

# OTHER THINGS EQUAL

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## Mottoes for Science: *Intendete Alte in Gubernando;* and *Qui scis?*

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This spring I've been teaching a course for our second- and third-year grad students here at UIC that helps them plan a dissertation. Students, after all, are not born with the knowledge of how to write a successful proposal and how to carry it out. My colleague Carmel Chiswick pioneered the course, but turned it over to me this year by way of introduction to our good, empirically oriented graduate program (you'll have heard about the intellectual excitement at UIC, nurtured by the economist Betsy Hoffman when she was Provost, before going off to be President at the University of Colorado). The course introduces me to the students and I introduce them to *Doing the Dissertation*. We sip port in my cluttered office every Tuesday and Wednesday from 4:45 to 6:00 (class times are like that on an urban, commuter campus) and I impart professional wisdom.

Bottom line? First, what my driving instructor in high school propounded: Aim High in Steering. It's the best advice I've ever gotten. Look down the road. Anticipate. Make no little plans—that way you're sure that your dissertation (or your next article or your next course plan) will be important even if you have to, of course, trim to a doable sub-project, early and late. If you really want to know what the channels of causation were for the Asian financial crisis, aim high and do it; if you *really* want to know whether the Mafia hurt the southern Italian economy, aim high and do it. Don't narrow the question immediately to an econometric test or an accounting calculation.

I like Aim High in Steering so much I've translated it into Latin—*Intendete alte in gubernando*—and have festooned the Latin in big letters over the couch in my office, so the kids won't forget it.

Just as the term got underway the University of Michigan Press issued my collection of other pieces of wise advice to aspiring economists, *How to be Human* \*\**Though*

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*an Economist*, so we've been using it as a text. Combined with the new edition of *Economical Writing* (Waveland Press), who could want more by way of (practically) free advice to graduate students? As Johnny Carson used to say, *everything*, just *everything*, you want to know about being an economist is contained in these two wonderful books. The Michigan book contains the advice-giving (as against facts-and-opinions-about-the-economy-giving) columns I've written in the 1990s for the *Eastern*. Now you've got something to send to your colleagues (anonymously, I'd suggest) if you *really* want to make them angry.

For example, about statistical significance. The students and I don't spend *all* our time on statistical significance, but enough that we are doing a little paper surveying its use by field in a few recent months of the magazine *Science*. The preliminary finding is that only some fields, and not the most data-intensive, make the mistake of using significance to find. . . well . . . significance. Physicists and astronomers and chemists and geologists and most biologists have more sense than medical researchers (uh-oh) and population biologists.

And economists. Economists still don't grasp—imagine, that: after all my years of trying in twenty different ways to tell them—that fit is not the same thing as importance. Actually, a few are starting to. I found out that the book they use for undergrads at Texas Christian University in Fort Worth, H. H. Studenmund and Henry Cassidy, *Using Econometrics: A Practical Guide*, warns and warns and warns about confusing statistical with scientific significance. As far as I know Studenmund and Cassidy are almost alone among the texts available for courses in econometrics (and the price of the book I can find on barnesandnoble.com is awfully high; try the old Wonnacott and Wonnocott).

That's the problem my students in the how-to-do-a-dissertation course have. By the time they come to my course they've had tons of econometrics, reading more popular and less intelligent books on econometrics. (I record my opinion, by the way, that graduate programs have way too much econometrics, two or three times more than is healthy for the quantitative common sense of our students. Economists have come to believe, without thinking about it too much, that formal econometrics is *the* way to do empirical work. *Everything*, just *everything*, you need to know about confronting the facts of the world is contained in this textbook by Dhyrnes or Johnston. Forget about data collection or index numbers or accounting or orders of magnitude or simulation or, in short, as I say, quantitative common sense.)

In each class meeting down the hall at UIC, my beloved colleagues the econometricians tell the students again and again, as the Oxford econometrician David Hendry puts it, test, test, test. Professor McCloskey replies, nonsense, nonsense, nonsense. Who's a graduate student to believe?

You know the answer. But it's hard on the students, so well have they been econometrized.

They say, "Oh, I see: you are a humanist complaining about the quantification of economics." No, dears, that's not quite right.

They say, "Oh, I see: you are making the old point that the levels of significance we publish are not the ones we have found, since only significant coefficients make it

through the ignorance of referees." No. I leave that to all the *other* economists who have criticized the misuse of statistical significance. I specialize in making the more elementary but more devastating criticism that fit is anyway not significance.

They say, "Oh, I see. But you will admit, surely, that selecting hypotheses by goodness of fit *approximates* the truth we seek." No, I do not. Coefficients that are statistically significant can be scientifically insignificant (for example, if the sample size is large enough, always). Coefficients that are statistically insignificant can be scientifically significant (for example, if the particular variable is the only policy tool we have and the policy need is urgent, always). If these two propositions are true—and they are, you admit—it looks bad for a statistical-significance-guided science, yes?

They say, "But surely there is *some* use for statistical significance?" A thoughtful student in the how-to-do-a-dissertation group forced me to admit this a few weeks ago, against my impulse to make radical though essentially correct but not perfectly accurate statements. Yes, I admitted, it tells you something about sampling variability (in the few cases in which that is actually a scientific problem). But, I added, in evaluating the significance you need a loss function, always. The student agreed.

The students say finally, "But what are we to do? What's your alternative? How do I write my dissertation?"

Ah, good: you have understood the point, and now we can move to the real question: What Is to Be Done? How do we make quantitative arguments in economics? What is the correct quantitative rhetoric?

It would be immodest of me to suggest as an example my own quantitative work in economic history (a selection of which is available right now from the British publisher Edward Arnold, a bargain at half the price); and almost as immodest for me to suggest historical economics generally, "cliometrics," as we call it—though any economist who looked into such work by Price Fishback or Steve Ziliak or Mary Beth Combs would I think agree that whether or not it is quite as wonderful as its authors and their friends believe, nonetheless it *is* quantitative economic science, and usually does not substitute statistical significance for oomph (listen up, my dearest Ken and Jeffrey). I ask you therefore to find your own examples, as an exercise. Look through the books and journals for cases in which little *t* statistics do not seem to run the quantitative show.

Go ahead, please. My colleague at UIC, whom I have known since Harvard in the 1960s, Lawrence Officer, told me the other day that when he had grasped my point about statistical significance he found it *quantitatively* liberating in his work—because now he realizes he can concentrate on what matters, the *sizes* of the coefficients (as against their utterly irrelevant *t*-statistic-hood), the accounting oomph by which we actually do measure the impact of one economic variable on another.

So be brave. Aim High in Steering. Do the science, asking the actual questions you want to answer. Diverge from the strange alliance of irrelevant econometrics and irrelevant "theory."

Irrelevant theory? Yes, it's the other half of what we so ill-advisedly over-teach our graduate students, diverting them from the big questions and the scientific and

seriously quantitative answers, getting them to look away from Aiming High. A while ago I told you about a public debate I had with Ken Arrow in which Ken accused me of being "against theory." At the time I was indignant. But Ken was right after all. I've been rearranging my books, following my move to a loft apartment downtown in Chicago. It's been almost a year since I moved but the rearranging of my books has only just concluded, the problem being that there are about 3,000 of them. So I was handling a lot of economic theory, books much beloved by me, and kept around long past any possible use they might have. I handled with care, for example, a battered copy of Milton Friedman's first paperback edition from Aldine of *Price Theory: A Provisional Text*, from which I learned among other things that all income is just the return on some sort of capital [1967, 245] and that you can't deduce the superiority of an income tax over excise taxes from a blackboard argument (p. 56ff, from a piece in the *Journal of Political Economy* in 1952; I. M. D. Little made the same point at about the same time, in the *Economic Journal* for 1951). Or I. F. Pearce's *International Trade*, from which I learned that anything can happen if you move from a 2 x 2 model to a 3 x 3 model [1970, 398ff]. Or the old first edition of Bill Breit and Hal Hochman's (Hal is the former editor of this very magazine) *Readings in Microeconomics* [1968], in which I first read and then taught to Chicago grad students Ronald Coase's "The Problem of Social Cost" from which I learned that anything can happen in the presence of transaction costs.

You may detect an empirical pattern in my theoretical education as an economist. It's the same as yours. We learned certain points of accounting and mathematics very worthwhile in thinking up quantitatively implementable functional forms (sorry for the teutonic phrasing). Aside from that we learned *nothing*; or, rather, that nothing will come from nothing, as King Lear expressed it. We learned in fact that on the blackboard Anything Goes. You can't find out facts of the world by an essentially philosophical exercise. The point struck me forcefully as I was hauling the books here and there, sneezing from the dust of decades. You should have seen me, talking to myself indignantly: "Wait a minute. What do we learn from *The Collected Scientific Papers of Paul A. Samuelson* [Vol. I, 1966, edited by a promising young student of his, Joseph E. Stiglitz], or for that matter from David Ricardo's *Principles of Political Economy and Taxation* [1817]? Nothing except that assumption A implies conclusion C, and alternative assumption A' implies C', and so forth. We learn nothing, zero, about the world. *You can't infer facts of the world from a blackboard.* Period."

I concluded on that dusty afternoon sorting through my books in Chicago that I *am* against theory, at least as Ken and most economists understand the word, and so should you be. I am against all theory that does not come with at least a suggestion about how you might find out its truth in the actual world (and forget the phony suggestion that it will be tested by its "implications" econometrically; this was the program of Tjalling Koopmans in 1957; it hasn't worked, ever: name the proposition in abstract theory that has been tested—whether rejected or not I do not care—econometrically in a way that has persuaded doubters). I am against theory that merely adumbrates a possibility, and gives no empirical purchase on its truth or falsity in the world. For example I am against game theory. And so should you be.

Leo Rosten (author of the wonderful *The Joys of Yiddish*, located in the Exotic Languages section of my home library) once portrayed Milton Friedman in fictional guise as asking "How do you know?" insistently, distressingly, irritatingly. I can testify that Rosten has it just right. Milton used to ask it all the time at the Money Workshop. The first time I encountered it was at the first cocktail party I went to as a new assistant professor at Chicago, at Bob Gordon's house in 1968. I was holding forth to a group including Milton about how professional sports was a monopoly. This I had learned from a column by . . . Milton Friedman. He turned to me and looked up through those spectacles and inquired, terrifyingly, "How do you know?" I could hardly say, "Uh, you told me so, Milton: that's how I know." *Embarrassing*. But a good lesson for a graduate student, or even an assistant professor.

So that's another motto, right up there with *Intendete alte in gubernando*. Let's see. The Latin must be something like, *Qui scis?* How do you know? Not by axiom and proof. Not by statistical significance. These alleged methods *look* like Real Science but are actually a Cargo Cult which has enchanted us and ruined our graduate programs, all these years.

If we'll adopt my two Latin mottoes as guides to our graduate students we can get out of the mess. Latin's like that: a guide to the perplexed.

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