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Perseverance Furthers: Aims, Constraints, Effects, and Sympathy for Both Student and Nonstudent Sampling

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Having myself conducted both laboratory experiments with college students and descriptive surveys with representative samples, I both resist and endorse P. J. Henry's message, though of course I applaud his right to say it. At fundamental issue here are the contrasting aims of laboratory and field research. Their relative strengths each deserve our efforts at the same time that we admit their shortcomings. This commentary assumes that most laboratory studies use student samples and that most field research uses nonstudent samples. In turn, I address their respective aims, implied constraints, relative effect sizes, and my sympathy with P. J. Henry's aims.

What are Laboratory Studies Trying to Do?

At issue here are three distinct goals of psychological research. Laboratory studies are best designed to provide what philosophers call existence proofs. They demonstrate that a hypothesized process *can* occur in a manner consistent with a theory. As such, they are enormously useful to test the plausibility of a theory. As a result of laboratory studies, we know that the theory holds for at least some of the people some of the time. Most of the 600 or so theories in social psychology hold this status.

Laboratory studies are *not* designed to test generalizability. They are agnostic about the frequency with which the theorized processes occur. That's why the most salient theories invite tests in both lab and field settings. Often the people who test the generality of social psychological theory are political scientists or sociologists, two of our field's closest neighbors, and they often do this better than we do. They are happy to adopt our theories because we provide the psychological processes for their more macro investigations, themselves ill-suited to test psychological processes.

Nor are laboratory studies designed to test *utility* of a theory. They are silent on the issue of practical interventions, improving the world. Often the people who test the application of our theories are educational, organizational, and health psychologists because that is what they do best. We cannot all do everything. Theoretical physicists and engineers know this.

Do Laboratory Studies Constrain the Questions We Ask?

Of course researchers gravitate to questions and methods that interest them. Lecturing people about what ought to interest them never had much impact. A prominent social psychologist once described reading a persistent methodological critic as like taking medicine; he knew it was good for him, he dutifully swallowed it, but he did not change his habits. The researcher's interests come first, then the questions, and then the methods. We cannot impose the reverse order on people.

Anticipating this argument, P. J. Henry notes that a crucial issue such as blatant prejudice is not often studied in the laboratory. But it *is* often studied by political and sociological researchers. Of course, blatant prejudice is more conducive to representative sample

surveys, because by definition it consists of ugly biases that some people will actually admit verbally. By the same token, of course subtle prejudice is more conducive to nuanced laboratory experiments, precisely because it consists of unconscious, unexamined, automatic biases that can be detected only by delicate measures. So are the laboratory samples showing only artificially inflated results that would not hold up in the rough realities of the real world?

Which Shows Stronger Effects: Laboratory or Field Studies?

Meta-analyses answer this question. Because this article focuses on prejudice research, the Peterson meta-analysis of consumer behavior is only of marginal relevance. Of more relevance is the cited meta-analyses by Mullen and Hu (1989) on perceived ingroup and outgroup variability. This meta analysis shows stronger effects for real and enduring groups, with weaker effects for artificial groups. Similarly, other prejudice-related meta-analyses not cited also show stronger effects in real groups, compared to laboratory groups: Ostrom and Sedikides (1992) also on perceived homogeneity; Mullen, Brown, and Smith (1992) on ingroup bias; as well as Pettigrew and Tropp (2006) on the prejudice-reducing effects of contact. Sometimes real versus artificial groups make no difference to effect sizes (Mullen, Migdal, & Hewstone, 2001).

Thus, contrary to most people's intuitions, laboratory studies do *not* necessarily create a frictionless vacuum that exaggerates effect sizes. On the contrary, in the messy confounding of real-world settings, all the factors can freely press in the same direction, magnifying the effect sizes. Real people live in a world of consistent social forces.

As P. J. Henry suggests, many of the factors relevant to the expression of prejudice may be stronger in nonstudent samples, compared with student samples. The nonstudent samples indeed are likely more conservative on average. This does not mean that they are the only people to study. College students at our selective universities are more likely to be running business and government, hence impacting institutional practice.

Sympathy with Henry's Aims

The article provides some helpful data on the use of student and nonstudent samples, though one might wish that the comparisons had shown chronological trends rather than dichotomous comparisons. And it is not surprising that applied journals publish more nonstudent research. But still, the record is useful, and the article will provide other frustrated nonstudent samplers with some ammunition for editors.

Frustration indeed is palpable throughout the article, and all of us who have published using nonstudent data can sympathize. Decades ago, one of my early jointly-authored submissions to the *Journal of Personality and Social Psychology* came back with a negative review (an experience shared by 90% of all submissions, not exactly unique to survey research!). But in our case, our first author, Robert Abelson, was not going to take no for an answer, at least not easily, so he fired back a letter to the editor, claiming prejudice against survey research and explaining why our paper made a theoretical contribution. The editor reconsidered, and that published paper is still frequently cited as a core contributor to the notion that affect may determine behavior more strongly than does cognition (Abelson, Kinder, Peters, & Fiske, 1982). Perseverance furthers.

In our current program of research on stereotype contents, we sample college students, convenience samples of adults, random samples of Americans, and convenience samples in other cultures. Each time we have submitted a nonstudent or nonAmerican sample, we have had to make it abundantly clear to the editor and reviewers that our study answered

theoretical questions. For example, in first moving to a nonstudent sample (Fiske, Cuddy, Glick, & Xu, 2002), we argued that students' liberal biases might cause them to rate many groups ambivalently, to demonstrate at least some positivity toward outgroups (high on one dimension, even if low on the other); the nonstudent sample replicated the student one. In moving to a representative American sample (Cuddy, Fiske, & Glick, 2007), besides generalizing, we argued that this context was most diagnostic for path analyses linking stereotypes and emotional prejudices to behavior. And it was. In moving to other cultures (Cuddy et al., in press), we made a case that more collectivist cultures might show theoretically predictable and distinct differences from Western samples. They did, though it replicated in most respects.

Student/nonstudent differences remain an empirical question, but they must be framed in specific theoretical contexts, to convince editors and reviewers. Perseverance furthers.

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