

A Revised Proposal, Proposal

Gary King, *Harvard University*

Receiving thoughtful comments in this symposium from 19 of the discipline's most innovative empirical researchers is gratifying and goes a long way toward bringing issues of replication and data accessibility into public view. The progress made in the preceding pages clearly demonstrates the virtues of a community of scholars willing to share sufficient information to benefit from each other's work.

Areas of Agreement

This symposium reveals widely differing views about specific data replication policies. The proposals represent, according to Kenneth Meier, "the single most significant contribution in turning political science into a rigorous discipline in my professional lifetime." According to Paul Herrnson, they "would harm researchers, journals, the discipline, and the acquisition of knowledge about politics." Much of the diversity of views across other disciplines reported by the Lineberrys is well reflected within political science.

Yet, despite the wide range of opinion as to how or whether to set specific replication policies in practice, I am encouraged that the authors in this symposium have demonstrated strong agreement on some fundamental principles, as Walter Stone and Jim Gibson emphasize. Perhaps readers will agree with these basic points too.

What are these apparent areas of agreement? To be specific, *every* article is consistent with the belief that more data of high quality should be deposited in public archives. Every author believes that at least some scholars should be encouraged to make their data more widely available, and that methods of data collection, documentation, and analysis should be improved. We all agree that making data available is an important contribution to the scholarly commu-

nity. And everyone agrees that making data available, when possible, helps scholars improve their own work, reduce errors, and, most important, build on each other's work. Indeed, the symposium articles do not contain a single objection to my central argument in support of the principle embodied in what I called the "replication standard." As I put it originally,

The *replication standard* holds that sufficient information exists with which to understand, evaluate, and build upon a prior work if a third party could replicate the results without any additional information from the author. The replication standard does not actually require anyone to replicate the results of an article or book. It only requires sufficient information to be provided—in the article or book or in some other publicly accessible form—so that the results could in principle be replicated (p. 444).

Participants disagree about what to call this principle, and whether it should be met by fully explaining data collection procedures in the text or by actually providing access to the data collected, but all apparently agree with the minimal position that empirical analysts should provide sufficient information in their original publication so that, in theory, a third party could re-collect their data from scratch and replicate their results. There also appears to be agreement that the replication standard is not being followed as much as it should be.

Clarifying Reasons for the Replication Standard

The Least and Most Important Reasons. In my view, the *least* important reason to follow the replication standard is to enable us to duplicate or verify each other's hard work. Duplication is not exciting and is not often published (only once in 27 years in *Social Science Quarterly*, according to the Line-

berrys). Positive replications are almost never published. Even negative replications that make no additional contribution can often best be dealt with by a letter to the journal from the original author.¹

However, duplicating a prior study will lead any good scholar to improve on the study in some way. Far from being the drawback indicated by some symposium participants, enabling researchers build on prior research and to improve their own research is the *most* compelling argument for the replication standard. That is, *duplicating past research is rarely an important contribution in and of itself, but it is often a necessary step to building on prior research.* Unfortunately, without access to the original data, it becomes exceedingly difficult to build on prior research.

The replication standard makes it possible to teach from real political science research so students can retrace the steps of their favorite scholars. Students in introductory graduate classes would routinely be able to replicate prior work—not merely to duplicate what someone else has done but to apprentice themselves to the best scholars in the discipline, to learn how to do what was done so they might eventually do it as well or better themselves. Replication also leads to many creative and productive research practices such as those effectively articulated by Miriam Golden and Jonathan Nagler.²

Replication From What Starting Point? Should we ask that scholars follow the replication standard only by including enough information to collect the data from scratch rather than providing the data? No participant would seem to disagree with the concept of publications including as much of this type of information as possible. However, Peterson, recognizing the limitations to this approach, suggests that access to the original data might be pro-

vided only if the replication “were being done at a later time when conditions have changed so much that independently assembling the same data is impossible.”

Although “replication from scratch” is desirable, the political world changes so frequently that it is usually infeasible and often impossible to recreate another scholar’s database. Moreover, even if the author of the replication study believes that conditions have not changed, the leading alternative hypotheses for any results in this new study that differ from the original analysis will almost always be that they have, or that some of the measures or methods were not precisely replicated from the original study. In most cases, the only way to be certain is for the second researcher to have access to the original data.

Replication and “Breakthrough”

Works. One method of argument in discussions about academic policy such as this symposium is to point to major “breakthrough” works and to ask whether a proposed policy would help write books that good. Of course, choosing only famous books, and concluding that whatever their authors did is what made them famous, generates inferences with massive selection bias. Nevertheless, these comparisons are sometimes useful, at least as a way of understanding the proposed policy from the worst possible vantage point.

For example, Maisel argues, and Sniderman suggests as well, that the insights from breakthrough works in political science “are the building bloc[k] for our cumulation of knowledge. . . . [I]mposition of a uniform replication standard would have no impact on that critical aspect of knowledge acquisition.” Both cite *The American Voter* (1960) as a leading example of a breakthrough work. They note, correctly, that Campbell, Converse, Miller, and Stokes did not write such an influential book because they were able to take advantage of a strong replication policy in place at the time. However, to conclude that the book would not have been helped by such a policy is falla-

cious reasoning. If Campbell et al. had been easily able to include some reanalyses of aggregate election data, which is what their predecessors used, and to compare it to their new work with surveys, their book would probably have been even better.

Although a formal replication policy is not what caused *The American Voter* to be a great work, the book is a spectacularly successful example of the benefits of adhering to the replication standard. The authors provided their data to all interested researchers, helped create the ICPSR to store and distribute their data, and began another institution devoted to conducting follow-up data collections. These data have been analyzed and reanalyzed by generations of scholars who built on their original work and made it more valuable as a result. How influential would *The American Voter* have been without the National Election Studies or the ICPSR? How much influence would Campbell et al. have had if the only way to build on their work was for individual investigators to spend tens of thousands of dollars to conduct sample surveys from scratch? Campbell et al. did not benefit from many prior researchers having followed the replication standard, but the entire research community benefits from their having followed it.

Many scholars in political science try to write breakthrough works. Indeed, it sometimes seems as if every book in the discipline claims some sort of “paradigm shift,” and comparatively few focus on answering existing questions.³ This focus makes some sense since so much of political science involves figuring out the right questions to ask and convincing the research community to study new topics in different ways, an endeavor to which replication policies will not contribute significantly. However, influencing each other to think in different ways is ultimately important only as a means to an end. What is important in the end is learning truths about the political world.

To me, *The American Voter* is an important work *not* because it

caused researchers to switch from analyzing aggregate election statistics from a sociological perspective to survey data from a social-psychological perspective. The book is important because the hundreds of works that it generated produced much reliable knowledge about the political world. We do well as a discipline at asking new questions; I hope we can do somewhat better at finding answers.⁴

Disciplinary Principles, Individual Policies

Symposium participants recognize that no single data access policy can cover all data sets, from reanalyses of publicly available data to original and confidential elite interviews, to field notes of qualitative researchers. Indeed, there is much disagreement over what specific policies to adopt. Fortunately, the discipline as a whole need not adopt a single detailed policy to cover all instances. The discussion over replication issues can instead begin with general principles that might encourage individual scholars in practice, and specific policies for individual circumstances as deemed appropriate by editors or other relevant decision makers.

The recommendation of Bond and Portis, Box-Steffensmeier and Tate, Herrnson, and Gibson that APSA establish a committee to develop a recommended data archiving policy is a good one, but any recommendation intended for the entire discipline would likely be limited to a statement of principle along with a list of policies that journals, presses, graduate programs, and funding agencies might wish to choose from. Such a committee might also be able to develop some minimum standards to apply universally, but only if a widespread consensus could be formed.

Presently, some journals require a footnote addressing data access; some require authors to make the data available themselves; others require data to be deposited in a particular public archive; some make decisions based on how valu-

able the data to be released by the author seem to be; and most have only informal policies. We benefit from this diversity. If someone wishes to start a specialized journal to publish articles without footnotes, citations, or information about scholarly methods, no one is going to stop them; nor should they be stopped. Similarly, if a specialized journal were created where every article submitted were fully replicated by the journal or reviewers before publication, no one would or should stop them.⁵ These hypothetical journals would serve, and all the real ones do serve, many purposes in our discipline.

Tailoring policies to the needs of the journals should be left where it always has been, with the editors. For example, it was entirely Edward Portis and Jon Bond's decision to "have decided to wait until a thorough discussion of the issue is conducted under the auspices of the APSA before finally deciding upon a policy"; as it was the prerogative of editors Kenneth Meier and John Freeman to adopt versions of their own innovative data access policies over a year ago. It was also the prerogative of scholarly journal and newsletter editors Michael Alvarez, Nathaniel Beck, John Berg, Bob Brookshire, Laura Brown, Uday Desai, Lee Epstein, John Green, David Sanders, Mack Shelley, Harvey Starr, Arnold Vedlitz, and Albert Weale to move forward with their own replication policies over the last year in a manner that they saw fit (as was described in my original article). Similarly, we need no restrictive disciplinewide policy to impose on book presses, graduate programs, individual authors, or funding agencies. Their publishers, faculty members, authors, and foundation directors have made and can continue to make all these relevant decisions in a manner that best suits their organizations.

My suggestions involve regulation, as Linda Fowler's interesting interpretation suggests. However, they do not involve "state" regulation, as APSA is not a legislative, governmental, or even licensing

body and cannot enforce anything on its members. The policy suggestions involve self-regulation, which is something we do all the time as an academic discipline. For example, almost all outlets require formal citations when referring to prior work. We also seem to believe that help received in writing an article should generally be acknowledged. Indeed, my primary goal in writing "Replication, Replication" was to influence our disciplinary norms so that more of us adhere to the replication standard that everyone seems to support, not to propose that a uniform rule be enforced disciplinewide.

However, many symposium participants hope, perhaps even more than I do, that a common set of specific policies eventually be developed. As I described above, the outlines of the principles that would underlie these policies are already clear. Perhaps we should work toward what we might wish to recommend as a set of minimum policies to meet the replication standard. Indeed, according to Janet Box-Steffensmeier and Katherine Tate, we have a special responsibility to graduate students and junior faculty to try.

Box-Steffensmeier and Tate favor a formal, uniform, disciplinewide policy, more specific and stronger than I have advocated. Part of their reasoning is that junior scholars often feel it necessary to turn over their data upon request from a senior colleague, but, without a formal rule, senior scholars can too easily ignore a junior colleague's request. A formal standard, they argue, would prevent these inequities and level the playing field. The relevant disparity in the discipline is that some of the most active data analysts are the youngest, and those with the best data to analyze and most grant funding with which to collect it are the most senior. Other concerns about data replication policies may affect young members of the profession in different ways, but clearly stated rules of the game seem like a fair demand for any individual scholar to place on the political science community.

Proposals

Perhaps this symposium will raise public attention sufficiently so that many authors will construct valuable replication datasets and routinely submit them to public archives. However, as Kenneth Meier emphasizes, at present the "voluntary system of standards does not work." As Linda Fowler explains, "the amount of information sharing is likely to be suboptimal without some sort of incentive or requirement to bring it about." If we start recognizing the valuable contributions of data collectors more often, perhaps including formats for data citations as Jim Gibson suggests, our present system may begin to work. With the increasing numbers of journals, book presses, and funding organizations in political science adopting versions of replication policies, this combination of voluntary compliance, strong encouragement, and some requirements may begin to solve the problem.

But what can we do as a discipline? For one, every symposium participant apparently believes that authors with valuable data sets, who have no reason to object to releasing them, should be strongly encouraged to make them publicly available. This is the group to which I hope we address our attention first. Encouraging, nudging, prodding, or in some gentle way requiring these scholars to make their data available would hurt no one, might get them more recognition, and would likely improve the ability to other scholars to build on their work. If this one advance were made, this symposium will have accomplished an enormous amount, as this group constitutes a large fraction of authors (Kenneth Meier estimates 95%).

Accomplishing this goal would have no effect on authors with confidentiality concerns, or those who favor periods of embargo, or anyone who has reason to keep their data private. It would not be a mark against these researchers; the requirement would simply not apply to them. I hoped this was clear even in my original article: some data should remain confidential. To

be specific, Paul Herrnson could keep his interviews with congressional candidates and their staffs; no one should inquire of Aberback and Rockman whether they will release their data on federal bureaucrats; and you can keep your hands off my confidential data on Harvard's graduate admissions process!

How do we motivate those with no objections to make their data available, while not causing problems for others? I suggest three specific policies. These seem close to the minimum possible while still having a chance of meeting the replication standard.

First, *when articles or books are accepted for publication, the editor should send information about data access and available archives along with the usual acceptance letter.* At a minimum, the purpose of this information would be to inform the author about the concept of replication data sets, what archives they can use, and which APSA section newsletters will announce the availability of their data sets along with citations to their published articles. Authors who have data they do not intend to use again, and for which confidentiality is not an issue, might also be specifically *encouraged* by editors to deposit their data, thus benefiting themselves, the journal, and the discipline. As a convenience for editors who might wish to distribute this information, I include here an example of what might be provided. (Note that this includes updates and new developments not included in my original article.)

Information About Data Access

and Advertising. Having your article accepted by this journal [or book press] entitles you to the services of other organizations. For example, if you construct a "replication data set" (i.e., a set of electronic files containing all the data and information necessary to replicate the results of your published work), you may submit it to one or more archives, which will, in turn, make your data available to other researchers and advertise your published work. In addition, most APSA section newsletters will announce the availability of your data, and the publication of your article, if

you send the editor the citation to your article and where your data are archived.

To submit a replication data set to the "Publication-Related Archive" of the *Inter-University Consortium for Political and Social Research*, send e-mail to the administrator at pra@icpsr.umich.edu. and then ftp the data to ftp.icpsr.umich.edu:/pub/PRA (or send it by surface mail to Administrator, Publication-Related Archive; ICPSR; P.O. Box 1248; Ann Arbor, MI 48106). To submit data to the "Social Science Research Archive" of the *Public Affairs Video Archive*, email the administrator at info@pava.purdue.edu and ftp the data to pava.purdue.edu:/pub/incoming (or send it to PAVA/SSRA; Purdue University; 1000 Liberal Arts Building, West Lafayette, Indiana 47907-1000). You can also submit data to the "poliscidata" collection in *StatLib* (the system for electronic distribution of statistical software, data sets, and information) by e-mailing the data to statlib-submit@lib.stat.cmu.edu; for questions, see the Web server at http://lib.stat.cmu.edu or e-mail mikem@stat.cmu.edu.

Alternatively, the editor could merely add a two-sentence note encouraging data access in the acceptance letter and referring the author to this page in *PS* for details.

Perhaps this policy of informing scholars upon publication is all we can agree on. Perhaps it is all we should agree on. But I think we can go further, even as a discipline. For example, we presently have a custom or standard of reporting that requires authors to reveal in some fashion the data or information on which they base their conclusions. I suggest we add a second policy, that *each scholarly work should explicitly address the issue of data access—not necessarily granting access, but at least addressing the issue.* This footnote or appendix could indicate in what national archive the data necessary to replicate the results of the article have been or will be deposited. It could suggest that researchers write the author for more information. The footnote could explain that the data exist in electronic files that took many hours to create and will not be released to the public (for a

specific period, or indefinitely). Or an appendix could list some or all of the necessary data.

Isn't the scholarly community entitled at a minimum to have the issue addressed explicitly so everyone knows what would be involved in conducting follow-up work in the field? Should every student thinking of following up a study need to track down the author of the study just to find out? This seems like a minimal requirement that could greatly help future scholars when they attempt to build on the work of those who came before. I believe that many, albeit not all, authors would respond to this principle by depositing their data, but at least it would keep the issue of data access on the table.

Reviewers can consider the degree of data availability in their reviews even now, and many do, but my third suggestion is that *editors should encourage reviewers to comment on the quality and extent of data to be released by the author, if any and if appropriate.* If the work is especially innovative, and the author has burned the data, reviewers will probably favor publication anyway. If the data are from elite interviews, for example, and could not be released due to confidentiality concerns, the issue of data access would be irrelevant. If the manuscript is not especially interesting and might not ordinarily merit publication, but the data appear very worthwhile and the author plans to make them available, perhaps the reviewer would recommend publication of an abbreviated version that highlights the data. Providing access to data along with published articles is one of the factors that makes work more or less valuable, which is precisely the information that reviewers and editors need for evaluation. The ultimate decision to publish would remain, as always, with the editor.

What about qualitative research? Although Linda Fowler speculates that qualitative researchers "already provide the type of information King identifies as being necessary for replication," she also recognizes the "dearth of empirical support" for this and other views. Fortunately, Miriam Golden meets

this challenge and supplies some evidence to the contrary. For example, through a creative analysis of articles from *Comparative Politics*, she finds that qualitative researchers have been just as lacking in meeting the replication standard as those in quantitative research.

According to Golden's study, scholars pursuing this style of research have rarely followed any version of the replication standard, either as part of the written publication or by supplying information to archives. She also explains how easy it would be to satisfy the standard in similar ways to quantitative researchers and even in difficult situations due to confidentiality concerns. She writes, "Even if public archiving proves inadvisable, it should be possible to provide enough information to allow other scholars to retrace the field process."

Similarly, Maisel cites Richard Fenno's *Home Style* (1978) and its famous methodological appendix, and Golden cites David Laitin's *Hegemony and Culture* (1986) for a similar, and more informative, appendix. These works show how to follow the replication standard in important studies that happen to be based on qualitative evidence. The appendices are excellent models because they greatly improve the scholarly product. They show successors roughly what to do by showing them precisely what was done. In the situation where journals and some book presses do not allocate adequate space for appendices like these, qualitative researchers can avail themselves of the same archives that quantitative researchers use.

Similar appendices, relevant portions of field notes, transcripts of interviews, or audio or video or photographic evidence can all be readily digitized, if not already in electronic form, and deposited in the ICPSR publications-related archive or the PAVA social science data archive. Alternatively, they can make use of specialized qualitative data archives, such as the newly established *Qualidata* collection at the ESRC Data Archive. *Qualidata* is also working out issues raised by confidentiality in

sensitive data sets and "conditions of access as well as means of monitoring the research use of the material."

Under these alternative proposals, qualitative and quantitative researchers would not be required to make data available, only to address the issue. Since even now reviewers can and sometimes do comment on data access statements (or lack of such statements) in manuscripts, and they would not be required to do so in any event, the proposal would not create much extra work. Some journals and presses already require more of authors than this, as is their right, but perhaps we might agree that very few should do less.⁶

Concluding Remarks

Among all the sciences, political science has often been at the forefront of data collection and distribution. The Inter-university Consortium for Political and Social Research, now the largest collection of social science data in the world, was originally formed as the "Inter-university Consortium for Political Research" by political scientists. University data centers at many institutions are housed in the political science department and run by political scientists. A disproportionate number of university computing center directors have been political scientists. It should be no surprise that our journal and book editors, faculties in charge of our graduate programs, and directors of political science-related funding agencies are now considering or already experimenting with a diverse range of policies about data access and replication. What they learn from these experiments will benefit the political science community for many years to come.

I hope everyone looks forward, as much as I do, to the results of the two research projects spawned by this controversy and planned by the Lineberrys and by Walter Stone. The results should be very useful to the discipline's ongoing discussions about data access and how to meet the replication standard. I also look forward to the

2010 A.D. meeting of the American Political Science Association to see if Kenneth Meier's prediction comes true. Perhaps a set of diverse detailed data access policies will be as accepted as the replication standard appears to be today.

Notes

1. As Sniderman, Aberbach, and Rockman correctly emphasize in their "replication" of my reading of Dewald, Thursby, and Anderson (1986), substantive conclusions are not always sensitive to methodological errors revealed during duplication of past research. This point was demonstrated by Dewald et al.'s replications, and by the fact that my substantive conclusion—that the research practices revealed by Dewald et al. were methodologically horrific—remains unaffected. Of course, the fact that many errors are made but some do not affect substantive conclusions is cold comfort, as Robert Hauck's discussion of Louis Pasteur makes clear. Implementing the replication standard will probably increase the probability of finding errors, but however many are found, this sort of checking up on each other is of only secondary interest.

2. I hope an enterprising publisher is able to convince Golden and Nagler to write a book on "Improving Social Science Research Practice," covering both quantitative and qualitative research styles. The kind of specific, detailed advice they each give is extremely valuable and not widely known or followed.

3. Recently, *Science* magazine wrote to 60 scientists in many fields, approximately three of whom were social scientists, and asked them to write about what they saw as the future of science in their fields (1995). Although perhaps a dubious measure of the future of science, it is an excellent measure of these scientists' goals. Virtually all of the natural and physical scientists wrote about what they hope to learn about the world or about the policy or engineering implications of this new knowledge. These little essays convey very well the authors' excitement with the world they are trying to grasp. In sharp contrast, the social scientists largely ignored what our disciplines might learn about the world and instead wrote about how our theories, perspectives, and methods would change. The natural and physical scientists wrote about the natural and physical world. The social scientists wrote about social scientists, about asking new questions rather than providing some answers.

4. I am reminded of my colleague Bonnie Honig's job interview. The interview was going well, and she and I were discussing the differences between the type of work that she, as a normative theorist, and I, as a methodologist, did. At that moment, my teaching assistant, who did not notice anyone else was in my office, walked in and said, "Here are the answers." Without the

slightest pause, Honig said, "That's the difference!"

5. Paul Sniderman is incensed about the two sentences in my article that suggest that "some journals might wish to experiment" with an extreme policy like this. I was not proposing that this policy be applied to all journals or imposed on anyone, although I can think of no reason to *prevent* the editor of a journal who might have reason to adopt this or any other policy from doing so.

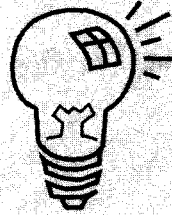
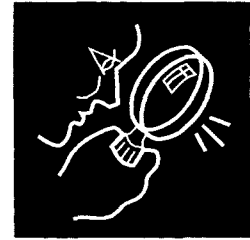
6. Some participants like the idea of authors of data being listed as coauthors of any article that uses the data. I see no reason why this should not happen even more frequently than it does now. But it is certainly not always appropriate. If someone is using my data to criticize me, I should not be given the right to veto publication or negotiate about conclusions. Coauthor credit in other fields sometimes works, but it also leads to articles with hundreds of coauthors. Similarly, the policy of the *New England Journal of Medicine* insists that submitting an article for publication constitutes an acknowledgement that every author has *read* the article. Not authored, contributed to, or even agreed with—just read!

References

- Campbell, Angus, et al. 1960. *The American Voter*. New York: Wiley.
- ESRC Data Archive. 1995. "Qualidata: The Need for a Qualitative Data Archive Policy." *ESRC Data Archive Bulletin* 59 (May):19.
- Fenno, Richard, Jr. 1978. *Home Style: House Members in their Districts*. Boston: Little, Brown.
- ICPSR Services. 1995. "ICPSR Establishes Publication-Related Archive." *ICPSR Bulletin* 15, 3 (February):8-9.
- Laitin, David. 1986. *Hegemony and Culture: Politics and Religious Change Among the Yoruba*. Chicago: University of Chicago Press.
- Science. 1995. "Through the Glass Lightly: A Collection of Scientists at the Frontier Were Asked What They See in the Future for Science." *Science* 267 (17 March):1609-18.

Looking for sources?

That cover important national issues ... are impartial ... and start your students on self-guided research?



Discover Congressional Quarterly...

We've analyzed and reported on Congress and national issues for 50 years!

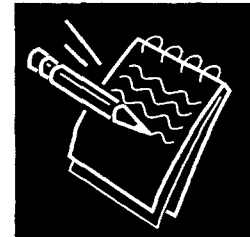
The CQ Weekly Report

translates partisan rhetoric on all the issues before Congress into facts your students can understand. And **CQ's Washington Alert** puts all of CQ's publications and more online, as part of a complete state and federal legislative and regulatory tracking service!

Return the attached coupon or a photocopy to get a free evaluation issue of the CQ Weekly Report and information on CQ's Washington Alert!!

Write for free materials today!

YES! Send me a free evaluation issue of the CQ Weekly Report and information on CQ's Washington Alert!



Name _____
Title _____
Institution _____
Address _____
City _____
State _____ ZIP _____
Phone (_____) _____

Please also send information to my university librarian:

Name _____
Title _____
Institution _____
Address _____
City _____
State _____ ZIP _____
Phone (_____) _____



Return this coupon to: Congressional Quarterly Inc., 1414 22nd Street N.W., Washington, D.C. 20037

For fastest service, contact CQ at 5LPJ32@cqalert.com, or call:

Darryl Roots at (800) 432-2250 ext. 456, or (202) 822-1456, for print publications; Cindy Krick at (800) 432-2250 ext. 366, or (202) 887-6366, for CQ's Washington Alert.