

Replication in Social Science

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Abstract

Across the medical and social sciences, new discussions about replication have led to transformations in research practice. Sociologists, however, have been largely absent from these discussions. The goals of this review are to introduce sociologists to these developments, synthesize insights from science studies about replication in general, and detail the specific issues regarding replication that occur in sociology. The first half of the article argues that a sociologically sophisticated understanding of replication must address both the ways that replication rules and conventions evolved within an epistemic culture and how those cultures are shaped by specific research challenges. The second half outlines the four main dimensions of replicability in quantitative sociology—verifiability, robustness, repeatability, and generalizability—and discusses the specific ambiguities of interpretation that can arise in each. We conclude by advocating some commonsense changes to promote replication while acknowledging the epistemic diversity of our field.



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INTRODUCTION

Science is presently in the throes of a crisis of replication. US National Institutes of Health director Francis Collins described recent concerns about replicability as “a cloud over biomedical research” (Hughes 2014). In 2014, *Science* and *Nature*, along with 30 other journals, issued a nearly unprecedented joint editorial to announce new reproducibility initiatives that seek to “raise the standards . . . as part of the quality control that justifies the public trust in science” (Nature 2014, p. 7). In behavioral science, psychology has had a series of dramatic episodes surrounding replication (including one with the catchy hashtag #repligate), provoking a movement whose recent influence in social psychology has been described as “staggering” (Finkel et al. 2015, p. 278). Economics and political science have both advanced their own initiatives to address transparency, reproducibility, and replication.

Amid these transdisciplinary developments, sociology’s own institutions and research practices remain relatively unaffected. A passing glance at sociology might even suggest that replication has become a vanishing art: sociology’s two flagship journals have published 27 articles over their history with the term replication in the title, but only one in the past 25 years. What makes this especially ironic is that sociology not only provided some of the most compelling early statements about replication in social science (e.g., Mack 1951, Rose 1953), but, through the sociology of science, the discipline has also arguably contributed the most of any social science to understanding how replication actually works in practice.

In recognition of both traditions, we attempt here to advance a sociologically self-aware discussion of replication in social research: that is, a review of issues regarding replication in social science that is informed by and consistent with the contributions science studies have made regarding scientific cultures and replication. We begin by synthesizing the key insights regarding replication produced in the sociology of science. We then use these ideas to organize and consider some issues that have emerged in discussions about replication in quantitative social science. We spend the bulk of the review on quantitative social science because qualitative inquiry poses very different issues about which replication may not even be the best term. We do discuss these issues in relation to qualitative social science later in the article.

SOCIOLOGY OF SCIENCE ON REPLICATION

Replication played a central role in the theories of science popular in the mid-twentieth century. Philosophically, replication was considered essential to science because theories could be “falsified only if we discover a *reproducible* effect which refutes the theory” (Popper 1959 (1992), p. 66, emphasis ours). Sociologically, the possibility that one’s work could be replicated provided a social control mechanism that discouraged shoddy or fraudulent work. This was articulated as part of the norm of organized skepticism taken as key to the successful functioning of science (Merton 1973, Zuckerman 1977).

However, empirical studies of replication attempts and controversies in the natural sciences presented a more complex image. For one, many indisputably successful sciences rarely perform exact replications (Hacking 1983). Replication often involves craft knowledge that is not in published reports (Collins 1974, 2001; Mulkay 1984). Even for experts, original studies often do not provide enough information for replications to occur (Travis 1981). Instead of a neutral verification process, replication is sometimes wielded strategically to undermine adversaries (Pinch 1979).

This work makes clear that replication in practice does not operate the way an abstract, standard view of science suggests. But, then, how should replication be understood? For us, a sociologically consistent view of replication in social science emphasizes four things. First, replication contains a series of unavoidable interpretive ambiguities. Second, these ambiguities are partially rooted in

a tension between the epistemic values of similarity and difference in replication. Third, scientific communities develop rules and conventions around replication that are meant to alleviate these ambiguities but can themselves become objects of epistemic debate. Finally, because these rules and conventions develop in path-dependent response to both internal and external pressures, it is important to recognize the particular challenges that each epistemic culture faces rather than accept a universal theory of scientific replication.

Ambiguity and Interpretation in Replication

Consider the case in which a scientist sets out to determine if a published experiment can be replicated. If the replication fails, it may indicate a problem with the original result, but there might be something wrong with the replication. Or perhaps the failure is caused by some subtle difference that, if understood, would represent a scientific discovery in its own right.

Given this ambiguity, one might conclude that we can never know whether a replication study has been properly done unless it works. Yet, if replications are only interpreted as evidence when successful, this would imply that they cannot be properly considered to be testing anything. Thus, we need some way of judging the quality of a replication that is independent of its outcome.

This circle is known as the experimenter's regress. It was first described by Harry Collins, whose work over more than four decades represents science studies' most significant contribution to understanding replication (e.g., Collins 1974, 1985, 1991, 2001, 2016). To Collins, judgments about experiments are necessarily interpretive matters, and the prevailing interpretations in a field are the outcome of social interactions among scientists.

The conclusion fits the broad position of constructivist science studies that "there can never be strictly logical grounds for forsaking a theory" (Yearley 2005, p. 30). If logic alone is never sufficient, then it falls to sociologists and historians to help understand how scientific controversies are eventually resolved. If replication is always potentially subject to doubt, sociologists and historians must "show how communities come to discriminate between reasonable and unreasonable doubts" (Panofsky 2014, p. 160). Scholars of science have continued to explore issues of replication by extending Collins's framework into new areas (e.g., Kennefick 2000) or highlighting the emerging challenges for replication brought about by technological or institutional developments in scientific practice (e.g., Edwards et al. 2011, Leahey 2008).

The constructivist position on replication has been the target of criticism by scholars who emphasize that replication—even if infrequently done, technically challenging, and ambiguous—plays an irreducible role in shaping scientific arguments (Cole 1992, Kitcher 1995). Although Collins may dispute the independent power that replications hold, he admits that the ideal of replication is a lodestar, not just for natural science but even in social sciences in which replication presents additional challenges. Thus, he argues that "the job of the social sciences is not to show that replication is futile or impossible, but to show how to pursue replicability in the face of its recalcitrance" (Collins 2016, p. 78). For Collins, even as replication involves interpretive ambiguities and social negotiations, it remains "the only criterion [we have] of what is to count as a natural regularity (or social regularity)" (quoted in Ashmore 1989, p. 138).

Similarity Versus Difference in Replication

Central to Collins's contribution is his demonstration that replications are characterized by an unavoidable tension between the perceived epistemic merits of maintaining similarity to the original study and the perceived merits of difference. Everyone recognizes that one cannot conduct exactly the same experiment on exactly the same subjects at the same time, but differences are sometimes understood as a limitation to be minimized and other times as a virtue.

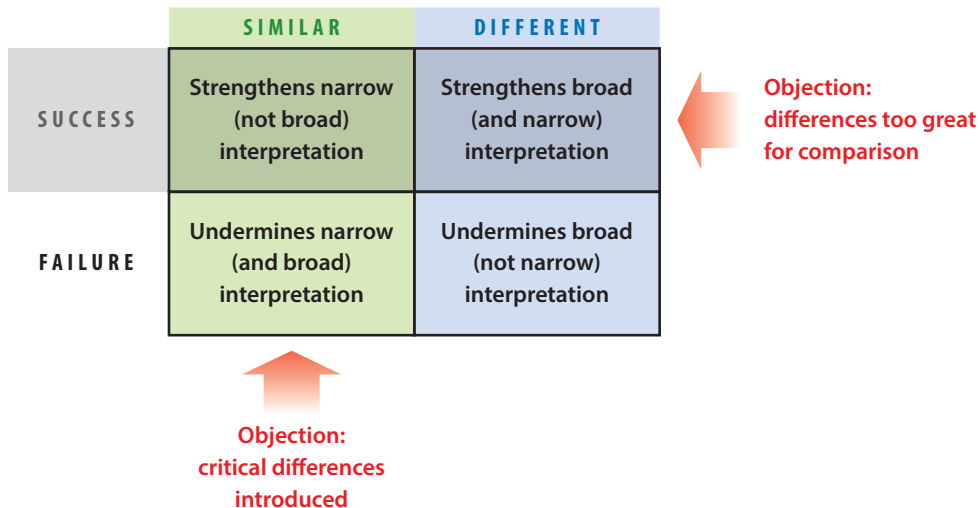


Figure 1
Replication success and similarity on interpretation.

In **Figure 1**, we illustrate how similarity and difference in replication design and success and failure in replication outcome affect both narrow and broad interpretations of a finding. Here, a narrow interpretation of a finding sticks closely to the literal results and conditions of the study, whereas a broad interpretation speaks in terms of whatever theory or larger generalization the finding may be taken to support. When a replication attempt is extremely similar to the target study, it adds little additional information beyond evidence that the result of the first experiment was not simply a fluke (Collins 1985, p. 34). It will not, for instance, aid in adjudicating between competing hypotheses. In terms of **Figure 1**, success strengthens the original results in a narrow sense but, beyond this, does not add anything to our confidence of a broader interpretation of the findings.

The more ambiguous situation occurs when a replication designed to mimic the original study fails. As mentioned above, this may cast doubt on both the original study’s literal findings and its broader implications. However, as we outline below, authors of an original study will often discount such failures by arguing that critical changes were introduced that vitiate the new study’s interpretation.

As a replication introduces greater contrast from the original experiment—for example, if it is performed in a new location or using new methods—more information may be gained. However, when a replication that differs in key respects from a target experiment fails to replicate, it is easy for authors to attribute the failure to those differences because the replication knowingly altered the original design.

More ambiguity arises when such a replication succeeds. On one hand, the broader interpretation is strengthened, which also increases confidence that the original study was correct. On the other hand, as noted above, as a replication becomes more dissimilar, it becomes increasingly difficult to interpret. Thus, critics can argue that such replications are too different and no longer speak to the original finding.

Across science, debates over replication arise along the similarity/difference, success/failure axes. This has proved a useful tool for social scientists who study scientific conflict. Yet this general schema says little about the specific ways that scientific fields manage such debates. Additionally, because most science studies work on the topic has concentrated on experimental research, it may

say little about the particular issues that arise in social sciences in which observational studies predominate.

Epistemic Cultures and Replication

Among the fundamental shifts that the second wave (Collins & Evans 2002) of science studies brought was to stop viewing science as a unitary activity. As Shapin (2010, p. 5) explains, scholars have come to believe that “science is not one, indivisible, and unified, but . . . many, diverse, and disunified.” Even a seemingly universal notion such as objectivity has evolved different historical forms (Daston 1992, Daston & Galison 2010, Porter 1995). Science studies has highlighted how scientific communities are characterized by methods, theories, and practices that developed in path-dependent ways, yielding unique epistemic cultures (Galison & Stump 1996, Hacking 1996, Knorr Cetina 1999).

Thus, the role of replication within a field ought to be understood as the outcome of a process of cultural development which is influenced by both internal dynamics and external pressures rather than a universal feature of an idealized scientific method. Epistemic cultures may include replication rules, which are explicitly stated policies regarding replication expressed by professional associations, journals, and funding agencies. These include journal rules about what information about a study must be reported and rules surrounding sharing data and materials.

In addition to explicit rules, however, replication conventions involve all noncodified knowledge about replication. Examples include shared understandings of what it is appropriate to ask another researcher for, what studies are worthy of replicating, when replication becomes considered an attack, and so on. Within a field, of course, researchers may have different ideas about what these conventions are, and that such differences exist may become recognized only in episodes in which some perceive a breach.

The success of scientific communities in achieving their ostensible epistemic goals is up for debate by both those within a field and outsiders. When the replication rules and conventions of a field are deemed problematic, activists may seek to change them. Recent years have seen so much activism about replication that it is characterized regularly as a movement (Finkel et al. 2015). This has involved efforts to change existing rules, change prevailing conventions, and, especially, codify matters that had been left to convention as explicit rules.

Integrative Replication

In fields characterized by rapid technological development, replication is often piecemeal and informal. As new techniques and technologies are developed by other labs, there are quick attempts to integrate them into practice to stay at the cutting edge of a constantly receding horizon of experimental possibilities, a process referred to recently as bench-building (Peterson 2015). For instance, if a molecular biologist publishes a finding based on a new strain of mouse she developed, other molecular biologists may ask to use the strain, not to explicitly replicate the experiment or evaluate its authors’ factual statements but to play with and, possibly, integrate the technology into their practice. In this case, replication occurs as a byproduct of bench-building rather than an explicit goal in itself.

This form of integrative replication represents the major form that replication takes in laboratory science. This is why Hacking (1983, p. 231) dismisses the focus on repeatability as a “philosophical pseudoproblem” because “roughly speaking, no one ever repeats an experiment. Typically, serious repetitions of an experiment are attempts to do the same thing better—to produce a more stable, less noisy version of the phenomenon.” For Hacking, most replication

occurs as a natural product of developing the technological capacity of the lab rather than as an explicit test for truth. Hacking famously distinguished intervening from representing, arguing that philosophers have been too concerned with whether scientific facts properly represented nature while ignoring the importance of actually intervening in nature [an argument furthered in the sociology of science by Pickering (1995)].

In fields that rarely intervene—the situation of the vast majority of social science—replication is instead necessarily concerned with the accuracy of representations. The difference to us seems not to reflect any immaturity of social science but inherent differences in subject matter: Social science is not centered on the development and deployment of (literal) machinery to (literally) manipulate (literal) material objects. We emphasize this here because so much discussion of what social scientists should be doing proceeds from the premise that natural science provides the best role model. For replication, we think that most replication work in the natural sciences is simply incommensurate with what social scientists are trying to do, resulting in very different replication rules and conventions. Consequently, normative questions about replication for social scientists need to be understood on their own terms.

FORMS OF REPLICATION IN QUANTITATIVE SOCIAL SCIENCE

As with the natural sciences, replication in quantitative social science has been used to characterize a variety of different activities. Clemens (2015) impressively catalogues a crazy quilt of overlapping and not-infrequently incommensurate terminological distinctions that past authors have presented regarding replication. Our discussion here is organized by whether the replication attempt is based on the original data or gathers new data and the tension between similarity and difference described above.

We end up with four analytically distinct categories (**Figure 2**). (a) Verifiability involves taking the results of an original study as the object of inquiry and asks limited questions regarding whether the same results are obtained by doing the same analyses on the same data. (b) Tests of robustness conduct a reanalysis on the original data using alternative specifications to see if the target finding is merely the result of analytic decisions. (c) Tests of repeatability involve collecting *new* data to determine whether key results of a study can be observed by using the original procedures. (d) Finally, for inquiries into generalizability, the original study provides a premise for research trying to evaluate whether similar findings may be observed consistently across different methods or settings.

Verifiability

We use verifiability to describe activities directed at interrogating whether the findings presented in a study follow properly from the study's data. The most straightforward version is someone

	SIMILAR	DIFFERENT
OLD DATA	Verifiability	Robustness
NEW DATA	Repeatability	Generalization

Figure 2

Forms of replication in quantitative social science.

determining whether they can reproduce results using what are understood to be the same data and same code. For some, this might seem so pedestrian as to not even count as replication (Collins 1991); to others, it is pure replication (Brown et al. 2014). Either way, the recent experience of the *American Journal of Political Science* in actually using a third party to do this for articles it publishes indicates that it is not to be taken for granted. The first 15 articles all required at least some additional information from authors after data and code supposedly sufficient to verify results were submitted (Janz 2015).

Even when results can be reproduced, the data or code may contain errors. A graduate student discovered that findings by Reinhart & Rogoff (2010) that had been used to support austerity policies were artifacts of a spreadsheet error (Herndon et al. 2014). Even if all the analyses are correct, problems may exist in the raw data. In quantitative social science, data are usually not collected by a study's authors, and a complicated chain of actors may separate the origins of data from its analysis. For instance, skepticism of findings by McPherson et al. (2006a) about increasing social isolation in the United States led to the discovery of 41 people about whom the survey center providing the data had coded incorrectly as having no confidants (McPherson et al. 2006b).

Independent verification of results is possible only if data are available to others. Researchers who can keep anyone else from verifying results are insulated against any definitive demonstration of error. Efforts to verify results after publication may be the most interpersonally fraught type of replication because they are often interpreted as signaling distrust, and it may raise the possibility of the researcher's honesty or competence being called into dispute. As described in Tischler (2007), Peterson (1996a,b) had been thwarted for several years in his effort to verify Weitzman's (1985) widely reported finding that women's standard of living declined over 70% in the year following divorce. Peterson was allowed access only after intervention from the National Science Foundation and found that the finding could not be reproduced (his own estimate was 27%).

The convention in sociology has largely been to treat the availability of data for verification as an ethical matter to be handled by individual requests. Systematic inquiries in other disciplines have found widespread failures of investigators to respond to postpublication data requests (McCullough et al. 2006, Parish et al. 2015, Wicherts et al. 2006). In sociology, Young & Horvath (2015) reported more than half of authors have refused to provide data or code to graduate students trying to verify 53 findings as a class exercise. In economics, a systematic inquiry revealing poor response to data requests led the *American Economic Review* to impose data-sharing guidelines at the time of publication and monitor authors' compliance with them (Bernanke 2004).

Replication rules by journals stipulating transparency regarding data availability at the time of publication have spread over the past decade in social sciences other than sociology. Authors are usually urged to deposit data in independent repositories, developed and maintained by archiving professionals, rather than leaving long-term data availability contingent on authors maintaining their personal websites. Major repositories include the Open Science Framework, openICPSR, and Harvard's Dataverse initiative.

Although activists sometimes treat openness as an unambiguous good, requiring social scientists to make data available as a condition of publication raises various concerns about confidentiality, proprietary sources, and compromising future intellectual property rights of investigators. The latter is not unique to social science: A *New England Journal of Medicine* editorial noted concerns that clinical research could be taken over by "research parasites" whose reliance on others' data was "possibly stealing from the research productivity planned by the data gatherers" (Longo & Drazen 2016, p. 276).

Numerous ideas to address these concerns have been advanced, including redactions, embargos, and improved data citation practices to give those who collect data greater credit when they share it. More fundamentally, however, it seems almost certain that any feasible mandate for

sharing data upon publication in social science must include possibilities for exemption. One-third of the articles in the *American Economic Review* in 2014 were exempted from its data-sharing requirements (Goldberg 2015). When authors articulate explicitly why data cannot be made available, the justifications may be evaluated by reviewers or approved by editors. Major political science journals encourage authors to provide information about data availability in the first footnote (starting with Meier 1995). The larger point is that good reasons may be given for why data cannot be made publicly available, but it is hard to argue against expecting researchers to be transparent about what the conditions of data availability are so that readers can know whether work might be verified independently in principle when assessing its credibility.

Data access is only one part of transparency relevant to verification. Guidelines by political scientists have highlighted what they term production transparency and analytic transparency, with the former involving documentation of data collection and preparation processes and the latter involving documentation of steps by which findings are derived from data. Authors may believe the details in their articles are sufficient, but efforts to reproduce findings have resulted in divergent outcomes. For example, Breznau (2015) reports that Brooks & Manza (2006) declined requests for information to help reproduce findings from publicly available data that they published in the *American Sociological Review*, but Breznau's efforts based on his reading of the article led to estimates that were not especially close to the original.

Page limits once severely restricted the information authors might be expected to provide about their research procedures, but online supplements and the ability to link to online sources have eliminated technical barriers. In *Science*, a group of mostly behavioral and social scientists published a set of Transparency and Openness Guidelines that journals could adopt, tiered into three levels of stringency (Nosek et al. 2015). Whether fields do so, or press for increased transparency in other ways, is now a matter for social and political settlement within epistemic cultures.

No particular movement to improve transparency in sociology is known to us at present. The longtime editor of one of sociology's flagship journals offered one of the few arguments we have seen for how transparency in research procedures could be scientifically bad: "There may be something very useful in not having clarity in data conventions and even in having code mistakes hidden from inspection. Maybe those kinds of things are necessary to produce (false) findings that lead us up out of local optima . . . in the great state space of truth" (Abbott 2007, p. 218). Yet external pressures for more transparent and reproducible research practices in sociology may increase, especially for interdisciplinary-minded sociologists, as expectations in neighboring social sciences increase. Fortunately, available guidance for doing reproducible research has increased enormously in recent years (Long 2009; see also the collection of course syllabi at <https://osf.io/vkhbt/>).

Standards that make work more verifiable are sometimes opposed on the grounds that insufficient resources exist to actually do the verification (e.g., Abbott 2007). Transparency advocates stress that much of the scientific benefit of transparent practice is likely realized even if verification rarely occurs. The practices involved in making work transparent to others may improve the work by pressing researchers to be clear about the rationale for their own practices. It may also make work more efficient insofar as documentation that makes work transparent to others is likely the best way of ensuring it will be able to be understood by collaborators and, perhaps most urgently, by oneself later in the long gestation that characterizes many research projects (Bowers 2011).

Robustness

Researchers may reexamine data from a published result to perform sensitivity analyses or robustness tests beyond those reported. This work seeks to assess whether key results are observed consistently across alternative analyses. Freese & Powell (2001) find that any of a series of plausible

alternative analyses to those presented by Kanazawa (2001) yield results that are greatly attenuated and no longer statistically significant. Young (2009) demonstrates that Barro & McCleary's (2003) findings about a nation's religiosity and its economic growth change considerably under small changes to model specification. And Abascal & Baldassarri (2015) argue that Putnam's (2007) findings regarding lower trust in ethnically diverse communities are an artifact of general low trust in nonwhites and the overrepresentation of nonwhites in diverse communities.

An abiding concern is that published findings represent a best-case scenario among all the arbitrary and debatable decisions made over the course of analyzing data. This might be due to p-hacking, in which a researcher runs different analyses until they find support for their preferred hypothesis (Simonsohn et al. 2014), or to HARKing, in which hypotheses presented as a priori were actually developed after results were known (Kerr 1998). Even when authors of an original study report extensive robustness tests of their own, one might still worry that researchers are biased toward reporting such tests when they strengthen findings but not when they weaken them. None of this necessarily requires any conscious deceptive motive, but the result is that reported *P* values may greatly understate the prospects that the observed finding was simply due to chance.

Researchers have introduced various proposals to provide ways of reducing latitude for p-hacking or HARKing (e.g., Nosek et al. 2012, Wagenmakers et al. 2012). Journals have been encouraged to articulate specific standards for what researchers are expected to disclose about the analyses they have done—for example, whether outliers were dropped or subgroup analyses were performed—and journals may even provide a checklist in which authors must affirm they have followed the journal's expectations regarding disclosure (Eich 2014, Nosek et al. 2015). The website <http://www.equator-network.org> collects guidelines that have been offered for health research. Reporting guidelines do not prevent deception, but making disclosure a matter of rule draws a line where otherwise there could be disagreement about convention (Freese & Peterson 2016, Frow 2012).

In experimental fields, there has also been movement toward preregistering studies by archiving design and analysis plans publicly beforehand, which allows authors to demonstrate that the hypotheses really were a priori and that the presented analyses were not cherry-picked based on results (Gonzales & Cunningham 2015). Preregistration allows researchers to allay one type of potential distrust in their accounts of the research process. It does not displace the need for trust entirely: The biggest fraud scandal of recent years in political science, notably, involved a field experiment that was preregistered, but the ensuing data were simply fabricated (LaCour & Green 2014).

Reanalysis of original data may also be done when someone believes they have a better way of analyzing data than what was done in the original study. Killewald & Bearak (2010) argue that Budig & Hodges's (2010) study of the motherhood penalty should have used unconditional quantile regression instead of conditional quantile regression, and that this change dramatically alters the study's main result (see also rejoinder by Budig & Hodges 2014). Lall (2016) reanalyzes every comparative political economy study published in two journals over a five-year period and finds that key results become nonsignificant when multiple imputation for missing values is used instead of listwise deletion. Of course, such work is only possible when data from the original study are available to other investigators. Thus, providing the material to explore possible improvements in technique is another justification for encouraging researchers to make data and code available.

Repeatability

Of course, replication involves more than reanalyzing old data. Concerns about potential replication crises in experimental psychology and biomedical research have focused on whether reported effects can also be observed by other scientists collecting new data. For example, Bem (2011)

published a paper in a leading social psychology journal reporting a series of experiments providing apparent evidence of the paranormal phenomenon of precognition. Ritchie et al. (2012) report three separate efforts to follow the protocols of one of these experiments exactly; none of these found any evidence of the reported effect.

Although this exchange may seem to exemplify ideals of scientific replication, it was actually a key episode in raising alarms for many that the replication conventions in social psychology may be problematic (e.g., LeBel & Peters 2011). For observers who flatly reject paranormal claims, that standard methods were used to generate evidence for precognition served as a high-profile demonstration of the vulnerability of standard methods to false positives. More dramatically, the Ritchie et al. (2012) study was also desk-rejected by the journal that published the Bem (2011) article on the grounds that it did not publish replication studies. To critics, the combined lesson was that ludicrous findings could find a prominent home in psychology journals, whereas refutations of those findings could not.

With the Bem (2011) study, critics-of-the-critics can wonder whether replications were truly faithful to the original study in key details; the phenomenon could depend on some condition no one understands yet. Moreover, Ritchie et al. (2012) seemed to undertake the replication because they were skeptical. This skepticism could have led them to be careless about details—thinking that they could not matter anyway, as they believed the phenomenon to be nonexistent. Moreover, those trying to replicate original findings may have more to gain from a failed replication than a successful one.

Recent activism in social psychology has led to policy changes at many journals regarding the publication of direct replication studies. Yet formal changes to replication rules do not resolve the difficulties in interpreting attempted replications. Indeed, when a large-scale effort to repeat 100 experiments published in three psychology journals found that only 39% replicated successfully (Open Science Collaboration 2015), a different team reanalyzed the Open Science Collaboration data and argued that claims of a “replication crisis” were greatly exaggerated (Gilbert et al. 2016b), which led in turn to another round of disagreements (Anderson et al. 2016, Gilbert et al. 2016a).

Social psychology has been involved in numerous initiatives to strengthen the credibility of replication studies (Brandt et al. 2014). Preregistration, noted above, provides details about protocols in advance and may address concerns about researchers making decisions as data are being analyzed that are biased toward their favored results. Additionally, researchers involved in the original study may be asked to evaluate protocols before the new data are collected (see Kahneman 2014 on replication etiquette). The preregistered experiment can be made available for other researchers to join if they agree to adhere to the protocols, producing a data set potentially larger and more generalizable than the original study. Journals may be asked to accept these multilab replications before data have been collected, eliminating any perception that publication depends on the replication study’s results (Simons et al. 2014).

Although these initiatives have generated enthusiasm from activists, our familiarity with science studies’ lessons about replication makes us unsurprised that they have not been as powerful for generating consensus as some advocates may have hoped. For example, a recent large-scale replication effort involving 23 labs and over 2,000 subjects failed to replicate results of a previously published experiment finding evidence of the phenomenon of ego depletion (Baumeister et al. 1998, Hagger & Chatzisarantis 2016). Afterward, two social psychologists closely associated with ego depletion argued that “we understood our approval [of the project’s design] to mean ‘Sure, go ahead’ and not ‘Yes, that’s a definitive test of the phenomenon we’ve been studying all these years.’” (Baumeister & Vohs 2016, p. 574).

In their view, ego depletion had been demonstrated by a variety of different experiments in different domains, and the failure to replicate this one experiment, no matter how comprehensive,

did not undermine all these results. In social psychology, this has sometimes been characterized in terms of a debate over the value of conceptual replications, which are deliberately different studies intended to test an existing theory in a novel way. Conceptual replications overcome the problem of replications being devalued as noncreative work by allowing researchers to develop imaginative ways to test existing theories. But conceptual replications have been criticized because, when they fail, they are difficult to publish because of the chronic ambiguity regarding whether they actually speak to the theory in question.

Generalizability

Of the different forms of replication we discuss, repeatability likely accords best with commonplace understandings of replication. Nevertheless, across quantitative social science discussions of replication, repeatability per se is noticeably de-emphasized relative to the reexaminations of original data, on the one hand, and inquiries directed to the generalizability of findings on the other (see, e.g., Lucas et al. 2013, Wilson et al. 1973). The former is illustrated in the first paper to describe sociology as having a “replication problem”; Wilson et al. (1973, p. 141) present replication specifically with respect to generalizability, stating, “It is not the task of the scientist to uncover truths specific to the particular experimental setting he employs. A key test of the generality of scientific knowledge is that of replicability.”

The emphasis on generalizability is also evidenced in the frequency with which social scientists use replication to refer to standardizing measures or holding constant parts of a research design to facilitate comparison. For example, the General Social Survey keeps the same wording of core items year after year, and the International Social Survey Program coordinates the asking of the same survey questions across member nations. Panel studies ask respondents the same questions, and studies of social continuity or change have visited the same sites in different eras. All these follow, in different ways, a dictum we have heard attributed to Otis Dudley Duncan: “If you want to measure change, don’t change the measure.” The most recent *Annual Review of Sociology* article ostensibly about replication focused predominantly on such issues, particularly in the context of collecting longitudinal data (Bahr et al. 1983).

We do not think this reflects the unusual epistemology of quantitative social science so much as the typical sorts of data used. Most quantitative social science uses data that were not collected by the authors of the paper and were instead assembled as large-scale public goods for a research community. The nature of these data sets is that often they are uniquely suited for the questions posed using them. Their unique potential is part of what justifies the expense of their existence. Even when the same question can be put to multiple such data sets, the data sets differ in ways that are prominent to the understanding of any findings.

Even when social scientists do collect their own data, these data often exhaust the narrow scope of their results in a way that is not compatible with repeatability as a subsequent research goal. To illustrate, Smith & Papachristos (2016) present findings from data they assembled of 3,000 persons connected with organized crime in the Chicago area from 1900 to 1950. Regarding the replicability of the conclusions, we might imagine that someone could challenge their results by asserting problems with their analysis or with the data, and the latter could even involve someone redoing some of the work involving primary sources. We can also imagine someone challenging the conclusions using data from organized crime elsewhere. What is *not* possible is trying to see if the results are observed again by repeating their research with an independent sample of 3,000 other organized crime figures in Chicago in the same period.

Of course, the examples of repeatability from psychology in the preceding section are also not studying the same population at the same time. Occasionally, these differences come to the

fore when understanding differences in replication (Schnall 2014), but we suspect sociologists may be surprised by how much they do not. Our own impression is that, when psychologists are faced with divergent results from studies asking the same question, they tend to first consider how the designs of the two studies are different, whereas sociologists in the same situation tend to first consider how the samples for the two studies differ. Experimental psychologists regularly use deliberately nonrepresentative samples that are interesting only because they are presented as broadly illuminating about generalized human psychological processes; sociology focuses more on population samples or historical cases for which results might be taken as instructive even if they do not generalize farther than their target population.

Connecting particular findings to general conclusions is often done by reference to an animating theory that is supported by the original study's results but also asserts broader application. When Walker & Cohen (1985, p. 288) write that "every general sociological proposition is both true and false," they mean that general propositions are true under some conditions and false under others, a position elaborated by Cartwright (1999). To the extent this is true, what empirical work tests is not so much propositions as such but ideas about the scope in which the propositions apply.

For example, the fundamental cause theory of disease proposes that the inverse relationship between socioeconomic status (SES) and health emerges as a result of differential returns to advancing knowledge about how to prevent and treat disease. Evidence presented for the theory includes evidence of SES and health for causes of death that have become preventable and the absence of such relationships when causes are absent (Link & Phelan 1995, Lutfey & Freese 2005). Meanwhile, strong socioeconomic gradients in diseases that preceded accurate understanding of their causes or prevention, such as what existed in Britain during early cholera outbreaks (e.g., Johnson 2006), would indicate some need to amend the theory.

When replication is formulated in terms of evaluating generalizability, the advantage of designs that introduce differences seems plain (Lucas et al. 2013). Showing that a relationship between SES and diabetes mortality is repeatedly observed does little to advance a broader understanding of the scope over which SES negatively affects health. In the case of well-defined scope conditions, diverse findings may be said to be replicated when consistent with the expected scope. In practice, scope conditions are often not well defined, and, regardless, findings inconsistent with existing notions about the scope of a proposition seem common and provide one way new findings offer innovations on existing understandings.

The chronic ambiguity of generalizability is whether explanations revised to accommodate new results constitute a legitimate advance or are simply ad hoc explanations that mask deeper problems with the validity of some of the studies in question. Ultimately, social scientists must resolve such questions on a case-by-case basis. A general lesson, however, is that worries about the basic validity of original findings often lead to efforts to repeat studies. To the extent this avenue is often foreclosed in social science, it is understandable that one might witness greater concern at least for seeing if findings are verifiable. Beyond this, one might suppose such enterprises would have ongoing difficulties resolving various ad hoc explanations of inconsistent results.

REPLICATION IN QUALITATIVE SOCIAL SCIENCE

The flip side of saying replication is essential to science is that replication may be viewed as a shibboleth of scientism in social inquiry. The editor of a political science journal that focuses on qualitative research recently refused to adopt transparency standards proposed for the discipline because he viewed them as embodying a "neo-positivism" in which political science becomes "not a never-ending contest between perspectives on politics but instead about the veridical understanding of the world as a set of objective processes" (Isaac 2015, pp. 275–76). For qualitative sociology,

Abramson & Dohan (2015, p. 273) note that methodological discussions about replication are complicated because qualitative researchers “do not necessarily share basic epistemological assumptions about the research enterprise with either their quantitative colleagues or one another.”

Qualitative social science is often presented as firstly an interpretive enterprise. In sociology, concern with issues of motivation and meaning have given the *verstehen* approach lasting appeal. It may be that replication is simply the wrong language to apply to qualitative studies. Attempts to make qualitative research more scientific through intermediate steps such as rigorous coding might only obscure unavoidable interpretive work (Biernacki 2012). Those who disagree will need to be more explicit about what replication means in fields defined by thick forms of evidence.

Sometimes cited as a counterpart to failed replication in qualitative research is the famous episode in which Derek Freeman (1983, 1999) disputed Margaret Mead’s (1928) classic anthropological study of adolescent sexuality in Samoa. But this work did not involve Freeman trying to deliberately redo Mead’s fieldwork; rather, he drew on his own expertise on Samoa and revisited some of Mead’s sources. More to the point, Freeman’s critique largely circumvents the difficult issues surrounding interpretive inquiry by accusing Mead of simply being wrong on objectively available facts. Although this bears some affinities to verification as discussed above, the better analogue to any sort of replication may be fact-checking in journalism: that is, determining whether details presented as matters of objective fact can be independently verified.

However, fieldwork methods often involve considerations of confidentiality that prompt researchers to use pseudonyms and even change details to prevent identification of parties or settings, which can strongly limit possibilities for fact-checking. Anonymization practices are routine in fieldwork studies, but their use has also been occasionally questioned for the lack of researcher accountability (Duneier 1999, Murphy & Jerolmack 2016). Desmond (2016) recently offered an alternative. He hired an independent, named fact-checker who was given access to otherwise confidential materials.

Besides limiting fact-checking, obscuring details may constrain the ability of work to be extended by others or to be subjected to alternative interpretations. Murphy & Jerolmack (2016) note an example in which Bosk (2008) reflected on his classic ethnography of medical errors from three decades earlier. Because he changed the gender of the only female medical resident in the group he studied, others were not able to consider whether gender had shaped interactions.

Once one moves from “brute data” (Taylor 1985, p. 118) to matters of interpretation, the possibilities for fact-checking become murkier. Of course, there is the question of whether others who had access to the same materials would have interpreted them in the same way. For archival research, for instance, questions of whether interpretations of primary sources are idiosyncratic are longstanding and potentially addressed by other experts having access to the same archives. One recent suggestion is active citation, in which authors provide far more detail to citations, including very specific hypertext links to the original materials for individual claims (Moravcsik 2010).

In regards to ethnography, Abramson & Dohan (2015) offer a new graphical display, the ethnarray, which, they argue, may aid ethnographic replications by providing more explicit systems for coding evidence. Hammersley (1997) is encouraged by the potential for growing archives of ethnographic data to aid in the assessment of claims and support reanalysis, but he cautions against the idea that ethnographic findings can be audited in a way equivalent to quantitative data sets. He argues that field notes are open to different interpretations and are a part of a process that occurs largely within the original writer’s head.

Importantly, the usual premise in qualitative research is that the understanding of any piece of primary material might not be correctly understood solely from that material. Rather, it requires a larger understanding of the setting that the researcher has gained over the extensive course of their project. As Collins (1998, p. 299) puts it, “Interview extracts illustrate what analysts understand to

be going on as a result of their experiences in the field and are used to convey this understanding to a less expert audience; they are not thought of as data.”

Small (2009) provides an influential argument for the centrality of the analyst in qualitative research. He advocates an iterative procedure in which qualitative researchers replicate observations through several distinct cases (following language of Yin 2002). After each case, the research questions evolve to become more sophisticated. The process ends once the researcher subjectively feels saturated and believes that additional sites will yield little new information.

Although Small (2009) uses the language of replication, the procedure he advocates is very different from the forms of evaluative replication that we describe above. As Small points out, qualitative data collection often changes as one’s questions evolve. Unchanging and ubiquitous aspects of a site disappear from field notes as the analyst’s interests become refined. Subtle phenomena emerge and become central topics. The presence or absence of some phenomena in field notes is meaningless because they are not an unbiased record of a site. Unlike most forms of quantitative data, field notes lose much of their value when divorced from the note taker.

CONCLUSION

This article has sought to bring together sociological research on replication in science with recent discussions about improving replication practices in social science. It should be clear that none of the four forms of replication in quantitative sociology completely overcome the issues of ambiguity and interpretation that sociologists of science have observed. We conclude by offering a typology of the ambiguities that the different types of replication described above confront and then by reflecting briefly on what we consider the primary normative takeaway for replication policies in our own discipline of sociology.

The Routine Ambiguities

We presented replication in quantitative social science as divisible into four goals—verifiability, robustness, repeatability, and generalizability. In each case, the interpretation of results from replication efforts confronts routine ambiguities that often serve to structure professional debates about contentious replications (Figure 3).

Verifiability gives rise to the ambiguity of intentionality. Because verification is directed so specifically to the details of another’s work, interpretation of verification problems often raises questions about the intentions of the authors of the original study. When failures are due to problems in the original studies, should they be interpreted in terms of dishonesty, incompetence, or carelessness—that is, as something that should have reputational consequences? Or does it just reflect the occasional bugginess that researchers recognize as a pragmatic reality of their craft?

	SIMILAR	DIFFERENT
OLD DATA	Ambiguity of intentionality	Ambiguity of judgment
NEW DATA	Ambiguity of similarity	Ambiguity of difference

Figure 3

The four routine ambiguities.

Tests of robustness can result in an ambiguity of judgment. When reanalyzing old data under new specifications, the debate often turns on whether certain decisions were appropriate and whether the consequences of these decisions ultimately matter for the conclusions that are drawn. This raises the potential for expert disagreement on whether such choices represent incorrect practice or a defensible exercise of professional discretion.

When a replication attempts to collect new data, the ambiguities center on Collins's discussion of similarity and difference. The ambiguity of similarity can emerge from tests of repeatability. On the one hand, attempts to repeat an outcome with new data can be met with charges that the data diverge from the original in ways substantial enough to undermine any equivalence. On the other hand, the attempt to generalize a finding can produce the ambiguity of difference because replicating in new environments or using new methods can weaken the link between the replication and target study.

Replication Policy in Sociology

As sociologists, the most striking thing in reviewing recent developments in social science replication is how much all our neighbors seem to be talking and doing things about improving replicability. Reading economists, it is hard not to connect their relatively strict replication culture with their sense of importance; shouldn't a field that has the ear of policy makers do work that is available for critical inspection by others? The potential for a gloomy circle ensues, in which sociology would be more concerned with replication and transparency if it were more influential, but unwillingness to keep current on these issues prevents it from being more influential. In any case, the integrative and interdisciplinary ambitions of many sociologists are obviously hindered by the field's inertness on these issues despite the growing sense in nearby disciplines that they are vital to ensuring research integrity.

At the same time, many sociologists pride ourselves on the way that being the most undisciplined social science promotes creativity. Discussions elsewhere have ended up with proposals for top-down regulation of research practice. We strongly suspect such regulation works best when the regulating entity (e.g., a journal, a professional organization) shares an epistemic culture. Cases in psychology and political science demonstrate that arguments about raising standards can be easily interpreted as treating particular types of research as the prototype and marginalizing others. Even when one epistemic culture is powerful enough to impose its regulatory ideas on others, doing so prioritizes one group's methodological progress over the many values of disciplinary pluralism.

By the same token, epistemic communities need to be secure and generous enough not to impede efforts at improvements by other communities with whom they share a discipline. Code- and data-sharing guidelines for quantitative sociology should move forward even if there is no counterpart for qualitative sociology.

For example, some journals in psychology and political science have adopted badges allowing papers that follow guidelines about certain practices—for instance, open data or preregistration—to advertise that they have done so. Different communities could, if they wish, develop their own badges reflecting what they regard as important. Pluralistic disciplines flourish when their constituents work together yet give one another the latitude to strengthen separately. Changing ideas about replication almost certainly present a case in which the different epistemic cultures of sociology need to help one another find their own ways.

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Contents

Prefatory Article

- A Life in Sociology
Robert M. Hauser 1

Theory and Methods

- Data ex Machina: Introduction to Big Data
David Lazer and Jason Radford 19

- Field Experiments Across the Social Sciences
Delia Baldassarri and Maria Abascal 41

- Genealogical Microdata and Their Significance for Social Science
Xi Song and Cameron D. Campbell 75

- Network Sampling: From Snowball and Multiplicity to
Respondent-Driven Sampling
Douglas D. Heckathorn and Christopher J. Cameron 101

- New Developments in Survey Data Collection
Mick P. Couper 121

- Replication in Social Science
Jeremy Freese and David Peterson 147

- Studying the Digital: Directions and Challenges for Digital Methods
Keith N. Hampton 167

- Theorizing in Sociological Research: A New Perspective, a New
Departure?
Richard Swedberg 189

Social Processes

- Decision-Making Processes in Social Contexts
Elizabeth Bruch and Fred Feinberg 207

- Social Networks and Macrosocial Change
Emily Erikson and Nicholas Occhiuto 229

- Toward a Sociology of Privacy
Denise Anthony, Celeste Campos-Castillo, and Christine Horne 249

Formal Organizations

- The Social Bases of Philanthropy
Emily Barman 271

Political and Economic Sociology

- The Demand Side of Hiring: Employers in the Labor Market
David B. Bills, Valentina Di Stasio, and Klarita Gërxbani 291

Differentiation and Stratification

- Categorical Inequality: Schools as Sorting Machines
Thurston Domina, Andrew Penner, and Emily Penner 311

- Gender Quotas for Legislatures and Corporate Boards
Melanie M. Hughes, Pamela Paxton, and Mona Lena Krook 331

- Graduate Education and Social Stratification
Julie R. Posselt and Eric Grodsky 353

- Wealth Inequality and Accumulation
Alexandra Killewald, Fabian T. Pfeffer, and Jared N. Schachner 379

Individual and Society

- Skin Color and Colorism: Global Research, Concepts, and
Measurement
Angela R. Dixon and Edward E. Telles 405

- The Development of Transgender Studies in Sociology
Kristen Schilt and Danya Lagos 425

Demography

- Social Structure, Adversity, Toxic Stress, and Intergenerational
Poverty: An Early Childhood Model
Craig A. McEwen and Bruce S. McEwen 445

- The Second Demographic Transition Theory: A Review and Appraisal
Batool Zaidi and S. Philip Morgan 473

Urban and Rural Community Sociology

- Ethnographies of Race, Crime, and Justice: Toward a Sociological
Double-Consciousness
Victor M. Rios, Nikita Carney, and Jasmine Kelekay 493

- Explicating Divided Approaches to Gentrification and Growing
Income Inequality
Japonica Brown-Saracino 515

Policy

The Social Safety Net After Welfare Reform: Recent Developments
and Consequences for Household Dynamics
Laura Tach and Kathryn Edin 541

Indexes

Cumulative Index of Contributing Authors, Volumes 34–43 563
Cumulative Index of Article Titles, Volumes 34–43 567

Errata

An online log of corrections to *Annual Review of Sociology* articles may be found at
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