Report on Improving the Publication Process in Economics

By an ad-hoc joint AEA-EEA-ES-RES committee (Joseph Altonji, Guido Imbens, Kevin Lang, Erzo Luttmer, Imran Rasul, Stefanie Stantcheva, and Romain Wacziarg). This report is intended to encourage discussions of the future of the economics publications process. It reflects the recommendations and ideas of the committee members and does not necessarily reflect the positions of the American Economic Association, the European Economic Association, the Econometric Society, or the Royal Economic Society

1. Executive Summary

The formal organization of the publication process in economics has remained remarkably stable over a long period of time, with only gradual changes in turnaround time and practices and the emergence of a few new journals. In February 2024, the American Economic Association (AEA), the European Economic Association (EEA), the Econometric Society (ES), and the Royal Economic Society (RES) established an *ad hoc* joint committee tasked with discussing ways to improve the publication process in economics. The committee was asked to refrain from discussing competitive aspects, such as pricing, page charges, referee compensation, review duration, and publication timelines. The decision to implement any recommendations will rest with individual journals and/or professional associations.

Journals traditionally play three roles in the research process: (i) dissemination of research, (ii) editing/improving the quality of the research, and (iii) certification of the research. The importance of these roles within economics has changed substantially over time and differs from other academic disciplines.

Dissemination. Although journals were traditionally the early disseminators of economics research, this clearly has changed. For many years, authors have been posting their research in departmental working paper series, and now they also post on preprint sites, such as arXiv. The publications of a number of disciplines embargo authors from posting research before it is published (e.g., *Nature* and *Science*), but in economics, there is no such tradition. We see no path where dissemination recovers its former importance for economics journals again.

Editing. The role of editors and reviewers in making substantive comments on papers to improve the quality of the work is greater in economics than in other disciplines and has increased since the 1990s and early 2000s. As a result, the time from submissions to decisions is longer than in other disciplines, reviews are longer and are more detailed and prescriptive, and the peer review process tends to require multiple rounds of revisions. It is not clear that this increased emphasis on trying to improve the papers has, in fact, done so. Although there are some claims to this effect (Hadavand, Hamermesh, and Wilson, 2024), there appears to be widespread concern that reviewers and editors may often come too close to playing the role of an anonymous coauthor (Siemroth, 2024). A few journals have pushed back on this by committing to a maximum number of rounds of revisions. The limited time reviewers and editors

spend on a paper relative to the authors calls for coeditors and reviewers to focus on major issues of the paper and to exercise restraint in micromanaging details of the revision.

Certification. In economics, publishing a paper in a particular journal is a stamp of approval that is used both by authors to signal the quality of the paper and by departments in hiring decisions and rankings. This role of certification and implicit ranking has increased in importance over time (many hugely influential papers were at one time published in nominally lower-ranked journals), placing increased stress on authors to develop submission strategies. Given the prohibition on simultaneous submissions and the length of the review time, researchers often plan to submit to a sequence of journals, with the time from first submission to eventual publication sometimes becoming extremely long.

To better understand the issues and to solicit views on a range of topics under consideration, the committee launched a survey which was emailed to the membership of the four professional associations. We received 2,986 completed responses.

The large majority of survey respondents viewed the publication process as very inefficient (38%) or somewhat inefficient (41%), reflecting on the editing part of the publication process. Slightly more respondents saw the publication process as somewhat meritocratic (41%) or very meritocratic (8%) than as somewhat unmeritocratic (29%) or very unmeritocratic (13%), reflecting on the certification part of the publication process. Nevertheless, we consider it a major concern for the profession that a large fraction of respondents do not consider the publication process to be meritocratic, given the importance of publication records in employment decisions.

The open-ended comments on the survey revealed what respondents saw as the key sources of inefficiency and the key factors that undermined meritocracy.

The factors that were most prominently mentioned as limiting the efficiency of the publication process were (i) long lags between initial submission, editorial decisions, and ultimate publication, (ii) excessive demands by reviewers for revisions, which were often viewed as idiosyncratic or having low returns for the work involved; (iii) too little editorial guidance and lack of protection by editors from all the reviewer demands; (iv) papers, online appendices, and response letters to reviewers becoming excessively long; (v) too many rounds of revision; and (vi) duplication of reviewer effort when a manuscript is reviewed by subsequent journals.

The key factor that is seen as undermining meritocracy is network connections among economists, often described as clubbiness, in-crowds, elitism, and nepotism. Closely connected to this concern is the view that journals have rules and procedures that favor well-resourced researchers, often employed at leading institutions, who almost always are also part of elite networks. Another frequently mentioned concern was that journals are biased toward authors from, and topics pertaining to, the United States and, to a lesser extent, Europe.

Identifying policies that journals can adopt to address these problems is a challenge. Some of the problems, such as the role of networks, reflect the profession and the structure of academic institutions more broadly. Other problems were outside the remit of this committee because they concern dimensions on which journals compete. Finally, the committee recognizes that policies may have unforeseen, unintended consequences and that this calls for humility in its recommendations.

The committee identified four complementary approaches to addressing the key issues facing the publication process. The first approach is to recommend policies with a clear benefit of harmonization, for example, because of network externalities, or to minimize costs to authors of tailoring papers for specific journals. The committee hopes these policies will become professional norms. The second approach is to recommend policies regarding transparency that make it easier for authors and readers to compare journals, thereby fostering competition among journals. The hope is that the resulting increased transparency will incentivize all journals to find innovations that increase meritocracy and efficiency. The third approach is to recommend policies that the committee believes would foster efficiency and/or meritocracy, but where the benefits of harmonization are more limited. Hence, here journals may differ in their choices, with some adopting them and others holding off. Finally, in the fourth approach, the committee shares the ideas and viewpoints of respondents without offering a recommendation.

The committee hopes that these ideas and viewpoints will encourage journals to think creatively about policies to become more meritocratic and efficient and that resulting policies that turn out to be successful will proliferate through competition among journals. We note that other professions have significantly different publication cultures and customs and that there has been relatively little change in the publication process in economics. Perhaps the main change in recent years has been the emergence of new journals such as the *American Economic Journals* initiated by the AEA, *Quantitative Economics* launched by the ES, and the new *JPE* journals. Without taking a position on what is right, we think there would be value in more experimentation in economics and greater differentiation in publication processes. Experimentation could take many forms, but we note that many other disciplines have journals that publish different types of submissions beyond standard research papers, including letters to the editor, comments, interviews, and reviews that are not common in economics. In this regard, we think a step in the right direction is the founding of *AER: Insights* as a journal focusing on shorter papers and some journals adopting policies establishing a norm of a single revision. Experimentation in reviewing policies and in communication between reviewers and authors would also be informative.

The committee recognizes that some recommendations may be viewed as second order (or in the words of some respondents, “arranging deck chairs” or “window dressing”). However, the view of the committee is that it is worthwhile to pursue all policies that can help improve the meritocracy and efficiency of the publication process and help to set norms, even if not enforceable. The committee did not explicitly prioritize the recommendations—the recommendations below do not appear in order of importance as views will invariably differ on the relative importance of each recommendation. However, the committee wishes to emphasize

the importance of the set of recommendations that directly address the profession's concern over meritocracy and efficiency. For instance, recommendations related to reporting data on journal performance metrics go directly to the issue of efficiency. Advocating for stricter and more transparent conflict-of-interest rules is meant to directly address issues of fairness and meritocracy. The full rationales for the committee's recommendations are explained in the subsequent sections of this report.

A. Recommendations on Policies with Returns to Harmonization

- **Recommendation A1: Reviewer transfer mechanism**
  We recommend that journals (i) establish policies regarding the circumstances under which they, upon an author's request, will accept a transfer of reviews and reviewer identities from a journal that had previously reviewed the submission; (ii) ask reviewers, as part of the review process, if they may share the review and the reviewer's identity with other legitimate journals requesting that information; and (iii) establish safeguards that ensure that a reviewer's identity is shared, if the reviewer granted permission, only with a current coeditor[1] of a legitimate journal handling the paper that the reviewer reviewed.
- **Recommendation A2: "Essential" vs. "optional" reviewer comments**
  We recommend that journals ask reviewers to separate their comments into essential and optional comments, that they tell authors that their response letters need not address optional comments, and that they tell reviewers that they should not recommend rejection of a revision for failure to implement optional comments. We further recommend that journals should provide guidance on the extent and nature of essential comments consistent with recommending revision at that journal.
- **Recommendation A3: Not uploading manuscripts to AI**
  We recommend that journals communicate to reviewers that they should not upload any part of a manuscript they are reviewing to AI, whether for having AI generate a report or for other purposes.
- **Recommendation A4: Clarifying AI use**
  We recommend that journals ask researchers to acknowledge in their submissions the use of AI beyond spell checking and grammar checking (e.g., drafting and rewriting sections) and to acknowledge that they remain responsible for all content, whether based on AI or not.
- **Recommendation A5: Author order**
  We recommend that journals require authors to state in the opening footnote how author order was determined but leave the choice of the principle governing the ordering (e.g., alphabetical, by contribution, random, or otherwise) to the authors.

[1] Journals differ in the titles they use for what we call editors, coeditors, and associate editors. In this report, we use the term editor to refer to a person who assigns manuscripts to coeditors, the term coeditor to a person who invites reviewers and makes the final or near-final decision on the manuscript's disposition, and the term associate editor to a person who, when requested, provides advice on individual papers or a referee report to a coeditor on a regular basis either with significance or frequency beyond that of a standard referee. At many journals, the editor is also a coeditor.

- **Recommendation A6: Use of ⓡ symbol**
  We recommend that journals should use the symbol ⓡ between authors' names for papers with a random author order, both in the reference section and for in-text citations.
- **Recommendation A7: Contribution statement**
  We recommend that journals encourage contribution statements clarifying the contribution of each author but require them only if the number of authors exceeds a specified number. Contribution statements may state that all authors contributed equally, but this is discouraged for cases when the number of authors is sufficient for a mandatory contribution statement.
- **Recommendation A8: Responsibility statement**
  We recommend that, upon acceptance of a manuscript, journals require that all authors indicate that they take responsibility for all aspects of the paper unless the authors provide a statement allocating responsibilities among the authors in the opening footnote and explicitly state that some author has no responsibility for a certain aspect of the paper.
- **Recommendation A9: Citation style**
  We recommend that journals adopt the following citation style: List all authors' last names (up to some reasonably large number) and the year of publication the first time. Subsequent mentions of papers show the initials of the authors' last names and the year of publication, e.g., BGA2019. Journals can choose various modifications of this style.
- **Recommendation A10: Discourage terminology that promotes artificial distinctions between journals**
  We recommend that authors, reviewers, and editors do not use terms that promote distinctions of journals into fixed tiers, such as "top 5."
- **Recommendation A11: Collecting ORCID iD upon paper submission**
  We recommend that journals incorporate ORCID iDs for all authors as a standard component of the manuscript submission process. This information would not be shared with reviewers or anyone from whom the authors' information should be blinded.
- **Recommendation A12: Reviewer disclosure of prior review**
  We recommend that journals ask reviewers who have previously reviewed a paper for a different journal to disclose this to the coeditor upon receiving the invitation to review the paper.

B. Recommendations on Policies That Foster Transparency

- **Recommendation B1: Harmonized reporting of journal performance metrics and editorial policies**
  We recommend that journals include on their website harmonized information on their performance and policies. The harmonized information consists of (i) standardized metrics on journal performance, such as decision times, desk rejection rates, and number of rounds for accepted papers and (ii) information about their editorial policies. The initial set of metrics to be reported is defined in the appendix of this report. We recommend the creation of a Journal Information Center that would compile this information on a central website. After the Journal Information Center is formed, we

recommend that journals follow this center's definition of the statistics to be reported and also report the statistics directly to them.

- **Recommendation B2: Harmonized reporting of decisions by author categories**
  We recommend that journals report on their website a breakdown of submissions and decision types by a standardized set of author categories. The initial suggested set of metrics and author categories to be reported is defined in this report. Suggested author categories include institutional rank, geographic region, and gender. After the Journal Information Center is formed, we recommend that journals follow this center's definition of the statistics to be reported and also report the statistics directly to them.
- **Recommendation B3: Harmonized reporting of decisions by reviewer categories**
  We recommend that journals report on their website a breakdown of submissions and decision types by a standardized set of reviewer categories. The initial set of metrics and reviewer categories to be reported is defined in this report. After the Journal Information Center is formed, we recommend that journals follow this center's definition of the statistics to be reported and also report the statistics directly to them.
- **Recommendation B4: Survey of authors**
  We recommend that, upon the request of the Journal Information Center, journals send a manuscript-specific link to a survey, administered by the Journal Information Center, to authors whose paper received a final decision.
- **Recommendation B5: Asking authors to disclose conflicts of interest**
  We recommend that journals ask authors, as part of the submission process, to disclose conflicts of interest with the journal's editor or coeditors. This disclosure complements the journal's own efforts in identifying conflicts of interest.
- **Recommendation B6: Asking reviewers to disclose conflicts of interest**
  We recommend that journals ask reviewers, as soon as they are invited, to disclose to the coeditor any conflicts of interest with the authors. Having, or working on, a competing paper should also be disclosed. It is up to the coeditor's judgment whether to retain or excuse the reviewer.
- **Recommendation B7: Identification of the handling coeditor**
  We recommend that journals publish in the opening footnote the identity of the person who decided to accept the paper (typically, this is the handling coeditor).
- **Recommendation B8: Communication of conflict-of-interest policies**
  We recommend that journals adopt a conflict-of-interest policy and publish it on their websites.
- **Recommendation B9: Communication of appeals policy**
  We recommend that journals publish their appeals policy on their websites: both the process and valid grounds for appeal.
- **Recommendation B10: Communication of term limits**
  We recommend that journals publish on their websites any term limits for editors, coeditors, associate editors, and board members.
- **Recommendation B11: Selection procedures for the editorial team**
  We recommend that journals publish on their websites how editors, coeditors, associate editors, and board members are selected and what criteria are used (including qualifications, experience, and diversity of institution, background, and geography).

C. Recommendations on Policies with Limited Returns to Harmonization

- **Recommendation C1: Not revealing author identities to reviewers**
  We recommend that journals consider instituting a practice by which they avoid revealing author identities to reviewers. Under this practice, journals would ask authors to submit a version of the manuscript that does not reveal their identities and would share this version with the reviewers. Journals would also ask the reviewers to refrain from looking up author identities and not rely on these identities if they know them.
- **Recommendation C2: Desk rejection protocol without author identities**
  We recommend that journals consider adopting a protocol by which they avoid revealing author identities to coeditors and ask coeditors not to search for them at the desk rejection stage. If adopted, the identities of authors would be shared with the coeditor only after the coeditor has committed to a decision on whether to send the manuscript out for review so that they can avoid inviting reviewers with conflicts of interest.
- **Recommendation C3: Data and code policies**
  We endorse a general principle of transparency whereby data, code and, if applicable, pre-analysis plans and experimental protocols used in accepted empirical papers should be made available publicly by authors, with flexible allowances for disclosed and explained special circumstances. Journals may consider signing on to an existing data and code availability standard (such as DCAS).
- **Recommendation C4: Adoption of term limits**
  We recommend that journals adopt meaningful term limits for editors, coeditors, associate editors, and board members.
- **Recommendation C5: Diversity of institution, background and geography in the editorial team**
  We recommend that journals aim to have an editorial team with a wide range of backgrounds, institutions where they were educated, current institutional affiliations, and geographical locations.
- **Recommendation C6: Limit concentration of influence**
  We encourage journals to ask their editors and coeditors to avoid holding simultaneous editorial positions at other journals and, ideally, to refrain from simultaneously holding positions that allow them to select participants for recurring influential conferences or members of professional networks.
- **Recommendation C7: Limit coeditor workload**
  We recommend that journals attract a sufficient number of coeditors so that the workload of each coeditor allows them sufficient time to clearly explain the rationale of their decisions and to offer clear guidance on revision requests to authors.
- **Recommendation C8: Training materials**
  We recommend that journals provide coeditors with materials that give them guidance on their job. Journals should consider publishing these materials on their website.
- **Recommendation C9: Providing guidance to reviewers**
  We recommend that journals provide general guidance to reviewers regarding the structure, content, and length of a good review and, when appropriate, coeditors provide guidance about particular issues or sections for the reviewer's focus. Having this guidance, or a link to it, in the reviewer invitation email would be helpful. Coeditors

should also be encouraged to provide *ex post* feedback concerning reports that are unprofessional in tone, and, if necessary, call out such reports in their decision letters.

- **Recommendation C10: Sharing decision letter and reports with all reviewers**
  We recommend that journals share decision letters and all referee reports with all the reviewers of a paper unless there is a good reason for an exception in a specific case.
- **Recommendation C11: Awards for excellence in reviewing**
  We recommend that journals institute an annual award to recognize outstanding reviewers.
- **Recommendation C12: Transmission of review completion to ORCID**
  We recommend that journals collect ORCID iDs for reviewers who wish to have their completed reviews acknowledged in ORCID and transmit the completion of each review to ORCID, along with the year in which the review was completed, but without identifying the paper that was reviewed.

D. Ideas and Viewpoints for Consideration by Journals and Associations

The survey respondents, as well as members of the committee, offered many additional ideas that journals could consider to improve their meritocracy and efficiency. However, the committee refrains from recommendations in favor or against these ideas for one or more of the following reasons. First, the committee is not sufficiently confident the idea constitutes an improvement or the committee worries about unforeseen negative consequences. Second, the idea may work well for some journals but not for others. Third, the idea can be refined and more evidence on its effectiveness can be gained if there is experimentation across journals. These ideas are mentioned in Appendix C of the report.

E. Issues for the General Profession to Consider

In Section 7 of the report, we come back to collate points that do not directly relate to journal policies, but where collective action by associations and other organizations might further help meritocracy and efficiency of the publication process, address other issues highlighted by the survey, or be complementary to other recommendations put forward.

2. Charge to the Committee

In February of 2024, the American Economic Association, the European Economic Association, the Econometric Society, and the Royal Economic Society established an *ad hoc* joint committee tasked with proposing ways to improve the publication process in economics, in light of the substantial technological advances in the last half century but the relatively limited changes to the publication process. The decision to implement any suggestions will rest with individual journals.

The committee acknowledges the importance of innovation within individual journals and the adoption of successful new practices across the field. Our goal is to supplement this ongoing innovation by offering recommendations that are most effective when broadly adopted and to offer recommendations that allow authors and readers to make more informed choices among journals.

The committee was asked to refrain from offering recommendations on competitive aspects, such as pricing, page charges, referee compensation, review duration, or publication timelines.

The committee was appointed by professional associations with elected leaders, partly based on editorial experience. The background leading to this appointment is described in Appendix A. The committee recognized that it is not representative of the profession in many respects, including the background of its members, their current institutional affiliations, the institutions where they were educated, and their geographic locations. The committee therefore launched a survey to solicit views and ideas from the membership of the professional organizations that formed the committee. The results from the survey helped shape the committee's recommendations. In addition, the committee is making the survey results public.

3. Problems with the Current Publication Process

3.1. Sources of Problems with the Publication Process

The committee's assessment of the problems with the publication process in economics is based on four main sources. First, the committee fielded a survey of the members of the four associations that set up the committee. The survey both informed the committee about members' views regarding problems with the publication process and their ideas on how to address them.

Second, the committee was informed by academic publications that analyze how the publication process in economics operates. This literature is quite extensive, and a full literature review is beyond the scope of this report. Instead, we list papers that informed our thinking, even if not cited in the report itself, in the reference section. Our broad takeaway from this literature is that the publication process in economics faces issues both in terms of efficiency and meritocracy. Publishing in economics has slowed over time and is much slower than in natural sciences; papers have gotten longer; journals require more extensive revisions; and competition for journal space has become more intense, especially for the most prestigious journals (Ellison, 2002a; Card and DellaVigna, 2013; HHW2024). Economics is also a rather hierarchical discipline in which influence is more concentrated than in other related disciplines, both in terms of workplace institutions and in terms of journals (Wu, 2007; Fourcade, Ollion, and Algan, 2015; Wright, 2023; Ductor and Visser, 2023; Freeman, Xie, Zhang, and Zhou, 2024). The high concentration of influence amplifies concerns about the effects of networks on the publication process. Many papers provide evidence that network connections increase the likelihood of publication, though papers differ on the underlying mechanism, with Cloos, Greiff, and Rusch (2023) and Carrell, Figlio, and Lusher (2024) showing results indicating favoritism, with Laband and Piette (1994a), Medoff (2003), and Brogaard, Engelberg, and Parsons (2014) showing results indicating that networks provide information about quality, and with Colussi (2018) not providing evidence on the mechanism. In addition, as Bayer and Rouse (2016) point out, "the economics profession includes disproportionately few women and members of historically

underrepresented racial and ethnic minority groups." Although the evidence is somewhat mixed, there is cause for concern that the publication process contributes to this underrepresentation (see the discussion of this literature in section 3.4).

Third, several other initiatives analyzed issues with the publication process in economics, discussed possible solutions, offered recommendations, or undertook concrete steps to address issues. Charness, Dreber, Evans, Gill, and Toussaert (2022) wrote a very thorough report based on an original survey of nearly 1,500 respondents as well as an extensive literature survey, documenting problems with the peer review process and discussing or listing 164 potential ways to improve the process.[2] More material from this initiative is available on a dedicated website. Siemroth (2024) also analyzes issues with the peer review process, and his paper recommends six concrete proposals that could be reasonably easily adopted. Juan Carlos Suárez Serrato built a website that compares journal turnaround statistics for 14 journals.[3] Bayer, Kalemli-Özcan, Pande, Rouse, Smith, Suárez Serrato, and Wilcox (2019) developed best practices for economists, including practices relevant to journals, reviewers, and editors. The committee found these initiatives very informative and tried to incorporate their findings into this report.

Fourth, the committee members have witnessed many problems firsthand in their roles as authors, reviewers, and editors. The committee recognizes that their personal experiences and what they observed are unlikely to be representative.

3.2. Overview of the Survey

The American Economic Association, the European Economic Association, the Econometric Society, and the Royal Economic Society emailed a link to the survey to their members. In addition, the survey link was distributed on social media. The survey was fielded between June 13th and July 4th of 2024, and resulted in 4,079 people initiating the survey and 2,986 completed responses. The analyses of the survey are limited to completed responses but include the 4% of respondents (42 individuals) who did not indicate membership in any of the four sponsoring associations. A PDF of the full survey instrument is available here.

The first part of the survey asked respondents about their membership in the four associations, their position in their institution, their research field, and their country of residence. The response rate (for completed responses) to the survey was 13% among AEA members, 11% among EEA members, 19% among ES members, and 18% among RES members. While it is important to keep in mind that the response rates were low, the composition of responses in terms of seniority, country of residence, and subfield appears to align reasonably well with the membership composition of the four associations. About 39% of respondents are based in the

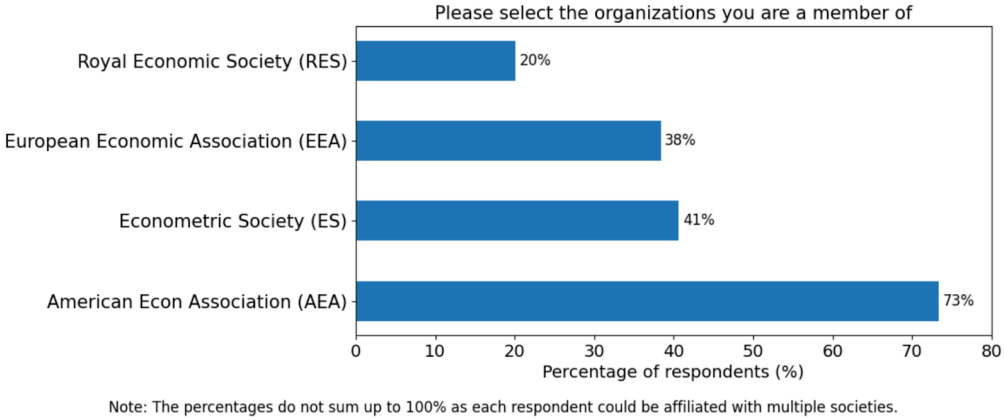
[2] The report is available here: https://evalresearch.weebly.com/uploads/1/3/3/4/133478410/improving_peer_review_in_economics_-_charness_et_al..pdf

[3] The address of his website is: https://jcsuarez.shinyapps.io/journal_turnaround_app/

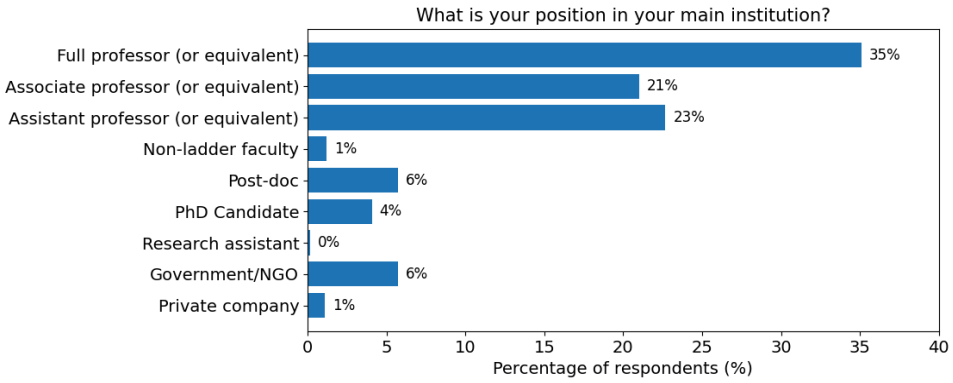
US, 45% in Europe, and 16% in the rest of the world. The breakdown by membership, position in one's institution, and main research field is shown in the figures below.

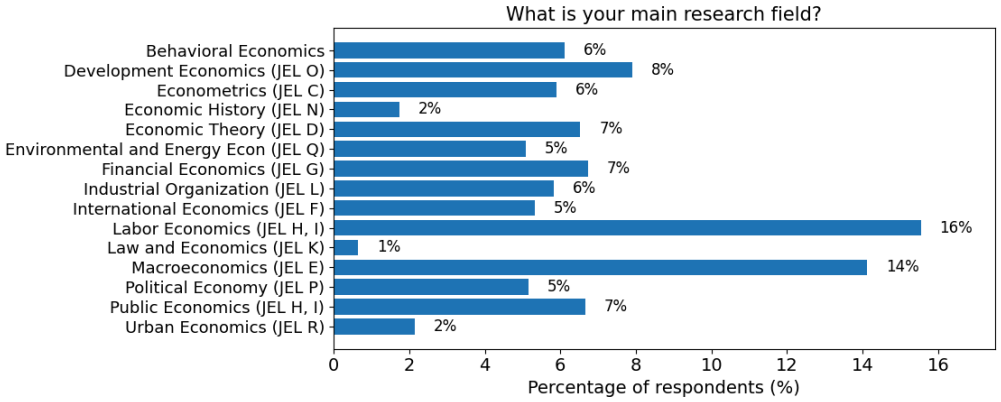
One part of the survey elicited members' views about issues and broad solutions related to meritocracy and efficiency in the publication process more generally. The responses to this part of the survey are analyzed in the remainder of this section of the report. The survey also asked more in-depth questions about particular aspects related to the publication process. Those parts of the survey are analyzed in subsequent sections of the report. We see the survey of CDEGT2022 as complementary—that survey collected detailed characteristics of authors and reviewers and also provided rich information on author and reviewer experiences and expectations. The CDEGT survey has very good coverage among behavioral and experimental economists, whereas our survey was sent to a broader group of economists. However, we have fewer author characteristics by which to judge the representativeness of the respondents.

Our survey contained open-ended questions in each of its parts, as well as a general open-ended question at the very end. The committee found the responses to these questions very illuminating because they offered insights into the reasoning of respondents, and they allowed respondents to bring up concerns and solutions the committee had not considered. The responses to the open-ended questions were categorized using artificial intelligence with considerable human guidance to ensure that the categories were meaningful and not overly broad. Details of this process are described in Appendix D of this report. In addition, committee members read all open-ended responses for a number of questions.



Note: The percentages do not sum up to 100% as each respondent could be affiliated with multiple societies.

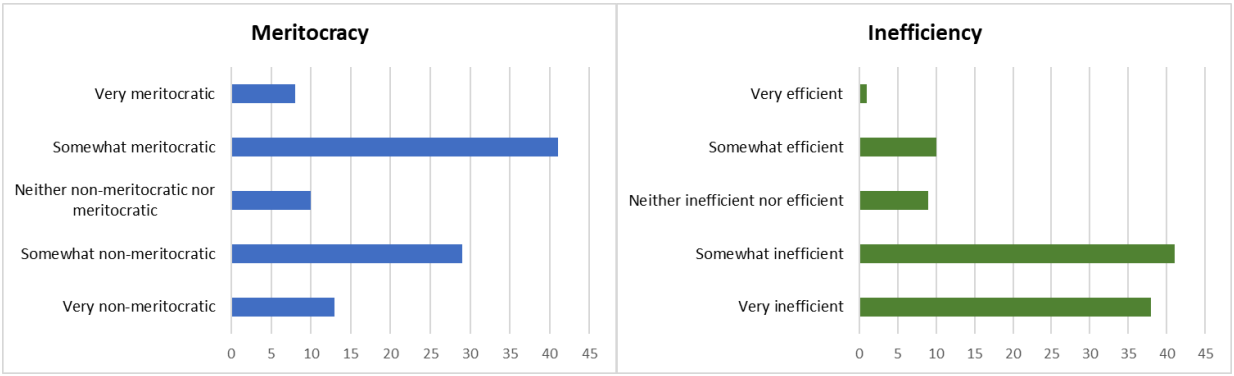




Descriptive statistics of all closed-ended questions, both in aggregate and broken down by respondent region of residence, seniority, and affiliated professional association, are available here. In addition, this link also shows the categorization of open-ended responses with examples of responses in each category (for an explanation of the methodology used to categorize open-ended responses, see here).

3.3. Meritocracy and Inefficiency

We first asked respondents their overall views on: (i) the meritocracy of the publication process in economics, which could be interpreted as the extent to which the process ultimately leads to the publication of the most scientifically impactful work; and (ii) the efficiency of the publication process, which could be interpreted as the smooth functioning of all stages of the process from the perspective of authors.

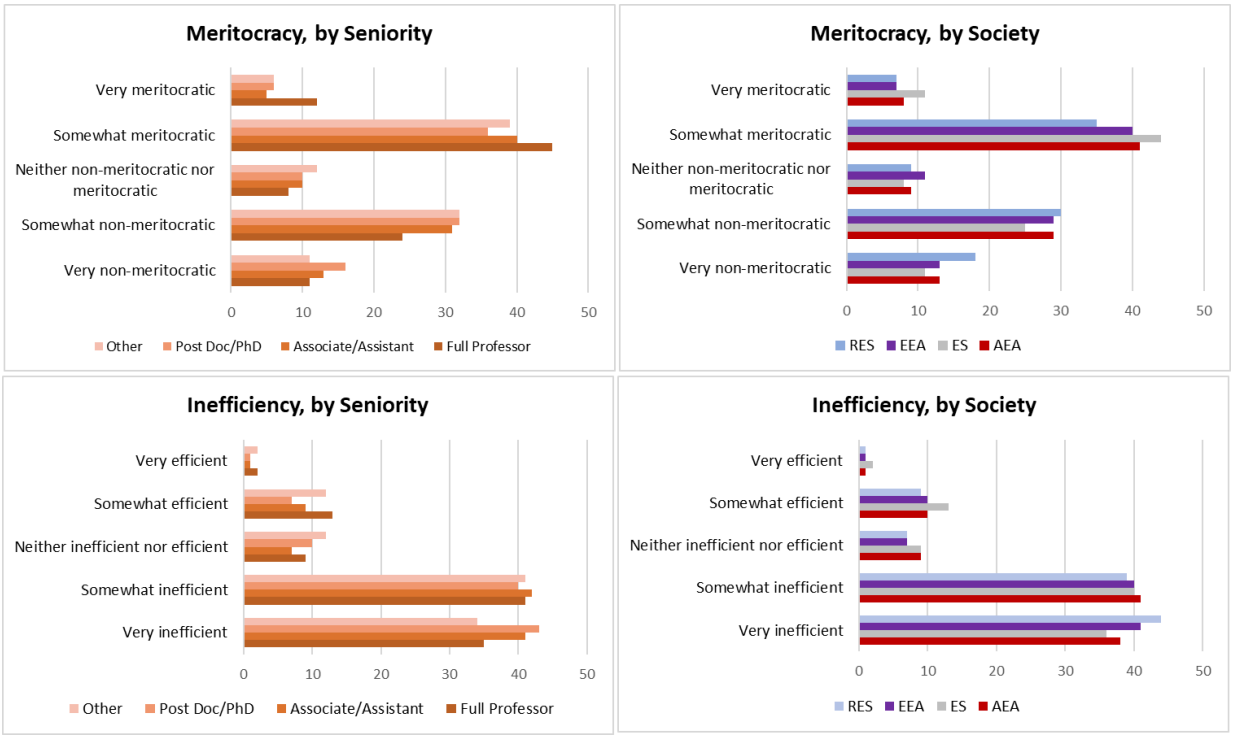


Survey respondents do not rate the publication process in economics as functioning extremely well on either margin, with particularly negative views expressed related to the efficiency of the process.

On meritocracy, around half (49%) of respondents report the process as either being “very” or “somewhat” meritocratic whereas 42% view the process as either “very” or “somewhat” non-meritocratic. Views are more skewed with regard to the efficiency of the publication

process: 79% of respondents report viewing the process as being either “very” or “somewhat” inefficient. Only 1% of respondents report the current publication process in economics as being “very efficient.”

The broad patterns of concern are generally held by the seniority of respondents and by their society of affiliation (noting that individuals can report being a member of more than one society). However, it is notable that the share of full professors reporting the process to be very meritocratic (12%) is double that reported in other groups (associate/assistant professors, post doc/PhD students, and other researchers), though in all groups few view the process as very meritocratic. Members of the Econometric Society are most likely to report the process to be very meritocratic (11% versus 7-8% in other societies), with RES members most likely to report the process to be very non-meritocratic (18% versus 11-13% in other Societies).

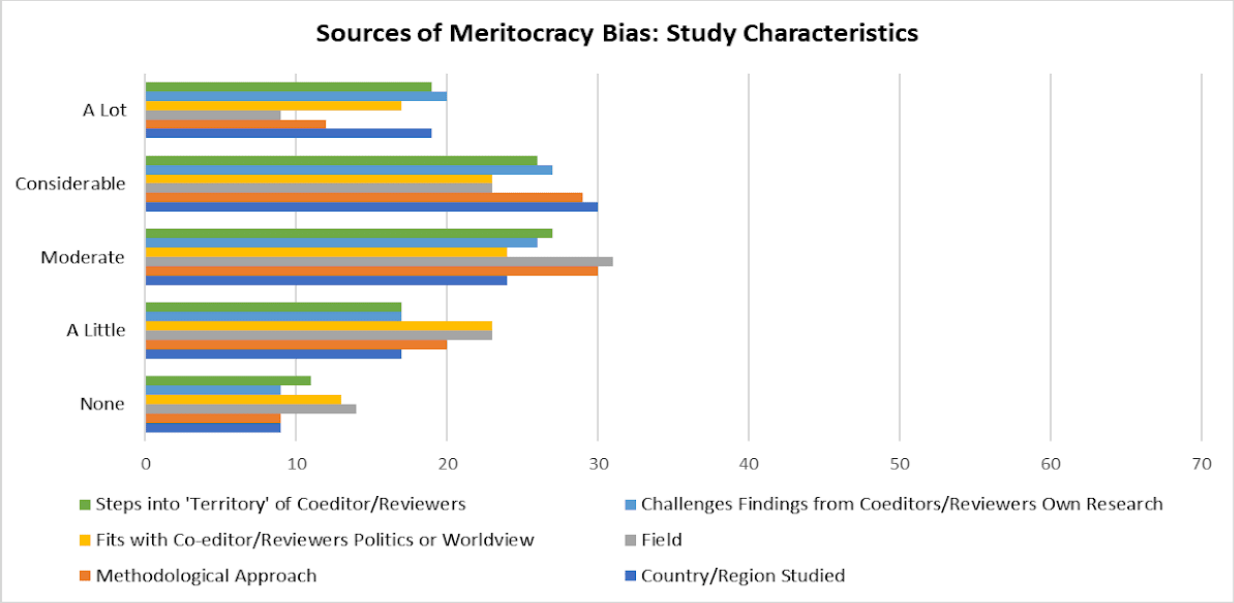


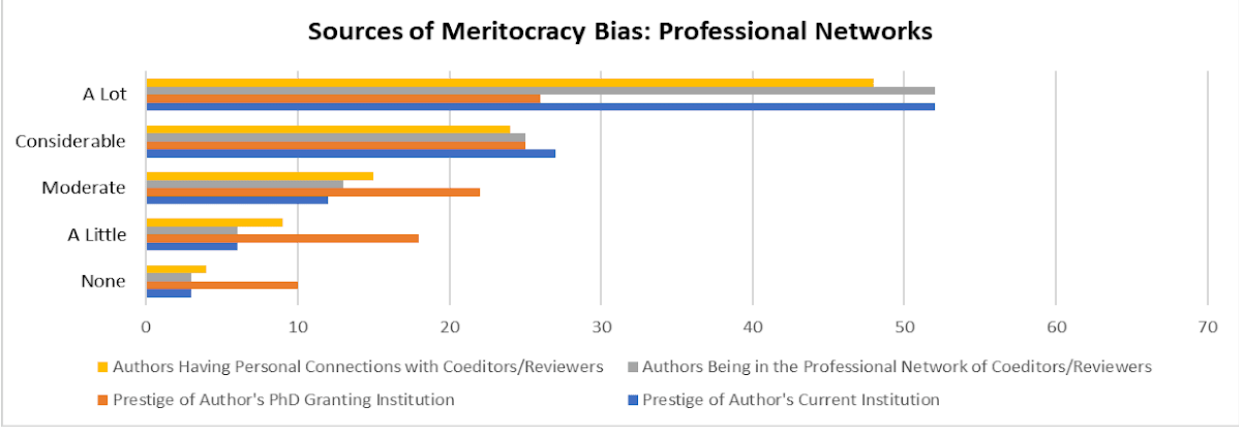
On the efficiency of the publication process, no group of respondents by seniority views the process as being very efficient (1 or 2% in each group) with between 34 and 41% of respondents in each group reporting the process to be very inefficient. Views on this margin are also similar across societies.

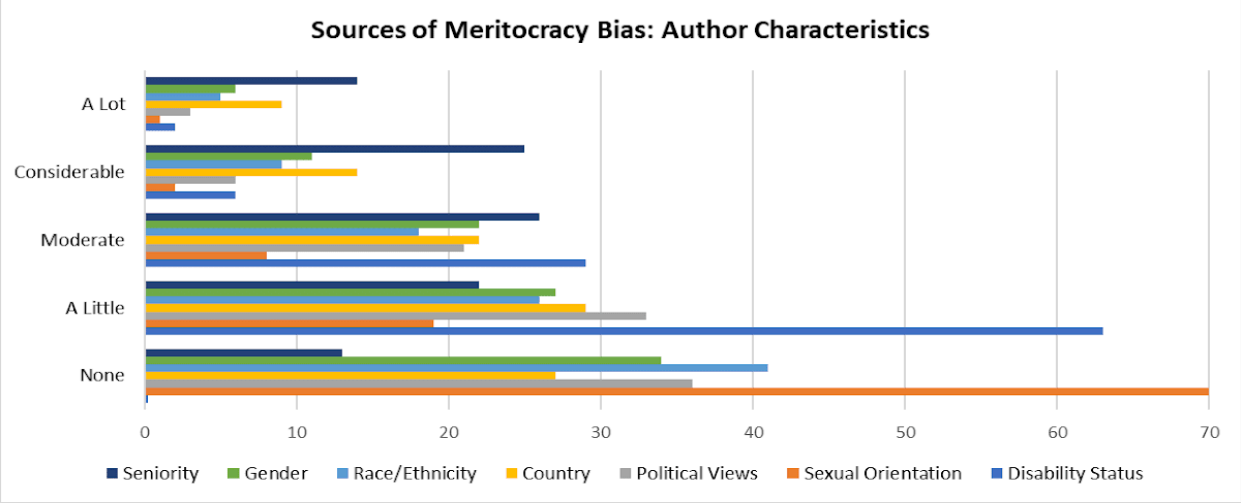
3.4. Perceptions of Sources of Meritocratic Bias

The next set of questions asked respondents about their views of the underlying sources of bias hampering the meritocracy of the publication process—leading coeditors or referees to accept or reject papers for reasons other than scientific merit alone. We split these among three types of sources of bias: (i) those related to characteristics of the study, (ii) those related to

professional networks, and (iii) those related to non-institutional characteristics of authors. Below, we report views on each dimension under each source. To aid comparability across sources of bias, we maintain the same x-axis scales across the three panels.







The picture that emerges is that factors related to professional networks are viewed to be the most important source of bias in the publication process, which is consistent with evidence from the literature (Laband and Piette, 1994a; Medoff, 2003; BEP2014; Colussi, 2018; CGR2023; CFL2024). More precisely, around half of the respondents view that being in the professional network of coeditors/reviewers, having personal connections with coeditors/reviewers, and the current institution of authors all matter “a lot” for publication outcomes. The prestige of an author's PhD granting institution is generally seen to be a less influential source of bias than the other three factors related to professional networks.

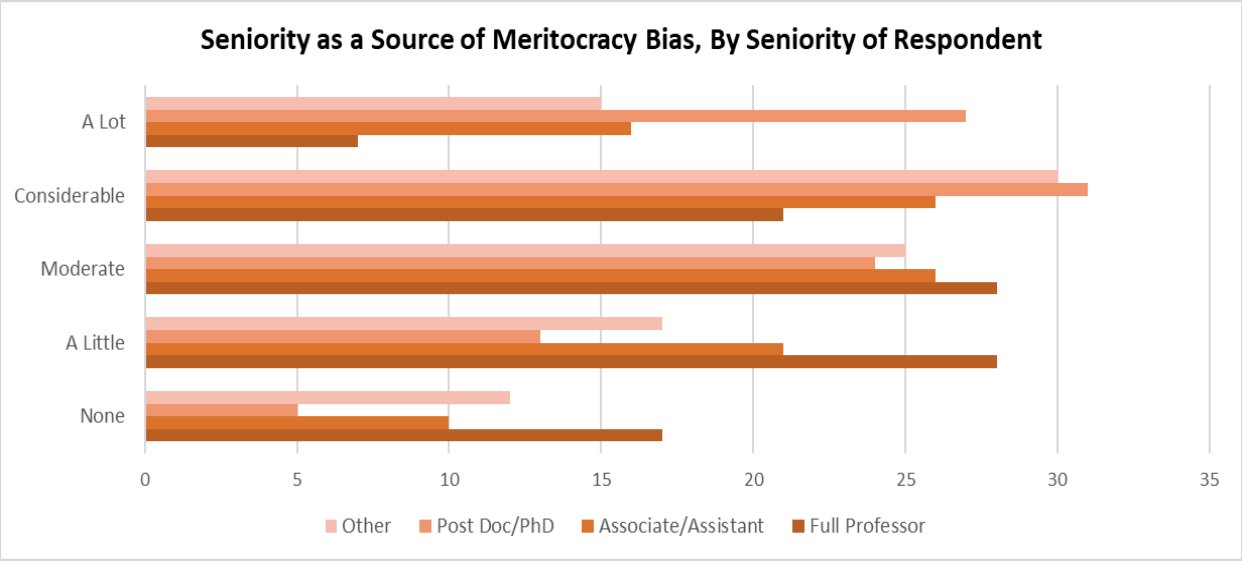
Characteristics of studies are viewed to be relatively less important sources of bias compared to those of professional networks. No single study characteristic is viewed by more than 20% of respondents to matter “a lot” for publication outcomes—this includes the country/region studied, methodological approach, and whether the study challenges findings from the work of coeditors/reviewers. Still, the finding that roughly 40% of respondents view study characteristics as mattering “a lot” or “considerably” does not reflect well on the perceived meritocracy of the publication process.

On many author characteristics, the majority of respondents view characteristics of the authors to either not matter or matter “a little” for outcomes. This includes the gender, race, political views, and country of authors. This might be a positive for the profession in a world of largely non-blind refereeing. An important caveat is that members of majority groups may not perceive bias against minority group members because they don’t experience it themselves. Hence, the relatively low levels of perceived bias by author characteristics may be substantially higher among authors belonging to minority groups than the levels reported in the figure below.

There is a longstanding literature on bias in publishing in STEM and economics. Ferber (1988) finds gender homophily in citations, causing women to be disadvantaged in male-dominated fields. Koffi (2023) identifies articles that were not cited but should have been and finds that these were disproportionately authored by women. In contrast, Card, DellaVigna, Funk, and Iriberri (2020) find women had higher citation counts conditional on publication in four leading journals (see also Hengel and Moon, 2023). This suggests that women are held to a higher standard. This view is reinforced by Hengel (2022) who finds that, relative to men, women’s writing improves more during the review process, and their published articles are better written. However, CDFI2020 do not find a discrepancy in citations based on gender concordance. Differences in outcomes between concordant and discordant pairs are frequently used as indicators of discrimination.

Citations have also been used to detect discrimination based on race. Mason, Myers, and Simms (2022) examine articles on race and crime in top journals and find that black authors are cited less than white authors. Koffi, Pongou, and Wantchekon (2024b) perform a related analysis over a longer period and more journals and find a similar pattern that is noticeably affected by racial homophily in citations.

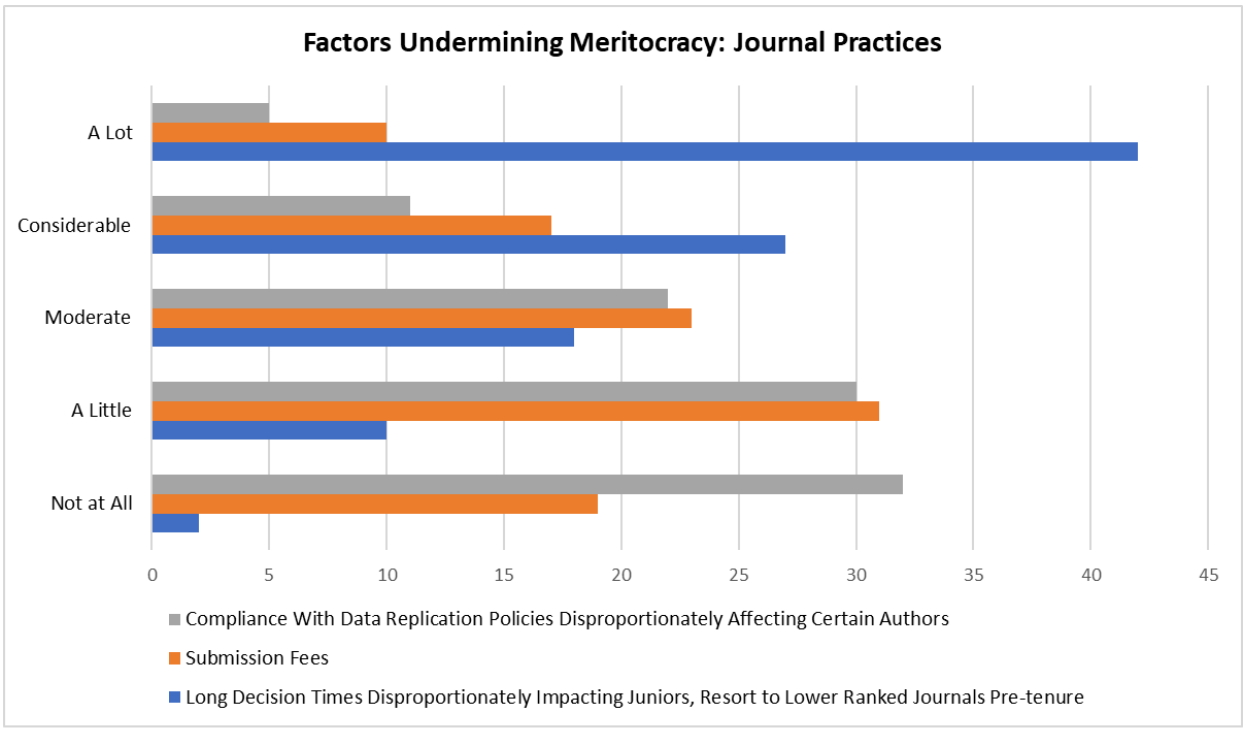
The one exception to author characteristics being perceived as mattering relatively little is the perceived role of authors' seniority: 14% of respondents view this as mattering "a lot," more than twice that for most other author characteristics (gender, race/ethnicity, political views, sexual orientation and disability). These perceptions fit with the evidence shown in Card and DellaVigna (2020). Consistent with Greenspon and Rodrik (2021) and Amarante, Burger, Chelwa, Cockburn, Kassouf, McKay, and Zurbrigg (2022), 9% believe that the country/region of the author matters a lot.

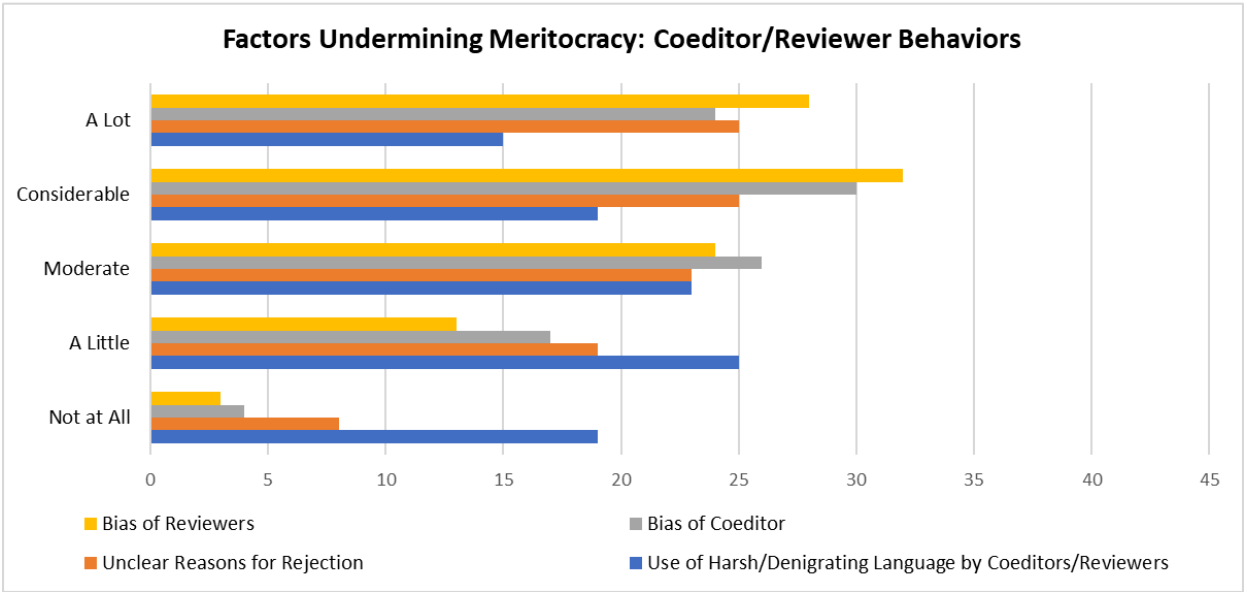


To dig into this a little more, we examine how views on the influence of authors' seniority depend on the seniority of the respondent: there is a clear gradient in responses across full professors, associate/assistant professors and post docs/PhD on this (so ignoring those of "other seniority"). While 7% of full professors report that seniority matters "a lot", this rises to 16% for associate/assistant professors and 27% for post docs/PhD students. There is also a gradient when it comes to the author's country of residence. While 5% of US-based authors think the country of residence matters "a lot," this figure rises to 9% for respondents based in Europe and to 19% for respondents based in the rest of the world. A similar gradient is present for views on whether there is bias based on the country or region studied in the paper. We cannot repeat this analysis for other author characteristics such as gender and race/ethnicity because we did not elicit these characteristics in order to keep the survey short and to safeguard respondent confidentiality. However, it seems quite possible that similar gradients would appear along other author characteristics.

3.5. Factors Undermining Meritocracy

We next consider views on factors undermining meritocratic outcomes in the publication process. We split these into two types: (i) those more loosely related to journal practices and (ii) those related to the behavior of coeditors and reviewers. Views on both are shown in the two figures below, and we set the x-axis to be on the same scale for both to facilitate comparisons across these sets of factors.





On journal practices, the most important factor reported to be undermining meritocracy was “long decision times disproportionately impacting junior scholars, who may resort to lower-ranked journals to secure publication before tenure deadlines." 42% of respondents view this factor as undermining meritocratic outcomes by “a lot,” and this figure rises for more junior scholars: 31% for full professors, 46% for associate/assistant professors, and 57% for post docs/PhD students (it is 43% for others).

Views on submission fees are more mixed, with the modal view being that submission fees undermine meritocratic outcomes “a little.” The least important factor is compliance with replication policies, which over 60% of respondents view as undermining meritocracy “a little” or “not at all.” We also see no gradient with seniority on this response (which might otherwise have reflected differences in resources that make it easier to comply for more senior researchers): 33% of full professors view compliance as mattering “not at all,” and the corresponding figure is 32% for associate/assistant professors, 29% for post docs/PhD students, and 35% for others.

On factors related to the behavior of coeditors/reviewers, the bias of reviewers is considered slightly more problematic than the bias of coeditors (28% of respondents report the former as hindering meritocratic outcomes “a lot” while this drops slightly to 24% for the coeditor bias). However, less than 5% of respondents report coeditor or review biases as having “no” impact on publication outcomes. Unclear reasons for rejection are also cited as being a factor undermining meritocracy—25% view this as mattering “a lot,” and 25% report this to be a “considerable” factor. Finally, the use of harsh/denigrating language is not viewed as overly problematic relative to the other practices. Indeed, 44% of respondents report this to matter “a little” or “not at all.”

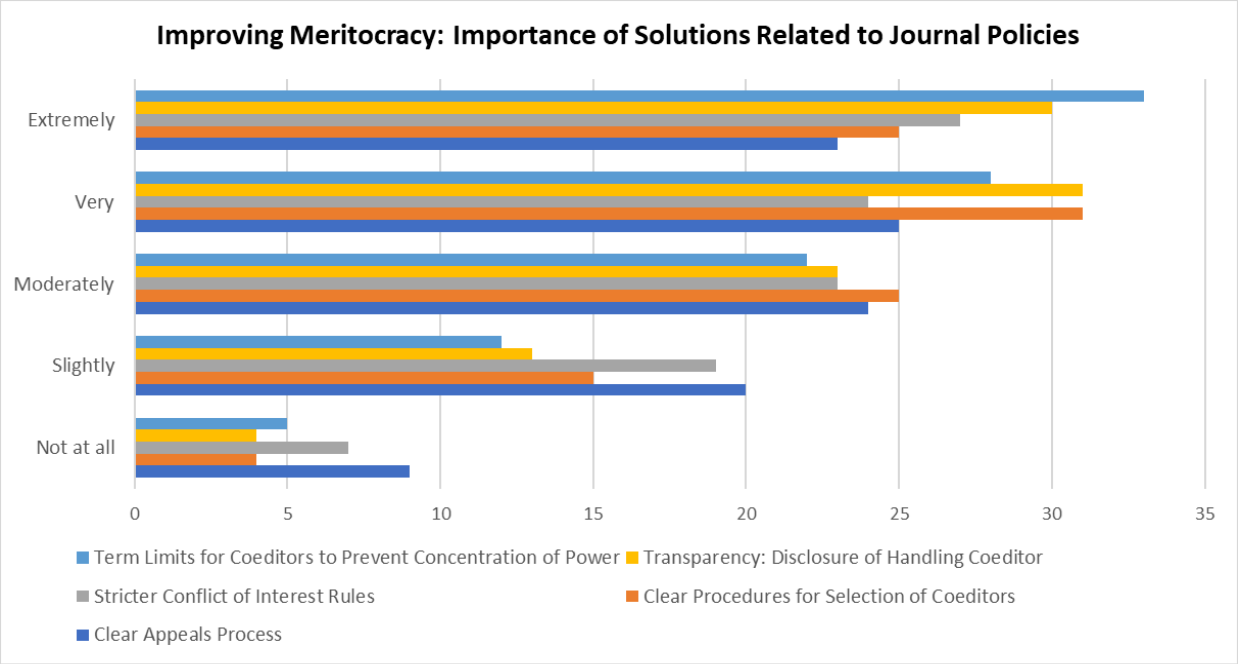
3.6. Views on Policies to Improve Meritocracy

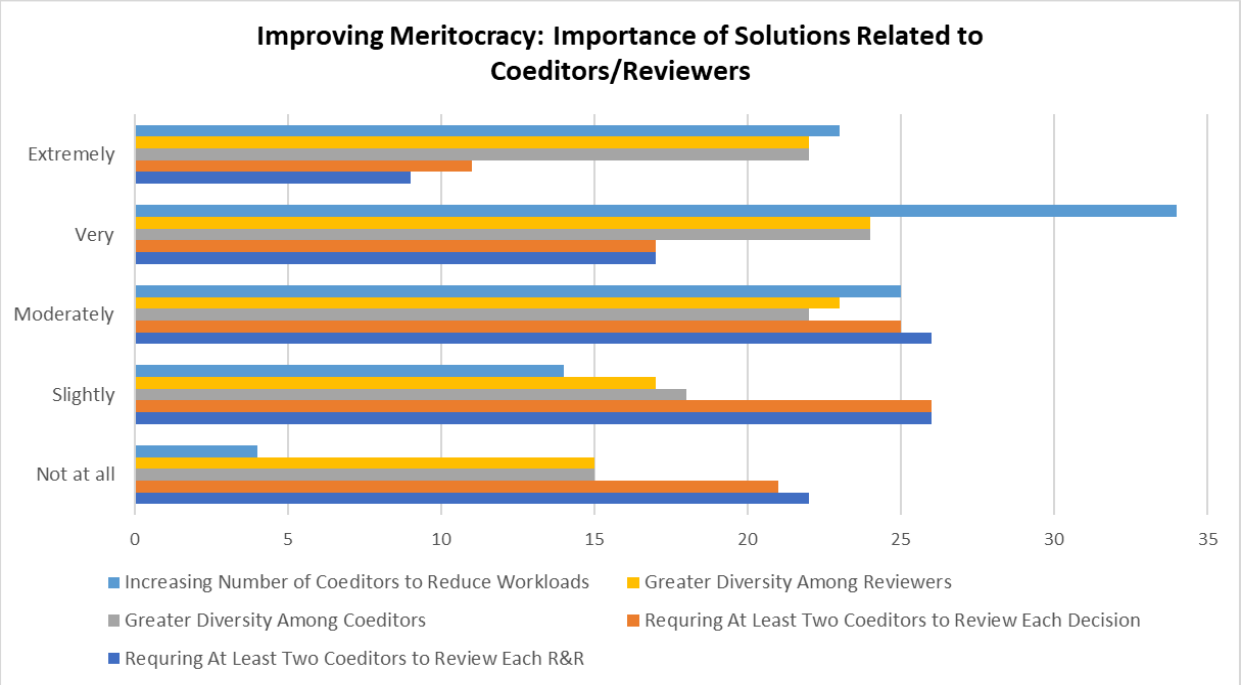
The next set of views elicited in the survey related to the importance of specific policies to improve meritocratic outcomes. As with factors undermining meritocratic outcomes, we split these into two types: (i) those related to journal practices and (ii) those related to the behavior of coeditors and reviewers. Views on the importance of both are shown in the two figures below, and we again set the x-axis to be on the same scale for both to facilitate comparisons of views across the sets of proposed solutions.

On solutions related to journal policies, the solution most viewed by respondents as being “extremely” important is term limits for coeditors to prevent the concentration of power (33%). This is followed by transparency in terms of disclosure of handling coeditors (30%), stricter conflict-of-interest rules (27%), clear procedures for the selection of coeditors by journals (25%), and clear appeals processes (23%). However, across each of these solutions, a wide range of views are expressed, with between 17% and 29% of respondents viewing any given solution as being of “slight” or no importance.

On the importance of solutions related to coeditors/reviewers, we again see a wide range of views for each. Increasing the number of coeditors to reduce workloads perhaps receives the strongest support: 23% of respondents view this as “extremely” important, and 34% view this as

"very" important. Policies to improve the diversity of coeditors and reviewers in terms of background and institution also receive considerable support. Policies such as requiring two coeditors to review decisions or review revise-and-resubmit decisions are viewed as being less important solutions to improving meritocracy in the publication process.





3.7. Perceptions of Factors Undermining Efficiency

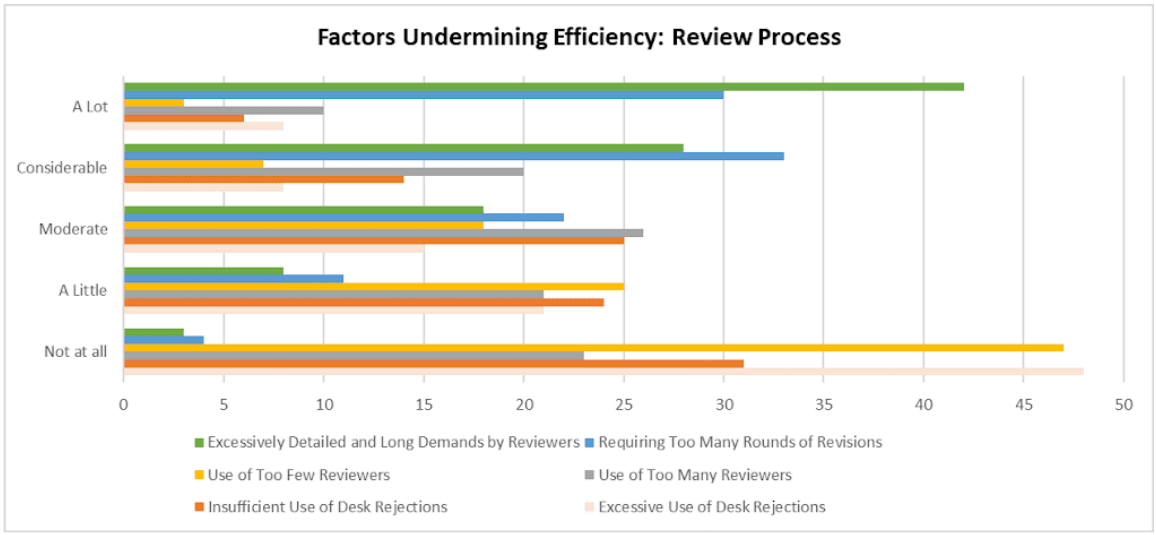
The remaining parts of this section of the survey focused on the efficiency of the publication process. We first report respondent views on factors reducing efficiency. To ease exposition, we group the factors into three types: (i) those related to the review process, (ii) those related to other coeditor practices, and (iii) those more closely related to journal practices. We set the x-axis on each figure to be on the same scale to facilitate comparisons of views across the sets of factors.

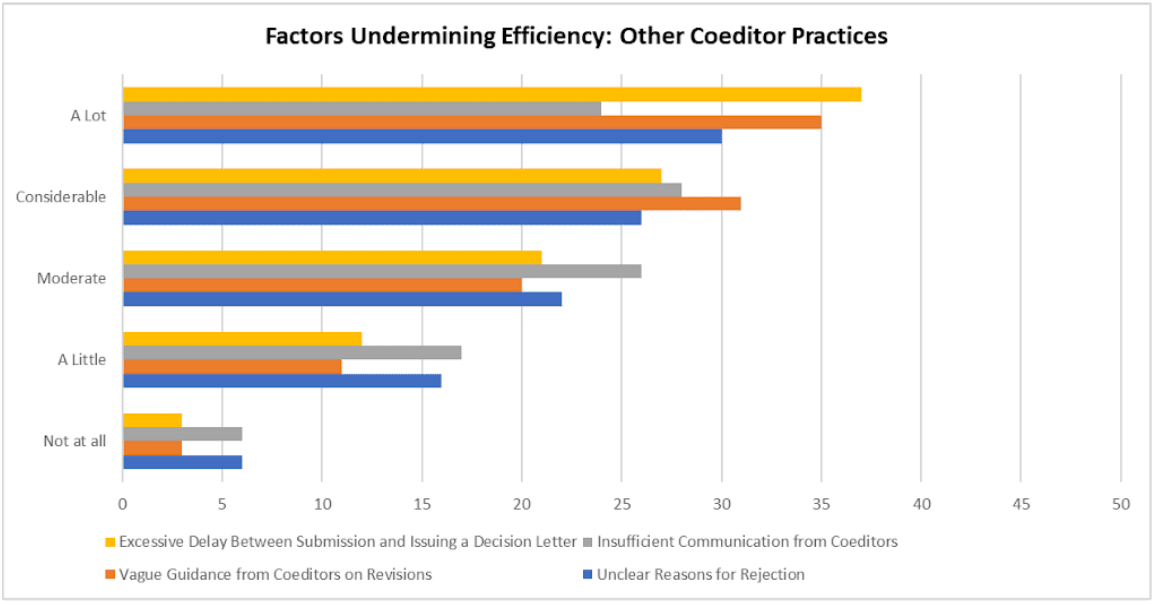
On the review process, two factors stand out as being viewed to matter “a lot” for efficiency: excessively detailed and long demands by reviewers (42%) and requiring too many rounds of revision (30%). Other factors—such as the excessive use of desk rejections, the insufficient use of desk rejections, the use of too many reviewers, and the use of too few reviewers—are generally viewed as less important for undermining efficiency. Notably, 23% of respondents view the use of too many reviewers as “not at all” important for efficiency.

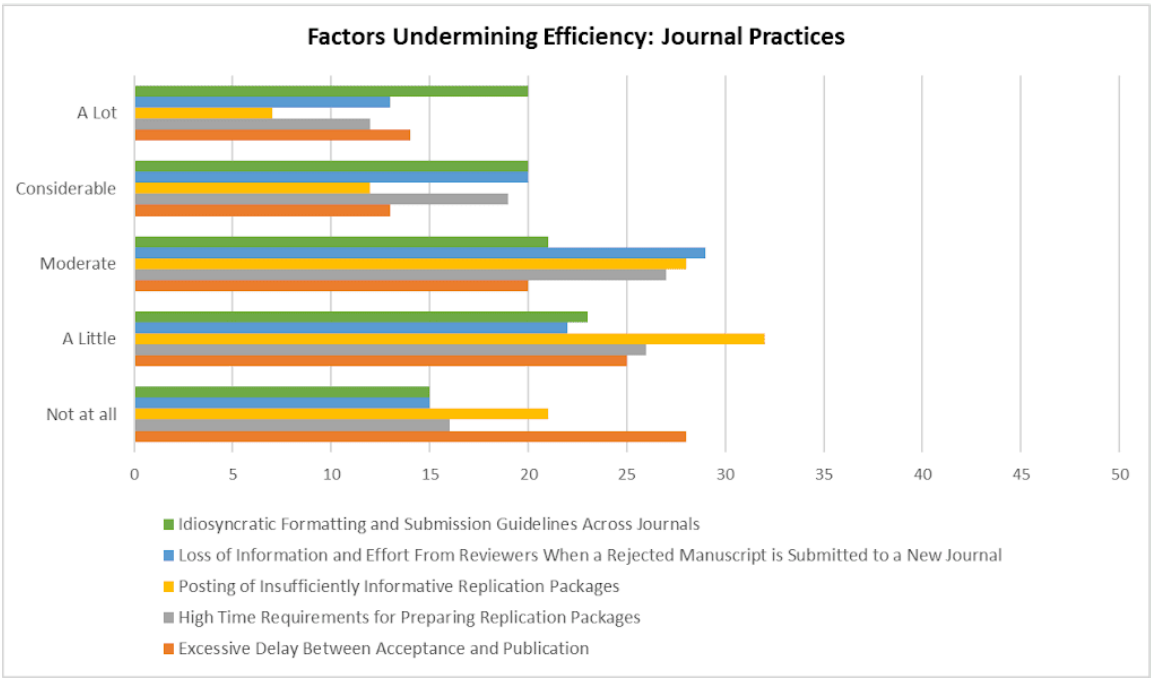
All other coeditor practices are viewed as mattering “a lot” for efficiency: excessive delays between submissions and the issuance of decision letters (37%), vague guidance from coeditors on revisions (35%), unclear reasons for rejection (30%), and insufficient communication from coeditors (24%) are all key concerns. Indeed, for all four practices, 6% or fewer of respondents view each as being “not at all” important for the efficiency of the process.

Finally, on practices more closely associated with journals (rather than individual coeditors), the practice most frequently considered as mattering “a lot” is idiosyncratic formatting and submission guidelines across journals (20%). On paper transfers across journals, views are perhaps the most spread out: 13% of respondents state that this matters “a lot,” and 15% state that it matters “not at all.”

Publication lags (from acceptance to publication) are not considered important for the viewed efficiency of the process: indeed, 28% of respondents report this as mattering “not at all.” In line with the earlier responses that showed few concerns with complying with replication processes, we also note that only 7% of respondents report that the posting of insufficiently informative replication package materials mattered “a lot.”


Factors Undermining Efficiency: Review Process
A Lot
Considerable
Moderate
A Little
Not at all
0
5
10
15
20
25
30
35
40
45
50
Excessively Detailed and Long Demands by Reviewers
Requiring Too Many Rounds of Revisions
Use of Too Few Reviewers
Use of Too Many Reviewers
Insufficient Use of Desk Rejections
Excessive Use of Desk Rejections


Factors Undermining Efficiency: Other Coeditor Practices
A Lot
Considerable
Moderate
A Little
Not at all
0
5
10
15
20
25
30
35
40
45
50
Excessive Delay Between Submission and Issuing a Decision Letter
Insufficient Communication from Coeditors
Vague Guidance from Coeditors on Revisions
Unclear Reasons for Rejection


Factors Undermining Efficiency: Journal Practices
A Lot
Considerable
Moderate
A Little
Not at all
0
5
10
15
20
25
30
35
40
45
50
Idiosyncratic Formatting and Submission Guidelines Across Journals
Loss of Information and Effort From Reviewers When a Rejected Manuscript is Submitted to a New Journal
Posting of Insufficiently Informative Replication Packages
High Time Requirements for Preparing Replication Packages
Excessive Delay Between Acceptance and Publication

3.8. Views on Policies to Improve Efficiency

Finally, we examine support for potential solutions for improving the efficiency of the process, grouping these into three types of solutions: (i) those related to the review process, (ii) those related to journal policies, and (iii) other solutions. In general, for all three types of proposals, there is a wide range of support expressed so that no one solution is overwhelmingly supported or opposed by respondents.

Among the proposed solutions related to the review process itself, respondents expressed the strongest support for establishing strict cutoff times for editorial decisions—25% of respondents view this as being “extremely” important (but, at the same time, 24% of respondents viewed this as being “slightly” or “not at all” important). The solutions of increasing communication between coeditors and authors beyond decision letters, limiting the length of response to reviewers, and setting limits on the number of substantive points to be raised by reviewers, each had around 16% of respondents viewing them as “extremely” important (but a greater share always viewed each solution as being “slightly” or “not at all” important).

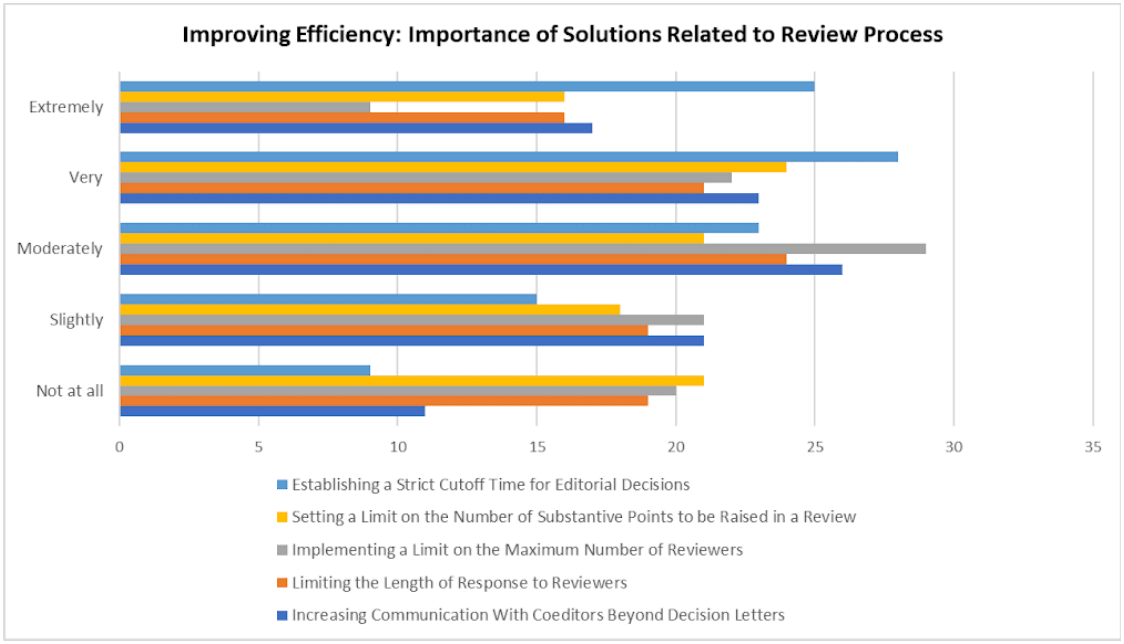
In line with the earlier finding that the number of reviewers is not seen as a major source of inefficiency, limiting the maximum number of reviewers is the proposal that has the smallest share of respondents viewing it as “extremely” important (9%).

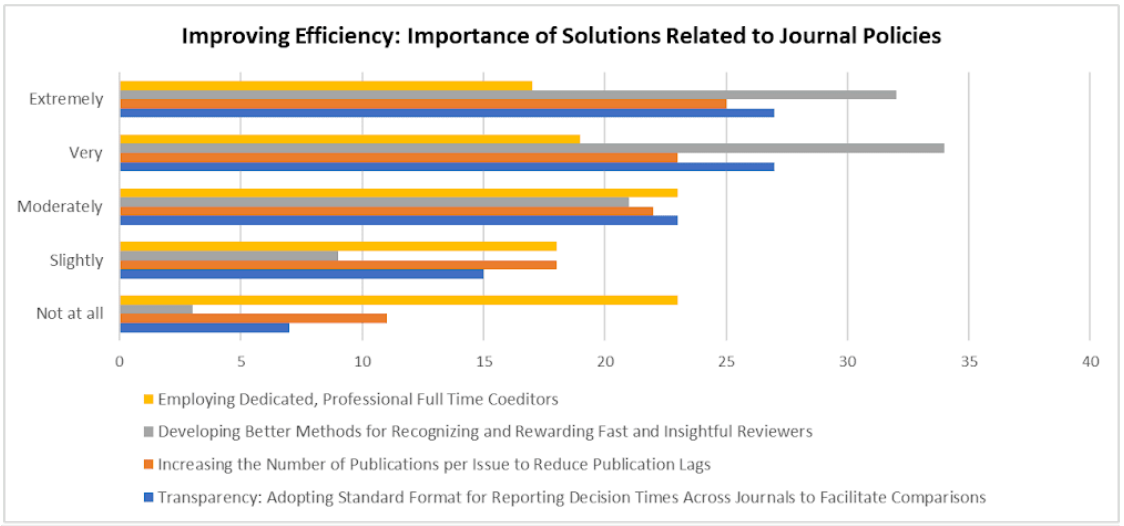
On proposed solutions related to journal policies, respondents expressed strongest support for the proposal for developing better methods for recognizing and rewarding fast and insightful reviewers, which 32% of respondents viewed as being “extremely” important (and only 12% of respondents viewed as being “slightly” or “not at all” important). Two-thirds (66%) of respondents viewed this solution to be “very” or “extremely” important to improving efficiency.

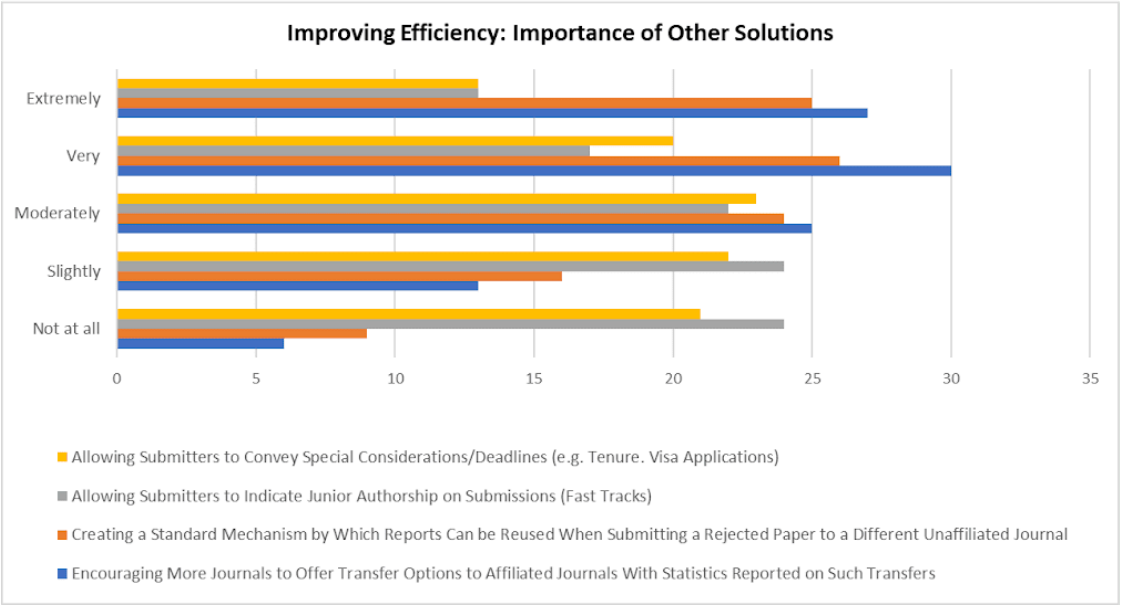
The majority of respondents (54%) viewed increasing transparency across journals—by adopting standard formats for reporting decision times—to be “very” or “extremely” important to improving efficiency. The proposal of increasing the number of publications per issue also received strong support: 48% of respondents viewed this solution to be “very” or “extremely” important to improving efficiency.

Employing dedicated full-time professional coeditors had the lowest share of respondents viewing it to be “extremely” important for improving efficiency (17%), while over 40% viewed this solution as “not at all” or “slightly” important.

Finally, on other solutions, 27% of respondents view the idea of encouraging journals to offer transfer options to affiliated journals as being “extremely” important, and 25% view the solution of creating a standard mechanism by which reports can be reused when submitting a rejected paper to a different unaffiliated journal as being “‘extremely”’ important. Only 6% and 9% of respondents view these two proposed solutions, respectively, as being “not at all” important.


Improving Efficiency: Importance of Solutions Related to Review Process
Extremely
Very
Moderately
Slightly
Not at all
0
5
10
15
20
25
30
35
Establishing a Strict Cutoff Time for Editorial Decisions
Setting a Limit on the Number of Substantive Points to be Raised in a Review
Implementing a Limit on the Maximum Number of Reviewers
Limiting the Length of Response to Reviewers
Increasing Communication With Coeditors Beyond Decision Letters


Improving Efficiency: Importance of Solutions Related to Journal Policies
Extremely
Very
Moderately
Slightly
Not at all
0
5
10
15
20
25
30
35
40
Employing Dedicated, Professional Full Time Coeditors
Developing Better Methods for Recognizing and Rewarding Fast and Insightful Reviewers
Increasing the Number of Publications per Issue to Reduce Publication Lags
Transparency: Adopting Standard Format for Reporting Decision Times Across Journals to Facilitate Comparisons


Improving Efficiency: Importance of Other Solutions
Extremely
Very
Moderately
Slightly
Not at all
0
5
10
15
20
25
30
35
Allowing Submitters to Convey Special Considerations/Deadlines (e.g. Tenure. Visa Applications)
Allowing Submitters to Indicate Junior Authorship on Submissions (Fast Tracks)
Creating a Standard Mechanism by Which Reports Can be Reused When Submitting a Rejected Paper to a Different Unaffiliated Journal
Encouraging More Journals to Offer Transfer Options to Affiliated Journals With Statistics Reported on Such Transfers

Less strong support is expressed for the other two proposals: 13% of respondents view allowing submitters to convey special considerations/deadlines as extremely important (while 21% view this to be “not at all” important), and 13% of respondents view allowing submitters to indicate junior authorship to facilitate fast tracking as extremely important (while 24% view this to be “not at all” important).

3.9. Responses to the Final General Open-Ended Question

The committee read all 550 responses to the open-ended question asked at the very end of the survey. Many respondents put a lot of thought and effort into their answers, and the committee truly appreciates this. The responses were very illuminating for two reasons. First, they add depth and context to the views expressed in the closed-ended questions. Second, they express views and offer suggestions on topics not covered in the survey. Some of these suggestions have shaped the recommendations of this committee; other suggestions fall outside the mandate of the committee because they touch on margins along which journals compete. The committee reports these suggestions in Appendix C so that journals can consider them.

Many of the responses focus on the role of networks, clubbiness, elitism, in-crowds, and repeat interactions in the profession. They also frequently mention an ubercompetitive system that gives too much credit to publishing in a few very selective general-interest journals. Comments explain that the intensity of competition is exacerbated by the number of publications in those journals not having risen as quickly as the number of academic economists (see Card and DellaVigna, 2013), and that the competition for these scarce slots has then moved to other margins, such as writing increasingly extensive papers and ensuring papers are widely presented at conferences and seminars. Many comments mention the hierarchical nature of the economics profession and how this is reinforced through networks and conferences that are not open to all. Because most editors and many reviewers are part of these networks, many respondents are concerned that being part of such networks influences editorial decisions, consistent with evidence in Colussi (2018) and CFL2024. Moreover, when influential positions are concentrated, and people hold them for extended periods of time, the number of opportunities for repeat interaction increases, and so does the risk for the reciprocal trading of favors. Many of these networks operate in the United States and, to a somewhat lesser extent, in Europe. A number of respondents express the concern that this biases the publication process against authors residing outside the United States and Europe and against papers that focus on countries outside the United States and Europe.

Respondents suggest many possible measures that could contribute to mitigating the biases associated with networks. Many call for wider institutional, field, and geographical diversity among editors, coeditors, and associate editors, including a better representation of “non-elite” universities and less “highly ranked” departments. More institutional and geographical diversity among reviewers is also mentioned. Respondents call for disallowing individuals to hold multiple editorial positions or to combine editorial positions with other influential positions, such as being

an organizer or director of prominent conferences or professional networks. Respondents also mention the possibility of having disclosure of whether coeditors or reviewers had seen the paper being presented or had conversations about it with authors, and journals presenting aggregate statistics on this.

In addition, many respondents call for the return to double-blind reviewing. Respondents mention that, even if reviewers can, in many cases, look up the paper, having the identities of the authors masked allows the reviewer to remain unaware of these identities and sends a signal to the reviewer that the journal does not want the reviewer to use information about the authors in their assessment. When the committee asked an AI model to distill the top-10 concrete suggestions from the open-ended responses, the top three suggestions all related to double-blind reviewing ("double-blind review," "anonymous submissions," and "omission of institution of the authors"). However, according to Figure 15 in CDEGT2022, support for double-blinded reviewing was rather mixed in their survey.

A second important theme in the comments concerns the high and increasing difficulty of getting published. Respondents mention the growing length of papers and the ballooning supplemental appendices, either in response to reviewers' requests or to preempt rejection for not being sufficiently thorough. They also mention referees taking on the role of shadow coauthor by asking for extensive revisions, often reflecting idiosyncratic tastes, and that editors do not sufficiently protect authors against such referees. However, some respondents point to authors submitting poorly prepared papers in expectation of substantial guidance or to authors doing so because they expect to lose control of their manuscript during the review process anyway ("so why polish it?"). Further adding to the arduousness of publishing is the practice of some journals of asking for multiple rounds of revisions. Respondents express concern about how long decision lags compound the difficulty of getting published and are especially costly for junior authors facing a tenure clock.

Related to the second theme, respondents offer a range of suggestions for improvements, including various rules on limiting the length of submissions and referee reports and on how the content of referee reports should be structured (between essential points and optional suggestions). They also suggest that editors should be taking a more active role in pushing back against overly onerous or idiosyncratic reviewer comments. Many reviewers also mention that increased publication rates or more journal space in good journals would be helpful because it would limit competition along the dimension of making manuscripts longer and more extensive.

A third theme in the comments centers around unevenness in the quality of referee reports, reviewer training, and reviewer accountability. The survey asked about problems, so it is natural that respondents emphasize negative experiences with reviewers, though quite a few also expressed appreciation for them. The committee's own (perhaps selective) experience with reviewers is that the overwhelming majority of them take their tasks seriously, are professional, and offer constructive feedback. Still, low-quality or unprofessional reports undermine the meritocracy and efficiency of the publication process, and respondents offer various suggestions

for improvement on this front. Some respondents suggest providing reviewers with better incentives, either monetary ones or through reputational mechanisms, such as a rating system for reviewers. Other suggestions include offering clear instructions and training materials to reviewers, disregarding poor referee reports, and providing a mechanism by which authors can flag unfair or off-base referee reports.

Respondents offered many additional observations and suggestions on a variety of topics. Some respondents express the concern that the publication process shows too much deference to received wisdom and is biased against new approaches and truly innovative papers. Respondents worry about the role of the uneven distribution of knowledge on "how the game should be played," hidden curricula, and unwritten rules. They suggest that journals can limit this by being explicit about their rules and procedures and by not publicizing rules that they do not actually enforce (which can lead to an unfair situation in which those in the know ignore the rules, whereas others take costly action to comply with them). Respondents suggest that journals should have clear standards for the use of repeat reviewers and that a particular reviewer should not be able to effectively veto a paper at multiple journals. Finally, respondents call for journals to have a clear and fair appeals process.

Consistent with dissatisfaction with the professional climate reported in the AEA climate surveys of 2018 and 2023 and in the EEA climate survey of 2023, many open-ended responses displayed a depth of hurt, disillusionment, and cynicism about the economics profession. These responses appear genuine, and many of the respondents describe experiences with the publication process that nobody should ever have to experience. These responses underscore the need for reforms. While many respondents appear to appreciate the initiative of the four associations to form a committee to propose improvements to the publication process, there was also a fair bit of skepticism. Some expressed the concern that better connected and better resourced economists will be in a better position to respond to changes proposed by the committee, which would further tilt the playing field. The committee acknowledges this concern and tried to take it into account in its recommendations. Others went a step further and suggested the express purpose of the committee is to solidify inequities or to hide fundamental problems, e.g., "You guys don't care. You guys are doing this to pretend you are not a part of the problem." and "We all don't expect much from this (many of my friends are not hopeful that any change happens out of this), since the people who benefit from the system are in charge." A similar view was expressed "Over the years, I lost any faith in the credibility of our publication process. It is governed by small cliques whose only interest seems to be protection of monopoly power and the associated rents." However, this respondent didn't completely rule out the possibility that the effort of the committee is genuine: "I am curious to see if this survey will be more than the umpteenth exercise in virtue signaling. Based on experience, I am not optimistic. I hope I will be surprised."

Some respondents shared particularly poignant experiences. One respondent describes how his coauthor passed away the previous night from cancer. Prior to tenure, this coauthor had his job-market paper rejected in the fourth round at one of the handful of most selective general-interest journals. When the respondent tried to congratulate his coauthor on a different

paper he did get accepted in that journal, he was too ill to respond. This respondent writes that he completed the survey in memory of his coauthor, thinking about all the unnecessary stress junior economists endured over the years.

The committee appreciated how many respondents inserted clever analogies, puns, and some humor into the serious points they raised. Examples include: “Please forgive the grumbling of a geriatric emeritus … I recognize that the last man who could fix the problem was crucified a couple of millennia ago” and, in reference to papers becoming unreadable due to frequent reference to online appendices, “The alternative option would be an appendectomy.” One respondent compared how journals were run to the UN Security Council: “This council has permanent members from the world's most powerful countries and it has voting members from less important countries who rotate out. Our journals are similarly controlled by a “permanent member” group from top departments. Opening editorial boards to a rotating group from smaller, less well-known departments would go a long way toward reducing the good ol’ frat boy feel of our current system.”

4. Recommendations on Policies with Returns to Harmonization

4.1. Reviewer Transfer Mechanism

One of the sources of inefficiency in the publication process is the duplication of reviewer effort for papers that are narrowly rejected at one journal and then submitted to another journal. The profession has made some progress in reducing this duplication of effort by setting up mechanisms that allow authors to submit prior reviews or request that prior reviews be transferred.

However, except when the journals are related (e.g., transfers from the *AER* or *AER: Insights* to the AEJs, from the *JPE* to *JPE Macro* or *JPE Micro*, and from *Econometrica* to *Theoretical Economics* or *Quantitative Economics*), journals that accept authors’ requests to use prior reviews must approach the prior editor on an ad hoc basis to request that they contact the prior reviewers for permission to reveal their identities to another journal. Alternatively, the editor of the subsequent journal must proceed without that knowledge and in the hope that the reviews and editor’s letter are unaltered.

The survey indicated support for policies that facilitate the transfer of referee reports across journals. Siemroth (2023) recommends making such transfers easier (his recommendation 5) and CDEGT2022 discuss it as an option (their section 3.2.4.4).

We recommend that, as part of the review process, all journals ask reviewers if the journal can share the review and the reviewer’s identity with other legitimate journals acting on an author’s request that they use the review. When permission is granted, it should be straightforward for an editor to obtain this information from a journal that has previously reviewed the submission.

We also recommend that journals establish policies regarding the circumstances under which they, upon an author's request, will accept a transfer of reviews and reviewer identities from a previous journal that had reviewed the submission. Such policies may, for example, limit the set of journals from which a journal will accept transfers or require the correspondence from all previous submissions of the paper. The policy should also clarify how transfers are handled: does the journal invite new reviews never/sometimes/always? Does it never/sometimes/always send the paper to the original reviewers? Such policies are part of each journal's competitive strategy and fall outside this committee's purview.

The transfer process will be most effective if reviewers habitually suggest journals at which they would recommend a "revise and resubmit" and if editors suggest submission to a specific journal or journals if and only if they believe a revised version of the paper they are rejecting would have a high probability of acceptance at the journal they are recommending. Hence, we recommend that journals ask their reviewers and editors to make such suggestions. However, authors should be aware that if they use the transfer process, their submission is likely to be returned to one or more of the original reviewers and should be revised accordingly.

We recommend that journals establish safeguards to ensure that, if a reviewer grants permission for their identity to be shared, it is shared only with a current coeditor of a legitimate journal handling the paper that the reviewer reviewed. For example, a journal could promise to only send such information to the email address of a current coeditor or managing editor listed on the official journal webpage of a specified list of journals.

The current frequency of transfers between journals is too low for us to recommend a centralized transfer system. Nevertheless, we hope that if the profession adopts these recommendations, transfers will become sufficiently common to justify one.

Recommendation A1: Reviewer transfer mechanism
We recommend that journals (i) establish policies regarding the circumstances under which they, upon an author's request, will accept a transfer of reviews and reviewer identities from a journal that had previously reviewed the submission; (ii) ask reviewers, as part of the review process, if they may share the review and the reviewer's identity with other legitimate journals requesting that information; and (iii) establish safeguards that ensure that a reviewer's identity is shared, if the reviewer granted permission, only with a current coeditor or managing editor of a legitimate journal handling the paper that the reviewer reviewed.

4.2 Structure of Referee Reports

Many economics reviewers are very generous with their time and genuinely try to help strengthen the papers they review. This benefits the field by making published economics papers more thorough, with findings that generally withstand scrutiny. However, in practice, reviewers often mix essential comments with more idiosyncratic comments and suggestions that improve the paper only marginally but require considerable time to implement. This concern, sometimes referred to as reviewers acting as shadow coauthors, was prominent in the survey.

Fearing rejection after revision, authors are understandably reluctant to ignore reviewer suggestions. Consequently, revision is frequently arduous and time-consuming. Author response letters to the reviewers in excess of 100 pages are no longer uncommon.

The committee believes that we can increase the value of reviewers' efforts while limiting the arduousness of the revision process if journals ask reviewers to follow Berk, Harvey, and Hirshleifer's (2017) recommendation to separate "essential" from "optional" comments. Essential comments are those the reviewer thinks the authors must address for the paper to become publishable in the current or, perhaps, a subsequent journal. Any suggestions the reviewer believes would strengthen the paper but are not essential should be labeled optional. Authors should be expected to implement optional comments only if they believe the improvement will likely outweigh the implementation cost. Consequently, authors should not be expected to respond to optional comments in their response letter, and journals should not reject a revision that fails to address them.

Journals should provide guidance on the extent and nature of essential comments consistent with recommending revision at that journal. This may consist of a normative maximum number; most committee members believe three will generally suffice, but more may be acceptable if each is straightforward. Journals should also clarify that a report with zero essential comments is perfectly acceptable; some papers may already have passed the journal's standard for publication, even if further improvements are possible.

Coeditors should give further guidance to authors about which of the reviewers' comments must be addressed in the revision and which may be treated as optional.

Having a norm in economics that reviewers separate essential and optional comments will reduce the burden on reviewers who will not have to familiarize themselves with journal-specific instructions. A common report structure will also facilitate the sharing of reports across journals.

Recommendation A2: "Essential" vs. "optional" reviewer comments
We recommend that journals ask reviewers to separate their comments into essential and optional comments, that they tell authors that their response letters need not address optional comments, and that they tell reviewers that they should not recommend rejection of a revision for failure to implement optional comments. We further recommend that journals should provide guidance on the extent and nature of essential comments consistent with recommending revision at that journal.

4.3. Policies on the Use of Artificial Intelligence

Survey respondents are generally supportive of rules regarding the use of AI in the editorial process. They do not say what the rules should be, and in fact, acknowledge that they do not have the knowledge to set the rules and that many rules may not be enforceable. That may be true for the members of the committee as well! In addition, this is an area that will keep changing fast. There is a lot of tension between what we can set as norms and what we can

enforce. Even spell checkers now advertise themselves as AI-driven. It seems clear we do not want to prohibit or discourage reviewers from using grammar-improving software. We recommend that reviewers refrain from uploading the papers they are reviewing into AI engines. One reason is that the content of the paper, which is the author's intellectual property, may become available as training data in AI systems, which may result in use by others without appropriate credit to the authors. Second, and more fundamentally, journals are interested in the views of the reviewers, not in the "views" of LLM's regarding the manuscript. While there may be a role for LLMs in detecting plagiarism and other issues, these are not the central questions that editors ask reviewers to consider when evaluating papers.

Recommendation A3: Not uploading manuscripts to AI
We recommend that journals communicate to reviewers that they should not upload any part of a manuscript they are reviewing to AI, whether for having AI generate a report or for other purposes.

Recommendation A4: Clarifying AI use
We recommend that journals ask researchers to acknowledge in their submissions the use of AI beyond spell checking and grammar checking (e.g., drafting and rewriting sections) and to acknowledge that they remain responsible for all content, whether based on AI or not.

4.4 Author Order

We focused on three options concerning authorship order. The first is alphabetical order, the current norm in economics, with occasional departures in situations where the person who is the primary author is listed first. Alphabetical ordering is a set rule that reduces potential conflict within a team but advantages individuals with last names that are early in the alphabet (Einav and Yariv, 2006; Efthyvoulou, 2008; Huang, 2015; Feenberg, Ganguli, Gaulem and Gruber, 2017; Weber, 2018) and provides no information about contributions. This is not race/ethnicity-neutral since, for example, names at the end of the alphabet are noticeably more common among Chinese than other economists (Li and Yi, 2021).

Alphabetical ordering is highly unusual in academia. It is only standard in economics and mathematics, where the number of authors has traditionally been small, and in high-energy physics, where there can be several hundred authors. Within fields that do not normally use alphabetical ordering, it is more common when there are few authors (Waltman, 2012).

The growth in the proportion of articles with four or more authors may create pressure to change the norm in economics.
We considered two alternatives. The first is random ordering. Like alphabetical ordering, it reduces potential conflict and provides no information about author contributions. However, random ordering has the advantage of being neutral with respect to place in the alphabet. The second alternative is ordering that reflects the authors' contributions to the article. In the sciences, the primary author is typically listed first, the lab director or senior author last, and other contributors in between, sometimes in descending order of contribution. This option has

the notable advantage of rewarding contributions but could lead to conflict within teams and has the potential for abuse by more senior, powerful collaborators. The written comments from respondents reflected the tradeoffs among the three options.

Author order should be considered in combination with whether or not authors are required to state how author order was determined. If some authors choose alphabetical order without stating that they did so and significant numbers of other authors choose random or contribution-based order, confusion about contribution would arise. Currently, if authors wish to depart from the norm to signal unequal contributions, they can only do so if the contribution order violates alphabetical order.

In the survey, we asked respondents to rank three options. The first is the status quo: authors choose the order of their authorship and **may** provide an explanatory footnote. Most authors choose alphabetical ordering and do not provide a statement in the acknowledgments. Authors who choose random ordering may say so or choose to place the ⓡ symbol between authors' names to denote random ordering. Authors are free to choose order based on contribution and to explain. Ray ⓡ Robson (2018) make a strong case for "certified" random ordering, where a symbol such as ⓡ indicates that order is random. The second option in the survey is for authors to choose order but **be required to** provide an explanatory footnote. The third option is that journals randomize order unless the authors explain how the order was determined. This option might shift the norm away from alphabetical ordering, but it would leave authors free to choose.

There are two main takeaways from the survey on this topic. The first is that respondents clearly prefer the two "authors choose" options, one of which was most preferred by 71% of respondents. The option that journals randomize the order is least preferred by about 49% of respondents and most preferred by only 29%.

The second takeaway is that there is a slight preference for allowing authors to choose order but requiring an explanatory footnote. It was the most preferred option of 37%, the middle preference of 48%, and the least preferred of only 15%.

We did not explicitly ask whether respondents would like to see the alphabetical-order norm replaced, but the strong support for letting authors choose and the responses in the open-ended comments suggest that most do not. Some respondents commented that randomization would create confusion and that there would no longer be an easy way to signal strong departures from equal contribution since readers would assume that departures from alphabetical order were random. Other respondents were concerned that there would be a long transition period in which readers would mistakenly misconstrue randomized order as reflecting contribution. In principle, if the randomization symbol is applied consistently in references, these problems would be minimized. However, they could still arise if, in the text, the citation continues to be listed as A and B (2024) or B and A (2024). Hence, we recommend that the ⓡ symbol is also used for in-text citations (e.g., AⓡB or BⓡA, following the order used in the published paper).

Several people raised concerns about the transition from working papers to articles if journals enforced or pushed for randomization. A working paper that circulated as Aardvark, Baboon, and Coyote might appear as Coyote, Aardvark, and Baboon, making it difficult for people to link the working paper and article. One solution would be to allow authors to certify that they had randomized authorship order before submitting, in which case, the order would not be randomized again.

Some respondents noted that the rules in the sciences are based on contribution and that they work well. More, however, argued the opposite: ranking by contribution leads to conflict and discourages cooperation among researchers. Some were concerned about bullying by more senior authors. A few noted that the science model works because a lab head or principal investigator typically serves as the last author and assigns authorship order. They argued that without the lab structure, ordering by contribution is too contentious. At least one respondent noted that economics is moving in the direction of the lab sciences and might adopt the contribution model if this trend continues.

The Committee recommends that journals allow authors to choose authorship order but require that they state *how* that order was determined. This seems low cost and allows readers, some of whom may not be economists, to understand the basis for the author order. We suggest that on the manuscript submission site, authors be required to check a box indicating whether order is alphabetical, random, based on author contribution, or some other basis, which they would explain (e.g., we alternate author order across papers). The final manuscript would include a sentence in the acknowledgments indicating how order was chosen. If necessary, authors can include a longer statement (e.g., Coyote is first author; Aardvark and Baboon are alphabetical). The papers with random author order would be required to include the symbol ⓡ between authors' names, as is currently an option for AEA journals. Authors can use the symbol even if the randomization occurs before submission. Although we have focused on journal policy, for the policy to have the biggest impact, it is also important that authors use ⓡ in working papers when order is random, and that organizations such as NBER include ⓡ in email communications advertising new working papers. Google Scholar and other sources of citation data and providers of citation management software such as Bibtex and Endnote should be encouraged to incorporate ⓡ.

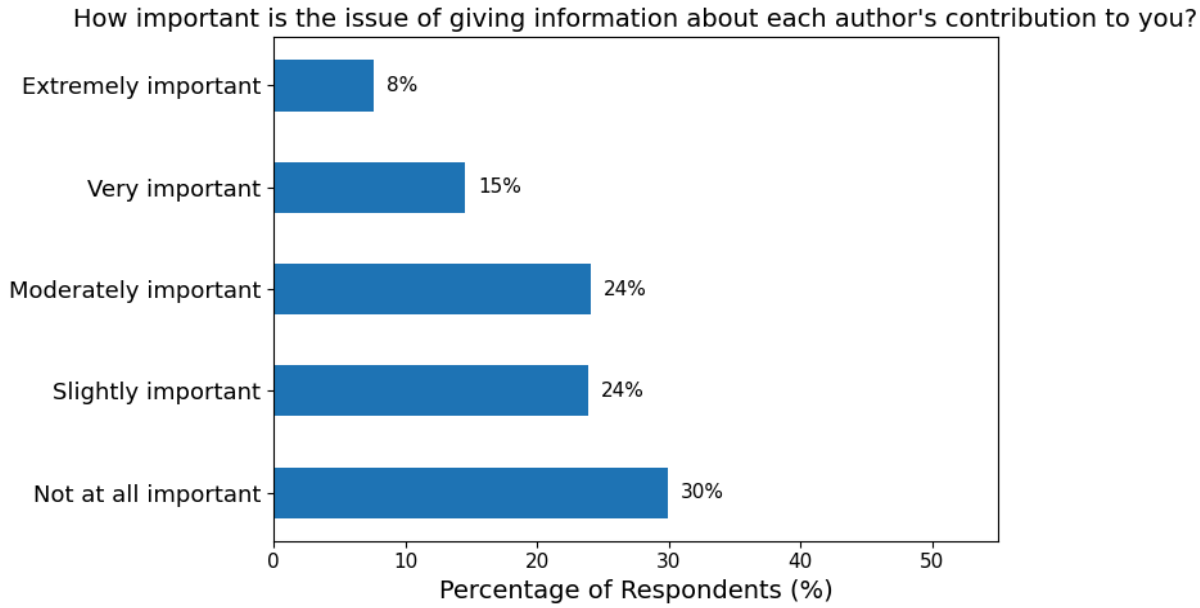
Recommendation A5: Author order
We recommend that journals require authors to state in the opening footnote how author order was determined but leave the choice of the principle governing the ordering (e.g., alphabetical, by contribution, random, or otherwise) to the authors.

Recommendation A6: Use of ⓡ symbol
We recommend that journals should use the symbol ⓡ between authors' names for papers with a random author order, both in the reference section and for in-text citations.

4.5 Contribution Statements

The *status quo* in economics journals is that authors acknowledge helpful comments, research assistance, help in accessing data, and funding. The authors also often state that they are responsible for errors but do not spell out each author's contributions. In recent years, the number of papers with four or even substantially more authors, as well as papers with clearly defined components such as extensive data gathering, has grown, making the issue of differential contributions more salient. There are two separate issues. The first is the overall amount an author has contributed to an article. The second is the specific tasks (e.g., designing the study, collecting the data, and writing the manuscript) that an author has contributed to. Our focus in this section is on the latter. Some major medical and science-oriented journals, including JAMA and PNAS, require that authorship contributions be spelled out. Some journals provide a list of specific contribution areas authors can use. As with authorship order, contribution statements provide useful information about each author's role in the study. However, spelling out contributions may lead to conflict and the potential for abuse.

The Committee surveyed participants about contribution statements, beginning with the question, "How Important is the issue of giving information about each author's contribution to you?" The results are in the figure below. As one can see, the majority say the issue is not at all important (30%) or slightly important (24%).



We then asked respondents to rank four options. The first is the status quo: providing a contribution statement is voluntary. This is the most preferred option of 45% of respondents. The second is that statements are voluntary, but any author who does not wish to take responsibility for some part of the study must include this in a statement. This was ranked first by 13% and second by 40% of respondents. The third option is that authorship contributions must be clearly stated with no requirement to follow a common format. This option was ranked third by 38% of

respondents and second by 28%. The fourth is that authorship contributions must be clearly stated using a common set of contribution categories, although not all would be used for every paper. Twenty-four percent preferred this option, but 57% of respondents ranked it third or fourth.

Digging deeper, we found more support for requiring contribution statements among development economists and energy and environmental economists, and, to a lesser extent, among labor and behavioral economists. Our impression is that large teams are more common in these fields, but a partial review of recent journal publications did not support this impression.

There is also greater support for required contribution statements among more junior members of the profession. Only 32% of Full Professors rank the required options as their top two options. This compares with 40% of Associate and Assistant Professors, 41% of non-ladder faculty, 59% of post-doctoral fellows and PhD candidates.

The major objections to contribution statements were:

1. It won't work. People will just say that everyone worked on all parts equally.
2. Powerful people who did little work will impose a contribution statement that gives them more than their share of the credit.
3. It will create conflict. Authors will not agree about who came up with the idea or the importance of different roles.
4. In many cases, it really is difficult to say who did what.
5. It will encourage adding authors, including RAs, who did very little.
6. It will discourage authors from giving credit to RAs who did much of the work.
7. It will enable researchers to disavow responsibility for aspects of the paper for which they do not claim credit and discourage them from contributing in those areas. We return to this concern in the section on accountability statements.

The major arguments for contribution statements were:

1. Without such statements, senior researchers get most of the credit. When appropriate, they will increase the credit junior researchers receive.
2. The current system allows minor contributors, often senior faculty, to receive equal credit with those who did the work.
3. Assessing contributions is currently difficult, especially for people outside the field even though in many cases it is clear that contributions are not equal. For example, it has become common to include as authors individuals from organizations that made the use of data possible. Promotion and tenure committees often try to infer contributions and do so incorrectly.
4. The current system worked well when articles with three or more authors were rare, but when there are many authors, it produces arbitrary outcomes.

Despite our efforts to describe contribution statements, some respondents clearly interpreted them as quantitative statements allocating percentage credit to each author.

Views on Contribution Statements

	Consistently support		
	Voluntary	Mandatory	N
All	46	31	2986
Behavioral	39	39	180
Development	31	43	239
Econometrics	50	20	177
History	41	30	54
Theory	59	27	193
Env. & Energy	26	55	153
Finance	48	27	198
IO	58	26	170
International	52	25	156
Labor	40	37	467
Macro	58	24	413
Political Economy	55	24	152
Public	49	29	201
Urban	46	25	67

Voluntary: Top two choices are both voluntary

Mandatory: Top two choices are both mandatory

Several respondents suggested that contribution statements are helpful/necessary only when there are sufficient authors. Based on a very small set of suggestions, we sense that a cutoff around four makes sense to this camp, although two respondents suggested ten.

The Committee recommends that journals encourage contribution statements but only require them if there are four or more or, perhaps, more than four authors. It may be desirable to implement this gradually, say by applying it to six authors or more for articles accepted in 2025, five or more in 2026, and four or more in 2027. This would give the profession time to refine the statements and determine whether there are categories of contributions that journals may want to recommend. “All authors contributed equally” should be strongly discouraged.

We reviewed a small nonrandom sample of contribution statements in *PNAS* articles with at least some economist authors. The two articles with only two authors stated that both authors contributed to all aspects of the paper. The two articles with three authors provided some differentiation: (i) “E.R., P.S., and L.Z. designed research; E.R. performed research; E.R., P.S., and L.Z. analyzed data; and E.R., P.S., and L.Z. wrote the paper.” and (ii) “N.M. and P.M. designed research; N.M. and P.M. performed research; C.T.E. analyzed data; and N.M. wrote the paper.” The two papers with four authors claimed that all four authors “designed research, performed research, analyzed data, and wrote the paper.” The two papers with five authors and the one with seven all had elements of differentiation among authors.

We note that such contribution statements can be article specific. For example, “All authors met regularly to discuss all aspects of the paper. A and B led the development of the theory. C was primarily responsible for running the experiment and for coding. C and D did the initial analysis. A and D wrote the first draft. All authors participated in revisions.”

Recommendation A7: Contribution statement
We recommend that journals encourage contribution statements clarifying the contribution of each author but require them only if the number of authors exceeds a specified number. Contribution statements may state that all authors contributed equally, but this is discouraged for cases when the number of authors is sufficient for a mandatory contribution statement.

4.6 Responsibility Statements

There appears to be near unanimous agreement among economists that all authors are jointly responsible for all aspects of the paper regardless of the content of a contribution statement. For example, an author primarily responsible for the design of an experiment is nevertheless expected to understand whether the proofs of the theorems in the theory section are correct, and even an author who does not have access to the data should be expected to ask the sorts of questions that can help reveal fraud.

We recommend that upon acceptance at a journal, all authors must choose between two options. The first is that they are responsible for all aspects of the paper. The second is that authors spell out responsibilities and justify why any author cannot serve as a check on other aspects of the study. This option may be appropriate for projects with many coauthors and specialized roles.

Recommendation A8: Responsibility statement
We recommend that, upon acceptance of a manuscript, journals require that all authors indicate that they take responsibility for all aspects of the paper unless the authors provide a statement allocating responsibilities among the authors in the opening footnote and explicitly state that some author has no responsibility for a certain aspect of the paper.

4.7 Citation Style

The survey did not reveal strong views for keeping or changing the current norm in economics where all authors up to some journal-specific maximum are listed when the paper is first cited but first author et al. is used on subsequent citations for three (sometimes four) or more authors. We suggest that subsequent citations to works with one or two authors continue to use the authors’ names. For three or more authors, we recommend that subsequent citations use the initials of the authors, up to some limit, e.g., BGA2019 to weaken the current emphasis on the first author.

Recommendation A9: Citation style
We recommend that journals adopt the following citation style: List all authors’ last names (up to

some reasonably large number) and the year of publication the first time. Subsequent mentions of papers show the initials of the authors' last names and the year of publication, e.g., BGA2019. Journals can choose various modifications of this style.

4.8 Excessive Focus on a Select Group of Journals

The adverse effect of the excessive focus on a select group of journals is analyzed and explained well in the article by Heckman and Moktan (2020) on the tyranny of the top five (T5). They write "In light of the many adverse and potentially severe consequences associated with reliance on the T5, we believe it unwise for the discipline to continue using publication in the T5 as a measure of research achievement and as a predictor of future scholarly potential. The need for change is made ever more apparent by the T5's inadequacy as a predictor of individual article quality, much less the quality of a person. It also has an apparent gender tilt." Serrano (2018, 2024) offers an exposition of the problems of focusing on a select group of journals, likening it to a disease, "top5itis," plaguing economics. The excessive focus on this select set of journals and the resulting adverse consequences for the profession also came out clearly from the open-ended comments in the survey.

The committee agrees with the views expressed by Heckman and Moktan (2020), Serrano (2018, 2024), and many survey respondents on the adverse consequences of the excessive focus on a select group of journals. Addressing this artificial distinction among journals is a profession-wide problem, but journals can contribute by not using terms that place journals into fixed tiers.

Recommendation A10: Discourage terminology that promotes artificial distinctions between journals
We recommend that authors, reviewers, and editors do not use terms that promote distinctions of journals into fixed tiers, such as "top 5."

Others in the profession can contribute by not using terms such as "top 5", especially not in letters of recommendation, promotion letters, and tenure letters. Similarly, departments can contribute by having promotion policies that depend on multiple metrics of the quality of a person's scholarship rather than on measures heavily (and often discontinuously) weighing whether and how often the person has published in a particular group of journals.

4.9 Use of a Unique Identifier for Authors

Having a unique, consistent identifier for researchers benefits both individuals and the broader research community. Such identifiers facilitate the accurate recognition of authors' contributions, overcoming the limitations of relying solely on personal names. Names can be ambiguous, change over time (e.g., due to marriage or divorce), and vary in formatting and order across cultures. These complications highlight the need for a unique, stable identifier for each researcher.

The organization ORCID—short for "Open Researcher and Contributor Identification"—was founded to meet this need. In its own words, ORCID is "…a non-profit organization supported by a global community of organizational members, including research organizations, publishers, funders, professional associations, and other stakeholders in the research ecosystem." The platform is funded by membership fees from these organizational members, as well as fees from organizations that use it. Individuals register for a free ORCID iD and can include their affiliations, contact information, and research interests. They can also link their research products. Individuals can connect their records to a large number of research and academic institutions, publishers, funders and organizations and can control what information is shared.

The ORCID website summarizes the typical use of ORCID iDs during manuscript submission as follows.

- "The author submits an article submission to the Publisher
- The publisher collects the authenticated author's ORCID iD and requests permission to interact with their record, and stores that permission.
- The publisher collects data from the author's record using the ORCID API and uses it to help populate the submission form.
    - This helps to save the author time manually completing information that is already available within their ORCID record.
    - Affiliations, funding, preprints and datasets can all be discovered
- When a submission is accepted and the article is published, the publisher:
    - Includes the ORCID iDs in the article metadata.
    - Adds the publication to the author's ORCID record, connecting the person with the publication.
    - Displays the iD within the article and the article metadata
    - Displays the iD on the authors information page
- Optionally, the publisher acknowledges reviewers for their peer review work
- Optionally, the publisher collects coauthor and collaborator ORCID iDs and updates their records as well."

As noted, using an ORCID iD during submission can save authors time by automatically providing affiliation and contact details. With appropriate configurations, publishers can also update authors' ORCID records upon publication. This streamlined system can then share information with funders and other interested parties, reducing the reporting burden. More generally, the widespread use of ORCID iDs makes it easier to reliably link authors with their published work, enhancing the discoverability of their research. The benefits increase if the practice of using ORCID iDs becomes standard across economics journals, creating a positive externality for the profession.

Many publishers of economic research—including Oxford University Press, Wiley, Springer, Taylor & Francis, Elsevier, AAAS, and PLOS—are already ORCID members. Leading submission management platforms, such as Editorial Express, ScholarOne, and Editorial Manager, also support ORCID iDs. According to the ORCID website, over 7,000 journals currently collect ORCID iDs from corresponding authors during the submission process.

Prominent publishers like the Royal Society, PLOS, and Science require ORCID iDs for submissions to their journals. This established infrastructure and growing user base positions the economics profession to readily adopt ORCID iDs.

The Committee sees considerable benefits to requiring ORCID iDs for all authors at the time of submission. Although we lack the technical expertise to assess how challenging it might be for some journals to integrate ORCID into their workflows, support is widely available through major submission management platforms, and many journals have already implemented ORCID integration. From our own experience, it is straightforward for authors to register for an ORCID iD. We recommend collecting ORCID iDs for all authors, not just the corresponding author. The recommendation is complementary to the collection of ORCID iDs for reviewers.

Recommendation A11: Collecting ORCID iD upon paper submission
We recommend that journals incorporate ORCID iDs for all authors as a standard component of the manuscript submission process. This information would not be shared with reviewers or anyone from whom the authors' information should be blinded.

4.10 Reviewer Disclosure of Having Previously Reviewed the Paper

Having a reviewer who has previously reviewed the same paper at a different journal has benefits and drawbacks. On the benefit side, such reviewers are often individuals who have the most relevant expertise to evaluate the paper. Also, the marginal effort of reviewing a paper a second time is typically lower than the effort of reviewing a paper for the first time. However, repeat reviewers may not sufficiently update their evaluation based on the revisions to the paper or on different standards of the new journal. Moreover, if a repeat review has idiosyncratic or biased reasons for recommending rejection, the repeat reviewer would have a disproportionately negative effect on the paper's publication chances. For the handling coeditor to evaluate the benefits and drawbacks of using a repeat reviewer, they should know whether a reviewer is a repeat reviewer. We therefore recommend that reviewers who have reviewed a paper at a prior journal disclose this to the coeditor upon receiving the invitation to review the paper. The coeditor can then decide whether to keep the reviewer or recruit a different reviewer.

Recommendation A12: Reviewer disclosure of prior review
We recommend that journals ask reviewers who have previously reviewed a paper for a different journal to disclose this to the coeditor upon receiving the invitation to review the paper.

5. Recommendations on Policies that Foster Transparency

5.1. Harmonized Provision of Information about Journals

There are limits to the extent that a set of rules/guidelines can improve efficiency and meritocracy in the publishing process. Moreover, identifying and enforcing such rules may be hard, and there can be unforeseen consequences. Hence, the committee believes an important complementary way of improving the publication process is to harness the power of competition

between journals by establishing a mechanism by which authors and readers can obtain better information about journals.

The idea behind this mechanism is that authors will prefer to send their manuscripts to journals that are known for handling manuscripts efficiently and not engaging in favoritism. Moreover, readers will lend more credibility to publications in journals that are known to operate in a more meritocratic way. Currently, authors and readers have some information about journals' performance, but this information is often based on a limited number of own experiences (or that of friends). On statistics on journal performance, CDEGT2022 note that "Even when available, statistics are often not directly comparable between journals and say nothing about the subjective experiences of authors and reviewers who both support and depend on the system." Moreover, most of these statistics need to be looked up on various websites rather than being available at a single site, though Juan Carlos Suárez Serrato privately started an initiative to share statistics on a single website.[4]

There were suggestions about making journal data available for researchers to analyze. While occasionally individual journals have made some data available to limited sets of researchers, and we see this as a worthwhile goal, privacy considerations make this challenging, and we leave it to individual journals to set policies on this.

The committee hopes to build on the initiative of Juan Carlos Suárez Serrato by having journals prepare a standardized set of statistics and, ideally, have a dedicated entity take care of maintaining a website with these statistics (so that the burden does not fall on a single individual). We refer to the dedicated entity as the "Journal Information Center." The setup of the Journal Information Center is yet to be determined; it could be run by a professional association or it could be a newly formed entity. Whether or not the Journal Information Center is formed, the committee encourages journals to report a standardized set of statistics, in addition to any journal-specific statistics they may wish to report.

The proposal for harmonized information consists of three parts:

- First, the harmonized reporting of a set of journal performance metrics and journal policies. Performance metrics could include decision times, the number of submissions, the number of publications, and the fraction of desk rejections. Journal policies could include a measure of the strictness of its conflict-of-interest policies, whether the journal operates in a double-blind fashion, participates in the reviewer transfer mechanism, or has published its selection procedure for members of the editorial team.
- Second, harmonized reporting on a select set of author and reviewer characteristics of all submissions and by type of decision (desk rejection, rejection with reports, acceptance).
- Third, reporting the results of a survey of authors. Participating journals would provide to authors who received a final disposition (rejection or acceptance) a survey link to a survey administered by the Journal Information Center. A survey information center

[4] The address of his website is: https://jcsuarez.shinyapps.io/journal_turnaround_app/

would then publish the results of these surveys once a year, including breakdowns by journal.

A. Harmonized Reporting of Journal Performance Metrics and Journal Policies

Many journals already report statistics on their operation, such as turnaround times, but these statistics are often not comparable because they use different definitions or sampling frames. Moreover, these statistics are scattered, increasing search costs for authors who want to compare them when deciding where to submit. Ideally, the collected statistics are:

- based on information that most editorial systems already collect,
- easy to compute (e.g., not Kaplan-Meier),
- hard to manipulate (and corrected for censoring), and
- sensitive to costly outcomes for authors (e.g., long right tails of decision times).

As a starting point, the committee recommends that journals report the following set of harmonized statistics that fulfill the objectives listed above. The appendix contains definitions and rationales for these statistics. The ten suggested statistics are (i) number of first submissions, (ii) coeditor workload, (iii) fraction of desk rejections, (iv) pareto-penalized mean decision lag, (v) stock of manuscripts that has been waiting for more than 6 months for a decision, (vi) number of articles published, (vii) mean time between initial submission and publication, (viii) the number of submissions the journal received via the transfer mechanism, (ix) the number of articles published that were submitted via the transfer mechanism, and (x) the mean time between initial submission and publication for articles submitted via the transfer mechanism.

The Journal Information Center may want to fine-tune these statistics or request additional statistics from participating journals. If so, the list of statistics and their definitions provided by the Journal Information Center will supersede those in the appendix of this report.

In addition, the Journal Information Center would publish the editorial policies of each journal in a standardized fashion. This information could help authors and readers assess the meritocracy of a journal's publishing process. The Journal Information Center would decide on the exact list of characteristics to be collected and participating journals would commit to providing this information to the Center. The committee recommends that, as a starting point, the Journal Information Center would collect and publish which of this committee's recommendations the journal has adopted. The Journal Information Center may adjust the list of editorial policies it collects from participating journals and publishers.

Recommendation B1: Harmonized reporting of journal performance metrics and editorial policies

We recommend that journals include on their website harmonized information on their performance and policies. The harmonized information consists of (i) standardized metrics on journal performance, such as decision times, desk rejection rates, and number of rounds for accepted papers, and (ii) information about their editorial policies. The initial set of metrics to be reported is defined in the appendix of this report. We recommend the creation of a Journal

Information Center that would compile this information on a central website. After the Journal Information Center is formed, we recommend that journals follow this center's definition of the statistics to be reported and also report the statistics directly to them.

B. Harmonized Reporting of Journal Outcomes by Author and Reviewer Characteristics

It would be informative for researchers deciding where to submit papers to have more detailed information about submission and acceptance patterns by author characteristics. For example, Jeff Weaver took the initiative to show publication patterns by institutional affiliation of the author for a select set of journals. His tabulations received a lot of attention on social media, indicating a demand for such information. Jeff Weaver kindly shared an expanded set of tabulations with the committee, which have been included in Appendix E. The committee proposes to expand on Jeff Weaver's analysis by having journals report a specific set of outcomes by a standardized set of author characteristics.

Different journal outcomes across different groups of authors could arise for many reasons, including selection and favoritism. Still, knowledge about differences in outcomes, especially when comparing similar journals, can be useful. For example, if a journal's outcomes by author characteristics differ markedly from those of comparable journals, the journal may want to investigate why this is the case and possibly adjust how it operates. Similarly, knowledge about reviewer characteristics can also help authors decide where to submit their paper. The committee therefore recommends that journals report outcomes by author and reviewer characteristics for three metrics: rank/classification of institution, region of institution, and gender.

The committee recognizes that journals' information about author and reviewer characteristics is typically limited to institutional affiliation and that collecting more detailed information may require costly adjustments to editorial systems and impose time costs on submitting authors and reviewers. However, given concerns about possible gender bias discussed elsewhere in this report, we strongly suggest that journals add questions to their editorial systems asking authors and reviewers to self-identify their gender. If self-identified gender is not available and can't be inferred from a title (e.g., "Mr" or "Ms"), journal editors or their staff may use personal knowledge about a person's gender or could use https://genderize.io/, which can identify the gender of most names with a high degree of precision. The cost of searching for the few remaining persons' gender should be low.

To limit statistical noise, the committee recommends reporting by broad groups over rolling three-year periods. (Note: the reporting window should be three calendar years, t-4, t-3, and t-2, where t-1 is the last full year to have elapsed prior to the report. For example, a report prepared in early 2025 would include the outcomes of manuscripts submitted in the years 2021, 2022, and 2023. Having a year's lag—2024, in this example—allows time for most submissions in the reporting window to have received decisions.)

The committee suggests as a starting point the following three types of author and reviewer categories.

1. Four categories based on the classification of the institution with which the author or reviewer is affiliated, independent of department. The categories are (i) universities ranked 1-25, (ii) universities ranked 26-100, (iii) other universities, and (iv) non-university institutions. The university rankings are taken as the average of the following three rankings, as of July 1st in the previous calendar year: (A) "Best Global Universities for Economics and Business" by *US News & World Report*, (B) "World University Rankings by Subject: Business and Economics" by *Times Higher Education*, and (C) "Top Economics Departments" by IDEAS based on RePEc data. The Journal Information Center would make this average of these three rankings available to journals.
2. Six geographical regions of the institutional affiliation of authors and reviewers. The regions are defined by the Econometric Society and consist of Africa, Asia, Australasia, Europe and Other Areas, Latin America, and North America.
3. Male, female, and unknown/other. We combine unknown and other to preserve confidentiality and to avoid bias since authors are more likely to be classified as nonbinary if the editor knows of them.

The five metrics to be broken down by author and reviewer category are:

1. The number of first submissions during the three-year reporting window.
2. The fraction of manuscripts submitted in the reporting window that were rejected without reports.
3. The fraction of manuscripts submitted in the reporting window that were rejected with reports.
4. The fraction of manuscripts submitted in the reporting window that are on a path toward acceptance, that is, manuscripts that have a revise and resubmit or that are accepted or conditionally accepted.
5. The fraction of manuscripts submitted in the reporting window that do not yet have an initial decision.

Because manuscripts often have multiple authors and it is useful to consider characteristics of all authors (the identity of the submitting author is endogenous), manuscripts cannot be uniquely classified into one category if their authors belong to different categories. Moreover, given that journals make decisions at the manuscript level (not author level), reporting statistics on manuscripts rather than authors is most meaningful. The committee recommends that the five metrics above be calculated by making the author-by-manuscript combination the unit of observation and weighting each author-by-manuscript observation by one over the number of authors on the manuscript.

Similarly, when calculating reviewer characteristics, the committee recommends that the five metrics above are calculated by making the reviewer-by-manuscript combination the unit of observation and weighting each reviewer-by-manuscript observation by one over the number of distinct reviewers consulted on the manuscript. In this calculation, a reviewer who is consulted on multiple rounds of review of the same manuscript is counted once.

To minimize the risk that authors or reviewers can be identified from the reported statistics, we recommend that any cells with fewer than 5 manuscripts be suppressed.

The Journal Information Center may want to fine-tune these metrics, the categories, or the method calculation. If so, the guidance by the Journal Information Center will supersede what is described above.

Recommendation B2: Harmonized reporting of decisions by author categories
We recommend that journals report on their website a breakdown of submissions and decision types by a standardized set of author categories. The initial suggested set of metrics and author categories to be reported is defined in this report. Suggested author categories include institutional rank, geographic region, and gender. After the Journal Information Center is formed, we recommend that journals follow this center's definition of the statistics to be reported and also report the statistics directly to them.

Recommendation B3: Harmonized reporting of decisions by reviewer categories
We recommend that journals report on their website a breakdown of submissions and decision types by a standardized set of reviewer categories. The initial set of metrics and reviewer categories to be reported is defined in this report. After the Journal Information Center is formed, we recommend that journals follow this center's definition of the statistics to be reported and also report the statistics directly to them.

C. Survey of Authors

Better information about authors' perceived experiences with different journals can help other authors decide which journal to submit to and, thereby, provide incentives to journals to improve how they treat authors. Of course, most authors will disagree with rejection decisions and most authors will find reviewers too demanding. However, differences across journals in how authors with the same decision type rate their experience will still be informative.

The author survey would be administered and designed by the Journal Information Center, thereby guaranteeing that responses are anonymous to editors. The survey would be sent to the corresponding author upon final disposition of a manuscript (a rejection or an acceptance). Each year, the Journal Information Center would publish the results of the survey by journal. Ratings by coeditor would be shared with the editor of the journal but not be made public because ratings by coeditor can be noisy. The ratings would be reported only conditional on decision type (desk rejection, rejection with reports, acceptance) because the decision type naturally influences the authors' feelings toward the journal.

The Journal Information Center would decide the exact questions on the author survey. As a starting point, the survey could include questions about the coeditors' clarity of reasoning for the decision, perceived bias in the decision, perceived quality of the referee reports, whether reviewers were overstepping their role by acting as shadow coauthors, the use of mean-spirited or unprofessional language by reviewers or by the coeditor, whether the coeditor provided

enough guidance on the revision, whether revisions were inappropriately extensive, perceived timeliness of the overall editorial process (from initial submission to final decision), and any feedback the author wishes to anonymously share with the journal (coming with a caveat that the content of the feedback may implicitly identify the author).

Participating journals would commit to providing a survey link to all authors who received a final disposition on their manuscript. The link should be unique to each manuscript so that only the corresponding author can complete the survey for that submission and so that the Journal Information Center can link the responses back to the submission. The Journal Information Center should provide journals with information aggregated to at least the coeditor level by decision type, blinding cells with fewer than 5 observations to ensure that coeditors do not receive author-specific responses.

Recommendation B4: Survey of authors
We recommend that, upon the request of the Journal Information Center, journals send a manuscript-specific link to a survey, administered by the Journal Information Center, to authors whose paper received a final decision.

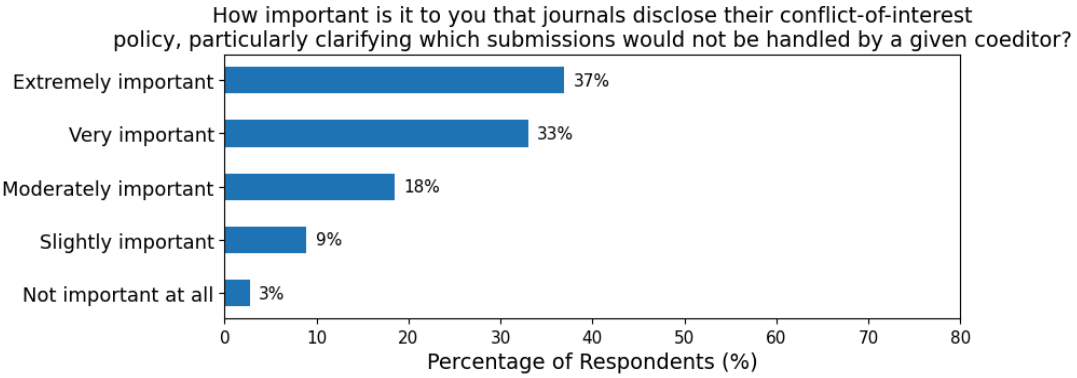
5.2. Conflicts of Interest

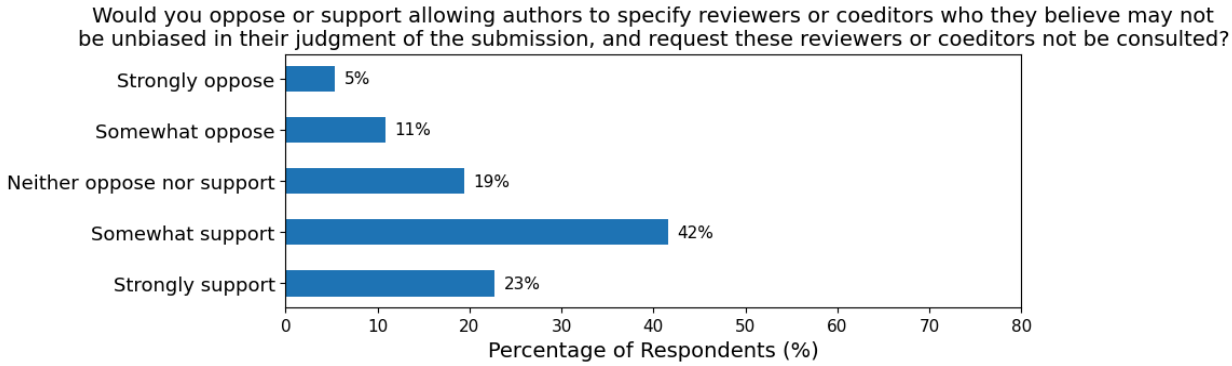
Respondents view it as important that journals disclose their conflict-of-interest policies, with 70% of respondents rating this “extremely important” or “very important.” They also generally support the idea that authors can request not to have particular coeditors or reviewers handle the paper, with 65% of respondents saying they “somewhat” or “strongly” support this. In addition, 80% of respondents in the survey by CDEGT2022 believe that reviewers should never or very rarely review papers by coauthors or friends (their Figure 11). Finally, 63% “somewhat” or “strongly” support also making submitting authors responsible for identifying conflicts of interest, rather than having this responsibility lie solely with the coeditors. We begin with authors, before turning to reviewers, and coeditors.

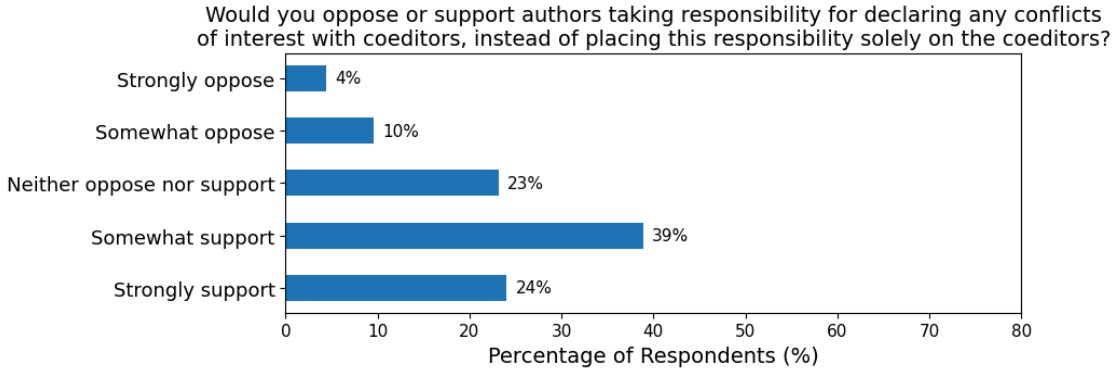
Recommendation B5: Asking authors to disclose conflicts of interest
We recommend that journals ask authors, as part of the submission process, to disclose conflicts of interest with the journal’s editor or coeditors. This disclosure complements the journal’s own efforts in identifying conflicts of interest.

In line with Siemroth’s (2024) third recommendation, we recommend:

Recommendation B6: Asking reviewers to disclose conflicts of interest
We recommend that journals ask reviewers, as soon as they are invited, to disclose to the coeditor any conflicts of interest with the authors. Having, or working on, a competing paper should also be disclosed. It is up to the coeditor’s judgment whether to retain or excuse the reviewer.







Current conflict-of-interest policies differ by journals. While the committee sees benefits from standardization, it also recognizes that conflicts-of-interest rules involve a tradeoff between impartiality and having a coeditor who is knowledgeable on the topic of the paper. This tradeoff may be different for different journals and there are benefits to journals experimenting in finding the best way to navigate this tradeoff. For example, journals with more coeditors who have similar expertise can have stricter conflict-of-interest rules. Finally, the strictness of conflict-of-interest rules may be a dimension on which journals compete. The committee therefore does not recommend a standardized set of conflict-of-interest rules, but instead recommends transparency. Because not all conflicts of interest can be classified easily, transparency through identifying the handling editor is also important. Moreover, this allows others to verify that the posted conflict-of-interest rules were followed.

Recommendation B7: Identification of the handling coeditor
We recommend that journals publish in the opening footnote the identity of the person who decided to accept the paper (typically, this is the handling coeditor).

Recommendation B8: Communication of conflict-of-interest policies
We recommend that journals adopt a conflict-of-interest policy and publish it on their website.

To facilitate the comparison of the stringency of conflict-of-interest rules across journals, it would be helpful if journals followed a standard template of potential components of their conflict-of-interest policy, which they can supplement with additional journal-specific components. This will then enable others, such as the Journal Information Center, to rank or classify journals based on their conflict-of-interest policies. The committee suggests that journals use the following template, with journals deciding for themselves the level of stringency (option a, b, or c) for each component:

The editor is included in "coeditors" in their capacity of handling papers.

A. Coeditors are recused from papers with authors…
    a. currently at the same institution as the coeditor (regardless of department), where institutional affiliation is defined as current employment, a visiting position, or being recruited (e.g., having given or scheduled a job talk or having an offer) by that institution.
    b. currently at the same institution as the coeditor (regardless of department), where institutional affiliation is defined as current employment or a visiting position.
    c. currently employed at the same department in their institution (excluding visitors).
B. Coeditors are recused from papers with authors…
    a. who are current students or current visiting students at the same institution.
    b. who are current students at the same institution.
    c. who are current students at the same department.
C. Coeditors are recused from papers with authors…
    a. who were graduate students in the department at which the coeditor then worked or who, as an undergraduate or graduate student, were advised by the coeditor.
    b. who are untenured and as a graduate student were actively advised by the coeditor.
    c. who are untenured and had the coeditor on their PhD thesis committee.
D. Coeditors are recused from papers with authors…
    a. with whom they are currently working on a research project or with whom they have published a paper.
    b. with whom they are currently working on a research project or with whom they have published a paper in the past 10 years.
    c. with whom they are currently working on a research project or with whom they have published a paper in the past 2 years.

E. Coeditors are recused from papers with authors…
    a. who were professors of a class the coeditor took as a student, advised their undergraduate or graduate thesis, or wrote them a letter of recommendation.
    b. who served on the coeditor's PhD thesis committee or wrote them a letter of recommendation.
    c. who were the coeditor's main PhD thesis advisor.
F. Coeditors are recused from papers with authors…
    a. who are family members.
G. Coeditors…
    a. are not allowed to submit to their own journal after they have been appointed (even if their term has not yet started) until two years after their term has ended.
    b. are allowed to submit to their own journal if they are not the editor-in-chief, but the submission will be handled by a guest editor. If the coeditor is the editor-in-chief, they follow the rule of option "a" above.
    c. are allowed to submit to their own journal.
H. Coeditors are not allowed to handle a paper by an author who
    a. as a coeditor is currently handling one of the coeditor's own papers or has accepted one of the coeditor's own papers in the past 10 years.
    b. as a coeditor is currently handling one of the coeditor's own papers or has accepted one of the coeditor's own papers in the past 2 years.
    c. is currently handling one of the coeditor's own papers.
I. With regard to any other situation (such as a close personal or professional relationship, a former collaboration, a minor advising relationship, or another situation not covered by the rules above), which, in the coeditor's opinion, creates a conflict or would create an appearance of a conflict,
    a. the coeditor is recused.
    b. the coeditor must disclose it to the editor, who decides whether the coeditor must recuse themselves or can decide for themselves whether they want to recuse themselves.
    c. the coeditor themselves decides whether they should recuse themselves.

Most reviewer recommendations and editorial decisions rely on judgement calls that balance the value of the contribution against its limitations. Most journals have a policy that judgments are not amenable to appeal. However, editors and reviewers are not infallible, and it is possible that procedural mistakes were made or that a factual misunderstanding by reviewers or coeditors was pivotal in the decision. Having a journal clearly spell out the procedure for a possible appeal and valid grounds for appeal will help level the playing field and avoid disparities between authors who are in the know about the possibility of an appeal and those who are not.

Recommendation B9: Communication of appeals policy
We recommend that journals publish their appeals policy on their websites: both the process and valid grounds for appeal.

Establishing diverse editorial boards and clear term limits is also important to ensure that researchers are not disadvantaged by being forced to deal with a small set of editors who may not be favorably disposed to particular types of research. Moreover, more diversity in terms opf background and institution among the coeditors makes it less likely that multiple coeditors have a conflict of interest with the same author. In their section 3.2.3.4, CDEGT2022 similarly express concerns about concentration within editorial boards, long editorial terms, and editors serving simultaneously at multiple journals. Because journals have different constraints and the tradeoffs are complex, the committee decided not to recommend specific term limits or specific rules for ensuring diversity in terms of background and institution. Instead, the committee believes that transparency in term limits and the selection procedure used for the editorial team will put pressure on journals to adopt rules that will make their journal more credible among readers and more attractive to authors.

Recommendation B10: Communication of term limits
We recommend that journals publish on their websites any term limits for editors, coeditors, associate editors, and board members.

Recommendation B11: Selection procedures for the editorial team
We recommend that journals publish on their websites how editors, coeditors, associate editors, and board members are selected and what criteria are used (including qualifications, experience, and diversity of institution, background, and geography).

6. Recommendations with Limited Returns to Harmonization

6.1. Double-blind Reviewing: Not Revealing Author Identity to Reviewers

We strongly believe that submissions should be judged on their content, not on the name or affiliation of the authors. We hope and expect that most journal editors share this view and encourage them to make their position clear to reviewers and coeditors. One proposal to further this objective is to suppress information about authors to reviewers.

The survey did not ask about “double-blind” reviewing because, at that time, the committee believed that double-blind reviewing was a nonstarter given that authors of many submitted papers can easily be found by a quick internet search. However, the request to return to double-blind reviewing was a very frequent comment in the open-ended part of the survey. The committee felt it therefore deserved serious consideration. Moreover, double-blind reviewing was recommended by Siemroth (2024) and discussed in CDEGT2022 as an optional choice.

Universal double-blind reviewing may not be possible in economics because it is common for economics papers to be circulated prior to submission and reviewers may have already seen the paper as a result. Still, journals could refrain from revealing author identities to reviewers. Key rationales for not revealing author identities to reviewers include:

- It is the “first-best” policy in the sense that if we could design a review system from scratch, we would want reviewers to evaluate a manuscript solely based on its content without taking into account the identity of the authors.
- Reviewers generally want to give an unbiased assessment. If they received a manuscript without author information, they are not forced to be aware of the authors’ identities.
- It sends a signal that we do not want reviewers to take author identities into account when reviewing a manuscript.

According to Blank (1991), supporters of revealing the authors’ identities to reviewers typically make the following arguments:

- Hiding author identities imposes additional administrative costs on editorial offices.
- The name and institution of the authors provide useful information for reviewers, such as whether they should give extra scrutiny to the technical details in a paper.
- It is ineffective because often reviewers can infer the authors’ identities anyway (e.g., from the text, from the references, or through an online search)

The committee recognizes that hiding author identities imposes administrative costs but believes these are manageable. The committee is skeptical about the second claim—it implies that the amount of scrutiny depends on an author’s background, which means more prestigious authors will be less scrutinized.

The argument about ineffectiveness is, strictly speaking not an argument in favor of journals sharing author identities. It just says that hiding the identities does not matter. The committee recognizes that hiding the authors’ identities will not be effective if the reviewer already knows the paper or can infer the author's identity from the text or citations. However, it will still be effective in the remaining cases, and this is better than nothing. After all, many reviewers want to evaluate a manuscript solely based on its contents and prefer not to know author identities.

Hiding author identities will be ineffective if reviewers look up the paper online. However, journals can minimize the extent to which this occurs by explicitly asking reviewers not to look up the authors’ identities and explaining why. In addition, upon submission of the referee report, a journal can ask whether the reviewer knew the authors’ identities and how the reviewer found out. When faced with the prospect of either having to admit they looked up the authors or having to lie about it, reviewers may choose to keep their curiosity in check.

Possible additional concerns about hiding author identities include:

- Reviewers won’t be aware of authors’ conflicts of interest. However, very few authors have conflicts of interest. Hence, in cases where authors declare a conflict of interest, a journal can share this information with the reviewers if it thinks the benefit of sharing this information with reviewers outweighs the benefit of hiding the author’s identity. Or the editor can take this information into account when they write the decision letter.
- Reviewers may have a conflict of interest with the authors. If the reviewer does not know the authors’ identities, then the conflict of interest cannot affect their judgment, and is

therefore not a problem. However, it is a problem if the reviewer does know who the authors are, is biased due to the conflict of interest, but can claim *not* to have known. We recognize this concern, but our view is that most reviewers would rather declare the conflict of interest and be excused. Hence, we think that this can be largely addressed if journals ask reviewers to declare conflicts of interest if they know or become aware of the authors' identities and recognize they have a conflict of interest.

- Reviewers won't be able to try to give extra help to disadvantaged groups. The question is whether the merits of hiding author identities depend on which groups benefit from it or whether it is inherently desirable for author identities not to be used in evaluating a manuscript. On net, the committee leaned toward the second view.

The committee's views were also influenced by research showing evidence that network connections, gender, and race may affect publication outcomes (e.g., Colussi, 2018; CDFI2020; Hengel, 2022; CFL2024; PW2024), though not all studies detect such effects (e.g., Abrevaya and Hamermesh, 2012). To the extent these effects operate through reviewer perceptions of author characteristics, hiding author identities would mitigate them. The research on the effects of hiding author identities from reviewers in economics is limited. Blank (1991) analyzes an experiment in which the *American Economic Review* randomized whether a paper's authors were revealed to the reviewers between 1987 and 1989. The study finds that revealing author identity resulted in different patterns of acceptance rates and referee ratings by the institutional rank of author, but no significant effects by gender (though it could not rule out economically meaningful effects). Carlsson, Lofgren, and Sterner (2012) randomly assigned submissions to an economics conference to either single or double-blind review and found no effect on acceptance rates by gender. Huber, Inoua, Kerschbamer, König-Kersting, Palan, and Smith (2022) describe an experiment in which the *Journal of Behavioral and Experimental Finance* sent the same paper to thousands of potential reviewers, randomizing whether only the identity of a well-known coauthor (V.L. Smith, a Nobel Prize winner) was revealed to the reviewer, only the identity of a relatively unknown coauthor (S. Inoua, an early career research associate) was revealed, or neither author was revealed. The study saw large differences in reviewer behavior across the three experimental conditions. Potential reviewers were 10 percentage points more likely to accept the invitation if the more famous coauthor was revealed rather than the less famous one. Reviewers also give more favorable recommendations when the more famous coauthor was revealed: More than 20% of the reviewers recommended "accept" if the more famous coauthor was revealed compared to less than 2% for the less famous coauthor.

Experiments have examined the effects of blinding reviewers in settings outside economics. Tomkins, Zhang, and Heavlin (2017) find a statistically insignificant effect of double-blind review for papers submitted for publication in a conference proceedings, albeit one that favors women. Nakamura, Mann, Lindner, Braithwaite, Chen, Vancea, Byrnes, Durrant, and Reed (2021) find redaction reduces, but does not eliminate the disparity between black and white applicants in the evaluation of NIH grant proposals. Double-blind review lowered the evaluation, but not the acceptance rate, of papers submitted to a computer science conference by well-known authors (Sun, Barry Danfa, and Teplitskiy 2022). Fox, Meyer, and Aimé (2023) report on an RCT at *Functional Ecology* where a double-blind review helped authors from low-income and

non-English-speaking countries. Uchida (2024) compared outcomes when the same paper was subjected to single- and double-blind reviews and found differences in which papers were accepted but that double-blind review did not affect the quality of decisions. Ceci, Kahn, and Williams (2023) review the literature on double-blind review in STEM and find no clear evidence that it affects gender disparities, although they view the evidence as limited.

It is not clear to what extent the findings from these studies carry over to the economics profession currently. In most of the studies cited, reviewers would not have been able to find the authors through an online search, and other aspects of the institutional context often differ as well. However, the studies clearly suggest that evaluators can be influenced by the characteristics of the authors, particularly that they favor more prestigious and more highly published authors, consistent with concerns expressed in the open-ended responses to the survey.

The committee is skeptical that the merits of not revealing author identity to reviewers can be determined by the outcome of an additional randomized control trial because it is hard to find outcome variables that (i) are available in the short term, say within one or two years, (ii) cannot be easily manipulated, and (iii) are good proxies for a meritocratic editorial decision. For example, citations to a publication after one or two years are an imperfect measure of its contribution to knowledge. Moreover, many papers already have some citations at the time of submission. Hence, editors and reviewers, who would know to which treatment arm they are randomized, have a good predictor of citations in the near future by observing the current citations to the paper. While a finding of a treatment effect on measured author characteristics implies that author characteristics were used in the review process, a lack of significant treatment effects still leaves open the possibility that unmeasured author characteristics influenced the outcome of the review process. Moreover, insignificant treatment effects do not necessarily rule out economically meaningful treatment effects.

Based on a priori arguments and existing empirical findings, the committee thought the benefits of hiding author identities from reviewers outweighed the administrative costs associated with it. While doing so may not be effective in all cases, the committee felt it is still worthwhile to move toward it for those cases where it is effective and because of the signal it sends to reviewers that they are expected to evaluate just the manuscript without using information about the authors' identities. Journals can reinforce this expectation by explaining to reviewers that they, of course, are well aware that in many cases, reviewers could look up the authors, but that they ask the reviewers not to do so. However, the committee recognizes that reasonable people can disagree on the desirability of adopting a practice in which journals do not reveal author identities to reviewers. The committee therefore recommends that journals *consider* adopting this practice but does not give a blanket recommendation.

Recommendation C1: Not revealing author identities to reviewers
We recommend that journals consider instituting a practice by which they avoid revealing author identities to reviewers. Under this practice, journals would ask authors to submit a version of the manuscript that does not reveal their identities and would share this version with the reviewers.

Journals would also ask the reviewers to refrain from looking up author identities and not to rely on these identities if they know them.

6.2. Desk Rejection Protocol Without Author Identities

Coeditors need to know authors' identities when they send out a manuscript for review so that they can ensure that reviewers do not have conflicts of interest with the authors. However, there is no need for coeditors to be aware of author identities when they decide whether or not to desk reject a manuscript. Given the role of judgment involved with desk rejections, coeditors may be (subconsciously) influenced by authors' identities. This can be avoided by journals sharing authors' identities with coeditors only after the coeditor has decided whether or not to desk reject a manuscript.

Blinding desk rejections also has some downsides. It imposes additional administrative costs on journals. In addition, if authors fail to identify coeditors with a conflict of interest during the paper's submission, the paper may be assigned to a coeditor with a conflict of interest, which could lead to several difficulties: (i) If the coeditor decides to desk reject the paper, the decision could affect the coeditor's relationship with the author despite the author's knowing that the decision was blinded. (ii) If the coeditor decides not to desk reject the paper, they would have to recuse themselves once the author's identity is revealed (prior to the selection of reviewers). If the new coeditor does not see good prospects for the paper, they could desk reject it (exposing the authors to double jeopardy) or feel compelled to honor the first coeditor's decision by sending it out for review, which would lead to a waste of reviewer time and effort.

Overall, the committee thought the benefits of not revealing author identities to coeditors at the desk rejection stage outweighed its downsides. However, the committee recognizes that reasonable people may weigh the benefits and downsides differently. The committee therefore recommends that journals *consider* adopting a protocol in which they avoid revealing author identities to coeditors at the desk-rejection stage but does not offer a blanket recommendation.

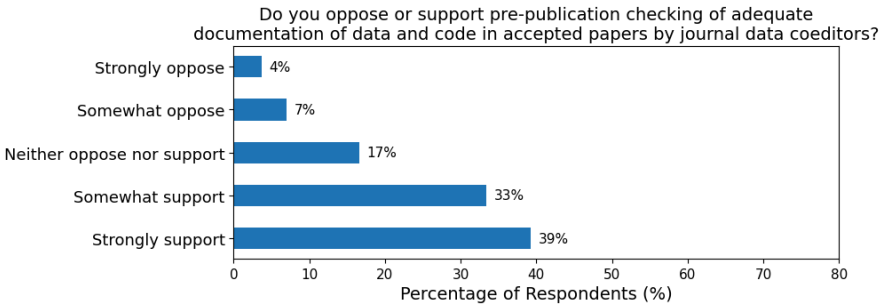
Recommendation C2: Desk rejection protocol without author identities
We recommend that journals consider adopting a protocol by which they avoid revealing author identities to coeditors and ask coeditors not to search for them at the desk rejection stage. If adopted, the identities of authors would be shared with the coeditor only after the coeditor has committed to a decision on whether to send the manuscript out for review so that they can avoid inviting reviewers with conflicts of interest.

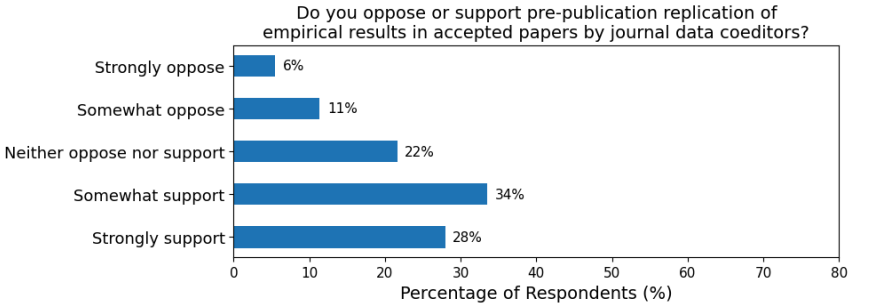
6.3. Data and Code Policies

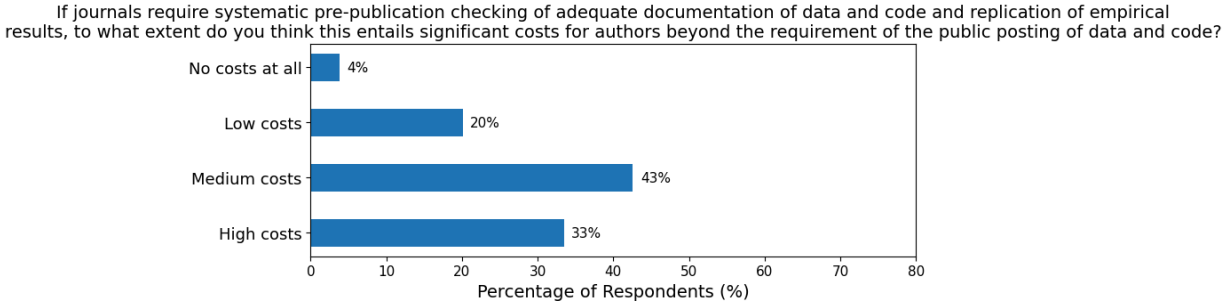
Closed-ended responses. The figure below reveals strong support for pre-publication checking of adequate documentation of data and code in accepted papers (>70% somewhat or strongly support) and a bit weaker support for pre-publication replication of empirical results in accepted papers (still >60% support), even while most respondents recognize significant costs (>70%

think it entails medium or high costs). This basic picture does not seem to depend much on seniority, society, or region (See the full survey here).

Open-ended responses. The survey contained about 1,200 open-ended responses to the two questions on this topic. The committee read all these comments and is very grateful for the thought and care respondents put into them. The open-ended responses add significant nuance to the close-ended replies and reveal some strongly held views on either side of the topic.







Meaning of the policies. The first question asks about the policy of checking that adequate documentation was provided for the code (the order in which programs need to be run, which programs need to be used, etc.) and for the data (how it can be obtained, what exact questions were used, etc.). Assessing whether documentation is adequate does not involve making code available to referees or the coeditor during the review process, it does not require that data be posted, and it does not involve the data editor actually checking that the code produces the results in the paper. The second question asks, in addition, about the value of checking that the data and code produce the results in the paper. In cases of proprietary data, data editors are sometimes able to obtain access, but in other cases, this is not feasible. Some respondents

pointed out that the term “reproduction” rather than “replication” should be used to refer to these policies because, in some fields, replication is understood to involve obtaining new data and doing the analysis from scratch.

Strong views both in favor and against these policies. Many respondents expressed strong support for these policies, noting that they were very important for the credibility of economics, saying that this was the most important topic of the survey, and arguing that complying with these policies does not take much time if the researcher has their empirical analysis well organized from the beginning. Some respondents argued that the policies needed to be stronger, with replication packages being available to reviewers and relegating papers with proprietary data to separate journals. However, there was also a sizable group of respondents expressing strong opposition to the current policies, believing that these policies yield only marginal benefits and are very costly in terms of the time requirements on researchers, delays in publications, and use of journal resources. Some saw them as entry barriers purposely put up by well-resourced researchers to keep other researchers out. Finally, the open-ended responses revealed important nuances, including that it may make sense for the most prominent journals to have such policies but not for all journals and that the fixed cost of learning how to organize empirical analysis to have it ready for a replication package is high, but that then the marginal costs of complying with these policies is low.

Strong concerns about disparate impacts of these policies. Many respondents, including many who support these policies, pointed out that these policies have disparate impacts across researchers and increase existing inequalities. Hence, journals may want to think about whether they can adjust these policies to reduce disparate impacts. The disparate impact most often mentioned is that these policies are easier to comply with for well-resourced economists who have research assistants who can put together their replication packages. Second, by resources of the journal—many journals don’t have the resources to hire a specialized data editor, and it may not be reasonable to add checking for compliance with these policies to the existing editorial workload. Third, by research methodology—these policies place an extra burden on empirical researchers. Some wondered whether there should be an analogous process for checking theorists’ proofs. Fourth, due to the computational complexity of empirical research—these policies are most costly for researchers that use thousands of dollars of computing time. Moreover, those with complex code sometimes view the code itself as intellectual property that they want to keep proprietary. Fifth, by the source of the data. Those that have proprietary data to which the data editor cannot gain access face less scrutiny and costs. Those who collected their own data, which in some fields such as economic history can take many years of work, fear that sharing their data will lead others to free ride on their data collection efforts. Finally, some worried that these extra requirements put economists at a disadvantage relative to other social scientists, especially in interdisciplinary departments.

Principle vs. practice. Many researchers who support having replication policies expressed concerns about how they were implemented in practice. They found that data editors at certain journals imposed idiosyncratic requirements and were too slow in their reviews. They found that requiring that programs work across different platforms, run with a single script, and produce

well-formatted tables (rather than having the results in the log file) went beyond what they interpreted as replication. Similarly, requirements on how README files are formatted and how data are cited were often mentioned as examples of what respondents saw as the process having gone too far and imposing costs on authors with limited benefits. Some respondents expressed opposite views. They saw the above mentioned concerns mostly as growing pains and a reflection of the fixed cost of learning how to better organize empirical research. They would like to see replication packages that are better documented and run with a single script, and some of them mentioned that they had experienced trouble getting existing replication packages to work.

Specific suggestions from respondents. Below are ideas mentioned by respondents that individual journals may wish to consider. The committee is sharing these ideas without endorsing or disapproving them. First, post data that was particularly costly to collect ("it took me 20 years") with a delay so that the author has exclusive access to this data for a bit longer and a chance to write other papers with it. The same might apply for computational techniques that constitute intellectual property. Second, allow for exceptions to the replication policy for particularly computationally heavy projects. Third, have quicker and more direct communication between the data editor and authors to clear up minor issues. Moreover, the data editor could specify the required fixes to things like citations, the README file, and so on. Authors often are at a loss for understanding exactly what they need to do, while the data editor knows what they are looking for. Fourth, replication policies should be complemented with journals being more willing to publish comments on papers that have mistakes; otherwise, replication packages still have a limited impact on rectifying errors.

Views on the role of these policies. The comments also reflected different views on the role and limitations of these policies. Many comments mentioned that these policies enhance the credibility of economic research and the credibility of economics as a discipline overall. Some respondents also mentioned that the requirement to put together a replication package caused them to catch inadvertent mistakes in their own work or caused them to adopt better ways of conducting their empirical research. Some respondents mentioned that these policies won't prevent ill-intentioned researchers from committing misconduct, though others pointed out that these policies make it harder to do so and make it easier to uncover. Some respondents also mentioned that these policies don't ensure the code is correct or corresponds to the methods described in the paper (data editors do not check code for correctness; they only check whether it reproduces the results in the paper). However, these policies enable others to uncover such mistakes.

The Committee's Take:

1. *Replication policies should rest with individual journals rather than be harmonized.* While the profession is generally supportive of these policies, and harmonization has the benefit that authors do not need to learn journal-specific policies, the committee believes that the profession is best served by refraining from offering recommendations on them for two reasons. First, recommendations on these policies may undermine ongoing efforts by journals to find the right balance between strictness and flexibility.

Experimentation by journals and competition between them push journals toward formulating policies that balance the benefits (publishing more credible research) and the costs (authors avoiding journals with especially onerous replication policies). Second, given the heterogeneity in views and the disparate impacts of these policies along the dimensions described above, having different journals with different policies allows authors to select journals with policies that fit their needs or preferences.

2. *Journals should consider the findings described above in formulating replication policies.* The committee hopes that its description of the views, considerations, and suggestions offered by respondents will help journals formulate or refine their replication policies.
3. *Some harmonization can happen organically.* Journals that wish to adopt similar policies can make life easier for authors by formulating a common replication standard. One such standard that already exists is the "Data and Code Availability Standard" (DCAS), to which many leading journals already commit.
4. While the committee isn't issuing specific recommendations on data availability and replication policies to be adopted by all journals, it does endorse the general principles of research transparency, non-exclusive data access, and code availability.

Recommendation C3: Data and code policies

We endorse a general principle of transparency whereby data, code and, if applicable, pre-analysis plans and experimental protocols used in accepted empirical papers should be made available publicly by authors, with flexible allowances for disclosed and explained special circumstances. Journals may consider signing on to an existing data and code availability standard (such as DCAS).

We refrain from endorsing the systematic pre-publication replication and checking of data and code by data editors, but we encourage experimentation by journals in this respect. Journals should consider the costs and benefits of the latter policy, as revealed by the survey, when formulating or adjusting their policies.

6.4. Measures to Reduce the Concentration of Influence

As documented by FOA2015, Ductor and Visser (2023), Wright (2023), and FXZZ2024, economics stands out as a discipline in its emphasis on hierarchy and the degree to which recognition and positions of influence are concentrated. In addition, Angus, Atalay, Newton, and Ubilava (2021) show that editorial boards in economics are highly geographically concentrated. The harmful effects of a concentration of influence are amplified when people in a position of influence favor members of their networks and when the profession places disproportionate value on publications in a select group of journals (Heckman and Moktan, 2020). Concerns about concentration of power, the role of networks, and the disproportionate valuation of select journals also frequently and forcefully came up in the open-ended comments to the survey. Journals can help reduce the concentration of influence in economics by adopting meaningful term limits, appointing an editorial team with a wide variety of backgrounds, and requiring that their editors and coeditors don't simultaneously hold other "gatekeeping" positions.

Recommendation C4: Adoption of term limits
We recommend that journals adopt meaningful term limits for editors, coeditors, associate editors, and board members.

Recommendation C5: Diversity of institution, background and geography in the editorial team
We recommend that journals aim to have an editorial team with a wide range of backgrounds, institutions where they were educated, current institutional affiliations, and geographical locations.

Recommendation C6: Limit concentration of influence
We encourage journals to ask their editors and coeditors to avoid holding simultaneous editorial positions at other journals and, ideally, to refrain from simultaneously holding positions that allow them to select participants for recurring influential conferences or members of professional networks.

6.5. Measures to Improve the Quality of Editorial Decisions and Letters

Quite a few respondents to the survey relayed bad journal experiences that they had as an author or witnessed as a reviewer. It is impossible to prescribe in a uniform set of rules how coeditors should handle manuscripts, because coediting is complex, journals face different circumstances, and views on what constitutes appropriate coeditor behavior are heterogeneous. The committee therefore did not want to recommend a specific set of rules. However, it does believe that overworked coeditors are less likely to do a good job. It further believes that journals should give coeditors training and guidance, based on their view of what constitutes good coediting practices. In addition, being transparent about this guidance will stimulate learning about good practices and may help potential authors decide where to submit.

Recommendation C7: Limit coeditor workload
We recommend that journals attract a sufficient number of coeditors so that the workload of each coeditor allows them sufficient time to clearly explain the rationale of their decisions and to offer clear guidance on revision requests to authors.

Recommendation C8: Training materials
We recommend that journals provide coeditors with materials that give them guidance on their job. Journals should consider publishing these materials on their website.

To assist journals in developing their own training materials, the committee formulated what it sees as “good editorial practices” for coeditors. These practices are not a formal recommendation by the committee. Rather, we view them as a starting point for journals formulating their own guidelines for coeditors, and expect that they will be adapted to reflect new experiences or journal-specific circumstances.

Good editorial practices:

- General:
  - Evaluate papers on their content; not by who wrote them.
  - The amount of work is the same whether you have a backlog or not, but with a backlog, all decision times are longer. Hence, being very focused on avoiding backlogs or clearing them has a large payoff in terms of average decision times.
  - Share decision letters and other referee reports with reviewers so that reviewers can learn from observing the editorial process and from the other reports. Ideally, continue doing so for rounds in which the reviewer is no longer consulted.
- When to accept manuscripts assigned to you by the editor-in-chief:
  - Don't handle manuscripts for which you have a conflict of interest according to the journal's conflict-of-interest policy.
  - Ask the editor-in-chief to reassign the manuscript if your handling could lead to the appearance of a conflict of interest or if you otherwise feel uncomfortable handling the paper because of your interactions with the authors or the paper's topic.
- Desk rejections:
  - Do them quickly.
  - Write what you appreciate about the paper. This should be specific to the paper.
  - Give an honest and clear reason why the paper was rejected without sounding harsh. The reason should be sufficiently specific that it does not apply to almost any rejected paper.
  - If you have time, offer some pointers for possible improvement.
  - Give suggestions for alternative outlets if you feel confident about your suggestions but do not make suggestions simply because it makes the rejection feel kinder. It is not helpful to authors to suggest that they send their paper somewhere you expect it to be rejected.
- Referee selection:
  - Avoid reviewers who have a conflict of interest.
  - Look up the reviewer's history. If a reviewer is habitually late, don't invite them or invite an extra reviewer so that you can give up on them if they don't deliver.
  - Aim for a diverse reviewer pool so that among the referees there is expertise to evaluate all important aspects of the paper.
  - Aim to have some reviewers who are in the same broad literature but who have not written on the specific topic (and ideally are not cited). This helps guard against a literature becoming too insular.
  - Try to expand the pool of people who sometimes review for your journal by "trying out" reviewers who have not (or hardly) reviewed for your journal before. Perhaps invite an extra reviewer just in case the new reviewer's report is not informative.
  - Avoid overtaxing certain reviewers (this also helps with creating a broader reviewer pool).
  - Be mindful of the number of reviewers that are invited. Each additional invitation imposes a time and effort cost on an additional reviewer. Also, a revision that

needs to address suggestions from more reviewers imposes extra work on the authors (and synthesizing more reports imposes extra work on the editor). On the other hand, having multiple reviewers can speed things up if you are prepared to reach a decision before all reports come in and let the remaining reviewers off the hook.

- Be proactive on late reviewers:
    - Follow-up with late reviewers with a personal email.
    - Unless a late reviewer promises to deliver a report within a week or two after the personal email, and then delivers a report, cut your losses: give up on the reviewer and either decide on the manuscript without the reviewer (if the other reports are sufficiently informative) or invite a trusted new reviewer (explaining the existing delay and requesting a timely report).
- When referee reports have arrived:
    - Screen referee reports for mean or unprofessional language. If that happens, ask the reviewer to adjust the language. If they don't, either say in your decision letter that you thought the language was not professional and you've discounted the report accordingly, or in extreme cases, say in your letter that you received a report that was so unprofessional that you discarded it.
    - Weigh the advice of reviewers by the quality of their arguments and their expertise on the topic; don't just go with the majority advice. The referee's arguments should convince *you* that the decision you are taking is correct.
    - Be mindful of the mechanism described in Ellison (2002b): If reviewers think that their own work is better than it really is (consistent with overconfidence bias that is well documented in the psychology literature), then reviewers would try to hold authors to a higher standard than the journal actually applies because they mistakenly believe that a higher standard was applied to their own papers. As a result, publication standards will rise over time, as observed in economics (Ellison, 2002a and Card and DellaVigna, 2013). To counteract this mechanism, coeditors need to counteract the tendency of reviewers to be overly critical and too demanding.
    - Be open-minded about new approaches, papers written in a different style, or papers that are a bit outside the box. Science progresses through innovations and many innovations may not fit the mold of typical papers. The literature can decide whether the innovation turned out to be valuable. In a similar vein, Akerlof (2020) warns against the sins of omission due to economics displaying a "hardness bias." Heckman and Moktan (2020) describe how the excessive value placed on so-called top-5 publications limits creativity, reduces entry into the profession of creative out-of-network scholars, and promotes "network-referential-citation circles" that screen out new entrants with "oddball" ideas. Coeditors should try to limit the effects of these professional biases.
- In rejection letters:
    - Write what you appreciate about the paper.
    - Give an honest and clear reason why the paper was rejected without sounding harsh.

    - If you have time, offer some pointers for possible improvement.
    - Give suggestions for alternative outlets if you feel confident about your suggestions.
    - Own your decision. The referees can have convinced you, but ultimately you, the editor, decide.
- In revise-and-resubmit letters:
    - Spell out the key revisions that the author needs to make for the paper to be published, synthesizing your own feedback with that of the reviewers.
    - Make sure the revisions are doable and do not involve an excessive amount of work.
    - Identify reviewer requests that do not need to be implemented. Protect authors from idiosyncratic requests of reviewers and make sure your own requests are not idiosyncratic.
    - Give a due date but say that you can extend it if it would impose hardship.
    - Tell the authors that they can contact you if they have questions about how to handle the revision.
    - If the paper has references to any of your own work, but your own work only seems tangentially related, ask the authors to remove those references. This may help against the perception among some that there is value to strategically citing the editor.
    - Try to be clear about whether you intend to return a revision to one or more of the reviewers and whether there is a serious risk of rejection after revision.
- Limit the number of rounds of revisions:
    - If a revision comes back, the authors made a good faith effort, and nothing truly major or unexpected shows up, the paper should be close to acceptance. Perhaps, one more round of revisions focusing on presentation and readability is needed.
    - Consider whether you can evaluate whether the authors implemented the requested revisions without the help of reviewers. If so, you may not need to involve reviewers when the revision is submitted, saving time and reviewer effort.
    - Don’t invite new reviewers for revised papers other than in truly exceptional circumstances.
    - Limit new requests from reviewers for revised papers.
    - For papers that were revised a second (or higher) time, which typically involve minor revisions focused on presentation, do not involve reviewers anymore.

6.6 Guidance and Recognition for Reviewers

The profession is fortunate to have strong professional norms about reviewing papers. The committee believes most economists take refereeing commitments very seriously and spend considerable time and effort producing timely, thoughtful, and impartial reports. Still, this is not universally the case, and survey respondents expressed concerns about the quality and timeliness of some reports. Respondents suggested timeliness and quality could improve if reviewers had better guidance and if they received more recognition for their work.

A. Guidance for Reviewers

Recommendation A2 already asks reviewers to clearly separate essential points that are critical for publication from optional suggestions. The committee believes that reviewers would benefit from additional guidance from journals, which could be journal- or paper-specific. Such advice may be along the lines of:

> Start with a short summary of the paper's contribution (typically one or two paragraphs) and follow it with your overall evaluation of the paper (one paragraph). Give a limited number of essential comments, typically not more than three or four. If you have a large number of essential revisions, the paper should probably be revised before being submitted elsewhere. In such cases, it is kind of you to be helpful to the author, but not necessary. Suggest additional revisions that the authors may consider but are not essential. Typically, your report will be 2-3 pages if you think the coeditor might offer a "revise and resubmit" but could be shorter if the paper is in very good shape. Avoid becoming an unnamed coauthor! For a clear rejection, a single page can succinctly and politely provide one or two key reasons for your recommendation. You can help authors by honestly suggesting journals at which the paper might find a home after revision. Be kind but honest; there is never a reason to be mean.

Strong guidance from the coeditor may be particularly helpful when a coeditor asks a reviewer to evaluate a revised submission. For example, a coeditor may write: "I don't need you to review the entire paper, but I would like your advice on whether the authors have addressed your concern about instrument validity." By limiting the scope of the review, the coeditor can lighten the reviewer's load and speed up the review process.

Recommendation C9: Providing guidance to reviewers

We recommend that journals provide general guidance to reviewers regarding the structure, content, and length of a good review and, when appropriate, coeditors provide guidance about particular issues or sections for the reviewer's focus. Having this guidance, or a link to it, in the reviewer invitation email would be helpful. Coeditors should also be encouraged to provide ex post feedback concerning reports that are unprofessional in tone and, if necessary, call out such reports in their decision letters.

Making reviewers ex post aware of the content of other reports on the same paper could also be helpful for several reasons. First, it makes reviewers aware of the views of other scholars on the same paper. Second, it allows them to anticipate the content of revisions. Third, it can serve to create benchmarks for what constitutes excellent reports that reviewers can learn from. Figure 22 in CDEGT2022 reveals strong support among economists for sharing the decision letter and reports with all reviewers. Coeditors should also provide feedback to reviewers regarding reports that are unprofessional in tone. Such reports disproportionally affect junior authors and create an environment that is not welcoming to new researchers. Coeditors may ask reviewers to adjust the tone so that it is professional and polite. If they don't ask reviewers to adjust the tone or reviewers don't not comply, coeditors should also comment in their decision letters on how they used and interpreted such reports.

Recommendation C10: Sharing decision letter and reports with all reviewers
We recommend that journals share decision letters and all referee reports with all the reviewers of a paper unless there is a good reason for an exception in a specific case.

On an ad hoc basis, some coeditors may also want to reach out to individual reviewers to thank or praise them for exceptionally thoughtful reports or, in exceptional cases, to give feedback on aspects of a report (e.g., to remove language that may come across as mean or unprofessional). However, doing so on a systematic basis would add an unreasonable burden on coeditors. Moreover, coeditors are grateful for the service that reviewers provide, and it is hard to convey gratitude while also conveying what could be improved. Overall, the committee did not see good options for giving reviewers systematic feedback on the quality of their reviews on an individualized basis, either qualitatively or through some form of rating system. Many journals may have the capacity to provide feedback to reviewers on the timeliness of their reviews and the number of reviews they completed, relative to other reviewers for that journal. We believe such feedback would be helpful for reviewers, but we leave it to journals to decide whether to provide it.

B. Recognition for Reviewers
While reviewers generally provide reviews out of a sense of professional duty, providing recognition to reviewers can help motivate them. We see two ways to strengthen recognition of reviewers.

First, journals can offer awards recognizing excellence in reviewing. Several journals have already adopted such awards. The selection of awardees is typically done by editors on the basis of reviewer timeliness and report quality. These awards are particularly helpful to junior members of the profession who can benefit from the visibility they confer and the consequent career rewards. The existence of such awards, especially if they become more widespread among journals, can in addition provide *ex ante* incentives for timeliness and thoughtfulness on the part of reviewers.

Recommendation C11: Awards for excellence in reviewing
We recommend that journals institute an annual award to recognize outstanding reviewers.

Second, reviewers could get better recognition for their service if they could verifiably communicate how much they review in a given year and for which journals. CDEGT2022 recommend (in their section 3.2.4.2) that economics journals use the nonprofit ORCID system to relay who reviewed for them and how often in a given year. Collecting reviewer ORCID iDs would go hand in hand with our recommendation A11 to also collect author ORCID iDs upon submission. Journals with blinded reviews, as is the case for virtually all economics journals, would *not* relay to ORCID which paper the reviewer refereed, thereby maintaining reviewer anonymity. The reviewer's refereeing activity (year and journal, but not which paper) would appear in the peer review section of their ORCID record. The reviewer can decide, by going to their ORCID page, whether to make their ORCID peer review record public or keep it private.

Recommendation C12: Transmission of review completion to ORCID
We recommend that journals collect ORCID iDs for reviewers who wish to have their completed reviews acknowledged in ORCID and transmit the completion of each review to ORCID, along with the year in which the review was completed, but without identifying the paper that was reviewed.

7. What Profession-Wide Actions Can Help?

Action by the four Societies/Associations and other organizations in the discipline can also help to improve the publication process in economics. Such action can take the form of information provision to authors and referees—we give examples below, distinguishing between information that would be complementary to the recommendations put forward, information that would help level the playing field for authors and so reduce the perceived returns to networks or being an insider, and other kinds of action.

7.1 Information Provision Complementary to Our Recommendations

A number of recommendations can be supported by the provision of information to authors and reviewers. This might be best facilitated by webinars that allow multiple points of view and outlines of best practices.

For example, it can be useful for junior authors to hear alternative viewpoints on (or examples of) how to decide on author order (Recommendation A5), the content of contribution statements (Recommendation A7), responsibility statements (Recommendation A8), and what constitutes conflicts of interest (Recommendations B5 and B6).

A natural counterpart to best practices for editors (discussed under Recommendation C8) is for Associations to also actively disseminate information on best practices for reviewers (discussed under Recommendation C9). This could be in relation to the structure of reports (distinguishing between essential points and those that are optional, see Recommendation A2), using constructive and professional language in reports, and sharing information on the process through which reviewing awards are decided and which reports editors view as the most useful.

7.2 Leveling the Playing Field

Some of the comments we received suggest that many economists would benefit from advice about the submission and review process at economics journals. Our advice below does not necessarily apply to interdisciplinary journals or those in other fields. We all serve or have served as editor or coeditor, and many of us have participated in editor panels where participants have expressed divergent views on the issues below. Therefore, although our comments distill the collective views of a set of experienced editors, there are many caveats to any advice, and hence having the issues openly discussed would be beneficial.

Choosing a journal. Take time to make sure that your paper is within the journal's remit, for example, check whether the journal publishes comments or review papers if you plan to submit one. What the journal has published previously is a good guide to the topics it is likely to consider within its remit—although journal editors and their tastes change. However, if a current editor published an article on a topic close to yours, they may feel that the earlier paper raises the standard for another paper on the same topic. Also, term limits of editors are there partly to limit an individual editor's tastes. Choosing a journal editor is as important as choosing a journal—with most journals having grown in the size of their editorial teams, there is a lot of heterogeneity in the views of editors within a journal. Selecting an editor in the field increases the chances that your paper will be assigned to knowledgeable referees, but it might also raise the bar of what is considered a potential contribution to the field.

Cover letters. Some journals discourage cover letters. Even when a journal expects a cover letter, keep it short unless you have something substantive to say. You do not have to summarize your paper. If the journal's submission website does not have space for the following, you should address them in a cover letter:

1. Exemption from some policy: Most commonly, you may need a waiver from a policy requiring you to post your data. Some journals will have a place to make your request as part of the submission process. If not, it needs to be part of your cover letter.

2. A preference for a particular coeditor: Provided you do not ask for someone with a conflict of interest, expressing your preference is unobjectionable. You will not necessarily get your choice; your preferred editor may have a backlog, or the editor may desk reject without sending your paper to a coeditor. Still, most journals will try to be responsive to your request.

3. Request that the paper not be sent to a particular reviewer: This is relatively rare, but you can tell the editor if you have a personal conflict with someone. If you have an intellectual conflict, the editor may still want a review from someone "on the other side." At the very least, you can alert the editor that there are two sides so that they can be aware of the issue when selecting reviewers. According to Figure 14 in CDEGT2022, about 80% of respondents have a neutral or favorable view of journals taking into account author suggestions for which reviewers should not be invited.

4. Suggest a type of reviewer: This is extremely rare. Very occasionally, you may be concerned that the editor will seek reviewers in one area when the real audience is in another area. Outside economics, some journals let you suggest individual reviewers. We do not know of any economics journals that permit this.

Dealing with rejection. All of us get upset when reading unfavorable reviews of our work. In most cases it is best to fume briefly and move on. There are many journals, but the high rates of rejection at most journals make it imperative for you to perfect your paper before submission and not just rely on a "lucky draw."

Deciding whether to appeal a decision. It is common for reviewers to make some mistakes, but in most cases, for appeals to be successful, the errors must be so egregious that correcting them would likely change the editor's decision. For example, the fact that a reviewer complained that you should have clustered your standard errors when, as stated in the text, you did, is not grounds for an appeal unless that really is why the editor rejected your paper. Before sending in an appeal, it is advisable to consult the journal's policy on appeals and it is wise to wait a few days (to "cool off") and ask advice from a mentor, friend, or colleague. Formal appeals are very infrequent (< 0.5% of decisions are appealed) and successful ones are even rarer.

While appeals often have limited value, most editors will welcome polite feedback. If you believe a reviewer or coeditor did a poor job, a nicely written email to the editor that accepts the decision but explains your concerns may be in order.

Communicating with the editor during a revision. Ideally, the editor's decision letter gives you clear guidance about how to revise your paper. Unfortunately, there will be times when one reviewer tells you to cut section three while another says to expand it, and the editor simply asks you to address all reviewers' comments. It is perfectly reasonable to write to the editor for clarification if you do so quickly after a decision letter is sent (editors will have a hard time recalling the specifics of your paper after a few weeks). The editor is not your thesis advisor; they generally do not want to go back and forth with you. Still, most editors will be happy to give some advice if it will help produce a successful revision or forestall an extra round of revision.

7.3 Other Profession-wide Discussions

The publishing process in economics could also benefit from associations facilitating open discussions around emerging topics. The most obvious of these is the use of AI—while in our report we have focused attention on the role of AI in the review process (Recommendation A3), the committee does not have the expertise to discuss the impact AI can have on the generation of research. This might well have further implications for author declarations at the point of submission (Recommendation A4) and the reviewing process. It would be useful for Associations to foster open debates about the uses of AI (both positive and negative) in the production, dissemination, and reviewing of research.

In light of Recommendation A6, associations and research networks in economics can ensure that they enable the use of the ⓡ symbol in relationship to the order of authors in non-journal communications (such as advertising working paper series). The same applies to journal publishers enabling the use of such symbols in both online and offline publications (so, moving away from the current practice where some publishers only print the first author's surname online).

Open discussions organized by associations can be used to reiterate the discouragement of terminology that promotes artificial distinctions between journals (Recommendation A10), to help establish norms of adherence to sending manuscripts out for review with disclosing author

identities (Recommendation C1), or sharing good practices for preparing replication data and code while research projects are in progress.

Finally, many of the additional issues raised (though not endorsed by this committee)—and summarized in Appendix C—could also form the basis of open discussion within associations. Similarly, CDEGT2022 and Siemroth (2024) offer additional suggestions that associations may wish to discuss.

8. References

Below is a list of papers that the committee consulted. Not all of these papers are cited in the report itself. Additional useful references, organized by topic, are available on the website set up by CDEGT2022.

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Appendices

Appendix A. Background of Formation of the Committee

Some editors of the *American Economic Review*, the *Quarterly Journal of Economics*, *Econometrica*, the *Journal of Political Economy* and the *Review of Economic Studies* exchanged ideas about relatively minor changes where harmonization would be useful, such as the way authors are cited. During the course of this exchange, the editors realized that an initiative from these five journals would go counter to their desire to reduce the "tyranny" of these journals. Subsequently, editors of the *Economic Journal,* the *Review of Economics and Statistics*, and the *Journal of the European Economic Association* were also involved in the exchange of ideas. During the subsequent discussions, three important points came up. First, there are larger issues with the publication process than those (such as citation format) that led to the initial discussions. Second, any recommendations for larger changes should be informed by the views and suggestions of the broader community of economists who publish in or read academic economics journals. Third, recommendations for larger changes should come from a committee that is appointed by professional organizations with elected boards. The editors then wrote the following letter in August 2023 to the presidents of the American Economic Association, the European Economic Association, and the Econometric Society with the request that these organizations set up such a committee.

> *To: Susan Athey (AEA); Maristella Botticini (EEA); Rose Matzkin (ES)*
> *Dear Susan, Maristella, and Rosa,*
>
> *Many innovations in the publications process happen when individual journals innovate, and other journals adopt those innovations because they recognize the benefit of the innovation or face competitive pressure to follow suit. This process will undoubtedly continue.*
>
> *However, there are also innovations that occur more easily when there is coordination. As editors of various leading journals, we recently started discussing coordinating on various innovations including possible ways for authors to allocate credit and how to cite papers with many authors in a way that does not implicitly give most of the credit to the first author for papers where authors are listed in alphabetical order.*
>
> *We also discussed the importance of conflict of interest rules to make the profession more inclusive, transparent and equitable. Journal editors are important gatekeepers of this profession, as they ultimately decide what and who gets published in their journals. Different journals have different rules concerning editorial conflict of interest. Some journals have no requirement, while others have strict rules. While some variation may be natural and even desirable, advisory guidelines about editorial conflict of interest rules could be helpful to foster inclusivity, transparency and equity in the publication process.*

As we were discussing possible innovations, we felt that the AEA, EEA, and ES jointly have much more legitimacy in trying to set norms for the profession than a select group of appointed journal editors. Moreover, the AEA, EEA and ES have the infrastructure to solicit input from their membership on innovations that professional economists would like to see.

We therefore would appreciate the AEA, EEA, and ES forming a joint committee charged with setting advisory guidelines for economics journals. The guidelines should not describe any dimensions along which the journal compete, such as subscription fees, page fees, referee payments, or decision times. These guidelines are not mandatory for editors to follow but would serve as a coordinating device. In addition, journals would feel pressure to conform to these guidelines or explain their reason for deviating.

Please find below a list with possible topics on which this committee could give guidance. Of course, the committee could add to the list of topics.

Thank you,
Ruben, Ray, Guido, Larry, Francesco, Erzo, Magne, and Romain

Ruben Enikolopov, Chair of the Review of Economics Studies editorial board
Ray Fisman, Co-Chair of the Review of Economics and Statistics editorial board
Guido Imbens
Lawrence Katz, Editor of the Quarterly Journal of Economics
Francesco Lippi, Editor in Chief of the Economic Journal
Erzo F.P. Luttmer, Editor of the American Economic Review
Magne Mogstad, Editor of the Journal of Political Economy
Romain Wacziarg, Managing Editor of the JEEA

List of possible topics on which the committee could give guidance (just a start)

1. Style for references so that the first author doesn't get disproportionate credit
The current norm for most economics journals is to list all the authors in the first reference (unless there are very many) and then to refer to the paper by the first author "et al." if there are more than two authors. Due to the increasing number of authors, papers are getting increasingly known by the name of the first author, thereby exacerbating the advantage for people whose name comes first in the alphabet given that this is the predominant norm for author order in economics.

An alternative on which journals could coordinate would be to list all names (up to some reasonably large number, 8 or so) in the first reference to a paper and then to use the abbreviation of first letters of the authors' last names and two-digit year for subsequent references to that paper. E.g., "Botticini, Eckstein, and Vaturi, 2019" for the first reference and "BEV19" for subsequent references. Of course, the committee could think of a better solution than this suggestion.

2. A way for authors to describe their contributions to an article and to possibly allocate credit

Some other professions, e.g., medicine, explicitly list each author's contributions. Perhaps the current model in economics of not spelling out credit is optimal, but we thought it would be useful for the committee to consider the benefits and drawbacks of alternative models, and possibly propose an alternative. It would also be useful to clarify the meaning (or lack thereof) of "corresponding author" in terms of their contribution to the paper, and give guidance to journals on whether they should identify the corresponding author.

3. Guidelines about authorship

Authorship gives credit and implies accountability for published work, so there are academic, social and financial implications.

It is very important to make sure people who have contributed to a paper are given credit as authors. And also that people who are recognized as authors, understand their responsibility and accountability for what is being published.

The production function in economics has materially changed over the past few decades. Our profession is becoming increasingly specialized. Research is often done in larger teams, sometimes with the assistance of one or more (part or full time) research assistants. Thus, it would be useful to establish clearer advisory guidelines for what it means to have contributed to a paper and, as a result, about who should (not) be credited and accountable as an author.

4. Reducing the disproportionate credit going to "top-5" publications

The very term "top-5" and general agreement within the profession of which journals constitute the so-called top-5 creates an artificial discontinuity in the credit awarded to publications in the so-called top-5 relative to close competitor journals, whereas in fact the quality differences are more gradual and there are large overlaps in quality between papers published in so-called top-5 journals and close competitor journals. Clear guidance that discourages the use of the term "top-5" would reduce this artificial discontinuity, thereby reducing pressure among (junior) authors to publish in so-called top-5 journals, the resulting stress for authors, and excessive submissions to these journals.

5. Minimal conflict-of-interest rules

Setting a minimal set of conflict-of-interest rules together with the publication of a list of journals that are in compliance with them (and those that are not) would put additional pressure on journals to adopt conflict-of-interest rules that meet or exceed this minimum.

The presidents of the American Economic Association, the European Economic Association, and the Econometric Society, later joined by the Royal Economic Society, formed an ad hoc

committee consisting of Joseph Altonji, Guido Imbens, Kevin Lang, Erzo Luttmer, Imran Rasul, Stefanie Stantcheva, and Romain Wacziarg with the following charge:

> Thank you so much for being willing to serve on the joint AEA-EEA-ES-RES Committee on Improvements to the Publication Process in Economics. We hope that your discussions will cover, but not be limited to, the topics listed as examples below. **However, please be mindful that it would be inappropriate to discuss or attempt to coordinate on areas where journals compete, such as prices or length of time in review or to publication.**

The list of topics mentioned in the charge refers to the same list as the five topics in the letter of the initial group of editors, mentioned above. The committee met multiple times over zoom starting in February 2024.

Appendix B. Initial List of Harmonized Information to be Reported

This list may be refined or amended by the Journal Information Center.

A. Harmonized performance metrics

The number of performance metrics is kept limited on purpose to limit the burden on journals. Journals are welcome to publish additional performance metrics as they see fit as long as they include the following set of metrics.

1. Number of first submissions in the previous calendar year. The number includes both the first submissions received directly and those received via the transfer mechanism.
2. Coeditor workload in the previous calendar year
    - Calculated as the total number of submissions in the previous calendar year divided by the average of number of coeditors in function during the past year.
3. Fraction of first submissions in the previous calendar year that were desk rejected
    - Calculated as the fraction of submissions received in the previous calendar year that were rejected without referee reports prior to Feb 1st of the current calendar year. There is censoring on this variable to the extent that there are desk rejections that took longer than one month to issue, but I suspect this is rare and even if it happens will not bias this statistic much.
4. Mean “pareto-penalized mean decision lag” on decision letters with referee reports of first submissions received in the first half of the previous calendar year.
    - A = the mean decision time in days of first submissions received between Jan 1 and June 30th of the previous calendar year that received a decision with referee reports by December 31st of the previous calendar year.
    - B = two times the average number of days since submission of all first submissions submitted prior to July 1st of the previous calendar year that have not received a decision by Dec 31st of the previous calendar year.
    - The pareto-penalized mean decision lag is the weighted average of A and B, weighted by the number of manuscripts in each category.

- The properties and rationale of this measure of decision times is explained in more detail below.

5. Stock of manuscripts that have been waiting for more than 6 months for a decision.
    - This is the total number of manuscripts (whether first submissions or higher-round submissions) that as of December 31st of the previous calendar year had been waiting for more than 6 months for a decision (so had been submitted prior to July 1st of that calendar year). This is a metric of backlogs of any form at the journal.
6. Number of articles published in the previous calendar year. The number includes both articles submitted directly and articles submitted via the transfer mechanism.
    - Note: by dividing this number by the number of first submissions, readers can get a sense of the acceptance rate under the assumption that the journal is in equilibrium. By having the reader do the division themselves (rather than provide the ratio), readers clearly see that the numerator and denominator refer to different time periods. Given lags between submission and acceptance, calculating an acceptance rate for manuscripts submitted in a given time period either results in a statistic that is out of date or runs into censoring issues.
7. Mean number of days between initial submission and acceptance of the articles published in the previous calendar year. This metric captures an overall sense of the arduousness of the revision process, whether it is due to long decision lags or many rounds of revisions. The sample includes both articles submitted directly and articles submitted via the transfer mechanism.
8. The number of first submissions the journal received via the transfer mechanism in the previous calendar year.
9. The number of articles published in the previous calendar year that were submitted via the transfer mechanism.
10. The mean time between initial submission and publication for articles published in the previous calendar year and submitted via the transfer mechanism.

Rationale and Properties of the Pareto-Penalized Decision Lag

- Statistics that are sampled on decisions made during a given timeframe don't suffer from censoring but are subject to manipulation. E.g., a journal that never issues a decision on a manuscript that has gone longer than 2 months would look very fast even if they build up a huge backlog of manuscripts without decisions.
- Statistics that are sampled on the date of submission suffer from censoring: long decision lags are not yet observed. Without a censoring correction, decision times are biased downwards. Kaplan-Meier can correct for this, but it is relatively complicated to calculate and does not deal well with long right tails. Censoring can be mitigated by looking at submissions from a long time ago, but then the statistic becomes less relevant for the current period.
- The "pareto-penalized mean decision lag" is a way of dealing with censoring by assuming censored observations follow a **Pareto distribution** with parameter 2. By construction, censoring can only happen beyond 6 months (because we only look at submissions submitted prior to July 1st that don't have a decision by Dec 31st). If in truth

the tail is thinner than a pareto parameter of 2 (which seems likely), it effectively **penalizes** decision lags beyond 6 months. Given that such long decision lags are typically perceived as especially costly, such a penalty creates an informative statistic. Recall that a property of the pareto distribution with parameter 2 is that E[X | X>T] = 2T.

- A second way in which very long lags are penalized is that all submissions submitted prior to July 1st and without a decision by December 31st are included in the calculation, not just the ones submitted on or after Jan. 1st. This puts an extra penalty on submissions without decisions that have gone over one year.
- The pareto-penalized mean decision lag has a number of desirable properties:
    - It is equal to the mean decision lag of decision letters with reports if a journal issues a decision on all first submissions sent out for review within 6 months. (I.e., censoring is not an issue in this case).
    - It would be equal to the mean decision lag of decisions with reports if the distribution of decision times beyond month 6 follows a Pareto tail with parameter 2 if we had excluded manuscripts without decisions submitted before Jan 1st from the calculation of component **B** (in step 3). The inclusion of those earlier manuscripts without a decision means that, if the distribution is pareto with parameter 2, the statistic is equal to the mean decision lag plus a penalty for manuscripts going for more than one year without a decision.
    - Issuing a decision will always reduce the pareto-penalized mean decision time. So, journals have no incentive to ever delay a decision to improve this statistic.
    - Journals are extra incentivized to issue decisions on papers that have gone over 6 months and are undecided by Dec 31st because if they are undecided, their contribution to the decision lag is twice as long as when they are decided (because of the Pareto tail parameter of 2).
- One drawback of this measure is that it is based on submissions during the first half of a year rather than submissions during the entire year. This was necessary to eliminate the need for a censoring correction for manuscripts that are decided within 6 months. I think most journals have sufficient submissions that sampling noise is not much more of an issue if statistics are based on half a year than a full year. Going back further in time makes the statistics more outdated and it is no longer possible to say they apply to a given calendar year. Another possible drawback is that the Pareto tail is calculated based on undecided manuscripts on Dec 31st, which gives extra strong incentives to get manuscripts decided by that particular date.

B. Harmonized reporting of editorial policies

Journals should report which of the committee's recommendations they decided to adopt. For recommendations that ask journals to consider something, such as concealing author identities from reviewers or having blinded desk rejections, the journals should report whether they adopted what they were asked to consider.

Appendix C. Additional Open-Ended Comments

The survey's open-ended comments contained a wealth of ideas to improve the publication process. In addition, CDEGT2022 offer a list of 164 suggestions to improve the publication process (their Table A1). The committee, however, could not adopt some of these suggestions because they touch on competitive aspects of the publication process, which fall outside the scope of the committee. In addition, the committee did not want to recommend measures that might work well for some journals but not for others. Nor did it want to recommend measures for which it could not predict their effect with sufficient confidence or for which it was worried about unintended consequences. Finally, the committee thought that some of the comments were not practically feasible or would have negative consequences that outweighed their purported benefits. Still, the committee decided to share these comments, even if we cannot or do not want to endorse them. Individual journals may wish to adopt some of these suggestions, and the profession could learn from their experience. In addition, some of these comments may form the basis for brainstorming about other measures to improve the publication process. The committee therefore decided to relay many suggestions from the open-ended comments, without recommending or endorsing them. Below are comments that were brought to the committee's attention in the open-ended comments or through other channels, but that the committee did not endorse.

Comments that were *not* endorsed by the committee:

On systemic changes

1. Increase acceptance rates, especially at top journals.
2. Create more publication venues, such as more quality journals, could possibly alleviate the excess competition for limited space in quality journals. Obviously, journals gain their reputation over time, so it is not easy to create a quality journal from scratch but some professional societies or institutions could use their reputation to launch/sponsor new journals. Alternatively, economic societies could convince institutions that journals that used to be of limited quality have become higher quality outlets over time (e.g. by publishing up-to-date rankings, recognizing works in these journals more frequently, etc.).
3. We need journals that do the following: 1) short papers (<20 pages) with a low bar for entry -- coeditor just picking stuff without peer review 2) opportunities for (lightly screened) responses. This allows rapid, public, dialogue. That will push us forwards.
4. As in science, make the papers shorter, accept more papers, and let history decide which paper is important. The current peer review system is out of date.
5. Have a secure repository of referee reports of rejected papers. After a paper is rejected, reliance on such a repository could significantly speed up the process at the next journal.
6. Have journals bid on manuscripts using this mechanism.
7. Allow simultaneous submission to multiple journals.
8. Experiment or cooperate with alternative mechanisms for evaluating papers, such as the SQARE initiative (https://sqare.org/)

On comments and replication

9. Publish more comments or negative replications that challenge published results
10. Have an option to publish ongoing commentary or review electronically (though subject to minimal editorial oversight), such as the model used by Science. Science describes e-letters as follows: “eLetters is a forum for ongoing peer review. eLetters are not edited, proofread, or indexed, but they are screened. eLetters should provide substantive and scholarly commentary on the article. Embedded figures cannot be submitted, and we discourage the use of figures within eLetters in general. If a figure is essential, please include a link to the figure within the text of the eLetter.”
11. All journals should offer a comment section and a replication section. Successful replications in other journals should be linked to the original paper.

On appendices

12. Specify limits on paper or appendix length
13. Require that papers make no references to the appendix longer than one sentence except in the discussion section, which could say something like: "In the appendix, I conduct the following exercises, which I briefly summarize below," which could be followed by a few paragraphs. This has the advantage of making the reading of the paper linear. Furthermore, being at the end, it also has the advantage of helping the reader figure out if the appendix is worth looking at. One sentence references like "I discuss how I fuzzy merge addresses in the two data sets in Appendix A" would still be okay. The alternative option would be an appendectomy.

On transparency and information provision

14. For published articles, publish referee reports and author responses online without identifying the reviewer
15. For published articles, identify the reviewers. Some journals in sciences (e.g., the very respectable *Nature Genetics*) publish the referee reports alongside the paper, indicating the name of the reviewers. I think that this practice improves the publication process a lot. Too many referees make outrageous claims behind the veil of anonymity. They would be more careful, less discriminatory, and take more responsibility toward authors if not anonymous. The problem of "exchanging favors" when signing referee reports can be avoided by reducing the number of referees per paper and forbidding cross-reviews.
16. Allow the reviewers to jointly write the limitations section for an article accepted for publication (with a word limit; with an opportunity for authors to react).
17. For published articles, post the decision letters online.
18. For published articles, post the initial version of the paper submitted to the journal online so that changes that occurred during the review process are public.
19. For published articles, post online a few bullet points describing the key changes made to the paper during the revision process. For example, the author could provide these bullet points, subject to editorial oversight.
20. Each published paper should include an estimate of the resources used for the project. Of course it will always be hard to verify it but at least know: a) total amount spent and/or b) number of RAs involved (for how many months) will help to put things in perspective.

Some people simply play in different leagues with resources that are not even comparable with the majority of researchers. It would help to put things in perspective.

21. Authors of papers that rely on primary data and experiments should be required to submit their ethics protocols as part of the review process and publish it as part of the supplemental appendices. There's a non-trivial number of studies that I suspect deviate from their original ethics protocol.
22. Survey reviewers and publish the results of this survey.

On restrictions on reports and revisions

23. Have a limit on the maximum number of rounds of revisions
24. Have a limit for the the number of reviewers on a manuscript
25. Put a length limitation on referee reports
26. Ask reviewers to limit the number of substantive points that a manuscript needs to address.
27. Normalize reviews that don't recommend changes. This would reduce the degree to which reviewers feel the need to find *something* to demand of the authors. Instead it should be normalized for reviewers to explain why they think the article is already complete and does a good job of adding something new to joint knowledge in a credible way. Reviewers are not coauthors.
28. A major problem with the review process in economics is the extensive, overly detailed and thoughtless comments by reviewers and the extensive responses expected by authors. Unlike other fields, our reviewers almost act like coauthors and editors are almost completely deferential to reviewers. I believe we need to do two things about this:
    a. First, I think we need very clear guidelines for reviewers about what is and is not an appropriate thing to request in a revision. Too often, I see authors struggling to address every single random thought a reviewer might've had about their paper . This is completely inappropriate and a terrible waste of time, and fails to improve the paper's quality. On the other hand, some review comments are absolutely critical to making the paper as good as it can be.
    b. This leads me to the second recommendation which is to have stronger editors. I don't know how to do this, but I believe the norm should be changed so that instead of deferring completely to reviewers an editor should use their judgment and take the time and effort to be clear to the author about what is and is not needed for revision to be successful. This clearly requires a lot more effort on the part of the editor. But more importantly, it requires that editors have the power to do this. Editors seem to be so terrified of offending their reviewers that they will not take a stand on some of the stupid things reviewers ask for. Review suggestions can really improve a paper. Or not.
29. Have shorter deadlines for reviewers. Most people have a backlog of reports they need to do, and they do each report shortly before the deadline, no matter how long this deadline is. Most people write some reports every month. Therefore, imposing a much tighter deadline will only have a one-time negative effect while people clear out their backlog of old referee reports that still had a long deadline while at the same time having

to deal with new reports with a shorter deadline. After that, everyone will write the exact same number of reports each month, but everyone will get much faster decisions.

On reviewer incentives

30. Reward reviewers, with recognition or monetary incentives
31. Have a system that keeps track of reviewer contributions across journals
32. I think there should be more professional rewards for good referee reports. E.g. some kind of points or scoring system showing how many / how helpful referee reports one has written.
33. Have a review scoring system (by authors and coeditors) on reviewers such that bad-faith reviewers can eventually get singled out and not asked again to review for the same journal for some time? Categories to consider may be: "do you think the reviewer read your paper?", "did you find the reviewer's comments helpful?", "do you think the reviewer's comments, if implemented, would help improve your paper?".
34. Let reviewers build up "credits" or "demerits". Slow reviewers will get penalized when their own papers are submitted. Fast reviewers will get rewarded by being prioritized in the editor's queue. Try to align reviewer incentives with those same reviewers want as authors. This system could be shared across journals too. This would also be one way to actually reward (fast, and high quality) reviewers with something tangible.

On guidance and feedback for reviewers

35. Provide more guidance to reviewers
36. There should be more feedback from editors and maybe even authors to referees regarding the quality of their reports. I think most people would like to write better / more helpful reports but they never get told by anybody how helpful their reports were (or not). I understand this is tricky to implement but maybe could be achieved via a centralized website where feedback is aggregated and sent to referees after they have done e.g. 5 reports (for different journals).
37. Each journal should force first-time referees to sit through a short online tutorial on how to referee. This tutorial should ideally be designed by a joint team across the bodies that designed the current survey. There is a huge heterogeneity in approaches to refereeing. At no point in my career did I ever receive formal or explicit training on how to act as a referee. I've uncovered my own approach through either introspection or talking to my seniors, which itself comes with history-dependence that might be inefficient.
38. Reviewers should not refer to previous results for rejecting a paper without providing a specific citation to publications of those results. Economics should impose the same standard on reviewers, and coeditors should be obligated to only accept referee reports with appropriate documentation.
39. Consider adopting the instructions that some top psychology journals give to their reviewers:
    a. “Please do not assume that findings from studies that are not preregistered were the result of a detailed a priori plan - that would be unfair to authors who allow us to see their plans by preregistering them. Instead, consider that risk of bias (e.g., resulting from flexibility

in data collection or analysis) is often higher for studies that are not preregistered than studies that are preregistered (even when authors deviate a lot from their preregistration). We do not want to create a situation where authors who preregister have their submissions evaluated more harshly than authors who do not preregister."

b. "We are committed to rewarding authors who appropriately calibrate their claims, including caveats and limitations presented prominently (e.g., in the abstract and conclusions) where appropriate. Please be conscious not to penalize authors for being honest about what their research can and cannot show."

On communication channels

40. Have a mechanism by which authors and reviewers can communicate anonymously through the editorial system (e.g. "a chat feature") to resolve issues on which reviewers might want clarification prior to submitting their report (this is common in other disciplines, including computer science; it may increase the burden on the editors to monitor this communication).
41. An author or supervisor should only be allowed to communicate with editors and coeditors through the messaging system of the journal.

On redress options for authors

42. Make it easier and less dramatic for authors to point out "issues" in referee reports or associate-editor decisions. Sometimes the referee reports are biased and sometimes the referee or associate editor made factual mistakes (beyond reasonable opinion). There should be a professional way to indicate that and ask for a review of the decision.
43. Set up some anonymous reporting system for aggressive and inappropriate referee language and other forms of inappropriate behavior, e.g., having RAs write reports and instructing them to reject to avoid a second round, saying to have re-read the paper and rejecting it because "it has not changed". All of us should be held accountable for such behaviors, there should be deterrents: just like our google scholar shows our number of citations, we should have a public display of the number of cases we were reported for misconduct in the review process. We have introduced many nice novelties in the social sciences in the past (PAPs, RCTs), it's high time we start doing that again.
44. Currently, there is no mechanism to guard against insincere/weak/sloppy referees. Without that, editors often make decisions with less than ideal information about a paper. This simple modification to the way submission process operates may improve the process without imposing undue costs on editors, reviewers, and authors. The key point is there could be an opportunity for authors to respond to referee comments before the editor makes a decision. One (not the only) way to implement this would be as follows.
    a. Editors decide which papers go for review and choose referees.
    b. Referees submit their report.
    c. Authors but not editors get to see the report as soon as a referee submits.
    d. Authors have the opportunity to submit a response letter outlining what they will do in response to the comments of all referees.

e. There can be a word limit on this response letter, and a time limit (say 1 month after all referee reports are in).
f. Once an author submits their response letter, only then the handling editor gets to see the referee reports and the response letter by authors.

45. Have an appeal's process that protects authors against a fear of backlash by editors.
46. If there is an appeals process for any decision, that is news to me. To my knowledge, any kind of appeal is endogenous to the seniority of authors.

On the selection of reviewers, coeditors, and editors

47. There is not enough diversity of referees; a small number of them are overworked while many of them are not solicited. Again, there may be good reasons for this, but there are costs too: these active referees are overworked and the skills of others are not put to use.
48. Use a broader pool of reviewers. Editors tend to rely on a limited set of reviewers that they have used in the past (and with whom they are acquainted and where they have an idea of what the outcome of the reviewing will be). They should be forced to select a more diverse set of reviewers, i.e. not use the same reviewer more than say 2-3 times per year.
49. For coeditor or editor positions there should be nominations or applications.
50. Editors or coeditors choosing their replacement constitutes a conflict of interest.
51. Randomly select coeditors among a subset of those qualified candidates (e.g. tenured faculty that have published in journal within the last 5 years, opt in to the lottery, and were timely reviewers) to limit selection of coeditors within certain professional networks
52. Random allocation of a manuscript to authors who have published previously in the journal and indicated they are willing to contribute (2 persons for coeditor roles, 5 persons for reviewers role) with appointed editors no longer making the acceptance/rejection decision (which is made by 2 coeditors randomly chosen). Coeditors ensure the decision process was well performed (and facilitating a consensual decision when the 2 coeditors disagree; if needed following clearly set criterias, making the final decision).
53. Editorial positions should be paid and there must be accountability (evaluations by all submitters, etc.). Annual reviews should be conducted.
54. There should be accountability for coeditors and reviewers. Coeditors and reviewers involved with paper that turned out to be fake should face professional consequences such as being required to step down or being identified.To prevent a coeditor monopolizing a field, submitters should be able to rate their experience and the group of editors should receive yearly anonymous feedback.

On guidance and rules for editors and coeditors

55. An editor should not be able to change the decision of a coeditor unless another coeditor agrees with the editor.
56. A reject and resubmit should only be allowed if at least two coeditors agree with the decision.

57. A coeditor should not be allowed to disregard the majority view of reviewers, unless another coeditor or the editor agrees with the coeditor.
58. Have a prohibition against editors or coeditors soliciting manuscripts.
59. Editors actually need training about best practices. Most of the problems are with editors who are terrible at shepherding manuscripts in a timely way and/or making decisions. The first thing I would require is that they are not allowed to go past two substantive reviews on a paper to make a decision. After two reviews, you better know if you are going to accept the paper or not. Also, stop letting reviewers act like they are coauthors.
60. Desk rejections play an important role in speeding up the publication process and can be advantageous for authors. But, to be advantageous to authors, the desk rejection letters need to be informative. The fact that the editor is sorry about this is not informative. Two suggestions: (i) Do a double-blind audit of your journals rejection letters to assess their information content for the author. (ii) Have desk rejection letters reviewed by at least one coeditor to determine their information content.
61. After having made their choice of reviewers, (co)-editors should not be allowed to add reviewers after reports from original reviewers are in, fishing for the support they like for the paper. They should then either have the guts to overrule original reviewers or follow their advice even if it goes against their "expected outcome".
62. For articles that are not desk-rejected, a coeditor or a dedicated follow-up editor (akin to data editors) could assist authors in quickly finding an appropriate outlet for good but rejected articles through direct contact with coeditors of other journals.

On quality standards for theoretical results

63. Require proofs to be written in replicable, formal languages such as *lean*[5] -- this makes checking proofs an objective, verifiable task that doesn't depend on reviewer/editors tastes. It requires the cost of learning and writing proofs in lean (or any other verification language) but it preempts potentially difficult future questions about the reproducibility of formal results.
64. I think it is important to add "technical referees," who are RAs whose job is to check proofs, code, and data. If these referees are added, it is important to tell the "academic" reviewers that these referees exist, so that these reviewers can avoid wasting time on these tasks (unless they want to!).

Appendix D. Survey Details

The full text of the survey instrument can be found here.

Methodology for Classifying Open-Ended Responses

To categorize the answers to the question regarding the country of residence, we first cleaned and normalized the responses (e.g., converting to lowercase letters and removing blank spaces). We then classified them while accounting for potential typographical errors made by respondents.

[5] For more information see:
https://leanprover.github.io/theorem_proving_in_lean/theorem_proving_in_lean.pdf

For categorizing the answers to all other open-ended questions, we employed a “manual” approach using ChatGPT (Version GPT-3.5 Turbo). This method significantly outperformed all other approaches we tested in terms of classification accuracy and the proportion of untagged responses. In summary, the manual approach to categorizing responses to an open-ended question involved five steps:

1. The dataset was divided into smaller batches of responses. These batches were created by randomly selecting responses, with a limit set on the token size per batch. Using smaller datasets was necessary because ChatGPT tends to miss some observations when given a large dataset. Additionally, this prevented us from exceeding the maximum token limit allowed by the API.
2. Starting with the largest batch of responses, we uploaded it and prompted ChatGPT to generate a certain number of categories with meaningful titles and short descriptions. To determine the optimal number of categories, we made several attempts, beginning with a high number of categories (e.g., 15) and then assessed whether ChatGPT provided sufficiently distinct category titles.
3. The calibration process for the number of categories was done manually and iterated until no two titles were too similar. Once a satisfactory number of categories was proposed, we also assessed the percentage of uncategorized responses. If the percentage of uncategorized responses was unacceptably high (i.e., greater than 30%), we increased the number of categories until the condition was met.
4. After obtaining the categorization of the largest batch of responses with the optimal number of groups (and meaningful titles), we manually inspected the results to ensure the titles were comprehensive and meaningful (e.g., avoiding topics that were too vague or failed to provide sufficient insight into the responses). The classification for answers could be refined through an iterative process, as we experimented with different parameters (returning to step 2 if necessary).
5. Once the work on the largest batch was satisfactory, we processed the subsequent batches and prompted ChatGPT to tag them based on the categories identified in the previous steps. The final output of the categorization was organized into a CSV file. Random samples of the final output were reviewed manually to check for any mismatches between titles, descriptions, and the content of the categories.

Appendix E. Institutional Representation in Selected Journals

One concern that was repeatedly raised in the survey is the clubbiness of the profession. To assess that concern, and to see changes over time, a useful set of metrics may be the number of papers published by categories of institutions. In this Appendix we include two tables constructed from public data by Jeffrey Weaver that give some of this information. The first table presents information for the period 2014-2024 on the number of papers published in a large number of journals by authors from a large number of institutions. The second table presents by year, for five leading journals the fraction of papers with authors from the top 10 academic institutions. For example, the first column in the second table suggests that during these years a stable 20% of the papers in the so-called top 5 journals had authors from the top 5 institutions

(MIT, Harvard, Princeton, Berkeley, and Yale), with almost 50% of the papers having authors from the top 20 departments. The first table shows that the leading departments vary in which journal they tend to publish in. For example, over the period 2014-2024 authors from the University of Pennsylvania published 14 papers in the JPE, but only 3 papers in the QJE, whereas authors from UC Berkeley published 13 papers in the JPE, but 30 in the QJE.

	university	top5 2019-24	oth gen int 2019-24	top5 2014-24	oth gen inter 2014-24	unique authors	aer 2019-24	ecma 2019-24	jpe 2019-24	qje 2019-24	restud 2019-24	aeja 2019-24	aejep 2019-24	aejmac 2019-24	aejmic 2019-24	aeri 2019-24	ej 2019-24	jeea 2019-24	restat 2019-24	aer 2014-24	ecma 2014-24	jpe 2014-24	qje 2014-24	restud 2014-24	aeja 2014-24	aejep 2014-24	aejmac 2014-24	aejmic 2014-24	aeri 2014-24	ej 2014-24	ej 2019-24	jeea 2014-24	restat 2014-24
1	MIT	161	72	278	118	40	49	32	27	26	27	6	11	3	7	25	6	3	11	83	67	48	48	32	23	19	5	9	25	9	6	7	15
2	Princeton	138	49	232	80	51	33	36	22	22	25	7	5	3	2	14	5	4	9	73	66	30	31	32	12	11	6	7	14	7	5	5	13
3	Harvard	122	45	205	69	40	34	18	20	30	20	2	7	3	6	10	2	6	9	66	30	29	51	29	2	10	4	6	10	6	2	16	13
4	Berkeley	121	71	208	117	46	34	20	13	30	24	6	5	13	6	9	7	12	13	68	31	20	55	34	11	15	21	8	9	8	7	19	19
5	Yale	111	45	184	83	45	33	27	20	10	21	3	3	7	6	12	5	6	3	52	64	26	15	27	10	6	11	11	12	10	5	10	8
6	Chicago	102	37	198	72	28	25	24	20	15	18	6	6	5	2	9	3	2	4	47	49	55	24	23	7	11	6	3	9	16	3	8	9
7	Stanford	90	44	158	84	35	29	18	19	9	15	10	4	5	3	4	4	4	10	58	32	27	20	21	14	9	8	11	4	5	4	15	14
8	NYU	83	35	132	64	42	22	19	19	6	17	2	0	6	6	7	8	6	0	41	30	27	11	23	5	1	10	9	7	15	8	7	2
9	Northwestern	80	30	134	57	41	14	23	18	8	17	2	2	2	3	8	4	6	3	28	38	24	15	29	7	4	6	5	8	7	4	9	7
10	UCLA	75	23	119	29	40	18	16	16	10	15	3	2	3	2	6	1	2	4	27	28	23	15	26	4	3	5	3	6	1	1	2	4
11	Columbia	74	27	118	58	37	18	18	18	8	12	1	3	4	4	5	3	2	5	35	22	30	13	18	3	5	7	9	5	12	3	5	9
12	Duke	60	26	107	56	41	19	6	11	8	16	4	1	2	6	5	5	1	2	41	17	15	10	24	11	5	4	10	5	8	5	3	5
13	UCSD	51	39	89	67	31	15	11	11	8	6	9	5	1	3	4	5	6	6	27	21	17	15	9	15	7	3	5	4	5	5	7	16
14	Cornell	49	29	76	60	37	12	10	7	9	11	2	7	3	1	2	5	5	4	19	15	15	15	12	3	16	4	2	2	9	5	9	10
15	BU	48	40	81	59	36	10	9	8	7	14	9	5	3	3	7	5	3	5	19	15	16	11	20	12	5	4	5	7	7	5	4	10
16	Brown	46	40	81	65	36	12	8	7	7	12	2	5	4	3	8	2	10	6	26	16	12	12	15	6	6	5	3	8	8	2	17	10
17	UPenn	44	19	77	42	29	5	11	14	3	11	0	0	7	0	3	5	2	2	13	23	20	4	17	0	1	11	2	3	8	5	8	4
18	UW-Madison	41	17	72	27	31	8	12	10	0	11	3	2	5	4	0	1	1	1	17	16	16	1	22	4	2	6	7	0	4	1	1	2

	university	top5 2019-24	oth gen int 2019-24	top5 2014-24	oth gen inter 2014-24	unique authors	aer 2019-24	ecma 2019-24	jpe 2019-24	qje 2019-24	restud 2019-24	aeja 2019-24	aejep 2019-24	aejmac 2019-24	aejmic 2019-24	aeri 2019-24	ej 2019-24	jeea 2019-24	restat 2019-24	aer 2014-24	ecma 2014-24	jpe 2014-24	qje 2014-24	restud 2014-24	aeja 2014-24	aejep 2014-24	aejmac 2014-24	aejmic 2014-24	aeri 2014-24	ej 2014-24	ej 2019-24	jeea 2014-24	restat 2014-24
19	Michigan	39	51	68	81	32	9	6	12	5	7	6	4	11	5	1	8	5	11	20	11	16	11	10	8	8	15	6	1	11	8	7	17
20	USC	34	27	57	51	33	7	7	8	1	11	4	1	1	6	0	3	4	8	16	14	10	1	16	9	5	3	7	0	5	3	7	12
21	Minnesota	32	15	49	22	16	9	8	5	5	5	1	0	4	1	4	0	4	1	13	9	10	9	8	1	2	5	2	4	0	0	6	2
22	UT-Austin	31	39	45	58	24	13	3	6	2	7	8	7	4	3	3	2	5	7	22	6	6	2	9	12	8	5	3	3	7	2	5	13
23	Penn State	27	17	38	30	20	7	2	8	5	5	0	0	5	4	3	1	2	2	13	4	8	6	7	0	0	5	11	3	2	1	3	5
24	W.P. Carey School of Business, ASU	25	14	35	23	16	6	8	3	1	7	0	0	6	2	0	2	1	3	13	9	3	2	8	1	1	8	3	0	4	2	1	3
25	Dartmouth	24	27	44	51	22	8	4	3	4	5	5	8	1	1	3	2	1	6	20	6	4	8	6	12	12	3	4	3	2	2	2	11
26	Boston College	22	13	38	26	18	5	6	4	3	4	0	0	4	1	1	4	2	1	9	12	6	3	8	1	1	5	4	1	5	4	2	3
27	Notre Dame	22	30	26	56	29	6	2	5	5	4	6	2	2	0	2	4	0	14	6	4	6	5	5	9	7	7	1	2	6	4	0	20
28	Davis	21	30	29	62	23	10	1	1	2	7	6	1	3	2	4	5	2	7	13	1	3	3	9	8	6	5	4	4	9	5	8	13
29	Pittsburgh	20	16	33	28	20	10	3	2	1	4	1	3	0	4	3	1	1	3	14	4	4	3	8	3	6	2	5	3	3	1	1	4
30	Rochester	19	13	36	20	17	5	4	8	0	2	4	4	2	0	0	3	0	0	12	10	8	1	5	6	4	3	0	0	3	3	0	1

year	All_top5 (%)	All_top10 (%)	All_top20 (%)	AER_top10 (%)	ECMA_top10 (%)	JPE_top10 (%)	QJE_top10 (%)	RESTUD_top10 (%)
2014	20.9	31.5	45.5	27.7	37.7	29	45	25
2015	21.7	33	48.3	34.8	40.6	33.3	37.5	14.6
2016	22.2	34.8	49.3	35	34.5	41	47.5	19.6
2017	24.5	33.5	45.2	33.3	30.6	40.5	40	22.6
2018	22.4	36.8	55.6	32	50	37.3	47.5	24.2
2019	24.5	38.7	53.3	37.2	33.3	41.4	57.5	32.9
2020	22.5	38.4	51.6	33.9	45.3	42.7	55	24.1
2021	23.3	32.8	48.7	33	35.9	26.5	43.8	29.1
2022	22.3	35.6	48.8	28.3	52.9	31.5	37.5	31.1
2023	20.3	35.7	49.9	34.1	39.1	27.8	40.3	38