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Neoclassical Theory in America: J. B. Clark and Fisher

By JAMES TOBIN*

The intellectual breakthroughs that mark the neoclassical revolution in economic analysis occurred in Europe around 1870. The next two decades witnessed lively debates in which the new theory more or less absorbed or was absorbed in the classical tradition that preceded and provoked it. In the 1890s, according to Joseph A. Schumpeter (1954, p. 754) there emerged "a large expanse of common ground and . . . a feeling of repose, both of which created, in the superficial observer, **an impression of finality**—the finality of a Greek temple that spreads its perfect lines against a cloudless sky." Of course the temple was by no means complete. Its building and decoration continue to this day, even while its faithful throngs worship within.

American economists were not present at the creation. To a considerable extent they built their own edifice independently, designing some new architecture in the process. They participated actively in the international controversies and syntheses of the period 1870-1914. **At least two Americans were prominent builders of the "temple," John Bates Clark and Irving Fisher.** They and others brought neoclassical theory into American journals, classrooms, and textbooks, and its analytical tools into the kits of researchers and practitioners. Eventually, for better or worse, their paradigm would dominate economic science in this country.

I. The Founding of the AEA: the Failed Rebellion

The neoclassical triumph was far from clear in 1885, when the American Economic Association was founded. The founders were young economists rebelling against the long dominant classical tradition of Ricardo and John Stuart Mill. Several of the organizers, notably the main entrepreneur, Richard T. Ely, had absorbed in Germany historicist and institutionalist views of methodology, along with reformist and statist ideas of policy. The profession, small as it was, was badly split. Some of the elders, among them Simon Newcomb and William Graham Sumner, declined to join.

The new Association's constitution contained a Statement of Principles ". . . accepted as a general indication of the views and the purposes of those who founded the . . . Association, but . . . not to be regarded as binding upon individual members." The quoted note was favored even by some participants in the organizing meeting who were sympathetic to the Statement, in order to make it possible for economists otherwise minded to become members. The first two of the four principles were: (AEA, 1887, pp. 5-46)

*Yale University. The assistance of Peter Mathews, a Yale economics graduate student, was invaluable to me in the preparation of this essay. I am deeply indebted to him, but the opinions expressed here are my own responsibility and all errors are my fault, not his.

1. We regard the state as an agency whose positive assistance is one of the indispensable conditions of human progress.

2. We believe that political economy as a science is still in an early stage of development. While we appreciate the work of former economists, we look not so much to speculation as to the historical and statistical study of actual conditions of industrial life for the satisfactory accomplishment of that development.

An echo of the second principle survives in the present charter as the first of the "objects of the society," adopted in 1888:

The encouragement of research, especially the historical and statistical study of the actual conditions of industrial life.

At the same 1888 meeting the original Statement of Principles was dropped, and to the initial purpose of "encouragement of perfect freedom of economic discussion" was added assurance that the Association will "take no partisan attitude, nor will it commit its members to any position on practical economic questions." (AEA, 1889, p. 86) This language too remains in the current charter. No doubt these catholic amendments made it possible for the Association to flourish.

As the marginalist revolution took root in America, it dealt a more decisive blow to the old orthodoxy, though not to its *laissez faire* implications, than the Germanic ideas inspiring Ely and his friends. The deductive method, "speculation," survived after all, indeed received a new and long lease on life. Pure theory was alive and well on this continent. Classical economics was demoted, as the rebels had hoped, but it was in an important sense revitalized, transformed from obsolescent tradition into exciting inquiry engaging the best analytical minds. This development in no way crowded out historical and statistical research or inhibited the policy interests and advocacies of American economists.

II. The Early American Neoclassical Economists

In Schumpeter's review of the American troops 1870-1914 (1954, pp. 863-877), the three superstars are Clark, Fisher, and Taussig. Frank W. Taussig (1859-1940) belongs in the triumvirate less for his own theoretical contributions than for his applied studies of international trade and tariffs, his distinguished public service, and his celebrated Socratic teaching of theory to generations of Harvard graduate students. He was a statesman of the profession. His theoretical ideas are scattered through his applied writings and through his *Principles of Economics* (1911, 1939) where they are

integrated with his considered interpretation and exposition of accumulated knowledge. This popular text, still used in Harvard's introductory course when I took it as a sophomore in 1936-37, was like Marshall's *Principles* a wise man's serious attempt to expound the whole field as he saw it. Alas, the days of books like that are past. Taussig's views and attitudes were Marshallian also: reverence for the classical elders, undiminished by acceptance of many of the amendments to their doctrines compelled by the neoclassical revolution; and unwillingness to carry theory, especially mathematicized theory, to extremes untempered by common sense, history, and observation.

I shall concentrate on Clark and Fisher, referring to their contemporaries, including Taussig, and predecessors only when their works are particularly relevant to my main task. Time and space do not allow me to recognize even in that degree several eminent theorists, among them: Thomas N. Carver (1865-1961), like Clark a distribution theorist; Frank A. Fetter (1863-1949), an effective neoclassical critic of Marshall; and Fred M. Taylor (1855-1932), a theorist of market socialism anticipating Oskar Lange and Abba Lerner. I cannot pretend to completeness in this review of the contributions of our early leaders to neoclassical theory.

A. Clark and His Conversion

John Bates Clark (1847-1938) was the most influential theorist and the most revered economist of the era in America. He was a founder of the Association and its third President 1894-95. In his time and ever since, he has been identified as the leading apostle of "marginalism" in general and of the marginal productivity theory of distribution in particular. *The Distribution of Wealth* (1899) is a genuine classic.

His standing in his time can be seen in Paul Homan's *Contemporary Economic Thought* (1928), where the first essay, all eighty-five pages of it, is devoted to Clark. "He is," the author says, "certainly the American theorist who during the past generation has made the most original and impressive contributions to abstract economic theory. Of international reputation, he has been classed by Professor Alfred Marshall as among the three or four great theoretical writers of the early twentieth century." Homan goes on to report the judgment of Edwin R. A. Seligman (1861-1939), another founder of the Association and later Clark's colleague at Columbia, placing Clark in the rarefied company of Ricardo, Senior, John Stuart Mill, Jevons, and Marshall (Homan, 1928, p. 17).

I recounted above the multi-faceted controversy that attended the founding of the Association in 1885, at the same time German versus English economics, historicism and institutionalism versus classical theory, and state intervention versus *laissez faire*. I pointed out how the issue was largely resolved by the triumph of an unforeseen third force, neoclassical economics. In 1885 Clark was definitely on Ely's side, although he was among those who favored stating the "principles"

moderately enough not to discourage membership. Like Ely and Seligman, Clark had been greatly influenced by his studies at Heidelberg under Karl Knies.

Clark's personal intellectual journey over the next fifteen years sums up what happened to the profession at large. His first book, *The Philosophy of Wealth* (1886) reflects his German training and the spirit of 1885. It contains an attack, albeit gentle and respectful, on the premises of classical theory: on its dismissal of human motives other than material self-interest; on its assumption that the economy is competitive and its glorification of competition; on its extreme individualism and neglect of the organic whole of society. The Ricardian system is described as "the apotheosis of selfishness." Clark proposes public interventions to restrain industrialists' economic power, to achieve through arbitration justice in distribution between capital and labor, to supplant competition and conflict by cooperation, and in general to subject economic processes to control by the higher morality of the community.

During the next ten years Clark became absorbed by the intellectual challenges of the theory of (functional) distribution. A series of papers paved the way to his *magnum opus* (1899). Homan says, "When . . . after years of patient thought and preparation, he published *The Distribution of Wealth*, the logical beauty and precision of the system of theory there displayed was like an illumination from Heaven to many of those whose goal for economic science was the reduction of economic life to terms of law and order." (1928, p. 34). However that may be, Clark had certainly discovered and embraced neoclassical economics; he completely reversed his earlier positions. Dramatic conversions are rare events at any age, and Clark's at fifty is as remarkable as Alvin Hansen's conversion to Keynesian economics at the same age in 1936.

Clark now finds that competition among self-interested individuals is the vehicle of social cooperation and justice he had found lacking thirteen years before. It now turns out that the organic whole of society is served by competition, because the market valuations of commodities and factors as derived from individuals' marginal utilities are their values to society as well. In later writings (Clark, 1901, 1904, 1912), it should be noted, Clark urged vigorous government intervention, not to supplant competition but to enforce it by anti-trust policies.

B. Fisher, the Theorist Born not Made

Today we look back on Irving Fisher (1867-1947) with awe and admiration, but he was not fully appreciated by his contemporaries. Today Fisher leads other old-timers by wide and increasing margins in journal citations. In column inches in the *Social Sciences Citation Index* (1979, 1983), Fisher led his most famous contemporaries, Wesley Mitchell, Clark, and Taussig in that order, by rough ratios 5:3:1:1 in 1971-75 and by 9:3:1:1 in 1976-80. Much more than the others, more-

over, Fisher is cited for substance rather than for history of thought.

Yet Homan's 1928 survey does not include Fisher. (Besides Clark, it covers Veblen, Marshall, Hobson, and Mitchell.) Today theorists would almost unanimously substitute Fisher's name for Clark's in the encomiums quoted above. Fisher was twenty years younger than Clark, and some of the contributions on which his current reputation is based came in the 1920s and 1930s. Nevertheless he had ample claim to recognition before the first world war, and certainly before 1928.

Time has thus substantiated Schumpeter's prediction that "some future historian may well consider Fisher as the greatest of America's scientific economists up to our own day." (1954, p. 872) Few would challenge that ranking in *our* own day, especially if we exclude immigrants like Schumpeter himself and candidates still living.

Schumpeter gives two reasons for the lukewarmness of contemporary opinion of Fisher. His zealous and almost cranky espousal of his many causes, some economic like the compensated dollar and 100% reserves, some non-economic like prohibition, eugenics, vegetarianism, and hygiene, distracted attention from his scientific achievements. His mathematical and analytical methods, the very features that appeal to modern readers and enabled Fisher to anticipate many later developments, were uncogent departures from the prevailing styles of doing economics at the time.

To these two factors might be added that unlike Taussig and Clark, whose cohorts of admiring students spread their fame throughout the profession, Fisher had few students and disciples. From personal experience I can also add that if Fisher was a prophet with insufficient honor in the profession, he was totally without honor in his own university outside the economics department. His crank causes were remembered, and also his incautious public and personal optimism about the stock market and the economy in 1929 and after, when he lost his family's fortunes and the university had to save his house for him.

Fisher was not involved in the controversies of 1885. Although he visited Germany during a post-doctoral academic Wanderjahr in Europe, he certainly was not subject to the Germanic influences on the Association's founders. He reflected on those old controversies and on the German influence on early American economics in his 1918 Presidential address to the Association, a strange speech greatly colored by the war just concluded (Fisher, 1919). He referred to "the curiously interesting fact that this Association largely owes its birth to German economics [which] brought us a new and altruistic impulse. In particular, we received from Germany the idea . . . of making economics of service to 'the state.' But . . . the war's revelations have made us realize that 'the state' served by German economists . . . was simply the Hohenzollern dynasty . . . Some among the very group of teachers who stirred the enthusiasm out of which this Association grew . . . helped to lay the foundation for the war," which Fisher identified as "predatory economics." He noted

that his own "revered master," Sumner, was among those who declined to join the new Association.

Fisher came to economics at Yale, where his whole life was spent from freshman year on. His mentors were Sumner, primarily a social theorist, J. Willard Gibbs, the great mathematical theorist of thermodynamics, and Arthur T. Hadley, an economist whom Schumpeter includes among six "who prepared the ground" (1954, pp. 865-66). Young Fisher's interests and talents were universal. By the time he finished his doctorate he had written and published poetry, political commentary, book reviews, a geometry text together with tables of logarithms, and voluminous notes on mathematics, mechanics, and astronomy for the benefit of students he was teaching or tutoring in order to support himself, his widowed mother, and his younger brother. Later Fisher published an introductory economics text (1910 and 1911); its graceful exposition of sophisticated theoretical material will impress a modern connoisseur, but it was too difficult for widespread adoption. Some of it survived in a leading introductory text of the 1920s and 1930s, by the younger Yale economists Fairchild, Furniss, and Buck (1926).

From the beginning Fisher was quite naturally and enthusiastically a mathematical economist, really the first one in the United States. Walrasian economics was the natural grist for his mill. But Fisher was never one to rely solely on deduction or "speculation," of which the insurgents of 1885 were so suspicious. He was as committed to "historical and statistical study of actual conditions" as any of them could have wished. Nor was he detached from policy and advocacy—quite the reverse, as already noted.

III. General Equilibrium: Value, Distribution, Capital, and Interest

There are several threads interwoven in the neoclassical fabric: "marginalism" in general, i.e. the calculus of maximization; marginal utility in particular, and the subjective theory of value; the marginal productivity theory of distribution; general equilibrium; and in some cases the mathematical method. Of course, none of these was entirely absent from pre-1870 economics. Marshall and Taussig could make a persuasive case for progressive continuity rather than radical innovation. Nevertheless the conjunction of these themes in the works of Jevons, Menger, and Walras—followed by Edgeworth, Böhm-Bawerk, Wicksell, Pareto, Clark, Fisher, and many others—generated discernible new style, scope, and substance in economic analysis.

The distribution of income and wealth, and in particular the sources, determinants, and social rationales of interest and other returns to private property, were obsessive topics in economics, in both Europe and North America, both before and after 1870. One reason, especially important in the intellectual environment of Europe, was the Marxist challenge to the legitimacy of property income. Answering Marx was a strong motivation for the Austrian school, in particular for the capital theory of Böhm-Bawerk and his followers. In positive as well as normative theory, neoclassi-

cal economics was in a much better position than classical economics to respond to the Marxist challenge. The labor theory of value, which Marx borrowed from the great classical economists, neither explains relative prices as commonly observed nor justifies functionally or ethically incomes other than wages. The contortions to which Smith, Ricardo, and Mill and their successors had to resort in order to remedy these failings were embarrassing. Solution of these long-standing problems was one great promise of the subjectivist-marginalist revolution of 1870.

These topics engaged the best efforts of Clark and Fisher, and each in his own way sought the answers in general equilibrium systems. Their methods were quite different. Clark did not use mathematics, and even diagrams are few and far between in his work. Fisher used mathematics and diagrams profusely.

A. Clark's Marginal Productivity Theory of Interest

The Distribution of Wealth is an ambitious undertaking, inspired by the vision of integrating into a single theoretical system consumption and production, capital and labor, interest, wages, and rents, marginal productivity and marginal utility. Clark limits his ambition to the analysis of stationary states. He distinguishes statics from dynamics. Acknowledging that the latter is more important as well as more difficult, he says that the static foundations must be laid first and, like many of us since, he defers the dynamics to later times and other theorists.

Clark is remembered most for the marginal productivity theory of distribution. Thünen, was there long before, in 1826, as Clark recognizes (Thünen, 1826). But no one paid attention, and in America and elsewhere the discussion of distribution was preoccupied with Ricardian rent theory and its implications for wages, with wages-funds, and with arguments over what incomes were residual claims. Clark contends that Thünen did not understand that payments of marginal productivity to intra-marginal workers did not exploit them, because all homogeneous workers are marginal, and that Thünen therefore did not appreciate the ethics of marginal productivity distribution. He claims also that Thünen did not understand that marginal productivity payments precisely exhaust the total product. Clark himself intuitively understood this theorem to depend on constant returns to scale, but he gave no proof. More important, Clark was seeking not only a common principle to explain all factor payments but also a distribution theory which, in combination with the marginal utility theory of consumption, would generate a competitive equilibrium, indeed an equilibrium with socially optimal properties.

Another independent discoverer or rediscoverer of marginal productivity theory was an American contemporary of Clark, Stuart Wood (1853-1914). A paper of his on the theory of wages (Wood, 1889) was presented at the very same session of the Association as one of Clark's early papers on the same subject. Modern readers would find Wood's paper the clearer and more precise, especially his treatment of labor/capital sub-

stitution. George Stigler recalled Wood to the attention of the profession (Stigler, 1947). In a series of remarkable papers Wood "discovered for himself a whole Walrasian system with variable production coefficients" (Schumpeter 1954, p. 869). Clark's 1899 opus does not cite Wood.

Clark's own approach was not Walrasian; it was quite aggregative. He assumes that both "interest," meaning the return to capital, and wages will be equalized across sectors. Competition and mobility achieve these equalizations, but in the equilibrium he is describing there is "mobility without motion"—a pretty phrase. For his purpose Clark needs a factor of production *capital*, homogeneous both across the whole economy and over time. He distinguishes this capital from capital goods.

Capital goods differ from industry to industry and from time to time. They are specific and transient embodiments of the general and permanent factor capital. This is the fund accumulated by the economy's savings up to date. (However, Clark takes land to be a part of the homogeneous capital stock, an awkward and unnecessary shortcut that destroys the equality of capital to accumulated savings and dismisses all Ricardian and Malthusian problems in one fell swoop.) In a stationary state the fund is constant, though the capital goods in which it is embodied may change with depreciation and replacement and with interindustry shifts. In these respects capital is like labor, which also is homogeneous and remains in constant supply while individuals enter and leave the labor force and their occupations change.

It is these two factors that produce aggregate output with constant returns to scale and have marginal productivities dependent on their relative supplies. It is these marginal productivities that determine interest and wages. The marginal productivity of a factor declines as its relative employment increases.

In describing marginal productivity as a function of inputs, Clark is at pains to distinguish between variations of labor relative to existing capital goods and variations relative to a constant stock of capital with capital goods appropriately adjusted to each situation. Clark calls the returns to existing capital goods, including land, rents. They will be equal to interest, the marginal productivity of capital, in full equilibrium, when the capital-goods composition of capital is appropriately adjusted. For durable but mortal capital goods these rents seem to be like Marshallian quasi-rents. But for natural and immortal land Clark ignores the fact that value must adjust since supply cannot and the resulting problems. Clark reserves the term "profits" for the temporary entrepreneurial surpluses of transitional dynamics.

Clark's model of aggregate production is essentially the same as that of the one-product two-factor "neoclassical" growth models of Roy Harrod (1939) and of several authors in the 1950s and 1960s, e.g. (Solow, 1956 and 1969). These models generalized Clark's stationary state to paths of steady growth. Of course modern neoclassical growthmen did not subscribe to Clark's case for the ethical justice of marginal produc-

tivity distribution. His stress on this normative argument nevertheless was the reason the revival of his model aroused the passionate fire it drew in the "war of the two Cambridges" in the 1950s and 1960s.

In referring to Clark's model, I slipped into modern parlance. Like most theorists of those days, Clark did not regard himself as building a model or telling a "story" or spinning a parable, as a theorist does today. In Fisher's more abstract modes, he thought of himself as a model-builder; indeed he actually constructed a hydraulic-mechanical model of general equilibrium (1892). Most theorists at the time, however, regarded themselves as discoverers of natural laws, like physical scientists. Their propositions, however derived and however, if at all, tested on observations, were meant to be taken seriously as accounts of the real world, not as logical exercises about hypothetical economies. Thus Clark regarded the diminishing marginal productivity of capital, as he defined it, as a law of nature. He did not—we know now after the heated capital controversies of the 1950s and 1960s, "double-switching" and all that, he could not—prove this was the case, either by deducing it from more primitive assumptions or by verifying it empirically.

The authors who revived the model a half century later did so in quite a different spirit. They were careful to specify the strict though unrealistic assumptions necessary to derive the model, and they did not regard either the assumptions or the model as incontrovertible law. But neither did they regard their theory of growth as an exercise of no relevance to the real world. Their justification was the rough congruence between the theory's implications and the "stylized facts" of economic history. They could not and did not deny that other models might "explain" those same facts.

Clark did not discover a natural law or an ethical justification for the functional distribution of income in capitalist societies. But his model of production and distribution remains the approximation of first choice for very many economists when they are confronted with such practical problems as accounting for observed growth rates over time and between different nations (Denison, 1967 and 1974), or estimating the consequences of tax incentives for saving and investment (Summers, 1981) or of public deficits (Gramlich, 1984) or of pay-as-you-go social security (Feldstein, 1974).

Clark's system has a demand side, where marginal utility is supposed to play a role analogous to that of marginal productivity on the supply side. Clark has been credited with independent discovery of marginal utility, a claim of which Paul Samuelson said, "To learn for yourself a new theory ten years or more after it has been widely published is to invite from the jury an indictment for negligence rather than an award for brilliance." (1967, p. 18) Clark's version does gain some original twists by recognizing that a given commodity might be a package of several different "utilities" provided in different proportions by other commodities. The idea is awkwardly suggestive of later formulations (Strotz, 1957 and Lancaster, 1966) where utility is

generated by characteristics possessed by commodities rather than directly by the commodities.

Clark's utility theory is inadequate for his system because it does not encompass future consumption and does not explain saving. The size of the permanent fund of capital in the stationary state is left hanging in the air. Clark criticizes "waiting" and "abstinence" as theories of interest by pointing out that no waiting is needed to obtain the consumable product of capital in the stationary equilibrium. That is true, but only because the saving to build up the stock has already been done, and of course because saving is by definition zero in the equilibrium. Why don't consumers try to eat up the fund? Had Clark applied here his "mobility without motion" insight regarding equilibrium, he would have seen that he was missing one of the scissor blades necessary to have a theory of interest. As it is, he is left without a platform from which to comment on Böhm-Bawerk and other participants in the controversies of the day regarding the sources of interest incomes.

B. Fisher: General Equilibrium with Intertemporal Choices and Opportunities

Throughout his career Fisher was fascinated by the same set of problems that engaged Clark. Fisher came back to them repeatedly. He attacked them in a more elegant, abstract, mathematical, general, and ethically neutral manner than Clark or Böhm-Bawerk, and at the same time in a clearer, simpler, and more insightful way than Walras. His first try was *Appreciation and Interest* (1896). This was Fisher's second substantial professional publication in economics. He returned to the subjects in 1906, 1907, and finally in 1930.

Fisher's first contribution to economics was his doctoral dissertation (1892). This is a masterly exposition of Walrasian general equilibrium theory. Fisher, who was meticulous about acknowledgements throughout his career, writes in the preface that he was unaware of Walras while writing the dissertation. His personal mentors in the literature of economics were Jevons (1871) and Auspitz and Lieben (1889).

The book describes the ingenious physical model noted above, which makes Fisher a precursor of a current Yale professor, Herbert Scarf (1973), and other practitioners of computing general equilibrium solutions. Fisher was greatly impressed by the formal analogies between the thermodynamics of his mentor Gibbs and economic systems, and he was able to apply Gibbs' innovations in vector calculus.

Fisher expounds thoroughly the mathematics of utility functions and their maximization, and he is careful to allow for corner solutions. He uses independent and additive utilities of commodities in his physical model; later he was to show how this assumption could be exploited to measure marginal utilities empirically (1927). But the general formulation in his dissertation makes the utility of every commodity depend on the quantities consumed of all commodities. At the same time, he states clearly that neither interpersonally comparable utility nor cardinal utility for each individual is

necessary to the determination of equilibrium. Fisher's list of the limitations of his analysis is candid and complete. The supply side of Fisher's model is, as he acknowledges, primitive. Each commodity is produced at increasing marginal cost, but neither factor supplies and prices nor technologies are explicitly modeled.

Finally, Fisher provides in appendices a survey and bibliography of applications of mathematical method to economics. From this beginning to his participation as elder statesman in 1933 in the founding of the Econometric Society, Fisher was a crusader for this methodological cause.

Fisher's general equilibrium system did not encompass capital and interest, but he attacked the subject soon after. His first contribution, one that should not be underestimated, was to get straight the concepts and accounting. This he did in 1896 and 1906 with clarity and completeness that have scarcely been improved upon. It's all there: continuous and discrete compounding; nominal v. real rates; the distinction between high prices and rising prices, and its implications for observations of interest rates; the inevitable differences among rates computed in different *numeraires*; rates to different maturities and consistency among them; appreciation, expected and unexpected; present values of streams of in- and out-payments; and so on. Schumpeter calls this work "the first economic theory of accounting" and says "it is (or should be) the basis of modern income analysis." (1954, p. 872)

Perhaps the most remarkable feature is Fisher's insistence that "income" is consumption, including of course consumption of the services of durable goods. In principle, he says, income is psychic, the subjective utility yielded by goods and services consumed. More practically, income could be measured as the money value, or value in some other *numeraire*, of the goods and services directly yielding utility, but only of those. Receipts saved and invested, for example in the purchase of new durable goods, are not "income" for Fisher; they will yield consumption and utility later, and those yields will be income. To include both the initial investment and the later yields as income is, according to Fisher, as absurd as to count both flour and bread in reckoning net output. This view naturally led Fisher to oppose conventional income taxation as double taxing of saving, and to favor consumption taxation instead. His views on these matters are loudly echoed today.

Fisher published his theory of the determination of interest rates in *The Rate of Interest* (1906). A revised and enlarged version was published in 1930 as *The Theory of Interest*. One motivation for the revision was that Fisher's many critics apparently did not understand the 1906 version. They typically concentrated on the "impatience" side of Fisher's theory of intertemporal allocation and missed the "opportunities" side. It was there in 1906 already; the theory is much the same in both versions.

In 1930 Fisher is at pains to label his theory the "impatience and opportunity" theory. "Every essential part of it," he acknowledges, "was at least foreshadowed by John Rae in 1834." He does claim originality

for his concept of "investment opportunity." This turns on "the rate of return over cost, [where] both cost and return are differences between two optional income streams" (1930, p. ix). As Keynes acknowledged, this is the same as his own "marginal efficiency of capital" (1936, p. 140).

In these books Fisher extended general equilibrium theory to intertemporal choices and relationships. His strategy was different from Walras's. Walras tried to extend his multi-commodity multi-agent model of exchange to allow for production, saving, and investment. This maintained his stance of full generality but was also difficult to expound and to understand. Fisher saw that intertemporal dependences were tricky enough to justify isolating them from the inter-commodity complexities that had concerned him in his doctoral thesis. Therefore he proceeded as if there were just one aggregate commodity to be produced and consumed at different dates. This simplification enabled him to illuminate the subject more brightly than Walras himself.

The methodology of Fisher's capital theory is very modern. His clarifications of the concepts of capital and income lead him to formulate the problem as determination of the time paths of consumption—that is, income—both for individual agents and for the whole economy. Then he divides the problem into the two sides, tastes and technologies, that are second nature to theorists today. One need only read Böhm-Bawerk's murky mixture of the two in his list of reasons for the *agio* of future over present consumption to realize that Fisher's procedure was not instinctive in those times.

Fisher's theory of individual saving is basically the standard model to this day. He stated clearly what we now call the "life cycle" model, explaining why individuals will generally prefer to smooth their consumption over time, whatever the time path of their expected receipts. But he was not dogmatic, and he allowed room for bequests and for precautionary saving. Where Fisher differed from later theorists, and especially from contemporary model-builders, was in his unwillingness to impose any assumed uniformity on the preferences (or expectations or "endowments"—the latter term was not familiar to him though the concept was) of the agents in his economies, and in his scruples against buying definite results by assuming tractable functional forms. In general, many of the advances claimed in present-day theory appear to depend on greater boldness in these respects.

On the side of technology, Fisher's approach was the natural symmetrical partner of his formulation of preferences, equally simple, abstract, and general. He assumed that the "investment opportunities" available to an individual (not necessarily the same for everybody) and to the society as a whole can be summarized in the terms on which consumption at any date can be traded, with "nature," for consumptions at other dates. In modern language, we would say that Fisher postulated intertemporal production possibility frontiers, properly convex in their arguments, consumptions at various dates.

All that remained for Fisher, then, was to assume complete intertemporal loan markets cleared by real

interest rates, count equations, and show that in principle the equalities of saving and investment at every date determine all interest rates and the paths of consumption and production for all individuals and for the society. Like hundreds of mathematical theorists since, he set the problem up so that it conformed to a paradigm he knew, in this case the Walrasian paradigm of his own doctoral dissertation. A more rigorous proof of the existence of the equilibria Fisher was looking for came much later, from Arrow and Debreu (1954). As we know, the problems of infinity, whether agents are assumed to have infinite or finite horizons, are much more troublesome than Fisher imagined.

In any event, Fisher had an excellent vantage point from which to comment on the controversies over capital and interest raging in his day. His formulation of "investment opportunities" seems to allow for no factor of production one could call "capital" and enter as argument in a production function. For that matter, he doesn't explicitly model the role of labor in production either, or of land. Strangely, in Fisher's insistence that interest is *not* a cost of production, he seems to say that labor is the only cost, evidently because labor and labor alone is a source of disutility, the loss of utility from leisure, the opportunity cost of the consumption afforded by work. Proceeding in the same spirit, he postulates that, from a position of equality of present and planned future consumption a typical individual will require more extra future consumption than present consumption as compensation for extra work. The difference, the *agio*, is interest, whether or not it is a "cost." Fisher attributes the *agio* to "impatience," at the same time scorning the notion that interest is the cost of securing the services of a factor of production called "abstinence" or "waiting."

In the 1890s and 1900s Knut Wicksell, discovering marginal productivity independently of Clark, was modeling production as a function of labor and land inputs with the output also depending on the lags between those inputs and the harvests (1934, vol. I, pp. 144-66). Similarly Taussig amended marginal productivity theory to discount wages and rents for the interest over the lag from input to output (1939, vol. II, pp. 62-66). These are "Austrian" formulations, akin to Böhm-Bawerk's examples of trees and wine, in which time itself appears to be productive. Fisher rightly objects to any generalization that waiting longer increases output. His own intertemporal frontiers are, to be sure, sufficiently general to encompass such technologies. They can also accommodate Leontief input-output tables and Koopmans-Dantzig activity matrices with lags, Hayekian triangular structures with inventories of intermediate goods in process, Solow technologies with durable goods and labor jointly yielding output contemporaneously or later. The only common denominator of these and other representations of technology is that they relate consumption opportunities at different dates to one another, though not necessarily always in the convex tradeoff terms Fisher assumed. There does not appear to be any summary scalar measure to which the productivity of a process is generally monotonically related, whether roundaboutness, average period of

production, or replacement value of existing stocks of goods.

Fisher describes himself as an advocate of "impatience" as an explanation of interest, though he realizes there are two sides of the saving-investment market, and though he acknowledges that real interest rates can at times be zero or negative. He does appear to believe that in a stationary equilibrium with constant consumption streams, consumers will require positive interest, and that only those technologies and investment opportunities affording a "rate of return over cost" equal to this pure time preference rate would be used. He does not face up to Schumpeter's 1911 argument that in such a repetitive and riskless "circular flow" rational consumers would not care whether a marginal unit of consumption occurs today or tomorrow (1936, pp. 34-36). Like Böhm-Bawerk, Fisher appeals to the shortness and uncertainty of life as a reason for time preference. For life-cycle consumers, however, time preferences are entangled with age preferences, and it is hard to defend any generalization as to their net direction. Fair annuities take care of the uncertainty.

Both John Bates Clark and Irving Fisher enlarged and improved the neoclassical temple, as Schumpeter described the structure in the passage I quoted at the beginning. The importance and quality of their contributions to theories of general equilibrium, capital, and distribution are shown by their absorption into the corpus of theory and their continued usefulness to economists today. Fisher's contributions have proved the more durable, and the more useful as foundations for further advances in theory. On a remarkable range of topics, modern theorists adopt and build upon Fisherian ideas, sometimes unknowingly. Fisher's methodologies, not just his use of mathematics but his explicit formulations of problems as constrained optimizations, is the accepted style of present-day theorizing. Those are the reasons that, of the two giants of theory in the early days of American economics, Fisher is accorded in fuller measure the esteem of his successors.

IV. Fisher as Monetary Theorist and Macro-economist

Although it may be questioned whether theories of money and of business fluctuations, macroeconomics in modern jargon, are neoclassical, I shall conclude this essay with remarks on this branch of theory. Here too Irving Fisher was the major American theorist of the early decades of this century, indeed until the early 1930s. An account of Fisher the theorist would be incomplete without some reference to this part of his work. Here especially Fisher combined theorizing with empirical research, both historical and statistical. The problems he encountered led him to invent statistical and econometric methods—index numbers and distributed lags are important examples—to apply for the purposes at hand to the data he and his assistants compiled. (He even studied the turnover of cash and checking accounts of a sample of Yale students, professors, and employees.) But I leave it to others to

report and appraise Fisher's econometric innovations and his empirical applications.

Money was a big subject in American economic literature in the nineteenth century, before Fisher came on the scene. The monetary events of the times—the inconvertible greenbacks issued during the Civil War, their redemption in gold in 1879, the demonetization of silver, the rapidly increasing importance of banks—stimulated research and controversy. Fisher's major treatise (1911) refers to works of Simon Newcomb (1885), Charles F. Dunbar (1901), Alexander Del Mar (1902), J. Laurence Laughlin (1903), Wesley C. Mitchell (1903), David Kinley (1904), and Edwin M. Kemmerer (1909).

Most of these are historical and empirical. In monetary theory, Fisher's most important predecessors are Newcomb (1835-1909), Del Mar (1836-1926), and Laughlin (1850-1933). Newcomb is a celebrated figure in American astronomy. He was also a mathematician and economist. He was one of the conservatives who shunned the Association in 1885, and most of his writings in economics were *laissez faire* propaganda. But he anticipated Fisher's Equation of Exchange and modeled the circular flows of goods and money that the equation summarizes (1885). Spiegel (1971) would cast him as America's first mathematical economist, but if that term is used in anything like its modern meaning the honor clearly belongs to Fisher.

Del Mar (1885) concluded, as did Fisher later, that money supply changes are wholly absorbed in prices after about ten years. His observations of transitional adjustments also anticipated Fisher in stressing the differences among individual prices in their rigidities, the relative stickiness of money wages, the cyclical movements of velocity due to changing expectations, and the roles of these phenomena in cyclical fluctuations. Del Mar was not an academic, but he was a great scholar, as his monumental work *A History of the Precious Metals* (1880, 1892) testifies.

Laughlin was the first of the series of distinguished monetary scholars and teachers at the University of Chicago. He was not an enthusiast for the quantity theory, largely because he saw that the money supply of any one nation was endogenous under the international gold standard (1903, 1905). Fisher saw the point and included it in his list of sources of variation of the American money supply, but he apparently thought the damage to his quantity theory was minor. Laughlin has recently been admirably rediscovered (Girton and Roper, 1978).

For all its theory, statistics, and index numbers, *The Purchasing Power of Money* is a tract supporting Fisher's proposal for stabilizing the value of money. This came to be known as the "compensated dollar," the gold-exchange standard combined with a rule mandating periodic changes in the official buying and selling prices of gold inverse to changes in a designated commodity price index. In 1911 Fisher proposed that the gold price changes be uniform and synchronous in the currencies of all countries linked by fixed exchange parities, in proportional amounts related to an international price index. Later he was willing to accept as

second best that the United States adopt the scheme on its own. Keynes proposed a similar but less formal rule for the United Kingdom (1923). The proposal is an early example of a policy *rule*, another Fisherian idea ahead of its time, more likely to be popular among economists today than it was with Fisher's contemporaries. Indeed some rules recently proposed are quite Fisherian, for example (Hall, 1985).

The "compensated dollar" is but one of several proposals Fisher advanced over the years for stabilizing price levels or mitigating the effects of their unforeseen variation. In the 1911 book he also writes favorably of the "tabular standard," which meant no more operationally than facilitating price-indexed contracts. In the 1920s he launched a crusade for 100% reserves against checkable deposits, culminating in *100% Money* (1935). This idea is also beginning to resurface in the 1980s as a preventive defense against the monetary hazards of bank failures. In Schumpeter's view, Fisher's zeal for monetary reforms lost him some of the attention and respect his scientific contributions to monetary economics deserved, and made him come across more monetarist than his own analysis and evidence justified (1954, pp. 872-73).

The Purchasing Power of Money is a monetarist book. Fisher asserts the quantity theory as earnestly and persuasively as Milton Friedman. There are two species of quantity theories. One is a simple implication of the "classical dichotomy," since only relative prices and real endowments enter commodity and factor demand and supply functions, the solution values for real variables in a general equilibrium are independent of scalar variations of exogenous nominal quantities. Walras exploited this implication of general equilibrium theory. Surprisingly Fisher does not. In any case, it does not quite apply to a commodity money system like the gold standard, which Fisher was analyzing. Fisher's theory is of the second kind, based on the demand for and supply of the particular nominal assets serving as media of exchange.

Starting from his Equation of Exchange, elaborated to distinguish the quantities M and M' of the two media currency and checking deposits and their separate velocities V and V' , Fisher argues: that the real volume of money-using transactions T is exogenous; that the velocities are determined by institutions and habits and are independent of the other variables in the Equation; that the division of the currency supply, the monetary base in current terminology, between currency and bank reserves is stable and independent of the variables in the Equation; that banks are fully "loaned up" so that deposits M' are a stable multiple of reserves, determined by the prudence of banks and by regulation; that exogenous changes in currency supply itself are the principal source of shocks, which, given the preceding propositions, move price level P proportionately. The many qualifications for transitional adjustments are conscientiously presented, but the monetarist message is loud and clear.

The argument is familiar to modern readers, but certain features deserve notice:

(1) Fisher gives the most illuminating account avail-

able of the institutions and habits that generate the society's demand for transactions media relative to the volume of transactions. He rightly emphasizes the fact that, and the degree to which, receipts and payments are imperfectly synchronized. He seeks the determinants of velocity in such features of social and economic structure as the frequency of wage and bill payments and the degree of vertical integration of firms. His belief that these institutions change only slowly supports his contention that velocities are exogenous constants.

(2) Much ink has been spilled on the difference between Fisher's velocity approach to money demand and the Cambridge (England) "k" formulation. The latter, like Walras's *encaisse desire*, directs attention to agents' portfolio decisions. To Fisher's critics that seems behavioral, while velocity is mechanical. The issue is overblown; the same phenomena can be described in either language. Fisher himself discusses hoarding. Fisher's explicit attention, in discussing economy-wide demand for circulating media in distinction to other stores of value, to the fact that money "at rest" soon takes "wing" to fly from one agent to another seems to me to be a merit of his approach.

(3) As already noted, Fisher resolved a question current in his day, whether banks' creation of deposit substitutes for currency should be regarded as increasing the velocity of basic money or as enlarging the supply of money. His choice of the latter course compels attention to the structure, behavior, and regulation of banks. He could not be expected to foresee that the proliferation of future candidates for designation as "money" would create the monetarist ambiguities we see today.

(4) Fisher's T covers all transactions, those for intermediate goods and financial assets as well as those for final goods counted in national product. His transactions velocity is correspondingly larger than circuit or income velocity. For the most part later writers have not followed his example. It is hard to attach meaning to the *real* volume of financial transactions, and therefore to see why a T that includes them should be constant or exogenous with respect to the Equation. On the other hand, modern students of money demand tend simply to forget transactions other than those on final payments.

(5) Fisher ignores the possibility that other liquid assets can serve as imperfect substitutes for money holdings because they can be converted into means of payment as needed, though at some cost. Partly for this reason, he ignores interest rate effects on demand for transactions media. In his day there may have been more excuse for these omissions than there was later. But they are still surprising for an author who elsewhere pays so much attention to the effects of interest rates and opportunity costs on behavior.

The quantity theory by no means exhausts Fisher's ideas on macroeconomics. His views were much more subtle than straightforward monetarism, but they are scattered through his writings and not systematically integrated. Consider the following non-neutralities emphasized by Fisher:

(1) Although the famous "Fisher equation" for nominal interest, real interest, and inflation is frequently cited nowadays in support of complete and prompt pass-through of inflation into nominal interest rates, Fisher's view throughout his career was quite different. From (1899) on he believed, and confirmed by sophisticated empirical investigation, that such adjustment takes a very long time. In the interim, inflation would lower real rates, as nominal rates would adjust incompletely. The effect was symmetrical; he attributed the severity of the Great Depression to the high real rates resulting from price deflation. Moreover, Fisher was always quite explicit about the effects of these movements of real interest rates on real economic variables, including aggregate production and employment, and their role in business cycles.

(2) An assiduous scholar of price data, Fisher knew that some prices were more flexible than others, that money wages were on the sticky side of the spectrum, and that the imperfect flexibility of the price level meant that the T on the right-hand side of his Equation would absorb some of the variations of the left-hand side. In the early 1930s he came to a very modern position. Real variables like production and employment are independent of the level of prices, once the economy has adjusted to the level. But they are not independent of the rate of change of prices; they depend positively on the rate of inflation. He even calculated a "Phillips" correlation between employment and inflation (1926). He was just one derivative short of the accelerationist position (Friedman, 1968); in a little more time he would have made that step, aware as he was of the difference between actual and expected inflation. Anyway, his policy conclusion was that stabilizing the price level would also stabilize the real economy.

(3) In the early 1930s, observing the catastrophes of the world around him, which he shared personally, Fisher came to quite a different theory of the business cycle from the simple monetarist version he had espoused earlier. This was his "debt-deflation theory of depression," (1932) summarized in the first volume of *Econometrica*, the organ of the international society he helped to found (1933). The essential features are that debt-financed Schumpeterian innovations fuel a boom, followed by a recession which can turn into depression by an unstable interaction between excessive real debt burdens and deflation. Note the contrast to the Pigou real balance effect, according to which price declines are the benign mechanism that restores full employment equilibrium. The realism is all on Fisher's side. This theory of Fisher's has room for the monetary and credit cycles of which he earlier complained, and for the perversely pro-cyclical real interest rate movements mentioned above. Fisher did not provide a formal model of his latter-day cycle theory, as he probably would have done at a younger age. The point here is that he came to recognize important non-monetary sources of disturbance. His practical message in the early 1930s was "Reflation!" He was right.

These insights contain the makings of a theory of the determination of economic activity, prices, and interest rates in short and medium runs. Moreover, in his neo-

classical writings on capital and interest Fisher had laid the basis for the investment and saving equations central to modern macroeconomic models. Had Fisher pulled these strands together into a coherent theory, he could have been an American Keynes. Indeed the "neoclassical synthesis" would not have had to wait until after the second world war. Fisher would have done it all himself.

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