Was There a Marginal Revolution?

Mark Blaug

I

THE TERM "marginal revolution" is usually taken to refer to the nearly simultaneous but completely independent discovery in the early 1870's by Jevons, Menger, and Walras of the principal of diminishing marginal utility as the fundamental building block of a new kind of static microeconomics. It constitutes, so the argument goes, one of the best examples of multiple discoveries in the history of economic thought, which simply cries out for some sort of historical explanation: it is too much to believe that three men working at nearly the same time in such vastly different intellectual climates as those of Manchester, Vienna, and Lausanne could have hit by accident on the same idea; it must be due to some common cause, which it is the job of the intellectual historian to identify. The only trouble is that none of the standard explanations are convincing.[1] The levels of economic development of England, Austria, and France were so different in the 1860's that all crypto-Marxist explanations in terms of changes in the structure of production, or in the relationship between social classes, tend to strain our credulity. Likewise, the utilitarian-empiricist tradition of British philosophy, the neo-Kantian philosophical climate of Austria, and the Cartesian philosophical climate of France simply had no elements in common that could have provoked a utility revolution in economics. In matters of economic policy, there was in fact continuity with classical thinking, and when Jevons and Walras wrote on policy questions, as they did, there was little or no connection between practical recommendations and their views on value theory. As for an alleged "need" to defend the capitalist system, there was hardly

MARK BLAUG *is Professor of Economics of Education at the Institute of Education of the University of London.*

1. I canvassed the various explanations in my *Economic Theory in Retrospect*, 2d ed. (Homewood, Ill., 1968), pp. 303–8. The present note is an attempt to rethink the issue raised in those pages.

anything more suitable than the old wages-population mechanism of classical economics or the writings of Bastiat, which owed nothing to marginal utility. Lastly, there was no real sense of intellectual crisis in the 1860's either in England or on the Continent which might have encouraged a search for alternative economic models; besides, historicism was such an alternative model which continued to gain new adherents after 1860, not only in Germany but also in England. In short, the simultaneous discovery of marginal utility may call for an explanation, but none of the available explanations is satisfactory.

Perhaps the difficulty is that the idea of a "marginal revolution" is the sort of "rational reconstruction" of the history of economic thought, like the concept of "mercantilism" or that of "classical economics" as defined by Keynes, that is bound to generate spurious historical puzzles. This is a large part of the problem, I think, but it is not the whole of it. The debate over the marginal revolution so-called has in fact confused two quite different things: the explanation of the origins of the revolution (if revolution it was) and the explanation of its eventual triumph. Some carelessness in the use of the concept of "explanation" in intellectual history has further clouded the debate.

II

A useful way to begin is to ask ourselves whether the discovery of marginal utility by Jevons, Menger, and Walras was in fact a "multiple," in Robert Merton's sense of the term.[2] After an intensive investigation of hundreds of multiple discoveries in the history of science, Merton concluded that "all scientific discoveries are in principle multiples, including those that on the surface appear to be singletons" (p. 477). Lest this should appear to be "a self-sealing hypothesis, immune to investigation," Merton conceded that it was only true of certain kinds of science at certain stages of their development: "A great variety of evidence . . . testifies then to the hypothesis that, once science has become institutionalized, and significant numbers of men are at work on scientific investigation, the same discoveries will be made independently more than once and that singletons

2. R. K. Merton, "Singletons and Multiples in Scientific Discovery: A Chapter in the Sociology of Science," *Proceedings of the American Philosophical Society* 105, no. 5 (1961).

can be conceived of as forestalled multiples'' (p. 482). Although two-thirds of his 264 intensively investigated multiples involved an interval of ten years or less, Merton refused to confine the concept of multiples to nearly simultaneous discoveries: ''Even discoveries far removed from one another in calendrical time may be instructively construed as 'simultaneous' or nearly so in social and cultural time, depending upon the accumulated state of knowledge in the several cultures and the structures of the several societies in which they appear'' (p. 486). Enough has now been said to indicate that the concept of ''multiples'' is difficult to interpret, particularly in fields less professionalized than the natural sciences. The gist of the argument, however, seems to be that ''mature science'' is characterized by cumulative, continuous progress such as to make the next leap forward, if not absolutely inevitable, at least highly predictable.[3]

We may now ask: Was the state of economic science in the 1860's such as to make the eventual emergence of the marginal utility principle a perfectly predictable phenomenon, in which case it is hardly surprising that Jevons, Menger, and Walras discovered it at just about the same time? The answer to that question must surely be No.

First of all, it is highly doubtful that we can speak of one economic science in the 1860's as if it were a heritage shared between economists all over the world, studying the same treatises, reading the same journals and employing a common set of tools in the analysis of a similar range of problems. A glance at Hutchison's terse accounts of the state of economic thought around 1870 in England, Germany, Austria, France, and the United States will show that there were at least two, if not three or four, ''models'' of economic science extant at that time.[4] Although Jevons struggled against the tyranny of Mill's influence, German economists had long since rejected ''Smith-

3. Merton guards himself against misinterpretation by denying that his thesis implies that ''all discoveries are inevitable in the sense that, come what may, they will be made, at the time and the place, if not by the individual(s) who in fact made them'' (p. 485). For a similar qualification, see his ''Resistance to the Systematic Study of Multiple Discoveries in Science,'' *European Journal of Sociology* 4, no. 2 (1963): 246.

4. T. W. Hutchison, *A Review of Economic Doctrines, 1870–1929* (Oxford, 1953), chaps. 1, 8, 12, and 16; see also the writers cited by Jevons, Menger, and Walras in their treatises, with hardly a name in common. R. S. Howey, *The Rise of the Marginal Utility School, 1870–1889* (Lawrence, Kans., 1960), chaps. 1–5.

ianismus'' and all Ricardian varieties thereof, while French economists for their part never exhibited much interest either in the analytical features of English classical political economy or in the rallying cries of the German historical school. The insularity of British economics and the lack of communication between economists in different countries right up to the 1890's[5] is perfectly exemplified by the fact that Jevons, a leading economic bibliophile, died in 1882 without realizing that a man called Menger had written a book on economics which would one day be likened to his own *Theory of Political Economy*. Secondly, the notion that economic science as such was inexorably moving towards the discovery of marginal utility somewhere around the middle of the century is simply a rationalization after the fact. Surely, the much more likely next step in English classical economics in the 1860's was either the generalization of the marginal concept in Ricardian rent theory to all factors of production, that is, the breakthrough to a marginal productivity theory of factor pricing, or perhaps the further refinement of Ricardian value theory into something like linear input-output analysis? But as we know, the former came only belatedly in the 1890's among the generation that succeeded our marginal utility trio, and the latter has only emerged in the twentieth century.

What of the counterargument, however, that marginal utility was not discovered but only rediscovered in the 1870's? Lloyd and Longfield had developed the distinction between total and marginal utility in 1834, followed soon after by Senior (I ignore Bernoulli in the eighteenth century as an ''outlier''). If Jevons, Menger, and Walras do not constitute a ''multiple,'' perhaps Lloyd, Longfield, and Senior deserve the title. But Lloyd, Longfield, and Senior made little substantive use of marginal utility and thus only illustrate Whitehead's adage that everything new has been said before by someone who did not discover it. The same objection does not apply to Dupuit (1844), Gossen (1854), and Jennings (1855), all of whom not only rediscovered marginal utility but employed it to analyze consumer behavior (and Gossen did so with all the confidence and revolutionary ardor of Jevons and Walras). Nevertheless, the same argument that applied to Jevons, Menger, and Walras applies now to Dupuit, Gossen, and

5. See T. W. Hutchison, ''The Marginal Revolution and the Decline and Fall of English Classical Political Economy,'' below in this volume.

Jennings: they struck on the law of diminishing marginal utility at about the same time, but in response to totally different intellectual pressures and without the benefit of an inherited corpus of similar economic ideas.

We have now collected three trios, nine names in all, of economists who between 1834 and 1874 seized on the idea of marginal utility, four of whom saw it indeed as the stock from which a new economics could be evolved. If we deny that this constitutes a Mertonian "multiple," are we not splitting hairs?

It is clear how we might escape the dilemma. Recall Merton's own words: "Even discoveries far removed from one another in calendrical time may be instructively construed as 'simultaneous' . . . depending upon the accumulated state of knowledge in the several cultures and the structures of the several societies in which they appear." Thus, from the fact that marginal utility was independently discovered over and over again in different countries between 1834 and 1874, we might argue that there must have been a core of economic ideas which was held in common by economists all over the world, whose inner logic would eventually dictate the exploration of consumer's demand with the tools of utility theory. In other words, we can infer the state of the science from the existence of a multiple, instead of the other way around. But that is to deprive the theory of multiples of its most attractive feature, namely, the idea that the development of a science is to some extent predictable. So long as we take Merton's argument seriously as providing something more than an inductive generalization with many exceptions, we must deny that even nine names necessarily make a "multiple." The point is very simple: if communication between scientists were perfect, all multiples would be forestalled and we would only observe singletons; at the other end of the spectrum, if there were no communication whatsoever between scientists, multiples would have no more significance than the fact that lightning does occasionally strike twice in the same place; thus, multiples are only interesting phenomena if there is a high but nevertheless imperfect degree of communication between the practitioners of a discipline.

It is true that classical economics had no theory of demand and that its theory of price determination would sooner or later strike

someone as peculiarly asymmetrical. But as the example of Cournot shows, it would have been perfectly possible to repair this deficiency without introducing utility considerations. It is also true that marginal utility was "in the air" throughout the nineteenth century and kept turning up afresh every ten years or so: Lloyd and Longfield, 1834; Dupuit, 1844; Gossen, 1854; Jennings, 1855; Jevons, 1862 (the date at which he first publicly proclaimed his theory); Menger, 1871; and Walras, 1874. But that is a far cry from saying that marginal utility economics was, in some sense, inevitable. We might as well say that the emergence of macroeconomics in the 1930's was inevitable because certain Swedish economists were thinking along the same lines in the 1920's as Robertson and Keynes. *Post hoc ergo propter hoc* is a perennial temptation in intellectual history.

III

Howey's *Rise of the Marginal Utility School, 1870–1889* has taught us that the "marginal revolution," like the Industrial Revolution, went unrecognized by those who lived through it. The now standard version, which dates the revolution near 1871 and links together the names of Jevons, Menger, and Walras as having written essentially about the same thing, was first announced in the late 1880's and (despite Marshall's endorsement in 1890) did not become a regular feature of histories of economic thought until well past the turn of the century.[6] The long-delayed acceptance of the marginal utility theory of value, which went hand in hand with the delayed acceptance of a rational account of its history, is perhaps the best indication we can have that it was indeed an anomaly which did not emanate logically from classical economics. This suggests, in other words, that the last quarter of the nineteenth century was one of those revolutionary phases in the history of economics when, in the language of Thomas Kuhn, economists adopted a new "paradigm" to guide their work.

Unfortunately, there appears to be no agreement as to just what the new paradigm was that Jevons, Menger, and Walras put forward. Was it a new emphasis on demand rather than supply, on consumer utility rather than on production costs?[7] Was it something as am-

6. Howey, chaps. 26 and 27.

7. See A. W. Coats, "The Economic and Social Context of the Marginal Revolution of the 1870's," below in this volume.

bitious as a subjective theory of value, which was to supplant the objective labor-cost theories of the past?[8] Was it rather the extension of the principle of maximization from business firms to households, making the consumer and not the entrepreneur the epitome of rational action?[9] Was it perhaps the equimarginal principle, enshrined in the proportionality of marginal utilities to prices as the condition of consumer equilibrium?[10] Was it instead, as Schumpeter liked to say, the explicit or implicit discovery of general equilibrium analysis?[11] Or lastly, was it simply the first conscious recognition of constrained maximization as the archetype of all economic reasoning? Whichever version we adopt, it is difficult to sustain the thesis that Jevons, Menger, and Walras were really preoccupied with the same paradigm.

Menger is in any case the odd man out: he was not self-consciously aware, as Jevons and Walras were, of being a revolutionary; he eschewed mathematical formulations and hence the pure logic of extremum problems; he only formulated "Gossen's third law" in words and certainly did not emphasize it; he certainly rejected cost theories of value, but on the other hand he was deeply suspicious of all determinate theory of pricing and he underlined discontinuities, uncertainties, and bargaining around the market price.[12] In other words, there is a great deal more to be said for coupling Jevons and Walras with Gossen than with Menger, and the only reason for the standard version is that Menger's name was continually invoked by his disciples Wieser and Böhm-Bawerk, both of whom were determined to persuade the profession that Austrian economics was a differentiated product. Similarly, it takes hindsight to see much in common between Jevons—a precisely formulated theory of barter exchange, an explicit mathematical statement of "Gossen's third law," a theory of the short-run supply schedule of labor, and some grandiose but unfulfilled promises of a new kind of utility economics —and Walras, who really did derive demand curves from utility

8. See R. L. Meek, "Marginalism and Marxism," below in this volume.

9. Ibid. It is worth noting that Adam Smith's theory of occupational choice certainly treats individual workers as maximizers. There was nothing new in the idea of extending the sphere of rational action to households, but the notion of extending it to consumer behavior was new.

10. Blaug, *Economic Theory in Retrospect*, pp. 301–2.

11. A. Schumpeter, *History of Economic Analysis* (New York, 1954), p. 918.

12. For a somewhat extreme statement of this argument, see E. Streissler, "To What Extent Was the Austrian School Marginalist?" below in this volume.

schedules, struggled likewise to derive supply curves from marginal productivity considerations, worked out a theory of market pricing, and wove all the elements together within a general equilibrium framework.

The whole question is made more difficult by the ironic fate which history visited on the founders. In the end, what proved important about marginal utility was "the adjective rather than the noun."[13] Utility theory was gradually deprived of all its bite, to end up as merely "revealed preferences"; cost theories of value were shown not to be wrong, but only valid as a special case; and general equilibrium virtually disappeared, only to be revived in the 1930's by Hicks and Samuelson as "everybody's economics." Could anyone have foreseen in 1871 the tortuous path by which marginal utility economics led via Paretian welfare economics to cost-benefit analysis and dynamic programing? Not for nothing do we speak of a "marginal revolution" and not a "marginal utility revolution," but marginalism as a paradigm of economic reasoning is a twentieth-century invention; there is as much marginalism in Ricardo as in Jevons or Walras, but it is applied to different things.

IV

The term "paradigm" as a self-authenticated viewpoint no doubt raises as many questions as it answers,[14] but if we equate it loosely with Schumpeter's Vision—"a preanalytic cognitive act that supplies the raw material for the analytic effort"—we may describe the last quarter of the nineteenth century as a period when economists did

13. Hutchison, *Review of Economic Doctrines*, p. 16.

14. Masterman has counted twenty-one different definitions of "paradigm" in Kuhn's *Structure of Scientific Revolutions*, ranging from "a universally recognized scientific achievement" to a "general metaphysical viewpoint." M. Masterman, "The Nature of a Paradigm," in *Criticism and the Growth of Knowledge*, ed. I. Lakatos and A. Musgrave (London, 1970), pp. 61–65. Without precisely defining his sense of the term, Coats has argued that economics "has been dominated throughout its history by a single paradigm—the theory of economic equilibrium via the market mechanism." A. W. Coats, "Is There a 'Structure of Scientific Revolutions in Economics'?" *Kyklos* 22 (1969): 292. Similarly, Bronfenbrenner first defines a paradigm as a "mode or framework of thought and language" and then gives instances of paradigms, such as the demand-and-supply cross, the equation of exchange, and Hicksian IS-LM curves, which are much more specific than a mode or a framework. M. Bronfenbrenner, "The 'Structure of Revolutions' in Economic Thought," *History of Political Economy* 3 (Spring 1971): 150.

develop a new view of their research agenda. A brief way of describing this new Vision is to say that pricing and resource allocation with fixed supplies of the factors of production became *the* economic problem, setting aside all questions about changes in the quantity and quality of productive resources through time. Whether we want to describe this shift as a "revolutionary phase," given the fact that it took at least twenty to thirty years and in some sense is still going on, is a matter of words. Jevons, Menger, and Walras are not the founders of this new way of looking at economic problems, but they are important landmarks in the early stages of the shift of emphasis. That they published nearly simultaneously is a pure coincidence, because their reflections on the problem are actually separated by more than a decade. Only biographical data can tell us why Jevons and Walras (and Gossen) each insisted on the novelty of his ideas, whereas Menger (and Lloyd and Longfield and Jenkin) did not.[15] Therefore, to try to explain the origin of the marginal utility revolution in the 1870's is doomed to failure: it was not a marginal *utility* revolution; it was not an abrupt change, but only a gradual transformation in which the old ideas were never definitively rejected; and it did not happen in the 1870's.

V

The fact that Jevons, Menger, and Walras all published their works within the span of three years, while a coincidence, was not an insignificant coincidence; it encouraged the acceptance of marginal utility economics, or at any rate greatly increased the probability of its early acceptance. Nevertheless, the new economics still failed to make much headway for at least a generation, despite the fact that all three founders were academic economists with established reputations, who argued their case persuasively and subsequently spared no efforts to push their ideas. The historical problem, therefore, is to explain, not the point in time at which the marginal concept

15. For some convincing biographical evidence, see R. D. Collison Black, "W. S. Jevons and the Foundation of Modern Economics," and W. Jaffé, "Léon Walras's Role in the 'Marginal Revolution' of the 1870's," below in this volume. As N. B. de Marchi argues, Mill and Cairnes were actually in possession of all the pieces required to make the breakthrough to marginal utility economics, but could not do so because of their Ricardian blinkers. "Mill and Cairnes and the Emergence of Marginalism in England," below in this volume.

was applied to utility, but rather the delayed victory of marginal utility economics.

This is not a difficult problem provided we do not insist that historians "retrodict" in essentially the same way that scientists predict; in other words, that historical explanations can be regarded as valid only if they take the form of counterfactual hypotheses based on some general "covering law." What historians do is to make past events intelligible—they illuminate rather than explain—and in the nature of the case, therefore, there can be no hard and fast rules on whether A caused B or was merely associated with B.[16] It is, therefore, fruitless to argue whether the diffusion of marginal utility economics, as distinct from its genesis, was largely the result of endogenous or of exogenous influences. It is precisely in this period that economics began to emerge as a professional discipline with its own network of associations and journals, the dilettante amateur of the past giving way for the first time to the specialist earning his livelihood under the title of "economist." A professionalized science necessarily develops its own momentum, the impact of external events being confined to the shell and not reaching the core of the subject.[17] But in 1870, or 1880, or even 1890, core and shell were still deeply intertwined. To argue, therefore, as Stigler does, that the retarded adoption of utility theory in economics can *only* be explained by "the rise of new values as the discipline became increasingly academic"[18] is merely to throw back the problem one stage further: Why did economics become professionalized in the last quarter of the nineteenth century and why should a professionalized science of economics find the truth of utility theory so self-evident that resistance to it becomes impossible?

It seems clear that no monocausal explanation can do justice to the long uphill struggle of the marginal revolution. One is struck in reading the treatises of the 1870's and 1880's by the bewildering

16. I am making tacit reference to a great debate that was started in the 1940's with an article by C. G. Hempel. See P. Gardiner, ed., *Theories of History* (Glencoe, Ill., 1959); and S. Hook, ed., *Philosophy and History: A Symposium* (New York, 1963).

17. For this useful distinction, see J. J. Spengler, "Exogenous and Endogenous Influences in the Formation of Post-1870 Economic Thought: A Sociology of Knowledge Approach," in *Events, Ideology, and Economic Theory*, ed. R. V. Eagly (Detroit, 1968).

18. G. J. Stigler, "The Adoption of the Marginal Utility Theory," below in this volume.

variety of attitudes adopted towards the principal tenets of classical economics, such as the labor theory of value, the quantity theory of money, the Ricardian theory of differential rent, et cetera. Jevons, Menger, and Walras each in his own way emphasized the methodological advantages of abstracting from historical and institutional considerations in the interest of obtaining perfectly general results from the minimum number of assumptions. But such considerations had little appeal to most contemporary economists, who still cared more about relevance than about rigor. As far as applied problems were concerned, marginal utility was, as we have said above, largely irrelevant, and the methodological problem that troubled most economists in the critical decade of the 1880's was the issue of induction versus deduction, the conflict between fact gathering and model building. Wherever there was a historicist bias—a pervasive bias in Germany and a widespread one in England—marginal utility economics was dismissed, together with English classical political economy, as excessively abstract and permeated with implausible assumptions about human behavior. The fact that Jevons and Walras chose to express themselves in mathematical terms was undoubtedly responsible for further resistance to their ideas; the notion of reducing social phenomena to mathematical equations was still new and profoundly disturbing to nineteenth-century readers. It was the rise of Marxism and Fabianism in the 1880's and 1890's that finally made subjective value theory socially and politically relevant; as the new economics began to furnish effective intellectual ammunition against Marx and Henry George, the view that value theory really did not matter became more difficult to sustain. Furthermore, the addition of marginal productivity to marginal utility in the 1890's related the new economics to the problem of distribution, making it virtually impossible to deny a logical conflict between the ideas of Jevons, Menger, and Walras and those of Smith, Ricardo, and Mill. In 1891 Marshall provided a reconciliation between marginal utility economics and classical economics which made the new ideas palatable by showing that they could be fitted together into a wider context. But even at this late stage, the Marshallian integration was not immediately accepted on the Continent, and the three interlocking "revolutions" that had characterized the last two decades of the nineteenth century

—the marginal utility revolution in England and America, the subjectivist revolution in Austria, and the general equilibrium revolution in Switzerland and Italy—continued well into the twentieth century.

VI

It may be convenient to seize on 1871 as marking the date from which all this started. But that date has no more special claim on our attention as historians of economic thought than any other. Classical political economy did not begin in 1776, and the birth of marginal utility economics—marginalism, modern economics, by whatever name we choose to characterize it—similarly, cannot be pinned down to any particular date. To sum up: (i) the "marginal revolution" was a process, not an event; (ii) there was no "multiple" discovery of marginal utility, but only the temporal coincidence of three or more singletons; and (iii) the success of the marginal revolution is intimately associated with the professionalization of economics in the last quarter of the nineteenth century, and it is this which constitutes the problem that must be, and to some extent has been, explained by historians of economic thought.