The process of getting published is critically important, steeped in secrecy and mystery, and often poorly understood. Although success in an academic or research career is not the same thing as success in publishing, it is very difficult to get and keep a good job, influence the field, advance science, make a name for yourself, and so on without developing skills in this area. My goals in writing this chapter are to share some of the things I have learned as an author, reviewer, and editor and to give advice and suggestions to others, particularly those who are just starting their careers in research and/or academics.

I first consider three questions: Why should you publish? Where should you publish? What should you publish? I then discuss the distinction (which took me several years and many rejected manuscripts to learn) between good research and good publication. Next, I comment on the roles of uncertainty and luck in the publication process, and their implications for researchers. Finally, I suggest some strategies for thinking about and presenting your research that might aid you in the publication process.

Why Publish?

In the early stages of your career, the question of why you should publish probably will seem a rhetorical one. There is a substantial element of truth to the axiom “Publish or perish,” and you are unlikely to give much thought to why you invest so much time and energy in publishing. That’s a shame, because there is much to be learned by thinking through exactly why you publish.

Once you get beyond the fact that most early career researchers publish because they have to (e.g., for promotion and tenure), you will find many of your colleagues and mentors surprisingly inarticulate and/or unconvincing when they tell you why they publish. When colleagues tell me that they publish to disseminate knowledge, or to help others, or to advance science, I doubt that they are being fully frank (or that they have a great deal of insight into their own behavior). Perhaps I am too cynical, but I think that most publication has a much more personal motive—that is, the desire for influence and reputation. I publish mainly because I want
to influence others and because I want to build and maintain a reputation in the community of scholars who do similar work, and I think most of my colleagues publish for the same reasons.1 These motivations have substantial impacts on both where I publish and what I publish.

Where to Publish

If your motivation to publish is to influence others, it follows that you should always try to publish in outlets that your peers read and pay attention to. A paper published in an obscure journal might as well remain unpublished; it will reach your colleagues’ attention only if it is frequently cited in the journals they read. Similarly, a paper in a nonrefereed journal will not cut much ice; the presumption is that a good paper would have appeared in a better journal, and that papers appearing in outlets that publish whatever is sent to them are probably not very good. This is sometimes unfair, but it is nevertheless true that papers in less selective journals get less attention.

You might notice that I have talked only about journals, not about books, chapters, and so on. In the past few years, I have spent more time writing books, chapters, and the like than articles, but that is because books are more fun, not because they are more functional. In writing a book, you can pursue a topic in depth, pursue tangents that seem interesting to you, speculate to your heart’s content, and more, without the normal constraints of a journal article (e.g., an article needs to be to the point, brief, and precise, which can add up to boring). The downside of book and chapter writing is that your work is less likely to reach the audience you most want to influence. There are some books that reach wide audiences, especially those that appear in series that are most like journals (e.g., Research in Organizational Behavior, Research in Personnel and Human Resource Management), but for the most part, book and chapter writing should be done because it is interesting, fun, and sometimes profitable, not because it is an efficient way to influence the field.

Even if you don’t succeed in publishing in the top journals in your field, it is usually worth your while to submit papers to them. My own experience is that the best journals also give the best reviews. The caveat is that they are also the most likely to be critical (and to reject your paper), and it is necessary to develop a thick skin if you are going to submit papers to these outlets. Nevertheless, reviews from good journals are usually quite useful. I can recall only one paper I submitted to a journal in this category that was not improved by the reviews I received, and in that case I attributed the truly awful review that led to the paper’s rejection to the fact that the one reviewer who provided detailed (and devastatingly critical) feedback was dying of cancer when he reviewed my paper, and simply wasn’t paying attention to what I actually said in the paper. In that case, I wrote to the editor, explained why I thought the reviewer had missed the point, fixed the fairly minor problems noted by the other reviewers, and resubmitted the paper, even though it had been rejected in no uncertain terms. Much to my surprise and relief, the paper was accepted. In any case, the main point still holds: Reviews, even those that question your competence, sanity, and parentage, usually contain suggestions, ideas, and questions that can help you improve your paper.

What to Publish

Most publications can be classified as data oriented, idea oriented, or integration oriented. The first category includes empirical studies of all sorts, as well as quantitative reviews of the research literature (e.g., validity generalization studies). Idea-oriented publications include methodological pieces, review papers that do not rely exclusively on quantitative summaries of study results to make their points, essays, and critiques. Integration-oriented publications include textbooks, summative reviews (e.g., Annual Review chapters), and chapters designed to educate readers in other disciplines.

Most researchers concentrate on data-oriented publications, at least during the first 5-10 years of their careers. This is a sensible strategy for a number of reasons. Data-oriented studies are most likely to appear in journals, as opposed to other, less accessible sources. (There are exceptions. I have a data-oriented chapter in a book I will not name, and I doubt if there are 20 people living who have read it.) More important, data-oriented studies are easier to
assess and predict than are idea- or integration-oriented works. The evaluation and review of data-oriented papers follow a pretty standard course (Is the question important? Do the data address the question? Does the answer to the question get us anywhere?), and experienced researchers can usually predict which papers will be published or rejected. There is much more subjectivity and unpredictability involved in the evaluation of idea-oriented and integration-oriented papers. I have submitted several papers of this kind and found the reviewers perfectly split between those who said my ideas or suggestions were clearly wrong and those who said my ideas were correct, but so obvious that everyone already knew whatever it was the papers said. It is a very risky strategy to hang your hat on nonempirical papers as a means of establishing your career in publication.

It is very tempting for new researchers to concentrate on books and chapters, because these represent almost certain publications (assuming you ever finish them). This strategy can work, but it has some hidden costs. In particular, if most or all of your work appears in what are essentially nonreviewed sources (most books and chapters go through some sort of review, but they are very rarely rejected, no matter how dreadful), people will start to wonder why. The inference is often that you are either unwilling to submit your work to review or that your work never survives the review process. In either case, books and chapters often involve a lot of work for relatively little credit, especially if this is the only outlet you pursue.

Publication Versus Research

It is important not to confuse good research with high-quality publication. Good studies are not always published in good journals (and sometimes are not published at all), and poor studies are not always rejected. As someone heavily invested in the publication process (as a frequent reviewer and an editor), I am probably biased in thinking that relatively few poor papers make it into good journals. In reality, it is probably a good idea for authors to be persistent, even with relatively weak studies. Virtually every piece of research you do will eventually find a home if you look hard enough.

My concern in this section is not with the poor studies that manage to make it into the journals, but rather with the good ones that never get published. There are at least three reasons good studies may fail to appear in print. First and foremost, many researchers can't write (a colleague once neatly summarized a terribly written paper by noting, "This isn't writing, it's typing"). Like any other skill, good writing requires lots of practice and lots of feedback, and too many researchers fail to develop even the most minimal skills in this area. No matter how good the study, a manuscript that is impossible to understand will never be published in a respectable journal.

Second, authors often fail to understand their audiences. Journals differ considerably in their emphasis and scope, and a particular paper might receive very different evaluations at two journals of comparable quality and selectivity. You need to think carefully about who reads any given journal, and what they expect to see in a paper. This is especially important when you are submitting a paper to a journal in which you don't ordinarily publish.

Third, many authors never learn how to read or interpret reviews. The journal review process is a good place to learn humility. No matter how good the initial submission, reviewers are likely to criticize some aspect of your work or its presentation, and many researchers take such criticism badly. In particular, when journal editors write to tell them their papers have been rejected, many authors decide to give up. It is important to understand that virtually every paper is rejected, at least the first time around. In my life, I have had two papers accepted for publication in the form that they were first submitted, and in one case this happened only because the editor needed to fill a particular issue. One key to successful publication is to learn how to read letters of rejection.

There are a few basic variations on the standard rejection letter. First, there is the letter that says the paper is rejected as it is, but that with specific revisions it will be acceptable for publication. For all practical purposes, this is an acceptance for publication, and it requires a pretty significant screwup on the author's part (such as not making the requested revisions) to fail. On the other end of the spectrum is the letter that identifies specific fatal flaws (e.g., the data do not address the research question, there
is an unsalvageable confound in the design) that cannot realistically be addressed. For all practical purposes, the paper that stimulates this letter will never be published, except perhaps in a journal that publishes virtually everything it receives. In the middle is the letter sent to the author whose paper presents some problems, but is potentially salvageable. My advice is that whenever an editor encourages, calls for, or even mentions a revision, do it. Most journals will not invite a revision unless the editor thinks that it has a reasonable chance of success. Once or twice I’ve been burned by following this rule (i.e., have made all the requested revisions and still been rejected), but I still always revise and resubmit when the option is suggested.

Earlier, I suggested that you should be persistent, even with a relatively weak study. At some point, however, you do yourself more harm than good by publishing studies that are clearly flawed. It is difficult (but very important) to learn the difference between a weak study and a bad study, and to avoid publishing bad studies. A weak study is one that is unlikely to have any substantial influence in the field, either because the question is a bit narrow or because the methods do not provide a definitive answer to the question. I am currently working with some colleagues on a series of studies conducted in organizational settings. It has not been possible to obtain large samples in any of the organizations, and we will probably not be able to draw very strong conclusions, no matter how clever our data analysis. I expect that this set of studies will be published somewhere, and that it will make a modest contribution, but I also know that these are relatively weak studies. Bad studies, on the other hand, are ones that have no possibility of answering a worthwhile question, either because the original research question was not a very sensible one or because the methods are simply inappropriate for addressing the question.

Weak studies probably do little to either advance or harm your reputation. They will probably end up in fairly obscure journals, will be only rarely read and even more rarely cited, and very little would be different (for you or for the rest of the world) if they had never been done at all. Bad studies, on the other hand, make you look foolish or incompetent. Another line on your vita is small consolation for a reputation as a sloppy researcher. Some judicious gatekeeping is probably a good idea, and there will be instances in your career (at least there have been in mine) when the decision not to publish, or the failure to persuade any journal to publish, a particular paper will probably do more good than harm, both to the field and to your own career.

**Publication and Luck**

There is an old baseball saying: It's better to be lucky than to be good. Although it might stretch things a bit to apply that same thinking when submitting a journal article, it is nevertheless true that luck plays an important and usually unacknowledged part in the publication process. Early in my career, I had the good luck to choose an area of research (cognitive processes in performance evaluation) that was just getting hot, and it helped me immeasurably. In fact, I was doubly lucky, because my "choice" of a research area was not so much a well-considered decision as an act of desperation. My original area of research was in judgment and decision making, and I had the good luck (although it did not look like it at the time) to fail so miserably in this area that I needed to choose another quickly, and I drifted into the right one.

There is an element of luck in choosing the right topics, in getting sympathetic reviewers, and even in choosing the right journals. One implication is that the publication process is filled with uncertainty, and no matter how good or how experienced you are at it, you can never count on a paper being published. The lesson I take from all of this is that if you are not born lucky, you'd better be born persistent. No matter how good (or how bad) the underlying research, you can never be certain of the outcome when you submit a paper for publication, and you should learn to expect the unexpected. I've lost count of the number of times I thought that a particular paper I submitted was good and one or more journals did not. Knowing that the publication process is fraught with uncertainty, I have learned to be persistent and active. The only way I know to publish a lot is to submit a lot, and to hang in there with papers that I think are really good.
Some Maxims to Write and Publish By

You can succeed in publishing by being very smart (so that the extraordinary quality of your work makes up for the poor quality of the writing, the poor choice of outlets, and so on) or very energetic (if you submit enough things, something is bound to be published), but neither of these avenues seems all that appealing or practical to me. My advice is to learn as much as possible about the strategy of the publication process, and to apply this knowledge to your own work. My understanding of the strategy of publication can be boiled down to nine maxims:

1. **Nobody cares.** This is the advice a boy receives from a gangster in the movie *A Bronx Tale*. It is not a bad thing to keep in mind when criticizing your own research. In writing articles, chapters, and so on, it is critical to remember that problems, questions, and the like that strike you as fascinating are likely to bore other readers. Always assume that nobody cares about your research problem, and that your first task in writing a paper is to make them care. That is, never assume that your problem is so interesting and important that you don’t have to convince your readers of that fact.

2. **No one ever went broke underestimating the American public.** The same is true of your readership. If you assume that your readers will be willing and able to wade through unclear prose and complicated presentations to glean the insights implied in your work, you are in for a rude shock. You have to present your research in such a way that readers can understand with a minimum of effort what you did, why it is important, what you found, and what it means. You cannot be too helpful to the reader.

3. **Add simplicity and lightness.** This was the design motto of the team that created the legendary F-5 fighter, and it is a good motto for both research and writing. If your research question can be settled with a single test, piling on structural equation models, meta-analyses, corrections for deviations from normality, and so on does not make it a stronger paper. Always look for the simplest and most direct way of getting at the question.

4. **Ask Aunt Clara.** If you want to find out whether your writing is clear, don’t ask a colleague who is equally steeped in the jargon of the day—ask your Aunt Clara. That is, your papers should be written in such a way that any reasonably intelligent person would be both willing and able to understand what you say.

5. **Who needs paper?** For me, the acid test of a paper is the feeling that I could stand up in front of an audience and read it to them without feeling stupid, and without confusing them. Read your papers aloud. Better yet, compose them as talks before you write them.

6. **The importance of the information on a written page is inversely proportional to the page number.** This is a hypothesis of mine that many other researchers and writers will hotly contest. My preference is to front-load a paper. The farther you are from the first sentence of a paper, the less important the material is likely to be. The key to a good paper is the problem, followed by the method. The results are likely to be mildly interesting (in a good piece of research, you already know pretty much what you are going to find), and the discussion hardly interesting at all (in a good paper, you already know what it means). A colleague claims that I once said that a discussion section longer than four pages means that the author is trying to hide something. I don’t know if I really said this, but if I didn’t, I wish I had.

6a. **The quality of a paper does not depend on its results.** If the question is truly compelling and the methods are appropriate and powerful, any result should be interesting and informative.

6b. **Surprises are bad for the heart and the vita.** In thinking about my own research, one way I clearly distinguish the good stuff from the bad is that in the bad studies, I had no clear idea how the data would come out (or I had a clear idea, but was completely wrong). If you know the domain well enough to predict in some detail your results, you are more likely to ask good questions and less likely to run studies that fail.

7. **The quality of your work counts, but you also need quantity.** One really good paper is better than three or four or five mediocre or poor ones. You could conceivably make your name, win tenure and promotion, and more, on the basis of one excellent paper, but don’t hold your breath. Really good papers are quite rare,
and we all tend to overestimate the quality of our own work. It is best to assume that none of your papers is so good that people will ignore the volume of your work and pay attention only to its sterling quality. Self-enhancement biases are so ingrained in our culture that even if you did happen to write a gem, you will probably find it difficult to distinguish its quality from that of several other of your works. As I noted earlier, a bad paper may be worse than no paper at all, but assuming that you can identify reasonably interesting research questions and apply reasonably sensible methods to the problem, my advice is to crank papers out at a pretty regular pace.

8. The quality of the journal probably counts more than the quality of the paper. I say this for a number of reasons. First, people pay attention to high-quality journals. When your paper is published in one, the halo effect alone will lead people to pay some attention to it. Second, as I have noted before, good journals usually give the best, or at least the most useful, reviews. They may not like your paper as much as the less demanding journals, but they are much more likely to help you improve both that particular paper and your research in general. Third, people read high-quality journals. Take a look at your own bookshelf and the shelves of a few colleagues, professors, and others, and you will see the same four or five journals on every shelf. You can always find some journal that will publish your work, but unless some of that work gets into the journals that someone reads, you might as well not bother publishing.

9. It's only business. The film The Godfather includes a line like this—one mobster is reminding another not to take his impending death personally. You should keep this saying in mind whenever you read reviews of your papers. Don't take negative reviews personally, don't assume that the reviewers are motivated by personal animus, and don't get offended or discouraged. Anyone who hasn't been called a fool by reviewers hasn't submitted enough papers to the journals. It comes with the territory.

Replies, Rejoinders, Rebuttals, and Rehashes

One final note on the publication process: If you publish enough, you are likely to be subjected to critiques, rebuttals, and so on, and your first temptation will almost certainly be to expose the errors and poor scholarship of your critics. Don't. Our journals are littered with series of comments, rebuttals, and counterrebuttals that do little more than make the authors look foolish and quarrelsome. I have never seen an important issue resolved in exchanges of this sort, but I have seen many reputations go down the drain as a result of intemperate charges and countercharges. You have little to gain and much to lose by responding in print to critiques of your work, no matter how unfair they seem to you.

One of the highest forms of praise you can receive is to have your work criticized in a major journal. This means that someone has read your work, thought about it, and convinced an editor that your ideas and findings matter enough to deserve further attention. Accept the praise and get on with your life. There is an old Hollywood saying: I don't care what they say about me, as long as they spell my name right. This really is a healthy perspective. The next time you see a journal article titled "Comment on . . .", hope that the comment is about you, and make sure they spell your name right!

Note

1. The other major motivation for publication is probably simple force of habit. Once you have been doing this for 10 or 12 years, it is hard to think of not publishing.
Peter J. Frost
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Editors

Rhythms of Academic Life
Personal Accounts of Careers in Academia

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