What an Editor Wants to See in Your Research Paper

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Each paper is different, and each journal editor has unique preferences, so you can never know beforehand what exactly will make the difference between publishing or not publishing your research paper in an economics journal. Sorry, I cannot offer any magic solution. Usually, it just takes lots of hard work (*before* you submit)! However, I was the managing editor of an economics journal for enough years that I might be able here to offer some inside scoop on what some editors might be thinking.

You've probably read enough economics papers already to see a pattern, a basic outline and common formula. It certainly cannot be a stream of consciousness, but must have logical section headings such as: introduction, literature review, theoretical model, econometric model, description of the data, discussion of identification, basic results, robustness checks, and conclusion. In addition, you can undoubtedly guess already that the paper needs to be written in good English sentences, in good logical order of development. You could even view yourself as a trial lawyer, building a case that needs to be convincing to the reader. But what else?

Whenever I start reading a new research paper submission, I want to know very early what is the point, or why am I reading it. You should provide only one paragraph of general background on the issue, and maybe only *one* additional paragraph summary of key published attempts to address that issue, but please have a sentence that begins "The purpose of this paper is …", while still on page one! Then page two can return to background information, more literature review, a summary of your own research design, how it differs from existing literature, and summary of your results – before the end of the three-page intro. Yes, that's a tough set of requirements for a three-page intro! You won't get it, the first time. Therefore, write the intro first, just in order to think hard about what you are going to say, then do the research and write the rest of the paper. Then return to rewrite the intro completely – after you know what the paper is really about.

How to Identify Your Topic

You will be tempted, no doubt, to look for a research topic by reading some published papers in your area of interest. You already know how to read published economics papers, how to replicate them, and how to interpret them. So when you read the next published paper in your general topic area, you might say to yourself, "oh well, that's been done". Then you can read another paper in your general topic area and say "oh well, that's been done, too!" The trick is how to read existing literature to identify a question *not* yet addressed or an approach not yet taken.

Sometimes a published paper will list and discuss some "possible future extensions" that can serve as suggestions for your own paper (if not yet undertaken). But many authors try to make their paper sound like the be-all and end-all, the final word on some topic. Let me assure you that it's not! Read the paper carefully to look for what is not said. What *other* question is not addressed? What simplifying assumption is really driving the results? How can that assumption be relaxed and still solve the model? What other model not mentioned here could be used as an alternative to provide a different answer to that question? What other data set? Is that variable really exogenous? Why not? What could serve as an instrument? What is the author *not* saying?

In my own research, I have several times started a little project that I thought was just a short "comment" on an existing paper, but which turned into a full-fledged paper on its own. To a certain extent, even if you don't know what to do, you just need to get started. Take a trial run at it, see if it

works, think about what else you could do, and elaborate the model on your own. Chances are that *something* you do will be different from what others have done, so your new paper will differ from existing literature in some respect – we just have yet to determine exactly how it differs and how important that difference might be.

How to Narrow Your Topic

An undergraduate once came to my office to talk about his independent study, and I asked him what topic he would like to research. He said "Well, maybe agricultural price supports?", to which I said "Fine, but can you narrow the topic?". After some thought, he suggested "Oh, I'm from Wyoming, so I could write about agricultural price supports in Wyoming!". The reason I like that story is that it's a perfect example of how not to "narrow your topic". You could choose a particular county in Wyoming, or even a particular farm, but that still would not identify the topic!

You need to narrow your topic by choice of the question you want to address. What *about* agricultural price supports? Do you want to know how they affect international trade patterns? How they affect farmers' incomes? How they affect rural to urban migration? How they affect economic efficiency in agricultural markets? You can't address all of those questions adequately, so choose only one. And please do not just provide information for its own sake; it should be in response to some real concern. Tell why you think it is an interesting question. For whom is it important? Why do they care? What are the possible ramifications of different answers? This process will not only help identify a good topic, but also help motivate it.

Even if you identify the question, chances are that some other economics papers have already addressed it. That's fine, you just need to do something different. However, I hate the phrase "gap in the literature", used by many authors trying to motivate their paper. For example, "This question has been addressed using models with competitive behavior, and models with Cournot behavior, but nobody has ever addressed this question using a model of Stackelberg behavior, and so this paper fills a gap in the literature by building a Stackelberg model to answer this question." So what? Not every "gap" needs to be filled! What is the purpose of doing it that way? When might the Stackelberg model be more appropriate? How might you expect the answer to differ by using that model instead of the usual models?

The Research Process

The next step in doing your research is to choose the approach you will take to answer that question. Decide what kind of model, what simplifying assumptions, and why those assumptions are appropriate. Why is that the best approach? What data are needed? Where are those data available? Then you carry out that plan: build the model, collect the data, and answer the question. When the original plan doesn't work, you will need a new plan, so you must explain why the ideal approach did not work and why your alternative is appropriate. Then, of course, you need to use the new model to generate results and think about their implications.

You probably know all that, but the reason I repeat those steps here is to point out that your research paper simply reports on the research process! In other words, the same basic steps apply to the thought processes, the actions in your research, *and* to the logic of the presentation in the paper. It's really just documentation, or a travel log, where you report on what you did along the way, and why. Explain everything as thoroughly and clearly as you can, in plain language, with no repetition, and no wasted words. Use footnotes or an appendix for extra detail. Don't try to be

dramatic; unjustified claims reduce your credibility. Don't use flowery language to try to spice it up; you need the question and results themselves to be interesting. The paper may seem long and dry, but that's what you need to cover all necessary possibilities and substantiate your results. This is "formula" writing, but it *works*! You will find this formula in the best published papers, so follow their lead.

Push on All Fronts Simultaneously

Writing a research paper is not a linear process, where you proceed through formulation of the question, research design, data, estimation, and results. If you take all those steps and then write the actual paper, you will find that some of the steps are hard to justify and should have been done differently. It is the act of writing itself that forces you to think hard about what you are doing, so don't leave writing until the end! As you formulate the research question and motivation, write the intro. As you read the literature, write the review. As you collect the data, write the description. As you think about each step, write it down, so that you can see if the argument holds together. Then after you get results that seem questionable, you will need to find new data and procedures, which will require new descriptions. Then, when you finally finish all the research, you will realize that your original question and motivation have morphed, and that you need a whole new introduction!

Preparing Your Paper for Submission

Way too often, I have read submissions to my journal that were simply "not ready for prime time." The sentences are not well constructed, words are misspelled, some steps are not explained, or the text cites previous papers that are not listed in the references. These kinds of errors are not a good sign; they always make me wonder, "If this author can't handle even the simple problems, then how can I trust him or her to handle the big problems?" Therefore you need to be careful. (That may seem obvious, but so many authors are *not* careful about these "minor details".) After your last revision, then go over your whole paper with a fine tooth comb.

When you are finished, and you think you are finally ready to submit the paper to a journal, then don't! When you do submit the paper, the editor will likely send it to two different experts in the field and ask them to read it, provide comments, evaluate the paper, and advise him or her on whether to publish it. Those referees will find something wrong, and recommend against publication. Therefore, *before* you go through that process, you first need to send your finished paper to your advisor, your mentor, your friend, your colleague, and your spouse or significant other. Ask each of them to read it and provide comments, as if pretending to be an official referee. Then you can get some idea of what some "outside" referees might think about what you have done, get their comments. Ask those readers for advice about how to fix the paper, and which journal would be best for submission.

Then, the editor of that journal will undoubtedly reject your paper! That's fine, don't think about it, just send it to another journal. After that next journal rejects your paper, just send it to a third or fourth journal. The fact is that the profession includes lots of journals, so you're bound to get published somewhere. The average time is about three years. When an editor likes your paper and thinks it might be publishable, he or she will send you a rejection! Read that letter carefully, because it might say "I'm afraid I must reject your paper, but if you can address all of the referees' suggestions, then a suitable revision could be resubmitted." That's great news, you're on your way!

Good luck, and enjoy the process!

Appendix: *Starter Ideas*

Read the newspaper and recent economics journals; see what's hot and see where assertions are made without adequate evidence.

When you read a theory paper, think about what assumptions are driving the results, and how to make alternative assumptions that might generate different results.

When you read a theory paper, think about how to test it.

For an empirical paper, think about "mistakes", or even just alternative procedures to test or measure the same effect.

Think about other propositions that can be tested with the same data.

Think about other data that can be used to test the same proposition.

Update the data to a new year, to a new country, or to add observations for more years or more individuals. However, always start with replication.

The "Introduction" is key; write it first, to get thinking, and rewrite it again at the end.

Use only the first paragraph to state the question and describe its importance. Don't weave around, be overly broad, or use prior literature to motivate it (the question is not important *because* so many papers looked at this issue before!).

Then use the second paragraph for a summary of the most relevant literature (not a full section!). Hint: use present tense, to be consistent. "Smith (1986) presents a similar model, …".

Next, while still on page one, the third paragraph must begin: "The purpose of this paper is ...", and summarize what you actually do.

That sets you up for the fourth paragraph, which lists "The contributions of this work" – relative to that prior literature. Clarify what you do that's different.

The fifth paragraph then summarizes your results. Tell the *answer*, so they know what to expect, and how to think about each step along the way, what's driving your results.

In the sixth and final paragraph, as an aid to the reader, plot the course for the rest of the paper. "The first section below presents a theoretical model that can be used to generate specific hypotheses. Then section 2 presents the econometric model, ...".