Dissertation Dilemmas

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Everyone with a PhD must have thought long and hard about how to conduct dissertation research. Everyone currently in graduate school must contemplate the same topic. Those two groups include virtually everyone reading the *Observer*. In my own case, conceiving, proposing, conducting, presenting, writing, defending, and publishing my dissertation consumed about one and a half years of my life, maybe more. For all of us, it is a crucial rite of passage into academia. Despite its critical importance, I see curiously little discussion of dissertations and how they should be approached. Different universities, disciplines, sub-disciplines, and even individuals have diverse opinions on what a dissertation should be. I will consider some issues in this column, continuing reflections on various topics in academia during my presidential year.

In most descriptions, a dissertation is supposed to represent “a significant new contribution to scholarship” to be made for the receipt of a PhD. In whatever topic the student has chosen to investigate, he or she is supposed to develop expertise in that area of inquiry, and the dissertation is supposed to move knowledge forward.

Now, for those of us who have served on dissertation committees for many years, the question arises: How far forward? Let’s face it: some dissertations report minimal progress, and perhaps even the term “progress” may be a misnomer in some cases. Faculty serving on dissertation committees vary in their criteria for exhibiting progress, and honest differences of opinion can exist. What about the well-designed dissertation that produces null results that cannot be readily interpreted? Does the student deserve a PhD for working earnestly for two years, and yet not making a contribution to knowledge? Or suppose you are the outside reader on a dissertation that did not produce interpretable results, but you suspect the student had really bad advice on the conception and design of the project. If the committee approved a flawed design at the outset, should the student be held liable at the final defense by others – the outside readers – who were not involved in the long process from proposal to dissertation? Reasonable people can disagree on these matters.

Most dissertations are never published as empirical journal articles. If the dissertation really contains a significant contribution to knowledge, failure to publish is at least unfortunate. Is it worse than that? Is it unethical not to publish (or at least to submit) a dissertation that represents a significant advancement of knowledge? Perhaps the PhD should not be awarded until the work is published in a refereed journal. That would reduce any glut of PhDs on the market and would doubtless improve the quality of dissertations.

My own criterion when serving as a member of a dissertation committee is to say to myself: Can I imagine this work, appropriately written up, being published in a refereed journal? By this criterion I mean “any refereed journal” (and standards differ widely even among refereed journals). Of course, my imagination is more active at some defenses than others, but at least it gives me a principle to try to follow.

I must confess that I come from the old school, where I believe a dissertation should really count for something. Ideally, it should contain a great literature review, excellent new research, and should be published and represent a real contribution. It should be readily publishable in a good journal. Not too many years ago, publication of the dissertation represented one's scientific debut – the first independent work a person published. That seems rarely the case any longer, with students now often being authors before they defend the dissertation.

I have recently been hearing that this grand view of a dissertation is outmoded and a more modest set of standards should be employed. Basically, the student should just write up a couple of experiments (or other types of research) that are ongoing during the appropriate year of graduate education and call it a dissertation. By this view, the dissertation is not perceived as any great intellectual project or a rigorous assessment of the ability to conduct research, but only as a modest hoop to jump through. Some dissertations I review for journals seem to have this character, but to me this kind of work misses the point. The student should carve out some piece of the intellectual world for her- or himself, really understand it with a thorough literature review, and then make a telling contribution. If the student simply writes up some ongoing research in the lab, those on the outside may wonder about the student's contribution to the dissertation relative to that of the major professor (or even that of other lab members).

But do dissertations ever meet the stringent criterion of making a signal contribution to a field? Yes, they do. In my own part of the intellectual universe (cognitive psychology), I think of George Sperling's dissertation (published in 1960) as a prototype. Sperling took a long-standing intellectual enigma – brief persistence of information in the visual system – and produced a breakthrough publication in his dissertation. His monograph provided a masterful review of the literature on this problem, clever and incisive new experiments using his newly developed partial report technique, and he introduced the concept of precategorical visual storage to explain the persistence he observed. Today the concept is known as iconic storage or iconic memory and it is described in almost all introductory psychology and cognitive psychology textbooks. The area still represents a lively arena of intellectual inquiry. Few dissertations could be as great as Sperling's, but it provides a model to emulate.

I have often thought that an interesting graduate course might be "Great Dissertations in Psychology." Each week students would read the published version of a wonderful dissertation like Sperling's and discuss the features that made it great. What was the intellectual background before the work was conducted? How was the research designed? How did it set the field ahead? How did it achieve its great impact? Of course, the dissertations selected for inclusion should be drawn from all across the intellectual landscape in psychology. Why have such a course? If students must conduct a dissertation that will take two years of their lives, why not give them outstanding models as possible guides? They may set their sights higher than they would otherwise and might also have a greater vision of what their work may accomplish.

I will make a modest beginning by trying to compile such a list of dissertations. If you have a suggested dissertation, please send it to me via letter or e-mail. Nominated dissertations should be published, well-written, and well-designed and constructed; should have compelling data; and should have great impact on the field in terms of citations. Citations are a standard measure of impact and, according to the Web of Knowledge (the former Web of Science) database, Sperling's dissertation has been cited 896 times! One hundred citations has traditionally been the criterion leading a paper to be considered a "citation classic." Perhaps 200 citations would be the minimum for the Famous Dissertation.

Students often flounder in looking for a dissertation topic. I suggest three general strategies when students are beginning to seek a dissertation topic. One tactic is to look for a relatively new or emerging phenomenon or approach in their general field of inquiry, and to design research seeking to answer fundamental questions about it. Several of my students have used this strategy in their dissertations. If care is taken in conception and design, the resulting data can have strong implications almost no matter how they turn out. Another general strategy is to take a nearly forgotten (but critical) problem in the field, re-introduce it to the modern world, and attack it in a fresh way, with newer and better techniques. This tactic is essentially the one Sperling used. A third general strategy is to find two or more theories that claim to explain a phenomenon and then test among them. Of course, many people use this strategy in their research.

In writing this article, I went back to examine dissertations I have supervised. I was happy to discover that nearly all of them have been published (that's at least step one). Four of them have been cited over 100 times (Blaxton, 1989; Weldon, 1991; Rajaram, 1993; and McDermott, 1996). Blaxton's dissertation has been cited 400 times, so maybe it would be a candidate in the Dissertation Hall of Fame. My own dissertation (Roediger, 1973) won't make it – just 83 citations, according to the Web of Knowledge, far less than my students' work listed above that was published much more recently. Before laughing too loudly, you had better check the citations to your own dissertation in the Web of Knowledge at www.isinet.com/isi/products/citation/wos.

Again, please send suggestions of Famous Dissertations to me via mail (Department of Psychology – Box 1125; Washington University; St. Louis, MO 63130-4899), or e-mail me at roediger@artsci.wustl.edu. I'll report the results in a future *Observer*. Self-nominations are welcomed, so long as they meet the criteria mentioned above.