Volume Title: Wesley Clair Mitchell: The Economic Scientist

Volume Author/Editor: Arthur F. Burns, ed.

Volume Publisher: NBER

Volume ISBN: 0-87014-052-3

Volume URL: http://www.nber.org/books/burn52-1

Publication Date: 1952

Chapter Title: The General Economist

Chapter Author: Joseph A. Schumpeter

Chapter URL: http://www.nber.org/chapters/c3106

Chapter pages in book: (p. 321 - 340)

The General Economist*

By Joseph A. Schumpeter

Mitchell died on October 29, 1948—active to the last, "in harness," as he once wrote me he would be.[1] We mourn a character of singular purity, a fellow worker of firm convictions and at the same time of infinite gentleness, a teacher who was wholeheartedly devoted to duty, an incorruptible servant of truth who was impervious to all temptations, even those subtle ones that proceed from warm and elevated social sympathies, a leader who led by example and performance, without ever asserting his authority or indeed any claims of his own. The aura of such a personality can be, and has been, felt by all who came near him, but it is as difficult to put into words as is the wide range of his interests or the effective service he devoted to so many causes—to all of them with a profound seriousness which never succeeded in extinguishing the humorous twinkle in his eyes. We loved him and we know that we shall not meet his like again.

This is all I shall say about the man. For the rest, this memoir will be devoted exclusively to an attempt to survey his work and to formulate what it means to the scientific economics of our age, if it is indeed possible to separate the work from the man in the case of a scholar whose greatest contribution was the moral message which speaks to us from every page he wrote.[2]

* This article, completed by Professor Schumpeter only two weeks before his death, which occurred on January 8, 1950, was published in the *Quarterly Journal of Economics,* February 1950, under the title "Wesley Clair Mitchell (1874-1948)." Here reprinted, with only minor changes, by permission of both the author and the publisher.

[1] The unfinished manuscript, entitled *What Happens during Business Cycles,* on which he was working at the time of his death, has been mimeographed and communicated to the participants in the National Bureau of Economic Research Conference on Business Cycles that was held in New York, November 25-27, 1949. [This book was published in the spring of 1951.]

[2] For all that is thus lacking in this memoir the reader is referred to the large number of obituary tributes which have appeared. I wish to mention specifically

I

Is there anything in the theory that a man's position in the sequence of "generations" is determined by the influences that impinged upon him during his twenties? If there is,we should look for formative factors in the decade that preceded Mitchell's migration, in 1903, to the University of California. This decade of scientific adolescence centered in his work at Chicago where he took his Ph.D. in 1899. But he was of the oak and not of the willow: his own mental and moral texture—traceable, if you wish, to his New England background and an eminently healthy youth on the paternal farm—was presumably too strong to be greatly influenced by his teachers in economics, though a good course on English economic history and J. Laurence Laughlin's guidance in matters of money and currency policy did leave discernible traces. Veblen was much more to the taste of a mind that was nonconformist by nature, of a quick intelligence that resented dogma and stuffiness more than anything else, that preferred the paddock to the stable and thoroughly enjoyed, though rarely produced, sarcasms and paradoxes. However, before long he also took Veblen's measure and if for the rest of his life he continued to emphasize the difference between making goods and making money he soon tired of the glitter of the more dubious Veblenite gems. But John Dewey and Jacques Loeb opened new vistas that were never to pall. They opened the avenues to a social science much broader than professional economics in which he loved to dwell. This being important in order to understand Mitchell's economics and the nature of his

---

several memoirs by Professor Arthur F. Burns—particularly the one contained in the *29th Annual Report* of the National Bureau of Economic Research—and Professor Frederick C. Mills' memorial address at the 61st Annual Meeting of the American Economic Association (see *American Economic Review,* June 1949) to both of which I am indebted for various pieces of information (as I am also to several communications from Professor Burns) ; and the memoirs by Professor J. Dorfman (*Economic Journal,* September 1949) and Professor Kuznets (*Journal of the American Statistical Association,* March 1949). Also, the present memoir should be compared with Professor Alvin H. Hansen's in *Review of Economics and Statistics,* November 1949. A bibliography has been compiled by the National Bureau. [The papers by Hansen, Dorfman, Mills, and Burns are included in this volume, as is the bibliography; all page references to these papers follow the pagination in this book.]

personal contribution, let us call a halt in order to cross a few t's and to dot a few i's.

The 1890's were the first of the three decades of what may be called the Marshallian epoch. However, since not every reader, and especially not every American reader, will agree with all that this phrase implies, let me spell out what I mean by it. Three tendencies then came of age and produced the New Economics of 1900. There was first a novel preoccupation with, and a novel attitude toward, problems of social reform, best exemplified by German *Sozialpolitik*. Second, economic history, amidst surf and breakers, established itself within the precincts of academic economics. Third, a new organon of economic theory—it is really difficult to decide which of the names affixed to it, marginalism, neoclassicism, etc., is the least misleading one—came into its own after a struggle which had lasted for a quarter of a century. But, with the possible exception of England where Marshall's leadership succeeded to some extent in uniting them all, those three tendencies were at war everywhere not only among themselves but also with the views and methods of a preceding period to which large parts of the national professions clung tenaciously. In the United States in particular, where the economic profession enjoyed tropical growth, the backward glance discerns little else but the outmoded textbook—improved no doubt by the work of such men as F. Walker but outmoded nevertheless—and, for the rest, chaos—fertile chaos perhaps, but still chaos. Without meaning disrespect to forgotten or half-forgotten worthies, we can easily understand that a youngster entering the Chicago department of economics around 1895 found nobody there to show him the wealth of ideas and research programs that lives under the smooth surface of Marshall's *Principles*, the only work from which Marshall's teaching could have been learned then without going to Cambridge and listening to him.[3] And it would have taken a teacher of supreme ability to present, in 1895 or even later, J. B. Clark's teaching in any really useful manner. So *Sozialpolitik* went

[3] For that matter—how many people know *now* what Marshall's critical presentation of the "doctrine of maximum satisfaction" did to the scientific basis of *laissez faire?* Or how much Marshall did to pave the way for modern econometrics?

by default, economic history remained on a side track, the new theoretical organon was easily disposed of as "marginalism" or "neoclassicism," and the dry-as-dust textbook—more or less shaped on the Millian model—triumphed to drive more active minds into "institutionalist" revolt.[4]

The curve on which Mitchell's own work was to move can, I believe, be readily interpreted as the intersection of two surfaces: one which represents these environmental conditions and another which represents the propensities of his own mind. A man of his ability was bound to be dissatisfied with the state of things he beheld, a man of *his type of ability* was bound to look for the remedy in the ocean of social facts of which economists seemed to him to absorb but a few miserable inlets. He wanted to swim and not to wade, to explore and not to turn round and round on a small piece of arid land. And two more points will finish off the picture. First, he was as suspicious of logical rigors as the colt is of bridle and saddle and soon spied behind the work of the tillers of that arid plot not only unrealistic "postulates" framed for the sake of methodological convenience and to be discarded at will, but also "preconceptions" (ideologies) which enslave the research worker instead of serving him.[5] Second, quite apart from this, his type of mind was not made to enjoy or to appreciate what he called "playing" with the postulates: the work on this arid ground was vitiated by political prejudice or metaphysical beliefs; but even if it had not been, it would still have seemed to him otiose.

If this defines the institutionalist position, then Mitchell was and always remained an institutionalist. I do not wish to enter the discussion about the precise meaning of that elusive concept, a discussion that still flares up from time to time and has produced such gems as the statements that Veblen was no institutionalist at all or that he was the only one. This would be the more unprofitable because everyone who participated in the "revolt" alluded to above filled in the blanks left by its essentially negative criticism with a

[4] In Mitchell's case, there was a year of study in Halle and Vienna to interrupt his work in Chicago. But it left no visible mark. And—again without disrespect to anyone's memory, least of all to that of the great Menger—this is what we should expect.

[5] For a characteristic quotation see Mills, pp. 112-13, notes 4 and 5.

positive program of his own. But Mitchell's own methodological position can and must be scrutinized more closely both because of the outstanding importance of his work and because it has repeatedly, and even recently, been discussed in a manner that seems to me not entirely satisfactory. We have to consider three different things: Mitchell's views on the proper attitude of the scientific economists toward "policy"; his views on the proper method of protecting the scientific result from ideological vitiation; and his views on "theory." His opinions on all three subjects changed but little throughout his adult life. And we may conveniently survey them now.

II

As regards the first, his practice is a shining example to all of us. Like other institutionalists, he resented the political alliance that existed between the economics of his formative years and *laissez-faire* liberalism. But he was one of the few who did so for the right reason. Although social sympathies and a sense of the practical inadequacy of straight *laissez-faire* programs presumably contributed to making him averse to that particular alliance, it is much more important that he felt that economists had no business to enter any such alliance. Economics was to be an objective science that puts a storehouse of carefully ascertained facts and inferences from such facts at the disposal of anyone who cares to use them. This did not induce him to shut himself up in an ivory tower. On the contrary he was always ready to render public service whenever called upon to do so. His work with the Immigration Commission in 1908, with the Bureau of Labor Statistics and the War Industries Board during the First World War, and later on, his work as chairman of President Hoover's Research Committee on Social Trends (1929-33), as a member of the National Planning Board (1933-34), of the National Resources Board (1934-35), and as chairman of the Technical Committee on the Cost of Living (1944) are sufficient proof of this. But the nature of this work only serves to bear out my point; it always fell in with his conception of his scientific mission—always consisted in observing and interpreting the facts of a situation, in presenting objectively what was actually happening. In cases where ends may be taken for granted he did not fight shy of practical recommendations. But he never went beyond the reserve that, like him, I think appropriate for the man who devotes himself to the analyst's task, and he never peddled any recipes, never advocated "policies."

As regards the second point, the ideological danger, his very awareness of it must be recorded as a signal merit. The only questions that can arise in this connection are, on the one hand, whether he was not too prone to suspect ideology ("preconceptions") in authors whose methods and results he did not approve; and on the other hand, whether the remedy he invoked was adequate. Thus, there are plenty of shortcomings in Ricardo's analysis; but if we neglect his policy recommendations and take account of the level of abstraction on which he moved, we do not find many ideologically vitiated statements—as Karl Marx readily recognized. And Mitchell's remedy—careful and "objective" investigation of facts—will indeed destroy many preconceptions but not all; no amount of care will protect research from the evil spirits that dwell in the investigator's very soul and never announce themselves to him. Never mind—this does not alter the fact that Mitchell was one of the very few economists who have seen the problem in all its depth and who have realized that preconceptions in our field are no mere matter of political prejudice or of sponsorship of some special interest.

The third point, the subject of "Mitchell and Economic Theory," presents much greater difficulties than the two others. In part, these difficulties proceed from an ambiguity in the meaning of the word. When in his main publications on business cycles, Mitchell, while listing a large number of theories of the phenomenon and declaring his readiness to avail himself of any *suggestions* they might convey to him, made it quite clear that he did not propose to ally himself with any one of them or to fetter himself by constructing one of the same type for his own purposes, he clearly used the word "theory" in the sense of "explanatory hypothesis." And what he meant may be expressed by the unchallengeable statement that such a hypothesis should result from, or be suggested by, detailed factual study rather than be posited at the start of the investigation. Fairly interpreted, this is a tenable position, and in particular not open to the objection that such a program is logically impossible because, in any case, we must first identify the phenomenon to be investigated and in doing so must inevitably introduce elements that will exert some guiding influence upon our factual research; in other words that there is no such thing as factual investigation, or, in particular, "measurement" without any "theory" at all. This is also true; but when we say it we become aware of the fact that we are now using the word "theory" in a different sense, namely in the sense of "conceptual tool." And in this sense Mitchell certainly did not wish to exclude "theory" from any stage of either his own or anyone else's work. This will be illustrated as we proceed. But it is not all.

Though Mitchell never committed the absurd mistake of objecting on principle to the use of conceptual tools or schemata, he did object to the ones that were actually in use in the "classic" literature with which he included also the postclassical literature available in his formative period.[6] And this for two reasons, one of which is closely connected with his personal achievement as a leader of economic thought, and the other of which indicates a limitation that prevented his achievement from extending his leadership over a still wider domain.

He strove no doubt to widen the frontiers of economics so as to include the province that is best called Economic Sociology—the analysis of social institutions or of "prevalent social habits."[7] The institutions of the "monetary" (capitalist) economy were not to be accepted as data—though changeable ones—from other disciplines, but were to be made part of the economist's research material. But the essential point was that he did not think of this material, or of generalizations therefrom, as a complement to tra-

[6] By classic literature I mean the publications of the leading English authors from 1776 to 1848. As regards the literature available in his formative period, we must not forget that Walras (except perhaps the dubious philosophy that surrounds the core of Walras' work) hardly existed for him and that Marshall's teaching, as indicated above, never became a living reality to him.

[7] The practice of discussing social institutions together with the economic processes that, controlled and controlling, take place within them, may be traced to the scholastic doctors and to Aristotle. J. S. Mill devoted about one-third of his *Principles* to what I call Economic Sociology above. But the subject had become dry and unprogressive, at least in this country, when, under the influence of Veblen, Mitchell attempted to infuse new life into it.

ditional theory but as a substitute for it. The theory of the economic process itself was to remain a theory, but it was to become a theory built from the results of detailed observation of actual behavior and—since he did not exclude on principle either introspection or psychological interpretation inspired by introspection—motivation. We shall readily understand why this approach should have led Mitchell to look upon economic life as a process of change, and why from this standpoint the analysis of business cycles should have appeared to him as the first step toward realistic analysis of the economic process in general. We shall not wonder at, but on the contrary admire him for, his emphasis upon sequences that characterized his thought from first to last. And we shall hail him —that is, the Mitchell of before 1913—as a forerunner of modern dynamics. But, having applauded his premises, we shall question one of the conclusions he drew from them, namely, that the economic logic of what he agreed with others to call the neoclassical theory should therefore go overboard.

When we study the mimeograph[8] of his famous course on the history of economic thought—*Types of Economic Theory* which I hope to see published some day—we are struck by the fact that he objected to his authors' "postulates" quite as much as he objected to their "preconceptions." Up to a point he was right once more: quite obviously, logical schemata or models are not the whole of economics or even of economic theory in his sense and in addition there is plenty to criticize in the manner in which these models have been set up and in the postulates or assumptions that are basic to them. But Mitchell did not object to individual postulates—or complete models—in order to replace them by others. He objected to them *qua* postulates or models and shrugged his shoulders at the people who were concerned about such questions as their determinacy and consistency. And he thought that "my grandaunt's theology; Plato and Quesnay; Kant, Ricardo and Karl Marx; Cairnes and Jevons, even Marshall were much of a piece."[9] It should be superfluous, at this hour of the day, to dwell on the error involved in this or to point out precisely where a

[8] Brought out under the title *Lecture Notes on Types of Economic Theory* (Augustus M. Kelley, 1949).

[9] Quoted from Mills, p. 112, note 3.

fundamentally sound methodological instinct drove him into error. The simple fact is that it takes many types of mind to build a science; that these types hardly ever understand one another; and that preference for the work one is made for easily shades off into derogatory judgments about other work which is then hardly ever looked at seriously. But it is not superfluous to point out the damage this attitude did to Mitchell's work and to the range of its influence. His aversion to making his theoretical schemata explicit makes it difficult for any but the most fervently sympathetic interpreter to see that they are there—the basic idea of his book of 1913 could be put into a dynamic schema that even enjoys the property of "completeness"—and such passages as that in which he disposed of the static theory of equilibrium as a "dreamland" makes it easy for any not-so-sympathetic critic to renounce his leadership on the ground that evidently he failed to grasp its meaning or the nature and meaning of models in general. He never would listen to the argument that rational schemata aim at describing the *logic* of certain forms of behavior that prevail in every economy geared to the quest of pecuniary gain—a concept he understood so well—and do not at all imply that the subjects of this rationalistic description feel or act rationally themselves. And I shall never forget his speechless surprise when I tried to show him that his great book of 1913, so far as the bare bones of its argument are concerned, was an exercise in the dynamic theory of equilibrium.[10] I am not writing these sentences in order to discount the fame of a man whom I not only loved but also admired. I am writing them simply in order to remove what I believe have been misunderstandings on all sides and to open up the way to him for a still larger crowd of potential followers.

III

We turn to the core of his work. The first thing to strike us is its imposing unity. It may have been a happy coincidence that Laugh-

[10] For what else are his "recurring readjustments of prices" to which he returned again and again but imperfect movements of the economic system in the direction of a state of equilibrium? If he failed to avail himself of the apparatus of equilibrium theory, so the (successors of the) builders of the equilibrium theory failed to avail themselves of his facts.

lin suggested to him the Greenback episode as the subject of his doctor's thesis. But, apart from the implications of the fact that the wilful candidate accepted the suggestion, it seems safe to suppose that Mitchell would have found the way to his Rome whatever starting point he might have chosen. In his hands, that subject became an investigation into the economic processes of the Greenback episode—of the ways in which the processes reacted to the impact of war finance, and to which the effects of the Greenback issue themselves were but an approach. The fact that, following Laughlin's teaching, he gave a bad grade to the quantity theory—which he was soon to modify[11]—is a minor matter. The really important thing to notice in the two works that grew out of this thesis[12] is the vision of the monetary—or "capitalist"—economy which they reveal. On the one hand, he integrated the monetary phenomena with the rest, thus anticipating tendencies that have asserted themselves of late; and, on the other hand, he analyzed the relations that bind "prices together in a system of responses through time"[13] which led him quite naturally to the study of business cycles as a first step toward a general *theory* of the money economy of today, his real topic throughout his adult life.[14]

The volume on *Business Cycles* that appeared in 1913 had been simmering since 1905 though the conscious resolve to write a

[11] An almost unqualifiedly negative verdict upon that "theory" was rendered in what I believe was Mitchell's earliest publication, "The Quantity Theory of the Value of Money," which he contributed to the *Journal of Political Economy,* March 1896, while still a student. It is characteristic of the man that he amended that verdict and condemned his early notions on the subject before long ("The Real Issues in the Quantity Theory Controversy," *ibid.,* June 1904).

[12] *A History of the Greenbacks, with Special Reference to the Economic Consequences of Their Issue: 1862-65* (1903); and *Gold, Prices, and Wages under the Greenback Standard* (1908).

[13] See Burns, p. 13.

[14] This important point had better be established. Reference to Burns (pp. 19-21) suffices for this purpose. Mitchell conceived the plan of a Theory of the Money Economy, and began to work out its "skeleton," in December 1905. Professor Burns' quotation from a letter of that date makes it quite clear that he set about it in the true Mitchellian fashion which made the study of business cycles a necessary *Vorarbeit* for the larger plan.

treatise on this subject seems to have been made only in 1909.[15] It is a landmark in the history of American economics—though its influence upon scholars spread far beyond the United States—and cannot be praised too highly. The product of its author's prime, of the span of years when freshness and vigor are unimpaired as yet but already matched by analytic experience and wide acquirements, it was both his masterpiece in this word's original meaning—the piece of work by which the medieval journeyman proved himself to be a master of his craft—and the code that embodied the law of all the work that was to follow.[16] The essentials of the plan of the book reappear in the volume of 1927. Even *Measuring Business Cycles* (1946) carries out, on a higher and wider plane, part of the ideas that first saw the light of publicity in 1913. Even most of the work of the National Bureau of Economic Research is in very truth their lengthened shadow.[17] Both the methods and the results of 1913 stood the test of the huge amount of research that was brought to bear upon them, although Mitchell, in his single-minded devotion to truth, always stood ready to modify them.[18]

Having defined, as best I could, the place of *Business Cycles* in Mitchell's individual evolution, I have now to define its place in the evolution of the science. This task I approach with considerable diffidence. First, as pointed out before, Mitchell's creative

[15] See Burns, p. 21; Mitchell was then 35.

[16] The reader will understand that this is meant to apply to his essential work only and not to all the parerga. But it applies more widely than one might think at first sight. The two most important exceptions, Mitchell's work on index numbers and in the field of the history of economic thought are readily seen, the first as a part of the general program outlined—and indeed already carried into effect, to some extent, in the book of 1913—the latter as the critical complement (see below, p. 338) of his positive work. And even most of the parerga are elements in the great mosaic.

[17] This turn of phrase is a slightly transformed version of Professor Mills' "... the National Bureau of Economic Research, an institution which in very truth is the lengthened shadow of Wesley Mitchell" (F. C. Mills, p. 114).

[18] The most important change in method consisted in what is known as the National Bureau method of time-series analysis (see below, p. 339). The most important modification of results consisted in the diminishing emphasis that he came to place upon the role of increasing costs in bringing prosperities to an end and of decreasing costs in stimulating recovery.

efforts were not simply directed toward the cyclical phenomena *per se,* but rather toward a new economics—or as he himself said, a new economic theory—to be inspired by the "ideas developed in the study of business fluctuations."[19] This makes his work incommensurable with the work of most students of business cycles. Second, like the majority of creative workers, Mitchell did not easily come to grips with the work of people who were, or seemed to him to be, widely removed from him in attitudes or methods. He was the most generous of men. He read widely. But, preoccupied with his own task at which for prolonged spells he worked with all but feverish zeal, he did not easily penetrate beyond a certain level into structures not his own. This makes it necessary, in justice to his mental stature, to fall back upon a distinction the necessity of which has often impressed itself upon me in my researches into the history of economic analysis—the distinction between subjective and objective priority. And third (as in the case of the discovery—or invention—of the calculus, and many similar ones) there is the fact that men's minds, at any given time, are apt to converge in similar views but in such manner as to make these men—and their pupils—see secondary differences between one another more clearly than the essential similarities. In the case before us, workers were under the impression that the number of different "explanations" was increasing, whereas the fact is that a certain family likeness in their conceptions of the problem—of cycles versus "crises"; their methods—involving increasing appeal to statistical material; and their results—such as emphasis upon a generalized form of what we call now the acceleration principle, became more strongly marked all the time. No one author led in this movement and none seems to have been greatly influenced by the others. But the date of Mitchell's volume assures to it an outstanding position in the history of the movement.[20]

[19] *Business Cycles: The Problem and Its Setting* (1927), p. 452.

[20] To mention but a few others: Aftalion's work, written in a kindred spirit as regards method though differing from Mitchell's in a few interpretative *nostra,* appeared also in 1913; Spiethoff's, though foreshadowed in some articles published during the first decade of this century, was not available in any well-rounded form and did not reveal the massive basis of fact on which it rested until 1925; Pigou did not definitely reveal his affinity to Mitchell's approach until 1927; D. H. Robertson not until 1915; Cassel (whose explanation

There was of course a forerunner to all these authors, Clément Juglar—the great outsider who may be said to have created modern business-cycle analysis. So far as Mitchell is concerned, Juglar was his forerunner in theory as well as in method. Not only did he write a "great book of facts" which spurned contemporaneous theory and made clear the necessity of passing from "crises" to "cycles,"[21] but he also indicated with truly Mitchellian reserve important principles of interpretation which he believed to rise directly from observation and which culminated in the famous dictum: the only cause of depression is prosperity, or, if I read this sentence aright, depression is the reaction to what happens in prosperity. This seems to me to be the first, though partial, formulation of the theory that every phase of the economic process engenders the next phase and that, in particular, stresses which accumulate in the system during prosperity lead to recession (which in turn creates the conditions for a new spell of prosperity). Mitchell, who independently adopted a similar schema, did not hesitate to call it a "theory" (see, e.g., *Business Cycles,* p. 583, or Burns résumé, p. 25), and this is exactly what it is if we take the term in its proper—that is, instrumental—sense: a schema that must derive justification, if at all, "in an independent effort to use it in interpreting the ceaseless ebb and flow of economic activity." And it formulates one of the two—there are only two—fundamentally different groups of cycle theories. There is the "theory" that the economic process is essentially nonoscillatory and that the explanation of cyclical as well as other fluctuations must therefore be sought in particular circumstances (monetary or other) which disturb that even flow. Marshall stands out in the large crowd that represents this "hypothesis." And there is the "theory" that the economic process itself is essentially wavelike—that cycles are the form

---

acquired different traits later on) not before the publication of his treatise on general economics. Professor Haberler calls Tugan-Baranowsky a forerunner of Spiethoff (*Prosperity and Depression,* 1941, p. 72), but I prefer to exclude him from this group. Let me emphasize that I am not trying to discount the theoretical differences within it. Their affinity in spirit and approach is all that I wish to emphasize.

[21] See Mitchell's own comment in the volume of 1927, pp. 11-12, where Mitchell also noticed Wade, Overstone and others who paved the way toward this step, but not Marx.

of capitalist evolution—the theory to which Mitchell was to lend the weight of his authority. I think it may be said that he went a step further than this: on the ground that the capitalist economy is a profit economy in which economic activity depends upon the factors which affect present or prospective pecuniary profits—equivalent, I believe, to the Keynesian marginal efficiency of capital—he declared that profits are the "clue" to business fluctuations, which seems to tally substantially not only with the "theory" adumbrated in Chapter 22 of Keynes' *General Theory*[22] but also with the theories of a group of business-cycle students that is almost as large as the group that looks upon cycles as inherent in the capitalist process. Beyond this Mitchell did not commit himself. In particular he did not go on to say that profits are evidently—somehow, but in any case closely—connected with the processes of investment. But even so we have before us a definite, if only verbal, schema that stands at the back of his factual work. If this schema seems to be less in evidence in the last stage of his work this is because the end caught him in midstream, that is, in the "factual" phase of his work and before he was able to co-ordinate the fruits of his labors completely.

Exactly like the volume of 1927, the one of 1913 starts with a brief survey of existing explanations. In both cases, they are presented, to say the least, succinctly and with a surprising detachment. Mitchell found them all "plausible" but also "perplexing." He classified them, but without attempting to criticize them systematically. Though he raised an objection here and there, the reader gets the impression that he looked upon them as so many statements of partial truths each of which was pretty much as good as any other and all of which had, on a common plane, to await trial in the court of facts. This impartiality also reveals one of the characteristics of Mitchell's methodological bent that has been mentioned above: for him there was nothing, or at all events nothing important, *between* the explanatory hypothesis and the facts; there was, in particular, no logical criterion that might rule out a

[22] There are differences no doubt that are emphasized by the reserve of one of the authors and the trenchancy of the other. But the "clue" or proximate cause of cyclical fluctuations is in the element of profits for both of them.

theory before it came up for factual trial. But, given Mitchell's distrust of "neoclassical" economics, such impartiality had its virtues. And it did not, as has been repeatedly stated, leave him without a compass for his voyage across the ocean of statistical facts.

Also like the volume of 1927, the one of 1913 next unfolded Mitchell's vision of the money economy. In both cases, these chapters are in fact introductory treatises on general economic theory as he conceived it. Closely knit and unadorned, lacking effective conceptualization, they have never received their due. To mention one example only: how many people know that the theory of money flows, which these chapters indicate rather than present, anticipates much of what is best in modern income accounting and aggregative analysis? And of course we have here the "theoretical background" that so many critics miss and which is further developed in Part III of the 1913 volume.[23] No doubt, this background exposition needs amplification and, in addition, the editorial services of a professional theorist. But it is a great performance all the same.

Part II of the 1913 volume, however, needs no editing by anyone. It is a gem and a pioneer achievement. Mitchell not only knew how to use statistical material but also how to develop it—how to get what he wanted, even if it was not already there. Perception of a need that proceeded from a comprehensive vision; diagnosis of the available means to satisfy it; and attack upon the problem—these things must have followed one another between 1909 and 1913, with the speed of lightning. Many men have had comprehensive visions. Many men have had a passion for detail. But he was one of the few to whom it is given to harness their visions into the service of their work on detail, and their passion for detail into the service of their visions.

[23] This Part III, reprinted in 1941 under the title *Business Cycles and Their Causes,* contains several points which, or the importance of which, Mitchell ceased to believe in later on. Nevertheless, in writing it he came as near to a fully articulate rendering of his *theory* of *the* business cycle as he ever did. The unpublished manuscript mentioned (note 1) is not only incomplete; it is the product of an uphill fight against unmanageable masses of material and against time.

IV

For the rest, no more need be said here about the volume of 1927 except that much more definitely than the volume of 1913 it was in the nature of a survey of work done and of a program for work to be done.[24] His labors during the years from 1909 to 1913 had taught him that the huge task he had attempted to accomplish was altogether beyond the possibilities of singlehanded effort. His activities during the subsequent years that produced, among other things,[25] his investigations into the subject of price and production index numbers,[26] taught him that he was gifted, as few people ever have been, for the task of leading teams in which, though he knew how to keep direction, he always participated as a fellow worker—throwing his mind into the common pool and spreading the spirit of intellectual fellowship. And so, quite naturally, in 1920 this work issued into the work of the National Bureau of Economic Research of which he was one of the founders and, to his death, the moving spirit, the kindly leader who led but never drove, who inspired but never crushed the initiative of his associates. This "bold experiment" was an act of self-realization. Its unqualified success is a monument to his intellectual and moral qualities.

The Bureau produced, and from the outset planned to produce, a series of investigations, starting from the famous study on the size and distribution of national income, which in appearance went far beyond business cycles and topics closely related to business cycles.[27] But Mitchell's conception of the phenomenon encompassed the whole of the economic process and thus made all that

[24] The reader is referred to my review article, "Mitchell's Business Cycles," *Quarterly Journal of Economics,* November 1930.

[25] The most important of the studies that should but cannot be noticed here were republished by Professor Joseph Dorfman in the volume entitled *The Backward Art of Spending Money.*

[26] See especially Bulletins No. 173 and 656 of the Bureau of Labor Statistics. *The History of Prices during the War,* a series of publications of the War Industries Board, was edited by Mitchell, who contributed himself the bulletin on *International Price Comparisons* and the *Summary.* The latter contains his production index.

[27] For details, see the annual reports or at least Professor Burns' brief story, p. 30, *et seq.*

happens in it relevant to the "theory" of business cycles. Considerations of means and opportunities determined only the time sequence of the individual projects, all of which had their place in his comprehensive plan. This must be kept in mind in any appraisal of Burns' and Mitchell's *Measuring Business Cycles* (1946).

The authors of this volume do not profess to have written a treatise on business cycles but only to present a "plan for measuring business cycles" or rather of the Economic Process in Motion. This "declaration of intention" fits the first eight chapters better than the remaining four (which deal with results rather than mere measurements) but I prefer to formulate the contents of the book somewhat differently: the aim is to make the phenomenon stand up before us and *by so doing to show us what there is to explain.* This endeavor is presided over by a set of analytic decisions which constitute an improved version of the ones we find in the volume of 1913 but which can hardly be called a definition. Here they are: "Business cycles are a type of fluctuation found in the aggregate economic activity of nations that organize their work mainly in business enterprises: a cycle consists of expansions occurring at about the same time in many economic activities, followed by similarly general recessions, contractions, and revivals which merge into the expansion phase of the next cycle; this sequence of changes is recurrent but not periodic; in duration business cycles vary from more than one year to ten or twelve years; they are not divisible into shorter cycles of similar character with amplitudes approximating their own."[28] There is a lot of "theory" in this, besides anticipation of several subsequent factual findings. The last sentence, in particular, boldly adopts a single-cycle *hypothesis* which makes it difficult to distinguish different kinds of fluctuations, the existence of which is not a matter of hypothesis-making but of direct observation.[29] However this and other points are, to some

[28] *Measuring Business Cycles,* p. 3.

[29] The second sentence seems to suggest that there is some point in recognizing four cyclical phases. As we shall see, this suggestion is not embodied in the pattern of the cyclical stages subsequently adopted. The reader will realize that Mitchell's old aversion to the use of the equilibrium concept—or even to its counterpart in the world of business, the "normal state of trade" which he

extent, matters of individual judgment and expository convenience, and we shall not go into them any further.

From Mitchell's general point of view it was right and proper to analyze *all* the time series—over a thousand—that the united forces of the National Bureau were able to unearth and to treat. For business cycles, considered as the form of the capitalist process, are of necessity "congeries of interrelated phenomena" coextensive with that process itself, and even if it were possible to imagine an element that has, in itself, nothing to do with cycles, it would still be necessary to investigate how it is affected by the cyclical movement.[30] If nevertheless, and in spite of all the qualms about the theoretical considerations involved, it proved necessary to make selections—as, e.g., in the four last chapters of *Measuring Business Cycles*—this was a concession to the limitations of the means available and not a matter of principle. However, Mitchell was well aware that even the most complete array of statistics would not do what he wanted. So, in order to check as well as to light up his statistical material and the inferences to be drawn from it, he hit upon the idea of collecting what he called business annals, as far back and for as many countries as possible. The well-known book by W. L. Thorp (1926) was the result. In a statistical age, the methodological merit in this recognition of the importance of non-statistical historical material cannot be emphasized too strongly. Though, as the years went by, Mitchell's confidence in this source of information seems to have decreased, and though it has been inadequately exploited from the first, it still redeems his work from the statisticism that threatens to swamp the field.

---

declared to be a "figment" in the volume of 1927, p. 376—may be the reason, or one of the reasons, for this. For the four-phase pattern has in fact little value unless we interpret expansions (prosperities) and contractions (depressions) as movements away from, and recessions and revivals as movements toward, comparatively equilibrated (and in this though in no other sense, "normal") conditions.

[30] Mitchell's conception of a cyclical situation may, I think, be best rendered by an analogy. The members of a family circle produce a certain moral atmosphere which, in a sense, is the result of their individual behavior. But nevertheless this atmosphere, once created, is in itself an objective fact that in turn influences the behavior of the members of the family: the members of the National Bureau's family of time series jointly produce the cyclical situations, but they are all of them also being shaped by the existing cyclical situation.

By now, everyone is familiar with what has come to be called the National Bureau method. Nevertheless, the ingenious idea that underlies this representation of cyclical behavior should be restated once more. On the one hand, every series, corrected for seasonal fluctuations, is treated by itself and its average behavior during its own expansions and contractions is brought out (specific cycles): each such cycle, identified by marking off the troughs and peaks in the series, is divided into intervals or stages for which the values of the series are expressed as percentages of its average value for each cycle—a judicious compromise between eliminating trend and leaving it in—and the averages of these percentages then serve to draw a picture of the typical specific cycle of the series. On the other hand, in order to display the behavior of each individual series in periods of expansion and contraction of the whole economic system, dates are derived for the peaks and troughs of general business activity, both from the approximate "consensus" of all series included and from the nonnumerical information presented in the business annals. The behavior of each series is then studied in each of the (nine) intervals or stages into which this "reference cycle" is divided, the "standing" of the series in each stage of its reference cycle being also expressed as a percentage of its average value during the whole reference cycle. The typical reference cycle of the series is produced by averaging the standings of the series in each stage of all the cycles covered. The comparison of the specific and the reference cycles of each series is perhaps the most illuminating of the operations or measurements possible within this schema. This dual representation of (potentially) every bit of statistical information is extremely well devised in order to marshal business-cycle facts so far as this can be done without postulating a priori any particular relations between them. Even so, many a Gordian knot had to be cut. And the engine naturally works with greater friction in the last four chapters where a sample of seven relatively long time series is made to bear a heavy burden of concrete inferences. But the purpose of presenting facts so as to make it possible to confront them with theories stands out impressively throughout.

Of course, this volume was only a beginning. And if Mitchell had been able to complete his unfinished manuscript, this also would have been no more than a beginning. Work of this kind has no natural end and of necessity always points further ahead into an indefinite future. This is true of the whole of the work of Mitchell's life. And it is this which makes its greatness and defines its unique position in the history of modern economics. Here was a man who had the courage to say, unlike the rest of us, that he had not all the answers; who went about his task without either haste or rest; who did not care to march along with flags and brass bands; who was full of sympathy with mankind's fate, yet kept aloof from the market place; who taught us, by example and not by phrase, what a scholar should be.