

## A PROPOS PUBLICATIONS

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### THIRTY YEARS OF CHANGE IN THE PUBLICATION PROCESS

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ОБ ИЗМЕНЕНИЯХ В ПУБЛИКАЦИОННОМ ПРОЦЕССЕ ЗА ПОСЛЕДНИЕ 30 ЛЕТ

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**Abstract.** This essay considers changes in the publication process in sociology journals over the last thirty years. Decisions of accept or conditional accept on the first submission have almost disappeared, and the use of revise and resubmit decisions has expanded to cover a wide range of papers. Other important changes have been an increase in the typical number of reviewers and a less

**Аннотация.** В эссе рассматриваются изменения, происшедшие за последние тридцать лет в организации публикаций материалов в социологических журналах. Решение о принятии или условном принятии первоначального варианта рукописи к публикации практически исчезли из практики работы редакционных коллегий. На данный момент широкое распространение получил алгоритм, при

active role for editors. The changes have resulted in an improvement of the quality of published papers, but have also had some negative consequences. One is a slowdown in the process. Another is a dominance of one kind of publication — the full-scale research paper — and a decline of other types, including comments, research notes, and essays. The paper concludes by discussing some changes that might encourage a wider variety of publication types and facilitate commentary and debate on published papers.

**Keywords:** publication process, publication practices, publication, sociological journals

котором автору предлагается внести изменения в текст и повторно отправить его на рецензирование. Кроме того, увеличилось стандартное число рецензентов при одновременном снижении значимости работы редактора. Данные изменения привели к улучшению качества публикуемых статей, однако мы можем наблюдать и некоторые негативные последствия. Во-первых, замедляется процесс принятия рукописей к печати. Во-вторых, в журналах доминирует один тип публикаций — полномасштабные, законченные исследования — в то время как другие жанры научной работы: комментарии, исследовательские заметки и эссе — представлены в гораздо меньшей степени. В заключение обсуждаются некоторые изменения, которые могут способствовать расширению разнообразия типов публикаций, а также облегчить комментирование и обсуждение опубликованных работ.

**Ключевые слова:** публикационный процесс, публикационные практики, публикация, социологические журналы

In its general outlines, the publication process is the same as it was when I received my Ph. D. in 1987: the only obvious change is that it is conducted online rather than through the mail. An author sends a paper to a journal and the journal sends a blinded copy of the paper to several reviewers. Reviewers are usually asked to provide two kinds of comments: the first are responses to fixed-choice questions about the quality of the paper in various respects, while the second is general commentary. After consulting the reviews, the editor makes a decision. The author is informed of the decision and provided with the general commentary, but not with the responses to the fixed-choice questions. If the paper is rejected, the author can send it to another journal, either without change or in a revised form, and the process begins again. The new journal will not have access to the reviews from the first journal, and normally will not know that the paper has previously been rejected. Virtually all journals have rules against simultaneous submission to other journals, and they seem to be respected: I have never heard of anyone submitting the same paper to more than one journal at a time.

However, on closer examination there are some important changes. The first involves the possible outcomes on a first submission. In principle, a paper can be accepted, accepted conditional on some clearly defined revisions, rejected with an invitation to submit a revised version, or simply rejected. By the late 1980s, the first two outcomes had essentially disappeared for first submissions to the top journals: realistically, the only two possibilities were revise and resubmit and reject. However, outright or conditional acceptance on a first submission still sometimes occurred for journals a step or two below the top rank — in fact, my first publication [Weakliem, 1986] was accepted outright. Over time, more and more journals have effectively abandoned outright or conditional acceptance on initial submission: even a paper that receives very strong reviews will be given a decision of revise and resubmit.

A second change involves the meaning of the revise and resubmit decision. Thirty years ago, a revise and resubmit meant that the paper had a better than even chance of being accepted, even at the top journals — at least, that was what I was told, and my own experience seemed to bear it out. A second revise and resubmit was unusual, and in most cases was almost a conditional acceptance: it meant that the editor thought the revised paper had a few specific problems that needed to be corrected. Today, multiple rounds of revise and resubmit are common, and papers are often rejected on the second or even third resubmission. Although there are no formal distinctions among types of revise and resubmit, authors and editors know that the decision can mean many different things: that the paper is likely to be accepted after following the reviewers' suggestions on a few points, that the paper has a chance after extensive revision or additional analyses, or that the paper is a long shot. It is possible that other variants of the revise and resubmit are emerging. A few years ago, I was told that my paper was rejected but that I could submit a revised version as a new submission when the new editor took over. On inquiring what this meant, I was told that paper would be sent to an entirely new group of reviewers. The incoming editor would know the names of the reviewers from the first round so that they would not be chosen to review the new submission, but had not read their reviews and would not consult them when making a decision on the revised paper. I have since heard of a friend who had a similar experience with a different journal.

A third change is an increase in the number of reviews on each round. Thirty years ago, two or sometimes three was standard, even at the top journals. Now it is not uncommon to have five or more reviews. Sometimes this happens because the initial reviewers disagree and the editor seeks additional opinions. However, sometimes journals simply ask for and receive a large number of reviews. The increased number of reviews is probably one of the causes of the increase in revise and resubmit decisions. The more reviewers there are, the more likely that one of them will disagree with the others: someone might see a flaw in a paper that the others praise, or potential in a paper that others see as weak or uninteresting. In such case, a revise and resubmit seems like a safe choice from the point of view of the journal.

A fourth change is that editors take a less active role. When I began my career, editors often gave their own assessment of the paper in the decision letter. If the decision was revise and resubmit, the editor would provide reasonably clear guidance: for example, pointing to some of the issues raised by the reviewers as important and

others as secondary, and sometimes even saying that they disagreed with one of the reviewers. If the decision was to reject despite one or more reviews that seemed favorable, the editor would give some explanation for the decision, and sometimes suggest directions for revision or other journals to which the paper might be submitted. Even if the reviews were clearly negative, the editor might mention the points that seemed most compelling. Whether you agreed or disagreed, at least you knew what the editor thought. Today, many journals use a standard letter that simply refers to the reviews. If the decision is revise and resubmit, the author is told to consider the reviews when revising the paper; if the decision is to reject, the author is told that the reviews provide the reasons. Sometimes reviewers point in very different directions when suggesting a revision, and sometimes papers are rejected when the reviews appear to be favorable, but many journals use a standard form letter even in these situations.

The fifth change is a consequence of the first four: the publication process is a lot slower than it used to be. This is partly because of longer wait times for decisions, but mostly because of an increase in the use of revise and resubmit decisions, especially multiple rounds of revise and resubmit.

From a reader's point of view, there are obvious differences: articles today are longer and contain more references. The expansion seems to be particularly great in the introductory sections: most papers devote a good deal of space to discussion of theory and hypotheses before beginning their empirical analysis.

The changes described above continue trends that began decades earlier. That is, the differences between the 1980s and the 1950s are similar to the differences between the present and the 1980s. Similar changes have occurred in many, although not all, disciplines: Ellison [Ellison, 2002a; 2002b] provides a detailed description of economics and some comparative data.

### *Explaining the changes*

One plausible reason for the change in the publication process is change in technology. As photocopies became cheaper, it became reasonable to ask authors to send in more copies, which made it possible to have more reviewers, and the shift to electronic copies removed all constraints. More reviewers meant more suggested revisions. The development of statistical software made it easier for authors to run additional analyses, and the development of word processing programs made it easier to revise the text. As a result, reviewers felt free to ask for more revisions, and authors were able to provide them.

However, although technological changes gave journals the opportunity to obtain more reviews and ask for more revisions, it is still necessary to explain why they took advantage of this opportunity. One possible reason is an increase in the number of submissions. Although the number of professors of sociology has not increased much since the 1980s, the pressure to publish has grown, so the top journals receive more submissions than they used to. However, on reflection it is not clear that an increase in the number of submissions makes the task of selecting the best papers more difficult. The editor of a journal that does not receive many high quality submissions needs to look for promise in papers and work with authors to improve them. In contrast, a journal that receives many high quality submissions can easily reject most of them as not up

to its standards. It does not need to make distinctions among the rejected papers, so it could have an initial screening based on one or two reviews and give additional attention only to papers with strong reviews at the initial stage.

Sociology journals may have followed the course that they did because sociologists have a relatively egalitarian ethos. In particular, there is growing concern about fair treatment for women, ethnic minorities, and authors at lower-ranked institutions. In sociology, almost all journals have blind reviews, so reviewers' judgment cannot be directly influenced by the characteristics of the authors. This fact means that editors can defend against charges of bias by following reviewers closely rather than emphasizing their own judgment. Obtaining additional reviews also helps to defend against criticism — authors may regard it as unfair if their paper is rejected on the basis of one review, but are more likely to accept the decision if several reviews make similar points. The egalitarian ethos also means that many reviewers are interested in helping authors to improve their papers, rather than in merely weeding out ones that are not strong enough, so they give detailed reviews and suggestions rather than summary judgments. Reviews of this kind make a revise and resubmit decision more appealing: if the paper is already good, it could become better, and even if the current version is not very good, there may be potential.

#### *Positive and negative consequences of the changes*

As a result of the changes in the editorial process, papers have a higher standard of craftsmanship. They are more likely to control for additional variables, contain various kinds of robustness checks, discuss alternative interpretations of the findings, and connect the research to a wider range of literature. Almost all of the papers published in the leading journals today are serious attempts to analyze a question of theoretical or practical interest. Although it is hard to be sure, there is probably less favoritism than their used to be. Connections with the editor have become less important, and the reviewers are a larger and more diverse group than ever before. These are important forms of progress. My discussion will devote more space to the negative consequences of the changes, but that is simply because they are less obvious.

One of the negative consequences has already been mentioned — the publication process takes longer than it used to. This is merely an annoyance for senior scholars, but can be a serious problem for untenured faculty. Going through several rounds of revision for a top journal may take years. If the paper is eventually rejected, the author has nothing to show for it — in fact, the expenditure of time and effort means that the author who almost made it is worse off than someone whose paper was rejected on the first round. Of course, the revisions may make the paper stronger and therefore more likely to be accepted at another journal, but this is not always the case. A paper may become less readable and less focused because it adds so much material to respond to the reviewers' concerns. That is, it may be better from the point of view of those reviewers, but less appealing to a new reader.

The slowdown of publication also has negative consequences for the discipline. The leading journals rarely contain articles on recent events, even very important ones. For example, many sociologists have thought about the 2016 American presidential election, and have offered views on it, but journals have not published much research. It is possible that some will appear in the next few years as papers make their way

through the publication process, but the slow pace of publication is an incentive to avoid research on current events — by the time your work is published, people may have lost interest. As a result, disciplines and journals that have faster publication times have more impact on public discussion. For example, a paper on the 2016 election by Diana Mutz [Mutz, 2018] published earlier this year has received a good deal of attention. The paper was published in the *Proceedings of the National Academy of Sciences*: it was received in October 2017, accepted in March 2018, and appeared in April. It is hard to imagine such rapid publication for a prominent journal in sociology or in Mutz's own discipline of political science.

Another consequence is that published articles now take more time and effort to read, even for professional sociologists. This is partly simply because they are longer, and partly because they include more material that is not essential to the main argument, but was included in order to meet questions raised by the reviewers. I used to read new issues of the *American Sociological Review* and *American Journal of Sociology* regularly — not cover to cover, but I usually found at least one article that I read almost immediately. Now I glance at the titles and put the issue aside. If I see an article that sounds interesting, I usually decide that I don't have time to read it right now, but will try to get to it later. Of course, this means that in many cases I never get to it. There are many reasons for this change, but one is that it has become harder to read an article quickly: you have to be willing and able to make a serious commitment of time.

A final issue is that journals have come to be dominated by one type of article — the full-scale research paper. At one time, leading journals also contained research notes, short comments on previously published papers, and essays offering ideas or observations supported by a few examples (the essays and research notes were not necessarily in different sections: they are my characterization of the nature of articles). Today, research notes have almost disappeared. Comments on published papers still sometimes appear, but they undergo the same review process as original papers. As a result, comments must offer extensive analysis and discussion in order to have a chance of being published. A published comment today usually means a comprehensive effort to refute the original paper: there is no place for a brief observation, even a valid and important one. Essays appear only if they are invited papers.

The dominance of the research paper might be regarded as a mark of improved quality. When journals receive rigorous analyses of theoretically important questions, they no longer need atheoretical data analyses or half-baked ideas to fill their pages. I once observed to the editor of a major journal that essays were no longer published in the leading journals — he agreed, but did not understand why I saw this as something to be regretted. I will address this issue in the next section.

### *What should be done?*

Research papers are valuable, but they require well-developed theory and abundant high-quality data, and there are many topics of sociological interest where one or both of these are lacking. The dominance of the research paper means that these topics get less attention than they should.

Sociology does not have a generally accepted set of principles that can be applied to new questions and used to generate hypotheses. In practice, «deriving a hypothesis

from theory» means connecting it with the writings of people who are regarded as theorists. This is not a bad thing — a discipline needs an intellectual tradition to provide a framework. Moreover, it is not all that restrictive: Marx, Weber, and Durkheim are the premier theorists (at least to most sociologists of my generation) but many others are regarded as having some stature. With a little effort, it is often possible to present a piece of empirical research as a test of theoretically informed hypotheses. However, it is not clear that this is the best use of an author's time or the journal's space. As a reviewer, I often find myself saying that the data analysis is interesting but that the hypotheses seem contrived.

More importantly, the insistence that research be grounded in «theory» can lead to neglect of important issues. Collins [Collins, 1984: 347] says that: «statistical sociology has been used. . . for the most part within the context of parochial social problems issues (for example, did American blacks make more progress in the 1960s than in the 1950s?).» From a contemporary perspective, it seems like the circumstances under which ethnic inequality declines more or less rapidly should be an extremely important question for sociological theory. However, it was not a question that received much attention from Marx, Weber, and Durkheim. Their primary interest was with the leading social problem of their time and place: the effect of industrialization.

One solution is to revive the research note: a paper that does not contain extensive discussion of theory, but launches into the analysis after a brief introduction. I am not suggesting that sociology can progress by simply accumulating a large heap of facts. Rather, theorists (or anyone who wants to contribute to the development of theory) should pay attention to these empirical studies and try to develop explanations that account for their findings. An example from economics is the paper by A. W. Phillips [Phillips, 1958] on «The Relation between Unemployment and Money Wage Rates.» The theoretical background was just a few sentences, with no references, and the entire paper contained only a handful of references to the scholarly literature. Nevertheless, it immediately attracted the attention of other economists, and economic theorists have been offering explanations, critiques, and refinements of the «Phillips Curve» ever since.

The essay also has an important role to play when theory or data are lacking. An essay is an attempt to propose an idea and make a case for it by referring to some examples, but not to conduct tests or provide a systematic review of the literature. When a new issue arises, ideas are likely to be more abundant than evidence. Even if it is not possible to test these ideas, it is useful to put them in circulation. It is unlikely that essays will return to the leading journals, since it is harder to get consensus on whether an idea is interesting or thought-provoking than on the soundness of analysis or the quality of data. However, the discipline would benefit from the development of new outlets for essays.

Finally, there should be more opportunities to comment on published articles. No published article is the last word on its topic: some readers will have questions or objections, while others will have ideas about how the research could be extended. Almost none of this discussion is reflected in journals. With the development of the internet, comments can go online rather than having to take up printed space. Most newspapers and magazines allow comments on their articles, and many of them receive a large number of comments, but simply adding an online comment section probably would



not have much effect on sociology journals. The online journal *Sociological Science* allows comments, but out of the last 100 articles published, only eight have received any comments, and none have received more than four. Of course, one reason for this is that most scholarly articles have few readers, but many of the *Sociological Science* articles have thousands of page views. Another is that most readers come to the article later, rather than when it first appears. After a year or two has passed, a comment is unlikely to spark debate, and may never even be seen by the author, so even a reader who has something to say might not bother. Finally, published comments usually involve an attempt to refute the original paper. As a result, readers may think that authors will take offense at online comments, and authors may tend to respond in a defensive manner if they receive comments. Thus, creating a culture of commentary and debate on published papers will take a positive effort. Journals in statistics have a tradition of including a number of brief comments on selected papers in the same issue as the original. The comments are not necessarily criticisms: they often involve questions, suggestions, and possible connections with other areas of research. They usually make interesting reading, and occasionally a comment becomes well-known in its own right. Sociology journals could adopt this practice. Of course, doing so would raise a question of how to select the commentators. A place to start would be to offer the reviewers of the paper a chance to provide a brief commentary. When I was a Deputy Editor of the *American Sociological Review*, I found the dialogue among the reviewers and the authors was not only interesting, but also helpful in understanding papers and the discipline more generally. Ordinary readers see none of this dialogue. Including commentary would also be a way to give reviewers some recognition for their work. Given the demands on space in the journals, the commentary might appear only online, rather than in print, and starting with a number of invited comments could encourage readers to join in the discussion.

The preceding proposals all involve major changes, so I will conclude with a few simple suggestions. First, editors should try to give clear direction in revise and resubmit decisions, particularly when the reviews point in different directions. Second, they should try to minimize multiple revise and resubmit decisions. Finally, three reviewers (which might be two outside reviewers and an editor or deputy editor) is enough for most papers.

In reviewing the changes in publication practices, I was struck by two points. First, the development of the internet merely continued the direction of earlier technological innovations such as photocopying. Sociology has not taken advantage of the possibilities for interaction created by the internet: in some ways, there is less interaction than they used to be. Second, the changes are the result of a gradual evolution, not a plan. As sociologists, we know that gradual evolution does not necessarily mean all-around improvement. In many ways, sociology journals are better than ever, but we have also lost some things of value.

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