What Economics Majors and Economists Should Know About the Supply and Demand Model: 5 Is the Supply and Demand Model Empirically Useful? 1/2/18

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"The only empirical research that has contributed to thinking about substantive issues and the development of economics is pragmatic empirical work …" Lawrence H. Summers: "The Scientific Illusion in Empirical Macroeconomics."

INTRODUCTION

This is the fifth of eight interlinked but largely independent papers in which I criticize the way that the supply and demand model is taught at the intermediate and advanced microeconomics levels.[1] In Paper 1 I argued that the supply and demand model is a qualitative model, but that is not a problem if we can put the model on a sound empirical footing. Unfortunately I believe that this cannot be done.

The paper contains a brief, but very skeptical, discussion of the empirical status of the supply and demand model. In Section 1 I discuss the importance of the assumption of perfect competition for the supply and demand model. In Section 2 I argue that attempts to add empirical flesh to the theoretical supply and demand skeleton are doomed to failure because of the transient nature of economic phenomena, both in time and across space. I briefly discuss the identification problem. In Section 3 I argue that the supply and demand model is an explanatory model, not a predictive model. In Section 4 I show how the *ceteris paribus* assumption immunizes the supply and demand model from attempts to refute it.

[1] Apart from a few diagrams economists have known most of the material in these papers for at least a half-century. But much of this material has still not made its way into the intermediate microeconomics texts and courses.

1. PERFECT COMPETITION

The center stone that holds up the supply and demand and the AD models is perfect competition, specifically the assumption that all transactors are price takers.[2] Only under perfect competition is there a well-defined supply function, in other market forms one of blade of Marshall's scissors is missing. The supply and demand model was never designed to describe the behavior of real world firms; its objective was to illuminate resource allocation and price mechanisms.

The number of real world markets that have the necessary very large numbers of very small transactors is quite small. Even fewer of those markets, for example, commodity markets, are ones that most people trade in. As for transactors, although the standard narrative is phrased in terms of households and firms, this terminology is designed is to invoke the penumbra of associations that these words have in real world situations. But the transactors in the supply and demand and AD models are really only buyers and sellers (whose roles are reversed as we switch between product and resource markets). The firm in perfect competition, and in economics in general, is a black box, an input output machine, a production function that is designed to output a profit maximizing quantity supplied for any given price and state of technological knowledge. The firm has no internal structure, produces only a single output, and has only a single objective, profit maximization (which is only well defined in situations of certainty, circumstances that few real world firms encounter.)[3]

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[2] Arrow (1959) rapidly drops the assumption of perfect competition and builds a monopoly model arguing, "that a competitive firm is a monopolist with a special environment … a monopolist confronted with a perfectly elastic demand curve." I would argue, but not in detail here, that the perfectly competitive firm does not possess a demand curve in the usual sense of the term. There are only three possibilities. Firstly, the firm is faced by the going price and its output is equal to the quantity at which that price is equal to the form's marginal cost. Secondly, if the firm is willing to sell at a price below the equilibrium price notionally it can capture the whole market although how this price information is conveyed to consumers is never spelled out (there are no institutions in this model.) Thirdly, the firm could insist on an above equilibrium price, in which case it would find no buyers. The demand curve appears to be the vertical axis for all prices above the equilibrium price, and the one point on the marginal cost curve corresponding at the height of the equilibrium price.

[3] For the fifteen or so years after World War II many distinguished economists sought to apply the techniques of operations research developed during the war to the solution of problems, especially inventory control, to the operations of real world firms. The British physicist P. M. S. Blackett, defined Operational Research – the British term for what Americans called Operations Research – as "social science done in collaboration with and on behalf of executives" Blackett's analysis of convoy protection essentially re-discovered marginal analysis. Wagner, which has

The standard exposition of the supply and demand model is an analytical exercise with an abstract representative commodity.[4] Any references to bushels of wheat or barrels of oil are merely notional, designed to avoid invoking the depressingly dull widget. However, not any good will do. The good must be nondurable; if we wish to analyze the housing market or the car market then we must take account of existing stocks of the goods, not simply flows.[5] Even the quantity demanded of pairs of jeans or cans of soup depends on the number of those items that the consumer already possesses. Non-durability also means that firms cannot hold inventories; they must sell before their output perishes, although this cannot be a problem in a model that is always in equilibrium. The standard supply and demand model is strictly applicable only to a very limited

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excellent chapters on all of the major topics of what would now be regarded as part of Operations Management, has a fine bibliography that covers all of the major publications by economists. Baumol (1962) provided an intermediate microeconomics textbook account of some of this literature. Koopmans (1957, sec. III.4) discusses the overlap between economics and operations research during this period. Much of this research was published in the *Bell Journal of Economics* (later retitled the *Rand Journal of Economics*).

Arrow was a major force in this movement and was instrumental in setting up the Operations Research Department (currently the Department of Management Science and Engineering) at Stanford (and later helped initiate the program that evolved into the Santa Fe Institute).

The vein either petered out or was abandoned in order to explore what looked like potentially more productive seams. Or perhaps economists remembered Pigou's disdain for acquiring knowledge of how businesses are operated. See Pigou's exchange with Clapham over the empty economic boxes, where Pigou (1922, 463) wrote: "It is not the business of economists to teach woollen manufacturers how to make and sell wool, or brewers how to make and sell beer, or any other business men how to do their job. If that was what we were out for, we should, I imagine, immediately quit our desks and get somebody doubtless at a heavy premium, for we should be thoroughly inefficient-to take us into his woollen mill or his brewery."

In the same period French economists were applying economic theory to the public enterprises, for example, Massé's use of marginal cost pricing, the Tarif Vert, in the allocation of electricity in France (see Drèze (1964)).

The theory of the firm, which we teach to undergraduates in 2017, is essentially the same theory that I was taught in 1957 and even then the theory was thirty or more years old. Majors should be reminded that very few economists have practical experience of firms. See also footnote 100.

[4] Lipsey (2001, 187) observes: "No one ever thought that the real world was diffused with perfect competition …", but someone attending an Intermediate Microeconomics course might think so. However, see Hicks (1939, 83-85).

[5] Bushaw and Clower (1954, 1957) discussed stock-flow microeconomic models long before they became a staple of macroeconomics. See also Smale (1976, 291)

range of commodities such as a hamburger purchased from a fast-food restaurant or a service such as an hour's legal work.[6]

The standard narrative distinguishes between the short-run (essentially a period in which there is a fixed factor, capital, and no entry or exit), and the long-run (when all inputs are fully adjustable and equilibrium is achieved by entry or exit until all firms are making normal profits – a normal rate of return on capital adjusted for risk). But there is only one supply and demand model and that model is timeless. The short-run/long-run distinction is not simply vague; it is incompatible with the way in which the model is specified.[7]

Finally, because of the static nature of the supply and demand model, competition in perfect competition is reduced to cost minimization; a far cry from the dynamic nature of competition in real world markets, where cost minimization is only one of the arrows in the firm's quiver.

2. EMPIRICAL SUPPLY AND DEMAND MODELS

If you were to ask a group of non-economists: "Do you think that economists spend a lot of time studying supply and demand models and using them to study real world markets?" you would almost certainly get a majority of affirmative answers. Non-economists touchingly believe that economists know what they are talking about, and since microeconomics is about markets and the market system they assume economists must know a lot about markets.[8]

"The practical men" and the "madmen in authority" of Keynes' celebrated final paragraph (1936, 383-384), "distill their frenzy" probably from some half remembered supply and demand analysis learned in a principles class taken – reluctantly – many years ago. Non-economists' ideas such as market fundamentalism are based on a simplistic interpretation of what economists have written about the nice properties of single-good markets, and the false belief that economists know a lot about real world markets when what we really

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[6] In the United States the Fortune 500 companies produce about two thirds of GDP. They are not perfectly competitive firms.

[7] The short-run/long-run distinction seems to refer to periods of time of different duration, but this is incompatible with the equilibrium, timeless, formulation of the model. Further, there is no operational specification of how to determine what is a normal as opposed to an abnormal rate of return. And, entry and exit must involve a period of time long enough for the adjustment to take place. Majors should know that the short-run/long-run distinction is meaningless.

[8] Most Intermediate Microeconomics texts have, at best, only a few very stylized examples of actual markets. Furthermore, most of the examples in the intermediate texts are ten or more years old and many much older than that. Ten years is a long time in the life of an undergraduate.

know is a lot about the AD model.[9] Non-economists believe that economists' assertions about, say, the market for crude oil, are based on an empirically well-established model. But where are these empirical supply and demand models? Although estimated supply and demand models do exist, how many of their builders would, to adapt Solow's phrase, "be willing to lay out in the sun, let alone be burnt at the stake" in defense of those models? Attempts to unearth examples of estimated supply and demand models do not yield many treasures, perhaps because computable general equilibrium models have supplanted simple empirical supply and demand models.[10] A deeply cynical person might assume that economists do not waste their time estimating things that it does not make sense to estimate, and would not be publishable if estimated.

[IF ANYONE KNOWS OF AN EMPIRICAL SUPPLY AND DEMAND MODEL THAT HAS PERFORMED WELL OVER TIME AND/OR SPACE PLEASE LET ME HAVE A CITATION.]

In *The Nature and Significance of Economic Science* Robbins not only formulated what is probably the most widely adopted definition of economics, but he also expressed extreme skepticism about the possibility that economics could be put on a sound empirical footing, a part of his message that economists have largely dismissed or simply forgotten.[11] Robbins (1932, 107-110) argued that any attempt to estimate statistical demand and supply curves would be vitiated by the fact that economic phenomena are specific to place and fleeting in time.[12]

Robbins would argue that a model of the market for gasoline estimated for Washington State in January 2017 would have little success in predicting the price and output of gasoline in Washington State in January 2018 and even less success in January 2020. Similarly, Robbins would question whether the estimated model for gasoline in Seattle in January 2017 could tell us anything useful about the market for gasoline in New York or Paris or London in 2017.

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[9] McMillan (2002) provides a very interesting nontechnical account of real markets and how they operate.

[10] Computable general equilibrium models, which are boiled down general equilibrium systems, usually start from individual maximization and are forced to make heroic assumptions in order to generate a model that is mathematically and statistically tractable. See Boland (2017, 145).

[11] Keynes, as is well known, was equally skeptical about the value of empirical work in economics.

[12] The originator of the backward-bending labor supply curve (Robbins (1930)) would have looked askance at any attempt to measure one. **LEVITT 2017**

The are few examples of the simultaneous estimation of supply and demand curves that are needed to validate the empirical relevance of the supply and demand model.[13] However, there is one market that economists have extensively discussed and in some cases subjected to empirical investigation, the market for fish or, rather, the markets for fish. There is a superb chapter in Kirman (2011a, chap. 3), which discusses the literature and his own work on fish markets in Marseille and Ancona. Because fish are perishable and the supply is essentially given each day, prices are demand determined.[14] Therefore, fish markets present less of a challenge when estimating supply and demand models.

I suggest that economists attempting to estimate supply and demand models should practice a version of Samuelson's forecasting maxim: If you must estimate supply and demand models, do so often.

3. THE IDENTIFICATION PROBLEM

It is easy for students to fall into the trap of believing that the demand and supply curves that adorn classroom white boards and textbook pages actually exist; exist in the sense that we could, in principle,[15] plot the quantity demanded and supplied at each and every price. In practice, as we have known for ninety years, all we observe are price quantity pairs (or n-tuples) for which there is little likelihood that they are equilibrium observations. In his classic article Working showed that if we are to pick out demand points from supply points then we must have a way to identify them. Although there is an extensive econometric literature on the identification problem its practical implementation in supply and demand estimation is less than impressive.

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[13] Smith's ingenious experiments (Smith (1962) and (1965)) are interesting but in my view not compelling because they seem to be far removed from the sort of market interactions envisaged in the standard supply and demand model. I would argue that List is subject to the same problem. See Levitt and List (2007), although they do not address Smith's work directly, and also Al-Ubaydli *et al* (2017).

[14] Marshall (1890, Book V, chap. 5) and Robbins (1935, 107-8) both discuss the market for fish. Marshall's and Robbins' analyses were purely hypothetical; it is unlikely that either of them ever visited a fish market, anymore than Adam Smith visited a pin factory. See Kirman (2011a), Kirman and Vignes (1991) and Kirman and Vriend (2000) on the Marseilles fish market. Jensen (2007) has an interesting discussion of price adjustment in fish markets on the Kerala coast in South India. MacMillan (2005, 65-68) describes the Tsukiji fish market in Tokyo. See also **Kirman *et al* (2008).**

[15] Stigler (1966, 68) notes that in principle is "a phrase used to denote the combination of incredible circumstances and unbelievable ingenuity", a phrase emblematic of much empirical research in economics.

4. AN EXPLANATORY MODEL?

Most economists, view the supply and demand model as a mathematical formulation of a scientific theory.[16] Science is usually characterized in terms of quantitative predictions; however, in Part 2 I argued that all scientific theories are qualitative, by which I mean that scientific theories merely specify whether the dependent variable will increase or decrease when there is a change in an independent variable; they do not specify by how much the dependent variable will change. For example, Newton's theory of universal gravitation was a qualitative theory relating two masses, m and M, and d the distance between their centers (appearing as an inverse square but still only qualitatively), and a gravitational parameter, G, which is assumed to be a constant. Because m, M, d and G can be measured with great accuracy the theory allowed excellent predictions of planetary orbits, which lead to attempts to account for anomalies in those orbits that ultimately relegated Newton's theory to a special case.

Quantitative predictions can be attached to qualitative theories in some disciplines, such as physics and chemistry, because they satisfy criteria that other disciplines, such as biology, geology, and climatology do not. (1) Physical systems are extremely well behaved; astronomers assume that elementary particles behave in exactly the same way anywhere in the observable universe (notionally a sphere with a diameter of some 90 billion light years), and that the particles have done so for all but a tiny fraction of the time the universe has existed (about 13.82 billion years). (2) There is a close correspondence between the variables measured by physicists and chemists and the theoretical variables that appear in their models. (3) Physical scientists can make measurements with extreme precision. (4) Physicists and chemists can do controlled experiments in which all of the independent variables except the one in question are actually held constant. Any science that satisfies these criteria I will call a predictive science.

Economics belongs with subjects such as geology, biology, and climatology, the explanatory sciences.[17] These subjects all fail to meet one or more of the

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[16] Rodrik (2015, 83) argues, "what makes economics a science is models." I believe that this is incorrect. Does a theoretical physicist believe that she is practicing science simply because she builds models? Science is a belief system that is usually characterized by its insistence that even cherished theories must be forsaken if the empirical evidence is inconsistent with what the theory predicts.

[17] There is a striking difference between economists and practitioners of other science disciplines in terms of the amount of factual knowledge that needs to be mastered in order to enter the profession. The average economist knows much less about the economy than the average

four criteria for being a predictive science. But they are sciences in so far as they play by the rules of the scientific method: State your hypotheses as clearly as possible, which usually means formulating the hypotheses mathematically, and attempt to refute your hypotheses as rigorously as possible. A volcanologist can use her understanding of the underlying processes of earth science to warn that earthquakes or volcanic eruptions will occur at some time in the future in some specified region. When the event occurs she can provide an explanation of what happened. Similarly, a meteorologist can explain, after the event, why a hurricane struck with the force that it did, given the path that it took, although she cannot predict that path beforehand with any degree of reliability.

In predictive sciences replication is possible and is essential for a theory to be taken seriously. But replication is not a simple concept, as Duvendack et al. (2015) point out.[18] If economics is to achieve the status of a cumulative discipline then when economists seek to expand on existing research they should at least subject the papers they plan to supplant to what Hamermesh calls "pure replications"; economists should check that the results in a paper can be reproduced using the same data and the same statistical procedures used in the original study. But vey few economists would be happy to stop there. If the original results pass scrutiny then most of us would want to move on to what Hamermesh refers to as "statistical replication": Determining how robust the research is by using additional data or different data sets, more sophisticated statistical methods, and alternative model specifications.[19] Finally, and this is seldom done, the original research could be tested by running "horse races" which compare the ability of different theories to explain a common data set.[20] To my knowledge there are no supply and demand models that have been subject to replications of any kind, let alone have passed these tests.

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biochemist knows about biochemistry. See Koopmans (1957, 145-146) on the division of labor in economics. One reason may be the Pigou view quoted in footnote 205.

I obtained my B.Sc. (Econ) degree from the LSE in Analytical and Descriptive Economics in 1960. In those days I was taught not only economic theory but also facts about the UK economy, its industries and financial institutions, and a considerable amount of economic history, especially the recent economic history of the United Kingdom. I do not know if the LSE still requires that its students know more than economic theory, mathematics and statistics. My majors' knowledge of the salient features of the United States economy and its recent history is, to put it charitably, rather meager. I think that the situation is actually worse at the graduate level.

[18] The 2017 Papers and Proceedings issue of the American Economic Review contains eight papers on replication in economics.

[19] I attempted to replicate the very influential Phillips and Lipsey Phillips curve results (Sleeman (1983 and 2007)). Wulwick (1996) provides a definitive guide to the Phillips and Lipsey data sets.

[20] See Henry *et al* (1976).

Although replication is particularly important in economics because false results can lead to harmful policies,[21] replication has not had a great deal of success in our discipline,[22] in part, I believe, because most economists seem to lack exposure to a laboratory science.[23]

If the supply and demand model has not been subject to careful empirical scrutiny, and seems to lack ex ante predictive power, then why does it maintain its pre-eminence as the quintessentially useful economic model? The answer seems obvious, the model is judged on its ex post explanatory power. But the most important question that we can ask of any claim, hypothesis, or statement is: How would we know if it were wrong? How do we determine if the supply and demand model's explanations are correct? The Ptolomaic system dominated astronomy for twelve hundred years; it was a way of organizing observations to fit a set of pre-conceptions about the nature of the universe and religion that, with some ingenuity, predicted planetary motions adequately for most purposes. Nonetheless, it was based on a false theory of the structure of the solar system and was abandoned because the Copernican system ultimately explained the data more parsimoniously and gave better predictions. Is the supply and demand model more like Ptolemaic astronomy than modern astrophysics?[24] Is it simply a way of organizing our thinking about markets? Not something to be sniffed at, but far from the omniscience that economists like to convey to the uninitiated.

One of the core ideas underlying the supply and demand model is the assumption that we can divide the forces that affect the prices and quantities

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[21] There are much publicized papers that were later shown to have coding or similar basic errors that falsified their conclusions.

[22] See Duvendack *et al*. (2015), and Chang and Li (2015), and Price (2011). Benjamin *et al* (2017) (a paper with 72 authors from many disciplines, including a number of economists) recommend raising the level of significance hurdle to 0.005 as one way to reduce the number published papers that turnout to be of non-replicable. As the authors note simply quoting the p-value and allowing readers to determine what they regard as statistically significant may be the best option. See Econometrics Beat: David Giles' Blog: "May I Show You My Collection of p-Values" 4-8-2011.

[23] A major reason why it is difficult and often impossible to replicate so much published empirical research in economics is the absence of an academic culture that insists that every part of any economic research program must be carefully documented. Lab-books should be mandatory in econometrics courses and an obligatory component of any empirical research in economics. There are very strict rules for keeping lab-books, which are explained in the following links: http://www.sciencebuddies.org/blog/2010/01/lab-notebooks.php and http://www.ruf.rice.edu/~bioslabs/tools/notebook/notebook.html

[24] Lipsey (2001, 187) makes a similar point about the disturbing similarities between economics and Ptolomaic astronomy.

transacted of any good or service into two sets, those that influence buyers and those that influence sellers, and that the sets are roughly disjoint except for the price of the good or service. This approach seems to provide economists with insights into market transactions that seem to elude non-economists.[25] Solow (1974), in a different context, might have been talking about the supply and demand model when he wrote: "I am not one of those people who believe that economics has the last word to say about every subject under the sun … But I do think that economists seem to be the only people who have any systematic way at all of thinking about certain problems." Economists believe that they have a better understanding of how markets work than non-economists do. They defend their claim by pointing out that non-economists seem to make egregious mistakes when advocating interference in markets; economists like to reel off the examples of rent controls, minimum wage laws, agricultural subsidies, protection, indirect taxes, etc. However, these examples show the *ex post* explanatory power of the model, not its usefulness as an *ex ante* predictive system or as a policy tool; and, as I argued in Part 8, some of the predictions we attach to our policy analyses are not strictly derived from the supply and demand policy models. It is surely a triumph of economics that it can explain why the Nixon gas price controls worked so poorly, and a tribute to the overall explanatory power of the supply and demand model that economists could make such good qualitative and atemporal predictions concerning the controls. But when the controls were introduced there were no supply and demand models that provided reliable quantitative predictions about the immediate and longer-term effects of the introduction of the controls, and if there had been such models, then in 2017 they would need to take into account the behavioral changes that have occurred since the oil shock.

5. THE CURSE OF *CETERIS PARIBUS*

Unfortunately, as we saw in Parts 2 and 3, the supply and demand model explains not too little but too much.[26] Economists wielding the supply and

[25] Non-economists and, alas, many of our students, fail to distinguish between shifting a curve and moving along a curve, which requires an understanding of the difference between endogenous and exogenous variables.

[26] Economists have a distressing tendency to believe that the broad foundations of economics are well established and that our task is to construct a suitably impressive edifice on those foundations. Real sciences, such as physics, astronomy, and neuroscience, thrive on anomalies and perceive them as challenges to be met, not things to be quietly swept under the carpet. Lipsey (2010) argues that economists have a tolerance for anomalies, discrepancies between what our theories assume and predict and what we observe, that is not found in other sciences.

demand model can explain *ex post* any change in the real world price–quantity configurations by appealing to a sufficiently ingenious set of shifts and pivots of the supply and demand curves. In the single-market case we can assign specific qualitative changes to one or more of the two shift parameters and generate an equilibrium at any of the infinity of points in the positive orthant, concocting an explanation *ex post*. When even a simple three-good model generates more than 847 billion QCS results there can be very few situations that economists cannot explain in multiple ways. Unless economists can pin down which specific exogenous supply and demand "shifters" (see pages 10 and 11) changed, we end up like the commentators in the financial media who are able to explain every random fluctuation in the stock market. But if the supply and demand modeler has to predict which exogenous variables were the culprits in a particular situation then we are no further forward in our attempts to use the supply and demand model to inform for policy.

Despite what the textbooks and the pundits say, the supply and demand model is only a powerful empirical tool *ex post*, not *ex ante*.

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