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IRVING FISHER'S ECONOMETRICS

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I

THE great American who has departed from us was much more than an economist. But the vast realm over which he held sway and the intellectual climate of the epoch that nourished his thought have been admirably surveyed in this journal,[1] and I shall confine myself to Fisher's purely scientific work in our field. This will restrict our subject. But it will not lower it—at least, it could do so only through my own fault. For whatever else Fisher may have been—social philosopher, economic engineer, passionate crusader in many causes that he believed to be essential to the welfare of humanity, teacher, inventor, businessman—I venture to predict that his name will stand in history principally as the name of this country's greatest scientific economist.

I shall restrict my task still further. Mr. Sasuly, who has been a close collaborator of Fisher's, has presented a vivid and adequate picture of his statistical work and in particular has set forth the historical importance of *The Making of Index Numbers* and of Fisher's most original contribution to statistical method, the Distributed Lag. I am not going to repeat what he has written. It is the theorist only, not the statistician, who will be considered in what is to follow. Nevertheless, the statistician cannot be entirely eliminated even from the section of Fisher's activities with which I propose to deal. For throughout and from the start, Fisher aimed at a theory that would be statistically operative, in other words, at not merely quantitative but also numerical results. His work as a whole ideally fits the program of "the advancement of economic theory in its relation to statistics and mathematics" and of the "unification of the theoretical-quantitative and the empirical-quantitative approach."[2] Considering the date of his first book, we must look upon him as the most important of the pioneers

[1] See Max Sasuly, "Irving Fisher and Social Science," ECONOMETRICA, Vol. 15, October, 1947, pp. 255–278. For other appraisals of the man and his work and for the external facts of his career, the reader is in addition referred to the "Memorials" by Professors R. B. Westerfield and P. H. Douglas published in the *American Economic Review*, Vol. 37, September, 1947, pp. 656–663.

[2] Section 1 of the constitution of the Econometric Society.

of econometrics since William Petty. It is this which I should answer were I asked to press into a single sentence the reasons I have for applying the epithet "great" so unhesitatingly to his work. Substantially, this work is contained within the covers of six books, the *Mathematical Investigations, Appreciation and Interest, Capital and Income, The Theory of Interest, The Purchasing Power of Money,* and *Booms and Depressions.*[3]

II

I am sure that Ragnar Frisch surprised his audience when, at the American Statistical Association's testimonial dinner to Irving Fisher, he described the *Mathematical Investigations* as a work of "monumental importance."[4] For although the reprint of 1926 and other circumstances have prevented this work from vanishing from the list of great performances, full justice has never been done to it by the economic profession at large. Usually, even competent theorists see Fisher's chief merit in having presented, as early as 1892, a succinct and elegant version of Walras's theory of value and price and in having illustrated it by means of ingenious mechanical models. It is therefore necessary to remind the reader of what the book's contribution really consisted in.

Before trying to define this contribution, we must attend to another duty. This is the place to do justice to Fisher personally. For this purpose, we must not confine ourselves to those points in his work that were *objectively* novel but we must take account also of all that was *subjectively* novel in it, that is, of all that he found out himself in ignorance of other work that anticipated his. We do this in other cases—e. g., in the cases of Ricardo or Marshall—and it is only by so doing that we may hope to get a true conception of the intellectual stature of some of the greatest figures of our science. Applying this principle to Fisher's *Mathematical Investigations,* we discover that the usual evaluation is inadequate even so far as it goes. In a history of analytic economics, no name other than Walras' should be associated with the equations of general equilibrium. But for our purpose it is pertinent to recall Fisher's

[3] *Mathematical Investigations in the Theory of Value and Prices* (his Ph.D. thesis; 1892, reprint 1926); "Appreciation and Interest," *Publications of the American Economic Association,* Third Series, Vol. XI, No. 4, August, 1896; *The Nature of Capital and Income* (1906); *The Rate of Interest* (1907), here considered in its later form, *The Theory of Interest* (1930); *The Purchasing Power of Money* (1911, revised ed., with H. G. Brown, 1913); *Booms and Depressions* (1932). We shall not consider books addressed to the general public (notably, *The Money Illusion,* 1928; *Stable Money,* 1934; and *100 Percent Money* (1935); or such pedagogical masterpieces as his *Brief Introduction to the Infinitesimal Calculus* and his *Elementary Principles of Economics.* But a few out of a great number of papers will be mentioned as occasions arise.

[4] See ECONOMETRICA, Vol. 15, April, 1947, p. 72.

statement (Preface of 1892) that he found the equations of Chapter IV, §10—which do not give the whole of the Walrasian system but do give its core—in 1890 "when he had read no mathematical economist except Jevons." Moreover, it was "three days after Part II was finished" that he "received and saw for the first time Professor Edgeworth's Mathematical Psychics," and, though the indifference varieties, preference directions, etc. rightly stand in Edgeworth's name and in nobody's else, we also have a right to recall this statement of Fisher when trying to form an idea of the mental powers of our departed friend. He had Jevons' work and that of Auspitz and Lieben to start from and to help him. But *subjectively* he did much more than reformulate, simplify, and illustrate Walras.

Wholly his own, however, was his performance in the field of what, for want of a better expression, I must call utility theory—unless the reader will allow me to use my own term, Economic Potential. I find it extraordinarily difficult to say what I want to say about this performance and not only for lack of space. The present state in that field renders it all but impossible to state my remarks so as to avoid misunderstanding. Above all, Fisher's contribution was curiously Janus-faced. Let us look at the two faces separately.

The one reminds us of Pareto. Eight years (at least) before the latter's renunciation of utility as a psychic entity (not to say quantity), Fisher, in Part II of the *Mathematical Investigations*, anticipated in substance the line of argument that then runs on from Pareto to Barone, Johnson, Slutsky, Allen and Hicks, Georgescu, and finally to Samuelson. Both Jevons' final utility and Edgeworth's indifference varieties were foisted upon Bentham's (or Beccaria's) calculus of pleasure and pain, and Edgeworth had gone out of his way not only to do obeisance to Utilitarianism but also to emphasize this lineage by introducing Fechner's "just perceivable increments of pleasure." Fisher felt that "utility must be capable of a definition which shall connect it with its positive or objective commodity relations" (Preface, p. vi). But in Part II he went further than this. After exploring trails that open up so soon as the utility of each commodity is treated as a function of the quantities of all commodities, he ended up with results (incompletely restated in §8 of Chapter IV) that go far towards the suggestion to do without any kind of utility at all: what is left is a concept that lacks any psychological connotation and contains the germs of all the pieces of apparatus that were to emerge in Pareto's wake. Though Fisher did not use the term, he really was the ancestor of the logic of choice. Even details—such as the question of integrability—that were to play a role in later discussions, are to be found in these pages.

But there is the other face which reminds us of Frisch. Before taking the road at the logical endpoint of which lies Samuelson's consistency postulate or, as some might put it, the proof that utility is both an inadmissible and a redundant construct, Fisher, with unsurpassable simplicity and brilliance, supplied the theory of the measurement of this nonexistent and superfluous thing by defining its unit ("util") under the restriction that the utility of any one or at least of one commodity depends on its own quantity only and is independent of the quantities of other commodities.[5] This restriction may be inadmissible. The defects of the method indicated may be as numerous as were the defects of Columbus' flagship if judged by comparison with a modern liner. Nevertheless, it was one of the greatest performances of nascent econometrics. I hope that the readers of ECONOMETRICA are familiar with the further developments that are mainly associated with the name of Frisch. But I wish to return to the question: how was it possible for a man who was able to write Part II of *Mathematical Investigations* to conceive of measuring marginal utility as a justifiable goal of econometric research? Did he turn out the concept by one door—as he undoubtedly did in Part II—only in order to let it in by another? The answer seems to be this.[6] Actually, he turned out psychological utility completely—also in Part I—without ever letting it in again although, just like Pareto, he did retain turns of phrase that tend to obliterate this. But, unlike Pareto, he realized that a meaningful problem of measurement occurs also within the logic of choice or, to put it differently, that cardinal utility and psychological utility are not as

[5] The reader knows how Fisher followed this up by the most striking of all his pedagogical masterpieces, the paper on "Measuring Marginal Utility" that he contributed to *Economic Essays in Honor of John B. Clark*, 1927. The method for carrying out actual measurements may not be statistically satisfactory. But it illustrates the idea to perfection and it also does something else: it indicates a possibility of relaxing on the condition of independence, a possibility that was developed in another connection, by A. Wald ("The Approximate Determination of Indifference Surfaces by Means of Engel Curves," ECONOMETRICA, Vol. 8, April, 1940 pp. 97–116). On the relation of Frisch's well-known work to Fisher's, see the former's "Introduction" to his *New Methods of Measuring Marginal Utility*, 1932.

[6] It is in part supplied by Frisch's axiomatics in "Sur un Problème d'Economie Pure" (*Norsk Matematisk Forenings Skrifter* 16, 1926) which goes far beyond Fisher. It is curious, however, that neither Fisher nor Frisch went further into a matter in which both were evidently deeply interested. Fisher, in particular, considering his partiality for mechanical analogies, might have been expected to grapple, however tentatively, with the problems that arise from the fact that the relations that enter any satisfactory theory of utility, in addition to being nonholonom (containing equations between differentials of the commodity coordinates that need not be integrable: this Fisher was the first to point out) are sure to be rheonom (to contain time explicitly).

closely wedded as most of us seem still to believe. We may wish to measure heat without wishing—or being able—to measure the sensation of heat. I am aware, of course, that the whole idea is under a cloud just now and that hardly anyone is interested in it. But it will come back.

III

The Walrasian system presents behavior (or maximizing) equations that embody theorems of the logic of choice, the choices being made subject to restrictions part of which enter into the behavior equations and another part of which is contained in the system's balance equations. This system is very general and admits of different interpretations, in other words, may be made to produce different "theories" according to the manner in which we conceptualize the phenomena of which it is to serve as a model. In order to have a unique meaning[7] it must, therefore, be supplemented by something which is, in the strict logical sense, nothing more than a semantic code but which, for the economist, involves his whole vision of the structure of the economic universe that he is to analyze, and prejudges many of the results that will emerge from his analysis. But concepts imply *relations* and since theory, so far as it consists in setting up rational schemata, is essentially a theory of an economic calculus, we may, instead of saying that the Walrasian system presupposes the solution of a problem of conceptualization, also say that it presupposes a schema of economic accounting. We know from experience, old and recent, that this conceptualization or schema of accounting centers in the themes of capital values and income values. This is why Walras included in his *Eléments d'économie politique pure* a few paragraphs that might have been entitled: elementary principles of accounting. And this is also why Irving Fisher supplemented the *Investigations* by a volume on the *Nature of Capital and Income*. So far as I can make out, this volume too was only moderately successful. Most people saw nothing in it but a continuation of the time-honored discussion of those two concepts of which they had every right to be tired. A few, Pareto among them, admired it greatly, however.[8]

[7] This uniqueness of meaning has, of course, nothing to do with the uniqueness of the set of values that satisfies it, i.e., with the question whether or not the system is uniquely determined that has attracted so much attention of late. The theorists of Fisher's formative age, and he himself, were in the habit of taking this latter question rather lightly. Still less than about the question of the existence of a unique set of solutions, they bothered about the question whether there is in the system a tendency to evolve toward this set, if it exist.

[8] I do not know whether Pareto ever expressed his high opinion of the book in print. But he did express it in conversation.

In the first place, Fisher accomplished a task that was long overdue. I do not know whether others are as impressed as I am by the historical fact that economists habitually neglect to avail themselves of obvious opportunities and to take the obvious line. The fate of D. Bernoulli's suggestive tract is a case in point. Economists' failure to join forces with engineers is another. But nothing is more illustrative of that attitude than is the neglect by 19th-century economists of the opportunities to learn from accounting and actuarial practice and in turn to try to rationalize it from the standpoint of economic theory. Attempts to do both are of comparatively recent origin and the more important of them, though no doubt subconsciously, follow Fisher's example. The response from accountants was only in part favorable, Professor Canning's work being the outstanding instance. Others criticized. But never mind. The essential thing is that Fisher broke the ice.

In the second place, Fisher's performance in this field may be likened to his performance in the field of index-number theory. When he entered the latter, about a century and a half had elapsed since Carli or nearly two centuries since Fleetwood. A huge amount of work had gone into the subject. Fisher's contribution was systematization on the one hand, and rationalization on the other, i.e., the setting up of a number of criteria that index numbers ought to satisfy. He proceeded similarly in matters of capital and income. Proceeding from the purposes these concepts were actually intended to serve, he deduced rationally a set of definitions of Wealth, Property, Services, Capital, Income that was new by virtue of the very fact that it fitted a rational schema. The result was not to everyone's taste. Again it is the exemplary procedure which matters and which, among other things, produced the modern emphasis upon the distinction of funds and flows. It also produced the definition: earned income = realized income less depreciation, or plus appreciation of capital (p. 238) which, each term taken in Fisher's sense, is associated with the much-discussed proposition that savings are no proper object of income taxation or that the taxation of savings spells double taxation.[9]

[9] We shall not expect that a conceptual arrangement that yields so unpopular a result commended itself to economists. All the more important is it to emphasize that Fisher made a strong case for it (see especially Chapter XIV, §10). Also, the unpopular result is inescapable if we accept his psychic-income concept (the idea of which and term for which are due to F. A. Fetter), and Fisher has invariably won out, by virtue of his impeccable logic, in the controversies that arose on the subject. But it is for me a source of wonder how he can have believed—as he evidently did—that this logic would convert anyone who wishes to see savings taxed or be needed by anyone who does not. Views on taxation are ideological rationalizations of interests and resentments, and even if they were more than that, we should certainly make the question of whether or not to

In the third place, the work cleared the ground for advance upon the theory of interest. The principle involved is, of course, Böhm-Bawerk's or, if you prefer, Jevons'. But one needs only to observe, and to purify analytically, the discounting processes of business practice in order to arrive at the conception of the relation between capital and income values that the book elaborates. This relation in turn suggests the idea that interest is not a return to a particular class of means of production but the result of that discounting process which is applicable—as a matter of logical principle—to all. That, e. g., the "rent of land" should not be coordinated with "interest of capital" had been seen though it had not been stated in so many words by Marshall whose concept of quasi rent points in this direction. It had been stated explicitly by Fetter. But it was Fisher who carried out all implications and erected on this basis a structure of his own.

IV

Thus, as *The Nature of Capital and Income* was, in a sense, a companion volume to the *Investigations*, so *The Rate of Interest* (1907) was the outcome of both and, of course, of *Appreciation and Interest*. In its revised form to which alone the following comments refer[10]—published under the title of *The Theory of Interest* in 1930—the book is a wonderful performance, the peak achievement, so far as perfection within its own frame is concerned, of the literature of interest.[11] First, but much the least, the work is a pedagogical masterpiece. It teaches us, as does no other work I know, how to satisfy the requirements of both the specialist and the general reader without banishing mathematics to footnotes or appendices, and how to lead on the layman from firmly

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tax savings dependent upon considerations (such as remedial effects of taxation of savings in a depression and remedial effects of an exemption of savings in an inflation) other than the logical implications of a definition. I mention this because belief in reason—formal logic even—was so characteristic of this modern Parsifal. This bent of his mind, together with his habit of taking slogans, programs, policies, institutions (such as the League of Nations) at face value, made him, perhaps a bad adviser in the nation's or the world's affairs. But it also made him still more lovable than a more wordly Fisher would have been.

[10] This is not to say that the specialist can dispense with the older book altogether. The sketch of the history of the theory of appreciation and interest in the appendix to Chapter V of *The Rate of Interest*, and the appendix to section 3 of this chapter, for instance, are left out in the later work.

[11] The reader will understand and appreciate it, if, throughout the section, I speak from the standpoint of the body of thought that culminates in Fisher's masterpiece, and if I refrain from saying what might be said against it from my own standpoint. In return, so I hope, the reader will do me the favor of not interpreting what he will read as a disavowal of what I have myself written on the subject.

laid foundations to the most important results by judicious summaries and telling illustrations. Second, the work is explicitly econometric in parts. The difference this makes can be made to stand out by comparing it to any other work on the theory of interest. Third and above all, the work is an almost complete theory of the capitalist process as a whole, with all the interdependences displayed that exist between the rate of interest and all the other elements of the economic system. And yet this interplay of innumerable factors is powerfully marshalled around two pillars of explanation: Impatience (time discount) and Investment Opportunity (marginal rate of return over cost).[12] The book is dedicated "to the Memory of John Rae and of Eugen von Böhm-Bawerk, who laid the foundations upon which I have endeavored to build." Quite so. But not everyone would have said it. Nor would everyone have disclaimed originality in fundamentals. Let us pause to pay our respects to Fisher's character but at the same time recognize the originality of the structure which he erected on those foundations.

The core of the work is Part III, which carries out, with admirable neatness, the program enshrined in the propositions that the theory of interest is really identical with the whole of the theory of "value and distribution" and that interest is not a separate branch of income in addition to wages, rents, and profits but only an aspect of all income streams. Part II goes over the same ground for the benefit of the non-mathematical reader. Part I links the argument to the conceptual apparatus developed in *Nature of Capital and Income*. Part IV is a receptacle for impedimenta that would have hampered the troops on the march and contains, among other things, the important Chapter XV—which, rather than Chapter XXI, is the real summary of the book's

[12] Lord Keynes stated explicitly (*General Theory*, pp. 140–141) that Fisher "uses his rate of return over cost in the same sense and for precisely the same purpose as I employ the marginal efficiency of capital." I think that this statement should be allowed to stand in spite of the protests of some of Keynes' disciples. More important is it, however, that Keynes *himself* also accepted (*ibid.*, pp. 165–166) the time-discount factor, i. e., the whole of Fisher's theory. The time discount he identified with his own propensity to save (therefore also with his propensity to consume) in nearly the same way in which he identified his marginal efficiency of capital with Fisher's marginal rate of return over cost. Only as an amendment and on the ground that it is "impossible to deduce the rate of interest merely from a knowledge of these two factors" [in the short run?], he introduced *in addition* liquidity preference. In itself, this does not make a great deal of difference. But actually it *was* to make a great deal owing to the increasing emphasis that Keynes and his followers were to put upon this element of the case. It then came to serve the purpose of making the rate of interest a function of the quantity of money, an arrangement that Fisher always repudiated. One reason for this difference is that Fisher's was not an underemployment model.

argument—the strikingly original Chapter XVI—Relation of Discovery and Invention to Interest Rates—in which Fisher broke new ground, and Chapter XIX that presents the result of no less original statistical work as stated already.[13] Splendid wheat, all of this, with very little chaff in between.[14]

Fisher's interest analysis is essentially income analysis in the sense that the principle of choice between alternatively available income streams is made the pivot on which economic analysis in general is to turn. This income analysis is couched in real terms, basically, and treats the monetary element as a vehicle of the shifting of receipts in time rather than under the liquid-asset aspect. Anyone who wishes to do so can, however, insert the latter and for the rest we should be further along if we had chosen Fisher's work for basis of our own. This, however, has not been done to any great extent.

V

A comprehensive system of economic theory, then, had been partly worked out and partly sketched out in *The Rate of Interest.* In particular, all the essentials of a theory of money were there. However, like most great system builders, Fisher felt the impulse of treating the problems of money in all the pomp and circumstance of a central theme. This he did in his *Purchasing Power of Money.* Again, let us first notice the work's most obvious claim to historical importance: it was another of Fisher's great pioneer ventures in econometrics. There was presented his early work in price-index numbers. There appeared his index of the Volume of Trade and other creations that were then novel, among them his ingenious method of estimating the velocity of money.[15] Also, there was an elaborate attempt at statistical verification of results.[16] All these pieces of research are among the classics of early econometrics.

[13] As pioneer work this chapter retains its historical importance irrespective of what we think of its methods in the light of later developments of statistical theory. Moreover, it contains suggestions for the construction of dynamic models (see below, section VI) some of which have not been exploited as yet.

[14] The criticism of Böhm-Bawerk's teaching on the "technical superiority of present goods" in §6 of Chapter XX must, I fear, be classed with the chaff. By that time it should have been clear that, whatever may be said about Böhm-Bawerk's technique, there was no real difference between him and Fisher in fundamentals. Other criticisms, however, e. g., that of waiting considered as a cost (p. 487), constitute brilliant pieces of reasoning.

[15] Fisher's first paper on this subject—which harks back to Petty, but had been taken up again by Kemmerer—appeared in December, 1909 in the *Journal of the Royal Statistical Society.* Kinley's work followed, largely inspired by Fisher's.

[16] Fisher subsequently published estimates of the items that enter the equation of exchange for a number of years.

The really important thing, however, is that the whole argument of the book is geared to the criterion of statistical operationality and that it avoids any concept or proposition that is not amenable to statistical measurement. Once more, for better and for worse, Fisher nailed his flag to the econometric mast.

It is less easy to show that the book is the most important link between the older theories of money and those of today. As was his habit, he made no claims to originality. The book is dedicated to Newcomb, and other predecessors could be readily mentioned. Yet the central chapters, IV, V, and VI, represent a contribution that was more than synthesis. Fisher accepted without question what then was still a new theory of bank credit. He assigned a pivotal role to the lag of the interest rate in the credit cycle. He explicitly recognized the variability of velocity—remember that the postulate of constant velocity used to be considered, and is sometimes considered even now, as the main characteristic as well as blemish of the "old" monetary theories. And he took due account of a host of factors (some of which combined under the label "conditions of production and consumption") that help to determine purchasing power. All this does not amount to a full integration of the theory of money with the theory of prices and distributive shares, still less with the theory of employment. But it constitutes a stepping stone between money and employment.

If that be so, why was it that friends and foes of *The Purchasing Power of Money* saw nothing in it but another presentation, statistically glorified, of the oldest of old quantity theories—that is, a monument of an obsolescent theory that was to become quite obsolete before long? The answer is simple: because Fisher said so himself—already in the Preface and then repeatedly at various strategic points. Nor is this all. He bent his forces to the task of arriving actually at a quantity-theory result, viz., that at least "one of the normal effects" of an increase in the quantity of money is an "exactly proportional increase in the general level of prices." For the sake of this theorem he discarded his recognition of the fact that variations in the quantity of money might ("temporarily") exert an influence upon velocity and reasoned after all on the hypothesis that the latter was an institutional constant. For the same reason he postulated that deposit currency tends to vary proportionately with legal-tender (reserve) money. All the rich variety of factors that do interact in the monetary process was made to disappear—as "indirect" influences—behind the five factors (quantities of basic money and deposits, their two velocities, and volume of trade) to which he reserved the role of "direct influences" upon the price level which thus became the dependent variable in the famous Equation of Exchange. And it was *this* theory which he elaborated with an

unsurpassable wealth of illustrations, whereas he shoved all his really valuable insights mercilessly into Chapters IV, V, VI, and disposed of them semicontemptuously as mere disturbances that occur during "transition periods" when indeed the quantity theory is "not strictly true" (Chapter VIII, §3). In order to get at the core of his performance, one has first to scrap the façade which was what mattered to him and both to his admirers and opponents and on which he had lavished his labors.

But why should he have thus spoiled his work? His own verification though declared satisfactory does not bear out the more rigid of his formulations (see, e. g., the result arrived at for 1896–1909, p. 307 of the revised edition). Several of his own arguments in *The Theory of Interest* and in his writings on business cycles clash with them. It cannot be urged that much of his or any quantity theory can in fact be salvaged by interpreting it strictly as an equilibrium proposition[17]—valid, as it were, for a sort of Marshallian long-run normal. For, on Fisher's own showing, this equilibrium is not arrived at by a mechanism that could be fully understood in terms of his five factors alone. It can only be summed up but it cannot be "causally explained" in terms of these. Moreover, he applied the equation of exchange year by year, hence also to conditions that were certainly far removed from any equilibrium. I cannot help thinking that the scholar was misled by the crusader. He had pinned high hopes to the Compensated Dollar. His reformer's blood was up. His plan of stabilizing purchasing power had to be simple—as were the ideas he was to take up later on, Stamped Money and Hundred Percent—in order to convince a recalcitrant humanity, and so had to be its scientific base. This is enough in order to suggest my own solution for what has always seemed to me an enigma.[18] I have no wish to pursue the subject of economists' crusading any further. Let me, however, ask the reader: in this case at least, if in no other, what did Fisher himself, or economics, or this country, or the world gain by this crusade?

VI

The monetary reformer also stepped in to impair both the scientific and the practical value of Fisher's contributions to business-cycle research. But in themselves they are much more important than most of

[17] In justice to Fisher we must never forget that most of the current objections to it are derived from phenomena that belong to Fisher's transition periods. Also the problem of verification looks somewhat more hopeful if this point be taken into account.

[18] The fact that his was an essentially "mechanistic" mind is also relevant, of course.

us seem to realize.[19] They are, once more, models of econometric research and have perhaps influenced the development of its standard procedure. Fisher's econometrics there took a definitely dynamic turn: the paper of 1925 suggested an explicitly dynamic model (see last footnote), several years before the boom in such models set in. Finally, with admirable intuition, he listed all the more important "starters" of the cyclical movement the modus operandi of which need only be worked out to yield a satisfactory explanatory schema.

But in order to realize this, we must again perform an operation of "scrapping the façade." The "starters" are not where they belong, viz., in the place of honor at the beginning. They are shoved into Chapter IV. On the surface, we have overindebtedness and the process of its deflation, "the root of almost all the evils." Or, in other words, everything is being reduced to a mechanically controllable surface phenomenon with the result that Fisher actually deprecated the use of the term "cycle" as applied to any actual historical event (p. 58). And expansion and contraction of debt, associated as they are with rising and falling price levels, land us again in monetary reform, the subject Fisher was really interested in when he wrote the book. This time the Compensated Dollar, while still recommended, received but modest emphasis. Instead of the vigorous advocacy of this particular plan that we found in *The Purchasing Power of Money*, we find in Part III of *Booms and Depressions* (entitled Factual) a simple and popularly worded survey of means of monetary control in which hardly any economist will find much matter for disagreement and which includes practically all the policies of "reflation" that were either adopted or proposed in the subsequent years. I do not want to belittle the merit or to question the wisdom, of almost everything Fisher wrote there. On the contrary, considering the date of publication, I believe him to be entitled to more credit than he received. But I do wish to emphasize that this was not the only merit of the book and that, though but im-

[19] Fisher's first contributions in this field are to be found in *The Rate of Interest* and *The Purchasing Power of Money*. Then came several important papers, chiefly "The Business Cycle Largely a Dance of the Dollar" (*Journal of the American Statistical Association*, December, 1923) and "Our Unstable Dollar and the So-called Business Cycle" (*ibid.*, June, 1925). I wonder whether I am right in believing that the latter paper was the first economic publication to present a dynamic schema—$T(t+w)=a+m^2P'(t)$—in which fluctuations were shown to result from factors that do not fluctuate themselves ("oscillators"). It was therefore a curious slip when Fisher wrote in 1932 (*Booms and Depressions*, Preface) that the field of business cycles was "one which I had scarcely ever entered before." His name would stand in the history of this field even if he had ceased to write in 1925.

perfectly sketched, something much larger and deeper looms behind the façade.[20]

VII

The *Investigations, Appreciation and Interest, The Nature of Capital and Income, The Theory of Interest, The Purchasing Power of Money, Booms and Depressions,* are the pillars and arches of a temple that was never built. They belong to an imposing structure that the architect never presented as a tectonic unit. From Cantillon through A. Smith, J. S. Mill, and Marshall, leaders of economic thought made their impression, on their epoch and on posterity, by systematic treatises. Fisher never expounded his thought in this way. The busy crusader had no time for it. And nevertheless this would have been the only way to rally his American fellow economists to his teaching. As it was, whatever the reason, he formed no school. He had many pupils but no disciples. In his crusades, he joined forces with many groups and individuals. In his scientific work, he stood almost alone. Thus, he had to do without all the benefits that schools, protecting, interpreting, developing their masters' every word, confer upon their chosen protagonist. There are no Fisherians in the sense in which there have been Ricardians or Marshallians and in which there are Keynesians. Strange as it may seem in the case of a man of such monolithic purity of purpose, of such width of social sympathies, of such unqualified adherence to one of the ruling slogans of his day—stabilization—he always remained outside of the current and always failed to convince either his contemporaries or the rising generations. But those pillars and arches will stand by themselves. They will be visible long after the sands will have smothered much that commands the scene of today.

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[20] This could be established still more convincingly from his paper "The Debt-Deflation Theory of Great Depressions" (ECONOMETRICA, Vol. 1, October, 1933, pp. 337–357). In itself, debt deflation is nothing but a piece of mechanism, the familiar spiral that we all of us understand well enough. If this were all the paper would not be worth noticing. But it is not all. In fact, the theory of the "starters" and its implications stand out much better than they do in the book.