

Other things equal: Aunt Deirdre's letter to a graduate student

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Dear Mr. Smith,

I got your eloquent e-mail [asking whether or not first-year PhD economic-theory courses have any value]. In the past few years, it may comfort you to know, I've heard from a lot of graduate students with the same question. They ask me because they hear I criticize the establishment in economics (comfort the afflicted and afflict the comfortable!) I refer them to Arjo Klamer and David Colander's book of interviews with graduate students, *The Making of an Economist*. Every intending graduate student should read the book. It's comforting to know that EVERYONE feels that the first year of graduate study in economics is a waste of time, demoralizing and unscholarly; it's comforting, but realistic-you have to go through it, too. Like Marine boot camp or all-nighters in medical internships. Makes a man out of you (the men say; men say a lot of strange things when they get into this man-making mood). You ask, in a nutshell, Is it worth it? Is this what I signed up for? I entered economics because I wanted to save the world or make a scientific contribution, or both. I see no signs of either. Help!

First off, you really need to give yourself a pat on the back for asking the question, Is it worth it? If anyone could go through the fourth-rate applied mathematics and misapplied statistics that makes up the Theory and Econometrics Sequence in modern departments of economics WITHOUT asking The Question, he would be a poor candidate for the job. No person who does not have doubts is going to be much of an economist.

And, yes, it's that bad. The "theory" is focused on proof, a quite unscientific obsession taken from the Math Department (not from the Physics or Engineering departments). It is puerile. What is worse, it becomes obsolete quickly-not because of scientific advances but because of scientific fashions. The half-life of the so-called "theory" you just suffered through in the first year is five years in macro and a maximum of ten in micro. I for example suffered through instruction in something called "activity analysis," which in 1964, they told us (I mean Bob Solow and Paul Samuelson and Robert Dorfman told us, people

you wanted to believe), was going to be the way that economics was expressed in the future. A lot like game theory now. So I know how to solve a linear programming problem better than anyone ten years younger than I am. Why don't younger people know how to solve such problems? Because in five or ten years after I learned it activity analysis was dead. The residue is that I still talk in metaphors of "slack variables" and the like, translating marginal productivity into linear algebra for no scientific gain. The only result is that we old folks can communicate with each other over coffee (decaffeinated).

But don't laugh too hard. You know how to translate market analysis into game theoretical terms for no scientific gain. Congratulations. Enjoy it while you can: in ten years it will be as dead as activity analysis, recommended by all the best authorities.

The econometrics is just as bad. I think you know my views on statistical significance. If you don't, I advise you find out, since they will save you having to remember a lot of pseudo-science beyond the final exam. Econometric estimation is a good thing, using the world's facts-though it does tend to cause of lack of interest in collecting the facts, at least in some people who study it. But econometric testing, as against estimation, is not worth anything at all. Its marginal product is zero. It is a slack variable. Really.

So you show good taste by being depressed after learning all this stuff.

And you show even better taste by going on to worry about the topic of your dissertation, worrying that it, too, might not survive The Question. It is worth the effort to produce yet another Three Points of Theory in Search of a Point? No, it's not. Aunt Deirdre is here to tell you that if you want to be an economic scientist you have to do economic science. And economic science is NOT searching through the hyperspace of assumptions or through the t-statistics of fifty different specifications.

Economic science is like other sciences: finding out things about the world and explaining them

so that they stay explained. There's a new book on German guilt in the Holocaust that meets scientific standards, as do many books in history—before you sneer at the soft little people over in the Department of History you might reflect that when they learn something it stays learned, and history therefore makes the kind of scientific progress that economists should envy. Or go attend some seminars in biology or geology, and notice what sorts of arguments impress the biological or geological scientists. Existence theorems. Statistical significance. Policy conclusions from same. Not.

It's pretty simple to know when you have a dissertation worth doing. It will come from The World, not from The Literature. You will care about it, because (if you are wise) you will have chosen an area that just plain interests you—some part of the economy your family has long been involved in, say, or some fact (e.g. corruption in building contractors) that excites your indignation or your anger or your love. You will look into explaining it from every conceivable angle, interviewing the people involved, reading history books, talking to sociologists, dreaming up six entirely different mathematical models for it, getting the statistics from dusty archives or from actually going out and watching what happens in the economic world. You will read an essay by C. Wright Mills, the great American sociologist, called "On Intellectual Craftsmanship" in his collection called *The Sociological Imagination* (you will find it in the library because you will realize for the first time today that only the most recent books are in the electronic catalogue). From this you will learn that being a serious scientist is a way of life, not the following of a formula of Fashionable Theory plus Fashionable Econometrics = Publication.

For example, in my dissertation at Harvard in 1970 I studied the question, "Was the British steel industry 1870-1914 more stupid than the industry in the USA?" It turned out that some bits of theory and econometrics just invented could be applied (total factor productivity: that same Robert Solow led the way, although as I explained in the dissertation his method had been invented in 1933 by a student of Alfred Marshall writing economic history). Why Britain? I had been there as a child; I liked English literature and history; I was lousy at languages (when I as a graduate student went to see David Landes over in the Department of History he threw me

out of his office as soon as he realized I did not know German). Why steel? I had second cousins who worked in Gary, Indiana; we would drive past the furnaces in the 1940s; big pieces of capital equipment are exciting. Why stupidity? It's always interesting, and was especially so in the 1960s, to ask if capitalism is wise or stupid, and I was shifting from the stupid camp to the wise. Why ask such a question? Historians such as David Landes had asked it and said "Yes" on what seemed to me pretty unimpressive evidence.

You can do it. You can be an economic scientist. Ask what matters to you. Do it. Find out something about the world. Really find it out. Really explain it, with reference to the great conversation of economics since 1776. Really. (You will find that the rubbish you learned in the first year of graduate school is not much use for real economic science; but you've already figured that out.)

I know, I know: fear. Will I get a job? Will I be a success? Oh, gosh. I should just take a piece of The Literature and run a new assumption or a new regression.

Please don't. It's not dignified. It's not ethical. It's not true to yourself. It does not advance economic science. (There's even some evidence that it's not prudent in a narrow sense: people with the Three Essays sort of dissertation are finding it harder to get jobs than people with Look What I've Discovered sort.)

There's an odd economic argument for doing the right thing, and breaking the cycle of non-scientific economics that your supervisors are locked into. It is that by choosing graduate school in the first place you incurred an opportunity cost that it would be irrational now to second-guess. You knew that you would make more money, get more settled employment, if you went to business school or law school. Or just got a job and worked: you are an intelligent and hard-working person (how do I know? People who go to grad school in economics are, and the one good thing about the first year is that it is an IQ-and-brute-energy test—though testing you in ancient Greek would be better). So it's not rational to suddenly get silly about job security now. If you wanted job security you could have had it: you've shown you value Glory or Goodness more. Go get it. The only way to do it

is to face up, now, to the unscientific silliness of modern economics and do your own thing.

I don't want to make an argument on the basis of prudence alone. I think economists have gotten carried away with prudential arguments, fouling them up because they do not include the other springs of action. But if you want to make a strictly Benthamite argument it would go like this. You have shown that you have a low rate of time discount by going to grad school in the first place. How did the famous people in economics get famous? By following The Literature? No. By doing their own thing. I speak of Ronald Coase and Robert Fogel and Robert Solow and John Hicks and nearly every Nobel since the beginning.

But forget about prudence. Be true to yourself, for God's sake. Be brave. You will think, She's in a poor position to say that-after all, she has a good job (three of them, actually); she's famous (well); she has tenure. But I didn't get it by being a coward. (True, if I had been a coward I would have a BETTER job; but I have to look at myself in the makeup mirror each morning, kids.)

When I was a graduate student I went into see Richard Caves, a serious and scientific economist among a dwindling number. I asked, Is it worth it? He said, in the words of Scripture, This too will pass. He was right. I went back to work and tried to forget my doubts. Unconsciously, I sidestepped the sillier of the economic fashions some of my classmates became entangled in. But mainly I just went stupidly on, and only later came to see finding out things and explaining them as our central scientific duty. I'm not recommending sleepwalking of the sort I engaged in. I'm recommending open-eyed courage.

Please, please, my dear, be brave, and remake our splendid subject, the intelligent student of prudence, by bringing it back to science. I'll hire you, if I can. And you'll have a worthwhile life in science.