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Peer Review as an Evolving Response to Organizational Constraint: Evidence from Sociology Journals, 1952–2018

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Abstract

Double-blind peer review is a central feature of the editorial model of most journals in sociology and neighboring social scientific fields, yet there is little history of how and when its main features developed. Drawing from nearly 70 years of annual reports of the editors of American Sociological Association journals, this article describes the historical emergence of major elements of editorial peer review. These reports and associated descriptive statistics are used to show that blind review, ad hoc review, the formal requirement of exclusive submission, routine use of the revise and resubmit decision, and common use of desk rejection developed separately over a period of decades. The article then argues that the ongoing evolution of the review model has not been driven by intellectual considerations. Rather, the evolution of peer review is best understood as the product of continuous efforts to steward editors' scarce attention while preserving an open submission policy that favors authors' interests.

Keywords Editing · Organizational adaptation · Peer review · Publishing

Introduction

In most contemporary academic disciplines, a large proportion of written scholarship is subject to formal evaluation by experts prior to publication in journals or as academic monographs—a process generically termed editorial peer review. Editorial peer review is not an intrinsic feature of a system of scholarly communication. Learned journals existed for centuries before formal peer review processes developed, and although a handful of prominent medical journals had adopted a recognizably modern editorial peer review model by the turn of the twentieth Century (Burnham 1990), the model did

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not begin to diffuse across a wide range of disciplines until the middle of the twentieth Century (Weller 2001: 3–8). There is a large empirical literature about contemporary peer review to be found in journals such as *Learned Publishing*, *Research Evaluation*, and *Scientometrics*. However, there is markedly less research that elucidates the long-term historical developments that produced the current model of editorial peer review, no doubt owing in part to the scarcity of data about a process that is, by design, confidential and largely anonymous. Historical studies of peer review frequently examine particular journals (or editorships at particular journals) where detailed records happen to have survived in accessible form. This article offers a wider historical view, describing the gradual evolution of several common features of contemporary journal peer review in sociology. The data used in this account are written annual reports of editors of American Sociological Association (ASA) journals, which begin in 1952, as well as annual descriptive statistics on the operation of these journals, published in standardized form since 1982. These data are not as detailed as those commonly used in historical studies of peer review, but are unusually broad.

Although a modest degree of pluralism in evaluative models persists in journals in sociology and neighboring social sciences, one specific model of editorial peer review is especially prominent. It is therefore worthwhile to begin with a plain statement of several characteristics of the operation of academic journals that may appear obvious to most contemporary scholars. In the main, editors are faculty members who operate a journal concurrently with ordinary work responsibilities; some receive modest, fixed remuneration, but editors have no strong financial interest in the journals they edit, and commonly serve for fixed or periodically renewed terms. (For most current journals, the economic interest rests primarily with one of a handful of large commercial publishers.) Journals do not restrict submissions by status criteria such as institutional affiliation or academic rank, and submission ordinarily carries little or no money cost, even at journals where authors assume a large part of the eventual expense of publication. Authors are expected to submit a given work exclusively to a single journal. After initial screening, submissions to a typical journal undergo double-blind review, in which the identities of authors and reviewers are not known to one another. Most evaluations of submitted manuscripts are produced by scholars who are not part of the appointed editorial staff of the journal. Work that is published has ordinarily undergone at least one formal round of revision and resubmission in response to the substance of external evaluations.

This article uses the editors' reports and descriptive statistics to show that major features of this integrated editorial model developed separately in English-language sociology journals. At ASA journals, blind review, external review, exclusive submission, the formal revise and resubmit decision, and a developmental (rather than advisory) model of assessment developed in succession over a period of more than 30 years. In the twenty-first Century, persistent difficulties in obtaining timely reviews prompted a rapid, order of magnitude increase in frequency of rejections without review, commonly called desk rejections. Blind review was the only feature of the present model adopted at an ASA journal with an explicitly stated, unambiguously intellectual aim. This article argues that the other features of the current peer review model emerged as improvised efforts to balance two competing organizational imperatives: editors must steward scarce time and attention, but have also sought to render reasonably timely decisions without a priori exclusion of large numbers of prospective

authors or capricious rejection of submissions. This pattern in journal operations in many ways reflects larger structural changes in sociology: rapid expansion of the field in the mid-to-late twentieth Century was succeeded by increasing competition in the academic labor market and heightened publishing expectations for tenure and promotion.

The argument that features of peer review emerged as organizational expedients is in some tension with rationales for extensive use of editorial peer review. Today, justifications for editorial peer review are intellectual (Jubb 2016): peer review is commonly treated as a practice for impartially assessing the soundness and significance of scholarly work, as well as a means of improving it. Successful navigation of editorial peer review is a crucial aspect of contemporary scholarly life. The results of the process figure in important individual outcomes like hiring, tenure, and promotion. In many national systems, journal peer review outcomes are also a major feature of materially consequential collective evaluations. For instance, in 2001, journal articles constituted 70% of the material evaluated in the United Kingdom's Research Assessment Exercise (Bence and Oppenheim 2004: 60), and high-status journal publications have had a particularly prominent role in national research performance evaluations in Australia (Martin-Sardesai et al. 2019: 52).

Though intellectual rationales are widely offered to support editorial peer review, the model has well-understood intellectual weaknesses that arise across academic disciplines. The reviewing process is poorly equipped to detect fraud (Bornmann et al. 2008), and is an unreliable means of detecting certain kinds of honest but serious analytical errors (Starbuck 2016). Across a wide range of the social sciences, a significant proportion of published findings cannot be replicated from the underlying data, or cannot be produced again by other researchers using similar study designs (Freese 2007; Nosek et al. 2015). At most journals, reviewing practices combine assessments of basic soundness with vaguer judgments about merit or importance, creating a strong bias against the publication of negative findings or replications of findings published in prior studies (Abbott 2007). Such judgments are also shaped, to varying degrees, by reviewer biases, errors, and random variances (Johnson and Hermanowicz 2017: 500–513). Peer review is also a relatively weak predictor of impact. In sociology, as in other social scientific disciplines, there is not a strong relationship between reviewers' assessments of a work and its subsequent citation rates (Starbuck 2005; Teplitskiy and Bakanic 2016). In both the natural and social sciences, it is common for highly innovative, important work to be rejected before finding its way to publication (Gans and Shepherd 1994; Campanario 2009). And although a prominent rationale for peer review is to assess the basic soundness of work, the empirical and analytic core of papers is ordinarily little altered by the peer review process: cross-disciplinary studies find that reviewing focuses much more on presentation and framing (Mulligan et al. 2013: 144), a pattern also specifically observed within sociology (Teplitskiy 2016).

The argument offered here will suggest that the limitations of this model are no great wonder: although peer review is, of course, a practice intended to assess the merits of pieces of scholarship, its features did not arise because they were believed to be an especially apt means of identifying the best work. Substantiating this historical claim is the core purpose of this article, which proceeds immediately to a description of the data and a historical exposition. This historical account provides a basis for thinking about

editorial peer review in different terms. The discussion compares journal peer review to the organization of evaluative work in literary and scholarly book publishing, literary periodical publishing, grant peer review, and other models of scholarly journal editing. Compared to these alternatives, the success of editorial peer review in identifying outstanding work is open to debate, and it is without question a notably labor-inefficient method of evaluation. The defining virtue of editorial peer review is moral as much as it is intellectual: it does a great deal to protect the interests of prospective authors, whereas restriction of submission opportunities and rapid disqualification of submitted work is a common feature of other approaches. The discussion then suggests that greater pluralism in editorial models might be a sound means for sociology, as a discipline, to protect many of the values of editorial peer review while also moderating some of its obvious limitations.

Data

This article makes use of a published but evidently neglected source of data on the operation of scholarly journals: reports from the editors of official American Sociological Association journals, which involve two different sorts of information. First, this article makes use of 351 written annual reports of journal editors. These reports began with the first published annual report of the editor of the *American Sociological Review* in 1952, and have since been published with only three notable gaps, in 1954, 1976, and 1994. The beginning of this series of reports antedates nearly all of the features of the current review model. Second, the article makes use of descriptive statistics on the operation of ASA journals, for which standardized annual reporting began in 1982. The journals and time periods included in the data are as follows: *American Sociological Review* (1952–2018); *Social Psychology Quarterly*, previously titled *Sociometry* and *Social Psychology* (1955–2018); *Sociology of Education* (1963–2018); *The American Sociologist* (1966–1982); the *Journal of Health and Social Behavior* (1967–2018); *Sociological Methodology* (1968–2018); *Sociological Theory* (1982–2018); *Teaching Sociology* (1986–2018); and *Socius* (2015–2018).[1] The Appendix lists the bibliographic information for the reports and describes the abbreviated citation method by which this article refers to reports.

The annual descriptive statistics report on matters such as the volume and outcomes of submissions, the average time to editorial decision, the lag before publication, the number and kind of items published, and (for some of the time period) the size and demographic composition of the editorial board and reviewer pool. Before the adoption of uniform statistical reporting for ASA journals in 1982, editors' written reports frequently included descriptive statistical information—sometimes vague, and sometimes more detailed than what would be adopted as the standard requirements.

The written reports vary widely in their scope and substance. Some reports are little more than a polite list of acknowledgments, or narrative statements of statistical information also provided in tabular form. Other reports range widely over administrative matters, editorial goals, organizational difficulties, responses to critics, and

[1] Because of this article's concern with the development of journal peer review, it does not consider written reports or descriptive statistics from the ASA's monograph series or *Contemporary Sociology* and *Contexts*, journals that employ different editorial models.

intellectual aspirations for the discipline. The annual reports offer markedly deeper insight into the organizational aspects of scholarly publishing than do editorial statements of the kind often published in the journals themselves.

Though richly informative, there are also obvious limits to what these data may show. First, and most obviously, the written reports offer only the editor's view of a process of historical change that has also affected the work of authors and reviewers. Nor are the reports free of persuasive purposes. Most obviously, they may be taken as implicit efforts to persuade contemporaneous readers—who were also the potential pool of authors and reviewers—that the editor is harder working and broader minded than the journal contents or long review times would suggest. Perhaps owing to its scope and the particular scrutiny its editors face, reports about the operations of ASR are particularly detailed and revealing. The richness of written reports has markedly diminished since the beginning of online reporting in 2005. The means of generating online reports evidently recycles text from year to year except when it is expressly changed.

The article makes sparing use of the descriptive statistics. Many pieces of statistical information, even where they are consistently labelled, are not firmly comparable over time—or, in some cases, comparable across journals at a given time. Acceptance rates have been calculated at least three different ways over the observed period. Both the substantive meaning and numerical reporting of intermediate decisions like "revise and resubmit" and "conditional acceptance" have changed, and it was not standard to report review times and results for initial submissions and revisions separately until 2015. The meaning of "publication lag" changed as it became possible to publish accepted manuscripts online before assignment to a print issue, and the laborious revision process that began to develop in the 1970s means that editorial and publication lag times, in sum, will often have little relationship to the true length of time published work spent in the editorial process. The figures for editorial lag and total submissions—two basic indicators of the volume and pace of a journal's work—do have substantively similar meaning over time, and also happen to be the statistics of greatest use for the present argument.

There is a significant labor of interpretation involved in the argument offered. Editorial reports are relatively brief—typically around 1000 words per journal each year—and have for most of the time period been published in newsletters, with the implication that the content will not be of enduring interest. The reports therefore focus on what is notable or novel, and do not dwell on aspects of journal publishing that could have been taken as obvious to a contemporary reader. The exposition therefore proceeds, in part, by paying attention to when matters are frequently discussed, and when they disappear. It is presumed that editorial practices will tend to be discussed when they are new, and recede from view as they become established facts or widely shared norms.

Resource Constraint and Improvisation in the Development of Journal Peer Review

In most contemporary sociology journals, as in the social sciences more generally, the article review process now has several relatively standard and tightly integrated features. None was in place when annual reports on the operation of the *American Sociological Review* began in 1952. This section uses evidence from editors' reports

and descriptive statistical data to describe when these features of the journal reviewing process developed, and connects the timing of new editorial practices to changes in journals' routine work. Three important historical changes in the organization of reviewing are considered in succession. The shift from reviewing by editorial boards to ad hoc review occurred rapidly between the late 1960s and mid-1970s. The management of the revise and resubmit decision following the rise of an author-focused, developmental model of review arose as an editorial problem by the late 1970s, and has to varying degrees remained a problem ever since. Beginning with a significant increase in 2008, editors have made much more frequent use of desk rejections to mitigate workload problems associated with the developmental evaluation model, as well as scholars' diminished willingness to accept invitations to review. These changes have yielded a model in which editors' ability to shape the overall direction of a journal may be modest, even if editors retain significant discretion over the disposition of particular manuscripts.

Double-blind review began in the mid-1950s, and was plainly intended to serve an intellectual aim. In 1955, editor-in-chief Leonard Broom began referring papers to the ASR's editorial board with the name of the author removed. As he said at the time, the policy "makes it easier to apply universalistic criteria" (ASR 57), and he later explained that he had been concerned that "reviewing was of persons rather than of submissions" (Broom quoted in Abbott 1999: 145). Broom was a notably early adopter of the practice. Double-blind review was not a feature of the medical or natural scientific peer review model that was becoming institutionalized at mid-century (see Weller 2001: 3–5), nor was it the standard practice in the social sciences. For instance, the *American Economic Review* did not begin to experiment with double-blind review until 1973 (Borts 1974: 478), and Broom's editorial contemporaries at the *American Journal of Sociology* had firmly opposed the practice (Abbott 1999: 145–147). Double-blind review did not immediately establish itself as a firm prerequisite of the review process at ASR. When Broom's successor, Charles Page, wrote a set of proposals derived from his own three year term, he found it necessary to expressly recommended that blind review be continued (ASR 60). Double-blind review evolved as a norm, not an organizational necessity.

In contrast to the clear purposes for employing double-blind review, later developments in reviewing reflect an effort to balance two organizational matters. The first was to control the amount of work to be carried out by the editor-in-chief.[2] The second was to render reasonably quick decisions while preserving an open submission process. The current peer review model distributes evaluative labor very widely, and delays the exercise of editorial judgment. To the degree that editors rely on reviews as a complement or partial substitute for their own evaluation of a paper, reviewing also simplifies the editorial task. (In their reports, editors rarely give an unambiguous account of what they do with reviews, but at most journals, reviews will function at least some of the time as a substitute for the editor's own complete reading and assessment of a manuscript.)

[2] Managing the editor's workload has also spurred a range of other responses not directly concerned with reviewing itself. The appointment of deputy editors and proliferation of editorial assistants was common during the period when little of the administration or physical production of a journal could be automated. More recently, it has become common for journals to be operated by pairs or teams of editors-in-chief. All ASA journals were headed by a single editor-in-chief from the 1950s until the late 1990s, but since that time, most ASA journals have had at least one team of editors. Team editing now appears to be a firm norm at ASR.

Ad hoc review, in which manuscripts are referred to scholars not formally affiliated with the journal organization, did not become an integral feature of the ordinary reviewing process until the early 1970s. Before then, virtually all evaluations were produced by members of journal editorial boards. Exclusive submission to a single journal also did not become a rule until the 1970s, and there is suggestive evidence that simultaneous submission to multiple journals may have been somewhat common until that time.

At first, reviewing was plainly intended to aid to the work of the editor; the occasional value of reviews to authors was taken as an incidental benefit. The evolution of a developmental model of review oriented toward the author was gradual, as was the emergence of the revise and resubmit decision as a nearly unavoidable intermediate step on the path to publication. The volume of reviewing work to be done for a given submission expanded as this author orientation solidified: both desk rejections and outright acceptances became rare. The result was significant growth in the quantity and complexity of reviewing work required for any given submission.

By the end of the twentieth Century, editors at most ASA journals assigned almost every submission—however unpromising—to reviewers. Maintaining this commitment has proven unworkable. Editorial lag times and difficulty in securing reviews increased steadily beginning in the late 1980s, and these problems were compounded rather than alleviated by a shift to electronic submission and reviewing practices in the early 2000s. Today, desk rejection rates above 25% have become the norm at ASA journals, and this exercise of discretion is evidently one of the more important powers of contemporary editors. Under this model of dispersed evaluative labor, editors may have less ability to direct the overall orientation of journals. Much of the time, of course, editors' views about particular submissions and the general state of a subfield will accord closely with those of reviewers. Yet the reports show that there are cases where lineages of editors have spent decades attempting to orient a given journal in some direction.

The Emergence of Ad Hoc Review

When reviewing first developed, nearly all reviewing work was carried out by members of journal editorial boards. Through the 1960's, ad hoc referees were used so sparingly that journal editors often thanked each of them by name in the annual reports. In 1961, 85% of reviews at ASR were produced by editorial board members; this number remained comparably high through 1967, when 90% of reviews at ASR were produced by the editorial board (ASR 67). That year, *The American Sociologist* used only two external reviewers for 86 submitted manuscripts (TAS 67). In the first report on a full year of operations, the *Journal of Health and Social Behavior* received 147 submissions and used seven ad hoc reviewers (JHSB 68). This was evidently the norm across the discipline: through the 1960's, editorial board members also produced 85% of the reviews at *Social Problems* (Smigel and Ross 1970), and members of the University of Chicago sociology department (the ex officio board of the journal) served as a major pool of reviewers for the *American Journal of Sociology* (Abbott 1999).

This reliance on a small, stable set of reviewers aligned with the avowed purpose of reviewing in this period: to help editors avoid erroneous judgment. ASR editor Kurt Schussler characterized his task thus: "The main idea is not to make a wrong decision

[…] the general procedure is to elicit information from reliable sources repeatedly" (ASR 69). By confining reviewing to the editorial board, evaluations would have come from trusted reviewers—and, perhaps just as significantly, reviewers whose biases and leanings would become familiar to the editor. Accounting for individual taste was often necessary to make sound decisions: *Sociometry* editor John Clausen noted that the most generous board members recommended acceptance 50% of the time, while the severest recommended acceptance 15% of the time (SPQ 61). Melvin Seeman, reflecting on his tenure at *Sociometry*, also noted high reviewer disagreement, especially between sociological and psychological reviewers. Seeman published a matrix of a year's worth of reviewer scores in support this contention. Excluding papers judged to be absolute failures, two reviewers gave the same score on a 1–5 scale only 16% of the time (SPQ 66).

This evaluative model asked a great deal of work from editorial board members. Before the wide use of ad hoc review, board members at ASA journals routinely produced 30 or more reviews a year—sometimes far more. In 1961, the average ASR board member produced 3.6 reviews a month (ASR 62). Although this manner of organizing review may have been more informative to editors, it was relatively inflexible. In view of high routine workloads, the board-centered reviewing model could not expand reviewing capacity except by securing the ASA Publication Committee's permission for incremental expansions in editorial board size.

This reviewing model, always under strain, became untenable when submission volume increased sharply in the late 1960s and early 1970s: at this time, the discipline was both expanding rapidly in size and undergoing a process of professionalization that created stronger pressures to publish. As Table 1 shows, the increase in the volume of article submissions was significant and affected all of the Association's journals; similar workload increases also arose at AJS (Abbott 1999: 167). In 1974, the *Sociology of Education* carried such a large backlog of work that it suspended submissions for ten months (SOE 75).

This increase in submission volume coincided with a comparably rapid shift in the organization of reviewing. In 1968, ASR editor Norman Ryder reported that 80% of manuscript reviews were produced by the editorial board. Facing a fixed supply of reviewing labor and increasing submission volume, mean editorial lag—already climbing steadily over the 1960s—jumped in one year from 12.9 weeks to 17.2 weeks (ASR 68). This lag time clearly exceeded an informal but remarkably stable sense of what constitutes a reasonable time to decision for the field. Although editors, from the 1950s to the present,

Table 1 Change in Manuscript Submission Volume at ASA Journals, 1967–1973

Journal	Submissions 1967	Submissions 1973	% Change
American Sociological Review	399	622	+56%
Sociometry	146	~350	+140%
Sociology of Education	146	212	+45%
Journal of Health and Social Behavior	~100	187	+87%
American Sociologist	86	180*	+109%

**American Sociologist* figures for 1971

nearly always profess a desire to render decisions more promptly, these concerns become more pronounced when review times stand above twelve weeks. This number is also mentioned in reform efforts. When the ASA imposed exclusive submission as a formal requirement for authors in the 1970s, journals were expected to furnish status reports to authors when articles had not received a decision within twelve weeks, and authors had the right to withdraw submissions at this point. In effect, exclusive submission and editorial timeliness were originally understood as reciprocal obligations. Gary Alan Fine echoed this when he announced an "author's bill of rights" 35 years later: the first two obligations of editors were to assign an article for review within one week of receipt, and render a decision within three months (SPQ 08).

Facing mounting volumes of submissions, editors of ASA journals made steadily greater use of ad hoc review to distribute the evaluative task. This was true under Schuessler and Short's editorships at ASR: when Morris Zelditch wrote his first report as editor of ASR in 1975, editorial lag had been brought under ten weeks, and 66% of evaluations were produced by ad hoc referees (ASR 75). *Social Psychology Quarterly* (formerly *Sociometry*), which evidently had not used ad hoc reviewers at all until the late 1960's (SPQ 68), used 101 ad hoc reviewers in 1974 (SPQ 74). *Sociology of Education* editor John Kitsuse gave effusive thanks to what he characterized as an "army of referees" (SOE 74). The same shift occurred at AJS in the first years of the 1970s (Abbott 1999: 154). By 1980, ASA journal editors had entirely ceased routine reporting of the relative proportion of reviews produced by the board and ad hoc referees. Ad hoc review, by then, had evidently become the norm at most journals, although it did not become typical at the *Journal of Health and Social Behavior* until the late 1980s (JHSB 88).

Similar changes in submission volume and the organization of reviewing occurred at other flagship social science journals. The *American Economic Review* also relied primarily on its editorial board at mid-century, using 46 ad hoc reviewers for 231 submissions in 1954 (Haley 1955) and 162 ad hoc reviewers for 451 new submissions in 1966 (Gurley 1967). In 1973, submission volume had increased to 758; the journal made use of 422 ad hoc reviewers (Borts 1974). At the *American Political Science Review*, whose submission volumes and acceptance rates were similar to ASR in the mid-1970s, the editors used 579 referees to produce 965 reviews of 385 new submissions during a period of observation in 1976 (Jones and Beck 1977).

ASA journal editors offered few intellectual rationales for this shift. Short noted that the relatively young pool of ad hoc reviewers often produced excellent reviews (ASR 74), and the shift allowed editorial board members to receive a larger proportion of submissions within their area of expertise (ASR 75); ad hoc review was also clearly desirable for papers relying on novel, advanced methods familiar only to a small number of current scholars (JHSB 75). But ad hoc review is best understood as a response to pressure arising from submission volume. Handling reviewing in-house plainly served editors' stated goals, and editors of the 1960s, who were not at all shy about criticizing authors or the ASA in their reports, uniformly praised their editorial boards—groups drawn from the field's core of research faculty, and chosen by the editors themselves.

In the following generation, reviewing labor at ASA journals has been dispersed across a steadily larger body of reviewers. In 2018, ASR assigned 439 initial submissions for review (ASR 18). The list published at the end of the 2018 volume credited

some 1300 ad hoc reviewers who evaluated at least one paper that year. Reviewing was only slightly less dispersed at ASA journals in general. In 2014, the last report in which journals regularly listed the number of referees employed in the preceding year, the number of reviewers used by ASA journals ranged from 1.3 to 2 times the number of submitted manuscripts, figures that strongly suggest that ad hoc referees produced the substantial majority of reviews at each of the Association's journals.

Although the evidence examined here suggests that the initial shift from board-centered to ad hoc reviewing was an organizational adaptation to a very sharp increase in submissions, it cannot show that this model remains a necessary organizational response to the submission volumes of contemporary journals. Some ASA journals were founded after the shift to the ad hoc review model and, owing to their specialization, have modest submission volumes—this is true, for instance, of *Sociological Methodology* or *Teaching Sociology*. An adequately staffed editorial board could conceivably carry out much of the reviewing at such journals without obliging board members to produce 30 or more reviews a year, as was common in the early 1960s. The general reliance on ad hoc review today thus has at least two possible explanations. It may be that reliance on ad hoc review, developed to weather the editorial pressures of the 1960s and 1970s, afterward became a norm emulated by new journals that might also have employed other models. It is also possible that other changes in the academic work role make board members unable to assume a significant reviewing load, or that reviewing a given manuscript is itself a more laborious task now than it was two generations ago, making a lessened volume of reviews comparably onerous for evaluators. As the next subsection will show, the functions of reviewing undoubtedly changed during the late twentieth Century.

Author-Focused Reviewing and the Institutionalization of the Revise & Resubmit Decision

The previous subsection has described aspects of editorial workload that were closely linked to volume of submissions. As editorial peer review continued to develop, it created organizational issues more closely related to the management of reviewers and the reviewing process. At first, editorial peer reviewing practices appear to have been oriented strongly toward the interests of editors eager to avoid erroneous judgments (ASR 67, ASR 69). The presumed utility of review for authors was modest. Reviews did not always figure in rejections: Broom, reflecting on the early years of blind reviewing, considered it specifically notable that some authors were provided with reviews even when their work was not published (ASR 57). A decade later, the value of providing reviews, or a digest of reviews, to rejected authors remained a contestable point. In the eyes of many editors, reviewing that went beyond a simple evaluation was undesirable: detailed reviews were a sign of weak or prematurely submitted manuscripts (SPQ 59, SPQ 65, ASR 68). Norman Ryder professed his intention "to continue the practice of rejection with reasons," but also took very seriously the idea that substantive criticism could hurt the feelings of authors, and considered bruised egos a potentially persuasive basis for relying upon form rejection letters (ASR 67).

When the review process was geared toward the interests of editors, reviewers' comments created only modest additional work for authors. Reviews were commonly

given to authors who were asked to revise their manuscripts for publication (ASR 56, ASR 67, SPQ 59, SOE 68), as outright acceptance became rare after the adoption of blind reviewing (ASR 58, SPQ 64). However, requests to revise had not assumed the quality of the formal revise and resubmit decision that later became the norm. A request to revise was effectively an offer of publication: reporting on the results of over 100 requests for revision, Smelser indicates that every manuscript actually revised and returned to the journal was published (ASR 63, ASR 65).

The development of a formal (and nearly unavoidable) intermediate revise and resubmit outcome appears to be associated with an ethical reorientation in editing as the discipline expanded and professionalized. Abbott (1999), writing about the history of AJS, describes important changes at the journal during the 1970s. Editor Charles Bidwell, recognizing that the journal received a growing proportion of manuscripts from younger scholars for whom successful navigation of peer review had significant professional implications, strove to develop a process that was fair to submitters, both in appearance and in fact. His successor, Edward Laumann, maintained this concern with fairness and embraced a model of review that would be instructive to both authors and, to a degree, reviewers (Abbott 1999: 177–8). Reviewing thereby came to be understood, in part, as a procedural safeguard for authors. Rather than serving as semi-private advice to editors, reviews increasingly provided editors with rationales they could invoke to support their decisions, and the provision of reviews came to be understood as a service for rejected authors. Such due process considerations became more prominent as the body of prospective authors became steadily younger, a transition associated with both mounting tenure-related publication expectations, and senior scholars' avoidance of the new, arduous revision model (Abbott 1999: 169–175). Although the public reports of ASA journal editors do not spell out this reasoning in the same manner as the private documents handled by Abbott, the observable changes in editorial practice are the same.

This reorientation coincided with the development of a revise and resubmit decision quite unlike the informal revise and publish outcome common through the 1960s. Although the editors' reports document the rapid institutionalization of this decision as an intermediate step to publication, the reports offer no direct speculation about why changes in reviewing practice evidently required this. Two factors appear to be plausible drivers. First, conferring a formal opportunity to revise may be understood as fairer to authors than issuing a nearly final editorial decision after initial review. Second, changes in the allocation of reviewing work and norms about reviewing probably yielded longer reviews with more concrete advice and criticism—though their genuine benefit may vary, longer reviews written with the goal of promoting improvement will generate more new work for authors than mere evaluations produced by overtaxed board members.

Whatever the causes, the adoption of the revise and resubmit decision created organizational challenges for journals as well as more work for authors. First, editors had to manage the supply of revise and resubmits: it was necessary to assure sufficient flow of publishable manuscripts while avoiding overly liberal invitations to revise that would create false hope and extensive waste of author and reviewer labor. Second, editors had to find a means to halt the revision process; in view of personal and intellectual differences in reviewers' attitudes, overly deferential editing could result in interminable revision.

Outright acceptances virtually disappeared at ASA journals over the 1970s, and the revise and resubmit process evidently created instability in the supply of acceptable manuscripts. Occasionally, this resulted in sudden, unexpected gluts of accepted papers (SPQ 75, ASR 77). The more common problem, however, was a shortfall of acceptances amid plentiful submissions. In the early 1980s, neither *Sociology of Education* nor *Social Psychology Quarterly* were using their full page allocations, despite stable or increasing submission volume (SOE 81, SPQ 81, SPQ 82). Owing to low throughput, editor Alan Kerckhoff even professed a strong desire to increase submission volume despite an acceptance rate around 15% (SOE 80). Editorial handovers could also result in changes in use of the revise and resubmit that, evidently, created shortages, such as years under Gerald Marwell's editorship when the ASR did not use its full page allocation (ASR 95). Dealing with the revise and resubmit also involved learning for authors: editors reported issues like wide misunderstanding of revision requests as rejections (SOE 80), inadequate effort in revision (TS 95), or simple difficulty in working through the abundance of advice and criticism offered by reviewers (SOE 02).

The development of a formal revision process required cultivating editorial judgment about what proportion of promising papers ought to receive an invitation to revise, and deciding when deference to reviewers ceased to be responsible or fair. Imbalance could produce fruitless or endless revision. Where there was a plentiful supply of work that was at least competent or promising, liberal invitations to revise and resubmit resulted in very low acceptance rates on revision. At *Social Psychology Quarterly*, most work used a settled experimental paradigm that, in the editor's judgment, often created solid data but uninspired manuscripts (SPQ 72); acceptance rates of revised and resubmitted manuscripts stood at or below one-third through the 1970s (SPQ 75, SPQ 78). This has also been a recurring challenge for editors at ASR. The journal has always been a desirable outlet for scholarship, and from the first published report (ASR 52), its editors have recognized their need to reject meritorious work. The revision process itself has evidently spurred papers to become longer, resulting in declines in the number of published articles by the turn of the twenty-first Century despite substantial increases in available space.[3] This imbalance between promising submissions and available space resulted in a run of years where the number of revise and resubmit invitations was more than 2.5 times as large as the number of published articles, a ratio that climbed above 4 to 1 in 2011. This was not peculiar to a particular editor's style: such patterns arose under three different editorial teams (ASR 03, ASR 06, ASR 07, ASR 10, ASR 11, and ASR 12).

Another challenge has been reviewer disagreement, which may be more difficult to manage when editors have less experience with the leanings of any given reviewer. Disagreement is evidently a persistent and ubiquitous feature of some subfields, especially work on sociological theory and the sociological profession. Under such circumstances, the reviewing process might continue indefinitely without approximating reviewer consensus: for instance, editors at *The American Sociologist* sought to add reviewers until consensus arose. They frequently secured five reviews during a period

[3] Article length has increased dramatically at both AJS and ASR. Between 1996, when Paula England had brought order to the revision pipeline (ASR 96), and 2013, when the journal reached its present size, page allocation increased nearly 40%, while the number of published articles dropped nearly 30%, meaning that average article length nearly doubled in less than 20 years.

when the disciplinary norm was two (TAS 77, TAS 82); this method evidently produced only shortfalls of acceptable manuscripts. At *Social Psychology Quarterly* in the early 1980s, where review repeated until consensus emerged, 75% of articles eventually published had passed through two or three full rounds of revision and resubmission (SPQ 82).

Faced with reviewer disagreements that did not converge after revision, many journals adopted formal or informal limits on the revision process. The *Sociology of Education* deliberately expanded use of conditional acceptance in preference to revise and submit (SOE 83), while other journals facing persistent reviewer disagreement (for instance, JHSB 92, ST 91) eventually developed a one revision rule (JHSB 95, ST 96). In recent years, journal editors have moved strongly in the direction of the older model in which the decision was all-but-made after the initial round of review; invitations to revise have been issued sparingly, and in 2018, ASA journals, in aggregate, issued a second revise and resubmit decision on less than one-fifth of revised submissions.

The New Reviewing Crisis and the Rise of Desk Rejection

In the 1970s and 1980s, editors at sociology journals embraced a developmental model of review. The extensive use of the revise and resubmit decision was one practical result of this orientation. Another was a commitment to send nearly all submissions out for review. Desk rejection at ASA journals nearly disappeared for a period of 20 years: rejection of a submitted article without review denied the author a chance to learn, which editors had come to regard as a major purpose of peer review. As ASR editor Glenn Firebaugh said, "Because most manuscripts submitted to *ASR* are rejected, the most I can promise authors is a commitment to seek *timely*, *fair*, and *constructive* reviews of their work" (ASR 99, emphasis in original).

Desk rejection rates above 10% had been common from the 1960s until the mid-1980s. Beginning in 1986, this pattern markedly changed. Pooling data on submissions to all ASA journals, the overall rate of desk rejection between 1986 and 2007 was 4.4%; aggregate desk rejection rates fell as low as 2.1% in 1997, and was never higher than 6.5% in any year during that period. Between 1990 and 2000, the ASR desk rejected only 4 manuscripts a year, while issuing outright rejections after initial review for 296 papers a year. The journal, along with the rest of the discipline, had embraced what Norman Ryder earlier condemned as a reviewing model that functioned as "a vast nationwide seminar on all sociological (and many non-sociological) topics" (ASR 68). This model has been abandoned in recent years, a shift evidently driven in part by growing difficulties in maintaining a timely review process.

Difficulties in securing reviews or rendering timely decisions are widely acknowledged problems in contemporary journal operations. Two explanations are commonly offered. The first is the development of electronic reviewing practices, which make it relatively easier to decline or ignore invitations to review. There is undoubtedly some truth in this. Scarcely any reports offer data about this matter, but two editors who did undertake a precise count of invitations illustrate an extraordinary change. In 1971, Herbert Costner reported that 85% of requests yielded a completed review (SM 71); in 2015, Rob Warren, whose reports evince particular satisfaction with reviewers, saw a yield of 32% (SOE 15). The second is a change in faculty workload, which includes

both overall increases in responsibilities, and specific increases in reviewing burden.[4] There are good reasons to believe both of these are contributing factors. This subsection will suggest another factor: editors began to report significant problems obtaining reviews during the period when they undertook to send virtually all submissions out for review, and there is a close relationship between the use of desk rejection at a journal, and editorial lag.

Editors' published reports virtually never aired complaints about reviewers before the late 1980s, roughly the moment when ASA journals had virtually ceased to issue desk rejections. Such complaints rose to a steady chorus over the following decade. Concerns about unresponsive or slow reviewers were voiced by editors of all the association's journals (ASR 89, SOE 89, ST 91, ST 93, ASR 95, SOE 96, ASR 97, TS 98, SM 99, ST 99, SM 00, JHSB 01, SOE 01). Editors' perceptions about slow reviewers are supported by the statistics: there was a substantial, stable increase in editorial lag at ASA journals from the late 1980s until 2005.

Thus, long review times were already a subjectively recognized and statistically attested problem when the first experiments with electronic reviewing began. The initial expectation was that electronic practices would speed the reviewing process (SOE 00, SOE 01, JHSB 02), in much the same way that the use of databases and desktop publishing was widely agreed to have improved the efficiency of journal operations during the preceding decade. In 1999, *Sociology of Education* editor Aaron Pallas began emailing invitations to review along with an abstract (SOE 99), and *Sociological Methodology* used an email reviewing process for referees outside the United States (SM 00). Electronic delivery of manuscripts and return of reviews became the norm over the following few years, but editorial lag times continued to increase. As *Teaching Sociology* editor Liz Grauerholz reflected: "to our surprise and disappointment, [electronic submission] seems to be a deterrent for reviewers. When contacted via email by the managing editor requesting reviews, more and more potential reviewers decline the invitation to review or delete the message" (TS 06).

The most significant shift in editorial practice in response to this problem has been freer use of desk rejection. Overall desk rejection rates jumped from 5.5% to 9.6% between 2007 and 2008, and have climbed steadily since. Across ASA journals in aggregate, the desk rejection rate has exceeded 25% in each year since 2016, which brings the field in line with political science, where desk rejections stand at 28% at *American Political Science Review* (König and Ropers 2018), 28.6% at *Political Behavior* (Peterson 2018), and 52% at *Comparative Political Studies* (Samuels 2018). Desk rejection is associated with genuine improvements in time to decision. Across all journal-years of data between 1982 and 2018, the correlation between desk rejection rates and mean editorial lag time is −.543. This is not simply a function of

[4] There is no simple way to reckon up the reviewing load in the field, but there can be little doubt that this burden has increased. Sociologists, like academic social scientists generally, face strong pressures to publish more frequently and at earlier stages in their career (Warren 2019). The number of refereed journals has expanded. The rise of English as an academic lingua franca, the growth in impact and performance metrics that strongly favor American journals, and the development of electronic submission models may all tend to increase international submissions to American journals that remain reliant on a mostly-domestic pool of reviewers. And the standard number of reviews per paper has increased: until 1990s, ASA journals typically sought two reviews per paper, but now commonly obtain three or four. The size of the sociology professoriate, however, is relatively stable.

speedy desk rejections reducing the mean editorial lag: where data allow it to be observed, freer desk rejection is also associated with quicker decision times for articles actually sent out for review. There are several possible causes: it may be more difficult to secure commitments to review weak submissions, such submissions may spend a longer time in the review process, and weak submissions may be disproportionately likely to be reviewed by inexperienced or untimely referees. Although this subsection has shown that the greater use of desk rejection followed upon significant difficulties in running the reviewing process efficiently, it is certainly conceivable that editors have made expanded use of the practice, in part, to protect authors' interests. While thoughtful reviews may certainly be of value to rejected authors, authors would not ordinarily wish to have their work under long consideration at a journal when it is understood, from the first editorial glance, that the work has no prospect of being published.

There is also, potentially, a vaguer matter of reciprocity and solidarity at work: scholars may be more willing to review, and review promptly, when journals do not ask them to review truly weak manuscripts. A related pattern suggests that this matter of reciprocity is not to be discounted. Editorial lag has two components: the time an article spends awaiting assignment and decision, and the time it spends in the hands of reviewers. In the time since journal operations became wholly electronic, the editors who have achieved the most remarkable improvements in overall editorial lag are often those who made a great point of visibly doing their own portion of the work quickly. This is exemplified by Gary Alan Fine (SPQ 07), Rob Warren (SOE 13), Kathleen Lowney (TS 13), and the Notre Dame editors at ASR (ASR 15), all of whom achieved major reductions in overall editorial lag, and also made commitments to extraordinary promptness—undertaking, for instance, to begin assigning reviewers on the day of manuscript receipt, or to render decisions within 48 h of receiving reviews. Although such social compacts about administration have been announced prominently, the relaxation of the editorial commitment to a wholly developmental approach has been quiet: although desk rejection has increased markedly at all ASA journals, only one editor, Neil Gross, expressly stated this intention in an annual report (ST 10).

Mirrors, Not Makers: Modern Peer Review and the Weak Editor

Modern arrangements for the submission and evaluation of work offer significant organizational advantages for scholarly journals. By deferring judgment, editors are able to equip themselves with external assessments that may guide or simplify their decisions, or serve as a substitute for their own direct evaluation of a manuscript. By distributing the evaluative labor to a large number of referees, editors can preserve a relatively open submission model while securing reviews and rendering decisions within timeframes generally acceptable to the scholarly community. However, in relying almost totally on unsolicited submissions and a large (and to some degree unfamiliar) pool of evaluators, editors may cede some control over the direction of journals. Given that many processes will tend to make specialized academic communities self-reproducing, and editors are prominent members of those communities, they will of course agree with reviewers much of the time, and may be expected to have a view of a field or subfield that approximates that of the scholarly community. Yet, at several ASA journals, editors have had difficulty credibly signaling openness to

methods and topics not currently well represented in the journal's pages, and have had limited success using their editorships to promote what they may regard as desirable or necessary intellectual change. Successions of editors at four journals illustrate such patterns.

Editorial difficulty in dispelling a perception is most apparent in the reports from ASR. Kurt Schuessler, the editor who initiated the transition to a primary model of ad hoc review, was also the first to acknowledge growing complaints about the content of the journal. He professed his limited ability to shape the ASR, suggesting that the journal is "more the mirror than maker of sociology" (ASR 69). Subsequent reports also acknowledged the perception that the journal was becoming methodologically overspecialized and unrepresentative of the discipline (ASR 70, ASR 71, ASR 74). Editors Morris Zelditch and Rita Simon suggested reviewers were driving this pattern: Zelditch noted that he would be willing to seek a larger page allocation, but that rejection rates largely reflected the critical bent of reviewers (ASR 75). Simon noted that her work was complicated by high reviewer disagreement about the merits of submissions (ASR 79). Nearly every succeeding editor has expressly proclaimed that ASR is open to a wide range of work, avowals that particularly emphasize openness to qualitative work (ASR 83, ASR 88, ASR 91, ASR 95, ASR 97, ASR 02, ASR 07, ASR 15). Many editors also marshaled evidence, such as comparisons of acceptance rates or counts of articles published by method, to suggest that various methodological approaches fared comparably well in the review process (ASR 89, ASR 92, ASR 04, ASR 04, ASR 17, ASR 18). The Notre Dame editors, seeking to promote fair evaluative practices for various kinds of work, developed specific reviewing guidelines for ethnographic, comparative-historical, theoretical, and policy-oriented papers (ASR 15). Yet the perception evidently persists.

Similar efforts to combat perceived hostility to qualitative methods recurred over a 25-year period at *Sociological Methodology*. A long succession of editors either professed their openness to qualitative methodological submissions, or actively encouraged them (SM 79, SM 84, SM 86, SM 87, SM 96, SM 97, SM 01). Editor Ross Stolzenberg, who criticized the "Pavlovian" tendency to equate sociological methodology with statistics (SM 04), also acknowledged that editors had a limited ability to alter these sorts of patterns: journals belong to "the discipline rather than the editor" (SM 03).

Editors at *American Sociological Review* and *Sociological Methodology*, serving on fixed terms, struggled to signal their own openness and commitment to fairness in ways that would attract more submissions of qualitative work. Similarly, editors at *Sociological Theory* and *Social Psychology Quarterly* have sought, again with apparently limited success, to use the journals to broaden the range of topics and intellectual approaches given prominent representation in their subfields.[5]

At *Sociological Theory*, editors have professed their interest in gender and race, and frustration that the subfield has resisted accepting gender and race as central concepts of theory. Alan Sica noted the difficulty of securing submissions of feminist work (ST 91), and reported significant conflict with the editorial board and reviewers over queer

[5] The following discussion implies no opinion of my own about the intellectual state of sociological theory or social psychology—it simply reports editors' judgments about the need for change in these fields, and their own avowals of difficulty in securing the changes they envisioned.

theory (ST 94), which he regarded to be a major theoretical development, but that others involved with the journal judged to be a "political-cultural position" (ST 95). Craig Calhoun, Sica's successor, professed that a major intellectual task for his editorship was to continue "fully integrating the category of gender (and feminist theory more generally) into sociological theory" (ST 96). Yet more than two decades later, Mustafa Emirbayer professed substantially the same goal, noting that both race and gender remained incompletely integrated into contemporary sociological theory (ST 17, ST 18).

Editors at *Sociological Theory* have long noted especially high disagreement among reviewers, and in seeking to incorporate race and gender into theory, they have perhaps been at odds with a reviewing pool (and subfield) that has not diversified as has the discipline in general. Table 2 reports on the percentage of men serving as reviewers, editorial board members, and editors-in-chief for all ASA journals over time—figures taken from annual reports—alongside the non-weighted percentage of new PhDs in sociology conferred upon men (data from American Sociological Association 2020). Descriptive statistics about the service of scholars from minority groups in editorial and reviewing roles are not examined here because they were less regularly or reliably reported by editors. Such counts might have been more difficult to compile from information available to editors, or the definition of the population might have been less clear.

The representation of women (and, for the short period where data are available, gender-nonconforming people) in various editing and reviewing roles has lagged behind the composition of the field overall, although some demographic lag in movement into senior editorial roles is to be expected. The general trend has been in the direction of parity. *Sociological Theory* has been an exception—the three-year running average of the proportion of men in its reviewer pool has always stood above 70%. This is, of course, an ecological form of reasoning that is at most suggestive; that most reviewers at the journal are men suggests nothing about the views of any particular reviewer about gender theory. However, compositional considerations of this kind have been observed to matter for the intellectual arc of other fields (see Feeney et al. 2018).

Editors of *Social Psychology Quarterly* have struggled from the start against what they understood as the subfield's substantive and methodological narrowness. Reporting on *Sociometry*'s first year as an ASA journal, Leonard Cottrell proclaimed a pluralist editorial

Table 2 Changing gender composition in reviewing and editorial roles at ASA journals, 1982–2018

Years	% Reviews by Men	% Editorial Board Men	% Editor-In-Chief Years by Men	% New PhDs Men
1982–1984	78.5	73.7	81	59
1985–1989	73.7	65.8	88	52.7
1990–1994	70.4	59.0	70	50.6
1995–1999	67.0	57.6	65	44.6
2000–2004	62.6	52.4	74	40.7
2005–2009	60.6	55.9	65	38.7
2010–2014	55.7	51.0	54	38.2
2015–2018	Not Available	46.4	78	39.1

stance, and expressly challenged the perception that it was to be a journal for quantitative research (SPQ 56). Similar concerns about methodological narrowness or overrepresentation of experimental psychological work were voiced by successors John Clausen (SPQ 59, SPQ 60), Ralph Turner (SPQ 64), Melvin Seeman (SPQ 65), Sheldon Stryker (SPQ 67), Carl Backman (SPQ 70, SPQ 71), Howard Schuman (SPQ 78), and George Bohrnstedt (SPQ 82). It is not until Peter Burke reflected with satisfaction on the growing number of non-experimental submissions in 1987 (SPQ 87) that an editor commented favorably about the diversity of material being published in the journal. But the chorus of concerns about methodological and theoretical narrowness resumed under successors Karen Cook (SPQ 89, SPQ 90, SPQ 91), Ed Lawler (SPQ 93, SPQ 95), Linda Molm and Lynn Smith-Lovin (SPQ 97, SPQ 98), and Cecelia Ridgeway (SPQ 02). Editors sought to broaden the content of the journal through commissioned review articles, book review essays, "debate" formats, introduction, abolition, and reintroduction of the research note format, special issues, and formal appeals in newsletters of other ASA sections. Yet the reports chronicle only one major change in content over this span of nearly fifty years: the supersession of the quantitative experiment by the quantitative analysis of survey data.

At SPQ, more than any other journal, there is the suggestion that editors who defer judgment about the merits of work, and seek to keep authors' interests and reviewers' criticisms in equipoise, are apt to find themselves running a journal whose content reflects the modal intellectual views within a given area of research, even if the editors do not agree with the prevailing view. Most active scholars will have some familiarity with the frustrations that editorial peer review creates for authors and referees. Fewer know from direct experience the frustrations the process may create for editors, one of which is editors' tendency to become the object of "full and public blame for any editorial errors" (SM 04), largely irrespective of where blame might properly rest. This is also a broader quality of the reports, which have always loudly proclaimed editors' strong desire to do their work fairly and impartially. Beneath this vigorous assertion, one hears a muffled grumble that doing fair, impartial work is a mostly thankless labor.

Discussion

Contemporary peer reviewing practices in sociology journals did not arise from a deliberate plan. Today's model is the product of decades of changes, many of which arose from editors' improvised efforts to balance two ends: editors have sought to find effective, expedient models of manuscript evaluation that also afford authors a reasonable chance of receiving a decision grounded in a considered assessment of a work's merit. Efforts to balance these organizational and value imperatives produced significant change over time in who produced reviews, how those reviews were transmuted into an editorial decision, and when and how firmly the editor intervened in the evaluative process. The result is far from a perfect model for judging the absolute or relative merit of submitted work, as editors well know. One may find high-flown language about the virtue (one might almost say the sanctity) of peer review in many places, but certainly not in the 351 annual reports of editors considered here. Editors have been more pragmatic and modest in their claims: as Norman Ryder put it two generations ago, "the test" for a journal editor is "one of survival" (ASR 67).

When journal peer review is compared to other forms of publishing and research evaluation, its most striking feature is the degree of meaningful choice and consideration it affords to prospective authors; other common approaches to organizing the evaluation of intellectual or cultural works simplify the gatekeeping task at authors' expense. (Stated abstractly, gatekeeping situations are those where an individual or organization controls access to a discrete, desirable symbolic or material outcome sought by others.) In publishing, in its various forms, an editor or editorial board decides whether a given piece of writing will be published. Very often, the quantity of available work is much too great to receive a direct, thorough evaluation by the editorial actors who are formally empowered to offer publication. This requires some set of practices to limit the editor's work, either by restricting the quantity of writing to be evaluated, or simplifying the evaluative task. Peer review stewards editorial attention by delaying judgment, enabling editors to allow others' evaluations, to varying degrees, stand in for an editors' direct, complete assessment of a manuscript (Petersen 2017). (However, as the historical exposition has shown, reliance on a large pool of reviewers allowed editors to simplify some of their work, but made the management of the reviewing process a major source of editorial work.) This is one of a handful of common organizational responses to the problem.

In literary book publishing, acquisitions editors manage workload by restricting the flow of manuscripts. Almost uniformly, literary presses in North America prohibit direct submission by authors; they will only consider manuscripts submitted by literary agents (Childress 2017). The number of trusted agents is limited in number. Because agents' income is wholly derived from sales, and the ability to sell is contingent on maintaining working relationships with acquisitions editors, agents have powerful economic incentives to be judicious in commending work to editors' attention (Childress and Nault 2018). In literary periodical publishing, authors are ordinarily permitted to submit their work directly and simultaneously to journals with little or no money cost (on the historical development of this model, see Merriman 2017a). The volume of submissions is therefore very high, and journals typically employ a multi-tiered organizational structure to manage this surfeit. For each genre of work, there will be a staff of readers, who have broad discretion to reject work; readers forward potentially promising work to genre editors, who in turn present the most promising work to the editor-in-chief (Merriman 2017b). In practice, this often results in near-immediate, peremptory rejection of at least 90% of submissions, and at many literary journals, eventual rejection of 99% or more of unsolicited work.

In scholarly book publishing, editors do not have the kind of procedural responsibilities to prospective authors that characterize contemporary journal publishing. Such editors ordinarily make initial judgments about projects on the basis of a proposal and limited sample of work, and may thereby quickly decide whether a given work will be given serious consideration (see, for instance, Luey 2008; Germano 2016). A classic study of academic book editors characterized their key task as "deciding what to decide on," and notes that "winnowing out of manuscripts occurred prior to the formal decision-making stage" (Powell 1985: 81). In all forms of literary publishing, and in scholarly book publishing, an author's existing reputation and interpersonal networks may all figure in this process (see Gerber and Childress 2015).

Although considerations of reputation and rank no doubt shape journal editors' choices about reviewer assignments, these are not formal criteria of exclusion as they

are in these other settings. Such considerations are also an important feature of panel-based grant and fellowship review processes, which seek to advance a form of procedural fairness very similar to journal peer review (Lamont 2009: 240), but tend to significantly restrict the population of potential applicants through threshold requirements and program objectives (Lesser 1999: 130; Lamont 2009). Another difference is that panels seek to manage workload by concentrating it in live deliberations, rather than dispersing it across time and a large number of evaluators. As a result, social interactions and sequence effects may have significant effects on outcomes (Lesser 1999: 128–133; Lamont 2009: 246–248).

In scholarly periodical publishing, there exist means of managing evaluative labor other than peer review. Law reviews permit simultaneous submission, and articles offered publication at one journal can receive an expedited evaluation at other journals. Higher status law reviews with higher submission volumes may therefore rely to some degree on other editors to provide them with signals of the quality of submitted work. In some economics and finance journals, high submission fees are used to curtail submission volume, and some economic models of scholarly publishing propose that fairness or efficiency would be best served by imposing markedly higher time or money costs upon submitters (Azar 2005; Cotton 2013).

When contrasted with the early culling that defines these other publishing models, the notable quality of the standard social scientific editorial peer review model is that it does not rely on formal exclusion of prospective authors nor the rapid disqualification of the bulk of submitted work. Authors, mostly irrespective of their existing reputation or means, can have their work considered at the journal of their choosing, and editors defer judgment until reviewers have carried out what is, by comparison to other publishing models, a very thorough assessment of the work.

Models of evaluation have characteristic strengths and weaknesses. The greatest virtue of a developmental model of double-blind peer review is that it is open to all authors. This openness has significant time costs for editors, reviewers, and authors, but is clearly concordant with two prominent values. First, a belief in social equality is one of the few values sociologists generally share. An open submission model that refers most submissions for review probably comes closer to achieving formal equality of opportunity for publication within the discipline than would other common editorial models. Second, double-blind review gives priority to assessing the work rather than its author. That commitment may promote "universalist" criteria of objectivity or validity of work, such as Broom first envisioned, and also serves to insulate authors from evaluative biases that may attach to professional status, reputation, or personal characteristics. In highly status-conscious enterprises like academic disciplines, the article review process is one of very few that affords even modest protection against the obtrusion of such considerations. As careful self-analysis of publication outcomes at political science journals demonstrated, manuscripts submitted by women enjoy chances of publication comparable to those of men (König and Ropers 2018; Peterson 2018; Samuels 2018)—a contrast to many other facets of social scientific disciplines, where there are clear disparities in citation and recognition of work, as well as hiring and promotion (see Weisshaar 2017).

Although the prevailing editorial peer review model is one that seems well-suited to protecting some of the interests of authors, its value as a means of identifying or developing the intellectual contributions of work would appear variable. It is doubtful that double-blind review is, as its severest critics might charge, an empty ritual. But the model does not serve

all scholars or works equally well: publication invariably involves passing through the ordeal of revision, but the genuine improvement likely to be achieved by revision varies. Many papers would improve incidentally in any process of revision; few papers could not be bettered with renewed authorial attention, and the foreknowledge of necessary revision may of course prompt authors to submit writing in a state that still admits of ready improvement. Many more manuscripts are certainly improved by reviewers' assessments. That empirical studies find that most revision in peer review is "reframing" hardly condemns the process: because academic social science is a collective intellectual endeavor, accounting for how one's work fits into a larger scholarly picture is one of the major tasks of research. This also suggests the kinds of work least likely to benefit from extensive revision: papers that are, in conception, primarily reports on routine empirical investigation, and notably polished, well-developed papers, especially those dealing with normative, theoretical, or conceptual matters where reviewers' judgements of validity or importance may be hardest to distinguish from tastes or leanings.

The labor of revision, when added to the time spent in successive rounds of review, also makes this model very slow. This pace, especially when coupled with commercial models of ownership and distribution of journals, is a potential problem for the prompt development and diffusion of knowledge. Such is the key contention of SocArxiv, a platform that seeks to expand and accelerate access to research by hosting preprints of works in progress, and author versions of published papers that might otherwise be immured behind journal paywalls. The pace of review and revision also makes professional advancement anxious and uncertain: the scholars with the greatest need to successfully navigate the process are new entrants in the discipline, who face short time horizons for hiring and promotion. Publishing pressures shifted to earlier and earlier phases of the scholarly career just as the slower developmental model of reviewing became dominant (Abbott 2019: 10; Warren 2019). Thus, nearly universal adoption of this single editorial model creates extensive labor both where it is warranted and productive, and where it may be needless or professionally harmful. Further, a reviewing model that seeks for evaluative consensus is unlikely to be the most intellectually generative model. Innovative and provocative work cannot be expected to produce immediate consensus, and persistent intellectual disagreement—a major source of friction and uncertainty within the prevailing model of review—can be salutary if organized and voiced appropriately, especially for a large and varied discipline like sociology.

Greater diversity of editorial models may therefore be desirable, both for individual authors and works whose goals do not fit well into the standard reviewing model, and for the intellectual development of the field, given that the dominant model plausibly disfavors certain kinds of work. Pluralism in editorial models may prevent a field from being distorted by the weaknesses of any one of them. There exist successful sociology journals that employ other models. The editorial board of *Politics & Society* evaluates submissions in-house at regular meetings, which yields predictable, short review periods with decisive outcomes, and confines evaluation to a small corps of skilled, experienced scholars. In these respects, it is the current sociology journal whose organization most resembles the journals of the 1950s and 1960s. *Socius*, emulating the successes of large open-access publications like the *PLoS* journals, employs an evaluative rather than developmental model of peer review, taking soundness rather than importance as the evaluative standard. The rapid growth in submissions since its

founding 2015 suggests that this journal has met a definite need; by its third annual volume, it had become one of the largest empirical journals in the field. *Sociological Science* also employs a non-developmental review model, but is as selective as the most august generalist journals. This model has the virtue of speed. It may be added that the intellectual and literary world is rich with examples of major innovations being developed and consolidated by the efforts of strong editors with a free hand: one may say without exaggeration that literary modernism in the English language was the invention of four or five strong editors. Student-run journals—now virtually obsolete, except as outlets for student work—were once more numerous and played a more prominent role in sociology's system of scholarly communication: there are significant works to be found in the archives of publications like the *Berkeley Journal of Sociology*, perhaps the most notable student-run journal still in operation.

None of these approaches could be expected to serve as a general editorial model for a system of social scientific scholarly communication. Board-centered evaluation created enormous workloads when used as a general model. A pure open-access, non-developmental publication model would forego the variable but genuine intellectual improvements frequently achieved through peer review. And though such a model would impose few barriers to publication, it would likely create extremely skewed patterns of attention of the kind that bedevil all manner of online platforms (Fourcade 2016). A journal system composed wholly of strong editors would put a field in danger of atomization into scholarly coteries; such was the situation Leonard Broom reacted against in 1955, when he judged that many of the field's key journals "looked like house organs" (Broom quoted in Abbott 1999: 145). The legal academy offers some indication that student editing, when a universal institution, may yield remarkably risk-averse, status-conscious forms of editorial practice. But each approach does some things well.

A primary constraint on editorial innovation is, of course, the professional and status structure of academic disciplines. An extensive body of research on disciplines, and on higher educational institutions more generally, has shown a powerful isomorphic tendency: such structures tend to converge on a given form of practice even if all the actors are wholly aware of its inadequacies. Further, change in such practices will, under most circumstances, be slow: individual academic advancement involves regularly submitting oneself to the judgment of the more experienced members of a discipline according to the standards those more experienced scholars impose. Those who may have the freshest view of an intellectual field, and perhaps a greater impulse to explore new lines of work, also face the strongest pressures to invest their time and effort conservatively in the oldest means of publicizing their work.

Efforts to change publishing norms therefore stand a much greater chance of success if they are adopted first, or early, by actors who occupy central places in a field, or if they are given the strong, credible endorsement of such actors (Starbuck 2016: 178). Conversations about academic publishing models, especially their relative unresponsiveness to changing circumstances in the twenty-first Century, often possess a degree of fatalism. But the development of editorial peer review itself is an important reminder of how rapidly a good idea may spread.

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Compliance with Ethical Standards

Conflict of Interest On behalf of all authors, the corresponding author states that there is no conflict of interest.

Appendix. Bibliographic Information for Editors' Reports

This article uses a simplified reference system for the editorial reports: reports are cited by journal initials and the last two numerals of the year of report publication.

Journal Initials

ASR	*American Sociological Review*
JHSB	*Journal of Health and Social Behavior*
SPQ	*Sociometry*/*Social Psychology*/*Social Psychology Quarterly*
SM	*Sociological Methodology*
ST	*Sociological Theory*
SOE	*Sociology of Education*
SOC	*Socius*
TS	*Teaching Sociology*
TAS	*The American Sociologist*

Sources of Reports

Editors' reports were published in the *American Sociological Review* from 1952 to 1964. Volume, issue and page numbers for the reports are as follows:

1952 17(6):780
1953 18(6):679-680
1955 20(6):730-731
1956 21(6):759-761
1957 22(6):735-736
1958 23(6):700-701
1959 24(6):873-875
1960 25(6):940-942
1961 26(6):983-985
1962 27(6):918-920
1963 28(6):1005-1007
1964 29(6):898-899

Editors' reports were published in *The American Sociologist* from 1965 to 1972.

Volume, issue and page numbers for the reports are as follows:

1965 1(1):36-37
1966 1(5):283-285
1967 2(4):234-237
1968 3(4):330-334
1969 4(4):348-351

1970 5(4):399-402
1971 6(4):345-349
1972 7(7):26-27

Editors' reports were published in *Footnotes* from 1973 to 2005.

Volume, issue and page numbers for the reports are as follows:

1973 1(6):13-14
1974 2(6):12-15
1975 3(6):12-13
1976 No reports published due to change in reporting deadlines
1977 5(3):9
1978 6(3):8-10
1979 7(2):6-7
1980 8(3):10-11
1981 9(3):14-15
1982 10(3):11-12
1983 11(6):13-14
1984 12(4):10-11
1985 13(4):15-16
1986 14(5):13-16
1987 15(5):14-16
1988 16(3):13-16
1989 17(4):10-13
1990 18(4):14-16
1991 19(4):13-16
1992 20(5):17-20
1993 21(5):20-22
1994 22(6):23
1995 23(4):13-15
1996 24(4):9-11
1997 25(4):13-15
1998 26(3):10-12
1999 27(4):13-15
2000 28(3):14-14
2001 29(3):9-11
2002 30(3):13-15
2003 31(4):12-15
2004 32(4):12-15
2005 33(4):12-15

Since 2005, editors' reports have been published on the American Sociological Association website. This change also led to a shift in the timing of reports: where the printed reports described the editorial activity of the previous calendar year, online reports are dated according to the calendar year of activity summarized.

https://www.asanet.org/research-and-publications/journal-resources/editors-reports/previous-editors-reports.

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