CSWEP NEWS

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Note: To better reflect CSWEP's fluctuating publication schedule, we have changed naming conventions. In past years "Issue II" was known as the "Spring/Summer" issue.

An Interview with Emi Nakamura

Serena Ng

Emi Nakamura, Associate Professor of Business and Economics at Columbia University, is the recipient of the 2014 Elaine Bennett Research Prize. Established in 1998, the Elaine Bennett Research Prize recognizes and honors outstanding research in any field of economics by a woman not more than seven years beyond her PhD. Professor Nakamura is recognized for her significant contributions to macroeconomics and related fields. Her research, which combines a powerful command of theory with detailed analyses of micro-level data has made important contributions to the study of price rigidity, measures of disaster risks and of long-run risks, exchange rate pass-through, fiscal multipliers, and monetary non-neutrality.

In 2011, Dr. Nakamura received an NSF Career Award. In 2014, she received an Alfred P. Sloan Fellowship and was named one of the 25 most influential economists under the age of 40 by the International Monetary Fund's *Finance & Development*. She is a research fellow of the NBER and serves on the Technical Advisory Board of the Bureau of Labor Statistics. She has published papers in most of the main economics journals and the papers are already well cited, in spite of Dr. Nakamura having only received her BA from Princeton University in 2001 and her PhD from Harvard University in 2007.

Visit CSWEP.org to view Nakamura's acceptance talk, *Positive Macroeconomics*, delivered at the CSWEP Business Meeting, held during the 2015 AEA Meeting in Boston.

Can you tell us something about growing up in an academic family of economists?

My parents love their work and really wanted to give me a sense of what they did. That's easy when your parents are firemen or policemen, but harder when your parents spend all their time sitting at a desk reading books and running regressions. How do you explain to a kid what it means to do research? So my mom brought me to a number of economics conferences when I was a child. Of course, I didn't understand much, but I did get some sense of what it meant to be an academic economist. It also led to some funny conversations when I grew up and met colleagues like Kevin Lang, who I'd first met as a child.

Because of my parents, I also got to take a bunch of economics classes at the University of British Columbia when I was in high school and over the summer when I was home from college in

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About the Authors

Daron Acemoglu, Elizabeth and James Killian Professor of Economics at the Massachusetts Institute of Technology and Editor-in-Chief of *Econometrica.*

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Nancy Lutz, Economics Program Director at the National Science Foundation.

Marjorie McElroy, Professor of Economics at Duke University and Chair of the CSWEP Board.

Amalia Miller, Associate Professor of Economics at the University of Virginia and a member of the CSWEP Board.

Serena Ng, Professor of Economics at Columbia University and a member of the CSWEP Board.

Ragan Petrie, Associate Professor of Economics at George Mason University and a member of the CSWEP Board.

Harald Uhlig, Professor of Economics at the University of Chicago and Head Editor of the *Journal of Political Economy.*

From the Chair

Marjorie B. McElroy

CSWEP News regularly features a popular section on some aspect of the professional development of economists. No exception, this issue features the first of a two-part series co-edited by Amalia Miller and Ragan Petrie on ethical issues involving publishing and grants. Editors from the *AER, Econometrica* and the *JPE* weigh in on publishing. Pinelopi Goldberg describes the evolution of her journal's policies to promote transparency, credibility and replicability and how these have evolved with changes in technology and data availability (proprietary, administrative and confidential data). The *JPE*'s Harald Uhlig notes that while ethical considerations are under constant discussion, his journal's policies have rarely changed. In an exception that proves the rule, he walks us through the initiation and discontinuation of the *JPE*'s second-editor approval process for revise-and-resubmit decisions and acceptances. *Econometrica*'s Daron Acemoglu draws on 8 years in editoral roles to advise five *don'ts* (don't get discouraged, over claim, be scared, be unethical or discourage others) and two *do's* (do go beyond the formal ethical guidelines and do have fun!).

Turning to ethical issues in grant writing, Nancy Lutz, Program Director of Economics at NSF, describes the entire grant review process and the interplay of the responsibilities of both "*ad hoc*" reviewers and NSF-panel reviewers to avoid institutional as well as personal conflicts. Noting that much of her discussion applies to NIH as well as NSF, Lutz encourages the use not only of NSF's online resources but also invites emails regarding complex, gray situations.

As you likely noted, for our cover story, Serena Ng interviewed Emi Nakamura on the occasion of winning the 2014 Elaine Bennett Research Prize. An inspiration to young researchers, Nakamura aptly describes her research and her career, including the process of "asymptoting toward tenure." See David Weinstein's introduction and her acceptance at CSWEP.org. Nakamura joins eight distinguished predecessors—Anna Mikusheva (2012), Erica Field (2010), Amy Finkelstein (2008), Monika Piazzesi (2006), Marianne Bertrand (2004), Esther Duflo (2002), Susan Athey (2000) and Judith Chevalier (1998). Nominations for the next Bennett Prize remain open through September 21, 2016 for nominees with PhDs from 2009 on.

Finally, Ivy Broder pays tribute to Barbara Bergmann, giving a brief overview of her career and influence, including Marianne Ferber's observation: "It may truly be said that Barbara has made it her mission to comfort the afflicted and afflict the comfortable."

Remember "**6, 3, 1**" for the upcoming January 2016 ASSA/AEA Meeting. CSWEP will sponsor six paper sessions, three mentoring breakfasts and one business meeting, featuring the presentation of the 2015 Carolyn Shaw Bell Award. Look for details in the next issue of the *CSWEP News,* including information on how to submit your paper for the 2017 AEA/ASSA Meetings.

I'm excited to report that CSWEP's Liaison Network now covers over 200 economics departments and other groups, with about 80 to go, see: **https://www.aeaweb.org/committees/cswep/Liaison_Network.php**. Liaisons distribute information on CSWEP activities and opportunities and help to round up data from departments in CSWEP's annual survey. Liaisons do essential work for CSWEP's mission—to monitor and promote the careers of women economists. Volunteer!

I hope this issue inspires you to forward *CSWEP News* to colleagues and students and to consider nominating an individual for the Bell Award, one for whom mentoring and inspiring others has been woven into the fabric of their professional life. See: **http://www.aeaweb.org/committees/cswep/awards/**.

As always, I'd love to hear from you—thoughts on CSWEP activities, ideas for the future and items for the Brag Box—**cswep@econ.duke.edu**. ■

Ethical Issues in Economics Research I

Amalia Miller & Ragan Petrie

Individual integrity in research is crucial for the advancement of science and accumulation of knowledge. Monitoring costs to ensure integrity are impossibly high for individual consumers of research, so our professional community relies on individual researchers, journal editorial boards and other institutions to adhere to and maintain standards of conduct that help guide the research process. The economics profession takes ethics in research seriously as lapses can hurt individual researchers and the profession as a whole and hinder scientific progress. Although many professional norms are informal and left unstated, journals and granting agencies have also responded to ethical concerns by adopting policies that require disclosure of conflicts of interest, data sharing after publication to enable replication, and documentation of protection of human subjects.

This issue of the *CSWEP News* addresses "Ethical Issues in Economics Research" with contributed articles from individuals representing different perspectives on the topic of how to ensure integrity in economics research and publication, including views from a program director at a federal grants agency and from editors of top economics journals. We asked our contributors to discuss how their journal or agency perceives its role in ensuring integrity in the research and review processes and what policies it has adopted to address ethical concerns. Examples of such policies include data disclosure and sharing requirements, rules to avoid, mitigate or disclose potential conflicts of interest, double-blind versus single-blind reviews, and Institutional Review Board (IRB) approval for human subjects research. Each contributor offers a unique and informative perspective on the topic and helpful advice for those engaged in the research process. In addition to the articles in this issue, readers may also be interested in the piece by Joshua Margolis, "Preparing for Ethical Challenges," published in the Winter 2003 *CSWEP Newsletter*.

Journal & NSF Submission Policies

American Economic Review
https://www.aeaweb.org/aer/authref.php

Econometrica
http://bit.ly/1NoKGxf

Journal of Political Economy
http://bit.ly/1TpuzYT

National Science Foundation
http://www.nsf.gov/bfa/dias/policy/

Conflicts of Interest and Ethical Research Standards at the AEA Journals

Pinelopi Koujianou Goldberg

In recent years, the American Economic Association has sought to improve the review processes at all of its journals: the *American Economic Review* (AER), the four *American Economic Journals,* the *Journal of Economic Perspectives* and the *Journal of Economic Literature*. To this end, it has both modified existing policies and adopted new ones. The general principles governing these changes are that: (a) the review process should avoid conflicts of interest; (b) the research results should be credible and—to the extent possible—replicable; and (c) the research that has led to the published results should have been conducted in a manner consistent with basic ethical principles.

One of the existing policies that was modified was the "double-blind" refereeing process. Starting in 2011, the *AER* transitioned from a "double-blind" to a "single-blind" policy; that is, the referees remain anonymous throughout the review process, but the names of the authors are disclosed to referees when the paper is sent for review. The double-blind policy aimed to address biases in the refereeing process that earlier studies of journals' processes and decisions had shown to exist. The discontinuation of the double-blind policy was not based on the belief that such biases no longer exist (of course one wishes that this is the case), but rather on the realization that in the age of search engines, double-blind refereeing is ineffective. In addition, the disclosure policies that were introduced shortly thereafter (as discussed below) were in obvious conflict with the double-blind policy given that the authors' eponymous disclosure statements are sent to the referees along with the paper.

The disclosure policy at the AEA journals requires authors to disclose any positions (current or previous), financial

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holdings or other issues that may lead to a conflict of interest for the specific research reported in the paper. All authors are required to submit disclosure statements at the time of the submission, and these statements are sent to the referees (hence, the conflict with the double-blind policy). If an author does not submit a disclosure statement, the paper is not checked in at the journal. The disclosure policy was implemented in response to increased demand, especially from outside the field of economics, for more transparency regarding authors' backgrounds and potential research motivations. The disclosure requirements as implemented at the AEA journals aim to accomplish exactly this, namely provide more transparency. But the journals have made clear that the acceptance criteria do not include the absence of a potential conflict of interest. Decisions are based (as they have always been) on whether the question the paper addresses is interesting and important and the research results are credible. If authors can convincingly demonstrate a certain point, we will publish the paper without regard to what motivated the research. Along the same lines, our instructions to the referees explicitly state that the evaluation of the paper and recommendation regarding publication should be based on the credibility and robustness of the results, and not on the potential motives of the authors. Of course, the existence of potential conflicts of interest calls for extra caution and induces referees and readers more generally to be vigilant. In my personal experience at the *AER*, I have not encountered any problems related to conflicts of interest since the disclosure policy's inception in 2012.

The rules governing the review process at the *AER* (as well as the other AEA journals) also seek to avoid potential conflicts of interest among editors and referees in the review of papers. As outlined in the annual Editor's Report, coeditors at the *AER* do not handle the papers of their colleagues, advisors, students, and current or recent coauthors. At times, there is a tension between avoiding potential conflicts of interest and the efficiency and quality of the refereeing process. For example, the most qualified coeditor and referees for a paper may all be related to an author. In such cases, it is a challenge for the journals' editors to strike a balance between maintaining the quality of the review process while minimizing the effect of potential conflicts of interest. A common approach in these instances is to involve more than one coeditor in the review of the paper.

Another recent change in journal practices regards the implementation of the data posting and replication policy. This policy had been nominally in effect at the *AER* for a very long time. But in reality it was not enforced. Improvements in data storage and processing technologies have made it possible for authors to submit their data and programs at the time of acceptance and for journals to store this information and make it available on the journals' web pages. Starting in 2004, the data posting and replication policy has been strictly enforced at the *AER*. Authors are required to submit all data and programs to the journal when their paper is accepted for publication. We then have an editorial assistant go through the files and replicate all results in order to ensure that the journal has everything needed for future replications. The data posting and replication policy was implemented in response to occasional complaints that published results could not be replicated as well as the desire to make practices in economics similar to those in the life and physical sciences. While the implementation of the policy has been running smoothly so far, it has a serious limitation that is becoming more and more severe over time: it has no bite in those cases where the data cannot be made publicly available, either because it is privately owned (by a company) or because it is restricted access. The increasing use of administrative data in recent years has made this issue more severe. In the 2014 Editor's Report of the *AER*, I reported that approximately 46% of all papers published in 2013 that used data had some type of exemption, so replication was not possible based on the data provided by the authors. The increasing role of confidential data clearly interferes with the replicability of results and has been of concern to all journals' editors. However, some of the most exciting, novel and thought-provoking research is based on data sets that are not publicly available, and driving such research out of the journals because the results are not readily replicable would be tantamount to losing out on some of the most interesting and promising contemporary research. Therefore, the consensus view has been that it is prudent to continue the current practice of allowing for exemptions from the data-posting policy in cases where the data cannot be made publicly available while encouraging authors to provide all information needed for other researchers to get access to the same data.

I will briefly comment on two additional requirements that the AEA has introduced in recent years. The first requirement is that authors state whether they have obtained Institutional Review Board (IRB) approval for their research (in those cases where IRB approval is called for). The reason for this requirement is to ensure that research, especially in and on developing countries, is conducted in accordance with the basic ethical principles all major educational institutions in the U.S. adhere to. The AEA stopped short of mandating IRB approval for paper submission to its journals because many authors, especially in foreign countries, are affiliated with institutions that do not have IRBs and it did not seem fair to punish them for their institutions' policies. The hope was that the AEA policy of requesting information on IRB status would serve as an impetus for such institutions to establish IRBs.

Finally, the AEA established a registry for Randomized Controlled Trials (RCTs). This registry is in operation, but importantly, registering RCTs with it is *not* a requirement for submitting and publishing papers in the AEA journals. The

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Ethical Issues in Grant Review

Nancy Lutz

Granting agencies like the National Science Foundation (NSF) and the National Institutes of Health (NIH) rely on the research community's contributions to the grant review process. Those contributions are absolutely essential, but they also raise the specter of possible ethical conflicts. NSF takes this issue seriously; the integrity of the merit review process rests on our ability not just to avoid conflicts but also to keep the trust of the research community. In other words, both actual conflicts and the appearance of conflicts matter. My goal here is to give an overview of how NSF approaches conflict of interest issues in the grant review process. Two necessary disclaimers: I am not speaking officially on behalf of NSF, and I will be using non-legal language in the interest of clarity.

One obvious approach would be double-blind reviewing, in which reviewers do not know the identity of the principal investigators. Double-blind reviewing is more difficult in practice than in theory because reviewers can sometimes infer the principal investigator's identity from other information in the proposal (for example, from the references). Even if this issue could be avoided, it is hard to imagine how NSF could implement double-blind reviewing without a significant change in the merit review standards. NSF's official Merit Review Guidelines require that reviewers evaluate (among other things) the qualifications of the "individual, team, or organization" to carry out the research. (See NSF's Grant Proposal Guide (GPG), Chapter III.A for the full and legal statement of the Merit Review Guidelines. The GPG is available at http://nsf.gov.) It is hard to see how reviewers can evaluate a researcher's qualifications in a serious way without knowing the researcher's identity.

Instead of a double-blind process, NSF relies on a set of policies and practices that focus on avoiding conflicts. Those policies are based on federal law about conflicts of interest in the administration of federal funds. We divide potential conflicts of interest into two categories: (a) conflicts about institutions and employers, and (b) conflicts about personal connections.

Institutional conflicts: Proposals come to us from institutions (e.g., universities), and those institutions have an obvious financial interest in obtaining grants. NSF prohibits reviewers from reviewing proposals that would benefit a current employer. Therefore, Professor Smith who works at the University of North Illinois (UNI) cannot review any proposal that includes funds going to UNI; this is also true if Professor Smith has been paid by UNI in the last 12 months or is applying for a job at UNI.

Conflicts about personal connections: NSF prohibits reviewers from reviewing proposals where they have certain personal connections with the principal investigator or other people who would be funded by the grant. This includes, for obvious reasons, parents, children, spouses/partners, and other household and family members. It includes dissertation and postdoc advisors and advisees; in other words, I have a life-long conflict with my advisor and with my own dissertation students. It also includes all collaborators within the last 48 months (and a collaboration continues until publication by our rules). Finally, it includes all co-editors for journals or conference proceedings in the last 24 months.

Obviously, these policies are only as good as our practices for identifying potential conflicts of interest and keeping conflicted reviewers out of the process. Those practices include: (a) extensive and repeated training for NSF program directors, and in-house legal counsel who specialize in conflict of interest issues; (b) requiring information from all principal investigators about conflicts of interest; and (c) requiring legal statements from all reviewers and panelists that they have disclosed all conflicts. We use that information to make sure that reviewers do not have conflicts with the proposals they review.

In-house training and resources: NSF staff are trained in handling conflict of interest issues; this includes both training for new NSF program directors and managers and required annual training for current staff. Each work unit at NSF has an identified "conflicts of interest" officer who is available to consult with program directors, and we have three in-house attorneys in our General Counsel's office who are also on-call to answer questions and give us legal rulings as required. In addition, our Inspector General's office takes reports from the community about potential violations of our conflicts of interest policies.

Researchers are required to disclose information about potential conflicts of interest as part of the proposal. NSF requires a "biographical sketch" from each senior researcher on the proposal, and researchers have to list collaborator, advisor/advisee and co-editor relationships. NSF staff check these documents after proposal submission, and if the information is not there we require that the proposal be revised before review can start.

NSF uses two different kinds of reviewers: "panelists" review a group of proposals and discuss them at a panel

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Goldberg

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registry was established in response to strong interest from those who are involved in RCTs and would like to have a permanent record of not only those trials that produced clear results (and were published) but also of those cases where the results were weak or inconclusive.

Overall, the policies at the AEA journals have evolved in order to ensure that the published research is transparent, credible and (to the extent possible) replicable and that it adheres to the same ethical standards endorsed by higher education institutions in the U.S. New technologies and data regularly pose new challenges, so it is likely that these policies will continue to evolve.

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meeting, and "ad-hoc reviewers" are asked to review individual proposals. Both kinds of reviewers use our FastLane website to look at proposals and submit reviews. FastLane requires that reviewers read information about NSF conflict of interest rules and certify that they have no conflicts. The actual form for submitting reviews includes space for identifying other possible conflicts of interest. In addition to this, panelists get briefed at the beginning of any NSF panel meeting about conflicts of interest and sign statements that they understand the rules and have disclosed all relevant conflicts.

NSF does not use conflicted ad-hoc reviewers. Of course, mistakes do happen and we do sometimes request a review from someone who is conflicted. Every program director sooner or later receives a review where the last section says, "I am now collaborating with this person, but my review was not affected by the relationship." We do not delete the review; it's retained in our database system. However, we "mark" the review as conflicted; it is not available to panelists and it's not used in the decision process. NSF program directors justify all funding decisions in written memos to their supervisors (Division Directors); those memos include a full statement about conflicts of interest in the review and are double checked against the database.

What about conflicted panelists? This is a little more complicated because NSF panels vary enormously in composition and workload. One panel may review 200 proposals at a panel meeting, and another panel may review only 20 or 30; this varies across NSF based on the nature of the competition. The iron-clad rule is that no panelist evaluates a proposal on which she has a conflict. She may under certain circumstances be part of a panel meeting that includes the proposal, but she leaves the room for discussion of the proposal and does not see the reviews or the identity of reviewers (access is blocked in our database). In general, panels that review a small number of proposals will include no conflicted panelists. Panels that review larger numbers of proposals (such as the Economics Program) may include panelists who have conflicts with some of the proposals reviewed at the meeting. The exact decisions here are not made by NSF program directors but by our attorneys, who are consulted about the "conflict rules" for all NSF competitions. Again, all panelist conflicts are noted in the database and in the official records of the meeting.

These formal rules and policies can only cover the kinds of relationships that can be described in writing. Some economists are concerned about more subtle kinds of conflicts: "Professor X is unfairly biased against my kind of research," or "Professor G dated my daughter in grad school." We have formal and more informal mechanisms to help us with these kinds of conflicts as well. NSF allows investigators to provide lists of suggested proposal reviewers AND lists of people who should not review the proposal. Program directors are always willing to answer questions from reviewers about possible conflicts of interest. The general rule we use here is, "If the reviewer thinks there is a conflict, there is a conflict." In other words, we do not want anyone to be reviewing in circumstances that make them uncomfortable.

So what advice do I have for scholars who are thinking about submitting or reviewing proposals? Agency staffers take conflict of interest issues very seriously indeed because we know that the integrity of the process depends on avoiding both actual and perceived conflicts. Sometimes when we ask for required information it may seem like unnecessary red tape, but we need that information to conduct reviews in an ethical way. Don't hesitate to let us know about other potential conflicts. Sometimes telling us about a conflict may involve private information you are reluctant to share (about job searches or personal relationships), but we keep those confidences. When in doubt, please do email your program director with your concerns; it helps us do our jobs in the best possible way. ■

Brag Box

"We need every day to herald some woman's achievements . . . go ahead and boast!"
—Carolyn Shaw Bell

Martha Bailey, Associate Professor of Economics at the University of Michigan, was named First Vice-President of the Midwest Economics Association.

Janet Currie, Professor of Economics at Princeton University, was named President-Elect of the Eastern Economics Association.

Li Feng of Texas State University, **Jessica Hennessey** of Furman University, **Adrienne Lucas** of the University of Delaware, **Raechelle Mascarenhas** of Willamette University, and **Evelina Mengova** of Governors State University were awarded tenure and promoted to associate professor.

Julie Hotchkiss, Federal Reserve Bank of Atlanta, was named Vice President of the Southern Economic Association.

Marjorie B. McElroy, Professor of Economics at Duke University and CSWEP Chair, was elected Vice President of the Society of Labor Economists (SOLE). She will succeed **Janet Currie,** Professor of Economics at Princeton University and current President of SOLE, in 2017–18.

For the first time, the top economists in both U.S. antitrust enforcement agencies are women. **Nancy L. Rose,** Charles P. Kindleberger Professor of Applied Economics at MIT and former CSWEP Board Member, was appointed the Deputy Assistant Attorney General for Economic Analysis in the U.S. Department of Justice Antitrust Division in September 2014. **Francine Lafontaine,** the William Davidson Professor of Business Economics and Public Policy and Professor of Economics at the University of Michigan, joined the Federal Trade Commission as its Director of the Bureau of Economics in November 2014.

We want to hear from you!
Send announcements of honors, awards, grants received, promotions, tenure decisions and new appointments to cswep@econ.duke.edu. It will be our pleasure to share your good news with the CSWEP Community.

Ethical Issues in Economics Research: The *Journal of Political Economy*

Harald Uhlig

Economics is a science. As a science, replicability of findings and peer review are crucial. Necessarily, this involves judgment by fellow scientists in their role as authors and referees as well as editors. Science is a human enterprise. Whenever humans are involved, conflicts of interests and the vagaries of subjective assessments come into play. It would be foolish to believe otherwise. Nonetheless, authors, referees and editors can strive for the ideal and rigor of an objective and detached evaluation of results and the objective and detached pursuit of scientific truth. The colleagues that we most respect and the journals that we hold in the highest esteem tend to rank high in coming close to reaching that ideal. And, by implication, should authors, referees, editors or a journal as a whole cease to pursue this ideal with the appropriate vigor, they may soon find that their reputation will be damaged. As a community of scientists, we then turn to other authors or journals. The marketplace for scientific ideas, the competition for reputation and the competition between outlets to publish reputable scientific findings creates the necessary peer pressure to advance our science closer to the ideal of objectivity and creates the ongoing cleansing within our profession, even if individual authors or journals fail to abide by these standards. The system is not perfect. However, it is hard and perhaps impossible to think of a better one.

The *Journal of Political Economy* (JPE) is one of the premier journals in economics. It has a long and proud tradition. It has a reputation for publishing reputable scientific findings of the highest quality and impact. Its editors seek to uphold that reputation for the future. Therefore, they are constantly vigilant that scientific standards and ground rules are upheld. This may not always work perfectly. Each time it does not, the journal may be damaged. This we seek to avoid. Upholding these scientific standards may mean different things in different circumstances, but some general principles can be spelled out.

Electronic means have made it far easier to store material online in association with a published paper and to encourage replication of results with programs and datasets created by the authors or newly created by those who wish to check the published results. The *American Economic Review* (AER) was perhaps the first major journal in economics to spell out a detailed policy here. We adopted the policy shortly after the *AER* did, in response to evolving norms in economics and evolving technological possibilities. In order to keep the norm consistent, we used their policy verbatim. The policy was adopted before my time as lead editor, so I do not know the specific circumstances. I do not recall that there was a specific case or issue that led us to react to that situation by adopting that policy. However, there is wide agreement that this is a wise policy and that we should stick with it.

I do not have data to assess the effect of the policy on submissions or research. Given that the policy is known and used by a number of the leading journals, researchers presumably now adjust their procedures accordingly early on when pursuing a project, seeking to appropriately document their data and the steps taken. We do know that the data archives are accessed. Furthermore, we regularly get questions about the policy (1–2 a month, on average). Overall, we feel that research has become more replicable as a result, and that is surely a good thing.

In principle, referees are furthermore supposed to check the submitted material as to whether the results can be replicated. In principle, it might be desirable to hire graduate students to check into the replicability in detail. As a matter of practice, referees tend to check key findings and often ask authors to provide additional details and insights in the various stages of the review process. However, they tend to perhaps not check all the details, trusting instead in the interest of the authors to uphold their reputations. And as a matter of practice, we have found hiring students to replicate the results is prohibitive in a number of ways: it is costly, it delays publication and it may be seen as a burden by authors. We trust that key results will be investigated by others, and contested, should they be found faulty. Papers that are published in the *JPE* tend to draw a lot of attention, so we hope that the detailed refereeing process, the online material and the peer pressure generated by potential replicators is sufficient to assure reputability of the published results and sufficient care by authors in making sure their results are correct and replicable. Generally, referees are remarkably conscientious of getting “under the hood” and into the details of a submission and often provide the journal with remarkable effort and the author with considerable expertise and guidance on checking on the results and clarifying the key findings. Thus, while more could be done in principle, it is already remarkable how much gets done in practice. It would be hard to impose even more of a burden on all sides. This is no invitation to complacency, obviously, and we implicitly monitor challenges as they arise and keep thinking about potential adjustments for the future.

We currently do not require a disclosure statement about potential conflicts of interest. There may have been a reason for the *AER* or other journals to pursue this. So far, I am not aware of particular instances or reasons to do so at the *JPE*. Potential conflicts may arise, of course, if particular scientific findings by an author might jibe with his or her commercial

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Do's and Don'ts of the Publication Process

Daron Acemoglu

For many young economists, the process of publication is a mysterious one. What are the appropriate outlets? What determines the likelihood of success? What are the things to avoid? There are no straightforward answers to these questions. That being said, my own experience—first as a young and inexperienced author, then as a somewhat older but still inexperienced author, then as an advisor to over 50 PhD students, and then over the last eight years as coeditor and editor of *Econometrica*—has taught me a number of lessons on the do's and don'ts of the publication process that I would like to share.

Don't get discouraged. It's easy to get discouraged. I experienced firsthand, both as a graduate student at the London School of Economics and as a junior faculty at MIT, how one may get frustrated by an early stream of rejections from top journals. It took me a while to ultimately realize that the fault was partly (largely?) mine: I had to learn to express my ideas better and improve the craftsmanship of my work. Those lessons are even more relevant today: With ever-increasing competition from many more researchers from around the world, it is becoming harder and harder for young scholars to publish in the premier journals of our profession. Ultimately, it is the scientific content of a paper that matters most. But craftsmanship and clarity are absolutely necessary not just for getting published but also for the paper to communicate its content and ultimately be impactful.

All the same, there is also no denying that the publication process has a significant random component. As an editor, you cannot survive if you do not accept that you will make both type I and type II errors. But this makes it no less frustrating for authors, especially young authors, suffering as a result of the type I errors. The understanding that randomness is an integral part of the publication process should help you to persevere, and keep in mind that those mentoring you and ultimately evaluating you probably also understand the random nature of this entire process.

Do not overclaim. The pressure of getting published, and the discouragement that will inevitably come at various stages, should not obscure what is most important: the quality of the research. The best strategy is to let the quality of your research speak for itself. The high-stakes publication game does create incentives to overclaim, either in the form of not giving enough credit to prior literature or exaggerating the originality or robustness of results. We are probably all guilty of this to some degree. But stepping back, my sense is that not overclaiming is a winning strategy. For example, one might be tempted not to give enough credit to prior or contemporaneous related work. But besides being unethical, it is also a strategy that often backfires. Often, recognizing the contributions of others increases the value of one's own paper. The same is true for robustness of results. "Stress testing" of empirical and theoretical results is an integral part of the research process. But it would be unreasonable to expect that, in the complex world of social science, a result can be robust to all variations. Openly highlighting under what conditions a theoretical result might be overturned or when an empirical result ceases to hold will not only be more honest but also ultimately more revealing for readers.

Don't be scared. The decision of where to submit one's work is a difficult one. Some papers, by their nature, will be appropriate for more specialized outlets. (A useful question is: Is this paper of interest to specialists working on related problems or for a broader audience? If it's the former, the paper is unlikely to be appropriate for a top general interest journal.) The feedback from colleagues and experts will also give you an indication of whether a paper has a shot at top journals, and sometimes it may be best to be realistic in the choice of venue. But generally, if you are going to err, I would recommend erring on the side of aiming a little too high rather than aiming too low. Aiming too high does create a negative impact on the profession because it increases the total amount of refereeing and editorial resources that will be used up as a paper makes the rounds across several journals. But discouraging individual researchers—especially young researchers—from being ambitious with their research would be a greater cost. It's an unfortunate side of our profession that almost everybody gets a taste of rejections (I count myself lucky when one out of every three submissions gets ultimately accepted). And rejection is never pleasant. But it is also important to take risks both in terms of trying new things and trying to get them published in the best possible journals.

Ultimately, it would be our profession that loses if its young researchers shy away from ambitious research and from attempting to publish their work in the most visible outlets. In many disciplines of science, new ideas and approaches often come from younger scholars, and economics is no exception. It is then critical for us to engage and stimulate the next generation, and for young scholars to strive to make an impact and aim high in the process.

Do follow the ethical guidelines. In an age when the economics profession is under a greater spotlight than before, several organizations, including the American Economic Association, the Econometric Society and the National Bureau of Economic Research, have strengthened their ethical guidelines. These guidelines include: explicit disclosures on conflict of interest, which have been important in other disciplines but less so in the social sciences until now; strict adherence to established protocols of human subjects, for

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example as set out by an Institutional Review Board (IRB); and a general effort on research integrity and ethical behavior. By and large these are minimal requirements, and following these ethical guidelines is in the interest of both researchers and the profession at large. In many cases, we can and should do better than these minimal guidelines. For instance, our guidelines now require posting of data and programs for published papers. But in many cases, researchers should do more than the minimum and strive to make their programs easy for other to understand and use. As another example, some institutions provide loopholes for researchers to design experiments without IRB approval, but my view is that it is unacceptable for economists not to take every precaution against harming human subjects or to depart from other aspects of ethical behaviors when it comes to experimental work.

Don't discourage others. All of us play an important role in the profession as reviewers of others' work. It is tempting to be as tough on others as our toughest critics have been on us. But often it is both more rewarding personally and socially for us to try to see the positive in others' contributions. As an editor, it is both refreshing and gratifying to see referees trying to find the positive contribution in the paper rather than working hard to shoot it down. Moreover, encouraging others to do new things and original research and take risks is also essential for the advancement of the profession.

The value of cooperative behavior of course extends well beyond the refereeing process. Collaborating with and helping others is ultimately its own reward, but with the increasingly collaborative nature of economics research, as witnessed, for instance, by the growth of co-authored papers, such cooperation is becoming even more important. Working with, encouraging and seeing the positive in the work of others will not only help your colleagues but will also enable you to improve your own ideas and work.

Do have fun. Despite all the difficulties that young researchers are likely to encounter, ours is an open, exciting and ultimately rewarding profession. It allows us to pursue interesting and important questions of social science, sometimes with the promise of giving us clues about how to improve the lives of millions of people around the world. It encourages cooperation and teamwork, enabling us to work with other first-rate minds.

At the center of all this is our quest for knowledge and original scientific research, which is what the entire profession of economics and the publication process are built on. Many scholars are drawn to economics because they have an enthusiasm for the questions and the methods of our discipline, and it is most important for all of us as individuals and collectively as a profession to keep this enthusiasm alive because the vibrancy of our profession and our hope of impacting the world around us depend on it. And good, impactful research is more likely to result when we are all enjoying the process of generating ideas, developing them and crafting papers. ■

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interests due to, say, consulting or other efforts. However, so far such cases seem to be sufficiently rare as to prevent us from an overall policy of imposing detailed declarations by authors. Our foremost interest as a journal is to publish key scientific breakthroughs; there is a cost in imposing too much bureaucracy on authors to do so. As editors, we keep these matters in mind on a case-by-case basis, and we might adopt a change in policy in the future if the current one disappoints.

With regard to the issue of single-blind refereeing versus double-blind refereeing, we rely on the good conscience of reviewers to disclose potential conflicts and biases, which they frequently do. The editors decide on a case-by-case basis whether and how to use reports from self-reported potentially biased reviewers. While there has been a move in hard sciences to return to double-blind review, it appears to us to be difficult (if not impossible) to ensure the integrity of a double-blind process, especially in the age of the Internet. When a paper is submitted to the *JPE,* it frequently has been circulated and been presented at various venues and may be quite well known already. Even if not, it often is easy to find out about a paper and authors by using appropriate Internet searches. Finally, there also is a benefit in knowing the identity of the authors: it provides the referee with some guidance on where to pay most attention when evaluating the proposed findings, given the existing reputation and known expertise of the authors. Obviously, the identity of the referees needs to be protected and kept unknown to the author, in order to receive honest and clear assessments. We are very conscientious of this issue. The rare and unfortunate instances in which the identity was not fully protected have cost our reputation dearly.

For some time, the *JPE* required second-editor approval on revise-and-resubmit and accept decisions. I suspect that might have helped in some cases to balance overly favorable opinions, but it would have had no effect in cases where reviewers and/or editors held a negative bias. The second-editor approval process was discontinued because it was time consuming, leading to costly delays in publication.

All these policies are under constant discussion. We may not change policies lightly, and we may change them rarely. That does not mean that we are complacent about their effects or what needs to be done. We shall and we will act swiftly if the need for a policy change arises.

Acknowledgement: This article was written with considerable help and input by Connie Fritsche, managing editor at the *JPE.* ■

Remembering Barbara Bergmann

Ivy Broder

Barbara Rose Berman Bergman—economist, mentor and teacher, music lover, advocate for social justice, activist as well as mother and wife—died in April 2015. She was born in the Bronx in 1927 to immigrants from Eastern Europe. In 1965, she married Fred H. Bergmann, National Institutes of Health microbiologist, who died in 2011. They had two children, Sarah and David. Barbara would proudly talk of Fred's equal partnership in their household as being far ahead of its time.

Barbara had an undergraduate math major at Cornell University and a PhD from Harvard University. Between studying for those degrees, she worked at the New York office of the Bureau of Labor Statistics. She then continued government service as a senior staff economist at President Kennedy's Council of Economic Advisors, taught for two years at Brandeis University, and joined the Brookings Institution as senior economist from 1963–65. Her academic career continued at the University of Maryland where she taught from 1965–88, and at the American University (AU) where she taught from 1988–97 before retiring as Distinguished Professor Emerita. At AU, she developed new economics courses at both graduate and undergraduate levels called "Women in the Economy" and "Sex Roles in Economic Life" and she created the field in gender economics in the doctoral program.

Barbara participated in the caucus of women economists at the 1971 American Economic Association (AEA) Meeting that drafted the resolutions presented at the AEA Business Meeting to establish CSWEP. She later served on the CSWEP Board from 1983–86 and was chair from 1983–85. Barbara also served as president of the International Association for Feminist Economics, vice-president of the AEA, president of the American Association of University Professors, president of the Eastern Economics Association and president of the Society for the Advancement of Socio-Economics, among others. Her stature in the profession was also reflected in biographies and tributes, including a 1998 special issue of *Feminist Economics* honoring her and her work.

In 2004, Barbara received the Carolyn Shaw Bell Award for her pioneering work on the economic emergence of women in the United States and in recognition as a "renowned scholar whose research combined theory, quantitative modeling, and policy analysis on issues such as unemployment, urban development, discrimination, poverty and women's status."

One of Barbara's early major contributions was on the "crowding" hypothesis, which argues that gender discrimination leads to segmentation of labor markets between men and women. Since women were not allowed to compete for certain "male" jobs, women's labor markets became relatively more crowded, leading to lower wages for women compared to men. She was also the first to critically analyze the occupation of "housewife," arguing that the long tenure of such a job is associated with high physical and financial risks. But it is *The Economic Emergence of Women* (1986, 2006) that is considered to be her most significant work. In a review by Paulette Olson, it was described as ". . . one of the first books of its kind, examining the evolution of women's dual careers as homemakers and labor force participants . . . [and it] contains Bergmann's policy recommendations for achieving an equitable future such as pay equity, affirmative action and childcare assistance."

In the 1980s, Barbara turned her attention to the economics of family issues, especially child care and child support. She strongly and passionately advocated for subsidized child care as a means to reduce poverty among single mothers and lauded many aspects of the French child care system. She wrote on affirmative action and comparable worth in both academic journals and media publications. Barbara was also well known for her contribution to economic policy debates through her columns in the *New York Times* and articles in *Academe, Challenge* and *Times Literary Supplement*.

Although Barbara is best known for her work on gender-related issues, her publications on agent-based modeling and methodology in the 1970s and 1980s were significant as well, culminating in a 1990 article in the *Journal of Economic Perspectives*. In almost any conversation with Barbara up to the time of her death, she would lament on the methodological approach of most economists and urge them to spend more time on direct behavioral observations rather than isolated theorizing.

It is most fitting to end this tribute with a quote from Barbara's friend, the late Marianne Ferber (whose remembrance was published in the Winter 2014 *CSWEP News*), "There are few who have earned as much warm gratitude for generous help to her colleagues, or have gained as much genuine admiration for their dedication as Barbara, of whom it may truly be said that she has made it her mission to comfort the afflicted and afflict the comfortable."

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Vancouver, including a number of classes on economic measurement from Erwin Diewert. Measurement is a really understudied topic in economics today and you don't learn much about it even in grad school, so that was a unique opportunity. I have since written several papers on measurement issues where this experience was very useful.

You left home and Canada to attend college in the U.S. at a relatively young age. What was the transition like?

I was definitely not the most worldly of students when I went to college, partly because I was pretty young. I was lucky I went to Princeton, where life revolved around a small college campus. I remember being terrified when I took my first math exam that I would be massively outclassed by all the kids around me from expensive private schools since I went to a regular public school in Canada. It was quite a relief when I realized I was able to hold my own and actually do well. Princeton was an incredible experience overall, both socially and academically—I encountered all sorts of interesting personalities that I never would have imagined existed, and had amazing intellectual experiences.

Did you know you wanted to be a macroeconomist when you went from Princeton to Harvard, and what got you interested in the details of the data?

Not at all. I did my undergraduate thesis at Princeton on business cycles, mainly because I heard that Mike Woodford (now my colleague at Columbia) was a great senior thesis advisor. Having Mike as an advisor was a tremendous intellectual experience that really made me curious about macroeconomics—but I still wasn't sure of my field when I went to Harvard.

I always knew, however, that I wanted to work with data—this goes all the way back to my parents, who imprinted on me a deep sense of the importance of testing theories empirically. So the first thing I really studied was econometrics, since I was sure I would need it. I took almost two years of graduate econometrics from Bo Honore at Princeton that I've relied on ever since to do empirical work. And then when I came to Harvard, I took classes on all sorts of empirical topics, from labor economics to finance to industrial organization.

In the end, I had both Robert Barro and Ariel Pakes on my thesis committee, which I'm not sure had ever happened before! (A chapter in my thesis was on empirical IO.) But this eclectic background was a blessing for me in the long run since I think it gave me a unique perspective on macroeconomics.

How do you and Jon Steinsson come up with so many good research ideas one after another? What's the secret?

I think the key thing is to work on things that inspire you. I sometimes tell students that everyone I've known who has been successful in research has put an incredible amount of time into it, and that's hard to do unless you are truly fascinated by what you're working on. I think it's easy to find things to be fascinated by in macroeconomics since there are so many big questions on which we don't have great evidence. But of course, I might be a little biased on this . . . .

Your child is almost two years old now. What has changed since Odin's birth, what has been the biggest challenge and how has new family changed the way you work?

Every day is more fun since Odin has arrived. Of course, the days are also shorter and my schedule has to be more careful and compact. I can't stay at the office until 9:00PM anymore to finish a paper! Jon tells me I am good at outsourcing. This helps me save time so that I can focus on my family and my work.

What would you say is the biggest challenge in your career so far?

The last part of grad school and the first few years of being an assistant professor are very stressful. Journal decision lags are long, so it matters a lot what happens with every paper. Of course, all this stress can make you more productive.

You were a very young assistant professor in a business school. Some students were probably older than you!

That's true. The first year I taught, there were actually a couple of students I had gone to college with. I think that was helpful in being able to relate to the MBA students.

How are things different before and after receiving tenure?

Not all that different! Before getting tenure, you imagine having a huge celebration the day it happens. But actually, I found the experience to be more like "asymptoting toward tenure"' rather than getting tenure on any specific day since there are so many steps in the process, from getting your papers accepted to the final votes at the University. I remember Steve Levitt giving a talk when I was in grad school saying that by the time you get tenure you are so ensconced in the research lifestyle that it's hard to do anything different. I think that is true. And of course, I enjoy what I'm doing.

Your mom is an economics professor who started her career a generation ago. How do you think things have changed for women in the economics profession over these years?

Things have improved massively. When my mom went to grad school there was a quota of two women in her PhD class, and when she was a kid, she remembers women not even being able to enter the Harvard graduate library. There still aren't a lot of women in a field like macroeconomics. But I think there has been a sea change since when my mom entered the profession. ■

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Related Articles

"Two Cheers for CSWEP?" Barbara R. Bergmann, *The Journal of Economic Perspectives*, Vol. 12, No. 4 (Autumn, 1998), pp. 185–189 http://www.jstor.org/stable/2646902

"Pushing for a More Humane Society," *CSWEP News*, Fall 2005 http://bit.ly/1OmMaPo

"Barbara Bergmann Wins 2004 Carolyn Shaw Bell Award," *CSWEP News*, Winter 2005 http://bit.ly/1eh53Hk

"Barbara Bergmann, VFA Fabulous Feminist," *The Fabulous Autobiographies*, Veteran Feminists of America http://bit.ly/1TQZcsd

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