

OVERCOMING OBJECTIONS TO OPEN-SOURCE SOCIAL SCIENCE

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I am truly honored to have a current and a former editor of sociology's two most esteemed journals comment on my paper proposing standards regarding replication for sociology. As Firebaugh fully endorses my proposal and Abbott expresses many reservations, I will focus here on Abbott's remarks. While I appreciate Abbott's thanks to me for "open[ing] a conversation" about replication, his gratitude is misplaced. After all, *American Sociological Review* published an argument twenty years ago for more demanding replication standards than what I proposed (Hauser 1987), which went nowhere. Firebaugh mentions an informal proposal of his own when he edited *ASR*, which also went nowhere. Not only do these instances suggest that sociology has been stuck "at the beginning of a long conversation" for a couple decades now, but they also raise the possibility that a lack of conversation may not be the reason sociology has failed to act. Alternative candidates for the pertinent deficiencies are a lack of will and a lack of leadership.

Perhaps some of the sentiments responsible for sociology's broader lack of will are evinced in Abbott's own skepticism toward a project he regards as "positivism" and his interpreting as "elitism" my presumption that articles in top journals may provide especially useful exemplars to students and colleagues. Nevertheless, Abbott recognizes that "Given the canons that quantitative sociology elects to follow, we should allow and indeed encourage replication and reanalysis. There is no question about that." The problems for Abbott have to do with details. As in my original essay, my task is thus not about convincing others about the benefits of policy change, but about answering objections. Forthwith:

1. *Won't your proposal raise issues for protecting confidentiality?* Abbott regards this as "the biggest practical problem with the replicationist position." While I am enthusiastic about

the potential of the Dataverse platform described by King (this volume), I do not see a future in which individual-level microdata collected by social scientists will be regularly available to all with just a few mouse clicks. User agreements are already required to access most of the data that Abbott rightly characterizes as responsible for most of the individual-level analyses published in top sociology journals. Fortunately, this is not incompatible with either a “replicationist” position generally or my proposal specifically. If confidentiality requires agreements and secure conditions in order to use data, then so be it. My proposal is that authors provide the code and dataset version information, so that others who fulfill the requirements to obtain access to the data can reproduce results. As long as conditions of access are only as onerous as needed to protect confidentiality, and confidentiality is not used as a device for preventing verification, then the need to protect confidentiality does not undermine proposals for making materials other than data available at the time of publication.

2. *Won't your proposal undermine individual incentives for collecting data?* Both King (1995, 2003, and this volume) and Firebaugh (this volume) closely tie the issue of replication standards to the issue of pushing authors to make their data available to others for any purposes, rather than just those closely tied to verification and re-analysis. I do not. Abbott is absolutely correct in noting that "Freese does not really have an argument" against embargoed data, but this is because Freese does not necessarily disagree with the principle of embargoing data. Rather, my position is that those who publish articles using data they own should be expected to articulate the terms of ownership in articles, especially as it pertains to whether the findings for which they seek assent and acclaim are available to the social science community for independent verification. If data are not available for this purpose, this should be said. If data will be available for this purpose, but not for 3/5/10 years, this should be said. While journals

might then wish to consider whether to accept any possible terms of ownership an author might specify, this is a distinct question from the *simple disclosure* regarding ownership that I seek. Abbott does not really have an argument for why reviewers and readers of articles in esteemed journals should not be able to know whether presented results can be independently verified.¹

3. *Won't your proposal require advances in the standardization of data formats and documentation?* Abbott describes how, in collecting data, “taking the dataset from 97% clean to 99% clean takes as long as it did to gather the whole thing.” With respect to replication, I would urge him and other colleagues to consider whether, if 97% of the benefits of transparency standards can be achieved with half the technology and effort, then perhaps 97% of a loaf is better than none. To be sure, it would splendid if all data, code, and other materials pertinent for replication were provided in standardized formats consistent with official metadata standards, etc.. In the meanwhile, transferring data from one statistical package to another is typically straightforward, the vast majority of published analyses use a relatively small number of statistical packages, and many of us are quite handy at working with a readme.txt file if the authors have been conscientious in attempting to document the contents of data or analysis files. Granted, many researchers may not now be documenting work sufficiently so that anyone but themselves can figure out what they have done. Experience indicates this is a dangerous practice, however, as research commonly involves setting aside and then resuming projects after enough time has passed that memory offers one little advantage over a stranger in reconstructing work. In this respect, any additional work demanded by the replication policy I propose is work that

¹ Moreover, Abbott's comments illustrate the point in my original essay that there exists no clear consensus among sociologists about what the minimal obligations for cooperation with attempts to verify results are. Various of Abbott's comments, e.g., about findings being "hidden from inspection," seem to indicate a stance that researchers presently have no such obligations, which contrasts with what myself and at least some others regard as the current standard in sociology.

one can strongly argue researchers should already be doing. Progress in sociology toward more transparent research is only "hostage to the larger process of standardization in data formats" if we choose for it to be.

4. *Won't your proposal lead to an "obligatory replication review"?* I recognize that, as a purely strategic matter, I may have erred in trying to rouse sociologists toward research transparency by raising the example of the policy that has been successfully implemented in economics (a policy far more demanding than anything I propose). My intention was akin to those who point to Canada or various European countries when trying to rebut Americans who claim that single-payer health care is practically unworkable. Abbott's main concern about the "mechanics" of replication standards are that they will "virtually require that somebody associated with the journal push the buttons and do the replication," leading to "obligatory replication review" and a whole new "equilibrium" of peer review and mentoring. This sky has not fallen in economics. Given that many of us believe that published results are already supposed to be verifiable in principle and upon request, it is unclear why having them verifiable in practice and upon publication should prompt either a sharp increase in anxiety among editors or a felt need for extra review, unless we fear that results accepted at top journals really would commonly and nontrivially fail the kind of superficial "push the buttons" verification that Abbott describes. My stance is that if we indeed are so skittish about the competence of work published at our top journals, this is *more* reason to encourage that this work be transparent to scrutiny, not less.

5. *Won't your proposal undermine methodological variety in quantitative research? And don't hidden mistakes sometimes end up being a good thing?* As noted, my hope was that the lack of catastrophe in economics upon adopting stronger replication standards might help counter

imaginative speculation about what would ensue if sociology made a much more modest move toward greater transparency. However, conversations with fellow sociologists have now made me suspect that—given the ongoing success of economics with popular audiences, top-tier undergraduates, granting agencies, and those in charge of professoriate salaries—the current collective psychology of our discipline may make it difficult for us to admit virtue in anything economics as an institution is doing that we are not. Predictably, perhaps, in defending sociology as the “more effective” social science than economics—the latter supposedly being in “crisis” from “sterility” and really stronger as “ideology”—Abbott finds ways of intimating that economists’ adoption of collective replication policies actually reflect that discipline’s weakness rather than strength.² Economists are willing to share their data because they have a blithe attitude toward data to begin with; their strong interest in “getting things right” only reveals their methodological sterility and lack of creativity. Granted, none of this has any discernible evidence in fact beyond Abbott’s say-so, but these arguments may be appealing if only for the balm they provide to the chronic insecurities of sociologists.

Abbott voices as an aside the familiar complaint that parts of economics are just “reinventing sociology,” but the charge of reinvention is less persuasive when economists can argue that they are using better methods and that those methods often lead to different results. Far from exemplifying diversity in quantitative methods, sociology has instead exhibited much more homogeneity than economics, especially in the extent of its dependence on the

² Elsewhere, Abbott (1997:1151) has said of sociology “our science has a tired feeling” and “[I]t has been a long time since we sociologists saw an idea that got us really excited, an idea that could transform our intellectual practice, an idea that could make us actually want to read the journals.” Having just completed a fellowship that put me in close working proximity to young economists, one of the things that impressed me most was their enthusiasm for their work and their craft. If this be the result of a more effective “ideology,” as Abbott might suggest, it is perhaps an ideology with positive spillovers for generating ideas and advancing methods.

comparatively weak apparatus of simple covariance adjustment to carry its inferential burdens. This homogeneity has aided economic imperialism, as whole literatures can be cast into doubt as suffering from a common flaw (e.g., unaddressed homogeneity), for which the econometric toolkit offers hope of remedy. It would be overly dramatic to claim that failure to have open research practices may soon become another argument by economists for rendering swaths of sociological contributions suspect. As a comparative marker to outsiders of the seriousness with which the disciplines regard themselves, however, it can't help, especially if lower transparency is defended with rationalizations as patently counterscientific as "There may be something very useful in having unclarity in data conventions and even in having code mistakes hidden from inspection." (To be clear: wrong *ideas* can be immensely useful in advancing the long-term goals of knowledge production. Wrong *findings* are another matter, and another matter still are wrong findings *whose basis is obscured or hidden so that the error is difficult to discover.*)³ As we all know, disciplinary jurisdictional claims in the social science findings market are becoming increasingly irrelevant, and sociology can either make at least modest efforts to step up to the challenge, or it can continue on toward being set aside, keeping our practices comfortably private while we grouse to one another about our unjust consignment to the intellectual junior varsity. Replication here is only one front in a larger battle to improve the infrastructure of our craft, but an important one because of its potential communal benefits for instruction and for making cumulative research easier.

³ If sociology was an enterprise where knowledge claims could be readily tested by other laboratories, then maybe concealed mistakes would be less serious. In sociology, many findings are sold to top journals precisely on the uniqueness of the data that have been assembled and high cost in terms of money or labor in assembling something comparable.

Having said that, endorsing replication initiatives because of economics envy would obviously be no better than resisting them because of economics envy. Whatever one's feelings about "positivism," many research questions in the discipline are indeed posed as having answers, with researchers exerting much effort to convince readers they have done what they could to get those answers right. This research often pursues policy as well as intradisciplinary influence, and the hope of social science is that claims for attention are strengthened when credibility is strengthened. Most of us already believe that, if we are to believe quantitative findings, they should be independently reproducible, which is why permission to verify results is part of our discipline's code of ethics. For reasons outlined in my original essay, there are many good reasons to move beyond conceptualizing reproducibility as an individual and ethical matter, now that technology provides a ready means to restructure replication as a collective matter that can be implemented as a mundane part of the publication process. Such openness will add to the credibility of our work in top journals and will allow the work rewarded with prominent publication to serve an even greater contribution to the discipline. As sociologists, we should seek to find ways to have a conversation about replication that results in action.

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