REPLICATION STANDARDS FOR QUANTITATIVE SOCIAL SCIENCE:

WHY NOT SOCIOLOGY?

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Computing and programming advances continue to dramatically increase the scale and sophistication of analyses that quantitative social science conducts. One consequence of this increased complexity is that quantitative research articles are often required by space and stylistic constraints to "paraphrase" or omit discussion of many particulars of decisions about constructing variables, excluding observations, and specifying models (among other things) that are made in the course of research. This, in turn, has prompted increased concern about the latitude for published results to reflect a series of analytic decisions whose implications or even existence are underdocumented in the article, especially as these decisions might, for whatever reason, mostly happen to favor the author's argument (e.g., Peng, Dominici, and Zeger 2006; Ho, Imai, King, and Stuart 2005). Underdocumented analyses also make it harder to follow the analytic decisions of others in trying to build off and elaborate past research. In other words, the interests of quantitative social science are best served by maximizing the *transparency* of analytic work—the *extensiveness* and *precision* of information available about how published results were derived from data—but the increasing complexity of analytic work makes printed journal space ever more inadequate for providing such detail.

Fortunately, however, the Internet provides quantitative social scientists with enormous opportunity to the increase the transparency and thereby credibility of their results. The Internet makes it possible for any materials that researcher might have earlier characterized as "available upon request" to be instead provided online at the time of publication. Many journals now allow online supplements in which authors can provide additional tables and other information beyond what appears in published articles, and authors can also refer readers to information available on

their own websites.[1] Yet offering opportunities for individuals to have more discretion in elaborating analytic details is not the same as maximizing the transparency of quantitative social science *as a collective enterprise*. The Internet provides social science the opportunity toward much greater realization of the standard by which the combined material provided in print or online at the time of publication is sufficient to replicate (that is, verify or duplicate) results without needing to obtain additional information about procedures from the researcher (King 1995).[2]

Historically, sociology has been near the forefront of social sciences in advocating for openness and public accountability of professional practices. At present, however, economics has done far more to seize the opportunities for transparency in quantitative work provided by the Internet. Economists' concerns have been prompted by studies of the replicability of analyses of samples of economics articles that have reported alarmingly dismal rates of both cooperation and ultimate success (Dewald, Thursby, and Anderson 1986; McCullough and Vinod 2003; McCullough, McGeary, and Harrison forthcoming; see editorial statement by Bernanke 2004).[3] Official journals of the American Economics Association that publish original empirical research now have an extensive policy regarding the availability of data and materials. The policy of the *American Economic Review* begins (full text in Appendix):

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[1] Indeed, one might wonder why journals that offer online supplemental materials still permit "available upon request" to be used for anything that could be provided online.

[2] I use "replication" throughout to refer to using the *same* data and procedures with the goal of verifying results. Replication is also used to refer to attempting to see if the same findings are observed in a fully repeated study (i.e., data collection and analysis). Many believe that social science would benefit from conducting more replication work of the latter type; I happen to agree, but the issue is beside the point of this paper.

[3] The replication attempts reported in the cited articles were from journals that had *more* explicit and unambiguous statements about expectations about sharing and cooperation with attempts to replicate than what presently exists in sociology.

> It is the policy of the *American Economic Review* to publish papers only if the data used in the analysis are clearly and precisely documented and are readily available to any researcher for purposes of replication. Authors of accepted papers that contain empirical work, simulations, or experimental work must provide to the *Review*, prior to publication, the data, programs, and other details of the computations sufficient to permit replication. These will be posted on the *AER* Web site. The Editor should be notified at the time of submission if the data used in a paper are proprietary or if, for some other reason, the requirements above cannot be met.

Key points of this policy are that the standard is archiving materials *at the time of publication*, the materials encompass both data and the programs (e.g., batch/syntax/"do" files) necessary to replicate published results, and the policy recognizes and provides for flexibility in cases where data cannot be publicly shared.

The economics standard treats the disclosure of the maximum information that one is able to provide about one's analyses as a *mundane part of the publication process*, akin to the expectation of a full citation for all references.[4] By contrast, sociology continues to treat providing full details for replicating results as an *ethical* and *individual* matter. The only standard is that of the American Sociological Association (ASA) code of ethics, which is just that sociologists should "permit" attempts to verify results after their publication.[5] Conversations suggest wide variation in sociologists' understanding of what this statement exactly obligates (for that matter, conversations suggest variation in the extent to which publication in ASA journals obliges adherence to ASA ethics policies if these are inconsistent with a researcher's own inclinations regarding sharing materials). At its most demanding, the statement is interpreted as meaning that sociologists should be willing to cooperate with others

[4] Indeed, on the *AEA* website, a link to the data availability policy is included as part of the *style guide* for submissions.

[5] ASA Code of Ethics, 13.04(e): "Consistent with the spirit of full disclosure of methods and analyses, once findings are publicly disseminated, sociologists permit their open assessment and verification by other responsible researchers with appropriate safeguards, where applicable, to protect the anonymity of research participants."

seeking to verify their results by providing basically the same information upon request that the economist is expected to provide as part of publishing the article.

From the standpoint of collective knowledge production, the *social* policy toward replicability adopted by economists has several plain advantages over the individualistic policy of sociology. For less trusting souls, the obvious difference may be that readers can expect that the economist has already had to provide information sufficient for replicating results (to the extent possible) while readers are asked to have faith that the sociologist would do so if asked. The ethical frame obscures more fundamental differences, however, because its policy of individual responsibility presumes also that the sociologist has conducted quantitative analyses in a fully reproducible manner and that the sociologist will successfully preserve these materials (through whatever relocations and hard drive crashes) so that it can be retrieved potentially years hence. Moreover, the individualistic policy expires when researchers leave, die, or otherwise disengage from the discipline, while *the social policy seeks to decouple the content of articles from the contingencies of authors' futures*. The social policy treats information about replication as part of the *price of admission* to competitive journals, rather than as an act of *individual graciousness* following publication.

In addition, the social policy also seeks to encourage egalitarianism in social science research by minimizing the degree to which status and social networks affect access to materials necessary to verify, learn from, and build off of others' work. The individualistic policy opens itself up to the possibility—suggested to me by various colleagues—that investigators with faculty positions at prestigious institutions may receive more prompt and complete responses to inquiries about others' analyses than graduate students or those at less prestigious institutions. Indeed, the ASA policy invites authors to *judge the worthiness* of those who request information

pertinent for verifying results by restricting obligation only to "responsible researchers."[6] Some colleagues report having offered their names to vouch for the legitimacy of requests of students or others in the discipline who want more information about published analyses. A social policy seeks to maximize the extent to which those materials that can be shared with others are shared openly and with all interested parties. The social policy also increases the extent to which articles that command scarce journal space are instructive to other researchers, by allowing interested others to see more details of how exemplary work was done.

For all these reasons, as well as some others that will be noted below, a social attitude toward replication is to be preferred to an individualistic one, and the Internet provides opportunities for implementation of social policies that have not been readily available to sociologists before. Movement toward more explicit and social policies would have salutary effects for both the perceived and actual quality of quantitative work conducted by sociologists. My argument is not that the policy of economics must be adopted, as one can certainly argue with particulars of this policy, and, more importantly, intermediate progress can and should be made even if more demanding aspects of the economics policy may be resisted. Experience discussing these issues with others suggests the collective benefits of transparency are readily appreciated by sociologists, and resistance to the general proposal seems centered more on a relatively small number of recurrently voiced objections. Most of the rest of this paper will be devoted to addressing these objections, and in so doing it will elaborate what improved replication standards for sociology might look like.

Before considering these objections, however, let me make my own position more plain. Those who are rewarded with publication in official (ASA) sociology journals are already

[6] For an instance in which the similar policy of the American Psychological Association could be interpreted as obstructing a worthwhile attempt at verifying published results, see Johnson 2000.

expected to cooperate after publication with attempts by others to verify their results. My position is that, to the maximum extent possible, quantitative researchers should be asked to provide everyone (via the Internet) at the time of publication the same information they would now provide to anyone afterward in response to a request. To whatever extent there is information they can make available to individual inquiries but cannot (for whatever reason) make publicly available, researchers should explicitly state this. Similarly, to whatever extent researchers are unable or unwilling to provide any outside investigator with details sufficient for replicating results, researchers should be expected to be explicit about this. The idea is not that in raising replication standards quantitative sociology would be somehow "raising ethical standards," but that it would be using available technology to minimize the degree to which providing information about analyses remains an ethical matter. Increased replication standards would be beneficial for the credibility of sociological research because it increases confidence that work can be replicated, but they are also valuable because they make published work more available to elaboration and extension by others and they afford the best opportunity for exemplary work to contribute to teaching other members of the profession.

WHY NOT SOCIOLOGY?

1. *Won't this mean more work for researchers?* To my knowledge, no methodologist would disagree with the proposition that good data analytic practice implies the existence of a record of all procedures required to proceed from a pristine data set to the numbers presented in research article submitted for publication. This record can be thought of as the *implicit technical appendix* to any prospective publication of quantitative social research. Since the record is

already presumed to exist, making it public should typically require only the few minutes needed to send the files to an appropriate online archive.[7]

By this logic, the main instances in which depositing code would imply more work would be precisely those instances in which more work is desirable anyway, in the sense of ensuring the integrity of the "chain of evidence" (King 2003: 100) connecting data and published results. For example, researchers who use pull-down menus for analyses would need to be sure to save the syntax generated by these menus (e.g., by clicking on the "Paste" button in SPSS) for their final publication analyses. Researchers would also need to be sure to comment code enough so that it can be followed by others, which they should already be doing to ensure that they will be able to reconstruct work themselves in the future.[8] Part of documentation should include version information on the data and software used (McCullough, McGeary, and Harrison forthcoming; Altman and King 2006). These examples illustrate the broader point that adopting social policies toward replication will likely improve individual data analytic practice, by encouraging researchers to conduct analyses with the anticipation that code will be available for others to inspect. Attempting to reconstruct such information in response to a later request, after one has moved on to other projects, is almost surely more onerous work for the conscientious researcher. Indeed, accumulated anecdotes about attempts to verify results suggest that inadequate record-

[7] For those authors who will be making original data available, the policy may seem to imply more work is required for documenting this data. Hopefully, the researcher intended to follow existing guidelines and eventually make data available anyway (to the extent possible), and thus data would need to be documented for third-party consumption sooner or later. Regardless, if the purpose of providing data is just to permit verification, then probably less work is required than would be required if the research is intended to make data suitable for others using data for more elaborate research projects. That kind of documentation work is important for the vitality of the discipline but is a separate matter from the focus of this paper.

[8] In documenting code, researchers of course can presume familiarity with the software being used to conduct the analyses. That is, the arguments here are not meant to discourage researchers from using whatever software packages they currently use, nor are they meant to put researchers in the position of having to teach those packages to others.

keeping and competing demands of current projects pose larger barriers to efficient verification of results under individualistic policies than unethical spirit among investigators.

2. *Won't this mean more work for editors?* Economics may have more editorial resources that make it easier for the discipline to implement policies that imply direct oversight by editors or their assistants. Fortunately, many of the seeming benefits of a more social policy can be implemented in ways that are nearly costless for editors or ASA. Most importantly, editors need not be involved in the actual handling of data or code, but instead authors can be encouraged to deposit materials in the ICPSR Publications-Related Archive or other independent archives (e.g., the Murray archives at Harvard University) that accept code and data at no cost to authors. Having materials handled by professional archivists working for permanent archives would seem preferable to assigning the same work to editorial assistants at journals whose editorships change every few years. Likewise, independent archives would seem much preferable to authors posting materials on their own websites (even if they wish to do this as well), as then again the availability of materials is contingent on the author's continued engagement with the field (and with maintaining a website).

Instead of presenting materials to editors, authors can merely be asked to address plans for code and data availability in their article: "Materials sufficient for replicating results reported in this article will be [have been] deposited with the ICPSR publications-related archive." King (1995: 448) proposes that such information could be expected to appear in an article's first footnote. Regardless, if guidelines for submission include the expectation that availability of data and code will be addressed somewhere in the paper, reviewers can evaluate this information in the review process.

3. *There are good reasons for researchers not to make data publicly available.* The many complex issues that surround data sharing have power to overwhelm and thwart the more general interest in increasing the transparency of quantitative analyses.[9] Especially in sociology, perhaps, researchers often are not able to make the data on which their analyses are based publicly available, either because they do not have the right to distribute the data or because of confidentiality agreements. Even when researchers can make data publicly available, they may not wish to do so because they may not want others to use their data for further analyses, at least as long as they are planning any subsequent publications of their own from those data.[10] While compelling arguments can be made for why selfish interests are often well-served by a generous policy toward sharing one's data (e.g., an empirical study by Gleditsch, Metelits, and Strand 2003), these arguments do not persuade everyone.

Important to recognize is that no argument against making data available is a good argument against providing *explicit information* about data availability. Researchers could state "Data will be shared with individual investigators for verification purposes only" or "Due to confidentiality restrictions, data cannot be shared with others even for purposes of verification."[11] Whatever the particular restriction, openness is preferable to ambiguity in which

[9] This perhaps reflects the experience of political science; see King 1995 and the main objections in the nineteen responses the follow. For evidence that disciplines can make progress nonetheless, see political science policies collected by Gleditsch and Metelits (2003), as well as Gleditsch, James, Ray, and Russett (2003).

[10] Regarding data sharing, the ASA Code of Ethics does state that, when possible, "Sociologists make their data available after completion of the project or its major publications." While the moment of publication is well defined, the moment at which a project or all major publications have been completed is not, and thus there is never a distinct point at which a researcher is clearly obliged to share original data.

[11] Perhaps archives for depositing data (or, by extension, code without data) could include a means by which researchers so inclined to opt to deposit data so that its retrieval requires a user to enter identifying information and to select "I accept" to an agreement that contains appropriate ethical and legal language prohibiting use beyond verification purposes and prohibiting

readers might be led to believe that findings are more available for examination or elaboration by other researchers than they are. We already expect researchers to provide details about the data on which conclusions are based; information about the availability of data seems a reasonable addition to these expectations, especially given the wide variation in availability that exists and the obvious importance of availability for informed judgments about the verifiability of conclusions.

Regardless, it is false to conclude that depositing code at the time of publication is only worthwhile if data are also available. As already noted, requiring researchers to make code available has benefits for improving the quality of individual data analytic practice, and others might still inspect code to make judgments about the seeming competence of analyses of private or otherwise proprietary data. Beyond this, archiving code at the time of publication may increase credibility by committing that the code archived is sufficient to replicate findings if/when data are more broadly available. (Researchers who are not disclosing data may, if possible, wish to disclose logs of the output of their code, to further establish that the specific results presented in the paper can be generated by the deposited code.) In other words, the standard should be *maximum* transparency and detail at the time of publication, so conditions that prevent full disclosure should not be taken as justifying no disclosure.

4. *There are good reasons for researchers not to make code publicly available.* Researchers may have spent considerable time writing the code on which their analyses are based, and code might include ingenious solutions to data management and programming problems that would stymie others. Some researchers may then be reluctant to allow others to

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redistribution. An automatically generated e-mail could inform the author whenever data has been obtained according to this license. This would allow authors to take advantage of the existence of online archives as a permanent repository without having to feel that doing so forces them to forgo proprietary interests over data.

benefit from their labor, especially if they are planning further projects using the same code. Paradoxically, if programming skills are relatively scarcer in sociology than in economics and political science, those with such skills may feel a stronger individual incentive to "hoard" their code, when the collective interest would be even better served by such code being available as an instructive exemplar to others. The current policy of sociology can be perceived as providing incentives that encourage such hoarding, as opposed to encouraging those skilled practitioners rewarded with esteemed journal space to maximize the extent to which subsequent researchers can learn from and build off their work.

Indeed, more than one person has suggested to me that their own reaction (or that of hypothetical "others") to a more social replication policy would be to expend less effort writing code, articulating a surprisingly adamant aversion to having their work contribute to others' research unless accompanied by clear and complete assurance in advance that they would be credited copiously for any such contribution. Those with such sentiments might first reflect on whether and to what extent their own research has benefited from freely available research tools developed by others (e.g., those provided by Long and Freese 2005 and King et al. 2001, not to mention the entirety of R). Regardless, in forecasting consequences of policy changes, sociologists typically eschew speculation for data when possible. Here, policies and other initiatives that have prompted greater openness among economists and political scientists have not shown any deleterious consequence for the production of original and code-intensive quantitative research in these disciplines, but instead both appear in the midst of especially vibrant eras.[12]

[12] The strength of econometrics with economics and the rise of what could be called "public economics" (Levitt and Dubner 2005; Harford 2005) based on quantitative empirical studies is presumably well-known to quantitative social scientists. Less well-known may be that the

Of course, anyone using another person's publicly available code to advance their own projects should cite the original author's work accordingly. Beyond this, if researchers are unwilling to make their code available for others to use (or are willing only to make it available to individual researchers for verification purposes only), then the proprietary character of the code is at least something about which researchers can be expected to be explicit. The same kinds of statements suggested above for data can also be provided for code (e.g., "The code used in these analyses will made available to individual researchers for verification purposes only").[13]

If statements about availability are expected as part of a submission, editors and reviewers can assess it in regarding the work *as a contribution to sociology*. Articles accepted to ASA journals are commonly regarded as contributions not just for what they teach other sociologists about a particular substantive point, but also *for what they teach about ways of conducting good and informative research*. A reviewer might regard work that contributes both novel findings *and* materials that will help others build upon those findings to contribute more than work that offers findings but not code. In any event, researchers can at least be expected to discuss the availability of code sufficient to replicate the results they wish the discipline to expend journal resources to disseminate.

5. *What about qualitative research?* Unlike economics, a large and increasing percentage of empirical research in sociology does not involve code or quantitative results at all.

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political methodology section in political science has now grown to be the second largest section in the American Political Science Association.

[13] In addition, one can imagine scenarios in which code for analyzing confidential data can include specific instructions regarding individual observations (e.g., in data cleaning) in ways that, even if it does not pose a genuine threat to confidentiality, involves disclosure of information the researcher may not have rights to disclose publicly (e.g., case ID numbers). One can imagine the need for exceptions to any policy for such instances. Again, the larger value of a move toward increasing transparency of analytic work can accommodate exceptional cases without having such cases derail all efforts to raise standards for replicating results.

While one can engage in lengthy discussion about data sharing and archiving for various kinds of qualitative work, my argument is intended to pertain only to quantitative research or the quantitative portion of substantially "mixed-method" research.[14] Qualitative research, generally speaking, has no analogue of the code that takes a researcher from a pristine data set to the results presented as findings in a paper.[15] Indeed, this difference seems close to the heart of the distinction between qualitative and quantitative modes of inquiry, and of course the difference implies nothing about the ultimate value of either kind of research. Importantly, quantitative researchers should not be able to use the fact that qualitative research does not have an analogue of code—and so qualitative research cannot be expected to engage in disclosure analogous to providing code—as grounds for avoiding maximizing the transparency that quantitative research allows. Major journals in sociology already have standards that apply only to quantitative research (e.g., involving acceptable levels for reporting the statistical significance of results), so there is precedent for policy initiatives that pertain to some but not all of the splendid diversity of empirical research conducted in the discipline.

CONCLUSION

Quantitative sociology would benefit from adopting policies that treat providing maximum information for verifying results as less an individualistic and ethical matter and more a routine part of the publication process. The likely benefits include increased grounds for

[14] I do not intend qualitative studies that include some univariate or bivariate counts of events to be considered "mixed-method" for the purposes here.

[15] This is not to say that qualitative research in sociology does not make ample use of computers and software tools, but the relationship between the kinds of results produced by such tools and the findings presented in articles is typically much different than the direct copying of results from some analysis (however iteratively derived) to tables and graphs that characterizes quantitative social research.

confidence in the credibility of results; increased quality of data analytic practice; increased possibility for subsequent research to build off existing research; and increased contribution of published articles to the methodological instruction of the profession. Accordingly, to the maximum degree possible, sociologists should make available to everyone at the time of publication the same information they would later make available to anyone upon request. To whatever extent pertinent information for replicating results cannot be made publicly available, researchers should be explicit about its availability. The goals are that published results should ideally not require post-publication contact with authors in order to be exactly replicated, and to whatever extent they do—or, to whatever extent information that would permit their verification is not available even upon request—this should be known to readers in advance.

Many sociologists with whom I have discussed my position have been supportive of the idea but are surprisingly fatalistic about the capacity of their discipline to change its collective practices. For reasons already articulated, however, researchers may consider greater transparency of analyses to be desirable for their own practice even if not implemented more broadly. In other words, researchers may entertain the value of making data and code for their papers available through the Publications-Related Archive at ICPSR and noting conspicuously in their papers that they have done this. Such anticipatory attention may improve the quality of work, may enhance the credibility of work to readers, and may prevent cumbersome labor in responding to requests later.

Additionally, sociologists should recognize that they do not need to be elected ASA officials or journal editors to attempt to influence collective practice. Reviewers who believe that sociologists should be making code and data available when possible can include

recommendations to this effect in articles they believe merit space in sociology journals. In one's "Comments for the Author," one could include text like the following:

> I think the results of this paper are sufficiently interesting and provocative that I can imagine the possibility of others wanting to verify the results or build off these results in future work. For this reason, I strongly encourage the author to deposit code and other information relevant to replicating the results in a permanent online archive at the time of publication and indicate that this has been done in a footnote to the paper (e.g., "Code used in these analyses is available in the ICPSR Publications-Related Archive").

Those who advise students can strongly encourage them to give anticipatory attention to the replicability of results and to make materials available online (see King 1995: 447 as an example of a department introducing standards for depositing materials from dissertation research).

All this said, obviously better still would be change in official policy of collective practice that is endorsed by the authority of editorial policy for ASA journals. A short editorial policy that would be relatively modest in its demands but consistent with the spirit of my arguments would be the following:

> Authors of accepted articles of empirical quantitative research are expected to use online archives to deposit maximum possible information pertinent to the verification of presented results at the time of publication. Ideally, data, code, and other materials would be provided that would allow others to duplicate the analysis procedures that lead from original data to presented results without the need for any additional information about what was done from authors. We recognize this ideal is often not possible or may conflict with accepted proprietary prerogatives of authors. In whatever ways information sufficient for duplicating results will not be provided, authors are expected to be explicit in the manuscripts they submit for review about whether and how this information can be obtained by other researchers. We ask authors to deposit whatever materials they can at the time of publication even if that information is not sufficient for verifying results (e.g., depositing the code for analyses even if the data are not distributed).

Increased attention to replication in both economics and political science shows that other disciplines recognize the inadequacy of individualistic policies and recognize the potential provided by the Internet and social replication policies to do better. A larger movement is afoot,

and it will continue regardless of whether or when sociology chooses to take part. Sociologists have expressed much concern about its public profile in recent years, and many feel many of the discipline's valuable research contributions do not receive the outside attention they deserve. If neighboring disciplines are willing to act to make their research practices as transparent and public as possible, and we who do quantitative sociology are not, we will have no one to blame but ourselves if our work has less visibility and credibility.

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Appendix A. Data Availability Policy for the American Economic Review. (Identical policy is articulated for *Journal of Economic Perspectives*).[16]

It is the policy of the *American Economic Review* to publish papers only if the data used in the analysis are clearly and precisely documented and are readily available to any researcher for purposes of replication. Authors of accepted papers that contain empirical work, simulations, or experimental work must provide to the *Review*, prior to publication, the data, programs, and other details of the computations sufficient to permit replication. These will be posted on the AER Web site. The Editor should be notified at the time of submission if the data used in a paper are proprietary or if, for some other reason, the requirements above cannot be met.

As soon as possible after acceptance, authors are expected to send their data, programs, and sufficient details to permit replication, in electronic form, to the AER office. Please send the files via e-mail to Jenna Kutz (jkutz "at" econlit.org), indicating the manuscript number. Questions regarding any aspect of this policy should be forwarded to the Editor.

Our policies differ somewhat for econometric and simulation papers, and for experimental papers.

For econometric and simulation papers, the minimum requirement should include the data set(s) and programs used to run the final models, plus a description of how previous intermediate data sets and programs were employed to create the final data set(s). Authors are invited to submit these intermediate data files and programs as an option; if they are not provided, authors must fully cooperate with investigators seeking to conduct a replication who request them. The data files and programs can be provided in any format using any statistical package or software, but a Readme PDF file documenting the purpose and format of each file provided, and instructing a user on how replication can be conducted, should also be provided.

If some or all of the data are proprietary and an exemption from this requirement has been approved by the Editor, authors must still provide a copy of the programs used to create the final results. We require this because the criterion for exemption from the data availability policy is that other investigators can, in principle, obtain the data independently. These authors must also provide in their Readme PDF file details of how the proprietary data can be obtained by others.

For experimental papers, we have a more detailed policy, including requirements for submitted papers as well as accepted papers. We normally expect authors of experimental articles to supply the following supplementary materials (any exceptions to this policy should be requested at the time of submission):

1. The original instructions. These should be summarized as part of the discussion of experimental design in the submitted manuscript, and also provided in full as an appendix at the time of submission. The instructions should be presented in a way that, together with the design summary, conveys the protocol clearly enough that the design could be replicated by a

[16] At this writing, this policy was available online at http://www.aeaweb.org/aer/data_availability_policy.html.

reasonably skilled experimentalist. For example, if different instructions were used for different sessions, the correspondence should be indicated.

2. Information about subject eligibility or selection, such as exclusions based on past participation in experiments, college major, etc. This should be summarized as part of the discussion of experimental design in the submitted manuscript.

3. Any computer programs, configuration files, or scripts used to run the experiment and/or to analyze the data. These should be summarized as appropriate in the submitted manuscript and provided in full as an appendix when the final version of a manuscript is sent in. (Data summaries, intermediate results, and advice about how to use the programs are welcome, but not required.)

4. The raw data from the experiment. These should be summarized as appropriate in the submitted manuscript and provided in full as an appendix when the final version of an accepted manuscript is sent in, with sufficient explanation to make it possible to use the submitted computer programs to replicate the data analysis.

Other information, such as applications to Institutional Review Boards, consent forms, or Web signup and disclosure forms, is not required or expected. If it desired to make this kind of information public, it should be posted on laboratory or authors' Web sites.

If the paper is accepted by the AER, the appendices containing instructions, the computer programs, configuration files, or scripts used to run the experiment and/or analyze the data, and the raw data will normally be archived on the AER Web site when the paper appears.