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10 Things New Scholars should do to get published

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Hello, I hope you're having a great day so far. My name is Duane Wegener; I'm a professor of social psychology at Purdue University.

The title of my talk today is "10 Things New Scholars should do to get published." I'll present my recommendations as a top ten list, though I'm happy to admit that the order of the recommendations is somewhat arbitrary.

I'm making the recommendations based largely on my time as an editor for empirical journals in Social Psychology as well as my more recent editing on one of the Compass Journals, *Social Psychology and Personality Compass*. I realize that different fields operate somewhat differently, and I cannot claim to have expertise in other fields or in their editorial processes. However, none of the following are technical, or related to the psychology field itself. They are general points that I hope will have relevance across many disciplines and will prove helpful to people coming from different training backgrounds.

My students would recognize many of these points as advice that I give them whenever we chat about publishing or about career development. The two issues are intimately related, so you will notice that some of the recommendations are really more about preparation and training than about the publication process per se (though, of course, in our field at least, one's career is very much defined by what one publishes, and what you publish will be heavily influenced by how and by whom you are trained).

So, let's get down to it….

Number ten on our Top Ten list falls into this "preparation" category. It's, "Review every chance you get."

One of the best ways to extend your publishing experience beyond the specific papers you write is to serve as a reviewer yourself. All the better, if you can do so for the specific journals where you'd like to publish your own work. New scholars won't show up on editorial boards right away, but for many journals, half or more of the reviews are produced by Ad Hoc reviewers, reviewers who are not on the editorial board.

If you have published a paper related to the topic of a submitted paper, that journal editor may well find your name and contact you asking for a review. They might also find your advisor or senior colleague who has published in the area. And if that person is unable to complete a review themselves or if they are willing to help give you experience, they might suggest to the editor that you be involved in the review. Many journal editors are happy to have new scholars involved.

Take those opportunities seriously. We had a long-time editor on the faculty of the graduate program where I was a student who told us that he explicitly sought out new people in the field for reviews. He used those reviews to learn how these new people thought and to evaluate whether these new scholars were likely to make a lasting impact. For better or for worse, the quality of the reviews written by these beginning scholars probably helped to determine whether that particular editor sought out papers to read that were written by those scholars and also whether he would invite those beginning scholars to smaller conferences that he organized.

Now, of course, one does not only complete reviews in order to try to impress an editor. Perhaps the most important part of reviewing, at least in our field, is to receive copies of the other reviews once a publication decision has been made. This would be true for empirical papers and also for review papers. Carefully reading the other reviews gives one a window into how other people evaluated the same work. Did they see the same issues that I did? Did I agree or disagree with the others concerning which issues were most important? Which reviewer comments were used by the Editor to make the publication decision?

These pieces of information help one to socialize into a field – to see for a particular journal and (if you see themes across journals) for that particular field which types of theoretical points are viewed as contributions and which kinds of evidence are regarded as necessary or sufficient to support those points. Over time, you'll probably find that different journals have their own characters (they may shift at least slightly over different editorial teams but they'll have their own characters). I'm not suggesting that you treat the other reviews, or even the decision letter as the 'correct' view of the paper. The review process is fallible but over time being involved in that process will help you craft your own work in ways that are more successful.

Being able to take the perspective of a reviewer is an invaluable skill as you scrutinize your own papers. And how better to get inside the heads of other reviewers than to get to see their reviews (which you can, at many journals, if you are also one of the reviewers).

In my opinion, at least, it is crucial for graduate programs to incorporate journal reviewing in their training program. It helps students to step back and critically examine their own work from the eyes of the people who will read their articles. Even if you find yourself in a program without a substantial reviewing component, you can work with your advisor or maybe even other students to increase your reviewing experience.

Number 9: "Write like an expert in the areas where you publish." Of course, you should BE or should become an expert in the research areas you address in your own work, so perhaps some parts of this recommendation go without saying. However, you can be an expert without writing like one, and you should leave no doubt for readers that you well understand the theories and research that are relevant to the work that you are trying to publish.

This means directly addressing the prominent theories in that area. If a prominent theory forms the background for an area of research but is not the focus of the current research, cite it as background. Leaving that work out of the manuscript entirely may make reviewers (especially those reviewers who might take that theoretical perspective) wonder whether you know that work. Even better, if there is a clear point of departure for your research from the previous work that has been done in that area, make that point of departure clear to the reader.

The reader of your work should never have to guess at the major point of your paper. Whether it is a "transformative" point for the field or an incremental extension of an existing theory, the nature of the point and the data used to make the point (if it is an empirical paper) should be explicitly drawn out and repeated a number of times throughout the manuscript.

Recommendation number 8: "Make a point that goes beyond the existing data."

Don't forget the forest even if you are studying a specific tree. In an empirical paper, your study may address a specific type of person, group, or situation. However, in most scientific disciplines, we are in search of principles that can be generalized that have implications far beyond the specific observations in our studies.

I'm not suggesting that you speculate wildly or that you draw inappropriately broad conclusions. However, even a very specific study often makes a point that could have broader implications IF the processes at work are similar to those that operate across these other settings. You can acknowledge the dangers of over-generalization while still reminding people of the implications of your theory or findings might mean for conceptually related settings.

It might require additional testing or inclusion of other related theories, but don't forget to remind the reader of the broader theoretical forest to which you are contributing; even if you examined the implications of your theory for one particular tree in that larger forest.

Recommendation number 7: "Strive for consistency in your story."

Nothing undermines a new theory faster than inconsistent application of the theory across findings. Some of the strongest papers take two seemingly inconsistent findings and develop and test the theoretical construct that says when one result is obtained and when the other result is obtained. This kind of theoretical advance makes the seemingly inconsistent findings consistent within the context of the new theory.

But when there is no organizing theory of this type, people often focus on the result that follows from their theory and ignore the result that seems inconsistent with the theory. Worse yet, I have seen some people publish inconsistent results in different papers without mentioning the "other" result in the second paper published. Quite simply, this borders on dishonesty, even if the reviewers of the second paper don't catch it. It is in print for all to see, and it will eventually be discovered.

Now, of course, I realize that people's understanding of a phenomenon might change over time. That's fine when that happens, but it is best if the result itself is at least replicable (i.e., it has been found two or more studies previously) before one goes off to develop a new theory to account for that effect. Replication of effects before they are published the first time can help to ensure that one does not develop unnecessary theories or theories that have to be changed within the context of the next study.

I believe that even a productive scholar that continually changes his or her theoretical account in a particular domain will not have as much lasting impact on the field as a scholar who is perhaps more measured and conservative in making sure that there is an effect to be explained but then develops a coherent relatively stable theory.

This issue of consistency can relate to many aspects of a manuscript. In addition to consistent use of theory, one should not present different types of analysis for tests of conceptually similar effects. The inconsistency in the analyses makes it look like one is "picking and choosing" only the analyses that support his or her preferred theory. In many situations, there may be more than one statistical approach that can address the question, but inconsistent use of those approaches can raise suspicions on the part of reviewers, editors, or other readers.

Recommendation number six: "Don't expect the reviewers or the review process to finish the manuscript for you."

I've seen this on occasion at the top empirical journal in our field. Sometimes an experienced researcher who should know better, submits a weak initial effort, seemingly in hope that the reviewers or editor will tell them which study or studies to do to make the paper publishable.

Another version of this is when people take their data and submit it first to the top journal, regardless of whether those data or the theoretical point merit publication in that journal. It's as if the person simply starts with the top journal for every paper they write and then work their way down the journal hierarchy as the paper is rejected from each of the top journals.

Both versions of submitting papers that aren't ready for publication (at least not at that journal) waste a lot of people's time. Don't do this.

Having a paper ready for submission means more than having the right match between the point of the paper and the journal. It also means that you have put the time into writing the manuscript well, not only in terms of clarity and completeness, but also taking the extra time to make sure there are few typing errors, that all of the citations are included in the reference section, and the like. All of these things have to be done to make a paper ready to go for the journal where you want to publish.

Recommendation number 5: "Submit to the right journal."

This point is closely related to Recommendation 6, but I think it merits special emphasis. It taxes the publication system greatly when submitted manuscripts do not match up with the journal.

First of all, take the submission guidelines of the journal seriously. If it is an empirical journal, don't submit a review paper. The journal won't publish it, and it might take months for you to get a final decision if they still send the paper out for review.

Use the "correct" (standard) format for that journal. Failing to do so makes reviewers question whether you know this literature if you apparently don't even know the standard format for manuscripts in the field.

My previous points about data being appropriate to the status of the journal would also fit here. Don't just start with the "best journal" for every paper you write. And don't use the journal review process as research planning. Submit the paper where you think it should be accepted or continue your research and writing until it merits the journal where you'd like to be published.

As a new scholar you'll be better served by submitting strong papers from the beginning and submitting them to the appropriate journal. Not every empirical paper makes the point that merits publication in the top journal in one's field and not every theoretical point will merit writing a book on the topic or even publication in a theory orientated journal. The review system overloads and bogs down when manuscripts are submitted many times before finding the appropriate publication home.

Recommendation number 4: "Treat research like investing."

This may be more of a career-development recommendation than a publishing recommendation per se. But I think it is crucially important, especially for empirical disciplines in which one is collecting data in the lab rather than conducting secondary analyses of archival data. When one is manipulating variables, and constructing measures for one's own study, there may be many ways in which to change the manipulations or measures in search of evidence that supports one's theory.

But focusing on only one research question or theory is like investing only in one company's stock for one's retirement savings. If that one research question or theory is productive, one's career can start off very well. However, even if that research idea is a good one, support for that idea may not be forthcoming, and one's research career will be better served by diversifying those research efforts to address different research questions (often using more than one research paradigm).

This does not mean that one has to work in totally different fields, or to address totally unrelated questions, or work with multiple different advisors or collaborators. I'm just suggesting that focus on addressing one particular question has its limits. Far too many new scholars fall behind or struggle to become productive because they have trouble finding support for the theory they started testing in graduate school (or as a new beginning faculty member).

This may be less the case in disciplines where researchers routinely conduct secondary analyses on archival data, because such analyses often involve testing multiple hypotheses using the same dataset. But even in such disciplines, one would not want to focus too heavily on a preferred hypothesis to the point of ignoring other potentially productive questions or methods.

Also, just as investors make consistent investments over time to average out the costs over time, research productivity requires consistent effort. There are some times in which people make great progress in a short period of time. But this is often the result of consistent effort over time. One simply cannot plan for a burst of research productivity any more than one can plan for a quick run up in the stock market.

Recommendation number 3: "Collaborate (with productive people)."

Many people extol the benefits of collaboration for science more generally and for the integration of perspectives to address grand challenge problems. There may be something to these claims, at least for some problems and for some combinations of disciplines needed to effectively address a particular problem.

But here, I'm really referring to the benefits of collaborating with people in one's own discipline. As a new scholar, it is important to observe the ways in which productive people achieve and maintain their productivity. Having other people involved in one's work also increases accountability and provides positive "pressure" to get work done. Especially when you find a good collaborator (or more than one), I'm also a firm believer that more minds make the end product better. Plus, when each member of that team takes the lead on different projects, there is greater opportunity for each member of that team to be involved in a larger number of projects and, therefore, publications.

The potential costs, of course, include that not all collaborators work well together. Teams that don't work well together will end up draining one's time and energy - resulting in less rather than more productivity. Thus, it is important to use opportunities for collaboration as a way to search for collaborators that work together. When you find good collaborators, stick with them and maintain those working relationships over time. When the collaboration does not work, it will probably be better to finish up the current project together but to cut it off there and seek other opportunities. This can be difficult if collaboration with one's graduate advisor is not working well. And early in one's career, it may not make a lot of sense to turn one's back on multiple projects that are started but not complete. It may be necessary to tough out those projects and force oneself to push them through to completion. However, if the collaboration is not working well, it may be best not to start new projects beyond, say, the dissertation. It may be more productive in the long term to establish working relationships with others.

It is also true that one is unlikely to develop a strong research record if one is always a minor author on multi-author papers. Thus, collaboration can undermine perceptions of one's research abilities if one does not also have papers where one is the major author (in social psychology that would be the first author on the paper). Thus, it is important for each member of a collaborating team to carve out his or her own niche - their own area of expertise where they are consistently the major author on that topic.

You have to develop a research identity so people know who you are and what you study. You can definitely do that within a productive research team.

Recommendation number 2: "Anticipate editors and reviewers."

This point is clearly related to previous points about putting oneself in the position of a reviewer when developing one's papers. But there is a bit more to it than that.

Know the editorial board of the journal. These are the people most likely to handle your paper. You can't always fully anticipate who the reviewers will be, but you can usually identify a key person or two who is likely to see the paper at some point. When this is possible, write the paper with that person's views in mind. Directly address the way they are likely to think about the research. Cite the relevant research that follows from the person's theory or perspective, and then make clear how your research or theory fits with or goes beyond that previous work.

How you do this may depend on the point you want to make and also on the strength of your case for a different approach. You can be more direct about pitting the theories against each other if the theories clearly conflict, than if you can imagine a way that the other researcher might try to use their theory to account for your results. You might also be in a stronger position to make a clear distinction if you have data that provide clear and strong support for your theoretical approach. But even in these cases, you'll do well to anticipate (and then address in the paper) how likely reviewers or editors might approach your data differently than you would.

And the number one recommendation for new scholars seeking to publish is to "Take initial reviews as the first word, not the last."

Manuscripts are rarely accepted for publication without revisions, often substantial revisions. Thus, even seemingly negative reviews and a rejection letter from an editor can provide the path to eventual publication.

In a good decision letter, they will make it clear whether they believe that revision of the manuscript can result in a publishable paper. If you can't tell what the editor thinks about this, you should ask. The job of the editor is to communicate to the author what would be necessary to make the paper publishable (if they think the paper has a chance at their journal) or to explain why the manuscript does not have a chance (if they believe that it does not).

Of course, even when an editor or reviewers finds major problems that keep a paper from being publishable in its current form, this can tell you directly what additional research is needed to better make the case (or, if the issues are more theoretical, which issues you need to address more clearly in a future version of the paper). If you are submitting to an empirical journal, once you add data, you can resubmit the paper. Of course, when you do so, you'd better have directly addressed the concerns from the previous decision letter and reviews. In most cases, it probably also helps to submit a cover letter that specifically addresses the issues from the previous decision letter and reviews and explains how the new version of the manuscript addresses those issues (or, if they are not addressed, why those issues should not be an obstacle to publishing the paper).

Your job in a revision is to demonstrate that you have been responsive to the previous reviews and decision letter and, within that context, to make the point that the new and improved version of the paper is now publishable (i.e., that it makes a contribution to the literature that merits publication in that journal).

As a new scholar, it is easy to get discouraged by negative reviews or decisions. It's not a perfect system in that not every reviewer is an expert in the area where they are reviewing, and not every editor makes the "right" decisions or explains their decisions well. But it is the case that the reviewers are usually the types of people who are most likely to read your paper when it is published. Reviewer reactions can often provide a very valuable indication of which ideas or details are clearly explained in your papers and which are not. They can also identify the questions that readers are most likely to have when reading your paper, so even if some of their points are not well-reasoned or reflect less than expert knowledge, those comments can point you to places in the paper where clarification would be helpful.

The review process is a process, so it is important for you to treat it as such. When you receive reviews and a decision letter, read them carefully to determine whether you can address the concerns, and, if so, how. Are they simply writing revisions, are they new analyses, do they require new data? If new data would be necessary, you might have to decide whether you can collect those data quickly enough that it is worth waiting to resubmit the paper to the same journal with the new data. At some points in your career, waiting may be fine. At other points, you really can't afford to wait.

You might have to identify other possible outlets where the current data might be sufficient (with writing or analysis changes that address the other concerns from the reviews and decision letter). In either case, reviews are nearly always useful. Take them as a snapshot of reader reactions and revise the paper to change those reactions to what you would want those reactions to be.

Finally, I would be remiss if I failed to mention that some decisions simply call for some type of communication back with the editor. If crucial points in the decision letter or reviews are demonstrably in error, it is perfectly appropriate to respectfully point that out to an editor. I wouldn't recommend shooting off emails ten minutes after one receives a decision letter, however. This is another place where collaboration can come in handy, because people can work together to craft a response that is tactful and effective. Taking a little time to take in the reviews and think about it first makes a lot of sense.

And this approach is really only worth following at all if all if the major issues that are raised in that decision letter, but were in error, could be addressed through simply writing or analysis types of revisions. If you will need to collect new data anyway, it does little good to point out to an editor that he or she is wrong about something. You'll be better served by reserving those efforts for situations in which a writing revision would now have a chance at publication where the original rejection of the paper was based on a misunderstanding or a demonstrated error.

Of course when you point out such things to an editor it generally helps to take the blame for not making the issue clearer in the original manuscript. People generally do not react very well to "you made a mistake and now you must publish my paper." That simply doesn't work.

Let me wrap up by mentioning other related topics to be covered as part of the Compass Virtual Conference. I realize that many of my comments today have had a distinctive empirical bent to them. At least in my field, the larger share of publishing is definitely empirical articles. Some of my points would apply equally well to theoretical articles like those published by the Compass Journals and of course you can also tune into Michael Bradshaw's talk on why and how to write a good review article.

Of course Greg Maney's talk on how to survive the review process is also directly relevant. Many of his points and suggestions fit reasonably well with the points that I have tried to make today. Where we disagree, there may well be more than one way to approach the review experience and you should take each of those into account and decide how you want to deal with that situation.

I hope this discussion has been helpful, and I wish you all the best in your publishing efforts. Have a good day.