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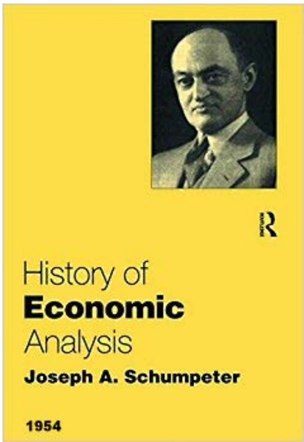

CHAPTER 8
Money, Credit, and Cycles

1. PRACTICAL PROBLEMS

ONCE MORE the bulk of the vast literature on money and related subjects, which the period under survey produced, grew out of the discussions of current problems. It contained, as the literature on money always did and does, a large quantity of completely worthless publications and a still larger quantity of publications which, though more or less meritorious within their range, are uninteresting from the standpoint of a history of analysis. It is nevertheless necessary, recalling what has been said in Chapter 2, section 3, to restate a few of those practical problems that induced discussions of some importance.

(a) *The Gold Standard.*

The literary reflex of the tendency that dominated the monetary policy of the period, the maintenance or adoption of the gold standard, merits more careful analysis than it is possible for us to offer. There were in all countries, among those who discussed actualities of national monetary policy in a practical spirit, very many unconditional 'pro's.' They included, as does every party to every practical controversy, narrow-minded fanatics without a trace of intelligence, but on its higher levels this was a respectable group. I shall mention, by way of example, Bamberger, Giffen, de Parieu, though a dozen other such trios would do just as well.[1]

In view of the superficial sentence that some of us are in the habit of passing on the monetary thought of that time, it should be noticed, first, that the opinions and recommendations of the unconditional 'pro's' were incessantly under fire—so that nothing could be farther from the truth than the idea that the economists of that period as a body worshipped the golden calf—and, second, that these opinions received but qualified support from those leaders of scientific economics *who actually worked in the field*. As we shall see, neither Jevons, nor Walras, nor Marshall, nor Wicksell, nor Wieser, nor Fisher can, without qualification, be called either theoretical or practical gold monometallists. Later on, moreover, the depressions of the eighties and nineties raised the question of gold's responsibility either for falling or for cyclically fluctuating prices. And the emergence of the gold-exchange standard raised the

[1] Ludwig Bamberger (1823–99) was a typical doctrinaire liberal of the German type—a revolutionary in 1848, a staunch enemy of socialism, protection, and even social insurance ever after. As a member of the Reichstag he established himself as its authority on money, and his great aim was to get Germany on the gold standard and to keep her there. He was a violent anti-bimetallist (see subsec. b), disposing of the bimetallist argument by pointing to the silver interests

behind it. But the particular task he manfully strove to accomplish and the particular historical conditions in which this task posited itself to him must be taken into account before we condemn his views on the score of theoretical inadequacy. The more important of his speeches and articles *(Ausgewählte Reden und Aufsätze über Geld- und Bankwesen)* have been edited by K. Helfferich (1900).

Sir Robert Giffen (1837–1910), an economic journalist and civil servant, belongs to that category of meritorious or even eminent economists to whom this book cannot do justice. His *Progress of the Working Classes in the Last Half Century* (1884) and his *Growth of Capital* (1889) are landmarks in the history of economic statistics. Here we have to notice his valiant defense of the gold standard (*Case against Bimetallism,* 1892; *Evidence before the Royal Commission on Gold and Silver,* 1886–8) and his almost ferocious hatred of Fancy (i.e. non-gold) Monetary Standards.

F.E.de Parieu (1815–93) was by far the most important of the three. A public man—half politician, half civil servant—he specialized in the fields of taxation (income tax and related matters) and monetary policy. From 1857 on, perceiving the ineluctable drift of things, he advocated the gold standard—but with due respect to the French silver problems—and international monetary co-operation (see subsec. c below). His work on money is in his various reports. His works on public finance have been noticed already. [J.A.S. intended to but did not do this in the unfinished sec. 6 of ch. 6.]

question of the merits of actual gold circulation to which, as we know, Ricardo had already returned a negative answer.[2]

(b) *Bimetallism.*

This was, throughout that period, the most fertile source of 'practical' controversy. The popular and political literature of the silver men—justice to silver; dollar of our fathers; You shall not crucify mankind upon a cross of gold—contains many arguments that kept on a much lower level than anything that can be found in the writings of the sponsors of gold. In particular, it is infested by products of a semi-pathological nature, for at that time bimetallism was the chief hunting grounds of monetary monomaniacs. Nevertheless, it is the fact—a fact that these semi-pathological products and also the victory of the gold party tend to obliterate—that, on its highest level, the bimetallist argument really had the better of the controversy, even apart from the support that a number of men of scientific standing extended to the cause of bimetallism.[3]

(c) *International Monetary Co-operation.*

The various international monetary unions and conventions, such as the Latin Union, the Scandinavian Union, the German Union (before the foundation of the empire), naturally suggested more comprehensive schemes. On the initiative of France, an international currency conference was held in Paris, 1867, that under the leadership of de Parieu succeeded to a surprising extent in keeping clear of the bimetallist hornets' nest, considered the question of a uniform world coinage of gold, and adopted what were so far the boldest proposals ever made for a world-wide monetary union. But at the subsequent international conferences of 1878, 1881, and 1892, pressure by the United States diverted discussion and proposals to bimetallism and thereby killed the original idea.[4] However, at the conference of 1892, the German economist, Julius Wolf, proffered a new idea, namely,

[2] The gold-exchange standard was essentially a practitioner's idea. Scientific analysis had little if anything to do with the 'discovery.' There are, however, a number of critical interpretations of the exchange standard by scientific economists of which it must suffice to mention: L.von Mises, 'The Foreign-Exchange Policy of the Austro-Hungarian Bank,' *Economic Journal*, June 1909; J.M.Keynes, *Indian Currency and Finance* (1913); Fritz Machlup, *Die Goldkernwährung* (1925); C.A.Conant, 'The Gold-Exchange Standard,' *Economic Journal*, June 1909; and a series of important papers and reports by E.W.Kemmerer, see, e.g., his analysis of the case of the Straits Settlements in *Political Science Quarterly*, XIX and XXI (December 1904 and December 1906).

[3] It is, however, quite impossible to sample that torrent of publications. Instead, I shall mention two works of undoubted scientific standing that may serve as an introduction to the popular literature also: J.S.Nicholson, *Treatise on Money and Essays on Monetary Problems* (1888), and F.A.Walker, *International Bimetallism* (1896). There was a Bimetallic League whose many publications are recommended to readers desirous of going further into the subject. Additional material is to be found in the reports and other writings of S.Dana Horton, next to Walker the leading American advocate of international bimetallism. The outstanding purely analytic performance on bimetallism is that of Walras (*Éléments, leçons* 31 and 32).

[4] On these conferences, whose reports contain many contributions of analytical merit, see H.B.Russell, *International Monetary Conferences* (1898).

that an international gold reserve be deposited in a neutral country and that international banknotes be issued on the basis of this reserve—the idea that, though in an entirely different form, was to be partly realized by the International Fund of Bretton Woods fame.

(d) *Stabilization and Monetary Management.*

The chief appeal of the bimetallist argument, at least for people not directly interested in silver production, was of course in the prospect it held out of *rising* prices. Officially, how ever, bimetallists preferred to speak of *stabilizing* the price level. But other schemes of stabilization, unconnected with silver, were also produced, for example, schemes that proposed to divorce circulation entirely from gold and to use paper money. And though, during three decades of falling prices, it was primarily the price level people thought of stabilizing (as always, there was intentional or unintentional confusion of this aim with the aim of keeping up individual prices, especially those of agricultural products), broader aims were by no means absent. Even mere stabilization of prices implies—as its main purely economic motive—concern with stabilization of a country's economic situation. But stabilization of employment was often mentioned explicitly. Further, especially in connection with discussions of the gold-exchange standard, there was much talk about stabilizing money rates.[5]

All this already meant monetary management of one kind or another. For instance, bimetallism spells management whenever, in order to make it work, it is necessary to regulate the price of silver—that is to say, to peg it by purchases in order to keep silver from driving gold out of circulation—for in this case the monetary system no longer works automatically. All schemes that

[5] The 'comedy of errors' present in almost any discussion of economic policy may be instructively illustrated by one particular instance pertaining to that range of problems. When Austria, in the nineties, adopted the gold-exchange standard, it was urged by politicians and in the press that one

of the advantages of this arrangement would be to secure lower interest rates than would prevail in the case of a fullfledged gold currency. Truth and error in this should be easy to disentangle. A central bank that is to keep exchanges within gold points must, in the long run, do pretty much all that a central bank does under the fullfledged gold standard, and refrain from doing what such a bank must not do. Therefore, interest rates in a money market that works under the gold-exchange standard cannot be normally lower than they would be in a money market that works under a fullfledged gold standard. But, first, the total amount of gold necessary in order to start a gold-exchange-standard system is smaller than the total amount of gold that is necessary to start a system with actual gold circulation. Hence money rates in the initial period need not be kept on so high a level for so long in the former case as would be necessary in the latter case. Second, with the central bank in control of the whole of a nation's monetary gold stock, it is easier in the former case to avoid the necessity of varying bank rate in passing spells of difficulty than it is in the latter. However, politicians and the daily press claimed that interest rates would normally be lower with a gold-exchange standard than they would be with a fullfledged gold standard. And in their zeal to refute this erroneous proposition, professional economists usually failed to admit the two true ones—so that, as so often happens in our field, both parties to the controversy were, in effect, right and wrong at the same time.

went further than this involved, of course, still more management. As an example, I shall mention a proposal that commanded some support: the proposal of an inconvertible paper currency to be regulated by a government department that was to buy government bonds for this currency—to increase liquidity—whenever the price level fell, and to sell government bonds—to decrease liquidity—for this currency whenever the price level rose. This proposal may be considered as one of the many precursors of the open-market operations of the Federal Reserve System. But the idea of open-market operations was familiar in other forms also. For monetary management was not confined to management of the currency. It extended to management of the foreign exchanges and, more important, of bank credit.[6] Nor did it remain in the realm of 'plans.' It was increasingly practiced by all the great central banks.[7] And it is not true that monetary management of this and other types knew no other purpose than to safeguard a nation's gold stock. It was practiced for therapeutic purposes. These purposes differed from ours and the full-employment purpose was not the dominating one. But it is as misleading to overstress the importance that was then attached to playing the gold standard game for its own sake as it is to speak of the monetary systems prior to 1914 as 'automatic.'[8] Unless this be clearly understood, it is impossible to appreciate the doctrinal developments of that age either in themselves or in their relation to the thought of our own time.

For the rest we must be content to notice a few of the performances, in the field of 'monetary reform,' of the scientific leaders. Jevons sketched out what seemed to him 'An Ideally Perfect System of Currency'[9] in which gold, while retained as means of exchange and common denominator of values, was to cease to be the standard for deferred payments, 'the amounts of debts, although expressed in gold, being varied inversely, as gold varies in terms of other commodities.' This revived the 'tabular-standard plan' of Lowe (see above, Part III, ch. 7, sec. 3) and is also the keynote of Marshall's suggestions.[10] The

[6] Thus, the issue of control of credit *vs.* control of money, which carries over into more recent times, was already discussed.
[7] For England, in particular, see W.T.C.King, *History of the London Discount Market* (1936).

[8] They looked more automatic than they were because they functioned so smoothly. Moreover, if the Bank of England seems (statistically) to have reacted, in its discount policy, mainly to the inflow or outflow of gold, it must not be forgotten that, in the conditions that prevailed roughly until 1900, reacting to the inflow and outflow of gold involved essentially the same behavior as would have reacting to the domestic business situation, in nine cases out of ten. When this ceased to be so, central banks increasingly resorted to 'gold devices,' i.e. increasingly abandoned the orthodox gold standard game.

[9] Written about 1875 but first published in his important *Investigations in Currency and Finance,* posthumously edited by Mrs. Jevons and Professor Foxwell in 1884. Attention is called to Foxwell's Introduction.

[10] In order to save space, I neglect the other features of Jevons' scheme, which are in the direction of an international note issue and clearing system based upon gold. Marshall's exploits in the role (as he styled it) of 'amateur currency-mediciner' saw the

latter include, however, a novel idea. Adopting Ricardo's ingot plan, he proposed that these ingots should consist of both gold and silver and that silver bars of a certain weight should be legally 'wedded' to gold bars of a certain weight so that the monetary unit would constitute a claim to quantities of both gold and silver in fixed proportion (Symmetallism). Irving Fisher's proposal,[11] the Compensated Dollar, combined adoption of the gold-exchange standard with the device of varying the gold content of the monetary unit according to the variations of an official price index so that a dollar should represent, instead of a constant quantity of gold, a constant quantity of purchasing power. Finally, Walras advocated a plan that linked up with actual practice in France in a manner that was as ingenious as it was simple. Gold was to remain the standard monetary metal and to be coined for private account without limit. Silver was to be the material of token coins *(billon)* which, however, were not only to provide small change *(billon divisionnaire) but also a type of legal-tender money that was to be used for the purpose of controlling the price level (billon régulateur):* government was to expand its circulation when prices were falling and to contract its issue when prices were rising. The modern ring of this proposal needs no emphasis. Walras added another, which makes him one of the precursors of our own '100 per cent plans.' He recognized, though only in the case of banknotes, the fact that banks create means of payment or, as he put it, that banks can lend to entrepreneurs without borrowing the same amount from capitalists (savers). But he disapproved of it. And he proposed that the silver surplus be used in order to coin additional silver tokens in the amount of banknotes outstanding—minus the amount of legal-tender cash held by the issuing banks—and to suppress the latter.[12]

The merits or demerits of these plans are not in question here. They have been mentioned for two reasons: first, because they show how utterly unfounded is the belief that scientific leaders did not attend to problems of monetary reform until our own day; second, because all those plans rested upon a basis of analytic work, the fundamental importance of which must be recognized quite independently of whether or not we like the plans themselves.

light in a paper he read at the Industrial Remuneration Conference in 1885, significantly entitled 'How far do remediable causes influence prejudicially (a) the continuity of employment, (b) the rates of wages?' (see Keynes's biography of Marshall, *Essays in Biography*, p. 204); in his evidences before the Royal Commission on the Depression of Trade and Industry (1886), before the Gold and Silver Commission (1887–8), and before the Indian Currency Committee (1899), published in *Official Papers* (1926); and in his article 'Remedies for Fluctuations of General Prices' (*Contemporary Review,* March 1887). See also F.Y.Edgeworth, 'Thoughts on Monetary Reform,' *Economic Journal,* September 1895.

[11] See Irving Fisher, assisted by Harry G.Brown, *The Purchasing Power of Money* (1st ed., 1911).

[12] *Études d'économie politique appliquée,* I and V.

2. ANALYTIC WORK

The story of the period's purely analytic work—to which henceforth we shall confine our attention almost exclusively—is a story of successful advance.[1] Though, as we have just seen, most of the leaders participated with zest in the discussions on the practical problems of their day, their work was less dependent upon this stimulus than had been the work of their predecessors: more than before analysis forged ahead, as it were, under its own steam, and the purely scientific filiation of ideas—doctrinal change that is not simply reaction to changing facts and changing political humors—is more in evidence than it was in the preceding period. And more than in other parts of economics new and valuable methods and results grew out of the pre-existing stock of knowledge: in 'general theory' it is possible, if we so choose, to speak of revolution; in monetary theory there was only vigorous evolution. No break occurred with the work that J.S.Mill had thrown into an imperfectly systematic form. Yet most of the ground on which the structure of monetary analysis stands today was actually conquered.

The general picture I am about to present suffers from the impossibility of giving an account, except on rare occasions, of the factual work of that period which is at least as important for our own as are the 'theories.' But all that can be done in a sketch like this is to mention types and give one or two examples of each. There are, first, some really excellent official reports: besides the English ones, which as usual hold first place, I will again refer to those of the international monetary conferences and of the U.S. National Monetary Commission (1911–12). Second, there are the histories of currencies and of banking—such as W.A.Shaw's *History of Currency,* 1252–1894 (1895) or W.G.Sumner's classic, *A History of American Currency* (1874). Third, the period produced repertoires of materials that are still of value—Adolf Soetbeer's (1814–92) *Materialien zur Erläuterung und Beurteilung der wirtschaftlichen Edelmetallverhältnisse* (1885; English trans. from 2nd ed., 1887, the seventh part of which contains his famous Table of Prices) is the outstanding performance of this genus. A fourth type is exemplified by Sir R.H.Inglis Palgrave's statistical work on central banks, especially the Bank of England (most of it summed up in his *Bank Rate and the Money Market,* 1903, which is a masterpiece of the art of making figures speak): it is very difficult to formulate particular results but he who peruses this book page by page suddenly discovers that he understands its subject. Fifth, we should note the infiltration of modern statistical methods into the field—the earliest example known to me being J.P.Norton's *Statistical Studies in the New York Money Market* (1902).

[1] Four references will suffice: Professor Marget's work (*Theory of Prices,* 1938–42), though not primarily written from the historical point of view, is yet by far the best guide to the history of monetary analysis during that period; Professor Rist's *History of Monetary and Credit Theory*

(English trans., 1940) must also be mentioned again? Professor Howard Ellis' *German Monetary Theory*, 1905–1933 (1934; together with authorities there quoted or mentioned in the Bibliography) presents an exhaustive treatment of the work within its field; V.F.Wagner's *Geschichte der Kredittheorien* (1937) usefully supplements Professor Rist's work.

Why is it, then, that the work of that period is sometimes referred to so slightingly and that many of us construct an entirely unrealistic cleavage between it and our own? One answer is precisely that the evolutionary quality of those new methods and results make them look like mere reformulations of old stuff. But there is another answer, one that is highly interesting for the student of the mechanisms of scientific 'progress.' That period failed to develop and systematize its conquests in a form readily accessible to all economists, with all implications and applications nicely worked out and displayed on a silver platter. These conquests therefore did not penetrate into the common run of literature, especially into the textbooks, so that derogatory criticism, while it arouses just indignation in scholars like Professor Marget, is at the same time in a position to justify itself by quotations from the common run—even from such well-known, successful, and (in their way) meritorious books as Karl Helfferich's *Das Geld* (1903), or J.L.Laughlin's *Principles of Money* (1903), or Horace White's popular *Money and Banking* (1st ed., 1895; 5th ed., 1914), or David Kinley's *Money* (1904), or Alfred de Foville's *La Monnaie* (1907). Even Adolf Wagner's *Sozialökonomische Theorie des Geldes* (1909), which takes a higher flight and contains several original points, is not in much better case, and Karl Knies's *Geld und Credit* (1873–9), important though it is in other respects, added but little to the topics covered by its title.

In conscience, we must, however, mention at least a few more of those textbooks that stand out from the rest for one reason or another: Jevons' *Money and the Mechanism of Exchange* (1875), which ran into many editions—a charming book in which rather trite elements are sometimes glorified by original sparks; J.Shield Nicholson's *Treatise on Money and Essays on Monetary Problems* (1888)—a work that has never got its due; F.A.Walker's famous textbook, *Money* (1878), perhaps the best means to familiarize oneself with the current doctrine of those times at its best; Tullio Martello's *La Moneta* (1883), the value of which is but slightly impaired by some liberalist vagaries on free coinage; A.Messedaglia's *La Moneta*…(1882–3), one of the best performances of the scientific literature on money that preceded the Walras-Marshall-Wicksell-Fisher achievements. In addition, the parts, books, or chapters on money of the general treatises—such as Pierson's, or Divisia's, or Colson's—ought to be mentioned.[2] But we must confine ourselves to the Third Book of G.Cassel's *Theoretische Sozialökonomie* (1918, 4th ed. rev. 1927; English trans., 1923, new ed., 1932). This work deserves to be singled out because it presents, with a clearness that does not admit of doubt, an instance of the view that the fundamental logic of the economic process is entirely independent of the monetary phenomenon, the theory of which fundamentally consists merely in the theory of the price level—by which relative prices (exchange ratios) are turned into

[2] On Pierson, Divisia, and Colson, see above, ch. 5.

absolute money prices on quantity-theory lines—and therefore *really* and not only *apparently* stands outside the body of general economic theory. In this respect, Cassel entirely missed the import of Walras' message, which in other respects he followed so closely. But if we take his treatment as an outstanding instance of what is indeed a completely antiquated view of the matter, we must add that he represents this view extremely effectively and that his treatment therefore retains importance. Nor is this importance merely historical. We may well use Cassel whenever we wish to find out what our own advance really amounts to.A brief description of the nature and fate of the chief analytic performances of the period will explain this paradoxical state of things.

(a) *Walras.*

First, by far the greatest of those performances was that of Walras.[3] In the same sense in which it is true to say that he created economic statics, the modern theory of economic equilibrium, it is also true to say that he created the modern theory of money. In fact, his theory of money and credit is simply part of this general theory of economic equilibrium. He therefore substantially fulfilled the great desideratum which has been so much stressed during the last twenty years, namely, the desideratum that the analysis of money should be built into the system of general theory instead of being developed independently and then plastered upon it. And, *so far as monetary statics is concerned*, all propositions developed about money and monetary processes are either contained in his system or may be derived from it by introducing additional assumptions. Thus, as has been shown by Lange,[4] the Keynesian analysis of the *General Theory* (not of the *Treatise* of 1930) is but a special case of the genuinely general theory of Walras. But, as we have seen, Walras did not come into his own until the twenties. Such influence as he exerted during the period under discussion was mainly through Wicksell and Pantaleoni. And even these two did not fully appreciate the importance of his work on money. His immediate successor, Pareto, was altogether blind to it and slid back rather than advanced in this particular field. Two excellent followers Walras did find. But they remained almost completely unknown, Aupetit and Schlesinger.[5]

So far as the period under survey is concerned, the Walrasian theory of money simply did not exist for the overwhelming majority of economists. I take, however, the opportunity to advert to the original work of Del Vecchio, which, in part from Walrasian bases, started in the last years of that period.[6]

Another body of original work on money, related to that of Walras, may be conveniently mentioned here, namely, Irving Fisher's. Most of it came too late to exert influence within the period. And when it did appear, professional attention was too much concentrated on one book, *The Purchasing Power of*

[3] It is only in the 4th ed. of the *Éléments d'économie politique pure* (1900) that we find Walras' pure theory of money fully developed. His slow progress toward this most important piece of monetary analysis covered the years 1876–99, the starting point and the individual steps being reflected in the first three editions and in a number of memoirs on applied problems which eventually went into the *Études d'économie politique appliquée* (see above, ch. 7, sec. 7e).

[4] See O.Lange, 'The Rate of Interest and the Optimum Propensity to Consume,' *Economica*, February 1938.
[5] A.Aupetit, *Essai sur la théarie générale de la monnaie* (1901); Karl Schlesinger, *Theorie der Geld- und Kreditwirtschaft* (1914). These two books, especially the latter, are striking instances of the fact that in our field first-class performance is neither a necessary nor a sufficient condition for success.
[6] Gustavo Del Vecchio, Professor at the University of Bologna, began publishing his important series of papers in 1909. They were summed up in his *Grundlinien der Geldtheorie* (1930) and more completely in his *Ricerche sopra la teoria generale della moneta* (1932).

Money (1911), the success of which obscured the fact that it presented only one aspect—and not the most important one—of its author's monetary theory *as this phrase is understood now*. Ever since the publication of this book Fisher has been classed as a sponsor of a particularly rigid form of quantity theory (see below, sec. 5) and all his other contributions to monetary analysis of the economic process as a whole—monetary analysis in the sense in which Keynes's *General Theory* is monetary analysis—have been neglected. This was and is because he did not call them monetary or income analysis but chose other titles, such as *Theory of Interest* or *Booms and Depressions*. In consequence, his readers never got a full view of his work on money and in particular never noticed the Walrasian streak in it.[7]

(b) *Marshall.*

The second great performance of the last three decades of the nineteenth century was Marshall's.[8] Like Walras, though less explicitly, he saw the monetary problem as part of the general analysis of the economic process and as one of the doors to the theory of employment. More clearly than Walras, though less emphatically than Wicksell, he taught the importance of the distinction between the 'real' and the 'monetary' rate of interest and of attending to the details of the mechanism by which changes in the amount of money act on the economic system. And there were many hints that suggest future developments though only a few of them will be mentioned in this chapter. He held all the elements required for a decisive step forward though he did not himself take this step. Unlike Walras he was indeed in a position of effective leadership. From 1885 on, the whole world's population of economists would have listened had he addressed it. But only glimpses of his views on

[7] Practically all of Professor Fisher's numerous books and papers are relevant for the scholar who may some day attempt the task of co-ordination. I mention here only the most important of those books that have not been mentioned above, ch. 5, sec. 7b. *Appreciation and Interest* (*Publications of the American Economic Association*, August 1896); *The Purchasing Power of Money* (with H.G.Brown, 1911; rev., 1913); *The Money Illusion* (1928); *Booms and Depressions* (1932). But the *Rate of Interest* (1907), fully developed into *The Theory of Interest* (1930), which has been mentioned already, is really still more important for *monetary* theory in the present-day sense. Fisher's work on index numbers will be mentioned later.
[8] Marshall's final presentation of his contributions, to be mentioned presently in the text, was preceded by a number of communications, mainly to official committees of inquiry, that were republished in his *Official Papers* and may be supplemented by a number of passages in the *Memorials*. But the *Principles* also contain important elements of an imposing total. The reader

finds a survey of most of the essential points in Keynes's biographical memoir (*Essays in Biography,* pp. 195–206), but must be warned again that this memoir was written by a (then) fervent disciple. In some points the large claims made by this disciple on behalf of the originality and priority of the master must certainly be discounted. For the rest, Keynes's statement that Marshall developed the whole of his monetary theory during the seventies should be accepted unreservedly—though without prejudice to the claims of Walras and Wicksell. Another point is interesting to note: Marshall's monetary analysis, like his economic analysis in general, clearly started from J.S.Mill's and must be understood as a development of the latter's teaching.

monetary problems were vouchsafed to it until the publication, in his extreme old age, of his *Money, Credit, and Commerce* (1923), when nothing in it seemed novel any more. His Cambridge pupils and other followers of his did listen. As a matter of historical justice, it should be emphasized that, in developing the English monetary theories of our own time, Hawtrey, Lavington, Keynes, Pigou, and Robertson developed Marshallian teaching—though on lines of their own.

It is unnecessary to comment upon works that are in every student's hands. All that is necessary to point out here are the links with Marshall. Professor R.G.Hawtrey should perhaps not be called a pupil in the same sense in which this term applies to the others. But most of the propositions that individuate his teaching—which, as the reader knows, is mainly geared to the problems of business cycles—may be traced to Marshall (and some to Wicksell). The best way of putting it is perhaps to say that Hawtrey's analysis is an original development, in a certain direction, of Marshall's analysis. Of his numerous works, it will suffice to mention here *Good and Bad Trade* (1913), *Currency and Credit* (1st ed., 1919), *The Art of Central Banking* (1932), *Capital and Employment* (1937). Frederick Lavington's works are not so well known as they deserve to be: *The English Capital Market* (1921) and *The Trade Cycle…* (1922). They are unconditionally Marshallian. So is Professor Pigou's article, The Value of Money,' in the *Quarterly Journal of Economics,* November 1917, his chief contribution to monetary theory per se. Other contributions are to be found in his *Industrial Fluctuations* (1927). Of all the rest, I will mention only his monetary analysis of the economic process, *Employment and Equilibrium* (1941). The theoretical skeleton of Lord Keynes's first book, *Indian Currency and Finance* (1913), was also Marshallian, and in his *Tract on Monetary Reform* (1923) he wrote that his 'exposition [of monetary theory] follows the general lines of Prof. Pigou and Dr. Marshall' (p. 85n.), though notes of his own are sounded at critical points. His most ambitious book, *A Treatise on Money* (1930), may be described as a development of (though also away from) Marshallian and Wicksellian lines—the Wicksellian elements were rediscovered, however, not taken from Wicksell. It was only in *The General Theory of Employment, Interest, and Money* (1936) that allegiance to Marshall was formally renounced. This makes it all the more important to note that it was not so much theoretical differences which produced this posthumous break with Marshall as the difference in social vision—in the diagnoses Marshall and Keynes formed about the economic situation of their times. As far as points of theory and not factual assumptions or practical recommendations are concerned, there was one important difference only—about the mechanism of saving and investment—but even this one could have been reduced to a matter of shift of emphasis, had it not been essential for Keynes to divorce himself from what he styled the 'classic theory.' Professor

D.H.Robertson's strikingly original *Banking Policy and the Price Level* (1926) went really further beyond Marshall than any of the works mentioned in this paragraph. If it stood alone, it would not be appropriate to pigeonhole Robertson with the Marshallians. Nor can he be so pigeonholed on the strength of his theory of business cycles. But the rest of his publications on money (including his well-known elementary textbook), the most important of which have been republished in his *Essays in Monetary Theory* (1940) may be said to have grown from Marshallian roots.

But this success of Marshall's teaching on money was to come later, so late that he lost part of the credit for it. Up to 1914, monetary theory outside of Cambridge was practically untouched by Marshallian influence.

(c) *Wicksell.*

The third great performance to be mentioned is that of Wicksell.[9] Posthumously he acquired even greater international reputation as a monetary theorist than either Marshall or Walras. This better fortune is due to the facts that his Swedish disciples never ceased to call themselves Wicksellians, even when they criticized and surpassed him, and that his message became accessible in German at a relatively early date and in a form that was not so forbidding as was that of Walras. But it took him decades to reach the Anglo-American sphere.

Again it is hardly necessary to mention such well-known names as Myrdal, Ohlin, Lindahl, Lundberg. Gunnar Myrdal's *Monetary Equilibrium* (Swedish, 1931; German, 1933; English, 1939), Bertil Ohlin's Swedish essay on the theory of expansion, 'Penningpolitik, offentliga arbeten, subventioner och tullar som medel mot arbetslöshet' published in a report on Monetary Policy to the Swedish Unemployment Commission, 1934), and Erik Lindahl's English summary of his contributions (*Studies in the Theory of Money and Capital,* 1939). Erik Lundberg's *Studies in the Theory of Economic Expansion* (1937) will represent the post-Wicksellian development. It is an interesting fact to note in a history of economic analysis that, until about ten years ago, this development paralleled and in some important points anticipated, the English (Keynesian) one without becoming known to English economists. Some mild protests naturally resulted from this state of things and also some discussions about the differences between, and the relative merits of, the two bodies of thought. See Ohlin's 'Some Notes on the Stockholm Theory of Savings and Investment,' *Economic Journal,* March and June 1937, and the subsequent discussions in the same Journal (see below, Part V, ch. 5). Professor D.Davidson, the contemporary and helpful critic of Wicksell, should not go unmentioned. The reader finds all he ought to know about Davidson's monetary doctrines in the excellent article, The Monetary Doctrines of Professor Davidson,' by Mr. Brinley Thomas (*Economic Journal,* March 1935). In the latter's *Monetary Policy and Crises* (1936) there is a brief but useful sketch of Swedish monetary theory since Wicksell.

(d) *The Austrians.*

In the fourth place, there were the contributions of the Austrian group. They all started from Menger,[10] who did not, however, strike out on a line for himself: his theory, though a masterly performance so far as

[9] Wicksell's chief contributions are in his *Geldzins und Güterpreise* (1898). R.F. Kahn's trans., *Interest and Prices,* with an introduction on the evolution of Wicksell's thought by Professor Ohlin, appeared in 1936, but some of the essential ideas, especially the famous Wicksellian 'cumulative process' *were* presented to the English public in the article on 'The Influence of the Rate of Interest on Prices,' *Economic Journal,* June 1907, and in vol. II of his *Lectures on Political Economy* (Swedish original, 1906; English trans., 1934). Very important, because emphasizing certain points that do not stand out so strongly in those two books is also his (Swedish) article on the obscure point in the theory of money, 'Den dunkla punkten i penningteorien,' *Ekonomisk Tidskrift,* December 1903. As in the case of Marshall, it should be observed that Wicksell started from Mill and that his monetary theory developed from a criticism of the latter and the English authors behind him, Tooke in particular.

[10] See *Collected Works* (4 vols., London School Reprints, 1933–6). Menger's chief pieces on money were the chapter on the theory of money in his *Grundsätze* and the article 'Geld' in the 3rd ed. of the *Handwörterbuch* (1909).

it went, was simply a descendant from Davanzati's. It was Wieser who attempted a new departure.[11] In trying to do justice to it we meet with the same difficulty that confronted us when we were trying to define his place in the history of general theory. Wieser's spacious vision of the monetary phenomenon is not adequately rendered by calling him a sponsor of the 'income-approach'[12] or a sponsor of the consumption standard. It comprised much more than that, in particular the conception of a monetary theory of the economic process as a whole. But he was so deficient in technique and so little able to coin his metal that nothing of this came out as it should have. And so his influence touched only a few individuals. The author of the group's standard work on money, von Mises,[13] who was also its foremost teacher in the field—in fact the founder of a school of his own—was no doubt one of them. But he was only partly in sympathy with Wieser's views.

3. FUNDAMENTALS

(a) *Nature and Functions of Money.*

Discussions on the nature and functions of money and hence on the question of definition were carried on throughout the period. But, with the exception to be noticed under (b), they did not excite much interest and, without any exception, they did not produce very interesting results. I believe that a majority of writers accepted, or would have been willing to accept, Roscher's definition.[1] Menger and his followers did so with particular emphasis—without any intention to commit themselves thereby to all its implications. Others, Americans especially, accepted Walker's neat phrase—'Money is that Money does'—in an equally non-com-

[11] Wieser's ideas on money, like those of Walras, developed when his original work on general theory had been done. His first publication in the field was his inaugural lecture delivered on his appointment to Menger's chair in Vienna ('Der Geldwert und seine geschichtlichen Veränderungen,' *Zeitschrift für Volkswirtschaft, Sozialpolitik und Verwaltung,* 1904). An improved version was presented in an address to the Verein für Sozialpolitik at its Vienna meeting in 1909 and published in the Verein's *Schriften,* vol. 132, and another in the article 'Geld' (Allgemeine Theorie des Geldes) in the 4th ed. of the *Handwörterbuch,* 1927.

[12] On Wieser as a sponsor of the income approach, see below sec. 6b.

[13] Ludwig von Mises, *Theorie des Geldes und der Umlaufsmittel* (1st ed., 1912, 2nd ed., 1924, English trans. under the title, *Theory of Money and Credit,* 1934).

[1] 'The false definitions of money divide up into two main groups: those that consider it to be something more, and those that consider it to be something less, than the most salable commodity' (Roscher, *Grundlagen,* Book II, ch. 3, §116 [trans. by J.A.S.]). As an example of the contrary opinion, I quote Richard (son of the more important Bruno) Hildebrand, *Theorie des Geldes* (1883), where we learn that money, far from being a commodity is 'the very opposite of a commodity.' In *Interest and Prices* Wicksell quoted both these authors. And his comments upon the issue illustrate well how little such general pronouncements really mean to the serious worker. But the contradictions between them help to discredit economics in the eyes of all those laymen and historians who take them too literally and believe that everything else follows from them.

mittal spirit. Most writers distinguished between money or primary money (meaning coin and government fiat, often but not always, also banknotes or at least notes of central banks) and 'credit' or fiduciary money (meaning means of payment arising out of credit transactions), a distinction to which some attached great importance[2] and which, in certain cases to be noticed, was in fact indicative of something more significant than terminological preference. We have seen above that the leading authorities on money were not addicted to any uncritical gold standard fetichism. Where they did stand for the gold standard, as in Italy, there were good and sufficient practical reasons for their doing so. But practically all must be classed as theoretical metallists in our sense of the term.[3] It seems worth our while to advert to the following points.

First, the practice continued to prevail of developing the theory of money from its old four functions: medium of exchange, measure of value, store of value, standard of deferred payments—many authors insisting both on the separability of these functions and on the practical reasons why we actually find them combined. Walras, anticipated of course by all those authors who—like A.Smith and Malthus—had used labor as a standard of value, introduced the useful fashion of keeping distinct the *numéraire*—a commodity whose unit is used in order to express prices and values but whose own value remains unaffected by this role—and *monnaie*—the commodity that actually serves as means of exchange and whose value is consequently affected because its monetary role absorbs part of its supply.

Second, many writers went out of their way to emphasize the store-of-value function of money. This is important because it raises the question how far the economists of that period were aware of the phenomenon that is called Liquidity Preference in the Keynesian economics of our own day. Marshall spoke of a law of hoarding according to which people's demand for gold hoards increases as its value rises (see *Official Papers*, p. 6). Occasionally he seems to have given thought to the fact that people sometimes fail to spend though they have the power to do so.[4] Von Mises noticed in passing that money is sometimes held as an asset *(Vermögensanlage)*. Going further, Kemmerer averred (*Money and Credit Instruments*, p. 20) that 'large sums of money are continually being hoarded' and that 'the proportion of the circulating medium which is hoarded from time to time…varies with all the influences which affect…business confidence.' Moreover, Marshall and others, especially Fisher, were aware of the role that hoarding, in the sense of unwillingness to

[2] See, e.g., Laughlin, op. cit. or Mises, op. cit. In our own time no less an authority than Professor Rist (op. cit.) may be cited in support of the opinion that neglect of that distinction has been the source of many errors, theoretical and practical. But the errors can be avoided even if we include 'credit' with money, and committed if we do not.

[3] Pareto, evidently disgusted by Italian currency troubles, went even so far as to call paper money 'false money' *(moneta falsa)*. Other Italians also, such as Pantaleoni, considered it as a pathological case. Equally strong metallism, though differently motivated, we can find only in Marx.

[4] So already in *Economics of Industry*, see J.M.Keynes, *General Theory*, p. 19n.

spend, plays in the mechanism of depressions. But only outsiders, such as Hobson, attached 'critical importance' to it as a cause of disturbance in general and of unemployment in particular.[5] Since it is this feature that constitutes the theory of Liquidity Preference, we must, I think, credit—or debit—the introduction of the theory to Lord Keynes (see, however, below, sec. 6).

Third, the theory of money of that period was not monetary analysis either in the sense of Becher and Quesnay[6] or in the modern sense; that is to say, it was not the general theory of a monetary economy. We have indeed seen that Walras' theory of money is fully integrated with his general theory of value and distribution. We have noticed and shall notice again other advances in that direction, in particular the one associated with Wicksell's name. On the whole, however, monetary theory remained in one separate compartment and the 'theory of value and distribution' in another. Prices (including rates of income) remained primarily exchange ratios, which money reduces to absolute figures without affecting them in anything except for clothing them with a monetary garb. Or, in other words, the model of the economic process was in all essentials a barter model, the working of which inflations and deflations might disturb but which is logically complete and autonomous. Practically all the most valuable work of the period—so far as it was not concerned with specifically monetary problems—was Real Analysis, even where it expressed its concepts in terms of money.[7]

This situation found expression in the creation of an interesting concept that emerged and vanished with it. If, on the one hand, the facts of value and distribution are logically so independent of money that they can be set forth with only a passing reference to it, but if, on the other hand, it is recognized that money may act as a disturber, then the problem arises of defining how money would have to behave in order to leave the real processes of the barter model uninfluenced. Wicksell was the first to see the problem clearly and to coin the appropriate concept, Neutral Money. In itself, this concept expresses nothing but the established belief in the possibility of pure 'real' analysis. But it also suggests recognition of the fact that money *need* not be neutral. So its creation induced a hunt for the conditions in which money is neutral. And this point eventually led to the discovery that no such conditions can be formulated, that is, that there is no such thing as neutral money or money that is a mere veil spread over the phenomena that really matter—an

[5] J.A.Hobson, *Physiology of Industry*, p. 102, approvingly quoted by Keynes; see preceding footnote.

[6] On Becher and Quesnay in this connection, see above, Part II, ch. 6.

[7] This statement may cause some difficulties for the beginner which an example will remove. Böhm-Bawerk's Fund of Subsistence is a real concept denoting all sorts of consumable goods. Nevertheless, he speaks of it in terms of money. But this does not mean either that he adopts a monetary concept of capital or that he attributes to money any influence on the process he describes. His money—like Ricardo's so far as the general theory of the *Principles* is concerned—is nothing but a homogeneous expression for a medley of quantities of physical goods.

interesting case of a concept's rendering valuable service by proving unworkable.[8]

Fourth, so long and so far as the theory of money actually did dwell in a separate compartment, its central—and practically only—problem was the exchange value or purchasing power of money. In the analytic work of the period this stands out much more clearly than it did before. Hence the popularity of the book title, Money and Prices, which persisted into postwar times.[9] No doubt influenced by the progress of the index-number method, most authors, especially in the United States, did not hesitate to define the value of purchasing power of money as the reciprocal of the price level. The Austrians distrusted index numbers,[10] and felt more theoretical qualms concerning the nature of the value of money.

A brief comment on these qualms seems justified. From the first, the Austrians entertained a wish, not unnatural from their standpoint, to apply their marginal utility theory to the case of money—which both the enemies of this theory and some of its foremost sponsors, Wicksell for instance, declared to be impossible. Now it was easy to apply the marginal utility theory to the significance that individuals attach to their monetary income. Daniel Bernoulli (see above, Part II, ch. 6, sec. 3b) had already done this. But this significance for the individual of a unit of his money income—its subjective exchange value as Menger called it—does not help us at all when we wish to explain the purchasing power or exchange value of money—Menger's objective exchange value of money. For the latter must be known to the individual—the individual must know what his money will buy—before he can put any subjective value upon his money. On the face of it, it is therefore impossible to do in the case of

[8] See J.G.Koopmans, 'Zum Problem des "neutralen" Geldes' in *Beiträge zur Geldtheorie* (1933). The problem in question must, of course, not be confused with such problems as stability of price level or stability of employment and the like. As soon as we hold that a monetary system or policy insures such stability, we admit precisely that it exerts an influence and hence that it is *not* neutral. The outstanding example, next to Wicksell's, of an economist's development from belief in the barter model and the possibility of a neutral money toward the belief that nothing can be averred about economic processes without specific reference to some given behavior of money, is afforded by the series of Professor Pigou's works. The turning point is to be found, I think, in his *Theory of Unemployment* (1933).

[9] A few examples in addition to others mentioned elsewhere: Antonio De Viti de Marco, *Moneta e prezzi* (1885); L.L.Price, *Money and its Relations to Prices* (1896); Richmond Mayo-Smith, 'Money and Prices,' *Political Science Quarterly* (June 1900); E.W.Kemmerer, *Money and Credit Instruments in Their Relation to General Prices* (1907)—a brilliant performance that had the misfortune of being overshadowed by the greater one of Fisher; J.L.Laughlin, *Money and Prices* (1919) and *A New Exposition of Money, Credit, and Prices* (1931); Albert Aftalion, *Monnaie, prix et change* (1927).

[10] They were, of course, not the only ones to do so. An American instance is Laughlin. Generally speaking, index numbers imposed themselves upon the profession as a whole by a slow process of infiltration which wore out opposition rather than convinced it (see below, sec. 4).

money what can be done in every other case, namely, to deduce its exchange value from curves or schedules of marginal utility: to attempt to do so seems to spell circular reasoning. We cannot stay to discuss the efforts of Wieser and especially of Mises to overcome this difficulty or the objections raised against their solution by Anderson.[11] But it should be pointed out that, quite independently of this question, the Austrian way of emphasizing the behavior or decision of individuals and of defining exchange value of money with respect to individual commodities rather than with respect to a price level of one kind or another has its merits, particularly in the analysis of an inflationary process: it tends to replace a simple but inadequate picture by one which is less clear-cut but more realistic and richer in results.

Most economists agreed—or would have agreed if asked—that marginal utility analysis does not apply to the case of the exchange value of money. But the question whether the supply and demand apparatus applies to it was answered affirmatively by most. This was the natural position to take for those who were prepared to treat money like any other commodity, as were the Austrians and E.Cannan. But it is curious that many of those who, by adopting a special formula for money such as the equation of exchange or the cash-balance formula (see below, secs. 5 and 6), testified to their belief that money cannot be so treated, should also have taken that position. In fact, both friends and foes of the 'quantity theory' agreed in describing it as an application of the demand and supply apparatus to the case of money.[12]

[(b) *Knapp's* State Theory of Money.]

In Germany what may be described as a tempest in a teapot was raised by Knapp's *State Theory of Money*.[13] This book presented a theory of money that turns upon the adage: Money is the Creature of Law. Had Knapp merely asserted that the state may declare an object or warrant or ticket or token (bearing a sign) to be lawful money and that a proclamation to this effect or even a proclamation to the effect that a certain pay-token or ticket will be accepted in discharge of taxes must go a long way toward imparting some value to that pay-token or ticket, he would have asserted a truth but a platitudinous one. Had he asserted that such action of the state will *determine* the value of that pay-token or ticket, he would have asserted an interesting but false proposition. But he did neither. He explicitly denied that he was interested in the value of money. His theory was simply a theory of the 'nature' of money considered as the legally valid means of payment. Taken in this sense it was as true and as false as it is to say, for example, that the institution of marriage is a creature of law.

[11] See von Mises, *Theorie des Geldes* (2nd ed., p. 100); B.M.Anderson, *The Value of Money* (1917).
[12] This idea was actually carried out by Professor Pigou in his paper on the 'The Exchange Value of Legal-Tender Money' (see *Essays in Applied Economics*, 1923).
[13] This is the title of the English (abridged) translation (1924) by H.M.Lucas and J.Bonar of G.F.Knapp's *Die Staatliche Theorie des Geldes* (1905). I shall not go into the copious Knapp literature, about which the reader finds more than enough in Professor Ellis' *German Monetary Theory*, 1905–1933 (see above, sec. 2). There he also finds a more generous appraisal of Knapp's performance than I feel able to present.

If this be so, however, how are we to account for the success of the book which, though substantially confined to Germany, was spectacular? An attempt to answer this question might make an interesting study in the social psychology of economic analysis. First, Knapp's exposition was extremely effective. His forceful dogmatism and his original conceptualization of his theory[14] impressed laymen and those economists who were laymen in economic theory. Second, many people and especially politicians at that time welcomed a theory that seemed to offer a basis for the growing popularity of state-managed money—during the First World War it was in fact widely used to 'prove' that the inflation of the currency had nothing to do with soaring prices. Third, in almost complete ignorance of both the literature and the logic of the subject, Knapp believed that his theory offered not only an alternative to theoretical metallism—his pet aversion—but the only possible one and that it alone was capable of explaining why such a thing as paper money can exist at all. *And this absurd claim was widely accepted,* although Knapp entirely failed to work out a non-metallist theory of the value of money.[15] Fourth, leaders such as Wieser and Hawtrey, who were themselves advancing toward such a theory, felt some sympathy for the work that bore a superficial resemblance to their own. He who is interested in the question 'what it is that succeeds and how and why' and who believes that the answer to this question is more revealing than anything else can be of the conditions prevailing in a field of human endeavor will do well to ponder this.

4. THE VALUE OF MONEY: INDEX NUMBER APPROACH

Much more important than the theoretical discussion on the purchasing power of money was its statistical complement: the vigorous developments in the field of price index numbers during that period constitute indeed one of the most significant facts in the entire history of economics and one of the most significant strides toward an economic theory that is to be not only quantitative but also numerical. Index numbers of production followed with a considerable' lag upon those of prices but the foundations for their postwar developments were also laid. And there was a beginning in the construction of wage and employment indices. But precisely because the subject expanded to vast dimensions, no attempt can be made here to survey its growth. I shall merely mention the outstanding efforts at systematization of what was becoming a semi-independent specialty or science, and then offer a few comments

[14] He was a master in the art of coining new concepts and naming them felicitously. It should be observed that the Greek words borrowed for the purpose served very well: the German economists of that time were not as a rule good theorists, but most of them had had a classical education and knew Greek.

[15] To some extent this was done by one of his critics who deserves to be mentioned: Friedrich Bendixen, *Wesen des Geldes* (4th ed., 1926) and numerous other publications.

that may help the reader to link up the subject with the rest of economic analysis and to see its more general bearings.[1]

[(a) *Early Work.*]

Index numbers having attracted the attention of the British Association for the Advancement of Science, Edgeworth, acting as secretary of the committee that was appointed for the study of the subject, wrote his two famous reports (1887 and 1889),[2] remarkable not so much on account of the recommendations proffered as regards practical methods of index making as on account of the comprehensive analysis of meanings and purposes—labor standard, consumption standard, question of all-purpose index, and so on. In 1901, C.M.Walsh published his *Measurement of General Exchange Value,* which also based discussion of statistical technique upon a comprehensive economic theory of index numbers elaborated in his important book, *The Fundamental Problem in Monetary Science* (1903). Next must be mentioned Professor W.C.Mitchell's monograph on wholesale price index numbers, *Index Numbers of Wholesale Prices in the United States and Foreign Countries* (Bulletin 173 of U.S. Bureau of Labor Statistics, 1915, to be used in its revised edition, Bulletin 284, 1921). But the American century in index numbers was to be ushered in by Professor Irving Fisher's monumental work on *The Making of Index Numbers* (1922),[3] the fountainhead of almost all the best later work. But all that can be noticed here of the wealth of its results is this: Fisher analyzed, classified, and 'rectified' existing and possible index number methods by means of certain previously established 'tests'; that is to say, he formulated certain conditions which index numbers ought to satisfy; and ever since most of the theory of index numbers has really been the theory of these tests. This is much more important than is the search for an 'ideal index number' per se, though of course the tests were devised in order to rationalize this search.

[(b) *The Role of the Economic Theorists.*]

The point about index numbers that is most relevant to a history of economic analysis is the dominant role played by economic theorists in their development. On the face of it, index

[1] The reader will find what he needs in the way of background in C.M.Walsh's article on 'Index Numbers' in the *Encyclopaedia of the Social Sciences*. On production indices, see A.F.Burns, 'The Measurement of the Physical Volume of Production,' *Quarterly Journal of Economics,* February 1930. The best reference on wage and employment indices is to the outstanding work of A.L.Bowley, especially *Statistics of Wages in the United Kingdom during the Last Hundred Years,* fourteen articles in the *Journal of the Royal Statistical Society,* 1898–1906 (partly with G.H.Wood, whose work on 'Real Wages and the Standard of Comfort since 1850,' ibid. March 1909, complements this investigation) and 'Measurement of Employment,' ibid. July 1912.

[2] They are most easily accessible in his *Papers Relating to Political Economy* (vol. I, sec. III), where they have been reprinted under the title 'Measurement of Change in Value of Money.'

[3] The links with monetary theory are more in evidence in the parts of the *Purchasing Power of Money* (1911) that are devoted to index numbers. These parts should be perused together with the book mentioned above.

numbers pertain to the province of the statistical technician and their theory should accordingly be part of the theory of statistics, just as is, for example, the theory of sampling. A great part of the work on index numbers was in fact done by statisticians or by economists who cared little for 'economic theory.' For instance, the formula that of all displayed the most indestructible vitality is due to a man who cannot without qualification be called an economist at all, Laspeyres.[4] But almost all the decisive impulses and ideas came from economic *theorists* as they had in the eighteenth century and in the first half of the nineteenth. In order to establish this point it is enough to mention the names Jevons, Edgeworth, and Fisher, to which should be added that of A.A.Young.[5] But these were not isolated cases. An ever-increasing number of economists whom everyone would class primarily as theorists took an interest either in developing the method or in elucidating, critically and constructively, the meaning and purposes of index numbers. Marshall suggested the chain system.[6] Lexis, Walras, Wicksell, Wieser, Pigou, to mention but a few leaders, contributed substantially to the theoretical foundations.[7] Their work was continued, on an enlarged scale, during the twenties and thirties. Unfortunately, we shall not be able to notice in any detail the developments since 1920. But three performances of this period will, nevertheless, be mentioned in what follows—those of Divisia, Haberler, and Keynes.

Before going on let me restate the reason why I thought it necessary to insist on the share of economic theorists in developing the index number

[4] E.Laspeyres published the formula $\frac{\Sigma p_1 q_0}{\Sigma p_0 q_0}$ (prices weighted by quantities in the base year), which secured him immortality—a student can no more go through any complete training in economics without hearing of Laspeyres than he can without hearing of A.Smith—in the *Jahrbücher für Nationalökonomie und Statistik,* 1864; also 1871.

[5] Jevons' two papers that gave indeed a decisive impulse but do not justify Fisher's statement that he 'may perhaps be considered the father of index numbers' or the concurring statement of Keynes, are: 'A Serious Fall in the Value of Gold…' (1863) and 'The Variation of Prices and the Value of the Currency since 1782' (1865), both included in *Investigations in Currency and Finance*. Splendid work of seminal importance but, for a theorist, surprisingly unmindful of the theoretical questions involved. Edgeworth's work, which partly remedied this shortcoming, and Fisher's have already been mentioned. Allyn A.Young's work in the field is in less danger than is the rest of his work of being entirely forgotten because some of it is embodied in his contribution to H.L.Rietz's well-known *Handbook of Mathematical Statistics* (1924).

[6] In the article on 'Remedies for Fluctuations of General Prices,' *Contemporary Review,* 1887.

[7] W.Lexis was, of course, not primarily an economic theorist. But his paper 'Über gewisse Wertgesamtheiten…' in *Zeitschrift für die gesamte Staatswissenschaft* (1886) was a piece of theoretical reasoning of great importance, though it attracted little notice. Walras' contribution (1874, 1885) has been included in his *Études d'économie politique appliquée* (*ed. definitive,* 1936, pp. 20 et seq.); Wicksell's is in *Interest and Prices,* ch. 2; Wieser's—'Über die Messung der Veränderungen des Geldwerts'—in *Schriften des Vereins für Sozialpolitik* (vol. 132, 1910); Pigou's in *Economics of Welfare (1920; and earlier* in Wealth and Welfare, 1912).

method. Some statisticians and some economists of anti-theoretic bent seem to think that this piece of 'realistic' analysis is something to set against the flimsy structures of theory, something that has been created, in the true scientific spirit, for the purpose of replacing mere speculation. It seemed important to correct this opinion. The subject of index numbers affords a good example of the manner in which theoretical research and statistical research are really related and in particular how statistical methods may grow out of the theorist's work.

[(c) *Haberler, Divisia, and Keynes.*]

With the exception of Wieser, most of the leading Austrians took a critical, not to say hostile, attitude toward the idea of 'measuring' variations in the purchasing power of money (reciprocal of price level) by index numbers. They were inclined to refuse citizenship to the concept of price level and, in any case, to deny its measurability on principle.[8] In view of the fact that so many economists placed and place an uncritical trust in index figures without troubling themselves about their meaning,[9] this attitude provided a much needed antidote. And not only that. The criticism, at first merely negative, eventually turned constructive in Professor von Haberler's book on the meaning of index numbers.[10]

The core of his analysis is an interpretation of price index numbers that turns upon the following proposition: *for a given individual of unchanging tastes,* the price level has fallen (risen) between the points of time t_0 and t_1 if, his money income remaining the same, the individual is able to buy at t_1 a collection of goods which he prefers to the collection he was able to buy at t_0 (is unable to buy at t_1 a collection of goods which he prefers to the collection he bought at t_0). This interpretation connects index numbers with welfare economics. But its chief importance is in the fact that it bases them upon the theory of choice and thus makes them come to anchor in the very center of modern value theory.[11]

Whereas Haberler abandoned the idea of an 'objective' price level and replaced it by what may be termed a subjective one, Divisia produced the theory of the objective price level or monetary parameter, or monetary index *(indice monétaire),* an achievement of first-rate importance. An attempt at a simple explanation of the essential idea is made in the footnote below.[12]

[8] This attitude found its strongest expression in Professor von Mises' *Theory of Money and Credit.*

[9] This applies to any index figures, including those of physical output. In the last ten years or so a reaction has set in of which the most important symptom is that Lord Keynes, who in the *Treatise on Money* (1930) evidently attached much importance to price indices as tools of theoretical analysis, entirely avoided their use in his *General Theory* (1936).

[10] G.von Haberler, *Der Sinn der Indexzahlen* (1927).

[11] Pareto's suggestion in a similar direction (*Cours,* vol. I, pp. 264 et seq.) and a number of related ones (of which one is contained in Edgeworth's reports mentioned above) were much less convincing. We cannot stay, however.

[12] If expenditure upon all goods and services, E, changes by a (positive or negative) increment ΔE, then it is evidently possible, in a purely formal way that does not imply

It stands to reason that the idea of an over-all price level, even if admissible, is for many purposes much less useful than is the idea of sectional price levels, for example, of a price level of consumers' goods (Consumption Standard) and services as distinguished from a price level of producers' (or else investment) goods, or of a price level of finished products as distinguished from a price level of productive services and so on. The over-all price level in particular hides the relative movements as against each other of these sectional levels, and these relative movements are of pivotal importance for certain cycle theories, especially for that of Professor von Hayek. They are also of pivotal importance for the 'monetary dynamics' of Keynes's *Treatise*, Book II of which, entirely devoted to this subject, is the chief reference for this type of analysis. [This section was left unfinished.]

5. THE VALUE OF MONEY: THE EQUATION OF EXCHANGE AND THE 'QUANTITY APPROACH'

We have seen that, so far as the large majority of writers on money are con cerned, there is some truth in the statement that monetary analysis of that period dwelt, as it were, in a separate compartment. It is also true—though we have noticed exceptions such as Walras and the Austrians—that the furniture of this separate compartment was designed for the special purpose of explaining the value or purchasing power of money and not intended for any other use. Now, whenever we propose to explain the behavior of a single variable of the economic system, it is evidently convenient to bundle up all the others

anything about causation, to divide up ΔE into three parts: one that is 'due' to the changes in prices that have occurred—this part is equal to the quantities previously bought each multiplied by the changes in the respective prices or, symbolically, to $\Sigma q\Delta p$, another part is 'due' to the changes in the quantities bought and is equal to the prices previously obtaining each multiplied by the changes in the respective quantities or, symbolically, to $\Sigma p\Delta q$; and the third part is 'due' to the fact that the increments of the quantities have also been bought at the changed prices and is therefore equal to those increments of the quantities each multiplied by the increments in the respective prices or, symbolically, to $\Sigma\Delta q\Delta p$. Now, if the changes in prices and quantities (the Δq's and Δp's) are small fractions of the quantities and prices themselves (the q's and p's)—which can be the case only if we consider a very short period of time—then their product will be still smaller, so small that we may neglect it for practical purposes. But then we are left with two terms only, the one expressing that 'effect' upon expenditure that we should observe if prices had remained unchanged and therefore free from the 'effects' of any changes in prices; the other expressing that 'effect' upon expenditure that we should observe if quantities had remained unchanged and therefore free from the 'effects' of any changes in quantities. And the latter figure ($\Sigma q\, \Delta p$), expressed as a percentage of the original expenditure $(E=pq)$, then serves to define the change that has occurred in the price level or monetary index—which thereby acquires an unambiguous and analytically important meaning. This theory, which had been partly anticipated by Lexis (op. cit.), was published by Professor François Divisia in several numbers of the *Revue d'économie politique*, 1925–6, under the title 'L'Indice monétaire et la théorie de la monnaie,' and again in his *Économique rationelle* (1928), ch. XIV.

into a few big aggregates and to consider these as the 'causes' that determine the one to be explained. The so-called Equation of Exchange is certainly the simplest possible system of such aggregates that contain the value of money or the price level at all. And if the latter be the thing to be explained, the others drop naturally (though illogically) into the role of its 'causes'—and the Equation of Exchange, in itself nothing but the statement of a formal relation without any causal connotation, then turns or may turn into the Quantity Theory. This is why during that period both the equation of exchange and the quantity theory enjoyed another lease on life and why so much of the discussion on the theory of money took the form of arguments for and against the quantity theory. We must therefore try to find out what the quantity theory of these writers really amounted to. To accomplish this in the way most useful to the reader, we shall concentrate on the outstanding achievement in this line, Professor Fisher's theory of the purchasing power of money.[1]

In itself there is nothing new about what has come to be called the Fisher or Newcomb-Fisher equation. It simply links the price level *(P)* with (1) the quantity of money in circulation *(M);* (2) its 'efficiency' or velocity *(V);* and (3) the (physical) volume of trade *(T)*. Let us express this by writing $P=f(M, V, T)$. To this functional relation the Fisher equation imparts the particular form: $P=f(M,V,T)=\frac{MV}{T}$ or $MV=PT$. Again, this equation is *not* an identity but an equilibrium condition. For Fisher did not say that *MV* is the same thing as *PT* or that *MV* is equal to *PT* by definition: given values of *M, V, T* tend to *bring about* a determined value of *P*, but they do not simply *spell* a certain *P*. But the really interesting monetary analysis begins behind the façade of the equation. Two sets of questions arise.

[(a) *The Definition of the Concepts.*]

First, what are the precise meanings of *P, M, V, T*? Whatever may be urged against the quantity theory approach, one virtue it certainly has: the obvious vicinity of its concepts to statistical material forces theorists to do what without this compulsion they often fail to do, namely, to define their concepts accurately and operationally. We cannot discuss or even list, but can only point to, all the problems that lurk behind the question which prices should, for the general purposes of the equation of exchange, be included in *P*, and consequently which transactions in *T*.[2] Fisher

[1] In doing so, we take quantity theory analysis at its highest. On the whole, the cost we incur thereby in terms of information about numerous other formulations is not great. But it must be stated that, though overshadowed by Fisher's performance, Kemmerer's (*Money and Credit Instruments in Their Relation to General Prices,* 1907) would serve our purpose nearly as well. Fisher gave generous credit to Simon Newcomb's treatment of Societary Circulation (*Principles,* 1885; see above, ch. 5, sec. 7a) which is in fact an important contribution. But we cannot go into the merits peculiar to it.

[2] An idea of these problems may be derived by perusal of the Appendices to Fisher's *Purchasing Power of Money* (1911). The notion of giving up altogether the concept of a general price level of everything that is bought and sold for money (an idea that was to be carried in the twenties to its extreme by Carl Snyder's general price-level concept;

himself, although in his introductory considerations he defined *T* as the amount of 'goods' bought by money, adopted a wider concept—that included securities—in his statistical work. But attention must be called to some problems concerning the definition of *M*.

Most writers on money displayed reluctance to calling checking deposits money—at least to doing so without qualification. As we have seen, they usually stressed the difference between money and 'credit' (see below, sec. 6) or 'primary' and 'fiduciary' money. But when it came to working the equation of exchange, the majority—especially the Americans, who did by far the greatest part of the statistical work—included the quantitatively most important type of 'credit instruments,' checking deposits, as a matter of course, often going so far as to call them 'deposit currency.' The *M* of their equation of exchange, then, meant substantially coin, government fiat, banknotes, demand deposits. Since this means including practically 'everything that buys,' it might seem that they should have, on the one hand, taken account of barter (and also of the fact that part of the social product is consumed directly by its producers) and, on the other hand, excluded non-circulating money (the cash reserves of banks and hoards). The first difficulty was, so far as I can see, not taken very seriously; as regards the second I shall simply quote Kemmerer's opinion (op. cit. p. 23): 'it makes no difference to the truth of the quantity theory whether new money is offered for commodities all at once, slowly, or not at all,' because money that does not circulate has simply the velocity zero.

In Europe, especially on the continent of Europe, this conceptual scheme was much less popular, in part, because most Europeans did not face up to the statistical task. To give a front-rank example for an alternative scheme: Wicksell (as Rodbertus before him) confined *M* to metallic money (and, I suppose, fiat paper money that does not carry any title to redemption in metal), and interpreted banknotes and deposits as devices for increasing the velocity of 'money'—so that bank reserves instead of having the velocity zero, would have a very high one (Fisher's 'virtual velocity'). The reader should observe that there is no intrinsic merit or demerit in either arrangement: convenience alone is the criterion for choosing between them. This criterion, of course, tells heavily for the 'American alternative.' But there is another point to attend to. Fisher introduced the checking deposits (M') with a distinct velocity (V') separately into his equation so as to make it read: $MV+M'V'= PT$. But he introduced two additional hypotheses. First, he assumed that there exists a very stable relation between the primary money (the hand-to-hand cash) people carry in their pockets or keep in their chests or vaults and the amounts of liquid means they keep on checking account. Second, he assumed

see 'A New Index of the General Price Level from 1875,' *Journal of the American Statistical Association,* June 1924) and of replacing it by several sectional price levels (consumers' goods, investment goods, and so on) was not, so far as I know, discussed during that period except that it was implied in the Austrian group's hostility to the price-level concept. The trend of opinion in favor of the idea of multiple price levels eventually triumphed conspicuously in Lord Keynes's *Treatise* of 1930, Book II.

that, *in equilibrium,* and for periods that are not too long, there exists a very stable relation between the reserves of the banking system and the sum total of checking deposits. Let us consider what this means. By virtue of these two hypotheses Fisher's position lies somewhere between the position of those who simply include in *M* demand deposits along with 'currency outside of banks' without making any distinction between these two categories (so far as purchasing-power problems are concerned) and the position of those who, like Wicksell, include only coin and irredeemable paper. For that part of the quantity of money which Fisher called 'primary' and which, envisaging AngloAmerican conditions of 1911, he identified with gold acquires a position not shared by the checking deposits. These remain indeed 'deposit currency,' but the idea is suggested that the variation in the amount of *this* currency is governed by the variation in the quantity of the 'primary currency' or, under those conditions, of gold. The reader will see how well this links up with the compensated-dollar plan, which aims at controlling the price level by appropriate variations of the gold content of the monetary unit.

Two additional points must be mentioned about the *V*—additional, that is, to the observation made above that the velocity concept depends upon the quantity concept we choose to adopt. First, no great advance beyond Mill was made in the analysis of the factors behind the velocity of money.[3] In fact, it was not before the publication of Pigou's *Industrial Fluctuations* [4] that the various types of velocity were clearly distinguished and that the most important of them, the now familiar Income Velocity, was brought home to the profession at large. But it should not be said that the economists of that period habitually considered velocity to be a constant. Kemmerer's[5] emphasis on its variability as a function of the general business situation should suffice to refute an accusation that is constantly being repeated and that has created, in many minds, an entirely unrealistic impression to the effect that it is the chief merit of modern analysis to have recognized this variability. Second, we must pay our respects to some pioneer efforts in statistical measurement of velocity—landmarks, even though only partly successful, on the road toward numerical economics, principally associated with the names of des Essars, Kinley, Kemmerer, and, above all, Irving Fisher.[6]

[3] On the fortunes of the concept of velocity of goods, see Marget, op. cit. *passim*. Kemmerer introduced it into his equation of exchange.

[4] A.C.Pigou, *Industrial Fluctuations* (1st ed., 1927), Part I, ch. 15. Prior to this work, there is not much besides Wicksell's contribution (*Interest and Prices,* ch. 6).

[5] See above, sec. 3a.

[6] Pierre des Essars in 'La Vitesse de la circulation de la monnaie,' *Journal de la société de statistique de Paris,* April 1895; David Kinley, Doc. No. 399 in *Reports of National Monetary Commission*, The Use of Credit Instruments in Payments in the United States,' and also two papers in *Journal of Political Economy,* 'Credit Instruments in Retail Trade,' March 1895, and 'Credit Instruments in Business Transactions,' March 1897; Kemmerer, op. cit.; Irving Fisher, op. cit., but originally in 'A Practical Method of Estimating the Velocity of Circulation of Money,' *Journal of the Royal Statistical Society,* September 1909. Having derived his figures for velocity, Fisher

[(b) *Distinction between the Equation of Exchange and the Quantity Theory*.]

The second set of questions turns upon our distinction between equation of exchange and quantity theory. How far did the writers of that period *actually* go beyond the statement of the formal equilibrium relation $MV=PT$? The task of answering this question is rendered more difficult by the fact that those writers themselves did not make that distinction but often described themselves as adherents of the quantity theory when all they meant was that they saw some advantage in the use of the equation of exchange or its equivalents. However, so far as the majority of first-flight authors are concerned, we may well take as typical the opinion that Pigou was to express a little later ('The Value of Money,' *Quarterly Journal of Economics*, November 1917):[7] 'The "Quantity Theory" is often defended and opposed as though it were a definite set of propositions that must be either true or false. But in fact the formulae employed in the exposition of that theory are merely devices for enabling us to bring together in an orderly way the principal causes by which the value of money is determined.' This statement, in which the words Quantity Theory should be replaced by Equation of Exchange, certainly holds true for Marshall himself and all Marshallians: they did not go at all beyond using their variant of the equation of exchange. The same applies to the Wicksellian treatment of the influence upon price levels of autonomous variations in the quantity of money: Wicksell put so much emphasis upon the role of the rate of interest as to leave little room for *direct* influences of autonomous variations in the quantity of money. Of course, from the standpoint of those extremist opponents of the quantity theory, presently to be noticed, who denied that autonomous variations in the quantity of money have *any* influence upon its value, he—and Marshall—would have to be classed as quantity theorists.[8] The case of Walras was different, at least on the surface.

actually proceeded (*Purchasing Power*…and papers there quoted, p. 492) to present the whole equation of exchange in numerical terms—a truly Napoleonic victory even though more like Borodino than Austerlitz.

[7] See also *Essays in Applied Economics* (1923; 'The Exchange Value of Legal-Tender Money').

[8] Wicksell was so preoccupied with driving home his point that autonomous increases in the quantity of money act on the economic process, via the rate of interest on bank loans, by expanding bank credit that he often came near to denying the direct influence. But he always recovered himself. For instance he showed that an increase in the gold stock must have a direct influence on prices, at least to the extent to which it increases the incomes and the expenditure of gold producers. On this see below sec. 6b.

The position taken by von Mises illustrates to perfection the difficulties with which we have to contend. He is the foremost critic of the price-level concept. He denied that there is sense in holding that an increase in money will *ever* increase the price level pro portionately. All he averred was (op. cit. 2nd ed., p. 111) that there is 'a relation' between changes in the value of money and changes in the proportion of demand for to supply of money. This he called the useful element in the quantity theory—which, moreover, he defends against many objections. I think we had better take the clue proffered by himself and pigeonhole him with the opponents of the quantity theory in the historical sense, i.e. the quantity theory opponents meant to combat.

Walras' position is extremely difficult to understand. His purely analytic work upon the problem (see his treatment in the *Éléments* and in the 'Note sur la '"Théorie de la Quantité"' in the *Études d'économie politique appliquée,* pp. 153 et seq.) presents first of all a most interesting feature: he did not simply posit that the value of money is inversely proportional to its quantity, but he tried to deduce it rationally from the marginal utility principle, going so far as to say that one would have to reject the latter in order to have a right to reject the former. Another interesting feature is that he lets the quantities of fixed and circulating capitals be determined beforehand as a function of a given rate of interest. But, proved under these restrictions, the theorem in question, while of course true, is extremely weak and fully open to the objection we so often meet, that the quantity theory is true only under assumptions that render it trivial and quite valueless. For Walras' theorem really amounts to not more than that, all other things being *strictissime* equal, a given amount of transactions could be effected as well by means of a smaller amount of monetary units if all prices were reduced in the same proportion. However, not only did Walras call *this* the *théorie de la quantité*—which in itself would entitle us to class him with its *opponents* for, if this is really its *formule exacte,* then there is certainly nothing to it—but he also seems to have been a victim of the delusion that this theorem was all the analytic basis needed for his plan of currency reform, that is, he identified this theorem with the proposition that practical control of the price level can be achieved by controlling the quantity of money, a proposition which, right or wrong, has certainly little to do with the theorem proved.

Kemmerer's proposition that the amount of the circulating medium that is being hoarded varies widely in the short run amounts to renunciation of the quantity theory in the strictest sense and reduces so much of it as we may impute to him to the statement that *P* is determined by the three variables *M, V,* and *T,* whereas we cannot say *just as well* that *M* is governed by *P, V,* and *T,* or *V* by *P, M, T,* or *T* by *P, M, V.* Fisher expressed this by saying (*Purchasing Power,* p. 172) that 'the price level is *normally* the one absolutely passive element in the equation of exchange.'[9] But he went further than this. He also held, not indeed as a matter of general theory but as a matter of statistical fact, that in practically all cases of substantial fluctuations of price levels it was *M* only, and neither *V* nor *T,* which varied sufficiently to be considered as the explaining variable, in other words, that *M* was normally the most important 'active' variable as *P* was normally the passive one. This seems

[9] The reader will realize that the words 'just as well' in the first formulation and the word 'normally' in the second are quite essential. To repeat a comment made on this point in Part III, ch. 7, nobody ever has denied or can deny that a rise (fall) of the price level will induce a fall (rise) in gold production and an outflow (inflow) of gold so that, in the case of a free gold currency, the price level *cannot* be 'absolutely passive.' Moreover, Fisher's assertion applies only for states in the neighborhood of equilibrium, not to states of disequilibrium ('transitional periods') as we shall presently see—a fact which, and the implications of which, the unwary reader is practically certain to overlook.

to come as near to teaching quantity theory in its boldest acceptance as any front-rank economist's teaching ever did.[10] If in addition we remember the rigid assumptions that Fisher made concerning the relation between total checking deposits and gold, by virtue of which the total quantity of the circulating medium is (under the Anglo-American conditions of 1911) governed by gold production and gold exports or imports, we seem to get not only a quantity theory of the value of money but (for those particular conditions) a gold-quantity theory of it.

All the more important is it to realize that those critics were wrong who classed Fisher as a sponsor of the most rigid and most mechanical type of quantity theory and who on the strength of this see a well-nigh unbridgeable gulf between the monetary theory of the period under survey, as represented by Fisher, and the monetary theory of the twenties and thirties. They are wrong for two reasons: (1) the monetary theory of the twenties and thirties is much more under quantity theory influence than is generally realized;[11] (2)

[10] It is interesting to compare Fisher's presentation with that of the only other front-rank economist who went equally far, Cassel (see, e.g., his *Theory of Social Economy,* Third Book). He first expounds a strict quantity theory but only for the imaginary case of two disconnected states of the economy exactly equal in every respect except for a difference in *M*—and hence in *P*. He then stresses what nobody else had ever stressed with such energy, that this proves *nothing whatever* concerning the effect which a change in *M,* introduced in a real economy, would exert—adopting at this point the view usually held by opponents of the quantity theory. But then, having stated that nothing can be said a priori about the effects of actual changes of *M* in real life and that we must simply look at the facts, he finds for 1850–1910 (and, with less confidence also for the first half of the nineteenth century) that the quantity theory holds after all, not as a theory but as a statistical fact. Boldly generalizing from this, he then puts forth his famous 'Law of 3 per cent': the Sauerbeck index number having been approximately equal in 1850 and 1910 and the world's gold stock having approximately increased during that period at the rate of 2.8 per cent per annum, the *T* must have a tendency to increase at approximately that rate—and price level will hence increase or decrease according to whether gold production increases the world's gold stock by more or less than this per year. This is indeed unconventional theory. But it is interesting not only in itself but also on account of its methodology. The reader should observe that a physicist would have much less objection to the latter than most economists had. On the facts, see e.g. J.T.Phinney, 'Gold Production and the Price Level…' *Quarterly Journal of Economics,* August 1933.

[11] This most important fact unfortunately cannot be fully displayed here. I shall give a mere pointer toward the bridge between the old quantity theory analysis and more modern works. All those, especially American, writers on money who, e.g., in connection with the open-market operations of the Federal Reserve System, reasoned in a manner involving belief in the possibility of controlling ('stabilizing') business by controlling the quantity of the circulating medium were quantity theorists with a vengeance, a fact partly obscured because, faced by a different institutional set-up, they naturally expressed themselves in ways different from the authors of the Currency School. Particularly interesting in this connection is the theory that banks are *normally* 'loaned up,' that is to say, that banks will normally extend their loans as far as regulative legis lation will permit them to go. The theoretical importance of this proposition is that it

it should be clear, not only from all the other writings of Fisher but especially from his *Theory of Interest*, that he cannot be classed with quantity theorists except in a special sense.

First, he stopped short of the quantity theorem in its fullest possible sense by admitting the influence of *T* on both *V* and *M* (*Purchasing Power*…, ch. 8, §6)—thisweakens the theorem considerably, at least as a long-run proposition, because it introduces a relation between the 'independent variables' that interferes with the direct effects of variations in *T* on *P*. Second, since the quantity theorem holds only in a state of equilibrium, it is of course neither a qualification nor an objection to say that it does not hold in what Fisher calls 'transition periods.' But actually, since the economic system is practically always in a state of transition or disequilibrium, phenomena that *seem* incompatible with the quantity theorem and have in fact furnished many of their arguments to its opponents are almost always in evidence. By paying careful attention to them—especially to one type of them, namely, the tendency of the interest rate to adjust itself to both rising and falling prices with a lag (see below, sec. 8)[12]—Fisher entirely changed this situation. In strict logic, of course, he thereby merely supplemented the information that the quantity theorem conveys. But for practical purposes and, especially, if we place ourselves on the standpoint of naïve friends and foes of the quantity theorem, we might say with almost equal justice that, in a large and particularly valuable part of his work, he shelved it. Third, Fisher untiringly emphasized that *M, V, T* were only the '*proximate* causes' of *P*. Behind them there are almost a dozen indirect influences on purchasing power (op. cit. chs. 5 and 6) which act on price levels through *M, V, T*. All quantity theorists of all times would have accepted this, at least under critical fire. But there is a point beyond which emphasis upon those indirect influences begins to impair the status of the proximate causes, which then easily degenerate into intermediate causes and finally into mere names for what we are then led to label 'real' causes. And this point Fisher seems to have reached: particularly in dynamic analysis (his analysis of 'transitional periods'), which is really the thing that matters, those indirect causes become much more interesting than

makes the quantity of 'money' (deposits) strictly dependent upon the action of 'monetary authorities'—i.e. that, from the standpoint of the economic process, *M* becomes a datum or a strictly independent variable. For a characteristic example of this type of neo-quantity theory, see L.Currie, *The Supply and Control of Money in the United States* (1934). But even the Keynesian group, which more than any other emphasizes antagonism to the quantity theory, is not free from its influence. Lord Keynes himself at first professed to accept it. (See *Tract on Monetary Reform*, p. 81.) But, like Pigou, he actually only accepted the equation of exchange. In the *General Theory* he professed to renounce it. But he did not succeed entirely in freeing himself from its shackles. Whoever treats *M* as an independent variable inevitably pays some tribute to it.

[12] Reference must be made in passing to one of Fisher's most original contributions, viz., his work on the problem of Lag Distribution. See his papers in the *Journal of the American Statistical Association,* 'The Business Cycle Largely a "Dance of the Dollar,"' December 1923, and 'Our Unstable Dollar and the So-Called Business Cycle,' June

the question whether or not they can be forced into the straitjackets of *M, V, T*.

But why should that great economist have insisted on adopting what on closer scrutiny turns out to be a particularly narrow and inadequate, if not actually misleading, form of his own thought? I will hazard a hypothetical answer: he had conceived a scheme—the compensated-dollar plan—which he believed to be of great and immediate practical utility; for the success of a practical scheme simplicity is essential;[13] hence it was the simplest aspect of Fisher's analysis, the quantity theory aspect, which presented itself to his mind and dominated his exposition. The theory in the *Purchasing Power of Money* is conceived as a scaffolding for statistical work that in turn was to serve a piece of social engineering. This is what pushed aside all other considerations. But they were there and by virtue of their presence his quantity theory, if quantity theory it must be, is something quite different from other quantity theories.

As the argument above amply shows, it is not easy to draw a convincing boundary line between economists who adhered to, and economists who rejected, the quantity theorem. But there were all the time many professed enemies of it—in Germany[14] and in France they were in the majority—who held that that theorem was untenable or else completely valueless. Compared with Fisher's performance and indeed with the performances of any of those leaders who may be credited (or debited) with having used the quantity theorem in some sense or other, the arguments of those professed enemies do not show up very well. This is due to the fact that, so far as those top-flight quantity theorists are concerned, opponents were really fighting windmills: as is so often the case in economics they were trying to knock down a creation of their own fancy; they were trying to refute what had never been held—for example, that the amount of money in circulation is the sole regulator of its value—or to urge what, unknown to them, was fully taken into account by any of the better expositions of the obnoxious theorem. They thus often raised objections that asserted nothing but what was factually and theoretically correct but were nevertheless incorrect *qua* objections. Vice versa, where their arguments would have constituted valid objections—for example, the argument that quantity of money has nothing at all to do with its value—they were often patently wrong. Finally, they sometimes made points that were

[13] That simplicity was a major consideration may be inferred from two facts: first that he stowed away all the most important things into the compartments labeled 'transitional periods,' a label that suggests the desire to focus the reader's attention upon the simple equilibrium proposition; second, that he expressed the latter in an equation instead of expressing it much more satisfactorily in a system of equations which could have been easily 'dynamized' so that the equilibrium proposition would have naturally taken its true place as a special case. In another author, the failure to adopt the latter course would be easily understandable. In the case of an expert mathematician like Fisher, only the intention to simplify can account for it.

[14] See S.P.Altmann, 'Zur deutschen Geldlehre des 19. Jahrhunderts' in *Festgabe für Schmoller,* 1908, I.

both valid and relevant but not decisive: this holds for Anderson's criticism, which otherwise stands out brilliantly from the rest.[15] These shortcomings also impair the critical implications of the factual research, very valuable in itself, that was done with a view to 'refuting the quantity theory.' Again and again such phenomena as that in the earlier phases of an inflation prices rose less than *M*, and in the later phases more than *M*, were adduced against its validity—a shot that completely fails to hit the target.[16] Fisher's attempt at verification, though open to certain criticisms concerning the correlation of time series, is greatly superior to anything done by opponents.[17] Nevertheless, these

[15] B.M.Anderson, *Value of Money* (1917). A sample of his criticism may be useful. Suppose that the wages of domestic servants be increased (without any servant being dismissed) and that these servants use their additional income exactly as their employers had used the same sum before. Therefore nothing has changed except that the price of directly consumed services that should be included in the price-level index has gone up: *M* and *T* have remained constant, yet *P* has risen. In his review of Anderson's book in the *Economic Journal,* March 1918, Edgeworth replied to this by pointing out that though *M* and *T* have remained constant, *V* has been increased. But, obviously, an increase in *V* which *occurs automatically in certain cases of price changes* cannot be set against Anderson's objection. Hence he was right. But while his objection stands, it would not tell heavily against any quantity theory that does not pretend to be more than a broad approximation.

[16] The following small sample from this literature may be welcome to some readers: H.P.Willis, 'History and Present Application of the Quantity Theory,' *Journal of Political Economy,* September 1896; Alfred de Foville, 'La Théorie quantitative et les prix,' *L'Économiste Français,* April and May 1896; D.Berardi, *La Moneta nei suoi rapporti quantitativi* (1912); J.L.Laughlin, 'A Theory of Prices,' *Publications of the American Economic Association,* 3rd series (February 1905); W.C.Mitchell, *Gold Prices and Wages under the Greenback Standard* (1908) and 'Quantity Theory of the Value of Money,' *Journal of Political Economy,* March 1896; J.Lescure, 'Hausses et baisses générales des prix,' *Revue d'économie politique,* July 1912; B.Nogaro, 'Contributions a une théorie réaliste de la monnaie,' ibid. October 1906; E.Dolléans, *La Monnaie et les prix* (1905). For Germany, I will mention two of the period's best men on money and monetary policy, though they do not present themselves favorably in their arguments against the quantity theorem—which were in part developed for the particular purpose of showing that the fall in prices, 1873–98, had nothing to do with gold production or with the extension of the area of the gold standard: Erwin Nasse ('Das Sinken der Warenpreise…' *Jahrbücher für Nationalökonomie,* July and Au gust 1888) and W.Lexis (the famous statistician), numerous papers, see, e.g., his criticism of Walras' plan in his review article, 'Neuere Schriften über Geld- und Edelmetalle' (ibid. July 1888); see, however, Rist (op. cit. p. 253n.) for quotations to the effect that Lexis accepted the quantity theory in principle. Their inability to handle properly what after all was not a very complicated argument is astounding. So is K. Marx's failure to see that the cost of producing money (however defined) must act on commodity prices through its effect upon the supply of money: he denies any influence of quantity of money upon prices, *Capital* (English trans., Kerr ed., vol. I, p. 136).

[17] Another attempt that corroborates Fisher's result is conspicuous for excellence of workmanship: Oskar Anderson, 'Ist die Quantitätstheorie statistisch nachweisbar?' in *Zeitschrift für Nationalökonomie* (March 1931). One of the reasons why both verifica-

did not yield. And they were justified in refusing to do so. For they had a case.

A simple example will elucidate this apparently paradoxical situation. Con*sider* a case of war inflation that runs its course like this: disturbance of domestic production and of export and import trade first raises most prices, the government's war demand being financed by means that would without the war have been spent by private individuals; this rise in prices together with an increase, at an increasing rate, in war demand *in physical terms* then enforces resort to the manufacture of 'money' (or credit instruments that do not have, in this case, the properties of the ordinary credit instruments of commerce); and finally there develops an increasing demand for loans by producers—a credit expansion in the commercial sense but incessantly fed by ever-increasing prices. Now, historians, politicians, businessmen will certainly describe such a process in terms of the war itself and of the disturbance on the one hand and the excess demand on the other which the war entails. They will be surprised to learn that, instead of war and war disturbance and war demand, it is just *M, V*, and *T* that 'cause' inflation and that it is only *M* and *V* that really matter. And if they are told that these are the 'proximate causes' whereas war, war disturbance, war demand are 'indirect' ones—the quantity theorist will always have to admit the 'direct' role of variations in *T*—which are operative but only at one remove, they will not be content. If anything, they will be annoyed, especially, if they suspect that more is at stake than a mere theoretical argument. In this they were right, of course: in the nineteenth century as well as in the twenties and thirties of the twentieth a rigid quantity theory, one that attributed to *M* an altogether unjustifiable role in economic therapy, had a way of suddenly emerging from more careful formulations. Especially in the United States, the sound-money men—and all those economists who felt quite rightly that currency troubles are but the reflex of deeper things—had plenty of reason for distrusting the possible practical implications of the quantity theorem, a distrust that then extended, however unfairly, to the quantity theory analysis itself. But they could have urged purely scientific reasons also. What I have described as straitjackets may be useful for certain restricted purposes exactly as are all such oversimplified set-ups, for example, the Keynesian system. Outside of the range of these purposes, they become inconvenient and impediments to more fundamental analysis. If, moreover, we admit cyclical variability of *V* and stress the importance of such 'indirect' causes as the rate of interest, the rate of change of *P* (vs. *P* itself), and so on, they become in addition useless. And it is hardly an exaggeration to say that

tions and refutations from statistical material failed to convince should be noted in passing: to a large extent, the decision to accept, or to refuse to accept, given statistical evidence, is a highly subjective matter. Since no material can ever bear out the quantity theory with a 100 per cent accuracy and no material that covers, say, at least ten years can ever fail to show some relation between *P, T*, and *M*, there must in most cases be room for fair difference of opinion as to what given statistical findings really mean. It is the merit of more refined methods, such as those of O.Anderson, that they offer criteria that are more reliable than is simple 'impression.'

the chief progress of monetary theory in more recent times has been the result of a tendency to tear up the straitjackets and to introduce explicitly and directly all that the best presentations of the quantity theory relegated into the limbo of indirect influences. Lesson: in economics more than elsewhere, a good cause and one that will win out eventually may be so inadequately defended as to appear to be bad for decades together.

[(c) *Purchasing Power Parity and the Mechanism of International Payments.*]

Before going on, let us touch upon two other matters. In that period, more definitely than before, we find in the neighborhood of the quantity theorem its old ally, the purchasing-power-parity theory of foreign exchange, that is, the proposition that, if left to itself, the price of a country's monetary unit in terms of foreign currencies tends to be inversely proportional to the relations between the respective price levels. It was repeatedly stated, for example, by Marshall and Schlesinger, but when, in the discussion on the exchange troubles that arose during and after the First World War, Cassel pressed it energetically into service, it struck most people like a new discovery.[18] As I have stated it, the proposition does not seem very exciting. Both Marshall and Schlesinger noticed it as they went along, without putting much emphasis upon it. And we may discern, in the torrent of publications which 'purchasing power parity' was to produce, a quiet little inlet of discussions about the merits of that proposition as a tool of analysis.[19] The excitement sprang from the fact that Cassel linked it up with a strict quantity theory and, in application, with the problems of war inflation. In consequence of this, the purchasing-power-parity theory turned into the so-called 'inflation theory' of foreign exchange, which reads: increase in *M* raises the price level; the rise in a country's price level decreases the value of its monetary unit in terms of non-inflated foreign currencies. Opposing arguments were marshalled under the flag of a 'balance-of-payment' theory, which often, though not always, went so far as to make the causal nexus run from exchange rate to price level instead of from price level to exchange rate. We cannot go into this controversy in which opponents never met each other's arguments on the same plane of fact and of abstraction and which, though better things were not lacking, on the

[18] Cassel's many publications on the subject started in 1916. The references that are likely to be most useful to the reader are to Cassel's *Theory of Social Economy* (ch. 12) and to Professor H.Ellis' work on *German Monetary Theory* (Part III), which goes far beyond the German discussion and will prove helpful to those readers who wish to enter more fully into a subject to which I can only draw attention.

[19] This inlet was mainly fed from English sources. See especially A.C.Pigou, The Foreign Exchanges,' *Quarterly Journal of Economics,* November 1922, and J.M. Keynes, *Tract on Monetary Reform* (ch. 3, glorified by an excellent treatment of forward trading in exchange). The discussion had the merit of raising several worth-while questions, but ended in the anaemic result that the purchasing-power-parity theorem, when properly qualified, was of hardly any value at all. As a matter of fact, this is not true, and Lord Keynes might have arrived at a better definition of the equilibrium rate of exchange than he produced when preparing his Clearing Union and Bretton Woods plans, if he had not disposed so lightly of what is a quite valuable starting point.

whole presents a sad example of the futility—largely due to inadequate analytic power of the participants—of so many economic controversies.

I take this opportunity of noticing another controversy (or set of controversies) that proved more fruitful: the controversy on the mechanism of international payments. It ran its course and produced its results in the twenties and thirties, but its sources are in the work of the nineteenth century and some of the most important participants drew inspiration from the contest between Thornton and Ricardo (see above, Part III, ch. 7, sec. 3).[20] We have before us what is indeed a typical case of normal scientific development. The older authors had, more or less explicitly, noticed all the essential elements of the problem. But when J.S.Mill summed up their work, it was nevertheless an incomplete and one-sided picture that emerged, namely, the schema of the mechanism of unilateral international payments (tributes, or loans, or repayment of loans), according to which the paying country first transfers gold, thereby increasing the price level of the receiving country and reducing its own so as to acquire an export surplus, which then takes care of the subsequent payments. The glaring inadequacy of this account, which not only puts the whole burden of adjustment on the price level but also neglects the phenomena inevitably associated with such an adjustment, was indeed felt and noticed by Bastable ('On Some Applications of the Theory of International Trade,' *Quarterly Journal of Economics,* October 1889) and others, but the theory proved a hardy plant and survived in current teaching right into the twenties, in spite of protests (e.g., Wicksell's in 'International Freights and Prices,' *Quarterly Journal of Economics*, February 1918). When the problem of German reparations drew everybody's attention to these questions of mechanism, relatively rapid progress was made in building up an organon of analysis that was new as such though none of its elements were. Ohlin's performance (*Interregional and International Trade*, 1933) supplies a convenient landmark in this as it does in other respects. The role of Taussig's teaching should be particularly noticed. He started from Mill's schema and, in spite of a number of improvements he added, personally never abandoned it. But by virtue of the criticism he elicited and of the work of his pupils, whom his leadership inspired, he helped the new analysis into existence almost as effectively as if he had created it himself. On the one hand, much of the most significant theoretical work developed from his teaching, Viner's especially. On the other hand, he started off an important sequence of factual researches.[21]

[20] The following brief and inadequate comments that cannot do more than indicate another 'bridge' between our own work and the past may be supplemented by J.Viner's treatment of the subject in *Studies in the Theory of International Trade* (chs. VI and VII). It is a pleasant duty to criticize the author for having impaired his picture by stressing inadequately the importance of his own contribution in *Canada's Balance of International Indebtedness* (1924). Relying once more on this reference, I shall in what follows mention contributions with great brevity.

[21] In general, that period's factual research on international capital movements is among its major titles to our gratitude. C.K.Hobson's *The Export of Capital* (1914) will serve as an example.

6. THE VALUE OF MONEY: THE CASH BALANCE AND INCOME APPROACHES[1]

The Newcomb-Fisher equation of exchange and expressions closely similar to it were indeed widely used (or implied by verbal circumlocutions) but not universally. We are now going to glance at two other important formulae. In both cases, it is as important to grasp that they were fundamentally equivalent to the Newcomb-Fisher equation as it is to understand the nature of the differences that induced many economists to prefer them. Or to put the same thing from a different angle: the important thing to understand is why those formulae, in spite of their fundamental equivalence with the Newcomb-Fisher equation, nevertheless suggested advance in a different direction.

(a) *The Cash Balance Approach.*

Walras often spoke of the quantity of money. But the central concept of his analysis of money is the *encaisse désirée*, that is, the amount of cash that people individually desire to hold at any moment. Similarly, the Cambridge economists, following Marshall's lead and in obedience to the Petty-Locke-Cantillon tradition, adopted a formula that expressed the same idea. Let n be the amount of 'cash in circulation' with the public, p the index number of the cost of living, k the number of 'consumption units,' also an index figure, representing the physical complement of the public's holdings of hand-to-hand cash, k' the number of consumption units representing similarly the physical complement of the public's checking deposits, and r the fraction of k' that banks keep as a cash reserve against k', then we have[2]

$$n=p(k+rk')$$

[1] Specific reference should again be made to Professor Marget's treatment of these subjects (op. cit., vol. I, chs. 12–16).

[2] See, e.g., J.M.Keynes, *Monetary Reform*, American ed., 1924, pp. 82–6. Three things should be observed with respect to this particular formulation. (1) The 'public' includes the business world; though business does not spend on consumers' goods, the physical complement of its holdings of cash in hand and at banks is nevertheless measured in 'consumption units,' exactly as is the physical complement of consumers' cash and balances. (2) In the chapter in which this exposition of the Cambridge theory occurs, Keynes confused—as did so many others—use of the equation of exchange and acceptance of the quantity theory; as a matter of fact, he did not mean to accept the quantity theorem in any strict sense. (3) In particular, he emphasized, already in *Monetary Reform*, the wide variability of k, k', and r, and he also protested, though mildly, against the uncritical assumption that 'a mere change in the quantity of the currency cannot affect k, k', and r'—statements that foreshadow certain features of the analysis of the *General Theory*. The *Treatise* takes up an intermediate position, the main features of which are the breaking up of the general price level into sectional price levels, and the explicit introduction of Saving and Investment among the variables. The equations of the *Treatise* (Book III) must be looked upon as developments of the equation above. They illustrate the meaning of my statement to the effect that progress of monetary analysis in the twenties and thirties largely consisted in brushing aside the comprehensive aggregates of equation-of-exchange analysis and in introducing explicitly the variables expressive of the 'indirect influences.'

This is the so-called Cambridge equation, which is to embody the Cash Balance Approach. It assumes and asserts exactly what the Newcomb-Fisher equation assumes and asserts. In particular, it is not more and not less of an identity. The feature that at first sight may seem to constitute a substantive difference, namely, the absence of velocity, is not very important: for all the problems that, in the Newcomb-Fisher equation, are treated under the heading Velocity turn up in much the same form when we try to work with the Cambridge equation. But there is nevertheless something about it which deserves notice because it sheds light on an important aspect of the Filiation of Scientific Ideas. In expressing the Cambridge equation in words, it is natural to say—and all Cambridge economists did say—that 'the public choose' or 'elect' to keep $p(k+rk')$ in cash and balances, and this manner of speaking constitutes a psychological bridge to later, especially Keynesian, opinions: for it *points* toward the individual decisions that are behind the public's behavior in the matter of holding liquid assets and *suggests* analysis of the motives that prompt them. Especially, if we express the matter by saying that there is such a thing as a 'balance of advantage' as between holding money and holding other forms of wealth, we cannot help seeing the signpost that points toward the Liquidity Preference Theory of Keynesian fame. But once more we have to add that this does not amount to the liquidity preference theory. It is clear, especially in the case of Walras' *encaisse désirée,* that we need additional assumptions concerning people's attitude toward holding cash to carry us from the one to the other.

(b) *The Income Approach.*

We have noticed that Tooke, in his '13th thesis,' had suggested that the explanation of money prices should start from consumers' incomes. As we know, he offered this as an alternative to the explanation of price levels by the quantity of money which he rejected. Ever since, the Income Approach has appealed to analysts—though it was also adopted by others—who disliked the quantity theory or even the equation of exchange.[3] But it is easy to see that, in itself, the former is nothing but another way of writing the latter. Moreover, the amendment might seem to be of doubtful value since incomes evidently 'determine' prices in the same sense only in which prices 'determine' incomes. Yet Wieser's [4] and Hawtrey's preference for this approach is quite understandable, though it yields no result that cannot be obtained via the equation of exchange: like the cash balance ap-

[3] This holds for A.Aftalion (*L'Or et sa distribution mondiale,* 1932), or for R.Liefmann (*Geld und Gold,* 1916), who said categorically: incomes determine prices, and also for Tooke's follower, Adolf Wagner, but not for the most eminent of the sponsors of the income approach, R.G.Hawtrey (*Currency and Credit,* 3rd ed., 1928), who starts from Consumers' Outlay, which is 'proportional jointly to the unspent margin [equivalent to *encaisse désirée,* J.A.S.] and the circuit velocity of money.' He calls this 'a form of the quantity theory' (p. 60). Several German writers, however, refused to see this and had to be taught by Hans Neisser, *Tauschwert des Geldes* (1928) that there is no contradiction between the income and the quantity theory.
[4] See his *Social Economics* or his article 'Geld' in the *Handwörterbuch* (4th ed., 1927).

proach, it points to individual behavior; more than the cash balance approach, it removes mere quantity of money from the position of a proximate ‘cause of the price level’ and substitutes for it one that is still nearer to prices—income, or even consumers' expenditure;[5] finally it relieves the theory of money prices from such questions as what is to be considered as money. The effect of an increase of money upon prices is indeterminate so long as we do not know who gets the additional money, what he does with it, and what the state of the economic organism is on which the new money impinges. The income formula does not in itself take account of all these questions but it directs our attention toward them and thus helps monetary analysis to step out of its separate compartment. This advantage is particularly obvious in analyzing an inflationary process. Though there is really not much more sense in quarreling over the question whether it is the increased quantity of money or the increased pay roll that ‘causes’ inflation than there would be in quarreling over the question whether it is the bullet or the murderer's intention that ‘causes’ the death of the victim, there is still something to be said for concentrating on the mechanisms by which the increased quantity of money becomes operative—not to speak of the additional advantage which counts for so much in economics, namely, that the income-expenditure formula does not meet with some of the prejudices that the equation of exchange encounters.

7. BANK CREDIT AND THE ‘CREATION’ OF DEPOSITS

The important developments that occurred during that period in the banking systems of all commercialized countries and in the functions and policies of central banks were, of course, noticed, described, discussed. We cannot survey the vast literature which performed this task and of which reports of official commissions and the articles of the best financial journals, the London *Economist* in particular, formed perhaps the most valuable part. It was written by businessmen, financial writers, business economists of all types who knew all about the facts, the techniques, and the current practical problems of banking but who cared little about ‘principles’—except that they never failed to refer to established slogans—and cannot be said to have had any very clear ideas about the meaning of the institutional trends they beheld. Considered from the standpoint of scientific analysis, these works were, therefore, raw material rather than finished products. And since the ‘scientific analysts’ of money and credit largely failed to do their part, namely, to work up this material and to fashion their analytic structures to its image, we might almost—though not quite—characterize the situation by saying that that literature on banking and finance was as much of a separate compartment within the litera-

[5] The reader will recall that this particular advantage does not amount to a great deal if, when using the equation of exchange, we pay proper attention to the factors that govern the variations, especially the cyclical variations, in velocity. On the other hand, it might be said that if we do this we have really accepted what the income approach is meant to convey.

ture on money and credit as the latter was a separate compartment within the literature on general economics.

There are a number of books for England, in particular, such as W.T.C.King's *History of the London Discount Market* (1936) and the various histories of the Bank of England (e.g., the recent one by Sir John Clapham, *The Bank of England,* 1944), which will supply part of the information that cannot be given here. For other references, see the little bibliography attached to the article on 'Banking, Commercial' in the *Encyclopaedia of the Social Sciences* (especially the books of the following authors: C.A.Conant, A.W.Kerr, A.Courtois, E.Kaufmann, A.Huart, J.Riesser, O.Jeidels, C.Supino, C.Eisfeld, H.P.Willis). This bibliography contains two items which, owing to their high quality, should be particularly mentioned: C.F.Dunbar's *Theory and History of Banking* (5th ed., 1929, but essentially a work of the nineteenth century) and F.Somary's *Bankpolitik* (1st ed. 1915; 2nd ed. 1930). Perusal of *A History of Banking Theory* by L.W.Mints (1945) will show the reader how far the descriptive literature 'spilled over' into the books on monetary and banking theory, though the author's presentation of his huge material is somewhat impaired by undue emphasis on the shortcomings of a particularly narrowly defined commercial theory of banking (the 'real-bills doctrine').

The situation described above by the separate-compartment simile accounts for the emergence of a special type of book which was written not only for the general reading public but also for economists in order to enlighten them on the facts and problems of banking or finance. The success of these books proves, better than anything else could, how far the separation of those departments, between which they sought to establish connection, had actually gone. Two famous instances call for notice. The one is W.Bagehot's *Lombard Street: A Description of the Money Market* (1873), one of the most frequently and most admiringly quoted books in the whole economic literature of the period. No doubt it is brilliantly written. But whoever now turns to that book with its fame in mind will nevertheless experience some disappointment. Barring a plea for the reorganization of the management of the Bank of England and for a reform of English practice concerning gold reserves, it does not contain anything that should have been new to any student of economics. Obviously, however, it did teach many economists things they did not know and were glad to learn. Our other instance is the not less brilliant book by Hartley Withers, *The Meaning of Money* (2nd ed., 1909), whose chief merit consists, as we shall presently see, in having boldly spoken of the 'manufacture' of money by banks. But this should not have surprised anyone. Yet it was considered as a novel and somewhat heretical doctrine.

Thus, academic analysis of credit and banking—including the contribution of writers who, without being academic economists themselves, conformed to the academic pattern, as did some bankers—went along on the stock of ideas inherited from the preceding period, refining, clarifying, developing no doubt but not adding much that was new. Substantially, this meant the prevalence of the commercial theory of banking which made the commercial bill or, somewhat more generally, the financing of current commodity trade the theoretical cornerstone of bank credit. We shall, of course, trace this position to

Tooke and Fullarton. But the currency school influence was stronger than appears on the surface. Toward the end of the period, it asserted itself particularly in the precincts of the theory of cycles (see below, sec. 8).

As regards central banking, economists enlarged indeed their conception of the functions of central banks, especially the controlling and regulating function of the 'lender of last resort' But most of them were surprisingly slow in recognizing to the full the implications of Monetary Management, which as we have seen was developing under their eyes. Adherence to the commercial theory was, of course, partly responsible for this. Because of it, control continued to mean—not wholly but primarily—control by 'discount policy.' The economics profession was not even sure whether it was in the power of central banks to regulate market rates or whether bank rate was merely 'declaratory.'[1] Votaries of both opinions then discussed the effects of bank rate in terms of the two classic *modi operandi:* on the one hand, pressure on prices by restriction of credit (*almost* equivalent to amount of commercial bills presented for discount); on the other hand, attraction from abroad of foreign funds or recall from abroad of domestic funds.

As regards banking in general, it is quite true that strict adherence to the commercial theory caused economists to overlook or misconceive some of the most important banking developments of that time. Nevertheless, the derogatory criticism leveled at it in our own day is not entirely justified. To begin with, it was not so unrealistic for England, and English prestige in matters of banking tended to make English practice the standard case. But, quite apart from this, it should be emphasized that acceptance of the commercial theory does not necessarily involve uncritical optimism about the working of the discounting mechanism. Economists stressed the 'elasticity' of the system that turns on financing commodity trade. But they had grown out, or were growing out, of the opinion that if banks simply finance the 'needs of trade,' then money and production will necessarily move in step and no disturbance will arise—which is the really objectionable thesis. On the one hand, most of them realized, as Ricardo and Tooke had done before them, that there is no such thing as a quantitatively definite need for loans or discounts and that the actual amount of borrowers' demand is as much a question of the banks' propensity to lend and of the rates they charge as it is a question of borrowers' demand for credit. On the other hand, they realized more and more that the practice of financing nothing but current trade—discounting good commercial paper—does not guarantee stability of prices or of business situations in general or, in depression, the liquidity of banks.[2] And it was Wicksell's achieve-

[1] The futility of this discussion, which could have been settled by a glance at the facts, should be obvious. We shall, however, think more kindly of it if we observe that the technique of 'making bank rate effective' was only slowly developing during that period and that economists were still slower in discovering what was actually being done. Without this technique it is indeed a fair question to ask whether central banks can do much more than follow the market—which is what is meant by the phrase that their rates are 'declaratory.'

[2] In other words—putting the matter from the standpoint of the policy of credit control—it was being increasingly realized that attention to the purpose to be financed (*current* commodity transaction) and to the quality of the credit instruments involved (*good* commercial paper) did not enable central banks to dispense with attention to

ment to introduce both facts into the general theory of money by means of his famous model of the Cumulative Process (see below, sec. 8).

Finally, there is another point, quite independent of all this, that must be noticed: the curious narrowness and lack of realism in that period's conception of the nature of bank credit. In order to make this point stand out clearly, let us restate how a typical economist, writing around 1900, would have explained the subject of credit, keeping in mind, however, all the limitations and dangers inherent in speaking of typical views. He would have said something like this. In the (logical) beginning is money—every textbook on money, credit, and banking begins with that. For brevity's sake, let us think of gold coin only. Now the holders of this money, so far as they neither hoard it nor spend it on consumption, 'invest' it or, as we may also say, they 'lend' their 'savings' or they 'supply capital' either to themselves or to somebody else. And this is the fundamental fact about credit.[3] Essentially, therefore, credit is quite independent of the existence or non-existence of banks and can be understood without any reference to them. If, as a further step in analysis, we do introduce them into the picture, the nature of the phenomenon remains unchanged. The public is still the true lender. Bankers are nothing but its agents, middlemen who do the actual lending on behalf of the public and whose existence is a mere matter of division of labor. This theory is satisfactory enough in cases of actual 'lending on account of others'[4] and of savings deposits. But it was also applied to checking deposits (demand deposits, the English current accounts). These, too, were made to arise from people's depositing with banks funds that they owned (our gold coins). The depositors become and remain lenders both in the sense that they lend ('entrust') their money to the banks and in the sense that they are the ultimate lenders in case the banks lend out part of this money. In spite of certain technical differences, the credit supplied by deposit banking—the bulk of commercial credit in capitalist society—can therefore be construed on the pattern of a credit operation between two private individuals. As the depositors remain lenders, so bankers remain middlemen who collect 'liquid capital' from innumerable small pools in order to make it available to trade. They add nothing to the existing mass of liquid means, though they make it do more work. As Professor Cannan put it in an article in *Economica* ('The Meaning of Bank Deposits') which appeared as late as January 1921: 'If cloak-room attendants managed to lend out exactly three-quarters of the bags entrusted to them…we should certainly not accuse the cloak-room attendants of having "created" the number of bags indicated by

the quantity of credit outstanding: this is implied, though perhaps not adequately, in the theory of the bank rate.

[3] We know that leading theorists described the process in terms of the commodities that credit operations were in the last analysis intended to transfer. But for our present purpose it is not necessary to go into this again.

[4] By this is meant a contractual arrangement by which an owner of large funds which he does not immediately need, e.g. an industrial corporation that has just received the proceeds of a bond issue, employs the services of a bank to lend out these temporarily idle funds in the money market, to stock brokers or bill brokers.

the excess of bags on deposit over bags in the cloak rooms.' Such were the views of 99 out of 100 economists.

But if the owners of those bags wish to use them, they have to recover them from the borrowers who must then go without them. This is not so with our depositors and their gold coins. They lend nothing in the sense of giving up the use of their money. They continue to spend, paying by check instead of by coin. And while they go on spending just as if they had kept their coins, the borrowers likewise spend 'the same money at the same time.' Evidently this phenomenon is peculiar to money and has no analogue in the world of commodities. No claim to sheep increases the number of sheep. But a deposit, though legally only a claim to legal-tender money, serves within very wide limits the same purposes that this money itself would serve. Banks do not, of course, 'create' legal-tender money and still less do they 'create' machines. They do, however, something—it is perhaps easier to see this in the case of the issue of banknotes—which, in its economic effects, comes pretty near to creating legal-tender money and which may *lead* to the creation of 'real capital' that could not have been created without this practice. But this alters the analytic situation profoundly and makes it highly inadvisable to construe bank credit on the model of existing funds' being withdrawn from previous uses by an entirely imaginary act of saving and then lent out by their owners. It is much more realistic to say that the banks 'create credit,' that is, that they create deposits in their act of lending, than to say that they lend the deposits that have been entrusted to them. And the reason for insisting on this is that depositors should not be invested with the insignia of a role which they do not play. The theory to which economists clung so tenaciously makes them out to be savers when they neither save nor intend to do so; it attributes to them an influence on the 'supply of credit' which they do not have. The theory of 'credit creation' not only recognizes patent facts without obscuring them by artificial constructions; it also brings out the peculiar mechanism of saving and investment that is characteristic of fullfledged capitalist society and the true role of banks in capitalist evolution. With less qualification than has to be added in most cases, this theory therefore constitutes definite advance in analysis.

Nevertheless, it proved extraordinarily difficult for economists to recognize that bank loans and bank investments do create deposits. In fact, throughout the period under survey they refused with practical unanimity to do so. And even in 1930, when the large majority had been converted and accepted that doctrine as a matter of course, Keynes rightly felt it to be necessary to reexpound and to defend the doctrine at length,[5] and some of its most impor-

[5] *Treatise on Money,* ch. 2. It is, moreover, highly significant that, as late as June 1927, there was room for the article of F.W.Crick, The Genesis of Bank Deposits' *(Economica),* which explains how bank loans create deposits and repayment to banks annihilates them—in a manner that should have been indeed, but evidently was not even then, 'time-honored theory.' There is, however, a sequel to Lord Keynes's treatment of the subject of credit creation in the *Treatise* of 1930 of which it is necessary to take notice in passing. The deposit-creating bank loan and its role in the financing

tant aspects cannot be said to be fully understood even now. This is a most interesting illustration of the inhibitions with which analytic advance has to contend and in particular of the fact that people may be perfectly familiar with a phenomenon for ages and even discuss it frequently without realizing its true significance and without admitting it into their general scheme of thought.[6]

For the facts of credit creation—at least of credit creation in the form of banknotes—must all along have been familiar to every economist. Moreover, especially in America, people were freely using the term Check Currency and talking about banks' 'coining money' and thereby trespassing upon the rights of Congress. Newcomb in 1885 gave an elementary description of the process by which deposits are created through lending. Toward the end of the period (1911) Fisher did likewise. He also emphasized the obvious truth that deposits and banknotes are fundamentally the same thing. And Hartley Withers espoused the notion that bankers were not middlemen but 'manufacturers' of money. Moreover, many economists of the seventeenth and eighteenth centuries had had clear, if sometimes exaggerated, ideas about credit creation and its importance for industrial development. And these ideas had not entirely vanished. Nevertheless, the first—though not wholly successful—attempt at working out a systematic theory that fits the facts of bank credit adequately, which was made by Macleod,[7] attracted little attention, still less favorable attention. Next came Wicksell, whose analysis of the effects upon prices of the rates charged by banks naturally led him to recognize certain aspects of 'credit creation,' in particular the phenomenon of Forced Saving.[8] Later on, there

of investment *without any previous saving up of the sums thus lent* have practically disappeared in the analytic schema of the *General Theory,* where it is again the saving public that holds the scene. Orthodox Keynesianism has in fact reverted to the old view according to which the central facts about the money market are analytically rendered by means of the public's propensity to save coupled with its liquidity preference. I cannot do more than advert to this fact. Whether this spells progress or retrogression, every economist must decide for himself.

[6] In consequence, there may be merit and even novelty in a piece of work which can be proved to say nothing that has not been said before in some form or other—which in fact we have had occasion to observe many times. It seems to me that Professor Marget's account of the development of the doctrine of credit creation (op. cit. vol. I, ch. 7) does not attach sufficient weight to this consideration.

[7] Henry Dunning Macleod (1821–1902) was an economist of many merits who somehow failed to achieve recognition, or even to be taken quite seriously, owing to his inability to put his many good ideas in a professionally acceptable form. Nothing can be done in this book to make amends to him, beyond mentioning the three publications by which he laid the foundations of the modern theory of the subject under discussion, though what he really succeeded in doing was to discredit this theory for quite a time: *Theory and Practice of Banking* (1st ed., 1855–6; Italian trans. 1879); *Lectures on Credit and Banking* (1882); *The Theory of Credit* (1889–91).

[8] In itself the idea was not new, see F.A.von Hayek, 'Note on the Development of the Doctrine of "Forced Saving,"' *Quarterly Journal of Economics,* November 1932, republ. in *Profits, Interest and Investment* (1939). But it now appeared in a larger con-

were other contributions toward a complete theory, especially, as we should expect, in the United States. Davenport, Taylor, and Phillips may serve as examples.[9] But it was not until 1924 that the theoretical job was done completely in a book by Hahn, and even then success was not immediate.[10] Among English leaders credit is due primarily to Professors Robertson and Pigou not only for having made the theory palatable to the profession but also for having added several novel developments.[11] Elsewhere, especially in France, resistance has remained strong to this day.

The reasons why progress should have been so slow are not far to seek. First, the doctrine was unpopular and, in the eyes of some, almost tinged with immorality—a fact that is not difficult to understand when we remember that among the ancestors of the doctrine is John Law.[12] Second, the doctrine ran up against set habits of thought, fostered as these were by the legal construction of 'deposits': the distinction between money and credit seemed to be so obvious and at the same time, for a number of issues, so important that

text and with a new emphasis. During the last decade, the concept has fallen into unmerited disfavor. But it has its merits. In particular, it clears up a point that has caused difficulties to many. Banking operations, so Ricardo had said, cannot create 'capital' (i.e. physical means of production). Only saving can do this. Now, whenever the expenditure from deposits that are created by banks increases prices, i.e. under conditions of full employment (and also in other cases), a sacrifice of consumption is imposed upon people whose incomes have not risen in proportion, which achieves what otherwise would have to be achieved by saving, and there is point in calling this, metaphorically, Involuntary or Forced Saving and in contrasting it with what is usually called Saving (Voluntary Saving). That under conditions of unemployment and excess capacity no such sacrifice need necessarily be imposed upon anyone is no reason for discarding the concept.

[9] Davenport's contribution merely consisted in hints which he threw out in his *Value and Distribution* (1908) without making much of them: he emphasized, e.g., that it is not correct to say that banks 'lend their deposits.' W.G.L.Taylor, in a book which (like Davenport's) never received the recognition it deserved, went much further (*The Credit System,* 1913). A great stride was made by C.A.Phillips (*Bank Credit,* 1920), who not only did much to clear up the theoretical questions involved but in addition pointed out the difference between the expansion of loans and investments that is possible for an individual bank which competes with others and the expansion that can be performed by a system of competing banks, considered as a whole.

[10] Albert Hahn, *Volkswirtschaftliche Theorie des Bankkredits* (3rd ed., 1930). One reason why this book left so many economists unconvinced was, however, the fact that the theory of bank credit there presented was wedded to certain highly optimistic views about the possibility of achieving permanent prosperity, which prejudiced some economists against its essential achievement.

[11] D.H.Robertson, *Banking Policy and the Price Level* (1926). Forced saving figures there under the name of Imposed Lacking. A.C.Pigou, *Industrial Fluctuations* (1927), Part I, chs. 13 and 14.

[12] Thus Walras saw the phenomenon of credit creation quite clearly (though he confined himself to banknotes). But he considered it as an abuse that ought to be suppressed and refused for this reason, to make it a normal element of his general schema (*Études d'économie politique appliquée,* ed. of 1936, p. 47 and pp. 339 et seq.).

a theory which tended to obscure it was bound to be voted not only useless but wrong in point of fact—indeed guilty of the elementary error of confusing legal-tender money with the bookkeeping items that reflect contractual relations concerning this legal-tender money. And it is quite true that those issues must not be obscured.[13] That the theory of credit creation does not necessarily do this seemed small comfort to those who feared its misuse.

8. CRISES AND CYCLES: THE MONETARY THEORIES

We have seen on the one hand that, broadly speaking, the monetary analysis of that period centered in the problems of Value of Money (or price level) but on the other hand that some leading economists were working their way toward monetary analysis of the economic process as a whole in which mere price-level problems fall into secondary place. This tendency has been illustrated by the implications of the cash balance and income approaches but it asserted itself also in many other ways. It is significant, for instance, that Marshall originally intended the volume that appeared as *Money, Credit, and Commerce* to carry the title *Money, Credit, and Employment:* and there are in fact many things in it that come within the range of recent Income and Employment Analysis. Much more significant was it that Wicksell, in his somewhat hesitating way that is so engaging, eventually made up his mind to the effect that we need a concept of monetary demand for output as a whole.[1] This revived the Malthusian idea and anticipated, though in an incompletely articulate manner, the consumption function of Keynes's *General Theory*.

But the most considerable advance in the direction of monetary analysis in the present-day sense occurred within the precincts of the problems of interest and business cycles. We have already noticed symptoms of a growing inclination of economists to recognize and to use a monetary concept of capital. Nothing came of this, nor did the few attempts that were made to interpret

[13] One of them is the old issue: control of 'money' vs. control of 'credit.' Considerations of the kind alluded to explain the aversion of many French authorities to the credit-creation idea. For instance, one of the leading purposes of Professor Rist's *History of Monetary and Credit Theories* is to combat the 'confusion' of money and credit.

[1] The reference that will be most useful to the reader is to Myrdal's *Monetary Equilibrium* (Swedish ed. 1931, English trans. 1939; see above, sec. 2c). Once more, the point to grasp is this: demand schedules are defined for a single commodity. According to 'classical' theory (Say's law), there would be no sense in speaking of a demand schedule for all goods and services (or all consumers' goods and services) taken together. If we do so, nevertheless, we are for a special purpose doing something that is not covered by the ordinary theory of demand and are taking therefore a step beyond it. This special purpose may or may not be meaningful. It may or may not be well served by the aggregate-demand technique. But in any case, it should be recognized as a thing *sui generis* that carries its own particular problems. Wicksell's adoption of it spelled renunciation of Say's law. He is, therefore, the patron saint of all those economists who renounce Say's law at present.

interest as a purely monetary phenomenon meet with any success.[2] Throughout the period, the rate of interest remained, for practically all economists, a rate of return—however explained—to physical capital and the money rate a mere derivative of the real rate.[3] It had long been recognized, of course, that the two may diverge from one another: Ricardo's explanation of how new money inserts itself into circulation implies recognition of this fact, and writers on banking must always have been aware of it. But nobody attached much importance to it until Wicksell made it the center of his theory of the value of money and the subject of an elaborate analysis that produced the Wicksellian Cumulative Process: he pointed out that, if banks keep their loan rate below the real rate—which as we know he explained on the lines of Böhm-Bawerk's theory—they will put a premium on expansion of production and especially on investment in durable plant and equipment; prices will eventually rise; and if banks refuse to raise their loan rate even then, prices will go on rising cumulatively without any assignable limit even though all other cost items rise proportionally.[4]

The analytic situation created by this argument may be described like this. In itself the Wicksellian emphasis upon the effects of possible divergences between money and real rates of interest does not constitute a compelling reason for abandoning the position that the fundamental fact about interest is a net return to physical goods, a position from which Wicksell himself never departed. However, it does constitute a good and sufficient reason for treating the money rate as a distinct variable in its own right that depends, partly at least, upon factors other than those that govern the net return to physical capital (natural or real rate). The two are related, of course. In equilibrium they are even equal. But they are no longer 'fundamentally the same thing.'[5]

[2] They were so little noticed or so completely forgotten that they were not even mentioned in the discussion on this topic in the 1930's. One of them, Silvio Gesell's, was however rescued from oblivion by Lord Keynes, see *General Theory,* ch. 23, VI.

[3] This meaning of real or 'natural' rate must not be confused with the wholly different meaning in which Marshall used the phrase (*Principles,* Book VI, ch. 6, concluding note), namely, the meaning of money rate (or 'nominal' rate) corrected for price-level changes. The two are related but not identical and Marshall has, so far as I can see, no share in the Wicksellian idea I am about to discuss. His own merit in emphasizing what may be termed the distinction between nominal and actual rate is shared by Irving Fisher (*Appreciation and Interest,* 1896).

[4] Böhm-Bawerk's comment on this argument was: 'Wicksell must have been dreaming when he wrote that.'

[5] The following paraphrase of the paragraph above may prove helpful. Into the Walrasian system enters just one rate of interest, which is a rate of net return on physical 'capitals.' Strictly, this implies that the money rate of interest is not only equal to this rate of net return in equilibrium but identical with it, in the sense that the money rate is merely the monetary expression of the rate of net return on physical 'capitals.' If we want to recognize explicitly that instead of being identical with this rate of net return (equivalent to saying that it is 'fundamentally the same thing') the money rate has some measure of independence, we must introduce it as another variable and posit equality with the 'real rate' as an additional equilibrium condition. This

And so soon as we recognize this, they will drift further and further apart and *we* shall drift further and further away from the position that the net return to physical goods of one kind or another is the fundamental fact about the interest rate of the loan market—the position which we have traced to Barbon and which Lord Keynes was to condemn on the ground that it involved 'confusion' between rate of interest and the marginal efficiency of (physical) capital.[6] Other factors, such as the loan policy of banks, will then seem to us to be just as fundamental, and the road opens toward the purely monetary theories of interest that emerged later and of which the Keynesian was to attract more attention than any other. Let us, however, keep in mind three things. First, we have been sketching a most interesting line of doctrinal development, which starts with Barbon and runs a course that, for the moment, ends with Keynes. But it is not suggested that the individuals who made themselves responsible for the newer monetary theories of interest consciously arrived at their conclusions by working out the implications of the situation created by the Wicksell analysis: this may have been the case with his Swedish disciples—though I do not wish to question *anyone's* subjective originality—but it was certainly not so with the others. Second, it is not suggested that, by retracing Barbon's steps, the economists of our epoch have simply returned to the monetary theories of pre-Barbonian times: though similar to them in important respects—and especially to those of the scholastics—theirs are unquestionably novel in others. Third, by defining the new variable of our economic system, money interest, as a thing that is monetary in nature and not only in form, we do not eliminate from the problem of the loan rate the 'real' factors as completely as some modern economists seem to think: the rate of net return to physical investment remains, at the very least, a factor in the demand for loans and therefore cannot vanish from any complete theory of the money rate.[7]

is what Wicksell did. His investigations into the conditions of monetary equilibrium were not entirely successful. They made history of analysis, however, through the impulse they gave to contemporaneous and later research, especially by his Swedish followers (see e.g. Myrdal, op. cit.).

[6] Wicksell's real or natural rate of interest is the marginal productivity of (physical) capital (more precisely, the marginal productivity of Böhm-Bawerk's roundabout process). It is, therefore, not identical with Keynes's marginal efficiency, which is the same as Fisher's marginal rate of return over cost (*Theory of Interest,* p. 169) and means marginal productivity of current investment. But the two concepts stand in a unique relation to one another so that, for the purpose in hand, they may nevertheless be used interchangeably. Lord Keynes may hence be said to have condemned the 'confusion' between money and real rate of interest or, better, the habit of nineteenth-century economists to link them together too closely. It then appears that Wicksell was the first to undermine this habit.

[7] This fact is important precisely because it is so often denied and because Keynes's exposition in the *General Theory* tended to obscure it, although it is not less essential for his monetary theory of interest than it is for any other. It comes in by way of the condition that the equilibrium amount of current investment is the amount for which 'marginal efficiency' is equal to the money rate. The statement that interest is the

Wicksell's position in the development of modern monetary cycle theories is quite similar to his position in the development of modern monetary interest theories. He himself no more held a monetary cycle theory than he held a monetary interest theory. But he opened the road for the former as he opened it for the latter. In fact, the Cumulative Process itself need only be adjusted in order to yield a theory of the cycle. Suppose that banks emerge from a period of recovery or quiescence in a liquid state. Their interest will prompt them to expand their loans. In order to do so they will, in general, have to stimulate demand for loans by lowering their rates until these are below the Wicksellian real rate, which, as we know, is Böhm-Bawerk's real rate. In consequence, firms will invest—especially in durable equipment with respect to which rate of interest counts heavily[8]—beyond the point at which they would have to stop with the higher money rate that is equal to the real rate. Thus, on the one hand, a process of cumulative inflation sets in and, on the other hand, the time structure of production is distorted. This process cannot go on indefinitely, however—there are several possible reasons for this, the simplest being that banks run up against the limits set to their lending by their reserves—and when it stops and the money rate catches up with the real rate, we have an untenable situation in which the investment undertaken on the stimulus of an 'artificially' low rate proves a source of losses: booms end in liquidation that spell depression.

This theory has been sketched out by Professor von Mises,[9] who, while extending critical recognition to Wicksell, described it as a development of currency school views. It was further developed by Professor von Hayek into a much more elaborate analytic structure of his own,[10] which, on being presented to the Anglo-American community of economists, met with a sweeping success that has never been equaled by any strictly theoretical book that failed to make amends for its rigors by including plans and policy recommendations or to make contact in other ways with its readers' loves or hates. A strong critical reaction followed that, at first, but served to underline the success, and then the profession turned away to other leaders and other interests.[11] The social psychology of this is interesting matter for study.

factor that limits investment is as true as to say that the price of motor cars is the factor that limits the demand for them, and is equally incomplete.

[8] Obviously the rate of interest, a minor factor in short-run investment, is a major one in long-run investment such as investment in durable machines, railways, utilities, the capital value of which increases rapidly as the interest rate is reduced. [J.A.S. intended to expand this—he penciled 'This is obscured by risk—otherwise.']

[9] *Theorie des Geldes*…1924, Third Part, ch. 5, secs. 4, 5. This reference is to the 2nd ed., in which the line of reasoning above is presented as an essentially complete explanation of cycles. The fundamental ideas, however, are already contained in the original edition of 1912.

[10] *Geldtheorie und Konjunkturtheorie* (1929); *Prices and Production* (1931). A new version that altered the argument in several important respects appeared in 1939: *Profits, Interest, and Investment;* and a further installment that covered much new ground, in 1941: *The Pure Theory of Capital*.

[11] Other successes of 'theoretical' books, in our time, for example, the success of Professor E.H.Chamberlin's *Monopolistic Competition* and Hicks's *Value and Capital,*

Hawtrey's[12] analysis makes business cycles, as he himself put it, a purely monetary phenomenon in a sense in which the Mises-Hayek cycle is not. Hawtrey makes no use of the element of disturbance (or maladjustment) in the time structure of plant and equipment; fluctuations in the flow of money income, themselves caused by exclusively monetary factors, are the only cause of general cyclical fluctuations in trade and employment. But he does use the Cumulative Process and traces it like Mises to the inherent instability of the modern credit system. Banks, then, are again supposed to start abnormal activity by easy conditions for loans. Only the main link of this with general booming conditions is not increase in orders for new plant or equipment but increase in the stocks held by the wholesale trade that also react to small changes in loan rates. Expansion leads to further expansion, hence to increased money incomes and to loss of hand-to-hand cash by the banks, whose inability to go on expanding loans indefinitely then leads to a rise in rates which reverses the process—which is why the central bank rate plays so great a role in this analysis. Thus, similarities are sufficiently pronounced to entitle us to speak of a single monetary theory, the votaries of which disagree on one issue only: whether bank-loan rates act primarily on 'durable capital' or via the stocks of wholesalers. Throughout the twenties, Hawtrey's theory enjoyed a considerable vogue. In the United States, especially, it was the outstanding rationalization of the uncritical belief in the unlimited efficacy of the open-market operations of the Federal Reserve System that prevailed then.

Nor is the fundamental unanimity of the votaries of the monetary theory of cycles[13] seriously disturbed by those economists who place responsibility for the phenomenon with the vagaries of gold. This idea commanded more assent when it was used to 'explain' those longer spans of prevalent prosperity or prevalent depression that are in fact associated (more or less) with significant changes in the rate of gold production, such as, roughly, 1849–72 or 1872–91. But it has also been used to 'explain' business cycles proper. In this case, since an accession of gold acts on bank reserves and hence makes banks more will-

were more enduring and therefore greater in the end. But they lacked the spectacular quality of Hayek's. The much greater success of Keynes's *General Theory* is not comparable because, whatever its merit as a piece of analysis may be, there cannot be any doubt that it owed its victorious career primarily to the fact that its argument implemented some of the strongest political preferences of a large number of modern economists (see below Part V, ch. 5). Politically, Hayek's swam against the stream.

[12] R.G.Hawtrey, *Good and Bad Trade* (1913), and many later works. Perusal of *Capital and Employment* (1937) will show the extent to which Mr. Hawtrey modified his earlier views.

[13] When we speak of monetary theories of cycles, a double meaning of the word theory (see Part I) leaps to mind. A monetary theory of cycles is an explanatory hypothesis of cycles that runs in terms of money and lending. But nobody denies that *any* explanation of the phenomenon must take account of its monetary features. We may, therefore, use the word monetary theory also for the sum total of propositions about the ways in which money and credit behave in the cycle. And, considered as contributions to monetary cycle theory in this sense, many arguments, such as Hawtrey's, retain importance even for those who do not accept them as adequate in the role of explanatory hypothesis.

ing and able to lend, we have a particular reason for expecting expansion instead of the more general reason formulated by Mises and Hawtrey but, for the rest, the argument will be much the same: again credit inflation owing to low money rates, again the point at which interest catches up with prices, and reversal of the process. The most eminent sponsor of this type of monetary theory, Professor Irving Fisher, at first stated it in this unsophisticated manner in his *Purchasing Power of Money*, 1911 (ch. 4).[14] But, though he continued to emphasize the monetary aspects of the phenomenon, he so broadened the basis of his analysis as to end up with the Debt-Deflation Theory, which, contrary to his unduly restricted claim, applies to all recorded business cycles and is in essence not monetary at all. Ostensibly, the burden is chiefly laid upon the fact that in the atmosphere of prosperity debts are accumulated, the inevitable liquidation of which, with the attendant breaks in the price structure, constitutes the core of depression. Behind this surface mechanism there are the really operative factors—new technological and commercial possibilities chiefly—which Fisher does not fail to see but which he banishes to the apparently secondary place of 'debt starters' (*Econometrica,* October 1933, p. 348), so that, exactly as in the case of his general monetary analysis (see above, sec. 2), the true dimensions of what is really a great performance are so completely hidden from the reader's view that they have to be dug out laboriously and in fact never impressed the profession as they should have done.

9. NON-MONETARY CYCLE ANALYSIS

It will be convenient to go on in order to glance briefly at some analyses of cyclical phenomena other than Hayek's that are non-monetary *in the sense defined,*[1] although we shall have to cross the frontiers of this chapter's subject in doing so. But we shall go no further than is necessary in order to establish one important proposition, namely, that *all* the essential facts and ideas about

[14] The version presented in *Purchasing Power* had been published before, in summary, in *Moody's Magazine* under the title 'Gold Depreciation and Interest Rates,' February 1909. The main stepping stones to the Debt-Deflation Theory are the articles: '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), both of which concentrate on fluctuations of prices and interest rates that are traced to purely monetary conditions, and the book *Booms and Depressions* (1932) partly summarized and partly complemented in 'The Debt-Deflation Theory of Great Depressions,' *Econometrica,* October 1933, to which reference is made in the text.

[1] The italicized words should be kept in mind because, in view of the fact noticed in the preceding section, namely, that the demand for money and especially for bank credit must always play *some* role, and mostly an important one, in explanations of fluctuations, any less strict definition of 'purely monetary theories' would result in the inclusion of many more. But even so dividing lines are very much a matter of subjective judgment and cannot be drawn sharply. Not all historians will, e.g., call the Mises theory purely monetary or the Hayek theory non-monetary.

business-cycle analysis had emerged by 1914: the subsequent thirty years brought forth, indeed, a flood of statistical and historical material, and many new statistical and theoretical techniques; by clarification and elaboration they may be said to have expanded the subject into a recognized branch of economics; but they added no principle or fact that had not been known before.[2]

(a) *Juglar's Performance.*

As we have seen, it was the spectacular phenomenon of 'crises' and the less spectacular but still more irritating phenomenon of depressions ('gluts') which, in the preceding period, first attracted the attention of economists. We have also seen, however, that some of them did look beyond depressions: such men as Tooke and Lord Overstone fully realized that crises and gluts were but incidents or phases of a larger process; many more displayed symptoms of a vague awareness of this fact. Nevertheless, it was only during the period under survey that the 'cycle' definitively ousted the 'crisis' from its place in economists' minds and that the ground was cleared for the development of modern business-cycle analysis, though practically all workers in the field continued to use the old phrase—an interesting case of 'terminological lag.' This is why the decisive performance is considered here although it was published in 1862. It was the work of a man who was a physician by training, but must be ranked, as to talent and command of scientific method, among the greatest economists of all times, Clément Juglar.[3] This evaluation rests

[2] This statement and my failure to make the (impossible) attempt to survey the achievements of this later literature on cycles must not be interpreted in a derogatory sense. On the contrary, I believe the work embodied in this literature to be as valuable as any ever done by economists. This much at least will be evident from what I shall say about it in Part V. It is nevertheless essential to realize the extent to which this work rests upon bases laid before 1914. Attention is called to Professor R.A. Gordon's 'Selected Bibliography of the Literature on Economic Fluctuations, 1930–36,' *Review of Economic Statistics,* February 1937, and to the list of *books* about Business Cycles published by the Bureau of Business Research, University of Illinois, College of Commerce and Business Administration, 1928. Professor von Haberler's masterly presentation of the modern material (*Prosperity and Depression,* 1937; 3rd enlarged ed., 1941) is recommended as an introduction to the subject: reliance on the fact that few if any students of economics fail to consult this work is my main excuse for keeping my own comments upon it as brief as possible. The reader will understand, however, that my admiration for it does not involve agreement in every point. Work prior to 1895 is fairly well covered by a history that appeared in that year: E.von Bergmann, *Geschichte der nationalökonomischen Krisentheorieen.* From a lengthy list of other historical and critical publications, I will mention only: Alvin H.Hansen, *Business-Cycle Theory* (1927); then, once more, F.Lutz, *Das Konjunkturproblem in der Nationalökonomie* (1932); and W.C.Mitchell's Business Cycles…(1927), especially ch. 1.

[3] Clément Juglar (1819–1905) abandoned medicine for economics in 1848. He had no formal training in the latter subject and cared even less than he knew about formal theory. His was the type of genius that walks only the way chalked out by himself and never follows any other. Many people do this in a subject like economics. But then they mostly produce freaks. The genius comes in where a man produces, entirely on his own, truth that will stand. Of his many publications it is only necessary to mention the principal one: *Les Crises commerciales et leur retour périodique en France, en*

upon three facts. To begin with, he was the first to use time-series material (mainly prices, interest rates, and central bank balances) systematically and with the clear purpose in mind of analyzing a definite phenomenon. Since this is the fundamental method of modern business-cycle analysis, he can be justly called its ancestor. Second, having discovered the cycle of roughly ten years' duration that was most obvious in his material—it was he who discovered the continent; islands near it several writers had discovered before—he proceeded to develop a morphology of it in terms of 'phases' (upgrade, 'explosion,' liquidation). Though Tooke and Overstone had done the same thing, the modern morphology of cycles dates from Juglar. And so does, in the same sense, 'periodicity.' This morphology of a 'periodic' process is what he meant when he proudly claimed to have discovered the 'law of crises' without any preconceived theory or hypothesis.[4] Third, he went on to try his hand at explanation. The grand feature about this is the almost ideal way in which 'facts' and 'theory' are made to intertwine. In themselves, most of his suggestions concerning the factors that bring about the downturn (loss of cash by banks, failure of new buying) do not amount to a great deal. But all-important was his diagnosis of the nature of depression, which he expressed with epigrammatic force in the famous sentence: 'the only cause of depression is prosperity.' This means that depressions are nothing but adaptations of the economic system to the situations created by the preceding prosperities and that, in consequence, the basic problem of cycle analysis reduces to the question what is it that causes prosperities—to which he failed, however, to give any satisfactory answer.

Economists were at first slow to follow up Juglar's lead. Later on, however, most of them, even those who were more inclined than he was to commit themselves to particular hypotheses concerning 'causes,' adopted his general approach—so much so that today Juglar's work reads like an old story very primitively told. And at the end of the period stands a work that, on the one hand, was entirely conceived in his spirit and, on the other hand, ushered in a most important part of the cycle analysis of our own time: Wesley C.Mitchell's *Business Cycles*.[5]

Angleterre et aux Etats Unis ('crowned' by the Académie des Sciences Morales et Politiques in 1860, publ. as a book in 1862, 2nd ed. 1889, English trans. by W.Thom, from 3rd ed., 1916). There is a *Notice* of his life and work by Professor Paul Beauregard, in the *Comptes rendus* of the Académie des Sciences Morales et Politiques (1909).

[4] Juglar seems not to have considered the implications of the fact that his 9–10 year cycle could not be expected to be the only wavelike movement in his material. Later workers naturally discovered others. At least the names of N.D.Kondratieff (1922) and Joseph Kitchin (1923) should be mentioned (on these and predecessors, see Mitchell, op. cit. pp. 227 and 380). But we can do no more than advert to this line of advance. Juglar's merit is hardly diminished by these developments—in fact, they only serve to enhance his historical position.

[5] *Business Cycles* (1913); entirely re-written version, *Business Cycles: the Problem and Its Setting* (1927); *Measuring Business Cycles* by A.F.Burns and W.C.Mitchell (1946). I do not mean to suggest, however, that Professor Mitchell derived his approach from Juglar, any more than I would suggest that the inventors of the 'Harvard

(b) *Common Ground and Warring 'Theories.'*

That period, then, established a method, at least the fundamental principle of a method, on which, by the end of the period, a majority of business-cycle analysts agreed and which was to serve the bulk of the work of our own time. Agreement went further than this however. By the end of the period the lists of the features or symptoms that characterize cyclical phases—which different economists did draw up or would have drawn up—looked much alike. And not only that: by the end of the period most workers agreed—or tacitly took for granted—that the fundamental fact about cyclical fluctuations was the characteristic fluctuation in the production of plant and equipment. Now, how is this? We seem to be discovering a lot of common ground that should have assured much parallelism of effort and much agreement in results. Yet this is not at all what a survey of that literature reveals. On the contrary, we seem to behold nothing but disagreement and antagonistic effort—disagreement and antagonism that went so far as to be discreditable to the science and even ludicrous. The contradiction is only apparent however. Agreement on the list of features, even if it had been complete,[6] does not spell agreement as to their relations with one another, and it is the interpretation of these relations and not the list per se which individuates an analytic scheme or business-cycle 'theory.' Even agreement to the effect that it is the activity in the plant-and-equipment ('capital goods') industries which is the outstanding feature in cyclical fluctuations does not go far toward ensuring agreement in results since it leaves the decisive question of interpretation wide open. And, in order to avoid misunderstanding, we must emphasize at once that the outstanding feature of cyclical phases, whatever it is, need not contain within itself the 'cause' that explains why cyclical fluctuations exist: this 'cause' may still lie somewhere else, for example, in the sphere of consumption. But in spite of all this, it remains both true and important that agreement went further than the troubled surface suggests and that most of the analysts of the business-cycle phenomenon who produced theories, which look so different, really started from a common basis.

I. The fact that the 'relatively large amplitude of the movements in constructional, as compared with consumption, industries' is one of the most obvious 'general characteristics of industrial fluctuations'[7] can hardly fail to ob-

Barometer' were subjectively dependent on him. All I want to point out is the objective contour line of the development of that method—Filiation of Scientific Ideas is an objective process which may, but need not, involve any subjective relation. Similarly, Menger had not heard of Gossen until long after he had developed his version of the marginal utility analysis. Yet Menger's work stands in an objective sequence in which Gossen stands, in time, above him.

[6] It was substantial but not complete. An example will illustrate: nobody can fail to recognize that prices move characteristically in the course of a cycle; but their behavior is not quite regular and there are prosperities in which they failed to rise; this left room for difference of opinion on whether or not they should be included in a list of 'normal' features.

[7] Pigou, *Industrial Fluctuations* (1927), Part I, ch. 2.

trude itself upon anyone [8] who has learned to look at a cycle as a whole, though it may escape attention so long as one looks merely at the depression phase. Nevertheless, it took time for it to be recognized consciously and with full awareness of its pivotal importance. Speaking very roughly, we may associate this achievement—or a decisive share in this achievement—with 'the work of Tugan-Baranowsky.[9] It is, however, only the emphasis upon the pivotal importance of that fact which constitutes the historical merit of the work. His own interpretation of it—that is, his distinctive theory—which runs in terms of alternating accumulation and release of liquid saving, is valuable only as an example of how short the way is from a promising starting point into a blind alley, even for an able and serious worker.

II. The outstanding work in the line under discussion is Arthur Spiethoff's.[10] His analytic schema first lists a number of possible starters of a process of expansion of plant and equipment, which process then accounts without difficulty for all the other observed phenomena of booms, great care being taken to account for the individual peculiarities of every historical instance. This em-

[8] Walras, it is interesting to note, treated as common knowledge the fact that the *production des capitaux neufs* goes on in alternating high tides and low tides—characterized by respectively high and low rates of discount and of prices—and identified it (in 1884) with what we call business cycles of about 10 years' duration. He does not quote Juglar but Jevons. (*Études d'économie appliquée,* 1936, p. 31.)

[9] Mikhail Ivanovich Tugan-Baranowsky (1865–1919) was the most eminent Russian economist of that period and should perhaps have been mentioned also in other connections. The methodological aspect of his work is particularly interesting: he did much historical work of high quality; but he was also a 'theorist'; and he combined, or welded into a higher unit, these two interests in a way which he had learned from Marx and which was by no means common. From Marx, too, he had learned to theorize, though he experienced the influence both of the English 'classics' and of the Austrians with the result that his theoretical work in the end amounted to a 'critical synthesis.' But neither his *Theoretische Grundlagen des Marxismus* (1905) nor his *Soziale Theorie der Verteilung* (1913) made any mark. This was but natural in view of the deficiency in rigorous thinking both displayed, which is as deplorable as it is curious in a man of his ability. More important were his work on the history of industrial capitalism in Russia (1st Russian ed., 1898; German trans. 1900) and *Modern Socialism in Its Historical Development* (1906; English trans. 1910). The only other item that need be mentioned out of what no doubt was an imposing total is the most important of all, for this did make a mark and did exert influence far and wide, viz., his history of commercial crises in England (first in Russian, 1894; German version, 1901; French, 1913). Again, the first and theoretical chapter is a distinctly poor performance. The rest stands in the history of our science.

[10] On Spiethoff, see above, ch. 4, sec. 2d. The main reason why his work developed so slowly was his heroic resolve to carry out a vast program of minute factual research single-handed—practically without any research assistance at all. Though he began to publish fragmentary results in 1902 (in Schmoller's *Jahrbuch*), a provisional presentation of the whole—really a preview only—was not published before 1925 in vol. VI of the 4th ed. of the *Handwörlerbuch der Staatswissenschaften,* article 'Krisen.' I understand that preparations are being made for the publication of a fuller version in English.

phasis upon the expansion of plant and equipment is reflected in the choice, for the role of fundamental index, of iron consumption (production plus imports minus exports). The problem that remains, namely why this expansion eventually runs into a general condition of production at a loss ('overproduction'), is then solved by means of several factors, such as shortage of working capital and temporary saturation of demand in particular directions. This schema, which at every step leaves plenty of room for alternatives, is admirably suited for absorbing, into their proper places and without exaggerating their importance, many other factors that are worked up into unique motors of the cyclical movement by other theories, such as 'psychological' factors, monetary factors, acceleration, undersaving. Spiethoff's analysis, therefore, comes nearest to an organic synthesis of relevant elements and to full utilization of the coordinating power of that starting point. And it has still another virtue: with the possible exception of Marx, Spiethoff was the first to recognize explicitly that cycles are not merely a non-essential concomitant of capitalist evolution but that they are the essential form of capitalist life. Also he was one of the first to observe that there are long periods during which prosperity phases of cycles are accentuated by favorable conditions ('spans of prosperity') and other long periods during which depression phases are accentuated ('spans of depression'). He refused, however, to combine these drawn-out spells of predominant prosperity and depression into 'long cycles' and he reserved judgment as to their causation.

It would be extremely interesting to compare Spiethoff's work on cycles with the work of Robertson, which though independent of Spiethoff's, displays affinity in important respects.[11] There is no similarity in method. Spiethoff

[11] Professor D.H.Robertson's publications start in January 1914 with an important but all but unknown article ('Some Material for a Study of Trade Fluctuations') in the *Journal of the Royal Statistical Society* that presented historical material in support of the promising idea—which Robertson failed to exploit but which never vanished completely from his horizon—that cycles have something to do with the impact upon the economic process of *new* industries, some booms being connected, e.g. with railroad building, others with inventions in steel production, electricity, the explosion motor, and so on. Next came his *Study of Industrial Fluctuation* (1915), which drew a picture closely similar to Spiethoff's. The monetary complement (saving, forced saving, credit creation, and so on) was added in his famous *Banking Policy and the Price Level* (1926; 3rd ed., 1932) and elaborated in various papers most of which are reprinted in *Essays in Monetary Theory* (1940). A passage in *Banking Policy*…(p. 5) is so important for the *histoire intime* of the monetary analysis of our day that quotation is imperative: 'I have had so many discussions with Mr. J.M.Keynes on the subject-matter of Chapters V and VI [containing the monetary analysis], and have re-written them so drastically at his suggestion, that I think neither of us now knows how much of the ideas therein contained is his and how much is mine.' This, of course, was J.M. Keynes of the *Treatise* and not of the *General Theory,* but there were in Robertson's book some pointers also toward the latter. In view of the later disagreements between these two eminent men, it is desirable to notice that, whatever their immediate cause, there was always this fundamental difference: Keynes concentrated on monetary aspects and monetary policy from the first, whereas Robertson emphasized 'real factors'—

started, in the spirit of Juglar, from minute investigations of available statistics; Robertson worked first and last as a 'theorist,' taking only the broadest and most obvious facts as a base and concentrating on forging tools of interpretation. Therefore, their work is complementary rather than competitive. But their general visions of the cyclical process and its causation were closely similar.[12]

III. A few examples will suffice to display the fact that most theories of cycles are nothing but different branches of that common trunk, 'plant and equipment.'

First, the reader will realize without difficulty that even the purely monetary theories of cycles may be included among the 'investment theories.' For although they locate the *causes* of the cyclical movement in the monetary sphere, *effects* upon the plant-and-equipment industries are bound to play some role. If, in particular, explanation pivots on the money rate of interest, disturbance in the structure of 'physical capital' must always be a factor in cyclical situations though, especially from a short-run point of view like, for example, Hawtrey's, it need not be made the decisive one. If we do make it the decisive one, we get the non-monetary or semi-monetary theory of Hayek—increased production of durable plant and equipment ('lengthening of the period of production') through a fall of the money rate of interest below the marginal rate of profit.

Second, writers who agree to interpret business cycles primarily as investment cycles—in the physical sense of the term investment—may still differ as to the 'starter' and such differences will then individuate their theories. Thus, what may be termed the *perpetuum-mobile* theory contents itself with the fact that depression itself will in its course produce conditions favorable, first, to revival and, then, to the construction of new plant and equipment. To give another example, Mrs. England, with a keener sense of the necessity for a more convincing cause, pointed to the activity of promoters or, more generally, to the intrusion into the horizon of entrepreneurs of new technological or commercial possibilities.[13]

Third, whatever it is that gives the prosperity impulse, we may derive a dis-

as against both monetary and psychological ones—from the first. There were thus wide stretches of ground that were Robertson's own and into which Keynes's analysis never penetrated. Within this wider frame, monetary propositions acquire a meaning—and one that is very relevant for practical applications—that is wholly different from the meaning and implications which the *same* monetary propositions convey if taken by themselves.

[12] Robertson repeatedly expressed awareness of this fact, regretfully hinting at the prohibitive barrier of language. It can, I believe, only happen in economics that a scientific worker would leave it at that. I do not say this in reproach. I say it because the case illustrates a state of things that is very general and explains much in the history of economics.

[13] Of the interesting papers by Minnie Throop England, we note especially 'Promotion as the Cause of Crises,' *Quarterly Journal of Economics,* August 1915, and 'An Analysis of the Crisis Cycle,' *Journal of Political Economy,* October 1913.

tinctive theory by emphasizing the indubitable fact that the plant and equipment, construction of which is undertaken in reaction to such an impulse, takes time to get into existence and working order—time during which there is nothing to blunt the edge of that impulse. Consequently, when later on the stream of additional products impinges upon consumers' goods markets, something like 'general overproduction,' that is, a price fall that turns expected profits into actual losses, may result. If we trust this explanation sufficiently, we can speak of a 'lag theory' of the cycle. We get another version if we put the main emphasis, instead of on the fall in the prices of consumers' goods, on the rise in the price of cost items. The former version may be exemplified by the works of Bouniatian and Aftalion, the latter by that of Lescure, though there is much in all three of them to relieve the pressure on the factor primarily stressed.[14] Incidentally, we may infer from this that he who says that business cycles are primarily cycles in prices *may* mean exactly the same thing as he who says that they are primarily cycles in investment.

Fourth, there was again, as there had been in the preceding period, a crop of those theories which, in one way or another, impute responsibility for depressions to the inadequacy of money incomes in general—more precisely their failure to expand *pari passu* with the production, actual or potential, of consumers' goods[15]—or to people's saving habits or, finally, to inadequacy of the incomes of some classes and the saving habits of others. I have had occasion already to comment on the indestructible vitality they owe to their popular appeal. It was to this appeal—particularly strong in prolonged periods of predominant depression—and not to any great improvement in their analytic foundations that they owed their survival. Leading scientific opinion, however, continued to be unfavorable to them and they continued, to borrow Lord Keynes's felicitous phrase, to live in a scientific underworld. So much was this the case that leading economists did not even bother to make the concessions that were obviously indicated. For though the argument against oversaving

[14] Mentor Bouniatian, *Wirtschaftskrisen und Ueberkapitalisation* (1908), enlarged as *Les Crises économiques* (Russian original, 1915; French trans. 1922); A.Aftalion, *Les Crises périodiques de surproduction* (1913); J.Lescure, *Des Crises générales et périodiques de surproduction* (1906; 3rd ed., 1923). All three of these authors, but especially the two last, are particularly notable for strict adherence to Juglar's methodological principles.

[15] This was sometimes called 'the flaw in the price system' and may also be expressed by saying that the expansion of production in capitalist society is normally attended by a long-run tendency in prices to fall ('deflation'). It is highly characteristic of the mental habits that prevail in economics that this fact, which received much attention, was hardly ever seen in its organic significance. Some economists—I think that Marshall was among them—noticed it with approval much as A.Smith had approved of 'cheapness and plenty.' For others, it was just a 'flaw.' The best that can be reported was that some writers pointed out that falling prices did not spell disturbance where they were a consequence of cost-reducing improvement; and that others pointed out that monetary remedies for falling prices would create disturbance of their own (profit inflation).

theories may be strong so long as they aver that saving is an ultimate and independent 'cause' of disturbance, it should never be denied, on the one hand, that there are plenty of hitches in the saving-investment mechanism and, on the other hand, that saving, in a depression that has already set in for reasons other than saving, may make things worse on balance than they otherwise need be, especially if saving takes the form of hoarding as it is likely to do in a depression. But the leaders of prevailing opinion, though they had occasional glimpses of all this,[16] completely failed to go into the matter properly—a fact that explains much in the recent history of economics. They evidently attached but little importance to these possibilities of disturbance. They did not even emphasize the role in the cycle of that saving which is being used for the repayment of bank loans. Thus a considerable tract of open country was left unguarded in which, to the backward glance of the economist of today, there seems to stand, in something that to many looks very like a halo of glory, the figure of J.A.Hobson. Actually, his was not a solitary figure. Nor did he come very near to having anticipated the doctrines of present-day Keynesianism. But we shall confine ourselves to him.[17]

In most cases, there is no sharp dividing line between underconsumption theories and others. Some, though not all of them, might just as well be couched in terms of overproduction or overinvestment, monetary or 'real'—whereupon it becomes easy to see that they are but another branch of the plant-and-equipment tree. This is particularly clear in the case of the type of oversaving argument that was espoused by Hobson. Today most writers who see saving in the role of villain of the piece aver that the mischief arises from savers' not spending at all, either on current consumption or on 'investment goods': the problem then is to show why, having saved, people refuse to invest, thereby creating unemployment and pools of idle money.[18] But though Hobson notices this aspect of the matter he based, not quite logically, his explanation of cyclical fluctuations and of the incident unemployment upon an entirely different argument. With him saving produces alternating prosperities and depressions precisely because savers do invest promptly and thereby increase the productive powers of the economic engine beyond the possibility of sale at cost-covering prices. This line of reasoning may be labeled Overproduction-through-Saving and is certainly not Keynesian. But Hobson, like Tugan-Baranowsky before him, went on to point out that most saving is done by the relatively rich, and he used this fact to arrive at the proposition that the ultimate cause of cyclical disturbance and of the incident unemployment is the

[16] For such a glimpse, in the case of Marshall, see Keynes's *General Theory*, p. 19n.

[17] See above, ch. 5, sec. 2a. The two books that bear most directly on the subject of this section are: *The Industrial System* (1909) and *Economics of Unemployment* (1922).

[18] This way of looking at the matter is, of course, related to the fact that present-day analysis is primarily short-run analysis. In the short run, saving can create trouble only if savings are hoarded; if they are quickly disbursed in acts of investments, they sustain activity in the first instance; and their long-run effects do not enter into a short-run picture.

inequality of incomes. Therefore, we shall understand why economists who are interested in nothing but politically relevant results will hail Hobson as a forerunner of Keynes.[19]

Fifth, it is only for the sake of convenience that I put Marx at the end of our list of examples. In justice, he ought to have been put first because more than any other economist he identified cycles with the process of production and operation of additional plant and equipment.

Both followers and enemies have experienced difficulty in attributing to Marx any clear-cut theory of cycles. The obvious reason for this difficulty is that Marx did not live to systematize his ideas on the subject: his theory remained the great 'unwritten chapter' of his work. But there is another and more fundamental reason. His topic was capitalist evolution. Everything he ever wrote, even his scheme of a stationary society, was written to elucidate this topic. Capitalist evolution was to end in the breakdown of the system. But he early adopted the idea—it is already in the *Communist Manifesto*—that the current crises were previews of this breakdown, that is to say, the same kind of phenomenon that need only intensify itself in order to bring about definitive breakdown (the economic complement of the Revolution).[20] Therefore, *all* the elements of capitalist reality were, directly or indirectly, relevant also to his vision of the cyclical phenomenon. The 'unwritten chapter' would have had to sum up the *whole* of his analysis of capitalism. And the whole of this analysis in turn centered in (1) the production of 'real capital' and (2) in the factors that change its composition (relative increase of constant compared with variable capital[21]). These are the unifying conceptions to which must be referred what otherwise may easily appear to be disjointed and even contradictory hints. There are, of course, many of these, such as: capitalists' ineluctable craving for accumulation (regardless of return) that is to motivate bursts of investment activity—the weakest point, though buttressed by various suggestions about more substantial factors; the ever-present impulse that produces manias and crashes (vividly but superficially described by

[19] As Lord Keynes himself has pointed out (*General Theory,* ch. 23, VI), Gesell's claims to that honor are much stronger.

[20] This is why it was essential for Marx to assume, and if possible to prove, that crises would increase in intensity as time went on, a thesis that was abandoned by Hilferding (1910) and eventually also by Kautsky, who had put up the most elaborate defense of it in 1902. Most other cycle analysts of that period either did not pronounce upon the subject—which means, I take it, that they did not see any reason why depressions should grow either more or less severe—or were inclined to take the opposite view. It is important to bear in mind that this opposite view may mean two different things: first that the *fundamental* movement would decrease in amplitudes or, second, that people would learn to handle surface phenomena and effects (speculation, swindling, bank failures, shrinkage of expenditure owing to unemployment) so that the *observed* amplitudes would grow smaller though the underlying process remains the same. No such distinction was explicitly made, however, so far as I know, in any of the more influential writings.

[21] Constant capital is, of course, not the same as plant and equipment, but the rela tive increase in the latter is the salient point about that process.

Engels); the tendency of the rate of profit to fall (whether or not satisfactorily motivated); overproduction and anarchy (uncertainty) of capitalist decision; recurring periods of reinvestment (renewal of the physical apparatus of production) with periods of reduced activity to follow. There were others, among them a clear pointer toward underconsumption by the laboring masses as the 'last cause of all real crises' (*Capital*, vol. III, p. 568) and toward the consequent inability of capitalists to 'realize' the surplus value that 'exists' in the commodities that have been produced. Conflicting evidence makes it impossible, however, to impute to Marx an underconsumption theory of *cycles* though it remains possible to attribute to underconsumption a role in conditioning an ultimate state of stagnation.[22]

But none of these hints, taken by itself, nor their sum total amounts to a theory of cycles. So far as Marx himself is concerned, the historian of analysis, after having noticed the basic conception and also perhaps the particularly unsatisfactory handling of money and credit, must leave it at that. All the same, there are a number of Marxist cycle theories. But they should be attributed not to Marx but to their authors—Marxists who, either selecting hints that appealed to them more than others or trying to develop, from the Marxist basis, ideas of their own, provided substitutes for the 'unwritten chapter' rather than reconstruction of it—fully believing, no doubt, that they were interpreting Marx and always keeping in mind the cherished relation between the crises of experience and the ultimate catastrophe of capitalism. It is not possible to survey them in a sketch like this.[23]

(c) *Other Approaches.*

Though it is impossible to survey all the other ideas that emerged during that period about the nature and causation of economic fluctuations, it is both possible and necessary to point out that most of them, besides being suggested by untutored observation, were bound to appeal to economists who had developed economic statics as the centerpiece of their science. As we have seen above, they naturally exaggerated the importance of their central achievement. They saw more in it than do we, that is, more than a logical schema that is useful for clearing up certain equilibrium' relations but is not in itself directly applicable to the given processes of real life. They did not realize how many and how important the phenomena are that escape this logical schema and loved to believe that they had got hold of all that was essential and 'normal.' Now, from the standpoint of this type of

[22] The conflicting evidence is widely scattered. But see, e.g., *Capital*, vol. II, p. 476, where Marx avers that 'the share of the working class in the consumable product increases in the period preceding a crisis. The weight of this passage is enhanced not so much by the fact that Marx, a few lines before, declared the proposition that crises were caused 'by the scarcity of solvent consumers' to be 'purely a tautology,' as by the fact that the proposition follows logically from his own scheme.

[23] P.M.Sweezy's work, though in this matter somewhat impaired by an evident desire to turn Marx into a Keynesian, will again prove extremely useful as a help for further study. I will merely repeat names already mentioned: O.Bauer, Bukharin, Grossmann, Hilferding, Kautsky, Luxemburg, and Sternberg. The best analysis of Marx's own views that I know of is that by H.Smith, 'Marx and the Trade Cycle,' *Review of Economic Studies*, June 1937.

analysis, it is natural to locate the 'causes' of observed disturbances *either* outside of the economic system[24] *or* in the fact that the economic engine, like any engine, never works with precision. And this attitude toward observed fluctuations was the common root—or common characteristic—of another group of theories that also seem at first sight to have nothing to do with one another.[25] We shall notice three examples.

First, the most exogenous of all factors that influence economic life is variation of harvest in so far as due to weather, a factor pressed into service for the purpose of explaining business fluctuations by W.S.Jevons, H.S.Jevons (his son), and H.L.Moore.[26]

Second, the fact that the economic engine is likely to stall may be exploited for the purposes of business-cycle analysis in various ways. The most direct one is to attribute responsibility to uncertainty in general, which will result in 'erroneous' decisions. But since this uncertainty is, in many respects, due to the fundamental properties of the private enterprise economy, we may also directly accuse the latter's institutions.[27] And since individual errors cannot con-

[24] Factors that act upon the economic system from outside are called external or exogenous factors, theories that work with such factors, exogenous (as distinct from endogenous) theories. It should be borne in mind, however, that this concept does not carry as definite a meaning as it might seem to do. On the one hand, its content will vary according to what we include in the economic system: everybody excludes uncontrollable natural events, but not everybody will also exclude 'politics.' On the other hand, even if we exclude from the concept everything that is not covered by the theory of 'business behavior'—difficult though this is in such cases as central bank action and the like—the content of the concept will still vary according to whether we mean by endogenous processes such processes only as are uniquely determined by an initial situa tion (Tinbergen's meaning) or also such processes as are influenced by factors not present in the initial situation, e.g. unexpected introduction of new methods of production.

[25] Another group of theories that would overlap with ours also may be related to the unduly great confidence that the best theorists of the period placed in the equilibrium analysis. This group may be called the Disproportionality Theories and comprises theories that locate the source of cyclical troubles in 'maladjustments' as between different groups of prices and quantities. This idea comes naturally to anyone who accepts Say's law as a starting point of his analysis of cycles (*not* necessarily his general theory of the economic process) and is moreover easy to substantiate from observation of certain very obvious facts. A large number of economists could be quoted—though principally economists who were not specialists of business-cycle analysis—who were content to accept it. But I have not chosen this point of view for discussion, because Disproportionality remains an empty phrase so long as it is not linked with definite factors that are to account for it and because, so soon as it is so linked, those factors and not disproportionality per se will individuate an author's theory. As an example of an analysis that stresses certain types of disproportionalities—that are mainly due to lags—E.Lederer's *Konjunktur und Krisen* (in *Grundriss der Sozialökonomik,* Part IV, xi, 1925) may, however, be mentioned.

[26] W.S.Jevons' papers were reprinted in *Investigations in Currency and Finance* (1884); H.S.Jevons. *The Sun's Heat and Trade Activity* (1910); H.L.Moore, *Economic Cycles: Their Law and Cause* (1914).

[27] The reader will realize that this 'explanation' may easily degenerate into generali-

vincingly be held to produce *big* disturbances, unless they are overwhelmingly one way, we may put our trust in 'waves of optimism and pessimism,' a version that was quite common and later on was to appeal to such authorities as Pigou and Harrod.[28] There are many other variations of this theme, none of which is entirely void of a modest element of truth and all of which are unequal to the burden put upon them.

Third, so long as we do not see much ground for believing that the economic system produces general fluctuations by virtue of its own logic, we may easily conclude that these fluctuations arise simply whenever something of sufficient importance goes wrong, no matter for what reason. Roscher had already delivered himself to this effect, and no lesser man than Böhm-Bawerk once expressed the opinion [29] that there was no general explanation of either cycles or crises: they belong in a 'last chapter' of an economic treatise where all their possible causes should be listed. There is more in this opinion—I am inclined to believe that Marshall would have agreed with it—than appears at first sight, though Juglar's achievement suffices to show up its inadequacy. It takes account of, though it overstresses, the fact which is so often neglected by ardent 'theorists,' namely, that every cycle is a historical individual to *some extent* and that unique combinations of circumstances must enter largely into every analysis of a particular case. Moreover, it bars effectively all those single-factor explanations that rest on nothing but their author's pet aversions—such as saving or exploitation. Finally, it invites detailed study of individual mechanisms, which carries us a long way, though not the whole way. The bulk of what has been done on this line belongs, however, to the postwar period: the necessary analytic techniques were slow to develop.[30] [On these postwar developments, see below Part V, ch. 4, Dynamics and Business Cycle Research.]

All this—together with what has been said above in section 8—seems to establish our thesis: the essentials of both the methods and the explanatory principles that serve in today's business-cycle analysis, barring refinements of

ties that are as indubitable as they are empty. A classical example of this is the statement that 'the "cause"…of business cycles…is to be found in the habits and customs [institutions] of men which make up the money economy…' (L.K.Frank, 'A Theory of Business Cycles,' *Quarterly Journal of Economics,* August 1923).

[28] See Pigou's *Industrial Fluctuations* (1927) and Harrod's *Trade Cycle* (1936). In justice to both authors it must, however, be added that their important contributions to our understanding of cyclical phenomena are entirely independent of, and but little impaired by, their partiality to that theory. In England, Professor Robertson is its most eminent opponent.

[29] I am sure of this but am unable to provide the reference. If my memory serves me, he said it in a review. [Professor Haberler, who read this work in manuscript, suggests that J.A.S. is referring to Böhm-Bawerk's review of E.von Bergmann's *Geschichte der nationalökonomischen Krisentheorieen* (1895), *Zeitschrift für Volkswirtschaft, Sozialpolitik und Verwaltung* (vol. VII, 1898).]

[30] Several authors of the period under survey made, however, use of the 'principle of acceleration' (see Haberler, op. cit. pp. 85 et seq.). And there were several contributions that, though they passed unnoticed, foreshadow later developments. The 'hog cycle,' e.g., was discovered by S.Benner as early as 1876 *(Benner's Prophecies of Future Ups and Downs in Prices).*

technique, date from before 1914—an instance of continuity in development or of filiation of ideas that is all the more interesting because conscious effort was all the other way. Fairly satisfactory synthesis that would have left no major fact unaccounted for and would have constituted an excellent basis for further research was ‘objectively’ possible by then. Why was it not attempted? The answer seems to be that objective possibility is one thing and its realization quite another thing: no more than any other history can the history of research afford to neglect the personal element. Entangled in controversy that was often petty, enamoured of their own ideas and particular emphasis, economists plodded along successfully enough. But nobody rose to what would indeed have been a most difficult feat of leadership.[31]

In view of an entirely unfounded criticism that many of us are in the habit of directing against the work of that time, it should be added that economists did not fail to offer explanations of unemployment that were certainly not obviously inadequate. By going once more over the contributions that have been mentioned and scrutinizing them for their implications concerning unemployment, the reader can easily satisfy himself of this. Sectional and general, technological and ‘monetary,’ temporary and ‘permanent,’ types of unemployment were all in the picture that would have resulted from an effort at balanced synthesis—even our own mistakes were there. The indictment that the economists of that time disposed of all unemployment as merely frictional is true only if we adopt so wide a definition of friction as to render the indictment tautological.[32]

But another indictment stands against the vast majority of the economists of that period if it be indeed proper, considering the analytic situation in which they worked, to call it an indictment: with few exceptions, of which Marx was the most influential one, they treated cycles as a phenomenon that is superimposed upon the normal course of capitalist life and mostly as a pathological one; it never occurred to the majority to look to business cycles for material with which to build the fundamental theory of capitalist reality.[33]

[31] In the postwar period, Pigou (op. cit.) came perhaps nearest to accomplishing that feat.
[32] The indictment may be made more tenable by reformulating it to the effect that, without denying persistence of unemployment as a fact, the analysts of that period, and Marshall in particular, treated full employment as the ‘norm’ toward which the system incessantly ‘tended.’ If by the term ‘norm’ we mean a property of the logical schema of perfect equilibrium under perfect competition, the indictment fails, because it can be proved that within this logical schema there would in fact exist no involuntary un employment. If by the term ‘norm’ we mean a property of reality, namely, a tendency of the capitalist system, as it actually works, to approach full employment and to stay there until something occurs to drive it off the full-employment state, then it becomes true to say that the economists of the Walras-Marshallian type were inadequately aware of the qualifications subject to which existence of such a tendency may be asserted. At the same time, the indictment does not amount to more than this.
[33] [This, of course, is what J.A.S., himself, attempted in his monumental *Business Cycles: a Theoretical, Historical, and Statistical Analysis of the Capitalist Process* (2 vols., 1939) and much earlier in his *Theorie der wirtschaftlichen Entwicklung* (1912; 2nd rev. ed. 1926; English trans., *Theory of Economic Development,* 1934).