

FORUM

What Good Is Anthropology? Celebrating 50 Years of *American Ethnologist*

Ethnography vs. zombie methodologies

What anthropology can teach psychology about nonreproducibility

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Abstract

Beginning in 2011, public scandals and high-visibility critiques of research methods in psychology fed a broader “replication crisis”: foundational experiments could not be replicated, and statistical methods in social psychology demonstrated vulnerability to fraud and manipulation. Even well-intended researchers following accepted psychological protocols—zombie methodologies—could unintentionally produce false positives. In response, social psychologists have called for greater sensitivity to cultural diversity, a deeper consideration of social context, and more methodological reflection. The contrast with anthropology is dramatic, highlighting some of the strengths of our field: methodological versatility, appreciation of human variability, theoretical creativity, and a solid foundation for synthetic, interdisciplinary collaboration grounded in our tradition of holism. The human sciences are an important audience for anthropologists, as the example of cognitive science shows.

KEYWORDS

anthropological holism, anthropological methods, cultural diversity, human variation, integrative pluralism, replication crisis, zombie methodology

From my perspective as a neuroanthropologist, some of the best things about anthropology are thrown into high relief by comparing it to related disciplines, especially the human sciences. This commentary discusses developments in psychology since 2010, especially a series of high-profile scandals and the field’s broader replication crisis. I do this to better assess what anthropology offers as a discipline. Although anthropologists suffer collective angst, neighboring disciplines seem mired in more grievous intellectual problems, including some that our field has years of experience in constructively addressing.

Precisely owing to disciplinary self-scrutiny and internal debate, we have developed diverse research methods and maintained greater openness to alternative thinking, a kind of epistemological unsettledness that protects us from some (though not all) hubris. The intensity of our internal theoretical disagreement, including a complex history of contending biological, social, cognitive, and cultural approaches, lends anthropology a vitality that some disciplinary rivals in the human sciences lack. Moreover, it has provided us resources

that we could share with a broader audience, beyond anthropology. Although large parts of the human sciences seem immune to our critique and closed to collaboration, anthropologists have already made significant contributions—the example of cognitive science comes to mind. These successes demonstrate ways we might reach psychologists and the multiplier effect that those successes can have. Anthropologists, however, can explore these avenues for influence only if we break some of our self-limiting habits, such as communication strategies that keep our work confined in a narrow disciplinary silo.

This commentary does not seek to paper over problems in our discipline. We have plenty of work to do (Gupta & Stoolman, 2022). The last few years have exposed a host of issues anew in anthropology: racism, entrenched inequity (including in hiring), systematic abuse by central figures (and academic protection rackets that enable abuse), corporatization of universities, precarious work conditions, ongoing patterns of colonialism and intellectual extractivism, and other injustices (Jobson, 2020). But alongside a clear-eyed appraisal of our

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problems, recognizing what we do well, especially in contrast to other disciplines' limitations, can reinforce our commitment to doing better anthropology. This commentary starts with a brief account of the storms roiling psychology before discussing how these problems highlight the strengths of anthropology and opportunities to broaden the influence of our research.

BIG TROUBLE IN PSYCHOLOGY

In psychology, a series of epistemological crises and ethical conflagrations, together with the collapse of methodological groupthink, has shaken one of our most excessively confident disciplinary rivals. Although psychology still is a dominant field in many ways—determining research priorities and practices in social service, mental health, and education—many prominent practitioners are calling internally for reform or radical change. Problems had been simmering for decades when, in 2011, multiple scandals and revelations exploded into public view, especially in social psychology. That year, a leading journal published dubious research on precognition, an internationally influential researcher was found responsible for decades of fraud, and a widely discussed article argued that “questionable research practices,” including statistical manipulation, were widespread even beyond psychology, inflating the rate of false positive results (Wiggins & Christopherson, 2019).

First, the *Journal of Personality and Social Psychology* (*JPSP*) notoriously published Cornell social psychologist Daryl Bem's (2011) article “Feeling the Future: Experimental Evidence for Anomalous Retroactive Influences on Cognition and Affect.” Bem's article allegedly presented evidence for precognition, or “psi,” which he has been exploring since the 1990s. Critics were scandalized that a leading journal in the field would publish research with such a dubious premise, yet the journal's editors stood by their review process and decision (Wiggins & Christopherson, 2019, p. 204). Other researchers complained and sought to publish unsuccessful replications of Bem's experiments—the mechanism that proponents said was appropriate to disprove flawed research. The editors of *JPSP*, however, argued that failures to replicate were insufficiently novel or important to merit publication. Original findings, however defective, even implausible, were more valuable than the exercise of corrective scholarship. The episode made many psychologists fear that scientific safeguards were much weaker than they had hoped (Earp & Trafimow, 2015; Pashler & Harris, 2012).

The second event was the exposure of fraud committed by influential Dutch social psychologist Diedrik Stapel, who frequently appeared in the media discussing his research, which included studies on how untidy environments encourage racism. Three of Stapel's graduate students noticed that their results were too perfect for the claims they were making and began asking questions that led to uncovering “years of data manipulation and blatant fabrication” (Callaway, 2011; see also Stroebe et al., 2012). Multiple papers were retracted in some of the field's leading journals (Levelt Committee et al., 2012). In retrospect, Stapel's ability to quickly produce data supporting his hypotheses was suspicious: “Effects were large; missing

data and outliers were rare; and hypotheses were rarely refuted” (Callaway, 2011, p. 15).

This case of scientific misconduct provoked worldwide popular condemnation (Stroebe et al., 2012), but it was hardly isolated, nor were the problems confined to social psychology, as demonstrated by disgraced evolutionary biologist and psychologist Marc Hauser, developmental psychologist Johnny Matson, cognitive psychologist Lorenza Colzato, behavioral economist and psychologist Dan Ariely, and behavioral scientist and business professor Francesca Gino (the latter is suing her employer, Harvard Business School, for its response, which included placing her on unpaid administrative leave, stripping her of her endowed faculty title, barring her from campus, and reviewing her tenure). The exposures of Stapel and others pointed to entrenched problems in psychology's incentive structure, publishing environment, professional standards, and intellectual climate.

Finally, 2011 saw the publication by psychologists Joseph Simmons, Lief Nelson, and Uri Simonsohn of a demonstration of how “degrees of freedom” in statistical methods generally accepted across psychology allowed researchers to engage in questionable research practices (Simmons et al., 2011, p. 1359). The team argued that these forms of data manipulation were likely producing an epidemic of false positive results, even in cases in which researchers were not intentionally fraudulent or pursuing fishy parapsychology projects (see also Masicampo & Lalande, 2012). The field simply had coalesced around statistical methods that were poorly understood by most practitioners, methods virtually certain to produce unsupported analysis. To highlight the risks, Simmons and colleagues used these widely accepted practices to generate statistically significant evidence that listening to the Beatles' song “When I'm Sixty-Four” could lead to “chronical rejuvenation”—a reduction in age of a year and a half—relative to a control song, “Kalimba” (Simmons et al., 2011, p. 1360). The authors provided computer simulations to demonstrate that, using widely accepted data handling techniques—like choosing among multiple dependent variables, adjusting sample size, and selectively carving subsets of a primary sample—they could generate a high percentage of false positive results.

In the following years these suspicions were confirmed by large-scale efforts to replicate a range of psychology experiments; researchers could not replicate a significant percentage of findings (Peters & Lemeire, 2023). For example, the Reproducibility Project (OSC, 2015), a massive effort led by psychologist Brian Nosek with 269 collaborators, tried to replicate 100 experiments reported on in leading psychology journals in 2008. Only 39 percent of the findings could be replicated, and many “successful” reproductions showed weaker effects than in the originals (Nosek et al., 2022). If, as philosopher Karl Popper (1959, p. 45) argued, falsifiability demarcates science from nonscience, then psychology (along with other fields undergoing parallel “replication crises”) faced profound questions about claims to privileged epistemological ground as a “science.”

Wary psychologists had anticipated these concerns. Psychologist Robert Rosenthal (1979) had warned about the “file drawer” problem, in which the value placed on positive results

leads failed studies or negative experimental outcomes to wind up left in a drawer (see also Francis, 2012). Surprising results were more likely to get published in prestigious journals, creating a bias toward poorly designed research that generated counterintuitive or spectacular results (Koole & Lakens, 2012). For decades, critics warned that researchers poorly understood statistical techniques in wide use and experiments employed sample sizes too small and easy to manipulate, especially in areas where many researchers chased statistical significance (Gigerenzer, 2004; Ioannidis, 2005).

Other psychologists had repeatedly warned that results were also biased because of the prevalence of convenience samples: principally undergraduates taking introductory psychology courses. Psychologist Jeffrey Arnett (2008) pointed out that researchers' excessive focus on North American university students meant that 95 percent of the world's population was understudied (see also Rozin, 2001). Joseph Henrich, Steven Heine, and Ara Norenzayan elaborated this critique in a widely cited paper on "WEIRD psychology" (Henrich et al., 2010). They laid out in extensive detail that this convenience sample of "Western, educated, industrialized, rich, and democratic" subjects was, by virtually all available comparative measures, a psychological outlier. University students were statistically anomalous, not defensible stand-ins for universal human proclivities in areas as diverse as visual perception, moral reasoning, and economic behavior (see also Barrett, 2020a; Rad et al., 2018).

This problem also afflicted experimental philosophy, which uses psychological methods to address philosophical questions. As critics have noted, the field is full of "hasty generalizations" from culturally limited samples (Peters & Lemeire, 2023). No matter the evidence base, the broader a paper's conclusions, the more likely it was to be cited, suggesting that unwarranted generalization was not just an incidental problem but positively incentivized. Despite these and other warnings, psychologists had persisted in a kind of convenience theory: lazy, unexamined universalisms about "human nature" permitted sweeping conclusions from tiny samples and a publishing pace that offered no time for critical scrutiny of methods or theoretical reflection.

One reason for these recurring problems was the outsized role that methodological orthodoxy played in professional cohesion in psychology. The field was united, psychologist Kurt Danziger (1990) argued, not by rigorous theoretical debate or a shared set of overarching questions about the nature of humanity, but by methodological commitments, the very techniques and professional standards exposed on multiple fronts by the replication crisis (Wiggins & Christopherson, 2019, p. 204; see also Muthukrishna & Henrich, 2019). Using statistical methods they do not fully understand, researchers often run slight variations on previous experiments. This pattern is what I would call "zombie methodologies": forms of inquiry that march on with minimal reflection, though mortally wounded by scandal and starved for theory. The overall result of all this turmoil is a profound crisis of confidence in psychology and allied fields beset by replicability problems or a state of denial (Pashler & Wagenmakers, 2012).

Psychology and economics have emerged as the privileged, authoritative, "scientific" discourses on human nature, not

just in government, health, business, and education, but also in the eyes of a wider public. In many discursive spheres, psychology is privileged over other equally valid academic disciplines, even though most psychological experiments are quite narrow—conducted in rarefied and unnatural laboratory conditions, using experimental protocols that often lack ecological authenticity. Any analysis of the positions of psychology and economics, then, must acknowledge that their dominance inside and outside academia owes in large part to their complicity in bourgeois ideological reproduction, compatibility with Western capitalism, and usefulness for social management. Psychology, like economics, provides a smartly dressed rationalization of Western—or more narrowly, North American—common sense.

Neoliberal ideology dressed up as "science," however, is vulnerable to empirical critique in a way not possible in a purely political sphere. For example, when psychologists' papers blithely assume that bourgeois acquisitiveness is universal "human nature," evidence sometimes (though not always) matters. Scientific claims can be falsified, a strategy not available in every discursive realm, which is one reason some of the worst offenders from psychology publish so much for popular audiences. Especially in Western countries like the US and Australia, where any progressive perspective or program can be dismissed as "socialist" or "Marxist," we should welcome the opportunity to contend these issues intellectually. Hegemonic assumptions about human nature are already being exposed as radically unfounded, not only by anthropologists but also by psychologists in widely cited research.

For instance, although some undergraduate psychology students, after discussing the overwhelming Western bias in sampling or the replication crisis, want to continue as if nothing is amiss, many are deeply disturbed and seek alternatives. According to a colleague who teaches a large service course in human sciences at my Australian home university, students had two reactions to the "WEIRD" critique of psychology: about half of them "buried their heads in the sand," while the other half wanted to "burn the place down." In recent years, I have been approached by an increasing number of psychology and cognitive science graduate students who want to discuss "decolonizing" their methods, engaging in cross-cultural research, learning ethnographic and qualitative approaches, and reforming their disciplines.

ANTHROPOLOGICAL STRENGTHS REFLECTED BY PSYCHOLOGY

This account of events in psychology is not simply an opportunity to indulge in cross-disciplinary *schadenfreude*. The profound problems in that field present a chance for anthropologists to share our contributions more widely with this academic audience and beyond them, with the general public, which often turns to psychology with questions we are better positioned to answer. In light of controversies in human sciences, anthropology's strengths stand out more clearly.

First, whereas psychology has suffered from an overly narrow methodological orthodoxy and its practitioners have used statistical tools that many of them did not fully

understand, anthropology has remained both creative and self-critical about its research methods. There is no professional consensus about what constitutes good ethnographic fieldwork. Although this leaves us with little agreement about professional standards, it also opens the research design space, nurtures innovation, and requires ethnographers to constantly confront methodological scrutiny. Visual anthropology, oral history, observational research, open-ended interviews, apprenticeship-based inquiry, collaborative action research, community-based ethnography, archival methods, cognitive data collection, digital ethnography ... it is hard to even track the variety of methods widely used in our field. Heterogeneity guards against methodological complacency. Perhaps more importantly, this methodological vibrancy and openness mean that our methods do not constrain our research questions, unlike zombie methodologies that live on beyond their usefulness, hungry for an intellectual agenda, yielding endless minor variations on accepted protocols, churning out publication with little insight.

Second, the replication crisis emphasized the need to consider cultural variation in psychology and allied fields. This was not the first time psychologists confronted the fragility of claims about universal human nature, but it was one of the most dramatic, and it led to many discussions of human cultural variation (e.g., Amir & McAuliffe, 2020; Barrett, 2020b; Hruschka et al., 2018; Rad et al., 2018; Roberts & Mortenson, 2023). The danger is that some approaches in human sciences use obsolete models of culture, often reified as shared information in the head, as isolated traits to be compared across cultures, or as the difference between mean performance in two populations on psychometric tests (Lende & Downey, 2020).

The emerging interest in cultural variation is often *ethnological* rather than *ethnographic*, a superficial practice of broadly comparing isolated traits reminiscent of earlier generations of armchair anthropology and outmoded cultural evolutionism. This ethnologically styled cross-cultural research lacks the depth of engagement that anthropologists see as a precondition for cultural understanding, trusting that cultural practices, responses to surveys, or experimental effects can be decontextualized without significant loss. The result is something like behavioral economics or cultural evolutionary theory, which behave as if they recently invented the concept of “culture.” We should seek to encourage an anthropological appetite for cultural variation and a more profound recognition of context dependency rather than allowing colleagues in the human sciences to think that shallow sampling of decontextualized culture traits redresses all their problems with sample bias or their hangover from assuming a universal human nature. My fear is not that these researchers will ignore variation; I fear they will act as if it were recently discovered and reinvent some of the most obsolete dead ends our field explored in decades long past.

Third, as psychologists like Christopher Green (2019) have argued, the replication crisis is not just methodological; the disproportionate emphasis put on achieving statistical significance meant testing limited, very narrow hypotheses. This blinkered approach generated a discipline without strong incentives to develop overarching theories that would account for experimental effects or produce more accurate predictions (see also Muthukrishna & Henrich, 2019). A psychologist could build a

successful career on the back of null hypothesis testing without ever needing to propose a more substantial theory, joining the shambling crowd of their colleagues infected with zombie methodologies, not knowing their destination.

Anthropology was once a hothouse of theory production, disproportionately influential given our size and prominence, and we could be again. Although anthropologists continue to place a high value on theoretical innovation, we are probably importing more theoretical work from fields like cultural studies, literature, and philosophy rather than exporting innovations (including to the human sciences). This suggests that we need to write in ways that external audiences can appreciate, understand, and use.

The late Marxist anthropologist William Roseberry (1996, pp. 19–21) warned about “shrinking” theoretical diversity in our field brought about by the conjunction of reduced opportunities in academic employment and monopolies on positions available by graduates of elite institutions (see also Carrier, 2016; Hann, 2009). Roseberry was worried about decreasing interest in political economy and the rejection of structural analysis, but the same could be said today about psychologically adjacent anthropology. Encouraging more anthropologists to write to influence the human sciences would require lifting the “Geertzian foreclosure” on psychological-level theory in our field (Molino, 2004, p. 22; see also Downey, 2023), but it could also help spur our theoretical ambitions and discourage intradisciplinary involution (Bunzl, 2008). If we write only for each other, ferociously engaging our internecine intellectual skirmishes touched off especially since the publication of *Writing Culture* (Clifford & Marcus, 1986), we risk producing obscure texts with severely limited reach.

Cognitive science is a success story for anthropologists, to some degree; anthropology was foundational in cognitive science, although our discipline absented itself as the field developed (Bender et al., 2010; Boden, 2008). Nevertheless, two of the most compelling theories currently debated there—accounts of “distributed” and “embodied” cognition—were inspired by anthropological theorists, and both are vital to “4E” cognitive science, so named for its focus on embodied, embedded, enacted, and extended forms of cognition (see Bender, 2019). Cognitive anthropologist Edwin Hutchins’s (1995) careful analysis of collaborative navigation onboard a naval vessel, and how social interactions and material technologies were taken up into cognitive processes, became a model for the “extended” mind. Likewise, anthropologist Tim Ingold’s (2000) work on embodied cognition is a crucial contribution to work in interdisciplinary fields that examine skill acquisition, sensory learning, and artificial intelligence.

Our ability to interact with the human sciences has been undermined by the acrimonious divorce between the scientific and humanist wings of anthropology and the marginalization of psychological approaches in our field. Many anthropologists, in fact, are uninterested in reaching colleagues outside the discipline. When I go to talks in human sciences about culture or human variation, I frequently find that nonanthropologist admirers of our field have gotten their hands on obsolete anthropological research. They grab outdated work written accessibly by one of our disciplinary predecessors or—worse—a popular

book by a dabbler from evolutionary psychology or ornithology who pretends to interpret anthropologists for the public. Many times, I rue the fact that more of us do not write accessibly to communicate anthropological theory with this audience: the theorists and researchers in human sciences who are open to what we share but who are unfamiliar with our jargon or uninterested in our intradisciplinary squabbles. We tend to divide our writing between academic and popular, but cross-disciplinary academic writing is its own genre, one that some of us do very well but that more of us should attempt.

Fourth, holism and the four-field tradition in US-based anthropology are both controversial within our discipline. Some of our colleagues call for one or another of the subdisciplines to liberate itself from the others (e.g., Segal & Yanagisako, 2005). But those who still aspire to holism and synthetic approaches find that some of the complex work we need to do is intradisciplinary to a degree impossible in other fields (e.g., Cabana et al., 2022). As anthropologist Rena Lederman (2005, p. 73) notes,

Fusion or fission are not our only options. Over its history, “four-field anthropology” as a pragmatically institutionalized discipline has produced humanist/scientist hybrids of various sorts; it has been the ambivalent guardian less of a “sacred bundle” than of a rare nesting ground—a condition of possibility—harboring anti-essentializing evolutionists, hermeneutic realists, and other third kinds.

The “condition of possibility” to produce hybrid bio-cultural, psychosocial, and cognitive-neurological approaches from within a single discipline positions anthropologists to embrace the opportunities emerging from fields like integrative cognitive science or postgenomic biology (e.g., Hutchins, 2014; Ingold & Palsson, 2013; Lende & Downey, 2012; Lock, 2017; Seeberg et al., 2020).

Holism teaches us to operate at multiple scales, with divergent ontologies and partial causal explanations, setting us up well for what philosopher Sandra Mitchell (2009, p. 2) has called “integrative pluralism”: approaches that combine more traditional reductive epistemologies with multilevel forms of analysis that better acknowledge context and complexity. More poetically, anthropologist Bruce Knauft (2006, p. 408) writes about the “braiding together of different approaches and perspectives like strands of a rope configured specially for a new topic, issue, or critical intervention.” Ironically, the collapse of traditional, a-culture-between-two-covers ethnographic writing has accustomed many of us to tracing processes across sites and scales, living with explanatory incompleteness, and sketching the outlines of complex mechanisms that link environment, society, or culture to biological or psychological outcomes.

Holism teaches explanatory humility: we must become comfortable with “good enough” mixed-method exploration, hold open the door for collaborators to add missing explanatory elements, and juggle shifting hybrid theoretical frames. If this work is to be ecologically and contextually rich, social and psychological, a hybrid of science and humanity, we must be

willing to let contending explanations sit side by side, giving all their due. These habits serve anthropology well and provide a resource that we offer to colleagues who are not equally prepared for the upheaval produced by research on complex psychological, neurological, and ecological systems.

When he accepted a prize in 2009, my former undergraduate adviser Richard Handler repeated an expression he had heard from his undergraduate professor, Robert Murphy (and which I had heard Richard say when I was his student): anthropology is “an intellectual poaching license” (Handler, 2010, p. 77). We could borrow from many other fields and range widely in search of inspiration for research design, analytical terms, or theoretical explanations. I have always loved this about our field: the internal diversity and the fact that we have taken insights and inspiration from many places—in my case, from neurosciences, psychology, and cognitive science—and traffic comfortably with integrative pluralism. In contrast, psychology achieved a kind of methodological unanimity, but at the expense of being so homogeneous and epistemologically uncurious, an intellectual gated reserve, that it was vulnerable to precisely the explanatory overreach that the “replication crisis” illustrates. In contrast, the messiness and lack of consensus in anthropology, together with our porous borders, are a source of our intellectual strength, but we too often seem content with talking to each other instead of transmitting our distinctive insights to a broader intellectual influence. When we go poaching, maybe we can pick off some of the zombies.

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