

Dear graduate students: Don't lose hope

Someone using the *nom de guerre* Thorsten Veblen asked me for a comment on [his rant against development economics](#):

While I can understand the views of Development Economists, I cannot really remember being impressed by any Development Economists, or have ever walked away from a Development Econ presentation/class feeling like I've really learned a new, important insight into the fundamental question of Economics: Why are some countries poor? And what can "we" do about it? And this was my feeling today when I sat in on a first graduate development lecture of the term.

Veblen's reaction is hardly unusual. I almost picked up and left after my first year for similar reasons. But it's a reaction I came to reconsider. Why is a subject for a longer post, on that elusive day when I have more time. But Veblen's impression is common enough that I want to respond.

First, something that becomes clear only in retrospect: a graduate class in any field of economics is not attempting to teach you about the big ideas in that field; it's attempting to teach you method. Your professors are showing by example how to write a tight, well-theorized, well-identified research paper. You have the rest of your life to learn about why some countries are poor (the logic goes) but you will *never* outside of that class teach yourself how to build a structural model.

Second, a lot of economics research aims to answer narrow questions very well—especially the kind of research that gets you a good job or tenure. Some of this work is mathematical masturbation. Some is a rehashing of puzzles that economists care about only because they are puzzles (and it's a chance to be clever). But a lot of the research is very good and very important.

To see an example, take a look at Esther Duflo and Abhijit Banerjee's [Growth Theory Through the Lens of Development Economics](#). They aggregate oodles of mainstream development research into a set of answers (and unanswered questions) about the larger process of development. I think it's marvelous. And there are many more examples (or I'd have little to write on this blog). Indeed, most of the mainstream development people, in their main work or behind the scenes, contribute to development thought that really matters.

Finally, those seemingly pointless papers that rehash an irrelevant question serve a higher purpose: they advance the method. I could care less about the 100th paper on the 'returns to education'. Who cares if it is 0.12 or 0.14? But the debate that raged to identify one silly

parameter did more to advance our understanding of causal identification in statistics than almost any other question in social science. And the literature is better for it.

Do I wish that junior faculty could pursue big and fuzzy questions in development without committing tenure suicide? Yes. Is the profession distorted towards a narrow set of questions that be answered with a specialized set of tools? To some degree. But do I think that the demand for excruciating rigor in theory and statistics has changed the world? Absolutely.

I finish with the advice from the brilliant, senior, fuzzy development economist who stopped me from leaving the PhD: “Yeah, there’s a lot of bulls**t, but it’s good for you, and it will get better, and soon you’ll get to do what you really want to do. So suck it up and stay.”

I did, and with no regrets. Don’t lose hope.