

remember that the underlying causes and nature of systemic issues such as discrimination and inequality cannot be reduced to (or sufficiently captured within) experiments alone. Rather, these realities – and the questions they raise – need to be explored within the worlds that give rise to them (Oishi & Graham, 2010; Trawalter, Bart-Plange, & Hoffman, 2020). Here, qualitative methods are often particularly valuable by virtue of their inductive, reflexive, and phenomenological potential. Critically too, these alternative (and complementary) methodologies are better able to capture the meaning of data in situ and prioritize community participation in the co-creation of knowledge – something which is all too often missing in experimental research (Burman, 1997).

In sum, as with a good breakfast, experiments are an excellent point of departure. But on their own, they can never be enough to satisfy our scientific appetites. For their scientific potential to be fulfilled, their contributions need to be consolidated with meaningful theory development and complementary methodologies. Lacking this, not only will our diet be unbalanced, but it will also be profoundly unsatisfying – and potentially harmful.

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What can the implicit social cognition literature teach us about implicit social cognition?

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Abstract

We highlight several sets of findings from the past decade elucidating the relationship between implicit social cognition and real-world inequality: Studies focusing on practical ramifications of implicit social cognition in applied contexts, the relationship between implicit social cognition and consequential real-world outcomes at the level of individuals and geographic units, and convergence between individual-level and corpus-based measures of implicit bias.

The target article calls for “systematically dismantling” the “fundamentally flawed” practice of using implicit social cognition research to inform our understanding of real-world inequality. Sweeping conclusions and comprehensive recommendations of this kind, published in a leading journal of our discipline, should be supported by powerful arguments reflecting the latest state of the literature. Instead, the target article mischaracterizes the methods, goals, and state of implicit social cognition research while referencing a mere eight empirical papers, the most recent of which was published over a decade ago.

According to the target article, the “standard research cycle” begins with the observation that groups differ on some real-world outcome and has the goal of explaining, and eventually eliminating, such differences. This statement is misleadingly narrow. Not all memory research seeks to cure dementia; not all phonological awareness research tries to eradicate dyslexia; and not all auditory perception research contributes to the development of hearing aids. Similarly, much implicit social cognition research explores basic aspects of thought and behavior, including learning and representation (Kurdi & Dunham, 2020), social cognitive development (Dunham, Baron, & Banaji, 2008), and cultural change (Charlesworth & Banaji, 2019), without making any claim of immediate applicability to real-world problems. Thus, whether implicit social cognition research can explain real-world inequality should not be treated as its sole measure of success.

Of course, some of this literature does speak to real-world outcomes and behaviors. But here too the target article misses the mark. Specifically, according to the target article, researchers establish some experimental effect of social category knowledge in a small sample of naïve undergraduate participants in the lab and, without any further ado, conclude that the processes observed in the lab directly explain real-world disparities. In fact, as discussed below, much recent implicit social cognition research does not bear much resemblance to this description.

One relevant line of research has documented practical ramifications of basic implicit cognitive processes. For instance, transgender and cisgender children have been shown not to meaningfully differ from each other in implicit gender identity (Olson, Key, & Eaton, 2015), thus providing a counterweight to prior claims of “psychological deviance.” In other cases, changes in implicit social cognition have been shown to track meaningful experiences in field settings: For example, exposure to female college professors in science, technology, engineering, and mathematics (STEM) fields can produce long-term effects on implicit gender stereotypes and self-concept (Dasgupta & Asgari, 2004), implying that the social structures in which we are embedded shape the ways in which we envision our future possibilities.

Other research has investigated the relationship between implicit measures and ecologically meaningful measures of intergroup behavior (Kurdi et al., 2019b). For example, implicit math-gender stereotypes predict actual academic achievement among high school students (Steffens, Jelenec, & Noack, 2010); implicit weight stereotypes predict actual callbacks of job applicants among human resources professionals (Agerström & Rooth, 2011); managers' implicit competence stereotypes predict actual job performance of their minority employees (Glover, Pallais, & Pariente, 2017); and doctors' implicit evaluations predict actual rapport, satisfaction, and treatment adherence among Black patients (Hagiwara et al., 2013; Penner et al., 2016, 2010).

Echoing an oft-repeated argument, the target article hastens to underscore that studies of predictive validity produce small correlations between implicit attitudes and intergroup behavior. The finding that the relationship between explicit attitudes and intergroup behavior is almost exactly the same size (Kurdi et al., 2019b) receives no mention. What's more, the mean implicit-behavior correlation sits right around the 25th percentile of all effect sizes in social psychology, with the largest implicit-behavior correlations at the individual level approaching the 70th percentile of that distribution (Lovakov & Agadullina, 2021).

Equally absent is any discussion of studies that investigate the association between implicit cognition and real-world inequality at the level of geographic units, which have produced large effects in multiple domains (Hehman, Calanchini, Flake, & Leitner, 2019; Payne, Vuletich, & Lundberg, 2017). For example, this work has demonstrated that regions with higher levels of implicit race bias are characterized by more frequent police killings of Black Americans (Hehman, Flake, & Calanchini, 2018), as well as more racial disparity in school disciplinary actions (Riddle & Sinclair, 2019) and upward mobility (Chetty, Hendren, Jones, & Porter, 2020).

Finally, remarkable correspondence has also been found between individual-level conceptual associations indexed by implicit measures and cultural-level conceptual associations computationally derived from vast amounts of text produced spontaneously and outside any experimental setting (Caliskan & Lewis, 2020). Evidence for such alignment has been provided across different contexts, including a comprehensive examination of social group attitudes and stereotypes (Caliskan, Bryson, & Narayanan, 2017), the relationship between implicit beliefs and evaluations (Kurdi, Mann, Charlesworth, & Banaji, 2019a), and the development of gender biases over the lifespan (Charlesworth, Yang, Mann, Kurdi, & Banaji, 2021).

Little, if any, of the criticism formulated in the target article seems applicable to methodologically sound implicit social cognition research conducted over the past decade. Far from simply assuming a one-to-one correspondence between findings obtained with small undergraduate samples in artificial lab settings and real-world inequality, an increasingly large group of investigators have made serious efforts to establish connections between implicit measures of social cognition and group-based disparities. Specifically, all of the studies discussed above include at least one (but typically all) of the following elements: samples consisting of experts or members of the general public; real behaviors of consequence observed under ecologically realistic conditions; and the availability of ample individuating information during the decision-making process.

Implicit social cognition research has obviously not been immune to some of the same methodological missteps that have troubled much of psychology and the behavioral sciences over

the past few decades. However, as should be clear based on even this brief review, there is considerable reason for optimism. Most importantly, further improvement and innovation won't be fueled by throwing out the baby with the bathwater. Instead, whether the goal is basic science or uncovering the antecedents, mechanisms, and consequences of real-world inequality, we urge renewed focus on theory building, study design, and statistical inference. And accurately characterizing the field that one critiques.

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The unbearable limitations of solo science: Team science as a path for more rigorous and relevant research

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Abstract

Both early social psychologists and the modern, interdisciplinary scientific community have advocated for diverse team science. We echo this call and describe three common pitfalls of solo science illustrated by the target article. We discuss how a collaborative and inclusive approach to science can both help researchers avoid these pitfalls and pave the way for more rigorous and relevant research.

In 1946, Lewin wrote about the importance of conducting “action research” that could improve intergroup relations. Lewin and his contemporaries recognized that to do action research well, psychologists could not work alone. To do so would limit their ability to answer three critical questions regarding the phenomenon under study: “(1) What is the present situation? (2) What are

the dangers? (3) And most important of all, what shall we do?” (Lewin, 1946, p. 34). They learned that rigorous and relevant social psychological research requires collaborating not only with scientists in other disciplines to understand the full range of forces acting upon a person in a social system, but also with community partners, governments, and other local stakeholders who have direct access to information and insights about how those forces operate in the specific context at hand (IJzerman et al., 2020). Indeed, a growing consensus across disciplines recognizes the value of a collaborative, multidisciplinary, and inclusive approach to science (Albornoz, Posada, Okune, Hillyer, & Chan, 2017; Disis & Slattery, 2010; Ledgerwood et al., 2021; Murphy et al., 2020).

The importance of a collaborative approach was well-known in the early days of psychology but has been neglected in the modern era (Cialdini, 2009). Neglecting the true *powers of the situation* the cultural, economic, historical, political, and sociological forces that affect the mind (including the minds of psychologists) limits the rigor and relevance of the discipline’s research, and hampers psychologists’ ability to truly understand the conditions under which our work is or is not relevant for social issues.

In his target article, Cesario discusses challenges he perceives in social psychological experiments on bias, and concludes that we should abandon such experiments. While we agree that many experiments have flaws, our view is that Cesario’s own critique suffers from three flaws that render his conclusion premature (Table 1). We further suggest that these flaws could have been avoided by collaborating with multidisciplinary experts or even experts in other areas of psychology.

The first flaw is the *biased search flaw*: When people’s expectations lead them to consider an incomplete set of possibilities or to search through available information in a manner shaped by personal expectations (Cameron & Trope, 2004). This flaw is costly because it leads to mistaken conclusions based on an incomplete survey of possible alternatives. For example, the target article correctly notes that effect sizes depend on the paradigm used to study them (Kennedy, Simpson, & Gelman, 2019; McShane & Böckenholt, 2014). However, it discusses only the possibility that effect sizes observed in the lab would diminish in the world, and omits the possibility that they would be magnified. After all, in the real world, effects of discrimination compound over time (Krieger & Sidney, 1996; Mays, Cochran, & Barnes, 2007); small effects can become large when compounded across many decisions (Funder & Ozer, 2019). Similarly, although lab studies typically only manipulate a single dimension of bias, in the world, dimensions of bias can intersect to produce compounded or unique effects (Berdahl & Moore, 2006; Remedios & Sanchez, 2018; Settles & Buchanan, 2014). Moreover, research suggests that biases can be magnified when people have access to rich information (as in the real world) that can be marshaled to elaborate and rationalize initial expectations (Darley & Gross, 1983; Taber & Lodge, 2006).

The second flaw is the *beginner’s bubble flaw*: when people know a little about a topic but overestimate how well they understand it (Sanchez & Dunning, 2018). This flaw is costly because it leads scholars to misapply or miss insights developed in other areas. For example, the target article relies heavily on the idea that in the real world, people use information that “may be probabilistically accurate in everyday life” (sect. 5, para. 7) and that using demographic information (e.g., race) to fill in the blanks when full information is unavailable is rational in a Bayesian sense and therefore unbiased. This vague and imprecise assertion muddies waters that