The Replication Crisis: How Might Philosophy and Theory of Psychology Be of Use?

Jill Morawski
Wesleyan University

Psychology is experiencing what many deem a “crisis,” often called a “replication crisis.” In response and with impressive speed, technical changes are being introduced to remedy perceived problems in data analysis, researcher bias, and publication practices. Yet throughout these large-scale renovations of scientific practice, scarce attention is given to philosophical and theoretical commitments as potential factors in the crisis problems. Analysis of involved psychologists’ understandings of scientific crisis, replication, and epistemology indicates the need for philosophical examinations. Likewise warranting close analyses are the associated assumptions about objectivity, credibility, and ontology (the nature of psychological phenomena). Such lacuna in the crisis interrogations constitute opportunities for researchers with expertise in the philosophy and theory of psychology to contribute to the science’s immediate problems and collaborate more closely with experimental psychologists.

Public Significance Statement
Psychology’s current crisis has affected the science’s credibility with the public as well as researchers’ confidence in their scientific practice. While technical adjustments are being made to remedy some of the problems, there remain significant philosophical and philosophical questions. Attention to these questions promises to help clarify psychologists’ conceptualizations of objectivity, credibility, and the psychological phenomena under study. Such clarification can be extended to enhance the public’s understanding of psychology and its products.

Keywords: replication, philosophy, theory

In his presidential address, Ronald Levant (2006, p. 384) called for “Making Psychology a Household Word,” and among the means of doing so proposed that individuals undergo routine “psychological checkups,” routine psychological screenings that would be akin to dental checkups. A few years later researchers began reporting worrisome cracks in psychology’s scientific foundation. Initially they observed disconcerting fissures rent by several shocking incidents of fraud, experimental evidence of a presumably nonexistent phenomenon, and numerous failures to replicate assumedly to be robust experiments. The fracturing expanded and multiplied and so came more reports of failures to replicate well-regarded experiments (Open Science Collaboration, 2012, 2015). Soon sighted were a host of questionable research practices, some bestowed ominous names like P-Hacking, V-Hacking, and HARKing (Bakker, van Dijk, & Wicherts, 2012). Added to these were problems previously identified: familiar hazards like the file drawer problem, publication bias, and experimenter bias. An auditor of this sizable list of defects would likely ascertain that one household needing assistance is Psychology’s.

Nearly a decade after the initial tremors, the scientific ground remains unsettled and fissures

Correspondence concerning this article should be addressed to Jill Morawski, Department of Psychology, Wesleyan University, 207 High Street, Middletown, CT 06457. E-mail: jmorawski@wesleyan.edu
continue to grow, with many (perhaps the majority of) psychologists believing the unstable situation constitutes a “crisis,” specifically a “replication crisis.” However, there is no consensus on this grim assessment nor even on the rudimentary diagnosis—crisis versus no crisis. Many urge substantive and immediate modifications of scientific practice, whereas others view the institutionalized reforms as overkill if not a serious hindrance to scientific discovery. Still others believe that a turning point has been reached, bringing the end of crisis. The situation is further exacerbated, as scientific family secrets are being made all too public not only in scientific reports (including multiexperiment studies reporting high rates of replication failures) and statements released by professional organizations, funding agencies, and conventional media but also through vibrant conversations on Twitter, blog posts, and newly created Internet fora. The reports are impressive not only in volume but also in technical sophistication, conscientious analysis, and acumen. Some are remarkable as well for their affect-laden displays and occasional hurling of emotionally charged accusations. Remediating these matters, therefore, is complicated by the fact that there remains no consensus on the roots of the problems or even whether they are problems at all.

Given the partial, often ambiguous, discussions of the foundational principles underlying these troubling matters, one can appreciate how the questions put to me, “what are the central philosophical issues of the replication crisis, and how might studies in the theory and philosophy of psychology contribute to its resolution,” brought to mind not a manifest list of problems but, rather, an alarming image of polygenic messiness. One can hear many voices: those of psychologists, journalists, statisticians, and philosophers. Upon listening to this cacophony, it became clear that the actors’ perspectives, psychologists’ voices, are crucial to engaging the question put to me. To proceed otherwise (by either foregrounding outsiders’ talk or backgrounding insiders’) is to suppose that there is something critical that the actors are unaware or about which they are aware yet choose not to address, thereby intimidating the workings of subterranean forces or adopting a “hermeneutics of suspicion” (Sedgwick, 2002). Above all, listening to the actors is essential to establishing more constructive relationships between researchers directly engaged with the crisis and those studying the philosophy and theory of psychology.

The present review thus begins with the actors’ accounts and then takes an “antiforensic” purchase (Collins, 2016; see also Fuller, 1988; MacMartin & Winston, 2000) that employs science and technology studies (STS) to connect the crisis concerns with knowledge about the structure and practices of science. The aim is not critique; in fact, STS provides no definitive analyst role. Instead, its reflexive practitioners take various approaches toward science. My own inclination is toward Woolgar’s (2005) reflexive “mode of being an insider and an outsider at the same time” (p. 311) and Haraway’s (1988) situated knowing that attends the situatedness of one’s knowledge making. Like many studying the philosophy and theory of psychology who are situated as insider–outsiders, my aspirations are reparative. As described by Simmons (2016), reparative work requires reassembling the problem at hand and undertaking “the essential critical work of thinking with care and anxiety” (p. 125); it means staying with and caring for the trouble (p. 121). Reparation is best begun, then, by close attention to and elucidation of the “symptoms” and exploring their performative effects, not their dysfunctions or hidden drivers (Fleissner, 2009). Commitment to repair and care entails simultaneously practicing skepticism and involvement. At the same time, I admit an anxious feeling that the stakes of the crisis might be too high to entertain generative alternatives or so high as to favor expediency over extended critical reflection.

The review makes its starting point the local to follow the chief philosophy-related symptoms reported by participants in the replication controversies. Given that the symptoms are multifold and often entangled, a prudent and practicable tactic is to begin with the primary keywords: “crisis,” “replication,” and the overarching gestures to “epistemology.” Drawing upon STS (including the history of science) to explore these keywords brought into view several significant matters that lay at the margins of the crisis conversations. Participants usually simply notice these matters from afar yet that very noticing intimates their operational presence and warrants their closer inspection. The consequences of sidestepping these marginal
matters are unknown; however, their significance to the broader aims of scientific practice is evident. Examined here are three of these: objectivity, ontology, and trust/credibility. The review concludes by identifying several places where further investigation is needed, encouraging their uptake by philosophical and theoretical researchers, and urging collaborative work with experimental psychologists.

What’s in a Word? “Crisis”

Published notice of a crisis appeared in 2012 and soon came to be depicted as a “replication crisis” (Pashler & Harris, 2012). Whereas the dramatic proclamations seemed to have materialized precipitously, it was preceded by indicators that serious trouble was brewing. Among the ominous signs were recurrent failures to replicate presumably robust experimental findings, a small outbreak of stunning frauds, and experimental demonstration of extra-sensory perception (ESP), a phenomenon that psychologists almost universally deemed to be nonexistent. To note, psychology was not exceptional in facing these worrisome signs for other sciences were experiencing ill ease about the reliability and validity of findings, a distress signaled by a 2005 *PLoS Med* article with the wickedly provocative title, “Why Most Published Research Findings Are False” (Ioannidis, 2005). There followed growing, triangulated evidence of a crisis of confidence albeit some psychologists continue to eschew diagnosis of pathology. These sceptics deem the unsettling as less a crisis and more a set of tractable problems localized in specific subareas or a result of the sensationalism incited by disaffected researchers (Baumeister, 2016; Fiske, 2016; Reis & Lee, 2016; Stroebe & Strack, 2014). Despite undertaking careful, sophisticated assessments they have been nearly overpowered by the explosion of crisis talk. And not just talk. The calamity has motivated large-scale programs to rectify the perceived problems; these include reforms of preexperimental practices, replication efforts, journal editorial policies, and open access to data and publications (see LeBel & John, 2017; Nosek & Bar-Anan, 2012; Nosek, Ebersole, DeHaven, & Mellor, 2018; Nosek, Spies, & Motyl, 2012; Open Science Collaboration, 2012). It has captivated the attention of granting agencies, professional organizations, journals, and academic institutions, many of which are introducing new regulatory measures or revising existing ones. Judging by the actors’ terms and behaviors, a crisis is upon psychological science.

Crisis in Psychology’s History

The expanding litany of dire scientific deficits accompanied by moral lessons, sizzling disputes, and personal assaults, has proceeded without either a shared or explicit definition of crisis. The absence of even a working definition is surprising given the disagreement over whether or not psychology is actually suffering a crisis. And the abeyance is notably odd in a discipline renowned for meticulous (operationalized, quantified, scaled, and empirically validated) definitions of concepts, constructs, variables, and instruments. One could conjecture that the meaning of crisis has an established history with the term already tacitly understood and, therefore, warrants no explication. Indeed, usage of the word crisis and its proclaimed occurrences have a long, heterogeneous life in psychology. So frequent are declared arrivals and departures of crises that it is reasonable to say that there exists a “distinct genre of crisis literature” (Wieser, 2016, p. 310). Its recurrent usages over psychology’s past 150 years establish crisis as a “vigorous actors’ category,” and perhaps for this reason Sturm and Müllberger (2012) advised that it is “naïve to take crisis talk at face value” (p. 428). Plentiful crises have been unearthed, yet the ample incidences indicate no common conception of the term (Faye, 2012; Goertzen, 2008; Müllberger, 2018; Sturm & Müllberger, 2012; Wieser, 2016). Sturm and Müllberger (2012) found that proclaimed crises vary in content; such variation is evident even in crises attributed to replication issues (Müller, 2018). The periodic, worrisome upheavals differ, too, in their temporal and evaluative dimensions. Even historical interrogations of a single temporarily circumscribed episode reveal divergent ideas. Faye’s (2012) insightful study of (social) psychology’s crisis of the 1960s and 1970s, a typical “self-diagnosed crisis,” found no single conception guided the discourse or its resolution (p. 517). So recurrent and rhetorically varied are crises that it has been proposed that they actually represent a singular, ongoing episode, suggesting that psychology is in
normal part of “rational science” (Sturm & Mül-berger, 2012). However appealing this last interpretation might be, it contravenes etymology, notably the general understanding of crisis as a “decisive turning point during a developmental period” (Wieser, 2016, p. 361).

Studies of psychology’s recurring crises reveal variations although have yet to uncover a conception that is either common to or missing from the actors’ accounts. Historical excavations reveal no overarching or shared conception, yet interrogation of the actors’ models rewards in other ways. Psychologists’ accounts of their discipline’s crises are seen to adopt one or more of three explanatory models that can be broadly categorized as philosophy of science, psychology proper, and cultural accounts. When employing the first kind of accounting, philosophy of science, crisis talk leans heavily upon several philosophers, notably Husserl, Heidegger, Popper, Lakatos, and Kuhn (the latter three being favored in North American psychology since the past quarter of the 20th century). Whereas Popper has served psychologists in setting grounds for evaluating truth claims and, more generally, for regarding crisis as a normal part of “rational science” (Sturm & Mülberger, 2012), Kuhn’s conception has afforded a decisive explanation of crisis and elevates it to an acceptable, even necessary condition of science. It has enabled, too, a redemptive narrative that pronounces an inevitable end to a crisis and (as many psychologists interpret him) chronicles the science’s material progress. The words paradigm and revolution became commonplace in psychology, and The Structure of Scientific Revolutions (Kuhn, 1962) appeared on the reading lists of undergraduate and graduate courses. Psychologists’ enthusiastic adoption of Kuhn’s theory of crisis and scientific revolutions has offered soothing balm for both self-understanding and confidence. It was especially useful in making sense of the crisis of the 1960s and 1970s and bolstering the “cognitive revolution.” These benefits accrued despite the fact that psychology’s crises do not meet Kuhn’s assumptions that (a) scientists rarely if ever explicitly recognize a crisis state; (b) crises follow discoveries of significant anomalies; and (c) the scientific change is monumental (see Sturm & Mülberger, 2012). Nevertheless, among the various crisis explanations, his has predominated in mid-20th century psychology, offering both persuasive “language for interpreting” the discipline (Faye, 2012, p. 518) and a manifest emblem of its genuine scientific status.

Some psychologists turn to psychology itself to explicate crisis situations, perhaps finding psychology’s theories make space for agency and/or nonrational processes or perhaps simply finding them to be comfortingly familiar. Notable here is use of Erickson’s developmental theory to explain one crisis as an “identity crisis” much like youth are posited to experience (Faye, 2012). Psychology has been summoned not only to provide an explanatory model as evidenced in the application of Erickson’s work, but also to diagnose the cause(s) of these major scientific ruptures. With an aim to diagnose the sources of a midcentury crisis, even experimental methods were used to detect impediments to reliability and experimenters’ unintentional influences in experiments (Morawski, 2015). Empirical methods also are being employed to study the current crisis albeit these mainly target researchers’ postexperimental behaviors, including their faulty cognitions (Flis, 2018; Morawski, 2019).

In addition to ready-made philosophical models and those crafted with psychological ideas are studies that examine the cultural context of scientific crises.1 Cultural analyses, for example, situate Vygotsky’s depiction of crisis in the social and political context of the then nascent Soviet Union, and Husserl’s account of crisis in the context of the “cultural crisis of European civilization between both World Wars” (Sturm & Mülberger, 2012, p. 429). Diagnoses of late 1960s social psychology “crisis of confidence” were informed by wide-spread social unrest in the 1960s; accordingly, one social psychologist drew upon historian Arthur Schlesinger’s analysis of American society as cycling through a period of experienced uncertainty (Faye, 2012). No cultural analysis of the current crisis has appeared, possibly not an inadvertent omission given the dire cultural climate in which it is transpiring. Psychology’s alarm about unreliable knowledge and questionable research transpired as North America experienced a massive economic downturn and subsequent detection of questionable, sometimes fraudulent financial

1 Notably, cultural studies typically occur retrospectively and, therefore, might be deemed historical interpretation, not a model.
practices (perhaps warranting the acronym QFPs). Alongside there emerged unprecedented political and social contestations over what constitutes a “fact.” Making comparisons to or drawing analogies from this culture might, at the least, cast an unappealing story.

The models advanced to explain past crisis episodes—from philosophy of science, psychological theory, and culture—have yet to be systematically applied to the present crisis. That said, informal notions of crisis are detectable in language, notably in uses of Kuhn’s terminology. Whether informed through textbooks or direct reading of Kuhn, participants frequently describe the situation in Kuhnian terms of crisis and revolution. Common are refrains like “replication revolution,” and “credibility revolution” along with a forthcoming “renaissance” (Nelson, Simmons, & Simonsohn, 2018), and scientific “revolution” (Nosek et al., 2018) leading to “utopia”—psychology’s postrevolutionary scientific world (Nosek & Bar-Anan, 2012; Nosek et al., 2012). In contrast, an avowedly non-Kuhnian yet positive view of the revolution urges seeing the crisis as a “political” not scientific one. This political revolution, “Revolution 2.0,” is not like the American revolution with its overthrow of extant powers. Instead, it is more like the French or Russian Revolution “that overturns the status quo within one country and leaves the same people to function in a differently structured environment” (Spellman, 2015, p. 887).

**Crisis and Culture**

Psychology’s crisis has been vigorously heralded in professional and popular publications and so too in blogs and Tweets, yet neither a clear nor consensual conception of crisis has emerged. Participants’ keen inward (reflexive) attention to psychologists’ behavior suggests an anomaly of a different sort: an anomaly in the scientific system not the content of science. This reflexive assessment describes much of the crisis in social terms, broadly defined. To note, although commentators use contemporary psychological knowledge to explain the inadequacies of research conduct (Morawski, 2019), some informally describe the crisis as older established psychologists rebutting younger psychologists’ use of innovative techniques for assessing the reliability and validity of extant experimental findings. However, in the absence of a coherent crisis model shared by the actors, the present situation can be understood quite differently. That is, it might not be a crisis but something else altogether, and this possibility has been considered in other sciences also said to be in crisis. Assessing the claims of the “Open Science” movement to which psychologists’ reparation plans belong, Mirowski (2018) documented mistaken assumptions about the claimed crisis conditions, including inaccurate historical accounts, confused notions that science has been “closed,” and unsupported claims that technical modifications provide the fix. His exhaustive review suggests that “The primary manifestations of the new regime are the marriage of an ethos of what has been called ‘radically collaborative science’ with the emergent structures of ‘platform capitalism’ all blessed under the neoliberal catechism of the market . . . ’ In short, the purported malaise might not be a substantive rupture in science as usual but, rather, an ideological backdrop for the Open Science movement to better establish a neoliberal market and social organization. Mirowski asked that we take up actors’ claims and try to “link some broad abstract cultural ideas about knowledge to pronounced transformation of scientific practice at the micro-level” (p. 173). The amorphous understanding of crisis signifies something much undesired and beckons if not insists on dramatic changes. This makes crisis a particularly knotty subject for analysis, albeit significant for better understanding psychology’s science system and mission.

**What’s in a Word? “Replication”**

In contrast to crisis, “replication” has been well defined; in fact, in has been well defined in multiple ways. Just as the word crisis functions as a signer of troubles in a tacitly assumed science system, so replication serves as signer of realizable truth claims (validity) or at least it has functioned as such in psychology’s recent history (Neuliep, 1991). The sometimes suspiciously loud refrains pronounce the preeminent value of replication, describing it as the “gold standard” (LeBel & Peters, 2011), a “central” (Fabrigar & Wegener, 2016, p. 68) and “defining” feature of science (Open Science Collaboration, 2012, p. 657). Replication is taken to be fundamental for discovery and necessary to realizing a cumulative (progressive) science.
Many if not most psychologists hold this epistemic tenet, whether or not they subscribe to Popper, Lakatos, or another philosophical school. Even when boldly proclaimed, the tenet delivers no certainties about the specific modes of scientific practice, including replications. As reported by the National Science Foundation (NSF) committee charged with assessing the status of replication, its meaning “becomes less clear the more closely we look” (Bolen, Cioppo, Kaplan, Krosnick, & Olds, 2015, p. 5). Histories of the scientific meaning and uses of replication similarly report opaqueness as well as variation. Over the past decade, psychologists devoted to dispelling such opacity have spotlighted two key questions, asking (a) what constitutes a replication? and (b) how should replications be conducted? The magnitude of writing devoted to these questions at once underscores the centrality of the epistemic tenet and also reveals that there remains much to be decided about best scientific practices and psychology’s overarching scientific aspirations. Nothing is new about these replication-related questions for psychology has addressed them over and again throughout the past century (Mülberger, 2018; Neuliep, 1991; Stam, 2018). Here histories of replication practices in other sciences offer reassurance, finding that replication has varied in meaning and use (Schickore, 2011; Steinle, 2016).

Defining Replication

Any summary of psychologists’ efforts to define replication would do injustice to the astute, elaborate, and labor-intensive work they are undertaking. Nevertheless, there are key elements of the meaning-making projects. First, most now generally agree that there are multiple kinds of replication (Earp & Trafimow, 2015; Schmidt, 2009; Zwaan, Etz, Lucas, & Donnellan, 2018). Among the kinds identified, the two forerunners are “direct” and “conceptual” replication. In a recent integrative report of replication in psychology, Zwaan and colleagues (2018) proposed using,

direct replication to refer to studies intended to evaluate the ability of a particular method to produce the same results upon repetition and conceptual replication to refer to studies designed to test the same theoretical idea using an intentionally different method than previous studies. (p. 5)

They along with others believe that direct replication best serves scientific progress, whereas conceptual replication risks post hoc reasoning and, not inconsequentially, an “inability to specify conditions needed to produce an effect is a serious impediment to scientific progress” (p. 7). Direct replication is believed to form the bedrock of falsification that many hold to be “not optional” (LeBel, Berger, Campbell, & Loving, 2017). Unpersuaded by the arguments supporting alternative conceptions of replication, proponents of direct replication have articulated its essential components and generated taxonomies, recipes, manifestos, guides, and utopian tracts (see Asendorpf et al., 2013; Brandt et al., 2014; Hales, 2016; LeBel, Vanpaemel, Cheung, & Campbell, 2019; Munafò et al., 2017; Nosek & Bar-Anan, 2012; Nosek et al., 2012; Simons, 2014; Trafimow, 2018). The proactive projects retain confidence in the canonical methods and typically locate the problems not in methods but in their execution, what Ferguson and Heene (2012) referred to as psychologists’ “pernicious aversion to the null.” Such aversion along with other forms of bias ultimately make replication and falsification “mere mockeries of what they should be in a fully functional science” (p. 559). The headwinds propelling direct replication have brought greater clarity about methods just as they have elevated the status of replication studies.

The winds of reform continue, yet not all of the arguments against privileging direct replication have been dispelled. Among the unresolved matters is how to treat “context and background,” one of the variables held to be relevant to replication (Schmidt, 2009). Context and background belong to a polymorphous variable class that includes research setting, participants’ experiences, language, environmental conditions, time, experimenter characteristics, aspects of stimuli, and so on. Proponents of direct replication acknowledge the impossibility of fully controlling all these variables yet nonetheless believe they will be sufficiently controlled with meticulous specification of context and boundary conditions along with improved reporting standards. On the other hand, proponents of conceptual replication and other means to establish valid knowledge assert that stringent replication requirements can stymy discoveries and, more importantly, carry a predetermined if not reductive view of phenomena that
severely constrains what can be discovered. According to this view, many phenomena are highly context sensitive: They are mobile and mutable such that, for instance, social psychological research manipulations can have “different psychological properties and effects if used in different contexts or populations different from the original experiment”. Given these characteristics of psychological phenomena, experimenters can never enter the same river twice (see Cesario, 2014; Crandall & Sherman, 2016; Iso-Ahola, 2017; Strack, 2017; Van Bavel & Cunningham, in press; Van Bavel, Mende-Siedlecki, Brady, & Reiner, 2016). Situated between these opposed positions are claims that replication is important though not a “silver bullet” given the inevitability of human variability and the unexplicated auxiliary assumptions (Earp & Trafimow, 2015, p. 9). In the end, arguments for the stability of phenomena or those for their context-sensitivity and cultural dependence call for deeper analysis of the generally tacit ontological assumptions made in empirical studies.

Complications

The uncertainties about epistemic objects and about researchers’ tacit knowledge are addressed in the concept of “experimenters’ regress.” When applied to experimental replications, experimenters’ regress refers to the intractable problem of determining whether a failed replication constitutes sufficient evidence that the original finding is unreliable or that the replication itself is not an adequate repetition of the original experiment (Collins, 1985). According to Collins, there are no rational means of adjudicating or choosing between these two interpretations of a particular replication. Regress arises due to the inescapable presence of scientists’ judgments and their tacit skills and knowledge that are not (and cannot be fully) explicited in experimental design or written procedures. The problem is particularly evident in cases where independent research groups report discrepant findings, such that “If both groups are determined, disputes between them cannot be settled simply by doing more experiments since each new result is subject to the questions posed by the regress” (Collins, 2016, p. 67). During acute controversies between groups, extrascientific factors like status or affiliation come into play, and statistical calculations offer no resort. Neither do meta-analyses settle interpretive differences, for they ultimately call for metameta-analyses. The social sciences, Collins added, encounter ontological entanglements when language is central to research; uncertainty arises here because “Language forms the world that humans inhabit ever as it describes it” (p. 80).

Elucidating the experimenters’ regress pinpoints thorny problems that are faced in any kind of replication (Feest, 2016; Teira, 2013). Feest took up one of the problems by reframing Collin’s notions of tacit knowledge and uncertainty. She proposed that tacit and explicit knowledge are not distinct cognitive conditions but are “intertwined throughout the entire process of research” (p. 35). Agreeing with Collins that science, particularly psychological science, confronts epistemic uncertainty and conceptual openness, Feest nevertheless believes this skepticism “can be turned into a productive research tool by way of an explication and critical evaluation of the tacit assumptions in question,” a process she called “operational analysis” (p. 45). Her proposal has potential to constructively inform psychologists’ approach to replication, especially as it encompasses concerns held by both advocates and opponents of direct replication. However, should this approach be taken up, it remains to be determined whether and how operational analysis can provide a viable tool for explicating the ontological dynamics and linguistic constraints identified by Collins and others.

What’s in a Word? “Epistemology”

Concerns about “epistemology” and the sometimes synonymously used term, “philosophy of science,” intermittently surface in the replication crisis literature. Although participants differ on some important points, they share the general epistemic belief that reproducibility is crucial to scientific discovery. Those committed to direct replication techniques engage epistemology with a signature premise that replication is not optional. It is with this commitment that they occasionally seek support

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2 Even with the complications of experimenters’ regress, Collins did not reject replication studies; rather he deemed them to be aspirational even when unrealizable.
from philosophy, although usually it is not Kuhn to whom they turn. The high value of philosophical guidance is seen in discussions of philosophy of science (see Earp & Trafimow, 2015; Schmidt, 2009), claims that the distress is “a crisis in the philosophy of science” (Barrett quoted in Palmer, 2016), and in proposals for making philosophy (and history) of science a regular part of psychology education (Open Science Collaboration, 2015; Spellman, 2015). Additional evidence emerges from close analysis of the mandates for change, which finds them to comprise “an epistemic project that is informed by epistemology” (Derksen, 2018, p. 1). Most of psychologists’ urgent calls for modifying practices rely on a philosophy of science that was imported into psychology some 60 years ago. In so doing, they rehearse an older, envious regard of the physical sciences (for instance, Trafimow, 2018), although some note that psychology and other “soft sciences” lower in the “hierarchy of science” unavoidably fall short of that ideal (Fanelli, 2010).

Relying on Philosophy

By so leaning on an established albeit what many philosophers themselves regard as outdated epistemology, conversants in the crisis exercise, first-order reflective learning, occurring when a scientific community “finds solutions to problems using several lines of inquiry” but only looking within the extant value system, background assumptions, and theories. By contrast, “second-order reflective learning” extends to incorporate reflection on these very values and beliefs whereby “value systems become the object of learning while in first-order learning these are taken for granted.” (Schuurbiers, 2011, p. 772).

The first-order learning predominating in the crisis deliberations is evident in discussants’ reliance on dated, philosophical models and preoccupation with conventional ideals of method while largely eschewing close, critical appraisal of the method itself. The remediation projects fix on recovering and ensuring appropriate methods not rigorously interrogating them. As one group describes this route to “Scientific Utopia,” the problems are “social” not “technical,” adding that the “ultimate goal is to improve research efficiency by bringing scientific communication practices closer to scientific values” (Nosek et al., 2012, p. 212). Unruly, unscientific investigative practices, according to Ferguson and Heene (2012), “reduce psychological science’s capability to have a proper mechanism for theory falsification” (p. 555). With the problems configured as such, conventional philosophies of science like Popper’s (and occasionally Lakatos and Meehl) are taken as recipes for falsification. They provide assurance that methods enabling falsification ultimately will yield “reliable,” “robust,” “valid,” “quality,” and “true” (terms often used interchangeably) scientific knowledge. Psychologists “translate Popper’s philosophy of science into a program of reform, with concrete rules of practice, an infrastructure for that practice, and research projects that realize the ideal” (Derksen, 2018, p. 5). Reliance on a philosophical rock, however, is proving less secure, as researchers are not all in agreement about the interpretation of Popper, prompting claims that some psychologists display an “uniformed reading of Popper” (Earp & Trafimow, 2015; see also Holtz & Monnerjahn, 2017). Differing interpretations of Popper (for reviews, see Derksen, 2018; Flis, 2018) raise other challenges, including central questions about what exactly constitutes the “auxiliary” or “background” assumptions described by Popper and modified by Lakatos. Also put to question is the exaltation of replication, reflected in one observer’s sense that “uncritical pursuit of reproducibility as an overarching epistemic value is misleading and potentially damaging to scientific advancement” (p. 4); following Popper carries big “epistemic risks” (Leonelli, 2018, p. 13). As another bystander discerned that in elevating replication, scientists are following a “methodological imperative” and erroneously taking “scholarly argument” as a mirror of practice (Schickore, 2011, pp. 514–515). What’s more, querying the meaning of auxiliary assumptions might jeopardize those very rules and the science’s infrastructure. With these concerns in mind, some researchers recommend additional or other methods for attaining valid knowledge, including triangulation, modeling, and inference, and urge downplaying the centrality of replication (Leonelli, 2018; Munafo et al., 2017). These and related alternatives move beyond presuming the necessity of replication to introduce epistemic models wherein replication functions differently; among the proposed mod-
els are variants of pragmatism (Gantman et al., 2018), McGuire’s perspectivism (Swiatkowski & Dompnier, 2017), and enactment (Derksen & Morawski, 2018).

Reliance on philosophy of science has grounded extensive appraisals of the current malaise although its uptake can and has imported problematic assumptions. The first of these inheres in the very idea that philosophy of science offers an appropriate and efficacious tool with which to interrogate psychological science. Psychologists’ resort to philosophy of science as a means to gain fuller self-understanding has a complicated history. Along with underwriting a concrete template for conducting research, it has soothed what Koch calls psychology’s “epistomopathy” by appeasing the discipline’s “physics envy” if in no other way than by associating psychology with indisputable sciences (Toulmin & Leary, 1985). Further, psychologists’ usage often belies the original intent of the borrowed philosophy by reframing its evaluative functions as programmatic ones and its descriptive aims as prescriptive ones. Third, such borrowing underappreciates the distance between philosophical accounts and actual science, leaving aside any substantive assessment of fit between scientific epistemology and practice. It overlooks the dynamics of science, missing for instance how current research uses powerful techniques and technologies that did not exist 25 years ago; these include automated statistical analysis and data checking, Internet experiments, and rapid dissemination of findings. It similarly elides differences in temporal orientation, between philosophers’ generally retrospective orientation and scientists’ decidedly prospective one. These gaps between epistemology and practice go unattended when models like Popper’s are programmatically used as “methodological imperative” (Derksen, 2018; Flis, 2018; Schickore, 2011, p. 515). The commitment to a “common method” grounded in a normative and presumably invariant epistemology is made despite evidence of the dynamic evolution of method (Daston & Galison, 2007; Fuller, 1988; Mrowski, 2018). These problems all might pass simply as disconnects in the intellectual interests of two disciplines, psychology and philosophy. However, when more closely inspected, they are found to introduce other problems, for instance, when the reform projects (seen as “social” and not “technical” repairs) assume a common method and proceed to devise practices to remedy that “social” problem (Open Science Collaboration, 2015). In so doing they neglect the distinct separation philosophers traditionally make between epistemic matters and social ones (Fuller, 1988).

The aspiration to affirm a common method based on certain epistemic principles is unambiguously pronounced in the “recipes,” “guides,” a “manifesto,” and scientific “utopia” and concretely evidenced in the social-oriented proposals for best practices whether they be data sharing, statistics, hypothesis tests, reporting, and open science practices more generally. These efforts, however, do not separate epistemic and social matters as evident in the premise “that science is not only an exercise in individual reasoning, it is a community enterprise” (Flis, 2018, p. 143). Spotlighting the social, the collective enterprise, is seductively reassuring even as it sutures the aforementioned lacunae between the evaluative and programmatic, theory and practice, past and present, and the epistemic and the social.

Indigenous Epistemologies

For nearly as long as many psychologists have incorporated philosophies of science into their reasoning others have developed epistemologies that mortar philosophy of science with bricks of psychological knowledge, including behaviorism, Maslow’s theories, and most recently cognitive psychology (Flis, 2018; Morawski, 1986; Smith, 1986). Smith has termed these epistemic models built with psychology “indigenous epistemologies,” distinguishing them from uses of conventional philosophies of science. Although unmarked by the actors, crisis assessments frequently advance an indigenous epistemology, a philosophical model that is cemented with contemporary knowledge about cognition (rationality/irrationality) and social behavior (individual self-interest). With concentrated focus on the psychological constituents of research, these psychological epistemologies dismiss the objective knower presumed in conventional philosophies of science, replacing it with a not always rational (prone to a range of cognitive biases), often self-interested (motivated by personal success) knower (Morawski, 2019). As described by Flis (2018), the current reconstructions use classical philoso-
Marginal Matters?

The ever-expanding literature on psychology’s scientific health queries its putative crisis state and identifies major causes. As indicated by the commonly used expression, “replication crisis,” many commentators fix on replication, reasserting its core function in science and, accordingly, rehearsing the underlying epistemology that replication serves. A substantial number of erratic practices and deficient procedures have been detected, prompting reforms of data generation and handling. At the margins of these intensive interrogations are numerous matters of concern that receive comment though rarely any exploration. Two of these, objectivity and ontology, are core epistemic elements being mentioned thought not analyzed; they warrant closer analysis. A third matter, public trust in scientific psychology, though not central to experimental practices, connects directly with the discipline’s lifelong commitment to serving human welfare. Although assigned marginal places in the ongoing conversation, these three matters of concern are germane to rethinking and reforming the psychological sciences.

Objectivity

This indigenous epistemology of irrationality, notably with its claims of researchers’ cognitive biases, has material implications for defining objectivity, especially in its emphasizing knowers’ limited capacities to be objective. Yet the situation is more complex in that some crisis-related critiques locate problems with the orthodox methods, finding for instance, that canonical methods harbor auxiliary assumptions about the nature of psychological phenomena (ontology) and inappropriate statistical assumptions about the nature of the world (Bayesian vs. frequentist). However, the majority of the trenchant appraisals target researchers’ failures to adhere to those canonical methods, which include though not limited to failures to execute hypothesis testing, use situation-appropriate statistical analyses, publish unsuccessful replications, and adequately report procedures. These failures are problems of researchers’ non-compliance, actions that are believed to stymy the objectivity requisite to canonical techniques. As the logic goes, fixes to the deficiencies in objectivity require modifying researchers’ behavior. Stated otherwise, many take the deficiencies to be shortcomings not of scientific method but, rather, of scientists. This reasoning might have prompted asking whether faulty, nonobjective behavior results from inadequate training, insufficient expertise (experience in conducting and judging research), researchers’ differences in internal or phenomenological representations of phenomena, or simply bad “habits.”

Although these possible deficits are occasionally mentioned, the chief suspect is the psychologist’s psychology, specifically less than objective actions caused by his or her (natural) cognitive and social thinking (Morawski, 2019). Diagnoses of such undesirable, counterproductive psychological conditions are supported with the language, theory, and empirical evidence of cognition and self-serving biases. Pronouncing internal (cognitive and social–psychological) sources of biases means, among other things, that individual self-correction and self-governance cannot prevent unscientific actions because they too are susceptible to non-conscious bias and self-interested decisions. In-

3 Sporting epistemological “naturalism” might be understood as aligned with recent philosophy of science “that asserts that philosophy is continuous with science and which attempts to formulate and evaluate philosophical theories by using research findings and investigative means of the various sciences” (Haig, 2018, p. 1).

4 Researchers’ bad habits and sinning are enumerated in Chambers (2017) and Grice et al. (2017).

5 These applications of contemporary empirical and theoretical work on cognitive biases have proceeded without even a nod to the irony of using scientific evidence to diagnose the unreliability of that science.
stead, attaining objectivity requires external regulation overseen by the social body: professional organizations, funding agencies, publishers, academic institutions, and the like.

Objectivity’s History

Of course, nothing is novel about modifying methods. The discipline’s history contains plentiful episodes of controversy and consensus making over methods, including linguistic ambiguities, modes of measurement, observers’ objectivity, and sociopolitical pressures (Carson, 2006; Danziger, 1990; Dehue, 1995; Hornstein, 1988; MacMartin & Winston, 2000; Morawski, 2015; Winston, 2018). Psychology’s chronicle also is not exceptional: Historical and social histories of science have found ruptures and changes in methods, thus challenging origin myths and beliefs in a common scientific method and epistemic stability. Such is the case of objectivity, which is generally understood to be a singular, stable ideal. However, tracing objectivity’s life span in modern science, Daston and Galison (2007) documented how it has evolved yet has been routinely defined in relation to the then-current notions of subjectivity and, importantly, to suppression of subjectivity. The enduring linkage between this epistemic ideal and the scientist’s subjective self illustrates what Porter (2008) described as a “tight relationship between modes of objectivity and conceptions of the scientific self” (p. 643). In other words, objectivity demands a certain kind of scientific actor. Modern psychology, for instance, embraces a “mechanical objectivity” that assumes objective acts are attained with aid of techniques to discipline the scientist (examples include operationalism, randomized clinical trials, and double-blind experiments). These and other “technologies of objectivity” presume certain virtues of investigators; they “depend on competent interpreters with the knowledge, will, and the authority to defend the spirit of the rules” (Porter, 2016, p. 460). In the current crisis situation, these presumptions have been revised or discarded. Detection of the scientist’s psychological deficiencies has prompted a host of novel, strictly regulated techniques and a “disciplining by disciplines” (Porter, 2008) through new institutes and centers of surveillance along with expanded oversight by journals, funding agencies, and academic institutions.

To reiterate, nothing is new about setting or revising rules to guide scientific practice. What is novel about the current measures are the comprehensive, remarkably detailed social programs that give rules substantive “administrative form” that demand great measure of power to enforce. As Derksen (2018) observed, the reforms are a massive, “innovative” extension of Popper’s social engineering idea “because such a strict adherence to the methodological principles has not been attempted before in psychology” (pp. 17–18). The magnitude and disciplinary power of this emerging administrative governance reassures those who believe practice will follow rules. For others the projects are a reminder that rules gain meaning in use, that scientific rules have “inescapable ambiguities,” and that they can acquire different meanings with new investigative tools, observers, or subjects (Porter, 2016, p. 462). Some observers have noted this greatly increased “discipline” and advise balancing disciplinary constraints with a move away from such hierarchical power relations and toward a “democratization of expertise” (Saltelli & Funtowicz, 2017, p. 9).

Authority

Whether one is assured or not about the effectiveness of enhanced, keenly regulated rules, what warrants reflection is the fact that scientific practices that are stringently mediated through administrative structures require both an authority to pronounce best practice and a conception of its collective body as social. These fundamentals require that someone or some group has authority to speak on behalf of individual scientists; in a general although not nefarious sense, this requirement is authoritarian. The so-determined practices also presuppose what is social at once represents the discipline’s social will and also displays a reflexive awareness of the science’s social nature. Yet the heated debates are not without challenges to the desig-

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6 Psychology’s conception of mechanical objectivity carries vestiges of positivism’s ideal of value-free science. Thus, it is not surprising that there is scarcely any consideration of “values” in the crisis literature. For illustration of the tensions between values and psychology’s epistemic ideals.
Ontology

The conception of objectivity embedded in the recent reform projects enfolds the longstanding ideal of mechanical objectivity and then modifies it, taking to a decidedly higher social–organizational level. Many of the modifications, both proposed and already instituted, involve historically unprecedented regulations and surveillance, including some to be implemented at digital distance. Several modifications also directly target behavior change through both rewards (like badges) and sanctions (like retractions). These multisited remediation projects are far from complete and most likely will undergo revision. They can and likely will be adjusted to accommodate various kinds of research (like surveys) and fine-tuned to accommodate empirical challenges (like representative sampling). In contrast there seems to be little flexibility and no discernable wiggle room for conceptualizing psychology’s objects. As Daston (2000) described such inflexibility, “Theories about the furniture of the universe may come and go, but the furniture stays . . . (p. 3). This certainty about ontology is read into Popper’s model (regardless of how it is revised) that was developed for the natural sciences. However, the objects in those sciences are not the same entities as the scientists observing them. Nor do natural science objects have the same capacity to take up the scientific representations of them as do psychology’s objects.

When the ontological premise of certain, stable things ascribed to natural science objects is extended to psychology’s objects, the logic that eventually follows takes precise replication as necessary evidence that a psychological object under study is real. The logic of this process ultimately prescribes what can be taken as real in psychology.

Psychology’s Objects

Few if any researchers engaged in the crisis controversies doubt that psychological phenomena are real, although they are not in agreement about the nature of those real objects. Advocates of standardized, direct replications generally hold that if an entity is real, then it can be reproduced using the same procedures as were employed in the initial discovery of that entity. They hold faith in the ontological premise of Popper’s work. At the same time a number of researchers entertain the possibility that an entity exists even when it fails to be detected via direct replications. Along with noting that replication success or failure offers no decisive evidence of an entity’s existence, these researchers emphasize the high context sensitivity or cultural dependence of phenomena (Crandall & Sherman, 2016; Fabrigar & Wegener, 2016; Strack, 2017). Directly or indirectly they question certain ontological assumptions borrowed from the natural sciences, arguing that unlike physics, “there are no static phenomenon particles” in psychology (Iso-Ahola, 2017, p. 2). Even in direct replications, time, place, and actors (participants and experimenters alike) vary. As Schwarz and Clore (2016) argued, “even a technically identical manipulation does not guarantee an equivalent test of psychological phenomenon when the context changes” (p. 1408). To many of these psychologists (who advocate conceptual replications as better suited to study context-sensitive phenomena), psychology’s objects are dynamic yet nevertheless very real. They urge looking beyond constructs and effects to investigate underlying mechanisms, moderators, and constraints, and they underscore the importance of theory to guide these explorations (Barsalou, 2016; Greenfield, 2017; Reis & Lee, 2016; Stroebe, 2016; Stroebe

7 A conception of scientific reasoning as “social competence” has been developed by Mercier and Sperber (2017).
What is needed, Barsalou (2016) posited, is a theory of “situated conceptualization” (p. 9).

The fundamental dispute over ontology emerges in review essays and empirical studies as well as deliberations over particular methodological reforms. On the one hand, those holding that psychology’s epistemic objects are stable, thus readily reobserved through replications, seek empirical evidence about the actual weight of context in replication failures. An extensive study replicating 100 experiments found contextual factors are not determinative of replication outcomes (Klein et al., 2018). On the other hand, those holding that psychology’s epistemic objects are dynamic and culture dependent offer comparably well-designed theoretical frameworks to explain the multidimensional context sensitivity of psychological phenomena. Working with this perspective, Greenfield (2017) explicated the variety of social, demographic, and cultural influences on behavior with the goal of developing theory that incorporates these dynamics. Her theoretical framework indicates that there is work to be done besides and beyond replication. She encouraged a psychological science that “moves from the issue of whether or not a psychological phenomenon replicates to studying the effects of sociodemographic change on culture and behavior” (p. 763).

If one bets their coin on the power of “disciplining by discipline” then the ontology disputes will fade into the background as discovered entities are observed again and again in replications and subsequently taken to be real, at least for a time. Strict adherence to a rule-bound system provides a powerful microscope for observing reality and certainly some reality will be reliably detected on multiple occasions. If such a future unfolds, then there would seem to be little need to query the background assumptions about ontology described earlier. Along with other “hidden” and “auxiliary” assumptions, they most likely would remain hidden in the epistemic commitments that are motivating the reforms. And the ontological assumptions identified here would be even more complexly entangled in any indigenous epistemology that harnesses psychology (with its discovered entities like cognitive biases) to explain the actions of scientific observers. One risk of ignoring such entanglements is the possibility that psychological science will continue to produce a proliferation of functional entities without explicating their ontological status (Stam, 2010).

Historical Ontology

A chancer’s gamble is to see the ontology predicament as an opportunity for close analyses and even reformulation of psychology’s epistemic objects. The currently predominant projects commit to the belief that psychology’s objects are warranteable and warranted by empirical evidence, but the science’s history indicates that its objects also are secured and warranted by additional or other means: consensus-making, compromise, and politico-cultural conditions. The case of “intelligence” is illustrative of these ontology-certifying engines (Carson, 2006; Hornstein, 1988; Winston, 2018). Thus, although the momentum of the reigning epistemology privileges empirical evidence as the best guarantee of an object’s existence, there is evidence that the veracity of such objects can and often does depend on actions and events besides controlled laboratory studies. This evidence indicates the need for other queries of psychology’s objects as well as for further reflecting on the underlying epistemic premises. One viable method is to compare the usually tacit ontological assumptions of the different camps (Derksen & Morawski, 2018; Flis, 2018; Langlitz, 2015). Another involves dissection of the specific scientific practices that are employed to search for and guarantee an object’s existence (Brives, 2013; Cohn, 2008). Such studies have documented gaps in reasoning between empirical observation and confirmation statements, including slippage from what is taken as “construct” to what is held to be real (Slaney & Garcia, 2015) and from data to interpretation (Hornstein, 1988; Klein, 2014; Teo, 2008). Other studies find subjective excess in measurement instruments (Derksen, 2001) and residual uncertainties of naming objects (Pickersgill, 2014). Another, decidedly more extensive method entails stepping back from examining specific scientific undertakings to assess the larger, longer life of psychology’s objects. Genealogical methods in particular enable tracing how objects come into being, how they are supported, nourished, travel, used, and survive or pass away. For instance, taking just a...
small step back, Latour (2000) described how the social sciences’ longstanding persistence in emulating a certain version of natural science ultimately has hindered their discovery of objects and possibly even hindered the entities themselves. Social sciences’ anxious mimicry of the natural sciences has fostered empirical techniques that constrict their objects. Ironically, he noted, natural science’s objects are treated quite differently than social scientists believe: They have “no scruples whatsoever in objecting to the scientist’s claim by behaving in the most undisciplined ways, blocking the experiments, disappearing from view, dying, refusing to replicate or exploding the laboratory to pieces” (Latour, 2000, p. 116). Also using a wide-angle lens, Stam (2010) charted psychologists’ practical abeyance of ontological matters in favor of concentrating on what is readily observable. In brief, “Psychologists generally make no commitment to the ontological status of entities (be they real or otherwise) but provide function descriptions of properties that are defined according to how they act rather than what they are.” The relative ease of introducing new scientific entities and eventually leaving them behind to propose yet others results in an “endless proliferation of hypothetical functional entities” (p. 145).

Standing back from particular scientific programs to appraise modern psychology not only can reveal the expediency, expendability, and replacability of objects but also can enable tracing the ways objects gestate, grow, respire, and transpire in cultures beyond the science system proper. Such out-of-doors’ studies are finding how psychology’s objects are dynamic, taking shape, circulating and developing both inside and outside the laboratory. Everybody is or could be involved in the ontological processes (Hacking, 1995; Haslam, 2016; Richards, 2002; Stam, 2015; Sugarman, 2015). Hacking (1995) succinctly summarized this ontological capriciousness of the human sciences:

To create new ways of classifying people is also to change how we can think of ourselves, to change our sense of self-worth, and even how we remember our own past. This in turn generates a looping effect, because people of the kind behave differently and so are different. That is to say the kind changes and so there is new causal knowledge to be gained and perhaps, old causal knowledge to be jettisoned. (p. 369)

Out-of-doors purchase on ontological flourishing also can be brought back to the laboratory space to ask whether and if so how particular practices play in enacting entities, not simply observing them. Thinking both broadly and deeply about ontology thus can move us toward understanding “science as a mode of performative engagement with the world” (Law, 2004; Pickering, 2010, p. 19; Woolgar & Lezaun, 2013).

**Trust in the Household Word**

In his phenomenally successful popular book on thinking, Kahneman (2011) championed discovery of the psychological entity of priming, calling it a “major advance” in our understanding of memory. He confidently asserted that priming is beyond individual awareness, informing readers that “You cannot know this from conscious experience, of course, but you must accept the alien idea that your actions and your emotions can be primed by events of which you are not even aware.” One year later, he registered doubts about the reality of priming in an open letter to colleagues (Kahneman, 2012). Calling the priming field “a poster child for doubts about the integrity of psychological research,” he warned of a “train wreck coming” and urged priming researchers to take immediate measures to reassess priming’s existence. Priming was one of the initial psychological objects targeted in the crisis but others soon followed, including the power pose, ego depletion, stereotype threat, gaydar, and facial feedback. Individual cases soon came to overshadowed by large-scale studies finding that many psychological studies failed to replicate. Just as priming no longer stands alone, neither does Kahneman with his worries about credibility. Hand-in-hand, trust and credibility circulate through the crisis ponderings (see Pashler & de Ruiter, 2017). Kahneman (2012) did not directly discuss public credibility but others have done so, worrying as well about the public’s dependence on psychological knowledge (Earp & Trafimow, 2015). With a broader landscape in mind, Fiske (2017) described the crisis as both “outside and inside the field,” discerning how the crisis casts shadows on the evidence-based advice to government agencies (pp. 652–3). Białek (2018) similarly observed how emphasizing replication itself “can cause distorted
belief in scientific progress” by introducing un-
certainty to a public holding high expectations
for science (p. 14). He suggested the public be
educated about the uncertainty of science: The
public’s acceptance of uncertainty would pro-
tect psychology’s image and foster psycholo-
gists’ aim to have “our findings to be imple-
mented in public policy, so that we can
contribute to making the world a better place”
(p. 15). Apprehensions about public credibility
have been voiced in other sciences as well (Leo-
nelli, 2018; Saltelli & Funtowicz, 2017) al-
though not all target methodological deficien-
cies as the problem. Credibility problems also
have been identified with the inevitable uncer-
tainties of quantification, especially as quantifi-
cation is marketed not as uncertain measures but
as certified scientific truth (Porter, 2016; Saltelli
skeptical about talk of credibility and a “democ-
racy deficit,” suggesting it to be ideological
argument for the open science movement.

Traveling Psychology

For psychology, trust is an especially indefi-
nite if not amorphous thing. Psychology’s dis-
covered objects (phenomena like anxiety, grit,
cognitive dissonance, obedience, or implicit at-
titudes) travel fast and far beyond journal pages.
They are taken up and used in policy, educational
programs, parenting techniques, and interper-
sonal relationships even when they are
revealed to represent the psychology of a very
small percent of the world (Henrich, Heine, &
Norenzayan, 2010; Rad, Marginiano, & Gingles,
2018). The livelihoods of psychology’s ob-
jects often eventually come to depend not only
on empirical warrants and successful replica-
tions. Trust in and the credibility of these ob-
jects also rely on kinds of evidence beyond
laboratory confirmations. The extensiveness of
credibility and trust becomes apparent with
closer consideration of the article cited at the
beginning of this review, one calling to make
psychology a “household word” (Levant, 2006).
This call would not have been so boldly made
after 2011: Since then it is no longer a trouble-
free claim to make. Even at the time its (precris-
sis) publication the call rang odd, for psychol-
ogy had entered the household more than a
century earlier and thereafter has produced a
plethora of evidently useful words, ideas, and
advice (for examples, see Herman, 1995; Jar-
zombek, 2000; Rose, 1989). Given the capacity
of psychology’s objects to travel swiftly to mul-
tiple locales, nearly inevitably changing along
the way, the matter of credibility (and respon-
sibility and trust) are not as simple as trust in
good statistics or rigorous replications. Once
outside the textbook or research setting psy-
chology’s knowledge and attending objects do
not rely much if at all on those grounds of trust.
Participants in the replication disputes have
voiced sober concerns about validity, truster-
whiness, and even credibility of data, but only a
few have extended those concerns to the public
dissemination of discoveries, some of which are
being disputed in the crisis-related studies (for
an exception, see Flore, 2018).

Dynamic Knowledge

To extend scrutiny of research findings to
their travel and uptake outside the lab demands
immense public-focused efforts. It would com-
pel recognizing the dynamism and fluidity of
psychology’s discoveries—a recognition that
these discovered objects circulate, becoming
more (and at times other) than products of sci-
entists’ work. In turn, acknowledging the back-
and-forth dynamics that connect knowledge-
making, dissemination, and lived experiences
will introduce ethical questions, compelling re-
alization of what Stam (2010) has termed an
“ethics of shared understandings.” Among the
few who have traced the travel of psychology’s
objects, Haslam (2016) found shifting, spread-
ing, and multiplying negative concepts. He
called this ongoing generative process “concept
creep” and attributed its occurrence to psycho-
logical science and culture alike. Haslam did
not examine this disconcerting creep in relation
to psychology’s crisis, although the speed of
concept expansion suggests it should be and
also because this traffic in knowledge is not
independent of public trust in psychology. Stud-
ies of the travel of psychological knowledge
need be integral to maintaining a responsible,
trustworthy, and robust science whose aim is for
the public good, the revered aim of “giving
psychology away” (Miller, 1969). Haslam inti-
mated such in concluding.

Understanding what drives this trend (concept creep)
and evaluating its costs and benefits are important
goals for people who care about psychology’s place in
our cultures. Equally important is the task of deciding whether the trend should be encouraged, ignored, or resisted. (p. 15)

The crisis-related apprehensions about psychology’s truth claims, credibility, and responsibility are motivating substantial appraisals and underwriting unprecedented transformations of scientific practices and their regulation. Far less is being done to address public trust, not only the possible damage to psychology’s public image but also the implications of that trust when the public takes up psychological knowledge. To date the remediations do not encompass the science’s postlaboratory conditions; perhaps this is because of the faith that the open science movement will restore psychology’s image and public trust. Overlooked is that fact that whether generously given away by psychologists and/or freely taken up by policymakers, industry and the public, psychology’s knowledge and its discovered objects are abundantly used to guide and govern human behavior. Psychology’s responsibility, its aim to “give away” its knowledge, should encompass at least a modicum of responsibility for what is given away, once it is given away. Haslam’s study serves as exemplar of the consequences of this giving away as do historical studies of how psychology makes its way to the public (Jennings, Wertz, & Morrisey, 2016; Rutherford, 2009, 2017; Watters, 2011; Winston, 2018).

Conclusion

Psychology’s replication crisis has been subjected to copious appraisals, including scrutiny by statisticians, philosophers, and journalists as well as psychologists. The present review takes up the actors’ perspectives and links them, when applicable, to relevant philosophy and theory of psychology. Navigating through the keywords regularly used by the actors inevitably forgoes detail and likely misses relevant matters. The objective here, however, is not comprehensiveness but instead is a reparative one: to identify both prospects and problems to which philosophical and theoretical studies can constructively contribute. So traveling along the actors’ expedition routes (which at points present divergent paths) reveals tangible opportunities for reparative work. Five of these opportunities are expressly evident, and readers probably will discern others. The five are as follows: (a) “Crisis” is looking like a tired word in psychology, yet its use remains essential in both the rhetoric and collective understandings of the current malaise. Although rarely delineated its usage nevertheless expresses intellectual and affective fears. If nothing else, the term signifies desire for major changes albeit what direction that change should take is not shared by all. Further analysis of the operative functions of talk about crisis can illuminate how its usage reflects scientific, professional, economic, and/or personal distress and assess whether the ongoing remediation measures accurately target that distress. Further, the near absence of cultural studies of the crisis might be itself symptomatic yet this invites studies of the crisis in cultural context. (b) Researchers with methodological expertise have artfully identified the alternative forms that replication can take. Regardless of what forms are promoted, the concept of “moderators” of experimental effects and, importantly, what moderators might tell us about the stability or variability of psychological phenomena remain unfinished business. The empirical efforts to check the existence and weight of moderators and the theoretical efforts to better understand their remarkable complexity are presently proceeding down parallel paths. Awaiting are theoretical studies that explore possible common grounds and/or irreconcilable difference between the different ways of thinking about moderators. (c) Conflicting ideas about the existence and nature of moderators represent a good, first attempt to explicate actors’ tacit or hidden assumptions. Becoming visible at the shoreline of the contestations are unstated assumptions about ontology—about the very nature of psychological phenomena. These forays rarely incorporate or even note relevant theoretical and philosophical studies yet such studies would offer rich perspectives. Here too collaborative dialogue is warranted. (d) Psychologists discussing the crisis have gestured to the philosophy of science, and when they have done so, they generally relied upon epistemology that is partial and outdated. The actors’ calls to introduce philosophy and history into the psychology curriculum beckon collaboration with researchers who are already knowledgeable about the philosophy, theory, and history of psychology. Likewise awaiting is joint work on the viability of psychology’s most recent indigenous psychology (of irrationality).
that circulates through the deliberations over methods and epistemic foundations. (e) The actors regularly exhibit reflexive self-awareness about the science system (mainly observing faulty bureaucratic and economic structures) and about scientists’ behavior patterns (mainly their biases). This self-awareness has yet to be extended to “second-order” reflection on the beliefs and values that are constitutive of that scientific system and ideals of human behavior. The reflexive work already accomplished by the crisis’ conversants affords a starting point for extended reflection on values and beliefs, from epistemic commitment (and dismissal of alternatives) to ethical responsibilities to consumers of psychology.

Readers of the Journal of Philosophical and Theoretical Psychology and members of American Psychological Association’s Division 24 will doubtless recognize these and other opportunities for philosophical and theoretical studies of the crisis. At the same time, they will likely recall experimental psychology’s longstanding disregard of the extensive library of philosophical, theoretical, and historical studies. Readers might then experience an anticipatory, even anxious feeling about moving forward: a feeling that perhaps past behaviors are the best predictors of future ones, and the repair work surrounding the scientific troubles will proceed without much regard for the philosophy and theory of psychology. Yet perhaps this time of extensive scientific uncertainty affords an occasion to “stay with the trouble” and act otherwise—a feeling that there are real opportunities to undertake collaborative and reparative work.

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