and 2006. The authors computed mean effect sizes for prejudice IATs (and comparable self-reports) that averaged over nine topics, reflecting biases based on race, age, religion, disability, sexual orientation, and body weight. Similarly, implicit and explicit stereotype effect sizes were averaged over five topics, reflecting biases based on gender and race (Blacks, Asians, and Native Americans). Nosek et al. presented these effects as a function of age, ethnicity, political orientation, global region, and gender. To provide a sense of the variability, I computed d scores to reflect differences within each of these demographic variables, beginning with age (see Table 2). Reasoning that 30 years should allow for sufficient attitudinal maturity, I compared people in their 20s to those in their 50s; high scores reflect more prejudice on the part of older adults. As shown in column 1, the differences between average effect sizes for younger and older people were trivial for both implicit and explicit measures, suggesting that age has a negligible influence on intergroup biases (combined $Md = .05$). Not shown in Table 2, the largest age difference concerned explicit ageism, which was lower for older adults ($d = -.45$). The comparable difference for implicit ageism was small but in the same direction ($d = -.21$; see also Greenwald et al., 2002).

Next, I computed effect sizes to reflect the difference between Whites versus the mean of six other ethnicities (Blacks, Hispanics, Asians, Native Americans, Multiracial and Other). A positive score reflects more bias for Whites. As shown in Table 2's column 2, results yielded modest effect sizes for both implicit and explicit measures (combined $Md = .14$). However, this does not mean that Whites reflect the viewpoints of

Table 2. *Effect Sizes (Cohen's d) Reflecting Group Differences for IAT and Explicit Prejudice Scores as a Function of Demographics*

	Age[a]	Ethnicity[b]	Political Orientation[c]	Region[d]	Gender[e]
IAT differences					
Prejudice	.02	.10	.31	.02	.23
Stereotypes	.15	.20	.24	.10	−.03
Explicit differences					
Prejudice	−.03	.14	.52	−.02	.28
Stereotypes	−.08	.10	.39	.06	.10

Note. Effect sizes were computed from Nosek et al.'s (2007) Tables 3 to 7, summarizing data from more than 2.5 million IATs and self-reports completed at the Project Implicit Web site. Prejudice was averaged over nine topics, reflecting biases based on race, age, religion, disability, sexual orientation, and body weight. Stereotypes were averaged over five topics, reflecting biases based on race and gender. The *d* shown reflects the difference between the superscript items. Positive *d*s reflect higher scores for older adults, Whites, strong conservatives, Americans, and men, respectively.
[a]Adults in their 20s and 50s. [b]Whites versus the mean of seven other ethnicities. [c]Respondents who indicated they were either strong liberals or strong conservatives. [d]Americans versus the mean of six other nations. [e]Women and men.

minority group members. Not shown in Table 2, large differences were found between Whites and Blacks on the Black–White IAT ($d = .95$) and self-reported prejudice ($d = 1.48$). Substantial IAT differences were also shown between Whites and target group members about whom stereotypes were assessed (Asians and Native Americans; ds = .56 and .80, respectively). Although ingroup bias can account for these effects, the larger picture is more complex. Of importance, when target categories in the IAT compared Whites to outgroup minority group members (e.g., when Asians, Latinos, and Native Americans performed the Black–White IAT), non-Whites scored remarkably similarly to Whites (i.e., they showed pro-White bias). This pattern generalizes to other types of prejudice, such that high-status groups are automatically preferred over low-status groups on the part of people who belong to neither group, in support of system-justification theory (Rudman, Feinberg, & Fairchild, 2002). Thus, unless ingroup bias is invoked, ethnicity may not be a powerful predictor of implicit or explicit prejudice and stereotypes.

Given considerable evidence (reviewed by Henry, this issue) that conservatives tend to report more prejudice than liberals, they might be expected to also show stronger implicit biases. To provide the strongest test, I computed effect sizes to reflect greater prejudice for people who indicated they were *strong* conservatives, compared with people who indicated they were *strong* liberals. Table 2's column 3 shows that, on average, implicit biases were only somewhat larger for strong conservatives ($Md = .28$). Of the nine prejudice topics summarized by Nosek et al. (2007), only one showed a large implicit group difference, with strong conservatives scoring higher on the heterosexism IAT than strong liberals ($d = .98$). By contrast, explicit measures showed the expected difference; conservatives reported more prejudice than liberals ($Md = .52$). The gap between conservatives and liberals was particularly large for explicit heterosexism ($d = 1.49$) and prejudices based on religion ($Md = .77$). Thus, depending on the topic, university samples that skew liberal may yield underestimated levels of prejudice, but particularly when self-reports are used or heterosexism is the topic.

Another criticism of social psychologists concerns our heavy reliance on American samples, which represent a minute slice of the world's population (Norenzayan & Heine, 2005). To test whether the United States differs substantially from six other regions (Asia, Australia, Canada, Europe, and the United Kingdom), I computed effect sizes comparing the United States to the mean of other regions, such that high scores would reflect more biases found for Americans. Table 2's column 4 shows the results. As can be seen, the effect sizes were weak for both implicit and explicit biases (combined $Md = .05$). Not shown in Table 2, the largest difference found for IAT scores concerned Americans' stronger association of Whites with America than Native Americans, compared with Europeans' ($d = .47$). For explicit biases, the largest gap was shown between Americans' and Australians' race-weapon stereotype ($d = .30$). For the most part, prejudice and stereotype scores showed remarkable similarity across regions. Of course, whether differences would be larger for Americans compared with samples from other regions (e.g., Africa or the Middle East) cannot be known from these data.

Finally, I computed effects sizes based on gender differences, such that high scores reflect more bias on the part of men, compared with women. Table 2's column 5 shows very little difference ($Md = .15$). In fact, both genders showed similar evidence of sex stereotypes (contrasting career vs. family and math vs. the arts), on both IATs and self-reports (Mds = −.15 and .13, respectively). Thus, for these particular stereotypes, gender differences were trivial (but see Rudman, Greenwald, & McGhee, 2001).

In sum, evidence collected from the Project Implicit Web site suggests that implicit and explicit biases were remarkably similar across diverse groups, with two exceptions: (a) ethnic group differences ranged from moderate to large when ingroup bias played a role, and (b) political differences between strong liberals and strong conservatives were large when the topics were heterosexism (both explicit and implicit) or explicit religious biases. Nonetheless, the pattern shown in Table 2 reveals generally small-group differences, suggesting that university samples (largely young, White, and

American) may not be misdirecting researchers down ecologically invalid paths. However, there are at least three demographic variables missing from this analysis that warrant investigation. The first is socioeconomic status; Project Implicit respondents have access to the Internet and thus may be better off than people in general. The second is education; Project Implicit may over sample from educated populations, which would presumably yield low estimates of prejudice, particularly when using self-reports (cf. Brigham, 1975; Stangor, Sullivan, & Ford, 1991). The third is religion, which is too often overlooked by social psychologists, although it has been a constant source of geopolitical conflict throughout human history (Rozin, 2001).

Conclusion

It is not in our best interests to discount research evidence based on methodological biases but rather because of substantiated limitations. Henry's (in press) call for decreasing our dependency on university samples must be heeded, because we will not know how limiting our past habits have been until we systematically assess the situation. With the availability of the Internet and other resources Henry notes (e.g., the Knowledge Network research center), it is increasingly feasible for researchers to diversify their sample portfolios. Internet research affords an efficient means by which college students and general adults can be compared, and it is comforting when results are similar (e.g., Barry, 2001; Rudman & Phelan, 2007). Project Implicit provides a laudable example, and its cross-cultural reach (e.g., translations into multiple languages) continues to expand. But of course we need many more researchers moving beyond the confines of American university students by multiple means. As evidence from heterogeneous samples accrues, meta-analyses can begin to provide answers regarding if, when, and why specific conceptual frameworks are more (or less) applicable to university students. Speaking from my own experience, reviewers have become more appreciative of, and insistent on, tests of ecological validity than in the past. The days when topics such as work–family conflict, political ideology, or close relationships were studied using only undergraduates are steadily disappearing. The same pressure needs to be applied to investigations of prejudice and stereotypes. As the reward system changes so will the research, and that can only benefit our scientific health.

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Note

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