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Concerning: **some idiosyncratic ideas about the prospectus**

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I'm often asked what a good prospectus should do. I guess that every faculty member has a slightly different view on this, which is why I stress the idiosyncrasy of these comments. The best way to clarify what a prospectus should be is to look at a number of successfully defended prospectuses

The purpose of the prospectus is to determine if the argument of the proposed thesis is viable in the sense that with a reasonable amount of work (given the capabilities of the candidate) it seems likely to yield a valuable contribution to knowledge in a form acceptable as a doctoral dissertation. The prospectus and the defense are thus a way of insuring candidates against the risk of embarking on a major commitment of time without some consensus on the committee that the project is promising and the candidate prepared adequately to undertake it.

A good prospectus is one which will allow an intelligent judgement to be made on the viability of the project. This means that it will reveal not only the strengths of the project but also its shortcomings and unresolved problems. Thus it should include (prominently, in the first few pages) an argument which is coherent and would be *interesting, if true*. Often this argument will be of a negative nature, namely to show that a widely accepted claim by reputable economists is false, and that a more convincing claim is supportable. If there is any doubt about it, the importance of this demonstration (its implications for policy, or for other important claims, for example) should be made clear. (This may justify including some review of the relevant literature, but literature reviews should be addressed precisely to the questions to be raised, not an aimless meandering through all the stuff you had to read to get to where you are.)

Then the prospectus should carefully develop reasons for believing that the questions you wish to ask can be answered satisfactorily by some reasonable standard. This part will normally include research of an exploratory and preliminary kind, some initial regression equations or data analysis, for example. If a new data set is to be used you must demonstrate that you have access to it and know enough about it to have confidence that you can use it for the purposes you propose (one way to do this convincingly is to do some initial data analysis with the data set.)

If you are going to collect new data you should indicate why you think it will allow you to test the hypotheses you are interested in . It helps if you are quite specific

in stating exactly which hypothesis will be tested for example by which estimated coefficients, in the cases of econometric work.

In thinking about how to write your prospectus, imagine that you are writing it for a very well read specialist in your field who holds a viewpoint different from your own, but who is not hostile to your argument. Think of all the objections that a knowledgeable critic might raise. You will presumably not be able to answer all of these objections, and your prospectus should be candid about where you have doubts (otherwise the committee may mistakenly approve the document and send you off on a wasted two year project resulting in an indefensible dissertation.)

When I review a prospectus I often ask three questions: is there an argument here which might eventually be published in a leading academic journal in the relevant field? does the candidate have the skills (language, mathematical, econometric, country knowledge) to carry out the project in a scholarly way? and...does the argument connect (logically, metaphorically) to matters of importance?

Remember, the dissertation should do a job for you; its not your life. It should help you find the position you want and to publish a couple of first rate articles. It is not the first draft of your collected works. A dissertation of 60 pages is more than enough if the pages are good. It is to be -- in the old guild usage -- your masterpiece: that which you use to demonstrate your capacity to become a master in your trade. But like the medieval sense of a masterpiece, it should not be confused with your life's work, or your best work. And contrary to the suggestion of the more modern usage of the term masterpiece, it does not have to be perfect.

Being thoroughly familiar with the literature in your field, particularly the new work, not sufficient, but it is necessary. So you should do two things. Spend some time reading the relevant articles over the past five years or so in the major journals in your field as well as the top journals in economics (the *Journal of Economic Perspectives* is a good place to start, along with the *Journal of Economic Literature*). As important, carefully check over the lists of available working papers from the main institutions contributing to your field. Working papers are leading indicators of what will be in the journals two years hence. Get any and all working papers which seem possibly related, read them and follow up their references.

Finally, when you have an idea that interests you, don't be too hard on it. New ideas, like infant industries, need a little protection. If it excites you, give it a chance, and don't shoot it down with "somebody must have already done this" or "this is obvious," or finding fault with it because you haven't had time to develop the idea

fully.

It takes some courage to try to write something new about something you care about, and to expose your ideas to well informed critics that do not share your priors. But that's what you have to do. That's what science is all about.

