Discussion Notes on “Formalization”

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It is a strange topic to be talking about. Could anyone believe that a little more or a little less formalization would change the content of economics significantly? More likely the true agenda is rather different.

Three economists meet. I will call them – formally – A, B, and C. A says: I think most businesses like to earn as much money as they can. B says: Business firms try to maximize their profits. C says: Firms choose the level of output at which MC = MR. Which one is the formalist?

Formalization is a technique that has exactly the pluses that go with its minuses. I hope that hostility to formalization is not just hostility to clarity. There is a symmetry here, it is not one-sided. You can sometimes hide the fact that you are not thinking clearly – being illogical, pretending determinateness where there is none, using the same word to mean different things – by avoiding formalization and just going on. But you can sometimes pretend that you are solving a problem when you are not – rather sweeping difficulties under the rug and solving a different problem – by formalizing and focussing on the formalization itself.

Keynes defined “scholasticism” as the attempt to be precise about concepts that are fundamentally vague. One understands what he meant: the cliché example of scholasticism was the debate about how many angels could stand on the head of a pin. There was a real problem to be solved: would heaven eventually fill up? The debate was certainly a premature appeal to geometry. One could find analogous examples in economics, no doubt. But being precise is not the same thing as being formal. Being formal is one way of being more precise than the material may justify. But it is that only if it misleads and directs attention the wrong way. But then why go after formalization itself rather than the particular misuse? Here I agree with Katzner.

It is easy to name a lot of achievements in economics that probably could not have been accomplished without formalization, including econometric formalization. I think of all game-theoretic notions (like the analysis of the prisoners’ dilemma, the properties of the tit-for-tat strategies, new and old growth theory, contract and agency theory, option values.) Keep in mind that formalization may be a way, possibly the only way, to achieve an informal result. It is a way to keep talking, and about the same thing.

Now let me come to my assignment, Don McCloskey’s paper. I think Field Marshal McC is fighting the last war, or the war before that, but in any case the wrong war. The world in which I live is not one in which one feels oppressed by existence theorems or proofs of them or proofs of them. There used to be more of all those things than there is now, but never very much. It seems to me that the number of theorists doing axiomatic theory is quite small and the subset doing general equilibrium theory even smaller. I was amazed at the force of McCloskey’s reaction.

His thesis could be turned on its head. If I feel oppressed by anything it is by the NBER and that flood of yellow-covered working papers. None of them contains an existence theorem. Most of them are empirical. They do indeed test hypotheses. The trouble is that so many of them are utterly unconvincing, utterly forgettable, utterly mechanical, and there is no way of knowing in advance which are and which not. That is more of a threat.

My goals are the same as McCloskey’s but I will offer a different diagnosis. The problem is that economics is not as cumulative as we would like in its quantitative understanding of the way the world works. Those yellow NBER papers are a symptom of that; they never settle anything. I think it is for a cluster of reasons that have to do with the way economics is done and with the very nature of its problems.

We do not have hard data; I think McCloskey is wrong about that. We have soft data because they are not collected under controlled conditions. Too many things are always happening at once. It is very hard ever to make a sharp inference. The traditional cure for that difficulty is the experimental method, and that is where the natural sciences (like biology, chemistry, physics, even theoretical physics) have it all over us. They cumulate better
because, as each question is answered, experiments to answer the next question can be designed and carried out. (Where will experimental economics lead? That is an interesting question but it is too soon to guess at the answer.)

The example of an observational science like astronomy is enlightening. As far as a complete ignoramus can tell, some astrophysical questions appear to be closely related to laboratory science. As for the others, I have the impression that on the nearby scale, where signal outweighs noise by a large factor, astronomy does beautifully well; but at very large distances, where signals are weaker and noise louder, it seems more like economics.

One important by-product of the experimental method is that the current set of hot questions is very clearly delineated; you know which articles you need to read.

The availability of experiment suggests an important difference between the typical scientific problem and the typical economic problem. Scientific problems tend to be isolatable (not always, of course), to relate to a small system: a gene, a molecule, a property like superconductivity or crystalline structure. The typical economic problem seems more closely embedded in the whole system, and not only the economic system. (That is the nagging limit to experimental economics.) If you tweak one part, much of the rest gets involved. Experiment may be impossible and even observation becomes difficult because it ramifies so far so quickly. That, not the presence of a handful of general equilibrium theorists, is what makes progress so slow.

Most of economic theory, formal economic theory, is much less general in its aims. The idea is to understand some restricted range of actual phenomena. Think of efficiency-wage theory, agency theory, even growth theory.

There is an important distinction to be made: do not mix up rigor and abstraction, even if rigor sometimes demands abstraction. There is no excuse for lack of rigor. You cannot have too much rigor. To make non-rigorous statements is to make false statements. (Of course the notion of rigor does not apply to statements like “the outcome is efficient” but to statements like “it follows that the outcome is efficient.”) There is not a category of non-rigorous truths, not in theory. The closest Don McCloskey comes to this question is his recording of the conversation between Wittgenstein and Turing. I am a great admirer of Wittgenstein, but in my opinion he did not cover himself with glory in that passage.

I conclude that this discussion is about the wrong thing. There are no general truths about formalization. What sorts of conclusions should economics aim at? Then we can talk about good ways to succeed.