What Post-Crisis Changes Does the Economics Discipline Need?: Beware of Theory Envy![*]

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During my first week as a graduate student in economics, I attended a pleasant social event organized by the more senior students in the department, a Saturday evening party that was meant to help first-year students ease into the demanding program. I sat down among a group of fourth-year students, and they kindly offered to give me the lay of the land regarding the challenges of the first-year curriculum. After several beers, they began to talk more expansively about economics and economists. One student said "if you took all the economists in the world and laid them end to end, they'd never reach a conclusion". Another student added, "economists have forecasted five out of the past three recessions". A third student claimed that President Harry Truman once asked for a one-armed economist to serve as his advisor and when asked why, Truman replied "So he can't say 'on the one hand, but on the other hand'". But the story I remember best was told by a graduating student who had worked as a research assistant for the head of the Council of Economic Advisors, a prestigious position that gave him unique insights into how economic policy was formulated. His story was about the glory days of the former Soviet Union, where the military would hold a parade each year to celebrate May Day, and leading the procession of tanks, missiles, and bazooka-bearing infantry marching proudly toward Red Square was a row of goose-stepping economists. One spectator turned to his companion and asked, "Comrade, why are there economists marching in this military parade?", to which his friend replied, "Comrade, do you have any idea how much damage they can do?!".

Were these funny? I suppose you had to be there. From my undergraduate economics classes, I had an idea about what they meant: the imprecision of economic forecasts, the raging debate among leading economists regarding the effectiveness of monetary policy, and the unintended consequences of regulation and policy interventions. I knew all of these things, but I presumed

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that the more advanced economics courses I was about to take would go beyond these limitations. I was dead wrong. After a semester of graduate-level microeconomics, macroeconomics, and econometrics, I came to the depressing realization that most of what I had heard at that cocktail party were not, in fact, jokes. At best, they were examples of gallows humor by economists who knew all too well how inexact a "science" economics really was. At the end of that first semester, I started filling out applications for law school.

What saved me from that ignominious fate I will describe later, but the uneasiness I felt as a graduate student was apparently more widespread than I had appreciated back then. The seeds of conflict in economics had been sown decades earlier by visionaries and iconoclasts such as Thorstein Veblen, Joseph Schumpeter, Maurice Allais, John Maynard Keynes, and most notably, Herbert Simon, a well-respected mathematical economist who abandoned the field of economics in the 1950's to study human behavior using computer simulations and psychology. Often acknowledged as one of the founding fathers of artificial intelligence, Simon argued that economics was based on a faulty premise: individual rationality. From his perspective, people rarely made "optimal" decisions as most economic theories assumed, but behaved according to simple rules of thumb that generally were not optimal but merely satisfactory. This idea of "satisficing" behavior—a term Simon coined to provide a concrete alternative to the optimizing behavior that is sacred to most mainstream economists—was greeted with hostility and disdain by the economics profession, which may have explained Simon's eventual shift from economics to computer science.

Despite these early critics, economics today is still dominated by a single paradigm of human behavior, a testament to the extraordinary achievements of one individual: Paul A. Samuelson. In 1947, Samuelson published his Ph.D. thesis titled *Foundations of Economics Analysis*, which might have seemed presumptuous—especially coming from a doctoral candidate—were it not for the fact that it did, indeed, become the foundations of modern economic analysis. In contrast to much of the economics literature at the time, which was often based on relatively informal discourse and diagrammatic exposition, Samuelson developed a formal mathematical framework for economic analysis that could be applied to a number of seemingly unrelated contexts. Samuelson's (1947, p. 3) opening paragraph made his intention explicit (italics are Samuelson's):

> *The existence of analogies between central features of various theories implies the existence of a general theory which underlies the particular theories and unifies them with respect to those central features.* This fundamental principle of generalization by abstraction was enunciated by the eminent American mathematician E.H. Moore more than thirty years ago. It is the purpose of the pages that follow to work out its implications for theoretical and applied economics.

He then proceeded to lay the foundations for what is now known as microeconomics, the subject of the first graduate-level course in every economics Ph.D. program today. Along the way, Samuelson also made major contributions to welfare economics, general equilibrium theory, comparative static analysis, and business-cycle theory, all in a single doctoral dissertation!

If there is a theme to Samuelson's thesis, it is the systematic application of scientific principles to economic analysis, much like the approach of modern physics. This was no coincidence. In Samuelson's (1998, p. 1,376) fascinating account of the intellectual origins of his dissertation, he acknowledged the following:

> Perhaps most relevant of all for the genesis of *Foundations*, Edwin Bidwell Wilson (1879–1964) was at Harvard. Wilson was the great Willard Gibbs's last (and, essentially only) protégé at Yale. He was a mathematician, a mathematical physicist, a mathematical statistician, a mathematical economist, a polymath who had done first-class work in many fields of the natural and social sciences. I was perhaps his only disciple… I was vaccinated early to understand that economics and physics could share the same formal mathematical theorems (Euler's theorem on homogeneous functions, Weierstrass's theorems on constrained maxima, Jacobi determinant identities underlying Le Chatelier reactions, etc.), while still not resting on the same empirical foundations and certainties.

Much of the economics and finance literature since *Foundations* has followed Samuelson's lead in attempting to deduce implications from certain postulates such as utility maximization, the absence of arbitrage, or the equalization of supply and demand. In fact, one of the most recent milestones in economics—rational expectations—is founded on a single postulate, around which a large and still-growing literature has developed.

In a recent article co-authored with physicist Mark Mueller (2010), I have argued that this research program is a reflection of a peculiar psychological disorder that seems to afflict economists exclusively: physics envy. We economists wish to explain 99% of all observable phenomena by three simple laws, like the physicists do, but we have to settle, instead, for 99 laws that explain only 3%, which is terribly frustrating! However, several physicists have pointed out to me that if economists genuinely envied them, they would place much greater emphasis on empirical verification of theoretical predictions, and show much less attachment to theories rejected by the data, neither of which seems to characterize our profession.[1] In fact, I believe we suffer from a much more serious affliction: theory envy.

The exalted role of theory in economics is not due to Samuelson alone, but was created by the cumulative efforts of a number of intellectual giants responsible for a renaissance in mathematical economics during the half century following the Second World War. One of these giants, Gerard Debreu, provides an eye-witness account of this remarkably fertile period (Debreu, 1991, p. 2): "Before the contemporary period of the past five decades, theoretical physics had been an inaccessible ideal toward which economic theory sometimes strove. During

[1] I'm especially grateful to Doyne Farmer, Paul Mende, and Mark Mueller for many illuminating discussions about the differences and similarities between physicists and economists.

that period, this striving became a powerful stimulus in the mathematization of economic theory". What Debreu is referring to is a series of breakthroughs that not only greatly expanded our understanding of economic theory, but also held out the tantalizing possibility of practical applications involving fiscal and monetary policy, financial stability, and central planning. These breakthroughs include:

- Game theory (von Neumann and Morganstern, 1944; Nash, 1951)
- General equilibrium theory (Debreu, 1959)
- Economics of uncertainty (Arrow, 1964)
- Long-term economic growth (Solow, 1956)
- Portfolio theory and capital-asset pricing (Markowitz, 1954; Sharpe, 1964; Tobin, 1958)
- Option-pricing theory (Black and Scholes, 1973; Merton, 1973)
- Macroeconometric models (Tinbergen, 1956; Klein, 1970)
- Computable general equilibrium models (Scarf, 1973)
- Rational expectations (Muth, 1961; Lucas, 1972)

Many of these contributions have been recognized by Nobel prizes, and they have permanently changed the field of economics from a branch of moral philosophy pursued by gentlemen scholars to a full-fledged scientific endeavor not unlike the deductive process with which Isaac Newton explained the motion of the planets from three simple laws. The mathematization of neoclassical economics is now largely complete, with dynamic stochastic general equilibrium models, rational expectations, and sophisticated econometric techniques having replaced the less rigorous arguments of the previous generation of economists. But something is missing.

Even as Samuelson wrote his remarkable *Foundations*, he was well aware of the limitations of a purely deductive approach. In his introduction, he offered the following admonition (Samuelson, 1947, p. 3):

> …[O]nly the smallest fraction of economic writings, theoretical and applied, has been concerned with the derivation of *operationally meaningful* theorems. In part at least this has been the result of the bad methodological preconceptions that economic laws deduced from *a priori* assumptions possessed rigor and validity independently of any empirical human behavior. But only a very few economists have gone so far as this. The majority would have been glad to enunciate meaningful theorems if any had occurred to them. In fact, the literature abounds with false generalization.
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> We do not have to dig deep to find examples. Literally hundreds of learned papers have been written on the subject of utility. Take a little bad psychology, add a dash of bad philosophy and ethics, and liberal quantities of bad logic, and any economist can prove that the demand curve for a commodity is negatively inclined.

This surprisingly wise and prescient passage is as germane today as it was over half a century ago when it was first written, and all the more remarkable that it was penned by a twenty-something year-old graduate student. The combination of analytical rigor and practical relevance was to become a hallmark of Samuelson's research throughout his career, and despite his theoretical bent, his command of industry practices and market dynamics was astonishing. Less gifted economists might have been able to employ similar mathematical tools and parrot his scientific demeanor, but few would be able to match Samuelson's ability to distill the economic essence of a problem and then solve it as elegantly and completely.

Unlike physics, in which pure mathematical logic can often yield useful insights and intuition about physical phenomena, Samuelson's caveat reminds us that a purely deductive approach may not always be appropriate for economic analysis. As impressive as the achievements of modern physics are, physical systems are inherently simpler and more stable than economic systems, hence deduction based on a few fundamental postulates is likely to be more successful in the former case than in the latter. Conservation laws, symmetry, and the isotropic nature of space are powerful ideas in physics that do not have exact counterparts in economics because of the nature of economic interactions and the types of uncertainty involved.

And yet economics has become the envy of the other social sciences, in which there are apparently even fewer unifying principles and operationally meaningful theorems. Despite the well-known factions within economics, there is significant consensus among practicing economists regarding the common framework of supply and demand, the principle of comparative advantage, the Law of One Price, income and substitution effects, net present value relations and the time value of money, externalities and the role of government, etc. While false generalizations certainly abound among academics of all persuasions, economics does contain many true generalizations as well, and these successes highlight important commonalities between economics and the other sciences.

Samuelson's genius was to be able to deduce operationally meaningful theorems despite the greater uncertainty of economic phenomena. In this respect, perhaps the differences between physics and economics are not fundamental, but are due, instead, to differences in degree along two dimensions: the amount of uncertainty and the role of empirical verification.

Although physics is no stranger to randomness—as the Heisenberg uncertainty principle and quantum mechanics attest—nevertheless the vast majority of observable physical phenomena can be explained by relatively simple deterministic relationships like "force equals mass times acceleration" or "attraction is inversely proportional to the square of the distance". Moreover, such relationships have been in place and largely unchanged for the past 13.7 billion years. Economics knows no such simplicity or stability. Accordingly, controlled experimentation and empirical validation is possible in physics, and most theories can be conclusively affirmed or rejected, leading to a much closer collaboration between theorists and experimentalists. Not so in economics. Because our models are far less predictive—I believe "stylized models" is the politically correct euphemism here—empirical research is often more an exercise in exploratory data analysis than a formal and definitive test of theory.

As a result, economic theories do not die easy deaths, but go in and out of fashion instead. Keynesian macroeconomics was the dominant theory in the aftermath of World War II among academics and policymakers, only to be discredited by the Lucas critique in the 1970s and replaced by rational expectations and stochastic dynamic general equilibrium models, which, in turn, has lost some credibility due to their failure to capture the recent financial crisis, paving the way for renewed interest in Keynes once more. No wonder Truman asked for a one-armed economist!

This cyclical nature of the history of economic thought suggests that cultural, political, and historical circumstances may play a more influential role in economics than in the hard sciences, an inevitable consequence of the fact human behavior is at the center of our discipline. As a result, economics can never rid itself of all imprecision and uncertainty. As the great physicist Richard Feynman put it, “Imagine how much harder physics would be if electrons had feelings”. However, until recently, mainstream economics has largely shunned the behavioral aspects of economic realities, preferring to focus, instead, on the orderly, antiseptic, and internally consistent theories of expected utility maximization, rational expectations, and efficient markets, even in the face of numerous well-documented and repeatable experimental and empirical violations. Theoretical foundations have become a hallmark of economics, making it unique among the social sciences, but any virtue can become a vice when taken to the extreme of theory envy. While economics has produced many genuine breakthroughs over the past half century, other fields have also developed unique insights about human behavior, and the intellectual gains from trade between these disciplines may be substantial.

This idea is embodied in the notion of “consilience”, a term re-introduced into the popular lexicon by E. O. Wilson (1998), who attributes its first use to William Whewell’s 1840 treatise *The Philosophy of the Inductive Sciences* in which Whewell wrote, “The Consilience of Inductions takes place when an Induction, obtained from one class of facts, coincides with an Induction, obtained from another different class. This Consilience is a test of the truth of the Theory in which it occurs”. In comparing the rate of progress in the medical vs. the social sciences, Wilson (1998, p. 182) makes a sobering observation:

> There is also progress in the social sciences, but it is much slower, and not at all animated by the same information flow and optimistic spirit…
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> The crucial difference between the two domains is consilience: The medical sciences have it and the social sciences do not. Medical scientists build upon a coherent foundation of molecular and cell biology. They pursue elements of health and illness all the way down to the level of biophysical chemistry…
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> Social scientists by and large spurn the idea of the hierarchical ordering of knowledge that unites and drives the natural sciences. Split into independent cadres, they stress precision in words within their specialty but seldom speak the same technical language from one specialty to the next.

This is a bitter pill for economists to swallow, but it provides a clear directive for improving the status quo.

Rather than focusing solely on elegant theories, economics is at its Samuelsonian best when it marries rigorous theoretical and empirical analysis with practical challenges, using realistic assumptions regarding human cognitive abilities, institutional constraints, and transaction costs. Economists need not shy away from sophisticated mathematics, but technique should be the servant, not the master, of a greater purpose. And when such analysis leads to a fork in research paths, one leading to elegant theory under counterfactual assumptions and the other to messier but more realistic empirical or experimental results, the latter should be given at least as much priority as the former. In this respect, economics may benefit from the examples of anthropology, psychology, the cognitive sciences, and medicine, in which theories emerge inductively as well as deductively, motivated by empirical regularities and anomalies, and expeditiously discarded when they cannot explain the data.

This approach also has significant implications for how economics is taught. Unlike mathematics and physics, where gifted students can quickly develop intuition for some of the most fundamental concepts of the discipline, economics demands considerably greater institutional and historical context to achieve a comparable level of understanding. Although students of any introductory economics course can easily regurgitate the mathematics of supply and demand, until they witness the price-discovery process in action in a market that they care about, it is virtually impossible for them to fully appreciate both the power of the Marshallian cross as well as its many limitations in the face of trading costs, uncertainty, limited and asymmetric information, and institutional rigidities. As a result, the "enfant terrible" is almost unheard of in economics, while commonplace in mathematics and the basic sciences.[2] One needs a minimum level of exposure to economic contexts and behavior before being able to develop new insights into its inner workings.

Therefore, one natural innovation is to teach economics not from an axiomatic and technique-oriented perspective, but by posing challenges that can only be addressed through economic logic. Instead of starting microeconomics with the consumer's problem of maximizing utility subject to a budget constraint, begin by challenging students to predict the impact of a gasoline tax on the price of gasoline, or asking them to explain why diamonds are so much more expensive than water, despite the fact that the latter is critical for survival unlike the former. Instead of starting macroeconomics with national income accounts, begin with the question of how to measure and manage the wealth of nations, or why inflation can be so disruptive to economic growth. Without the proper institutional, political, and historical context in which to interpret economic models, constrained optimization methods and fixed-point existence proofs have much less meaning and are more likely to give rise to theory envy. However, when

[2] Paul Samuelson, discussed above, and Ronald Coase—whose seminal article "The Nature of the Firm" was apparently based on his undergraduate thesis—are perhaps the exceptions that prove this rule?

students understand the "why" of their course of study, even the most complex mathematical tools can be mastered and are almost always applied more meaningfully.

For Ph.D. students, gaining exposure to live economic environments before they begin writing dissertations should be a priority, and can be accomplished via industry and government internships or field work. By observing or participating in real economic activity in the domain of their likely field of specialization, students will develop a much deeper sense of purpose as they begin their research careers in economics.

In my own case, this connection between theory and practice was the turning point for me during that fateful first semester in graduate school. I was saved by a single random conversation with a friend who suggested that I take a class at MIT taught by some professor named Merton. Such was my state of ignorance at the time that I had no idea what finance was about (balancing your checkbook?), much less who Robert Merton was or his singular role in developing the field I was about to choose for my career. Merton's lectures were unusually inspiring because of the remarkably close interplay between theory and practice in every topic he tackled, to an extent I had never seen before in any other branch of the Dismal Science. Like all the other students who attended his lectures during those years, I now feel greatly privileged to have been an eyewitness during this formative period of modern financial economics.

With the benefit of hindsight and some practical experience, I now understand that because economic decisions often imply winners and losers, public policy decisions will never be completely free of political considerations, nor should they be. But the demarcation between objective scientific analysis and political considerations in which such analysis is just one of several inputs should be made as explicit as possible. When economists proffer dogmatic policy prescriptions motivated more by their own politics and arrogance than scientific evidence, they undermine the credibility of the entire profession. The recent financial crisis has exposed some serious gaps in our understanding of the global economy, and the need to take stock and get our academic house in order has never been greater. This presents us with a precious opportunity to make wholesale changes to our discipline that would otherwise be impossible, so we should delay no longer. As Rahm Emanuel said, "Crisis is a terrible thing to waste".

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