Searching where the Light Is: Connecting Theory and Policy in Economics

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Mainstream economist's tendency toward abstract theorizing often provides an easy target for outside critics of economics who characterize theoretical economists as "idiots savants, brilliant at esoteric mathematics yet innocent of actual economic life." (Kuttner, 1985) Such views are shared by many heterodox economists. While this characterization has elements of truth, it misses an important justification for abstract theorizing when the economy is seen as a complex system. In this paper I discuss that alternative justification for such abstract theorizing.[2] I consider economist's tendency to do abstract theory, and suggest an alternative way of relating theory and policy that I provides a much more positive spin on mainstream economists' tendency toward abstract theorizing than that given it by most heterodox economists. The gist of the argument is that we should think of economic theory not as a precise map, but as a general pattern generator, which is useful to keep in the back of our minds and we approach policy problems.

My paper is centered around a series of economist jokes. Jokes provide a good vehicle with which to discuss methodological issues because they often mask issues about which people are insecure. They allow us to broach issues that otherwise would not be broached. The title of this paper comes from one of those jokes—the streetlight joke—a joke that is so familiar that I will not repeat it in the body of the text.[3] This is a joke that is told, and enjoyed, by neophyte heterodox economists—at least that's the stage of my life where I told and enjoyed it. They like it because it conveys a sense of economic theorists out there in Lala land, and allows one to contrast that with their vision of themselves as researchers with their feet on the ground. The parallels of the streetlight joke with economic research are straightforward: "Isn't it stupid—searching where you haven't lost the keys?" "Isn't it stupid—working on models that you know are so far from reality that they can't possibly describe reality: representative agent models, when it's obvious that the action is in interactive effects; static models, when it's obvious that the action is in dynamics; strict rationality models, when it's obvious that people are at best boundedly rational?" The list of stupidities can be extended substantially.

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[1] An earlier version of this paper was the keynote address at the Methodology in Economics Conference, Turin, Nov. 7th, 2005.

[2] Although the subject of this paper may be considered methodological, let me begin with a disclaimer; I am no methodologist. In fact, methodology scares me—it becomes so complicated so quickly that it takes off all on its own, much like theoretical economics often does. I'm sure that there's something there, but I often cannot figure out what. Unfortunately for methodologists and heterodox economists who want to raise methodological issues, that's a characteristic I share with the large majority of mainstream economists. While I am not a methodologist, I am an observer of economists, concerned with what economists do, and how they think about problems.

[3] In case there are any non-economists reading this paper, the joke is about an economist is searching for his keys under a streetlight, even though he lost the keys far from the streetlight. When asked why he is searching under the streetlight, he answers, "That's where the light is."

The reason that the joke is so appealing is that, on the surface, mainstream's theorists' tendency to "search where the light is" is clearly a stupid strategy; the obvious place to search is where you lost the keys, not where the light is. I understand these sentiments, and have expressed them many times in conversations and in my writings. But one of my messages in this paper is that the argument against theory and abstract models can be, and often is, overstated; the strategy of "searching where the light is" is far from stupid, as long as it is done for the appropriate reasons. Specifically, the search is to provide new patterns to guide us in our search in the dark, the search can make a lot of sense. Developing highly abstract theory—using the latest analytic techniques--is a necessary component of developing a firm scientific understanding of the economy. So in this paper I will defend abstract, esoteric theorizing and argue that we have too little, not too much, of it in economics.

However, in my view, not all the criticism about abstract theory is misplaced. My criticism is not about abstract theory and mathematics generally; it is about the nature of the abstract theory used, and how that theory is used to guide our thinking about policy. I believe that there is far too much activity in economics, which goes under the name theory, that is not abstract enough; it is the equivalent to searching under the streetlight to find the key, rather than searching to develop a pattern for searching in the dark. Such work is not theory, but rather a mathematical exercise that is simply a rehashing an explored terrain, using a canned set of directions—create a simple model; solve it, make some stab at providing empirical support for the model, and conclude with a statement that more work needs to be done. The paper is an end in itself, and is not a contribution to either theory or policy; it is simply an advanced problem set. The focus on such mathematical exercises has to do with incentives in the economics profession which place publication above a direct search for the truth.

While the arguments of this paper have a different form my earlier work, they fit with the arguments I have been making over the last twenty years (Colander, 2000) that we need two distinct branches of economics:

- a pure theoretical branch that is more mathematical, and more esoteric, than is much of the theoretical work now, and
- an applied policy branch that is far less esoteric, and far more pragmatic.

The two branches should be seen as distinct. The pure theory work has little interest in, or direct relevance to, policy; it is concerned with pushing analytic tools as far as possible to provide possible patterns for search. The applied policy branch will use theory loosely and concentrate on providing solutions to actual problems. While that applied policy branch will use significant mathematics and statistics, those mathematics and statistics will be applied mathematics and statistics, concentrating on finding numerical answers to real problems, not on finding analytic solutions to problems. It will be heavy on data analysis, and light on theory, and the theory it develops will be integrated into heuristic analysis; proofs and lemmas have no place in it.

The two branches are connected, of course. Theory provides guidance in applied policy and applied policy provides guidance for theoretical explorations. But the connection is not direct; pure theoretical work is guided by analytic technology; it is searching where the light is. Policy work is guided by the problem to be solved; it uses whatever works to come to a solution to the immediate policy problem, and is not directly drawn from theory. Its approach is: Here's a problem; here is a possible way of dealing with it; it will include non-economic aspects, and integrate ethics and social considerations into the analysis. In applied work theory is kept in the back of one's mind, and is used as a reference point—not as a set of blueprints.

Theory as a Pattern Generator

Let me explain what I mean in terms of the streetlight metaphor. Consider this scenario. You've lost your keys somewhere out in a field that has a streetlight illuminating one small area of that expanse; outside of a small radius around the streetlight, everything is dark. What would be a good way to proceed looking for the keys? The first question that seems reasonable to ask is: Where did you lose them? To make the argument cleaner, let's assume that you know that you did not lose the keys in the area illuminated by the streetlight. How should one go about searching for the keys? The way I'd go about the search is the following.

First, I'd search the lighted area to get a good feel of the lay of the land. That search under the streetlight, however, is not looking for the keys—I know the keys are not there. Instead, the search is conducted to gain information about the terrain--the patterns of the ground, determining where there might be indentations in the ground that one might miss if one doesn't have a light to guide one. Having gotten that sense of the terrain, I would determine an optimal search strategy for the lighted area. Then I would apply that same search strategy to the dark areas, each time checking, by empirical validation through other senses besides sight, if the terrain being searched matches the lighted terrain. Some terrain might match it; there, I'd use my search procedures determined in the light. Other terrain would not match. For those other areas, I would search without theory. As a byproduct of the search, I'd create a general map of the entire terrain and that map would tell me what terrain is similar to the lighted area, and what terrain is not.

Second, I would work on building both more powerful, and alternative, streetlights to extend the lighted area and to develop patterns that might fit other terrains. As the lighted area is extended, I'd follow the same pattern search process that I used initially. I'd take the patterns of the new, lighted terrain to the darkened areas, to see if the new discovered patterns fit some terrain that the previous patterns did not, extending the map of matching patterns and terrains. Eventually, I would find the keys, and have a set of maps to aid me in future searches.

The search process discussed above describes the approach that economics has followed in its study of the economy. We've developed Walrasian theory, which has proven useful in policy, and has had a number of successes. It has served as the foundation for a variety of useful applied policy tools—cost benefit analysis, much of management theory, taxation analysis, and the constrained optimization approach generally; the list can be extended enormously. An example of a recent successful search from Walrasian general equilibrium theory is marketable permits for pollution, which were initially developed in pure theory, and then were translated into policy in the Kyoto accord. In the process of developing Walrasian theory we have developed an expertise in the profession in dealing with highly technical models.

We've also developed an econometric and statistical expertise, giving us an ability to bring the models to the data.

While I believe that there is much that the profession is doing right, I also believe that we have been slow to integrate expanded illumination into our policy search process. Where we haven't done so well is on the applied policy front; we haven't dealt creatively with the difficulties that arise as we try to solve actual problems. For example, many actual problems have limited data and small sample sizes, yet we often deal with them with an econometrics that is based on asymptotic properties true only for large samples. Nonparametric econometrics and bootstrapping are too infrequently used. Statistical significance tests are overemphasized, even when the tests are not on data that directly address the issue, but are proxies with problems of their own; no "data proxy significance tests" are reported. In many analyses proxy problems far exceed the statistical problem in relating the data to a policy problem.

On the theoretical front, we have also had some difficulty in developing sufficiently complex theory to guide our policy analysis. We seem to use the same Walrasian map for all areas, including ones where it just doesn't seem fit either intuitively or empirically. Economists have focused too much on applying a pattern based around Walrasian general equilibrium theory, which is highly limited by assumptions, which, because limited analytic technology, were necessary to make at one time, but which are no longer necessary to make.

What's missing from economist's bag of tools is a tool that tells us where the theory fits, and where it does not. You just don't see a discussion in the literature stating: Here is where the Walrasian model seems to fit, and here is where it does not. I think that occurs because, instead of thinking of work in pure theory as a general pattern generator, which is useful to keep in the back of our minds and we approach policy problems, the profession has thought of theory as a precise map. Thinking of it in that way, the profession has gone about applying the theory to problems and to our consideration of the economy, even when it is obvious that the terrain we are dealing with does not match the terrain illuminated by the theory.

The problem is most evident in macroeconomics, where, at least in the US, theoretical work on the dynamic stochastic general equilibrium model has far exceeded credulity. It applies a theory relevant for a very small area to terrain that is fundamentally different from the terrain illuminated by the theory. It spends too little time on serious theoretical consideration of multiple equilibria, agent interdependence, and non-linear dynamics, all of which seem intuitively central in macroeconomic issues, and which we have the analytical tools to begin dealing with. Thus, in my view, the problem in macro theorizing is not that it is too esoteric and abstract; it is that it is not abstract enough. .

Muddling Through: Policy that Takes Complexity Seriously

The problem can be explained by another set of jokes that neophyte economists often tell. The first joke in the set is the can opener joke, which like the streetlight joke is so well known that I will not tell it.[4] On a superficial level, this joke expresses the same sense of futile research that the streetlight joke did. However, the joke does not faze standard economists. The standard economist's answer to the criticism of economics expressed in this joke can be captured in another, less well-known joke that portrays economics in a better light. The joke is:

> A physicist, an engineer, and an economist are given a stopwatch, a string, and a ball, and told that the person who can best measure the height of a building will get into a Scientific Hall of Fame. The physicist ties the ball to the string, hangs it down from the roof, and, using the stopwatch, and calculates the length of time it takes the pendulum to swing from side to side. From that information, he estimates the height of the building. The engineer takes the ball and drops it off the top. He then uses the stopwatch to determine how long the ball takes to fall, and estimates the height of the building accordingly. The economist, however, wins the place in the Scientific Hall of Fame by taking the stopwatch, trading it with a guard in the building for the building plans, and simply reading the height of the building from the blueprints.

This joke, obviously made up by a mainstream economist, shows both the benefits of trade and the way in which mainstream economists see economic theory. In this standard view, theory provides a blueprint of how the economy operates, and thus, once found, is to be guarded at all costs. Have an applied policy problem? Pull out the theoretical blueprints.

That is not the view of theory that I am supporting here. My problem with it is that it assumes that the economy is far simpler than I believe it is, and, therefore, does not capture the relationship between theory and policy that I am advocating here. Specifically, it assumes that that a set of blueprints exists, and that the building of the economy actually followed that set of blueprints if they did exist. I see the economy as a complex system in which the economy is emergent from a set of simple decisions in a way that no one pictured, nor could have pictured. Thus the complexity addendum to this story, which Robert Basmann suggested to me in private discussions, is that when the building took place, the builders adjusted the plans, which they never marked down on the blueprints. The economist reading from the blueprints got the wrong answer. In a complex system, with limited analytic tools, developing a single map to guide us is too much to ask. It seems much more reasonable to search for a variety of theories, each of which may be relevant for certain terrains.

In other work (Colander, 2005; Colander, Holt and Rosser, 2005) I have argued that the economics profession is currently evolving into a field that accepts that the economy is complex and must be analyzed as a complex system. One can see that evolution in the movement away from theory based on a highly specified holy trinity of rationality, greed and equilibrium toward theories based on more general assumptions--purposeful behavior, enlightened self interest, and sustainability. These shifts involve a movement away from a search for the Arrow Debreu McKenzie blueprints of the economic system, and toward a search for

[4] In it, a physicist and a chemist offer practical solutions to a problem of opening a can on a desert island, while the economist offers a useless solution--to assume a can opener. A full version of it at www.aeaweb.org/RFE/Neat/JokJokAboEco.html.

understanding a system in which the blueprints are missing, nonexistent, or so far beyond our analytic capabilities that we might as well forget about them. This new work does not deny the usefulness of the existing theory, but instead, sees itself as complementing it—providing guidance in terrains where the standard assumptions don't fit well.

I've followed this shift most carefully in macro, and in my recent work, (Colander, forthcoming-a) I've tried to distinguish Post Walrasian macro from Walrasian macro. Walrasian macro asks how coordination would occur if individuals were super rational and were operating in information rich environments. Post Walrasian macro asks how coordination would occur if individuals are less than super rational and are operating in information poor environments. There is one theory in the Walrasian approach, and the policy questions are all searched using Walrasian theory as a map. There are many theories in the Post Walrasian approach, and one is continually bringing the results of the model to that data to see where the patterns found in the theory match the patterns in the data.

Accepting that behavioralist view has implications for the way economists picture their role in policy. These implications can be seen by placing the New Classical argument for model and modeler consistency in reverse. The consistency argument for assuming rational expectations on agents' parts lies in the simple argument that agents can hire economists. If one assumes that economists know the correct model, which might be a stochastic one, then to maintain model consistency one must also assume that agents also can know the correct model, at least at some cost. Combining the two arguments led to the rational expectations revolution.

Rather than achieve the consistency between agent and modelers by assuming both have full rationality and rich information sets, the economics of muddling through approach is to start from the other direction--to assume both modelers and agents are operating within an information-poor environment, and, while bright, are not infinitely bright. Neither agents nor economists know for sure, even stochastically, what is going to happen. The best they can do is to muddle through. Thus, muddling through achieves consistency the other way around.

To distinguish applied policy in a complex system in which there is no map, from applied policy in a system in which one assumes that there is a unique map, I call the first—"muddling through", which emphasizes that in applied policy we are attempting "to cope despite lack of expertise or proper means" (the *OED* definition) rather than controlling anything. The argument I am making for "muddling through" is both prescriptive—I argue muddling through is what should be done, and also descriptive—I argue that in many areas of applied micro, muddling through is what is currently being done, although, like Monsieur Jourdain speaking prose in Molière's *Le Bourgeois Gentilhomme*, many micro economists don't recognize that that's what they are doing.[5]

[5] Actually, though, I suspect most economists do know that we're muddlers; they are just rather hesitant to admit it. In fact, it may be because we know deep down that we are muddlers that we spend so much time structuring our analysis so that it looks as if we are not muddling. I should also say that the focus of this article is on academic and textbook approaches to economic policy. Economists in actual line positions in government, business, and policy institutes generally recognize that policy involves muddling through.

Let me explain what I mean. Much of modern applied micro is, to a large degree, atheoretical; it is based on a general assumption that incentives matter, and uses sophisticated statistical methods, but is not based on any formal microeconomic theory. It is about as far away from general equilibrium theory as one can get. As examples of what I mean consider the work of Steven Levitt, who won the 2003 John Bates Clarke award in the US, and whose book, *Freakonomics*, (Levitt and Dubner 2005) has recently hit the bestseller list in the US. That work involves statistical analysis, creative questioning, and insightful questioning, but is quite independent of formal economics theory. One needs little economic theory to do it.

Another example of how the new statistical applied micro is occurring is in the American game of baseball. As recounted in the book *Moneyball*, (Lewis, 2003) whereas before decisions used to be made by hunch, more and more, they are being made by statistical analysis. Enormous statistics on players' actions are being collected and a player's attributes are being characterized by data, the value of that player to winning is being calculated. General economic theory guides the process, but the actual implementation is essentially statistical.

The reason the shift is occurring is because it works; the Oakland Athletics, which had the lowest payroll in the league, made it to the World Series, and when the Boston Red Sox adopted the method, they won the World Series. What makes this change possible are advances in computer technology, and we can expect an enormous amount of such statistical applied micro in the future.

There are numerous other examples, and in many ways, this type of empirical applied micro, with only a loose connection to theory, characterizes much of modern micro. Indeed, in my recent interviews with graduate students at top graduate schools in the US, (Colander, forthcoming-b) much of the applied microeconomics was essentially about applied statistics—finding the "killer application," finding the ideal instrumental variable, and, more generally, torturing the data to make it tell a story. What excites the majority of new economists were not theoretical issues in microeconomics, but statistical questions, and it was their statistical training that economists believed separated them from other social scientists. The methodology for modern empirical applied micro is muddling through, not control. This applied work is often highly practical, and universities are finding it hard to keep economists at the university, since the consulting possibilities for them are substantial.

In macro, where the issues are more complex, in the United States, at least, we've been much slower to move to a "muddling through" approach. The reasons why have to do with historical circumstance, and I will not discuss them here. But the change is occurring, as researchers come to accept that the macro policy terrain does not seem to match the terrain illuminated by Walrasian macro. More and more researchers are accepting that the theoretical action is in the interaction among heterogeneous agents, dynamic feedback effects, and agent learning. These are the starting point for Post Walrasian macro work that is searching for alternative patterns to the Walrasian patterns that guide much of the current macro theorizing in the US. Post Walrasian macro begins with the assumption that models requiring enormous knowledge on the part of agents of the underlying model just don't fit the terrain where macro problems reside. Thus, Post Walrasian macro rejects the rational expectations close of the general equilibrium macro model, and instead achieves model/theory consistency by assuming that not only do agents not know the correct model; neither do economists.

Let me give an example of what I mean by discussing the general approaches in a recent book I am editing on Post Walrasian macroeconomics. The theoretical work included in the book is highly esoteric, and is divided into two sections: one edging slowly away from the Walrasian model by incorporating more complicated issues in relatively standard models, and the other leaping away, replacing the Walrasian model completely with agent-based models.

In the “edging away” category, Masanao Aoki has a paper on random cluster analysis and ultrametrics, both of which use some results from nonlinear dynamic stochastic processes with heterogeneous agents, to match events that recently occurred in Japan. Bill Branch has a paper dealing with agent learning, where agents have less than perfect foresight, and Buz Brock and Steve Durlauf have papers dealing with model uncertainty—what happens when you can’t close the model with rational expectations—and on what happens when substantive sociological factors are introduced into macro models. In doing so, they deal with the complicated problem of equilibria selection mechanisms in multiple equilibria models that Walrasian macroeconomists have shied away from. The work doesn’t come to many policy relevant conclusions, but it is precisely the type esoteric work that I believe belongs in macro theory.

In the “Leaping Away from the DSGE Model” section of the book, Leigh Tesfatsion, Blake LeBaron and Rob Axtell have papers on agent based modeling. Their approach gives up on analytic theory, and tries to develop a *bottom up* theoretical approach; in it the researcher specifies agent behavior, and lets his created agents compete in a virtual economy. He then studies which behaviors continue, and which die out, to see what makes sense. More generally, the researcher studies a virtual economy to gain insight into the resulting model, and determines whether there are any stable patterns develop that might match observed data. The advantage of these agent-based models is that they allow much more agent interaction, and do not impose an equilibrium condition on the model. Thus they are the method that I believe will eventually become a central method of economic analysis.

Each of these papers I described above are exploring problems that develop when one moves away from the rigid assumptions that guide much macro theorizing and which underlie much of the Walrasian dynamic stochastic general equilibrium work that characterizes much of modern macro theory. I wish they led to policy results but unfortunately they don’t. They have no useful immediate policy results in the near future, nor do I expect them to have useful policy results. They are expanding the streetlight, and it doing so they are playing an important role that I see theory playing.

If the Post Walrasian theoretical models provide little guidance for policy, how does one conduct policy? In the econometric section of the Post Walrasian macroeconomic book Soren Johansen, Katarina Juselius, and Roger Farmer argue for a “general to specific” approach to econometrics in which one specifically deals with path dependence, unit-root nonstatioinarity, structural breaks and shifts in equilibrium means through a cointegrated VARs approach that has been championed by David Hendry and Soren Johansen. The Post Walrasian volume includes a paper by Soren Johansen and Katarina Juselius that examines the Danish data, looking for stable structural relationships and then use those statistical relationships to guide choices about the rules of thumb that will guide policy. It is pattern matching, guided by intuition in choosing among alternative explanations garnered from theory. This approach is precisely what I mean by pattern recognition and pattern matching. It is an approach that allows one to match the data with broad theories and to conduct policy with no firm theoretical foundation. This applied work is highly technical, but it is quite different from the mathematics used in theoretical models. It is much more statistical and applied mathematics, and much less pure deductive logic.[6]

To argue that much of economic policy is muddling through or searching in the dark is not as scary as it sounds. It happens all the time. I sometimes picture the difference in the standard approach to thinking about theory and policy, and the muddling approach approaches to thinking about theory and policy in reference to the building of medieval cathedrals. These cathedrals were built following a muddling through approach. They did not rely on scientific laws to guide the building, but instead on accumulated rules of thumb of what worked and what didn't. The building proceeded by trial and error. Different methods of construction would be pushed to the limit until a cathedral caved in somewhere, and then the rules of thumb would change. As the stored knowledge increased, the cathedrals became more grandiose, even without a specific understanding of the laws underlying them. That came much later.

Muddling through policy follows that same approach. It is conducting policy without a full knowledge of the general laws of the economy, if there are any. What you can find, at best, are general rules of thumb for how things have worked in the past, and possibly some exploitable patterns. Economics of control welfare economics follows a different approach to policy; it is basing policy on the underlying architectural plans of the economy.

In Defense of Theory: The Need for Slow Pattern Completers

If my argument is that much of applied work can take place independent of theory, what then is the role for theory? Wouldn't it be best to do economics without models—to simply analyze the economy using our common sense and intuition. I think not for two reasons. The first is that an atheoretical approach offers no hope for advancement in the future. We could build cathedrals without theory, but at some point some theory will help. It would have been almost impossible to develop modern skyscrapers without the theoretical work in physics. Maintaining a set of individuals struggling with theory keeps individuals in the profession who can integrate new analytic developments when they occur. Without wild eyed theorists economists would remain tied to current methods—searching in the dark forever. I can list numerous failures in theoretical economics—computable general equilibrium, LINK—large macro models. But the failures also involved considerable learning by doing, and the people who are leading the way in the development of the study of complex systems are theorists. You fail the first time, but you learn by failing.

[6] It is not surprising that the "general to specific" approach to macro econometrics has developed in Europe where, until recently, incentives for publishing were weaker, rather than the United States, where there are strong incentives for publication. I say this because the "general to specific" approach is less conducive to publishing articles than is the "specific to general" approach that is dominant in the US. The reason is that the cointegrated VAR approach requires the econometrician to pull information from data and test tightly many alternative specifications, rather than to assume a structural model and test loosely for general fit.

My second reason for favoring theory, even is most of the work will be irrelevant, is to counter a tendency of "intuitive economists" to be too fast pattern completers. As psychologist, Andrew Clark, (1993) has observed, human brains are "associative engines" that can be thought of as fast pattern completers. They can, from very little observation, complete patterns that are much more intricate than the observations. Brian Arthur gives an example: you see a black swishy tail, and conclude that there's a cat.

This tendency to be fast pattern completers has advantages and disadvantages; it is extremely helpful living in the wild. If you see some object charging you, you don't want to sit around and debate what it is; you want to move quickly; you don't want to spend a lot of time making sure that it is a bull; natural selection weeds out slow-pattern completers. But in a world where division of labor is possible, the need for such fast pattern completion is reduced. Some group of individuals can be assigned to be *slow pattern completers*—to contemplate questions that have no immediate payoff, but which may provide us with firmer patterns in the future. That's the role I see for economic theorists. They are scientists, whereas applied policy economists are engineers.

What We Teach

Let me conclude with a discussion of the implications of my "theory as pattern developer" approach suggests about the teaching economics. In my view, the problem in economics today less in what economists do—that seems to be moving along the right track, and more in what we teach, and how we teach it. At most economic graduate schools the core courses focus almost completely on abstract theory. They provide no context for the theory, almost no discussion about applicability, the reason the theory is important, or how the theory will be used. The problem is that most of the graduates will not be going into theory; they will be going into applied work in one area or another, and it is only in the second or third year that they begin to get into applied work. Just one example: One of the interesting results of my recent survey and interviews with top graduate students (Colander, 2005-b) was that student's views about monetary and fiscal policy had changed, and that, for example, Chicago students saw fiscal policy as more effective than Harvard students did. When I asked students about this, I was told that the views represented their undergraduate views—that their graduate macro course did not mention either monetary or fiscal policy. One of them stated that it might have been there in one of the variables, but he certainly didn't know where.

Except at Chicago, the core course in micro was the same; it focused almost completely on the technicalities of theory, and techniques. This is hardly the type of training that will give students a good foundation in theory for doing applied work, or a good feeling for the patterns that theory provides. Instead, it turns the students off to theory completely, and leads them to an atheoretical approach where they don't take theory seriously. In the US, at least, I believe that we could improve both theory and pedagogy enormously by teaching less theory in the core, and making it a separate track that students can follow.[7]

[7] The US and Europe are different in their approaches to teaching, and, over the next year, I look forward to learning from Europeans how pedagogy is done here is Europe, and whether my observations from the US carry over to European economics.

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