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WHAT MARSHALL *DIDN'T* KNOW: ON THE TWENTIETH CENTURY'S CONTRIBUTIONS TO ECONOMICS*

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Some of this century's many valuable contributions to economics, like macroeconomics, econometrics, and game theory, are widely recognized. However, arguably equally important is the enhanced role of empirical study permitted by more abundant data and improved methods. Also insufficiently recognized are the increased rigor and use of applied economics in public finance, regulation, corporation finance, etc., employing abstract theory and sophisticated data analysis. The striking contrast with earlier intuitively based applied economics and empirical study is illustrated. Comparison with Marshall's *Principles* also indicates that, except for macroeconomics, remarkably little space in today's texts deals with some of the rich contributions of this century.

What has twentieth century economics accomplished? A great deal, as will be shown here. But the discussion needs an illuminating starting point. In 1946, after arriving at the London School of Economics as an entering graduate student,[1] I soon found that in

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1. When I came in 1946, Keynes had recently died, and Marshall was, of course, long gone. But I did hear Pigou lecture and describe himself as an ancient squid who, purely by force of habit, still continued to eject squirts of ink. Dennis Robertson repeatedly told me how on passing Pigou's lair, the great man would regularly emerge, demanding "Robertson—tell me, what is the Pigou effect?" At the London School of Economics Lionel Robbins, Arthur Lewis, Friedrich Hayek, Nicholas Kaldor, and James Meade were on the faculty, and Frank Hahn and Ralph Turvey were fellow students. But enough of nostalgia. I promise to impose no more of it on this article. I must admit, however, with respect to the "It's all in Marshall" assertion, that I am considerably more sympathetic to the remark of logician-astronomer C. S. Peirce in an 1871 letter to astronomer-economist Simon

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the United Kingdom new ideas were frequently met with the Cambridge response: "But it's all in Marshall." Alfred Marshall's *Principles* [1890] was, at the inception of the twentieth century, already in the fourth [1898] of its eight [1920] editions, having first appeared ten years earlier. All of this patently invites use of the book, supplemented by his other writings, as the initial point—the standard, against which twentieth century contributions to our discipline can be measured. It is, indeed, the criterion proposed by the editors of this *Journal* when they graciously invited me to produce this piece. For all these reasons, in the pages that follow I will accept Marshall as the zero point of my measuring rod. But I will not do so without pointing out the shortcomings of that choice. For even in 1900 it was not all in his writings, and we can hardly credit to our century matters that, although apparently unrecognized by the professor at Cambridge, were known to others.

It should also be remarked that many of the twentieth century contributions that will be emphasized here were stimulated by historical developments that Marshall could hardly have foreseen. These include the Great Depression and its stimulus of macroeconomics, and the great outburst of innovation after the Second World War that no doubt played a substantial role in the return of economists' interest to growth analysis.

The scope of my assignment here is enormous, and much will have to be left out. Accordingly, although I will discuss them, only limited effort will be devoted to roundup of the usual suspects. Partly, this is because review of the obvious contributions will offer little of which readers are not fully aware, but also because I hold somewhat heterodox views on the century's most fruitful contributions. Much sophisticated theoretical analysis, sometimes using powerful mathematical tools, was already available, a good deal of it still being used in the literature, albeit in modified and (usually) improved variants. Rather, the major upheaval occurred in three arenas. The first is, of course, the formalization of macroeconomics. The second is the construction of powerful new tools of empirical study and their use to provide important insights on the workings of economic reality, as well as to investigate and add substance to the theory. The third, and least

Newcomb on profit maximization under perfect competition: "P. S. This is all in Cournot." This is, incidentally, particularly remarkable, as in 1871 Cournot had supposedly not yet been rediscovered (by Jevons) for the English and American adherents to our discipline.

widely recognized, is the widespread employment of theory and econometric analysis in *application*—the formulation of macro policy, the design of taxes, the analysis of portfolios of financial instruments, and the resolution of litigation on antitrust and other economic subjects before courts and regulatory agencies. Before the twentieth century there was nothing remotely similar to the frequent inquiry by learned judges into apparently esoteric mathematical theorems from the microeconomic literature, which they accept as a legitimate and important part of the basis for their decisions. The contention that the major departures of twentieth century economics are to be found in these three areas is the central conclusion of this paper. I will also suggest that a field about ready to burst forth is the *microeconomics* of innovation and entrepreneurship as a key to analysis of capitalistic growth, deriving from the Schumpeterian legacy. A substantial flow of rigorous and substantive contributions is already emerging, promising that the theory is about to take its appropriate place as a central element of microeconomic analysis, rather than a peripheral adjunct to the literature.

Throughout, I will resist the considerable temptation to refer to any of my own work, hoping it will manage to fend for itself.[2]

I. What the Century Contributed: The Textbook Test

Before embarking on the central part of my quest, I will undertake a preliminary inquiry that may, at first, be considered facetious. I will consider what portion of the materials in today's standard economics textbooks provided to beginning students in colleges and universities was unavailable before the arrival of the new (twentieth) century. Such a comparison is suggested by Marshall's *Principles* itself, which, after all, was intended ". . . as a general introduction to the study of economic science" [Preface to the eighth edition, p. xii]. But there is a reason that I find more persuasive. Today's textbooks, after all, are designed to be read preponderantly by students who will *not* become specialists in the field. The material selected for such a book can therefore be expected to focus on subjects deemed to shed light on the workings

2. Aside from sources of bibliography, there is only one insignificant exception that will go unidentified, and that is introduced only because it serves as a convenient illustration of the point being made. I may note that to save them embarrassment I have also avoided reference to any of the very valuable contributions of the editors of this *Journal*.

of the economy and the design of policy. They are intended to sum up the contributions of economics that really matter to others, and not just to those who labor at the frontiers of our discipline, sometimes perhaps, as Marshall put it, largely ". . . for the purpose of mathematical diversion" (see below for more of the quotation). It follows that the textbook criterion can indicate what economists believe others should glean from the work of our profession during the course of our century, that is, what *useful* pieces of analysis are known today that were not recognized one hundred years ago.

I believe that the results of the textbook test are mixed—in some fields there is a world of difference between the materials available at the century's end and those at its beginning. In other areas, however, the differences are disturbingly small.

The really big change in the contents of textbook volumes is, of course, to be found in the field of macroeconomics, a subject virtually excluded from or at least never given what may now be deemed a serious treatment in Marshall's book. In contrast, much of the textbook discussion of microeconomic subjects has shown much less change, other than in methods of exposition. This is true, for example, of the theory of the firm and industry under perfect competition, of the firm under pure monopoly, of market behavior under these regimes, and of the theory of income distribution. This is not meant to deny the very substantial and profuse new material in the specialist literature, but rather to claim that a relatively small part of these important contributions found their way into the elementary texts in the form of extended and integrated exposition.

So far as macroeconomics is concerned, it can reasonably be claimed that serious treatment of the subject had just had its inception with the completion of Wicksell's revolutionary work in 1898 (see the discussion in Blaug [1968, Chapters 14 and 15]). Of course, there had previously been a profusion of rather rudimentary models of the business cycle, the quantity theory, and other monetary issues. But it can surely not be claimed that any of these provided a systematic structure susceptible to extended analysis. Marshall himself constitutes a good example. Although he lived through several significant recessions, there is not in the *Principles* or in *Money, Credit and Commerce* [1923] or in the *Official Papers* [1926] anything that can lay claim to being a systematic discussion of unemployment. The following quotation is representative, and although it is just an excerpt, it is taken from the only two pages in the *Principles* in which the subject seems to be mentioned: "The chief cause of the evil [of unemployment] is a want of confidence. The greater part of it could be removed almost in an instant if confidence could return, touch all industries with her magic wand, and make them continue their production and their demand for the wares of others" [*Principles*, p. 711].

This is not meant to suggest that Marshall's views of the subject were uninformed or unhelpful. The point, rather, is that such a brief and intuitive discussion hardly provides the framework for a systematic and extensive analysis such as followed the work of Keynes.

Later, I will say more about the twentieth century contribution to, or perhaps more accurately, its creation of macroeconomic analysis. For now it need merely be emphasized that the fact that the field is not only included in today's standard text, but that it routinely takes up roughly half of its pages, constitutes a marked departure from the contents of Marshall's book—the quintessential textbook of 1900. Marshall did, of course, include some subjects that today fall in the macro sections of our texts—money, business cycles, and productivity growth. But these constituted no coherent and extensive part of the volume. There is no chapter on money in the *Principles*, and I have been able to find only three pages [709–11] on "trade fluctuations" (which are not even listed in the index of the eighth edition).

As already noted, matters are very different when it comes to today's core textbook chapters in microtheory. The demand chapters are almost entirely Marshallian, with their focus on elasticities (apparently Marshall's term but not his invention), and the grounding of the demand curves on utility, cardinal or ordinal. Even where ordinal utility is the focus, that need not be interpreted as a significant departure, since by the beginning of the twentieth century Edgeworth [1881] had introduced indifference maps in a book that was well-known to Marshall, and the discussion is cited in the *Principles*.

The textbook versions of the theory of the firm and the industry under perfect competition, it can be argued, are also unchanged in any essential, despite the major additions to the theory that pervade the literature. The exposition has, indeed, been modified and perhaps simplified by the introduction of the marginal cost and marginal revenue[3] curves and the $MC = MR$ requirement for profit maximization. But Marshall certainly used the concept of marginal cost, and his profit maximization requirement (that marginal profit be equal to zero) is surely a thinly reformulated variant of the necessary condition the textbooks use today since, obviously, marginal profit is identical with $MR - MC$.

It is also difficult to find a major change in formal theoretical structure in the chapters on income distribution and their reliance on marginal productivity theory, a theory well explored by 1900 by Walras, Wicksteed, J. B. Clark, and others. Even Euler's Theorem had been given its place by then [Flux 1894]. Marshall was thoroughly conversant with the theory, and even the institutional flavor of the discussion, with its use of what today would be called "casual empiricism," is paralleled in today's texts. He also took a general equilibrium view of the relation between the price of an input and its marginal productivity, e.g., "Marginal uses do not govern value, but are governed together with value by the general relations of demand and supply" [*Principles*, p. 521]. In sum, exposition aside, on the *formal theory* of distribution it can be argued that the textbooks of the end of the twentieth century have added little to the prime textbook of the century's inception. Descriptions of institutions and their implications have unavoidably changed. There have been relatively recent contributions such as human capital theory and the analysis of dual labor markets that modern textbooks do include. But the main formal theory, from the Ricardian rent model to marginal productivity theory, continues to be at the heart of the exposition.

No doubt, various bits and pieces have been added to other portions of the microeconomics sections of today's textbooks. But the startling fact is that there really seem to be only two sets of substantial changes in the microtheory that are found in all standard texts. The first relates to the role of externalities and public goods in the theory of welfare economics. The second is concentrated in what is often a single chapter in current texts: the chapter on oligopoly and monopolistic competition. And, even here, the novelties are not as novel as they may seem.

The absence from Marshall's *Principles* of the concept of externalities as a prime source of market failure entails a double

3. Indeed, it was apparently Marshall who contributed the term "marginal" to the literature of marginal analysis: "I got 'marginal' from von Thünen's *Grenze*" (letter by Marshall to J. B. Clark, 2 July 1900, cited by Guillebaud [1961, p. 8]).

irony. First, it was surely Marshall who invented the distinction between external and internal economies of scale and, apparently, introduced it in the *Principles,* no later than the second edition. But even in the final edition of the book the concept is given little space. Its main role is to point out that a small firm can sometimes reduce its costs not only through its own expansion, but also through benefits derived from "an increase in the aggregate volume of a national or a local industry" (see Guillebaud's notes in Volume 2 of the ninth edition [p. 347n]). The second irony is that it was left for Marshall's student and successor in the Chair, A. C. Pigou, to work out almost fully the theory of externalities in modern welfare theory (the term "welfare" also apparently introduced by Pigou in this connection—but see the note on Hadley, below). This new analysis appeared in 1912 (and more fully in 1920), nearly a decade before publication of the final edition of the *Principles.*

Yet, Marshall did contribute substantially to welfare theory. He had taken over the concept of consumers' surplus from Dupuit, and had shown its relation to producers' surplus, as well as the relation of the latter to rent, quasi rent, and profit. He also showed that the sum of the consumers' and producers' surplus is the proper measure of the contribution of an industry's output to the general well-being. The theory of quasi rents, to which Marshall devoted so much space, can itself be considered a major addition to the theory of welfare, as well as to the theory of distribution. The consequences of monopoly and perfect competition for welfare were also explored. Still, Marshall's discussion leaves an enormous gap, without the current understanding of the role of externalities, that now plays so critical a role in areas as diverse as environmental economics and the theory of innovation.

Clearly, in Marshall's day, game theory had not yet been invented, and Joan Robinson and E. H. Chamberlain had not yet written. But evidently there already was a good deal of material in the general area. The Cournot duopoly analysis remains a model of sophisticated reasoning, and its relation to game-theoretic concepts such as the Nash equilibrium as well as the frequent allusions to Cournot's work in the literature of game theory confirm this. Marshall knew Cournot's work well and admired it. He also was well acquainted with Edgeworth's work on duopoly [1897]. Moreover, Marshall himself recognized that pure monopoly was rare and that the more interesting case was something less extreme. Yet, it is clear that the modern textbook's discussion of oligopoly and monopolistic competition goes well beyond what Marshall said and, very probably, beyond what he knew. Certainly, one would not send a student to his book for a good grounding in either of these subjects.

Yet, it is easy to exaggerate the point that there is surprisingly little beyond Marshall in the micro sections of our textbooks apart from the theory of oligopoly and monopolistic competition. A number of texts do offer more new microeconomic material than this. Small amounts of human capital theory, the economics of discrimination, moral hazard, principal-agent problems, contract theory, and the Coase theorem are found in many principles texts. But these subjects do not normally constitute separate chapters, they are not usually discussed extensively, and are hardly near the heart of the microeconomic portions of modern texts.

The conclusion from all this must be that, macroeconomics apart, what the elementary textbook authors believe it is important for (nonspecializing) students in economics to learn differs surprisingly little from what Marshall already offered readers a century ago. One is tempted to argue from this that our century's microeconomics has contributed very little insight of importance for practice and application. But I will presently argue the contrary—that economics (both micro and macro) has progressed remarkably in what it offers for practice. Rather, there are at least two reasons other than paucity of progress in our discipline why much of this advance is not *substantially* reflected in our basic texts (although, as noted, items such as moral hazard, principal-agent theory, and Ramsey [1927] analysis are mentioned, often quite briefly). The first seems to me a legitimate reason for exclusion: the fact that these contributions are characteristically too technical and complicated to invite their teaching to beginning students. However, it is possible for much of this difficult material to be taught in elementary courses in a very simplified and intuitive way and, in fact, many instructors do so. The second, more questionable reason is the choice of subject matter by the authors of textbooks (including myself) that simply follows tradition in the teaching of these subjects and offers material calculated primarily to be attractive to the instructors. It may therefore not always focus upon the subjects that it would, arguably, be most useful for such students to learn.

II. What the Century Added: Roundup of the Usual Suspects

I have already mentioned a number of the areas of our discipline in which, most would surely agree, the past century brought substantial contributions to our discipline. The names of the major contributors are a good starting point, and they patently include (in more-or-less chronological order) Fisher [1892], Wicksell [1898, 1901], Veblen [1898, 1899], Pigou [1912], Keynes [1936], Hicks [1941, 1946], Samuelson [1947], Koopmans, Friedman, Neumann, and Morgenstern [1947], Tobin and Arrow [1951], and Solow [1956], among others, and their names readily suggest some of the fields in which significant breakthroughs have occurred. These obviously must include macroeconomics, encompassing recent growth analysis, the refounding of value theory, notably in the work of Samuelson and Hicks, game theory, general equilibrium, and its implications for trade theory and welfare economics. I will discuss some of these, but space limitations force me to omit a number of substantial contributions. Among others that come to mind I must mention, in no particular order, Arrow's analysis of social choice and his impossibility theorem, Tobin's contributions to monetary theory, Modigliani's work on corporate finance and his and Friedman's permanent income hypothesis, Lucas' [1987, 1988] creation of rational expectations theory, the work of Buchanan and Tullock [1962] on public choice and rent seeking, the advances in trade theory including the Heckscher-Ohlin model, the work of Stolper and Samuelson, of Dixit and Norman [1980], and of Rybezynski,[4] Patinkin's [1956] reformulation of the neoclassical monetary theory, the Anglo-Italian models, including work of Sraffa [1926], Robinson [1933], Kaldor and Passinetti, the theory of the second-best, with Viner's [1921, 1950] preliminary contribution and the analysis of Lancaster and Lipsey, work on moral hazard, principal-agent problems, and information costs, with contributors including Stiglitz and Akerlof, the work of the institutionalists, following Veblen's writings [1898, 1899]. This list omits major contributors such as Gary Becker, Oliver Williamson, Michael Spence, and William Vickrey, and new fields of study such as environmental economics (see Cropper and Oates [1992], behavioral economics (see Thaler

4. I also venture to predict that, although highly controversial, Gomory's recent work in international trade [1994] will eventually be recognized as a major contribution.

[1991], law and economics and cultural economics (see Towse [1997]—but one must stop somewhere).

I will conclude that these most obvious categories of research are not generally the ones that bring us most markedly beyond Marshall. Rather, my heterodox view is that advances in empirical work and application of theoretical concepts to concrete issues of reality are where one can find the most distinct advances beyond the state of knowledge at the beginning of our century.

Some Ruminations on Macroeconomics

I have already cited Marshall's views on unemployment as an indication of the rudimentary state of thought on macroeconomic issues in about 1900. And it will surely be admitted, even by those furthest removed from Keynes' positions, that his work, along with that of the Stockholm School, and that of several others, has injected a degree of depth and systematic thought into the field. Thus, the analysis of the monetarists, despite its conflict with that of the Keynesians, owes to the latter an enhancement of its structured investigations. It is arguable, for example, that without the macroeconomic literature that followed the *General Theory* the rational expectations analysis might never have been extensively explored. There is a clear link from Keynes to Friedman to Phelps and Lucas. Whatever the achievements of the Keynesian revolution may have been, it certainly succeeded in inaugurating a massive and extremely active field of specialization, as well as a more formal and more rigorous exploration of its relationships than we had ever possessed (on this, see Lucas and Sargent [1978], Lucas [1987], Blinder [1987], Gordon [1990], and "Symposium: Keynesian Economics Today" [1993]).

"Macroeconomics" can be taken to have two defining features. First, it deals with entire economies, rather than any of its constituent components. This, of course, is hardly new, as the title of Adam Smith's magnum opus reminds us. Second, the approach achieves analytic tractability through simplification by means of aggregation, discussing broad classes of agents as organic entities—consumers, investors, etc. But this, too, was previously done, as, for example, in the classical theory of distribution with its combining of all inputs into land, labor, and capital. It is the combination of these two attributes that makes the field different from others, and makes it, arguably, a creation of the twentieth century. For although early cycle theories dealt with related matters, it was not the underconsumption models alone that deserve to be called "naïve."

While I have argued that Wicksell [1898, 1901] was an exception, providing a sophisticated and coherent analysis of the process of inflation in a full-employment economy, one can conclude that his work, too, is essentially of the twentieth century. He did manage to get ahead of 1900 by a scant two years, but the revisions of this work and the subsequent publications by him on similar subjects are in the spirit of our own century, in which they in fact appeared. I conclude that the broad field of macroeconomics is in essence a twentieth century phenomenon, whose earlier predecessors are but feeble ancestors.

I end this discussion by disagreeing emphatically with the view, so often heard, that macroeconomics is in terrible trouble. I believe this opinion stems from a misunderstanding of what one can and cannot reasonably expect from it. The genius of macroeconomics consists of felicitous oversimplification, which is traded off for concrete conclusions that are much harder if not impossible to obtain from less simplified models. And macroeconomics has delivered on this promise, offering insight and understanding to economists and policy makers that were totally unavailable before. However, the very oversimplification that makes this possible means that the utmost caution is required in reliance upon and use of these conclusions. They must be labeled carefully to admonish the user to "handle with care" because, taken improperly, they can be dangerous to the economy's health. That is surely not a failure of macroeconomics, but one of its inherent features that was recognized from its beginnings. A second misunderstanding is the notion that it is desirable to impart great rigor to macroeconomic theory, perhaps even giving it strong microeconomic foundations. But such a move is likely to deprive the field of its very reason for being—the ease with which it can be used to derive concrete (if frequently controversial) conclusions such as results indicating public policies that promise to be useful in combating unemployment or inflation.

On Growth Analysis: Macro and Micro

In my view, it is a historical accident that, despite Schumpeter's [1912, 1942, 1954] emphasis on the role of the firm and the state of competition, growth theory does not reside primarily in the *microeconomics* literature, but instead became largely an offshoot of general macroeconomics in postwar contributions to the theory of growth. This is the literature flowing from the work of Solow and Lucas (for a survey of this subject, see, for example, "Symposium: New Growth Theory" [1994] and Jonathan Temple [1999]). This analysis provided enormous new opportunities for empirical investigation of the theoretical constructs. As will be described more fully later, the macroeconomic growth studies have, for example, yielded evidence on the magnitude of the contribution of innovation to growth and on the degree to which convergence among economies in productivity and GDP per capita (or its absence) has occurred. They have done more than that—for example, Romer and others have built on the convergence results to show the need for modification of the macroeconomic models, arguing that they relied too heavily on diminishing returns and failed to take sufficient account of the evidence that innovation is, at least partially, an endogenously determined activity. Thus, the macroanalysis of growth has very effectively brought together theory, empirical study, and it should be added, application to policy.

But looked at purely as theory, at least three observations can be offered about the novelty of the work and its explanatory power. First, it can easily be argued that today's growth models, *taken purely as theoretical constructs,* are not all that different from the classical model of Ricardo and his contemporaries. It is not difficult to translate the magnificent Ricardian growth model into mathematical terms. Indeed, one may well say of Ricardo what Edgeworth, perhaps with less justification, said of Marshall: that he bore ". . . under the garb of literature the armor of mathematics" (as cited by Guillebaud [*Principles,* ninth edition, Vol. II, p. 14]). In that well-known model, quantities of labor, capital, and land determine output, with diminishing returns to the first two inputs. The surplus of output over differential rent and the subsistence (but not actual) level of wages then determines both accumulation of capital and growth of population. This sequence easily translates itself into a formal dynamic model whose equilibrium point is the stationary state. And in the Ricardian discussion it is explicitly recognized that the production function can be shifted upward, thereby postponing the stationary state indefinitely.

An essential role in the mechanism is played by the production relationship, which one can write as $Y_t = A(t)f(L_t,C_t,R_t)$, with Y, L, C, and R, representing aggregate output, labor, capital, and employed land, respectively. $A(t)$ is productivity growth attributable to exogenous innovation. The point in all this is the close resemblance between this scenario and those offered in recent growth models. There are, of course, significant differences. For example, Ricardo did not use Cobb-Douglas functions, he did not deal with a separate $A_j(.)$ function for an individual industry, j, nor did he endogenize the innovation process by making A_j a function of total investment in human capital or total investment in innovation. But these are, arguably, modifications of a venerable construction and not theoretical breakthroughs of the twentieth century. The considerable achievement of that literature, in my view, lies elsewhere—particularly in its facilitation of empirical study, as will be argued presently.

Second, as is appropriate for a macroeconomic model, the new growth analysis is a deliberate and marked simplification of the pertinent relationships. For example, endogenization of the innovation process is represented by means such as the premise that innovation in a particular field depends on the size of the set of innovations in the entire economy (thus taking account of the spillovers created by the process—see, e.g., Arrow and Romer) or, alternatively, that it depends on the economy's investment in human capital. (For references, see "Symposium: New Growth Theory" [1994].) Now such assumptions are surely valid, but it is equally clear that they leave out much of what is entailed. It can even be suggested, with Schumpeter, that a theory that confines its description of the innovation process to these two phenomena and does not attempt to deal with the extraordinary growth record of capitalist economies amounts to a performance of *Hamlet* from which the Prince of Denmark is absent. Because the analysis is macroeconomic, it cannot easily take account of the market forces and fierce competition among firms for priority in new products and processes. Yet these, arguably, are among the key determinants of the magnitude of the resources the economy devotes to innovation and are at the heart of the explanation of the historically unmatched production and growth performance of free-enterprise economies.[5]

5. This is not meant to overlook or to denigrate the mass of valuable papers on the microeconomics of innovation that have appeared in recent years. And writers such as Grossman and Helpman [1995] and Aghion and Howitt [1998] have made major contributions to particular issues related to innovation and growth. Thus, the former have provided a profound analysis of the influence of international trade on these matters, while the latter have explored the detrimental externalities caused by the introduction of new products via obsolescence of older products. Thus, they have shown how spillovers can conceivably lead to socially excessive expenditure on R&D. But I have been unable to find any recent theoretical

It is, rather, to Schumpeter [1911, 1942] that we must look for insights closer to the core of the issue. It was he who described a competitive mechanism that spurs innovation and in which innovation is the critical source of profits that exceed the normal level. He also described how competitors' imitation erodes those profits and forces the profit-maximizing firm to leap once more unto the breach—to innovate further if the source of economic profits is not to dry up. In my view, as in that of the later Schumpeter, that is no longer the predominant scenario, since relative freedom of entry into the innovation process drives *expected* profits for the innovative industry toward zero. Nevertheless, it seems clear that only an explicit micro analysis of the process of competition that uses innovation as its most potent weapon, and in which firms are determined to be second to none, will bring the Prince of Denmark back to center stage. Thus, while, as I will argue, the macroeconomic investigation of growth constitutes a major twentieth-century contribution, its achievement is *not* primarily *as theory*.

Third, simplification, with its great payoff, forces these models to be ahistorical. They contain nothing that distinguishes *market* economies from Soviet or Roman or medieval Chinese economies. Thus, designed as they are to deal with other matters, these models are incapable of shedding any light on one of the most critical issues for growth analysis: the capitalist economy and its special accomplishment—its unprecedented growth performance.

I may add that in my view things are about to change in the theory of growth and innovation. Valuable theoretical work on this topic and on the related subject of entrepreneurship is pouring forth from an impressive multitude of sources. The contributions are many and extremely varied, including the invaluable earlier contributions of Griliches, Jorgenson, Mansfield, Nordhaus, Scherer and Shell [1973], and joined more recently by Nadiri, Richard Nelson, Kirzner [1973], Paul Romer [1986], and Bronwyn Hall [1993].[6] The theory is beginning to recognize that the amazing growth performance of the capitalist economies must be explained by the behavior of firms and such attributes of the competitive process as the use of R&D rather

exploration of what I believe to be the great gap in growth theory—explanation of the growth explosion in free market economies.

6. For references to all except those whose names are followed by a year, see the bibliography in Nelson [1996].

than price as a competitive weapon of choice. The result in at least some industries is an innovation arms race. In the process, innovation is co-opted into the set of routine business decisions, making it far easier to incorporate the analysis into the standard models of the theory of the firm and industry. This will, I believe, make it possible to bring innovation closer to the core of microtheory, rather than keeping it on the outer fringes of the analysis. This development will, in my view, also enable us to deal with the anomalous conclusion of welfare theory that the market economy has a propensity to approximate *static* efficiency (and actually to achieve it in a perfectly competitive market without externalities), while market failure from sources such as spillovers seriously damage its *growth* performance, cutting its investment in R&D and other innovative activities well below their optima. This conclusion is an anomaly because it flies in the face of the casual observation that the economy's static performance, beset by market power, government intervention, and externalities in many ways falls far short of optimality, and the well-documented fact that its growth performance has been spectacular and totally unprecedented in recorded history.

Value and Welfare Theory: Buttressing the Foundations

Along with some sophisticated nineteenth century work on the theory of utility, new tools were introduced, including more powerful instruments for maximization and minimization, and new methods of comparative statics.[7] It soon became clear that these approaches to the study of the consumer could also be applied to the activities of the firm and other entities. This led directly to the momentous contributions of Samuelson [1947] and Hicks [1939] who laid out the entire structure of value theory once more, but at a level of sophistication and analytic power that had never before been achieved. This material is sufficiently well-known to require no recapitulation and little comment here. It would, of course, be foolish to surmise that the task was completed in these two works. No doubt a future generation will find much more that is yet to be said and along lines that we cannot now hope to predict. Yet the magnitude of the accomplishment is attested to by the absence of significant attempts to replace or improve upon the materials as a body in the half-century after their appearance.

7. The Appendix offers some descriptive materials on nineteenth century contributions to modern theory of utility and demand, leading up to the discovery of the Slutsky relationship and further pertinent insights.

As an entity they stand unchallenged as both a summing-up and a vast step forward. It is ironic that Hicks always considered his part in this work as among his minor contributions.

Along with value theory, the century inaugurated a systematic analysis of the implications of the theory for economic well-being. The founder of welfare economics was, surely, A. C. Pigou, [1912]. It is noteworthy that perhaps the primary insight to emerge from his book was not an investigation of the benign side of the market mechanism—its vaunted static efficiencies. Rather, Pigou offered us a crucial element for study of market *failure,* taking the concept of externalities from Marshall and expanding it into one of the most powerful concepts of welfare analysis. Monopoly and oligopoly, too, have long been recognized as sources of imperfect performance by the market. More recently other sources of market failure have been identified, among them imperfect and costly information, moral hazard, and principal-agent problems in the work of economists such as Akerlof, Spence, and Stiglitz.

The very legitimate analysis of market failure became a key theme of twentieth century economics. It led to calls for government intervention which, in retrospect, seem in many cases, but hardly always, to have been justified. But it overlooked government failure—the fact that the imperfections of governmental decisions are probably at least as serious as those of the market mechanism. The work of Buchanan and Tullock [1962] on rent-seeking, and the structure of political activity more generally, provided some balance to this line of discussion.

It was left for Arrow [1951] and Debreu [1958] to carry out the other side of the task—rigorous investigation of the venerable insight that perfect competition can, under appropriate circumstances, yield maximum static efficiency. In a sense, this work of those two authors *can* legitimately be deemed to be the end of the line. There seems to be little if anything more that needs to be said about the topic. This is so, in my view, because the subject is really of secondary importance for the real economic welfare of society. As already suggested, at least in the long run, the state of welfare depends on growth and productivity far more than on static efficiency, and this is the really crucial issue that I believe welfare analysis must face.[8]

8. Here it may well be tempting to ask whether it isn't "all in Adam Smith," who surely wrote about both static efficiency and growth. There is some truth in the observation, but not as much as seems widely to be supposed. Despite diligent

On General Equilibrium Theory in the Twentieth Century

The twentieth century addition to general equilibrium theory is, perhaps, clearer than that in any other area. This is because there is a sharp and protracted break between what had been achieved before 1900 and what was added after that. Despite a few primitive predecessors, it was clearly left to Walras [1874] to lay out the formal relationships that make up a full model of general equilibrium. Jaffé tells us, ". . . it was this book that directly inspired Vilfredo Pareto, Enrico Barone, Knut Wicksell, Irving Fisher, Henry Ludwell Moore and Joseph Schumpeter during Walras's own lifetime" [Walras 1874, 1954, p. 5] (Walras died in 1910). Thus, the basic general equilibrium model had been laid out and was widely recognized by the beginning of the new century. Walras' mathematical knowledge, however, was relatively elementary. As we know, he really never was able to deal with three critical issues: whether the solution to his system would always yield nonnegative prices and outputs, whether the solution existed, and whether it was unique. Walras struggled with these issues simply by counting equations and unknowns, and devised a way to show that their number was equal.

It was left to F. Zeuthen in Denmark and Karl Schlesinger in Vienna to show that it could not be legitimately assumed that the identity of the resources that would be used to capacity was known ex ante, so that in a fully legitimate general equilibrium model many of Walras' equations must be replaced by inequalities. They also provided what were probably the earliest expressions of some of the Kuhn-Tucker [1951] conditions, notably the transverse orthogonality requirement that either price or excess capacity of an input (or both) must be equal to zero. Then, Abraham Wald, in three short papers (one of which is lost) solved the problem of existence and uniqueness of equilibrium. In it he introduced what amounts to the concept of revealed preference,

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effort I have only been able to find one or two passages in *The Wealth of Nations* (and none in *The Theory of Moral Sentiments*) that deal with the price mechanism and its role in the allocation of resources. The invisible hand passage deals with quite a different matter—the ineffectiveness of good intentions—of the wish (or the affectation) to "trade for the public good" as a means to promote the general welfare, and the far greater effectiveness of pursuit of self-interest here. Smith does comment on influences that promote growth (in Book III), notably division of labor (in the very first chapter of *Wealth of Nations*). But surely his growth analysis is quite primitive and ahistorical. After all, it is his "early and rude state of society. . . ." and similar constructs by other economists who followed him that led Marx to remark that to them ". . . there has been history but there is no longer any" [*The Poverty of Philosophy*, nd., 1846–1847, 1884, p. 102].

but his proof entailed some rather questionable assumptions that were needed to prove uniqueness. Then, Dorfman, Samuelson, and Solow [1958, pp. 366–375] provided a more straightforward and intuitive proof, using the Kakutani fixed-point theorem (for all the references in this paragraph, see Dorfman, Samuelson, and Solow [1958]).

From the point of view of the economics, one can remark that the path chosen by general equilibrium theorists then takes the direction opposite to that of macroeconomists. I describe the latter as "the simplifiers," meaning that their success rests on a willingness to trade off simplification of economic reality, for a marked increase in analytic tractability and enrichment of results. The general equilibrium theorists, in contrast, are "the complicators," who omit as little as possible from their models and, as a result, sacrifice tractability and the opportunity to derive conclusions that are directly applicable.[9] As Dorfman, Samuelson, and Solow put the matter:

> Before going on to formulate a simple general-equilibrium model . . . it is worth wondering what kinds of questions can usefully be asked of such a model . . . that cannot be answered by less ambitious models? It seems apparent that a system which leaves many supply and demand functions (or the utility and production functions which lie one step further back) almost completely unspecified as to shape can yield only incomplete results. If we ask the Walrasian equations what will happen to the price of Commodity A if the supply of Factor F shifts to the right, the answer we get is literally the disappointing "That depends"—depends on the shape of just about every schedule appearing in the equations of the system. . . . Actually, in connection with abstract Walrasian systems, the main question that seems to have been studied in the literature has to do with the *existence* of an equilibrium solution to the collection of equations and with the *uniqueness* of the equilibrium if it exists [p. 349].

More recently, there has been work indicating that, under plausible assumptions, not only is uniqueness likely to be violated, but the number of possible equilibria can be very large and unstable.[10]

9. It has been suggested by a reader of the manuscript of this paper that "generalizers" is perhaps a better description than "complicators" of the general equilibrium theorists. I will not quibble with this suggestion, although it strikes me that the models sometimes rely on rather drastic assumptions and so fall considerably short of generality.

10. Recent work by Sonnenschein, Debreu, Mantel, and others has shown just how serious the problem is. Given any finite set of n-dimensional vectors, and a corresponding set of n-by-n matrices satisfying only the basic requirements of demand response matrices, it is possible to specify an economy consisting of no more than n rational consumers, for which the specified vectors will be the equilibrium price vectors, and the specified matrices the responses of the excess

It may appear from all this that there is not much to be said for the achievements or promise of general equilibrium theory. But that, I believe, would be a great error. I am not seeking to praise the theory with faint damns by concluding that it is unreasonable to expect such a deliberately complex theory to provide general and rigorous results that offer substantial insights on the way the economy actually works. However, it can and often does prove that intuitively plausible conclusions of partial models do *not* always hold, so that they cannot be relied upon. It is claimed that an old Yiddish proverb asserts "for example is not a proof." But a general equilibrium counterexample can be a *disproof*. The theory thereby provides invaluable warnings that unqualified acceptance and promulgation of the results of plausible partial models can sometimes be little more than a leap of faith. For the general equilibrium analysis can demonstrate that very different alternatives are also possible and perhaps even likely. International trade analysis has provided a profusion of examples, showing, for instance, that it is not possible to predict the exact commodity composition of trade on the basis of comparative advantage information and autarky price ratios alone (see, for example, Dixit and Norman [1980, pp. 95–96]). Such negative results demonstrate the great value of general equilibrium theory, a product of the nineteenth century, to which much rigor has been added in the twentieth.

Imperfect Competition and Game Theory

Cournot's [1838] work, with its two chapters on the theory of monopoly, was well-known to Marshall, as was Edgeworth's pioneering article of 1897, at least by the time later editions of the *Principles* were published. Both these sources also dealt with duopoly and oligopoly. Edgeworth even arrives at and describes very clearly a saddle point solution (he calls it a "hog's back") and shows some of the game-theoretic properties of the solution when the participants do not adopt the strategies that are optimal only against the optimal strategies of their competitors. But Marshall chose to use little of this. Instead, he employed more rudimentary

demands to the prices at each equilibrium. There is no natural way to say which of these cases is more plausible than any other. Thus, the number of equilibria can be arbitrarily large. Moreover, since the local stability of an equilibrium depends on the matrix of price responses, and these matrices can also be specified almost completely arbitrarily, the stability properties of these equilibria are also almost completely indeterminate (on this see, e.g., McFadden, Mas-Colell, Mantel, and Richter [1974]).

graphic approaches, characteristically devoting the bulk of his discussion, instead, to wise intuitive observations and institutional material. His material on the monopolistic firm proceeds without even the concept of marginal revenue, and his basic diagram is not easy to read, so one can readily understand why it was avoided by later writers.

Yet, despite all this, Marshall is able to discern results now familiar but apparently new for the time. Thus, he notes that while monopolistic profit maximization obviously tends to call for an output lower than that which yields zero economic profits, it may still exceed the output of an otherwise similar competitive industry because amalgamation of production and distribution can shift the monopolist's cost curves downward. He also shows that a tax that is fixed either in total or as a percent of total profit will lead to no change in the profit-maximizing price and output.

He is, perhaps, most innovative in his comparative statics. Cournot had already introduced a comparative statics analysis of such matters as the effect of an excise tax on product price. But all of Cournot's analysis used differential calculus, so he could explore only very local movements resulting from very small changes in the parameter values. Marshall's diagrammatic method, in contrast, enables him to represent the entire range of possibilities, including multiple (local) maxima, where they exist. He is thereby able to show that where several local maxima are present, a small change in parameter value can make the global maximum jump from one such local maximum to another, even when the latter is located rather far from the first. And even if the two equilibrium points themselves are only slightly apart, the result can be a large modification in the payoff yielded by the global maximum. Thus, for example, he shows that a small rise in the cost curves produced by an excise tax can lead ". . . to a great diminution of production, a great rise in price and a great injury to the consumers" [*Principles*, pp. 483–484 fn]. Here, as he sometimes does elsewhere, Marshall shows that occasionally more can be learned using a rudimentary method than one that is more sophisticated.

Simply because it is so widely known even to beginners, there is not much to be said about the theory of monopolistic and imperfect competition, much of it stemming from debate over Marshall. The notion that there is something in between perfect competition and pure monopoly long antedated 1900. I know of no one who wrote analytically about pure monopoly who failed to recognize the existence of an intermediate state between that and perfect competition. Then, in the 1920s the subject came up in writings calling for modification of the standard model of perfect competition. For example, Viner [1921] pointed out the significance of product heterogeneity. Even earlier, discussions of price discrimination in railroad rates had injected doubts about the pertinence of the competitive model. In the United Kingdom there was debate on Marshall's attempt to use external economies to produce a downward-sloping average cost curve for a competitive industry whose firms had to have rising average costs for stability of equilibrium. In particular, Sraffa [1926] pointed out that an alternative way to reconcile stable equilibrium with scale economies is to abandon the assumption of perfect competition. These discussions led directly to the related but disparate work of Chamberlin [1933] and Robinson [1933], which hardly requires summation here. It is sufficient to say that the analysis provided some new tools that were not terribly complex but were helpful and rapidly attracted widespread use. It also offered some insights on firm behavior that are illuminating, if not universally accepted. Nevertheless, the initial popularity of the analysis has since waned, and even Joan Robinson herself later downplayed its significance.

Still, the framework continues to influence and complicate research. For example, in the significant recent work on international trade under scale economies a number of writers have devoted great ingenuity to incorporation of imperfect competition into their general equilibrium models (for references see Helpman and Krugman [1985]). The fact remains that it is far easier to deal with perfect competition in complex theoretical models, particularly those studying general equilibrium, primarily because for decision makers under monopolistic or imperfect competition prices are not exogenously given data. Thus, while the significance of these market forms in reality continues to be recognized, in recent years they have, with a few noteworthy exceptions, occupied a secondary place in purely theoretical analysis.

Matters are rather different when it comes to oligopoly theory. Game theory, invented by Neumann and Morgenstern, with rich contributions soon following from Kuhn, Nash, Shubik, and others, brought unity to the field, but only to a degree. (For references see Leonard [1995] and *The New Palgrave Dictionary of Economics and Law* [1998].) Game theory certainly contributed a powerful and revolutionary set of mathematical instruments, offering economists a route for escape from exclusive dependence upon the physicists' formal tools. The new approach is a flexible way to deal with a variety of special issues and situations in oligopoly markets. Add to that the demonstrated relationship of the mathematics of game theory to mathematical programming, duality theory, and other analytic developments of the twentieth century, and it is clear that the field of oligopoly analysis (as well as other areas interpretable in game-theoretic terms) has undergone a major and useful upheaval. Only one reservation may be appropriate here. The very substantial degree of generality of the concept of game theory means that its results can be expected to be tailored to the particular model, that is, the special case that happens to be under consideration. But then, as we know, heterogeneity of behavior is also characteristic of the real oligopoly markets. As in general equilibrium theory, the game theory results that are *general* are consequently likely to indicate what propositions *cannot* be assumed to have universal validity, rather than providing conclusions about oligopolistic behavior that are universally, or usually, valid.

Oligopoly theory and related analysis have also assumed great importance for application. The theoretical tools of imperfect competition and oligopoly analysis have contributed to practice in antitrust activities and in the regulation of firms designed to constrain the exercise of monopoly power (see below). At this point it should only be noted that, for application, various modifications of, and additions to, the theory were required. For example, it was necessary to focus on the multiproduct activities in which almost all firms in reality engage. For many of the competition issues that occupy the courts and the regulatory agencies are related to this attribute of the firm. For example, a common question is whether a firm under investigation has employed "cross subsidy," that is, whether the firm has used profits above competitive levels earned on products sold in markets in which it is suspected of possessing monopoly power to finance uncompensatory prices for other products. Later, I will cite Ramsey analysis as an example of the sort of theoretical development that is applied to such practical issues related to multiproduct firms.

Conclusions on the Usual Suspects

There is unlikely to be much controversy about the list of contributions reviewed here so far, although my conclusions to this point will, predictably, elicit some limited dissent. Before turning to the portions of the century's contributions that may not be quite so obvious, a few words should be said about its innovations in method, including formalization of comparative statics analysis, revealed preference theory, experimental economics, and duality theory, in the sense of correspondence between expenditure and demand functions. The advances continue, as in Dixit and Pindyck's [1994] new approach to analysis of irreversible investment decisions under uncertainty.

Clearly, the most radical change is the victory of mathematical economics. Of course, such work has a long and distinguished earlier history. But what the century brought was recognition and triumph where, previously, mathematical economics had been in the hinterlands. Far from the mainstream, it was an object of suspicion rather than admiration. It may be hard for younger economists to imagine, but nearly until midcentury it was not unusual for a theorist using mathematical techniques to begin with a substantial apology, explaining that this approach need not assume that humans are automatons deprived of free will.

A number of contributors, Fisher, Moore, Bowley, Hicks, and Allen, were among those who led the way, and it is undoubtedly Samuelson, with his magical powers, who secured the final triumph. Since then, the approach has gained strength from new methods such as linear, nonlinear, and integer programming, the work of analysts such as Danzig, Gomory, Kuhn, Koopmans, and Tucker. There are even some who are driven to argue, perhaps with some validity, that the takeover by mathematical methods has gone too far. That it is imposing too much uniformity on the training of our graduate students may be true to a degree. But the occasional claim that it has forced theory into pure abstraction and deprived it of all relevance to reality as well as applicability is emphatically not true, as will be shown presently.

I turn next, and finally, to the two types of contribution that I believe most sharply differentiate the century's termination from its beginning.

III. The Revolution in Application of Theory and Empirical Investigation

Econometrics and Empirical Analysis

I have already alluded to the elementary state of the empirical evidence cited by Marshall. It is easy to find other writings from the first decades of the century to illustrate this, without denying that the theory and practice of statistics was already making substantial advances. The interest in data and their analysis also affected economics. For example, in the United States the work under the leadership of Wesley Mitchell at the National Bureau of Economic Research on business cycles and other subjects added much to our knowledge of these matters. Kuznets is a heroic figure in the empirical fields, opening up major avenues to measurement in our subject.

Substantial activity applying measurement to policy and theoretical issues seems to have begun soon after the First World War. Solomon Fabricant [1984] tells of the origins of the National Bureau of Economic Research in the aftermath of conflicting testimony by conservative engineer-statistician Malcom Rorty of AT&T and Nahum Stone, an economist with Marxist associations, at the hearings of the famous 1915 New York State Factory Investigating Committee. Their difference was over the workability of a minimum wage, and both participants came to realize that they were arguing without benefit of the requisite information. No data were even available on the share of national income obtained by labor. Meanwhile, the war ". . . revealed an appalling lack of the quantitative information needed to cope with the urgent mobilization and reconstruction problems facing the nation [p. 7]." Rorty and Stone decided to take remedial action, and soon after the armistice Rorty succeeded in getting financial support from the business community. This was used to found the National Bureau of Economic Research as a nonpartisan organization dedicated to the collection of data that could be used to shed light on policy issues. With this, systematic economic-data collection and analysis was launched in the United States, with application as a primary incentive for the undertaking.

However, it is arguably with the work on econometric theory at the Cowles Commission and the inauguration of a journal, *Econometrica*, dedicated to the subject, that econometric research attained the status of an important subdiscipline of our subject. Koopmans' pioneering work on identification and estimation was followed by that of a number of noted contributors, including Frisch, Theil, Klein, Chow, Quandt, Goldfeld, Stone, McFadden, Hendry, and Deaton (for references, see *The New Palgrave Dictionary*'s [1998] entry on econometrics).

Empirical research has benefited not only from new and more powerful methods. The century has also provided invaluable new sources of data.[11] Government and international agencies such as the Bureau of Labor Statistics in the United States and various agencies associated with the United Nations have played key roles, along with the efforts of individual scholars such as Summers and Heston, Kravis, and Maddison. The data include the national income accounts inspired by the groundbreaking work of Kuznets, longitudinal data on households, firms and industries, extensive financial statistics, statistics on the state of the environment, productivity growth, and on and on. (For references to this work, see Baumol, Blackman, and Wolff [1989].) Today, it is hardly necessary to document the role of the study and analysis of data, its use to test theoretical models and hypotheses, and its place in the curriculum. Here, it is noteworthy that the first Nobel Prize in economics was awarded (to Tinbergen) for pioneering empirical work.

There is probably no significant economic issue that is untouched by investigation of pertinent data. For example, productivity growth and the hypothesis that the productivity levels of various economies are converging has been studied with the aid of the pertinent statistics by investigators such as Abramovitz, Wolff, Dowrick and Nguyen, Barro and Sala-i-Martin, and Quah. (For references see Baumol, Nelson, and Wolff [1994].) There are well-known studies of demand relationships, the behavior of firms and industries, and the fundamental relationships of macroeconomics. There are many commendable studies of behavior of the securities markets, of pricing in oligopoly markets, of the role of entrepreneurship, and so on. There are even empirical studies putting substance into welfare analysis (see Slesnick [1998]). At the same time, topics well outside mainstream theory are not

11. Of course, collection of economic data did not begin in the twentieth century. Indeed, since it is not just a century but a *millennium* that is being celebrated, it is appropriate to recall that early great database, the *Domesday Book,* whose principal investigator was, arguably, William the Conqueror. Lest it be argued that William was no economist, we need only recall that Ricardo was a stockbroker, Adam Smith was a professor, first of logic and then of moral philosophy, and that William Petty, generally taken as the founder of economic statistics, was a seaman, physician, surveyor, professor of anatomy, professor of music, land speculator, and jack of other trades. It should also be noted that in its survey of the King's new lands in southern England the book provided evidence of the degree of incursion of the prime instrument of the industrial revolution of the later middle ages—the water mill, that freed economic activity from dependence on human and animal power. The survey found nearly 6000 mills in southern England alone, estimated at about one for every 50 families. These mills did not just grind flour. They pitted olives, fulled (roughly, softened) wool, sawed lumber, ground mash for beer, crushed cloth to make paper, milled coins, hammered metal, and operated bellows for blast furnaces.

neglected. As just one significant illustration, only very recently a new and very illuminating empirical investigation of the effects of affirmative action on the lifetime performance of minority students by William Bowen and Derek Bok [1998] may for the first time have carried study of the subject beyond reliance on conjecture.

Empirical analysis has helped in a variety of other applications. For example, studies of the returns to education and the effects of education on income distribution have provided illumination for discussions of government spending on education. Studies of the incentive effects of taxation have contributed to rational examination of tax policy. The reader will undoubtedly find other illustrations.

The marriage of data study with systematic and rigorous methods of analysis has also led to new types of inquiry that themselves became specialized fields of study. Cliometrics is an illustration that brings out a significant point. By its very nature, economic history has from its beginnings emphasized facts rather than theory. Indeed, in the late nineteenth century the German historical school, notably Gustav Schmoller, had used its study of history as a weapon to attack economic theory (see Schumpeter [1954, pp. 814–820]). What is new about cliometrics is the sophistication with which it studies the facts and its propensity to act as a complement rather than a competitor to theoretical research. The field has progressed with the aid of contributors such as Fogel.

In some fields such as labor economics the new profusion of data has had revolutionary effects. It shifted from an arena that was heavily institutional to one that was primarily data-driven. Econometrics evolved along with labor economics, and great strength was added to the analysis of items such as limited dependent variable models, panel data, and selection bias. Moreover, the availability of household data sets shifted the focus of the field from labor demand and internal labor markets to labor supply and the returns to human capital. Thereafter, the recent emergence of matched establishment-employee data sets and the data files of the personnel of firms has returned the focus to labor demand and relationships within the firm.

But there is more than these observations to bring out the radical change during our century in the position of empirical research. For we have grown increasingly uncomfortable with theory that provides no instruments for analysis of the facts and no opportunity for empirical testing. Earlier, in the discussion of the recent macroeconomic models of growth and innovation, it was suggested that theoretical insights are not their only or even their most fruitful contribution. Rather, it is their role as the basis for statistical estimation and testing of theory that can perhaps be considered their primary accomplishment. From its inception in Solow's work, modern macroeconomic analysis of growth has featured empirical investigation as its most novel contribution, and one of profound significance. The growth analysis has permitted us to grapple with the difficult problem of estimation of the contribution of innovation to growth (see Temple [1999]). Thus, the theory has helped empirical research, and the favor has been returned. A good example is provided by studies of the convergence hypothesis, which have also led to modifications of growth theory. The various methods used by different investigators of convergence pretty much agree that there is a small group of wealthy nations whose productivity levels and per capita incomes have been converging toward approximately common levels. However, the poorer countries are falling further behind the members of that convergence club. Romer pointed out that these statistical results do not fit comfortably with the original Solow model in which diminishing returns to capital appear to call for universal convergence. This led to attempts to incorporate innovation as an endogenous variable in the growth macro models, as an alternative mechanism capable of creating convergence among more successful economies if technical progress stimulates more technical progress, but does not require that convergence be ubiquitous.

Interaction of theory and empirical research now pervades the literature. It is, for example, at the forefront of the writings seeking to account for growing income inequality in the United States and elsewhere, entailing an amalgam of international trade theory, theory of technical change, and extensive study of the data (for references see Burtless [1995]). Statistical investigations of income distribution in earlier decades of this century gave us the evidence that was claimed to have shown remarkable constancy of the share of GDP received by labor and led to much theoretical work designed to explain this observation. More than that, this work gave theorists the Cobb-Douglas function whose attractive and simple analytic properties have led to its invasion of various branches of economic theory, some very distant from the theory of distribution.

Here it should be noted that the emergence of data, the advance of theory, and the use of both in application have not proceeded in lockstep. In different fields sometimes data availability, sometimes theory, has lagged substantially behind the other, significantly impeding application.

One last example, input-output analysis, will move us toward my central conclusion that a major accomplishment of the century is the mutual support that theory, data *and application* have come to provide to one another. It has become a commonplace among those in the field, encouraged by Wassily Leontief himself, to assert that input-output analysis has emerged as the current end-product of a line of thought beginning with Quesnay. In outline, the usual story is that the *Tableau Économique* is the first general equilibrium model in the literature and that, minor figures such as Canard and Isnard apart, Marx was the direct successor of the physiocrats in the arena. Next, Marx having left his transformation problem unsolved, Bortkiewicz took up the implied challenge and built upon Marx's rudimentary general equilibrium model (the "simple reproduction model") to provide a viable solution to the transformation problem, one that is still widely relied upon. Then, when Leontief arrived in Berlin as a student, Bortkiewicz was assigned to him as dissertation adviser [conversation between Leontief and this author], thereby completing the chain that carried the interdependence analysis from Quesnay to Leontief.

But what a break there is between input-output and its presumed predecessors! The directly pertinent work of Quesnay, Marx, and Bortkiewicz in each case had its narrow circumscribed purpose, with no empirical connection. Quesnay used his table largely to support the view that manufacturing is a sterile activity and that only agriculture offers a surplus. Marx explicitly translated Quesnay's work into a static two-sector model, his "simple reproduction" concept. There the only immediate conclusion is that in a balanced and stationary economy divided into a sector that produces consumption goods and one that supplies producers' goods, the producer's goods used by the consumption sector must be equal in value to the consumption goods that go to the capital goods sector. Finally, Bortkiewicz used the Marxian reproduction scheme just to solve Marx's transformation problem. Marx had recognized that there must be some definable relationship between prices and the "values" of commodities, as he defined them, but he was unable to discover the precise character of that relationship. The solution was the last stage in the pre-Leontief story. Each step, it will be noted, pursued its author's immediate objective, *and was not designed to lead further.*

In contrast, input-output offers us a tool with a vast array of uses. The techniques have been applied to subjects as heterogeneous as international trade, economics of the environment, and productivity. It is not merely *capable* of using data; rather, it is designed for the purpose. Just to make the point—how such theory, a product of our century, permits both application and use of facts—I will provide a single illustration selected because it is so far afield from the topics to which input-output is commonly applied.

The topic is energy conservation, and various energy-saving projects such as public transportation by rail (subways), recycling of oil, and the use of solar energy and other new energy sources. As advocacy of such measures grew in intensity in the 1970s, dispassionate observers noted that these processes all *used up* energy resources, as well as providing or saving energy. For example, the agricultural products that are employed to produce biomass may be transported in trucks that use up gasoline, and the digging of subway tunnels also consumes enormous amounts of power. Seeking to analyze the issue systematically, engineers invented the concept of "net energy" in which the energy used up by a proposed activity is subtracted from the energy it is expected to contribute. But it soon became clear that the engineers' calculations had at least one major shortcoming. No account was taken of the fact that it requires inputs to make inputs—that the trucks carrying the biomass themselves had to be built and used energy in the process of their construction, and that the same was true of the assembly line used to build the trucks, and so on ad infinitum. Clearly, there was a Leontief process at work. In the usual notation, if we let D represent the vector of energy consumed per unit of output, and A is the Leontief matrix, then the proper measure of energy consumed is

$$D + DA + DA^2 + \cdots + DA^n + \cdots$$

But most of the engineers carrying out the net energy studies were considering only D as the measure of energy use. Some studies were more sophisticated and used $D + DA$ as their energy consumption measure. A *very* few studies even subtracted DA^2, but none went beyond that, thereby in effect assuming that $DA^3 + \cdots = 0$.

A full input-output calculation, using the standard data on the U. S. economy offered rather startling conclusions. The usual approach that takes into account only the energy of the directly used input overlooks, on average, over 60 percent of the true quantity of energy used. Even if a second round—the inputs used to make the direct inputs—is taken into account, some 28 percent of the total energy consumption is omitted. Thus, investments in what are deemed to be energy-saving measures that project, say, a 20 percent net energy yield were shown by the input-output calculation as more likely in fact to use up more energy than they provide.

Ménage à Trois: Marriage of Theory, Data Analysis, and Application

My central contention in this paper is that our century produced a new integration, or at least brought to a far higher level, the integration of theory, empirical investigation, and application. The preceding discussion of input-output analysis illustrates the sort of combination of these three strands that I have in mind. Marshall's *Official Papers* [1926] provides an illuminating contrast. Reading his extensive and impressive pieces of testimony, one comes away feeling that here is a well-informed man with considerable intuitive insight and common sense which, as is often true in our discipline, misleads him occasionally. But what is missing to a striking degree from that testimony is direct reliance on any theorems drawn from the formal analysis of economics, or any buttressing of his position with the aid of systematic statistical analysis. A few empirical data are occasionally cited, but they serve more as background, description, or illustration rather than anything that pretends to constitute analysis.

What led economists to turn to application based on more rigorous theory and analysis of data? Here it should be made clear that the focus on application is hardly a product of the twentieth century. Adam Smith, Ricardo, and their contemporaries did not study economic issues out of "idle curiosity" (as Veblen characterized the primary motive of academic research). What the century contributed was a new foundation for the discipline's applied work. Here it is difficult to provide any general explanation of this development, for its sources undoubtedly differed from one area to another. What is noteworthy is that in a number of cases it represents a response to external demand. For example, in industrial organization, at least some of the work grew out of consulting assignments, as attorneys for regulated firms seeking permission from the regulatory agencies for more flexible pricing rules heard that economic theory contained material called "marginal analysis" that could be used to help their cause. Economists (including this author) who were asked to provide help in their effort were not content simply to leave the story as the lawyers envisioned it. They were led to undertake further research, reinterpreting and applying older ideas (such as Ramsey pricing) and introducing new ones (see Schmalensee [1979]). I suspect that work on theory and data related to fields such as public finance and corporation finance was also stimulated by demand from outside our profession. But all this is conjecture about the mechanism driving the recent history of our field, conjectures themselves driven by neither data nor theory.

The Role of Government and Other Matters

Before continuing my discussion of applied work through the century, it is appropriate to comment on the role that economists have expected government to play. This is not to deny that applied economics has sometimes been oriented to bodies outside the public sector. Economists' writings on operations research and management science have addressed their advice primarily to business firms and other private organizations. Yet it is true that, even in these areas, research began in earnest during World War II, sought primarily to be helpful to the military services, and was heavily financed by them. However, work on topics such as inventory theory, transfer pricing, and transportation planning was not directed exclusively or even primarily to government operations.

Still, the applied economic analyses, on subjects such as inflation and unemployment, environmental policy, antitrust activity, taxation, and interest rates are surely aimed primarily at the public sector, and this immediately raises the general question whether the attitude of economists toward the role of the public sector has changed markedly since 1900. My impression is that there has never been a consensus on this role and that attitudes today are not radically different from what they were a century ago. There were, of course, economists who were passionately devoted to laissez-faire and some who believed that the market, left to itself, would cure most economic ills. But not many were disciples of Dr. Pangloss; few held that the ideal economic state of affairs was what has been described as "anarchy plus the constable" (on this, see Lionel Robbins [1952, 1978], especially Lecture II). After the creation of the Interstate Commerce Commission in 1887 in the United States, many economists, including Marshall (see below) discussed the regulatory role it was appropriate for the agency to play.[12] Similarly, following J. S. Mill, early twentieth century economists often examined the advantages and disadvantages of socialism dispassionately and did not simply reject it out of hand.

It is true that after the appearance of the *General Theory* many economists began to advocate a role to macroeconomic policy much more extensive than before. But this was merely a change in orientation; discussions of monetary and banking policy, including issues such as bimetalism and the gold standard, go back to the dawn of our discipline. The Great Depression also brought with it a school of market socialism led by Abba Lerner [1946] and Oskar Lange (see Lange and Taylor [1938]), but the same period witnessed the contrary positions of Hayek [1948] and von Mises [1949, 1966]. Not even the University of Chicago had a monolithic economics department. That department had a libertarian wing led by Milton Friedman, but it also contained more moderate voices such as those of Paul (later senator) Douglas, and Jacob Viner and produced Samuelson and Patinkin. In my view, then, the century displays no clear trend in the discipline favoring or rejecting government intervention markedly more than the past. Rather, the interesting and novel material is more systematic analysis of *what and how much* it is appropriate for the government to do and how it can best do these things.

And while in the macroeconomic arena many economists have advocated an enhanced role of government, in microeconomics the predominant trend seems to have gone in the other direction. Mainstream economics has generally applauded privatization and deregulation, and at the end of the century it surely contains few if any advocates of central planning. Most economists do favor intervention to protect the environment, to control monopoly power, to prevent fraud in financial markets, and so forth. Still, they recognize that all this incurs costs to society, and

12. Actually, some British regulatory measures were enacted before those in the United States. The early 1800s marked the beginning of British legislation intended to prevent railway companies from exploiting monopoly power. But only after the First World War, when the British government reprivatized the railways it had taken over as a wartime measure, was the Railway Act of 1921 adopted, which first imposed controls on rates and services.

that many types of intervention offer little benefit, or benefits that do not justify their costs.

Another noteworthy development related to application is the great expansion in the number of students completing graduate work in economics. This has been accompanied by a marked increase in the number of economists employed in government, the number of lawyers with degrees in economics, etc. This seems to have led to enhanced receptivity by government, including the courts, to the ideas of economists and that, in turn, has undoubtedly stimulated research activity designed to cast light on applied issues.

The enormous expansion in the number of undergraduates who have studied economics may have done even more to turn economists toward application, and to increase the interest of practitioners in the results. For example, the number of lawyers who studied economics as undergraduates surely far exceeds those who took graduate economics. The same is surely true of the politicians, judges, and ordinary citizens who studied economics as undergraduates and who demand the use of economic analysis in application.

The interest in application has also encouraged work in what can be called "the new institutional economics," encompassing a broad line of endeavor very different from that of Veblen and Wesley Mitchell. It has, for example, included work on law and economics by jurists such as Areeda and Posner and by economists such as Fisher, Joskow, Klevorick, Ordover and Willig, Peltzman, and Schmalensee. But the work is broader than that, and encompasses material on the workings of the firm (on matters such as corporate governance) and the household. It should be clear that there are significant arenas (such as the construction and working of contracts and the operation of markets with heavy sunk costs) in which traditional neoclassical economics needs to be supplemented by the sort of institutional material supplied by these writers. It is no denigration of their contribution to note that earlier economists recognized the need for such work. It seems clear to me that Marshall, in particular, was an institutionalist at heart, and that a hallmark of his writings is attention to such matters, with systematic discussions of theory judged by that author to be a matter of secondary importance. This differentiates his work from that of the new institutionalists, whose hallmark is analysis grounded, wherever possible, in systematic and extensively structured theory. (For references see Hodgson [1998] and the *New Palgrave* [1998].)

Contributions of Formal Macro Analysis to Policy Formulation

The century has witnessed a complete change from Marshall's circumstances in terms of the theoretical resources available to economists who are called upon for advice by policy makers. This has occurred both on the macro and micro levels. The macro story is so well-known that little need be said about it here. Soon after the Second World War the Council of Economic Advisers was established in the United States, by act of Congress. The Keynesian revolution had overcome all opposition, to the point where President Nixon was later moved to declare: "We are all Keynesians." The Council followed Keynesian formulas, and for a while they seemed to steer the macroeconomy with remarkable accuracy and predictability. Then, with the military spending of the Vietnam War, the economy began to misbehave in ways that had not been expected, and the Keynesian positions were assaulted by the monetarists, notably Milton Friedman and his colleagues, not without effect.

Today, there is much greater skepticism about the effectiveness of fiscal and monetary policy in steering the economy between unemployment and inflation, or in avoiding their simultaneous occurrence, except in the relatively short run. Still, policy designers surely feel that they understand their options much more clearly than their counterparts did at the beginning of the century. They also feel that there now exists a substantial body of analysis, both Keynesian and monetarist, which it is necessary for them to master to a degree, and from which they can expect illumination, if not a foolproof set of behavioral rules. Yet it is ironic that when faced with the prospect of an economic downturn we do still sometimes retreat to Marshall's suggestive dictum that the ". . . chief cause of the evil is want of confidence."

Microeconomic Policy Arenas

Though less widely publicized, contributions to policy from the microeconomic literature have been no less extensive or effective. The breakthrough came in 1927 with one of the great contributions of that young and tragically short-lived genius, Frank Ramsey. Expressed as a result about optimal taxation, it was only generally recognized as a rule for regulation of pricing by a firm with market power after Boiteux (along with many other noted contributors) rediscovered Ramsey's result independently, and recognized its other uses. The theorem tells us that where there are scale economies or diseconomies, so that marginal-cost pricing does not yield zero economic profits to the firm, second-best prices may be taken to be those that are Pareto-optimal subject to a profit constraint. The analysis then yields an explicit formula for determination of that second-best price. In the simplest case where all other activities are perfectly competitive and, for the products of the firm in question, cross elasticities of demand are all zero, the formula is particularly straightforward. It is what has come to be called "the inverse-elasticity rule:" that the percentage deviations of the prices of the firm's products from their respective marginal costs should all be equal to the same constant, multiplied by the inverse of the firm's elasticity of demand for that product. The intuitive explanation is simple—if to cover costs, prices must be raised above marginal costs, revenues can obviously be increased with minimal demand distortion by raising most the prices of the products with lowest demand elasticities.

To me it is astonishing that, in the many regulatory agencies with which I have dealt in the United States and a number of other countries since the mid-1970s, I have almost never met a regulator who was unaware of what now is called "Ramsey pricing," and who did not have strong views on its relevance. I can easily cite decisions of courts and regulatory agencies in which it plays a significant and explicit role. And this is but one example. Regulatory agencies routinely consider such matters as marginal costs, demand elasticities, and the other paraphernalia of elementary economics. Very recently, in the course of litigation before a panel of three judges, one of them asked me to explain the prisoners' dilemma, and the other two judges then used the concept repeatedly in their subsequent remarks.

Thus, the concepts used are often quite sophisticated, and microanalysis is often called upon to deal with new issues as they arise. For example, there is a debate under way throughout the industrial world on the appropriate pricing of access to "bottleneck" facilities owned by a monopolist, which the law requires the monopolist to rent to competitors in the final-product market. This plays a critical role, for example, in deregulation of electricity generation, where rival generators need access to the transmission facilities of the public utility that is itself also a generator. The same problem occurs under the Telecommunications Agreement of 1977 among 70 countries, in which each pledges to admit competitors from the other countries. For, given the high cost of plant replication, a rival from country X seeking to provide telephone service in country Y will need to rent facilities from a telephone company (often a monopoly) in Y. Economic theory has been used by Robert Willig [1976] to derive a formula for efficient pricing of such access, and the debate over the proposed formula occupies space not only in economic journals, but in a plethora of court and regulatory agency decisions throughout the world. Academic research on applied theory of industrial organization continues in profusion (see, e.g., Schmalensee [1979] and Laffont and Tirole [1999]).[13]

Fortunately, it is easy to compare this sophisticated analysis with its counterpart at the beginning of the century. Railroad rates had for some time been a subject of heated public debate. The Interstate Commerce Commission Act was only a bit more than a decade old, so that railroad regulation was an issue of great interest to economists and much was written on the subject. Arthur T. Hadley, the noted economist-president of Yale University, had devoted an entire book to the subject. In his *Principles of Economics* [1912] Frank Taussig of Harvard has two chapters, and in *Industry and Trade* Marshall includes four chapters on transportation, railroads, and rate setting. These discussions are marked by two attributes: remarkable intuitive insights and very primitive analysis. Marshall clearly is frustrated by the inadequate data available to regulators (and to economic analysts). Here is an illustrative passage:

> When the studies of the Commission have made considerable progress, it will probably be possible to arrive at an approximate judgment as to the relations between the total costs and the total charges of any particular railway that may fall under suspicion. Its original cost can be estimated roughly from the statistical history of the railway, and can be compared with similar estimates as to other railways: and its methods of administration can be noted; with special reference to the question whether fresh capital was raised to carry out simple improvements the cost of which should have been defrayed out of income. Also, a direct comparison can be made of its charges as a whole, with those of other railways which have about equal facilities for obtaining a dense and regular traffic, equal costs for materials, etc. Some of these railways are sure to be managed efficiently and honestly and they will serve as a touchstone for the rest [*Industry and Trade,* pp. 843–844].

The economists of the time were aware that there are two

13. For discussion of microeconomics and legal analysis more generally, see Cooter and Rubinfeld [1989].

main sources of the special problems in determining appropriate prices for different types of freight, different types of passenger traffic, etc. The first complication is the substantial share of common costs, notably rails and roadbed, that serve every type of traffic, and the second problem is the apparent presence of scale economies. To cover the common costs, the economists (not yet having the Ramsey solution) were prepared to approve, in principal, markups for each service proportionate to costs. "From a purely abstract point of view it might seem proper to assign to each service its own direct costs, together with a proportionate share of those which belong specially to services of a like kind with itself, and another proportionate share of those which are common to the whole railway" [*Industry and Trade,* p. 469]. But they recognized that the demands for the different services of the railroad may not permit such markups, so they were prepared to accept price discrimination of at least some degree. Thus, Hadley concluded in a discussion of rail regulation: "The principle of charging what the traffic will bear, adopted by our large corporations, is a good one; it is only when it is made a pretext for charging what the traffic will *not* bear, that it gives rise to abuses" [1902, p. 175].[14]

Taussig sums up the state of the analysis effectively:

> Railways have two marked economic characteristics . . . first, the great size of the plant; and, second, the fact that the operations are conducted largely at joint costs. Both have important consequences for the problems of public regulation. . . . Connected with the large plant is . . . a tendency to decreasing cost per unit of traffic . . . it follows . . . that concentration and monopoly promote the thriftiest way of laying out the railway net. . . . Many peculiarities in railway rates are explained by the principle of joint cost. It underlies the much-misconceived practice of "charging what the traffic will bear. . . ."
>
> [T]he great mass of joint expense . . . must be got back somehow, or else railways will not be built. Some items of traffic will "stand" a heavier charge than others; that is, they will continue to be offered even though the transportation charge be high. . . . The joint expense will be got back from the former set much more than from the latter. . . . That the principle of joint cost explains (in the main) the practice of charging what the traffic will bear does not prove the practice to be just. . . . To arrange [regulate] railway charges on a "just" basis . . . is a task of peculiar difficulty and complexity. . . . Rates as a whole should not be higher than will suffice to yield a normal return on the capital invested in railways. . . . Even though no absolutely precise settlement of such a rate of return be feasible, an approximation to it can be

14. Note, incidentally, that the book is subtitled "An Account of the Relations Between Private Property and the Public Welfare," thus bringing the term "welfare" into the literature nearly a decade before Pigou.

> reached—six per cent, or eight per cent, or something of the sort. But this helps very little as to any individual rate. Whether the individual rate is "reasonable" is a question of its right adjustment to traffic demand and to the best utilization of plant and equipment. It happens that this question of principle has not often been deliberately considered in the United States or in other countries [1913, Volume 2, pp. 366–376].

Application of sophisticated modern micro theory has also occurred in arenas other than rate regulation, such as securities markets, where portfolio theory and other sophisticated concepts have made a substantial mark. The work of Black and Scholes [1973] on real options can be considered one of the literature's great accomplishments since Marshall, and not just for finance theory. Tax policy, too, has felt the influence of microanalysis. The same is true of environmental economics. And much of this sort of application of microtheory *has* found its way into modern textbooks. Nor is this all. In short, application of formal economic analysis has achieved a substantial standing far beyond anything that could have been imagined at the beginning of the century.

IV. Concluding Comment

I am well aware that in attempting in so brief a space to select the main accomplishments of our discipline during the preceding century I have engaged in blatant chutzpah. Worse than that, I have undoubtedly overlooked some invaluable accomplishments whose authors will have good reason to take umbrage at my carelessness or ignorance to which such omissions must be attributed. Finally, in my efforts to avoid confining myself to the obvious, this review may well have gone off in odd directions. But this is all by way of apology for myself. It should not detract attention from what seems to me to be the main lesson of this discussion. In our discipline, the century has been full of accomplishments. New ideas, new directions, and powerful new tools have emerged in profusion. Evidently, our field of study is alive and well, and poised for a rapid start into the twenty-first century.

Appendix: On Nineteenth Century Precursors of Modern Utility and Demand Analysis

I will argue that the use of Marshall's work to indicate the state of the economic literature at the turn of the century is somewhat misleading. For this I will note some ideas that may be considered modern and that had already been explored before 1900 but were absent from the master's works, using utility and demand theory as my example. As a matter of fact, a considerable number of writers in the nineteenth century went well beyond Marshall in this area. Several of the writings were known to him, although he chose to mention their contributions only briefly, if at all.

Marshall's failure to build on these can be attributed to a considerable degree to his reservations about formal theory and the use of mathematics in economics. This was despite his having achieved the status of Second Wrangler in the Mathematical Tripos in 1861, and starting off in economics by translating ". . . as many as possible of Ricardo's reasonings into mathematics" [Keynes, 1951, pp. 131, 151]:

> In a stationary state . . . [e]ach effect would be attributable mainly to one cause; there would not be much complex action and reaction between cause and effect. . . . But nothing of this is true of the world in which we live. Here every economic force is constantly changing its action, under the influence of other forces which are acting around it. . . . In this world therefore every plain and simple doctrine as to the relation between cost of production, demand and value is necessarily false; and the greater appearance of lucidity which is given to it by skillful exposition, the more mischievous it is. A man is likely to be a better economist if he trusts to his common sense, and practical instincts, than if he professes to study the theory of value and is resolved to find it easy [*Principles,* pp. 367–368].[15]

And also:

> It is obvious that there is no room in economics for long trains of deductive reasoning. . . . It may indeed appear at first sight that the contrary is suggested by the frequent use of mathematical formulae in economics. But on investigation it will be found that this suggestion is illusory, except perhaps when a pure mathematician uses economic hypotheses for the purpose of mathematical diversion; for then his concern is to show the potentialities of mathematical methods on the supposition that material appropriate to their use has been supplied by economic study. He takes no technical responsibility for the material, and is often unaware how inade-

15. Note the similarity in spirit to Veblen's view: "the psychological and anthropological preconceptions of the economists [entail a] conception of man [as] that of a lightning calculator of pleasures and pains, who oscillates like a homogeneous globule of desire of happiness under the impulse of stimuli that shift him about the area, but leave him intact. He has neither antecedent nor consequent. He is an isolated, definitive human datum, in stable equilibrium except for the buffets of the impinging forces that displace him in one direction or another. Self-poised in elemental space, he spins symmetrically about his own spiritual axis until the parallelogram of forces bears down upon him, whereupon he follows the line of the resultant. When the force of the impact is spent, he comes to rest, a self-contained globule of desire as before" [1898, pp. 389–390].

> quate the material is to bear the strains of his powerful machinery [*Principles,* p. 781].

Marshall knew and cited a number of the earlier writings on demand and utility that anticipated things likely to be credited to the twentieth century, but he often chose not to build on them. Thus, Marshall was aware of the indifference curve concept. He mentions it in notes xii [first edition, 1890] and xii bis [second edition, 1890] of the mathematical appendix to the *Principles,* attributing them, appropriately, to Edgeworth [1881]. In that note Marshall also reports on Edgeworth's concept of the contract curve and (implicitly) notes that it is the locus of points of tangency of the indifference curves of the two parties under consideration. Two decades before the new century Marshall had also constructed a rather elaborate model with many diagrams employing offer curves throughout, although he did not derive them from any indifference maps [1879, much of it reproduced as Appendix J of Marshall 1923]. Yet, he did not choose to use anything beyond simple demand curves in most of his analysis. He does discuss utility and its relation to demand, but prefers simplistic functions that are additively separable, so that complementarity and substitutability are essentially ruled out: "Prof. Edgeworth's plan of representing [utilities] as general functions of *x* and *y* [the quantities of different commodities consumed] has great attraction to the mathematician; but it seems less adapted to express the every-day facts of economic life than that of regarding, as Jevons did, the marginal utilities of apples as functions of [the quantities of apples] simply" [*Principles,* p. 845].

As a matter of fact, utility and demand theory had gone well beyond this by the beginning of the new century. Although nine years were still to pass before publication of Pareto's *Manual* and fifteen years before the appearance of Slutsky's now famous paper (but neglected for two decades outside of Italy), there had already appeared the relatively primitive work of Edgeworth [1881] and Fisher [1892, 1925] on the subject. But major steps of far greater sophistication had already been taken by G. B. Antonelli [1886] and W. E. Johnson and C. P. Sanger [1894], bringing them close to the noted contribution of Hicks and Allen [1934]. It is easy to understand why Marshall did not know about the former's work, since it was privately printed by an engineer who never wrote anything else on mathematical economics, and remained unknown until Wald called it to our attention more than a half century later. However, the Johnson-Sanger piece might have stood a better chance of eliciting Marshall's attention since both authors became members of the faculty at Cambridge (at King's and Trinity, respectively), both subsequently published in the *Economic Journal*, and their paper was presented to the Cambridge Economics Club in 1894. The Antonelli article provides one of the first examples of the use of determinants in economics, and studies the issue of integrability ahead of Fisher's and Pareto's [1911] consideration of the subject. Johnson and Sanger study the theory of utility maximization subject to a budget constraint, examine the role of variations in income, and investigate the interpretation of the marginal utility of money, as well as that of consumers' surplus, when the model contains an arbitrary number of interdependent commodities. In short, the theory of demand had advanced well beyond Marshall by the beginning of the twentieth century. The same is true of a number of the other subjects he treated.

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