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Correspondence

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An Update on Leontief's Complaint

When I was young, the stereotypical economist was an armchair theorist who had never met a payroll and was out of touch with the real world. This stereotype still shows a flicker of life in the writings of distinguished economists, notably Wassily W. Leontief (1982) and Theodore Morgan (1988). "Page after page of professional economic journals," wrote Leontief (p. 104), "are filled with mathematical formulas leading the reader from more or less plausible assumptions to precisely stated but irrelevant theoretical conclusions." He backed up this indictment with a table showing that over two-thirds of the articles published in the *American Economic Review* in the previous ten years had been either "mathematical models without any data" or "analysis without mathematical formulation and data." Morgan extended Leontief's investigation of the *AER* into 1982–86 and compiled similar data for the *Economic Journal* with results that were like Leontief's though not quite as strong.

Using a larger number of journals, a different method, and a later time period, I have reached strikingly different conclusions. In my study less than one-third rather than more than two-thirds of the papers exemplify models without data. More important, two-thirds of such papers are empirically relevant.

In a sample of one issue each of nine leading journals, I found only 37 models without data out of 127 papers altogether (29 percent). Of the remaining papers, 31 included both mathematical modeling and econometric estimation of parameters, 26 used straight econometrics (no new model), 12 were on econometric methods, 12 historical-institutional, 4 eclectic, 2 on the history of economic thought, 2 were papers with abstract models and data, and 1 reported new experiments (another that reported experiments is otherwise classified).

To assess the force of Leontief's complaint for the 37 articles that included models without data, I devised an empirical rating system. The ratings range from zero to 3. A zero means that the model literally meets Leontief's criterion of "more or less arbitrary assumptions [leading] to... irrelevant conclusions." The rating of 1 is given to papers when the empirical relevance is not clear, yet they do not meet Leontief's criterion taken literally. Although these models may have no immediate value for empirical purposes, they may serve as stepping-stones in a research program that will ultimately shed light on the way the world works. If the empirical relevance of a model is clear even though it does not directly explain any empirical phenomenon, the paper is rated 2. The rating of 3 is assigned to papers that directly explain an empirical phenomenon.[1]

Table 1 presents the findings. In the table, "No." means number of papers in the journal issue in the given category, while "%" refers to the share of pages in the journal issue occupied by the specified papers.

The results in Table 1 contrast sharply with those of Leontief and Morgan. The single case of a mathematical model deserving a zero is an aberration, interesting only for what it suggests makes a paper publishable. Without the model, the paper would never have been published in a leading professional journal. This is not a criticism of the authors of that paper, who simply did what they had to do to get the paper published. Dressing up an article in mathematics does not just help make a paper persuasive to other economists. It is absolutely essential. In only twelve other cases was the empirical relevance not clear. Even they have possible value as stepping-stones.

Leontief also complained that economists fail to compile new data. I found some evidence supporting this complaint. In the 127 papers studied, there were only five with new data. Two of these reported experiments, not the kind of data Leontief had in mind. None of the five had reporting of new data as its primary purpose.

[1]For those interested in an example of each rating, Graves, Lee, and Sexton (1989) was considered a mathematical model of zero, although the verbal model in the paper merited a rating of two. Thus, the paper is included in the 2-column of the table, with a footnote under zero. Weil (1989) is an example of a paper that received a 1. Levine (1989) is an example of a paper that received a 2. Faust (1989) is an example of a paper that received a 3. Details of the tabulations are available from the author.

Table 1
Empirical Relevance Ratings for Models Without Data

	0		*1*		*2*		*3*		*Total*	
Journal	*No.*	*%*	*No.*	*%*	*No.*	*%*	*No.*	*%*	*No.*	*%*
Amer. Econ. Rev.	—[a]		1	2	8	20	2	6	11	28
Econometrica			3	13	2	21			5	24
Econ. Journal					4	28			4	28
Int'l J. Ind. Org.			2	19	1	18	2	24	5	61
J. Polit. Econ.			1	11	3	36	2	14	6	61
Rev. Econ. Studies			5	59	1	13			6	72
TOTAL	*		12		19		6		37	

[a]The mathematical model in Graves *et al.* (1989) is rated zero, the verbal model 2. It is listed here under "2".

Note: The specific journal issues listed in the table are: *American Economic Review*, September 1989; *Econometrica*, November 1989; *Economic Journal*, December 1989; *International Journal of Industrial Organization*, April 1989; *Journal of Political Economy*, February 1990; *Review of Economic Studies*, January 1990, Three journals studied had no papers in the category "Models without Data" and are not listed in the table: *Brookings Papers on Economic Activity* 1989:2; *Journal of Economic History*, December 1989; and *Review of Economics and Statistics*, November 1989.

Why do my results differ from those of Leontief and Morgan? Although I have chosen a different sample of journals, the data in Table 1 for the *AER* and the *Economic Journal* suggest that this is not the source of the difference. Though the nine journals chosen can't be called a random sample of recent economic research, it is more likely to exaggerate the number of models without data than to underrepresent them.

One possible answer is that counting the number of papers with models but no data may be misleading. A review of the June 1975 issue of the *AER*, an issue included in Leontief's sample, supports this conjecture. In that issue, thirteen of the papers (out of 30) represented mathematical or verbal analysis without data. Of them, only four have an empirical relevance rating of 1. (Seven papers with models but no data are rated 2, two are rated 3, none is rated zero.) Since such papers are shorter than average, the four accounted for only 6 percent of the pages in the issue.

Changes of editors and editorial policy may have made a difference. In the September 1989 issue of the *AER*, only 37 percent of the papers contained models without data (a much lower proportion than Leontief and Morgan found for their periods), and only one of them has an empirical relevance rating of 1 compared to four in June 1975.

One may doubt (I do) whether many of the papers rated 1 will lead to anything significant. One may suspect (I do) that economists in general and editors in particular overrate such work because it requires a high degree of technical expertise. One may think (I do) that conceptual exploration should be left to Arrow, Debreu, Georgescu, Samuelson and their like, while everybody else focuses on empirical matters. But there is little basis for asserting that all such papers are worthless or for thinking there is a serious misallocation of resources with too much research devoted to modelling without data.

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What We Do With Our Heroes

Science, defined broadly as an open and critical inquiry into how best to describe the world around us, depends upon the continual introduction of new ideas. The play and challenge of new ideas is, after all, what keeps us open, critical and honest. But while economists need and use new ideas just as much as other scientists, they also have a sorry tendency to reshape the past as a vehicle for their own ideas. Jacob Viner put this best when he said, "In the history of economic thought, the highest honor we can bestow upon our heroes is that they agree with us" [1972, p. 81].

An excellent example of Viner's dictum occurs in Paul Davidson's Winter 1991 article in this *Journal*. Davidson explains very well why Post Keynesians do not believe that probability is an appropriate tool for the economist's tool box, but in the process he leaves the historical Keynes in tatters. In particular, Davidson constructs a bipolar world of objective and subjective probabilities and then goes on to explain how Keynes rejected both and embraced a "non-ergodic" vision of the world. The problem with this is that early in his career Keynes explicitly referred to himself as an advocate of an objective theory of probability (1921, p. 3–4) and, indeed referred to these probabilities as "the guide of life" (p. 323).

The problem does not end there, however, for in 1931 Keynes capitulated to Frank Ramsey's (1926) well-known criticism and declared himself a convert to a subjective theory of probability. It is important to note that Ramsey's (and Keynes's) conception of subjective probabilities does not fit the narrow definition provided in Davidson's article, but this points to yet another problem with Davidson's approach: the effect to equate objective probability with relative frequency and subjective probability with Savage's axiomatic system provides a framework which does not explain how Keynes used either objectivity or subjectivity in talking about probability (Bateman, 1987, 1988).

Without going very far into the full story of Keynes's interest in probability, it is possible to see a very different kind of story than the one Davidson tells. Rather than a person who is faced with a narrowly defined choice set of subjective probability, objective probability and non-ergodicity, we find a person who came to probability through an interest and background in at least four distinct areas: ethnics, induction, statistics, and economics. More importantly, we find a scientist with the integrity to throw over fifteen years' of work in the face of criticism he took to be too valid to ignore (Ramsey's). Even this brief look begins to provide us with a picture of the type of person we have come to think of as instrumental to scientific progress in economics—knowledgeable in several fields, open, and critical. Then, too, a whole new set of possibilities and questions comes to mind: what is the influence of Keynes's training in other fields on his work in economics? Why did Keynes change his mind about the objectivity of probabilities? What is the full extent of his relationship to Ramsey? The choice between Davidson's three options, which is crucial to *his* vision, quickly becomes irrelevant and uninteresting to an understanding of Keynes.

The point of this criticism, however, is not to cast opprobrium on Davidson. This sort of problem is endemic, as Viner suggests. Alan Meltzer's recent effort to demonstrate Keynes's affinities with rational expectationists does just as great a disservice to the historical Keynes when it turns to the question of his work in probability. Rather than cast stones at victims of an occupational malady, it seems better to consider the nature of the malady.

Put most simply, this sacrifice of our heroes on the altar of our own ideas is a self-defeating practice for economists. We need and want the introduction of new ideas, but every time we try to make the past conform to current ideas we rob ourselves of the rich diversity which brought us to our present state: we bury the new ideas which produced scientific progress in the past. Thus, each theorist who thinks that science is being advanced by the use of some long dead hero as a comrade-in-arms is actually making science more difficult. The point is not that we lose particular ideas that we need today (although that may be true, too); the point is that we risk losing a sense of ourselves as a truly scientific community when we obscure and bury our past.

Will we ever learn to let our heroes speak to us on their own terms? Is there hope for economists, or are we doomed forever to Viner's dictum? Perhaps what we need is a better division of labor. If the theorists can agree to respect the past and leave the hard work of examining it to those who want to give it back to them as a means of preserving an open scientific dialogue in economics, then I am sure that the historians of thought will be very pleased with the world of diverse theories which the theorists produce in their turn.

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Paul Davidson responds:

Professor Bateman argues that the objective probability, subjective probability, uncertainty taxonomy that I used in discussing "Is Probability Theory

Relevant For Uncertainty?" (Davidson, 1991) "becomes irrelevant and uninteresting to an understanding of Keynes" because by using this tripartite classification I "try to make the past conform to current ideas."

The "Foreword" of the *Journal of Economic Perspectives* (1987, p. 3) states that "the title of the new journal was chosen to reflect two central aspects of its mission: to provide a range of perspectives on economics and to show how an economic perspective can help in understanding society and some of its problems." Given this mission, the primary purpose of my paper was to explain "how the Post Keynesian perspective differs from the orthodox probability approach thereby providing a more general theory which can explain long run... liquidity demands, investment decisions... underemployment equilibrium, the long-run nonneutrality of money, and the unique and important role Keynes assigned to nominal contracts and especially the money wage" (Davidson, 1991, p. 130). Secondly, I tried to encourage the reader to decide which perspective is more relevant in understanding current problems (Davidson, 1990, p. 140).

If one is going to describe differences in perspectives while maintaining a dialogue with those with differing viewpoints, then it is necessary to communicate via a common terminology. Otherwise the discussion can degenerate into a Tower of Babel. The language of stochastic processes, modern expected utility theory [EUT], and Savage's subjective probability analysis is the basic terminology of today's orthodox economists. It is, therefore, appropriate to adopt their terminology and then to cast the Post Keynesian views and Keynes's underlying ideas in this modern "scientific" language.

In the 1930s, the theory of stochastic processes was being developed by the Moscow School of Probability. EUT and Savage's subjective probability analysis are post World War II developments. Obviously Keynes did not have access to these works and language and therefore could not frame his arguments in their terms. Consequently, I can only seek to reinterpret into this modern lexicon Keynes's fine intuition regarding the distinction between uncertain and probable events. Bateman berates me for casting the discussion in this narrow frame of "current ideas"—even though this was necessary to meet the expressed purpose of my paper as well as the conditions in the letter from the Editors of the JEP inviting me to submit a paper on the Post Keynesian perspective.

Professor Bateman suggests other questions that he finds more fascinating in developing "the full story of Keynes' interest in probability." My paper was never intended to discuss such issues as to why Keynes's changed terminology or arguments regarding probability between 1916 and 1931. Neither was I interested in analyzing whether, after Keynes' review of Ramsey in 1931, Keynes's view on probability underwent any further "natural evolution of thought... [which lead to] certain changes in terminology which I felt compelled to make" [Keynes, 1936, p. xxii]. Bateman raises interesting questions regarding Keynes and the evolution of his ideas. Responses to these queries, however, would neither provide the general audience of economists with a comparison of a range of perspectives nor indicate how a non-orthodox perspective furnishes useful insights into the pressing problems of our society. The paper Bateman would have me write would neither have met the JEP's central mission not the Editors' invitational constraints.

Lastly, I strongly disagree with Professor Bateman on his final point. Theorists should never agree to leave the hard work of examining the past to others. As has often been noted, those who do not study history are destined to repeat the errors of the past. Modern day economic theory often suffers from the fact that some of "the best and the brightest" of our profession have not studied the history of economic thought and therefore continue to rediscover the wheel[1] (often in hi-tech form). Even worse, some theorists who do not study the history of thought of their discipline may be destined to rediscover the existence and importance of phlogiston!

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Why Monopsony is Different

Tibor Scitovsky's illuminating analysis of non-price competition in asymmetric markets holds that the combination of monopoly and competition is a powerful engine of growth ("The Benefits of Asymmetric Markets," Winter 1990). Monopoly power ensures a stable market environment for recouping the costs of innovations and the cost of consumer or producer services. Competition ensures that each such instance of monopoly power is temporary. In Scitovsky's words, "non-price competition not only compensates buyers for the monopoly profits they pay, it also improves the Pareto efficiency of markets."

However, in contrast to his detailed discussion of monopolistic competition, Scitovsky provides a much shorter discussion about the monopsonistic markets. He states that implications of monopsonistic relationships are the

[1]Apparently, Barro never read Ricardo and hence did not know that he had rediscovered the Ricardian view of government debt until several years after he published his paper on "Are Government Bonds Net Wealth?" (Barro, 1974).

mirror image of these discussed for the monopolistic competition situation. Here it is contended that such is not the case.

First, the implications for income inequality are more severe in monopsony case. The loss of consumers as result of monopolistic price markup above marginal cost is highly diffused, since consumers spend a small fraction of their income on any single commodity. But the loss to suppliers as a result of a monopsonistic price markdown below is highly concentrated, because such sales of labor or products generally constitute the entire income of a seller.

Buyers and sellers display a second basic difference: buyers have an option of postponing purchase of the commodity, which puts an effective limit on monopoly power. However, the options of postponing the sale of labor service or agricultural produce or handicraft products are negligible. This gives immense power to monopsonist buyers which is often ruthlessly exploited.

Thirdly, in theory at least, a monopolist generally will not find it profitable to operate in a range of demand where elasticity of demand is less than 1. But for a monopsonist, the range is unrestricted, and thus the power of a monopsonist can be very large. For example, if the elasticity of supply of (say) an agricultural commodity is 1/2, the monopsonistic price markdown can be as high as $66\frac{2}{3}$ percent, which means that a monopsonist is paying to the farmer \$1 for a commodity which he sells for \$3. This range of monopoly power is generally unknown, but for monopsony, it is not rare. For example, the exploitation of tribal groups selling forest produce has been phenomenal.

Fourthly, the effect of non-price competition in monopolistic market is to shift the demand curve to the right and make it less elastic by creating consumer loyalty. This affords an opportunity for widening the profit margin. On the other hand, the effect of non-price competition in monopsonistic competition is to shift the supply curve to the right and to make it more elastic, thereby lowering the profit margin. A profit-maximizing monopsonist will therefore weigh the benefits of larger turnover against smaller profit margins.

Finally, consumer loyalty in monopolistic competition is psychological and generally needs no formal entanglements. However, various services that the monopsonistic intermediaries or buyers render to the suppliers of inputs often need a formal tie-up, which may often be repugnant to principles of free contract. For example, extension of credit and warehousing facilities to farmers is generally tied to mandatory deliveries. While monopsonistic buyers may be willing to make specific investments in such cases, general investments which are versatile in nature may be starved while the buyers may be willing to make specific investments. The distinction between job-specific and general training of labor is a case in point.

Because of these differences, it is not clear that monopsonistic competition will further the cause of growth and productivity, in the way that Scitovsky argues that monopolistic competition will. In the markets for forest products, farm products and products of artisans, the buyers are traders, commission agents, wholesalers and exporters. Unlike monopolistic sellers who accrue larger gains, these monopsonistic intermediaries often have little insight or interest in production processes. When these monopsonistic intermediaries become rich, in the midst of toiling and poverty-stricken farmers and artisans, vast social discontent builds up. Governments have been compelled to take remedial measures ranging from enforcement of standard weights and measures and development of regulated markets and warehousing facilities to more complex measures, such as development of co-operative credit and marketing societies, curbing of speculation in commodity markets and state-sponsored procurement of commodities. These measures have been more often perceived as improving the incentives and ability of the farmers and artisans to increase production and productivity, rather than any increase in competition among the monopsonists, which may be very tardy in coming.

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Tibor Scitovsky responds:

Professor Kothari's points are well taken and I stand corrected. I should have said that the parallelism between monopolistic and monopsonistic markets is potential rather than actual, less often realized than it is not.

However, I did refer for a more detailed discussion of the subject to a 1985 paper of mine,[1] which has a section on "Why a sellers' market for labor has failed to develop" that does contain Professor Kothari's first point and elaborates in detail the main reason for that failure, mentioned on top of p. 144 of my paper in this journal.

[1]"Pricetakers' plenty: a neglected benefit of capitalism", *Kyklos*, 1985, *38*, 517–36.

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1. Steven B. Caudill, Franklin G. Mixon, Jr.. 1999. How robust is the rationality assumption in economics? A statistical test based on student grade distributions. *The Social Science Journal* **36**:4, 665-673. [Crossref]