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J. Daniel Hammond
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Introduction

In 1948 Milton Friedman and Anna J. Schwartz embarked on a National Bureau of Economic Research study of monetary factors in business cycles. According to a brief prospectus written by Friedman at the start of the project, their objectives included investigation of "the *causal role* of monetary and banking phenomena in producing cyclical fluctuations, intensifying or mitigating their severity, or determining their character ("Brief Statement," undated, p. 1, emphasis added). Their plan was to complete the project in three years, however, it continued for over three decades. It produced, among other publications, "Money and Business Cycles" (1963a), *A Monetary History of the United States, 1867–1960* (1963b), *Monetary Statistics of the United States: Estimates, Sources, and Methods* (1970), and *Monetary Trends in the United States and the United Kingdom: Their Relation to Income, Prices, and Interest Rates, 1867–1975* (1982).

The concept of causality carried no particular significance for Friedman and Schwartz in 1948. The pursuit of understanding money's role in business cycles, though a formidable challenge, seemed naturally and unobjectionably to call for analysis of causes and effects. As David Hume, himself a prominent eighteenth-century monetary economist as well as philosopher, noted, causality is the "cement of the universe." Our attempts to understand the material and social world around us through science are almost always attempts to sort out causes and effects and often then to gauge their magnitudes. Furthermore, the record of science reveals substantial success in attempts to do this sorting and measuring.

For Friedman and Schwartz, causality was to become a snare, or rather an issue that their critics would use to ensnare them. Throughout the project, their work in monetary economics was controversial, with a great part of the contention centered on their identification of causes and effects. Their quantity-theory conclusions, which assigned an important causal role for money and for the Federal Reserve, contrasted starkly with the prevailing Keynesian view that monetary policy lacked power. This difference between Friedman and Schwartz and their critics in *conclusions* about money's causal role is readily apparent from even a cursory look at their books and articles and their critics' reactions to them.

Two other dimensions of Friedman and Schwartz's monetary economics also made their work distinctive and controversial. Their National Bureau business-cycle analysis techniques, or *methods,* were unorthodox and came under attack as the econometrics revolution swept through the profession. Friedman and Schwartz began their project just after Tjalling Koopmans's influential and scathing review (1947) had tagged Arthur Burns and Wesley Mitchell's National Bureau book *Measuring Business Cycles* (1946) as "measurement without theory." The third factor contributing to the controversy surrounding their work was the unorthodoxy of Friedman's Marshallian *methodology* in a macro- and monetary economics landscape dominated by the neo-Walrasian IS–LM analysis of John Hicks.[1]

Their distance from the economics mainstream on each of these three dimensions, their conclusions, their methods, and their methodology, made Friedman and Schwartz particularly susceptible to causality challenges. The chapters that follow will show that the clash between National Bureau and econometric methods and between Marshallian and Walrasian methodology were more fundamental factors in the disputatious reception of Friedman and Schwartz's substantive conclusions about money's role in business cycles than the conclusions themselves. Moreover, the unorthodox character of Friedman's methods and methodology help explain a wariness about causality that Friedman developed over the course of the monetary research project.

Friedman's original openness to the conception of their work as sorting causes and effects did not last. By 1964, when he prepared a summary of the National Bureau monetary studies for the National Bureau's annual report, Friedman was using carefully chosen language to give an account of the findings. He summarized the conclusions he and his colleagues had come to without using the word "cause."

> . . . money does matter and matters very much. Changes in the quantity of money have important, and broadly predictable, economic effects. Long-period changes in the quantity of money relative to output determine the secular behavior of prices. Substantial expansions in the quantity of money over short periods have been a major proximate source of the accompanying

[1] The distinction between matters of method and of methodology is that method is *how* one pursues an investigation and methodology is the *justification* of one's method. Of course, the distinction should not be made too sharply because disagreements over method lead to arguments on methodology.

> inflation in prices. Substantial contractions in the quantity of money over short periods have been a major factor in producing severe economic contractions. And cyclical variations in the quantity of money may well be an important element in the ordinary mild business cycle (1964a, p. 277).

Though it may astonish economists familiar with Friedman's monetary economics, who have come to think of monetarism in terms of catch phrases such as "inflation is always and everywhere a monetary phenomenon," Friedman deliberately avoided the word "cause." Its omission from the cited passage was not by chance. In a letter to me commenting on an analysis of the causality in his monetary economics (Hammond, 1986), Friedman wrote:

> I have always regarded "cause" as a very tricky concept. In my technical scientific writings I have to the best of my ability tried to avoid using the word. In the quotation with which you start the paper I do not say at all that money stock is a cause. I believe that you will not be able to find a statement in the *Monetary History* or in other scientific writings of mine in which I make such an assertion (MF to JDH, June 13, 1985).

Clearly there existed something deeper than mere choice of words. Members of the economics community have interpreted Friedman's work as an attempt to sort out the cause–effect roles of money in business cycles, and that was exactly how he portrayed it when he and Schwartz commenced the research. How did Milton Friedman develop this sensitivity to causality? This is the question from which this book evolved. Friedman's methods and methodology in the context of post–World War II economics provide the key to this biographical puzzle.

This book is a history of Friedman's debates with his critics over money's causal role in business cycles. It is ordered chronologically, from the beginning of his collaboration with Anna Schwartz in 1948 through 1991. Most of the chapters are constructed around exchanges between Friedman or Friedman and Schwartz and their critics. The predominant issue in these exchanges was one or another dimension of causality. The chapters that do not cover criticisms of Friedman and Schwartz's monetary economics (Chapters 1, 2, and 3) provide background for understanding the disputes over money as cause and effect. Chapter 1 covers the "measurement without theory" issue concerning the National Bureau of Economic Research prior to Friedman and Schwartz's project. National Bureau methods, particularly those developed by Wesley C. Mitchell and Arthur F. Burns, which Friedman and Schwartz adopted for their studies, were highly controversial well before Friedman and Schwartz made public their conclusions about money's role in business cycles. Chapter 2 provides an exposition of Friedman's equally controversial Marshallian methodology. Of particular interest is the formation of Friedman's methodological position prior to the 1953 publication of "The Methodology of Positive Economics." Chapter 3 draws heavily on unpublished material for a history of the initial stage of Friedman and Schwartz's collaboration. Chapter 4 also uses unpublished documents to portray the initial critical reaction to their work, from colleagues at the National Bureau. Chapters 5 through 10 cover episodes of critical reaction to Friedman and Schwartz's publications at various stages of their extended project. Though much of the source material for these chapters is published, they too make use of correspondence and other unpublished documents.

Critics and allies of Friedman and Schwartz have regarded their views on money (monetarism versus Keynesianism) as the primary source of contention in the debate over Friedman and Schwartz's revival of monetary economics. But given the standards of the day, their use of National Bureau business-cycle methods and Friedman's Marshallian methodology played even larger roles in making Friedman and Schwartz's monetary economics unacceptable. Even though they were widely credited with revitalizing monetary economics and by and large dominated professional and popular discussions of macroeconomics, monetary theory, and monetary policy for a quarter century, Friedman and Schwartz labored from the beginning to the end of their collaboration under a cloud of professional doubt. The doubt was concerning the scientific credibility of their techniques, and thus of their results. The techniques in question were means of uncovering cause and effect roles, and the results were attributions of these roles. Even at the conclusion of the project, when the slow tide of professional and public opinion had turned their way, reviewers of *Monetary Trends* claimed *both* that Friedman and Schwartz had long since won the battle with their Keynesian foes over the power of money supply changes *and* that their book gave insufficient attention to issues of forward and reverse causation between money and income. To paraphrase Hume, causality was the cement of Friedman and Schwartz's monetary economics. It was also the Achilles' heel.

CHAPTER 1

Theory and measurement at the National Bureau

Introduction

"Measurement Without Theory" was the title that Tjalling Koopmans gave his famous review of Arthur Burns and Wesley Mitchell's *Measuring Business Cycles*. That phrase seemed to sum up the differences between the ascendant Cowles Commission approach and the National Bureau of Economic Research (NBER) approach to macroeconomic, or business-cycle, analysis in 1947. Whereas both organizations shared a concern for understanding business-cycle phenomena in order to provide a basis for control, their means to this end differed markedly. The Cowles Commission objective was to wed neoclassical economic theory and modern probabilistically based econometrics. They were actually *creating* what came to be recognized as modern econometrics, their emphasis was on theory. The National Bureau objective, as Koopmans indicated, had much more to do with measurement. A large portion of the effort at the National Bureau went toward developing measurement concepts such as national income accounts and the "reference cycle," along with their related data series. National Bureau analysis of business cycles consisted of separating trends and cycles in time series, and relating patterns in cycles across different series. Burns and Mitchell's book epitomized for Koopmans the National Bureau's atheoretic approach and its fruitlessness for understanding business-cycle phenomena.

Koopmans was neither the first nor the last critic to bring the charge of "measurement without theory" against the National Bureau. The question of the relationship between the work done there under Mitchell's leadership and neoclassical economic theory was an old one. It was a common theme in reviews of Mitchell's work going all the way back to his 1913 *Business Cycles*, published seven years before the National Bureau was founded. Paul Homan (1928, pp. 410–11) speculated that the general lack of attention readers gave to the theoretical implications of Mitchell's analysis may have been due to the book's size. Mitchell himself later wrote that the National Bureau produced books that only readers with "keen interest in a problem and uncommon power to assimilate facts" can appreciate (1945, p. 12). The sheer quantity of facts could get in the way of seeing the theory. Later, Friedman and Schwartz's books sustained the "weighty tome" reputation of National Bureau publications.[1] In a number of guises, the "measurement without theory" critique followed these two students of Wesley Mitchell throughout the course of their long collaboration.[2]

The purpose of this chapter is to review methodological controversy surrounding the National Bureau during the period when Mitchell was director of research, which was the backdrop for "measurement without theory" charges brought against Friedman and Schwartz.[3] The focus of the chapter is primarily on Mitchell's own work on business cycles, but we also consider Frederick Mills's work on price behavior, which was meant to complement the business-cycle project. Although the chapter does not provide exhaustive coverage of the National Bureau's programs over its first quarter century, it sufficiently reveals the history of the "measurement without theory" issue surrounding the National Bureau business-cycle project.

We begin by examining Mitchell's business-cycle volumes and reactions from reviewers. Then we consider Mills's *The Behavior of Prices* and its critics. In both cases the aim is to identify the methodology presented explicitly in the Mitchell and Mills texts. In reviewing the critical reactions we attempt to reveal some of the more important

[1] For example, see Thomas Mayer's favorable review of Friedman and Schwartz's *Monetary Trends* (1982).

[2] The methodological chicken-and-egg question of which comes first, theory or measurement, and the question of where the marginal value of effort is higher, have remained difficult and contentious issues for economists since the advent of systematic collection of data. Recent developments in macroeconomics have generated renewed interest in National Bureau methods of business-cycle analysis and have brought to the foreground once again these issues. See, for example, Sargent and Sims (1977), Neftci (1986), Prescott (1986), King and Plosser (1994), and Simkins (1994).

Robert Eisner (1989) made the relationship between theory and measurement the theme of his 1988 American Economic Association presidential address. The issue as he put it was, how can economists know what they are talking about? Eisner opened his address with a reference to Koopmans's review of Burns and Mitchell that reflects the conventional wisdom that Koopmans's critique was on target. Yet the balance of Eisner's message – that the marginal product of economists' effort is high for compiling and synthesizing data – was closer in spirit to the emphases of the National Bureau tradition than those of the Cowles Commission.

[3] Mitchell served as director of research from 1920 until 1945.

elements in the contextual backdrop from which "measurement without theory" issues arose. The critical reaction to Mitchell's work culminated with Koopmans's review of *Measuring Business Cycles.* As part of the contextual background, we will also consider the methodology of the Cowles Commission econometrics program.

Mitchell's business-cycle analysis

The first edition of *Business Cycles* was published by the University of California Press in 1913, just after Wesley Mitchell left the Berkeley faculty for Columbia University, which was to be his academic home for the remainder of his career. The book has three parts: Part I contains a review of then extant business-cycle theories, a review of the history of business crises, Mitchell's depiction of the organization of modern economies and his method of analysis; Part II contains detailed statistical data series for four countries for the 1890–1911 period; and Part III contains the bulk of Mitchell's analysis of cycles. A new, reworked edition of Part I was published by the National Bureau in 1927 as *Business Cycles: The Problem and Its Setting. Measuring Business Cycles* (1946) was an update of Part II, with Arthur Burns as primary author. Mitchell planned to redo Part III but, fearing toward the end of his life that he would not complete the project, consented to a new but mostly unchanged edition, published by the University of California Press in 1941.[4] At the time he died in 1948 Mitchell had made some headway on this revision; the result was published posthumously by the National Bureau as *What Happens During Business Cycles* (1951).

Mitchell characterized his method as "analytic description," which involved systematic and extensive use of statistical data to develop and test theory. He saw business cycles as complex processes that impose on the investigator the burden of acquiring extensive factual knowledge before any useful analysis is possible. He thought the conventional approach was flawed because it put theory too much ahead of facts. Yet Mitchell did not spurn the theories that he reviewed in Part I of the 1913 volume and in the 1927 volume. Indeed, he explicitly used them. What he considered their flaw was that each in and of itself had inadequate empirical foundations and was woefully incomplete. He thought the source of this flaw was the idea that one needs a more or less complete theory before one can know what facts to look for or how to interpret them. This idea, which Mitchell saw as a

[4] *Business Cycles and Their Causes.*

shortcoming, would become a theme of Koopmans's methodology. Mitchell contended that "the more thoughtfully one considers the relations between these two phases of knowing [the apprehension of facts and conception of theories], the less separable they become" (1927, p. 59, n. 2).

Mitchell set up the presentation of his approach to business-cycle analysis in the 1913 volume in a way that is useful to reproduce at length:

> Beveridge ascribes crises to industrial competition, May to the disproportion between the increase in wages and in productivity, Hobson to over-saving, Aftalion to the diminishing marginal utility of an increasing supply of commodities . . . Fisher to the slowness with which interest rates are adjusted to changes in the price level.
>
> One seeking to understand the recurrent ebb and flow of economic activity characteristic of the present day finds these numerous explanations both suggestive and perplexing. All are plausible, but which is valid? . . . Each may account for certain phenomena; does any one account for all the phenomena? Or can these rival explanations be combined in such a fashion as to make a consistent theory which is wholly adequate?
>
> There is slight hope of getting answers to these questions by a logical process of proving and criticizing the theories. For whatever merits of ingenuity and consistency they may possess, these theories have slight value except as they give keener insight into the phenomena of business cycles. It is by study of the facts which they purport to interpret that the theories must be tested.
>
> But the perspective of the investigation would be distorted if we set out to test each theory in turn by collecting evidence to confirm or to refute it. For the point of interest is not the validity of any writer's views, but clear comprehension of the facts. To observe, analyze, and systematize the phenomena of prosperity, crisis, and depression is the chief task. And there is better prospect of rendering service if we attack this task directly, than if we take the round about way of considering the phenomena with reference to the theories.
>
> This plan of attacking the facts directly by no means precludes free use of the results achieved by others. On the contrary, their conclusions suggest certain facts to be looked for, certain analyses to be made, certain arrangements to be tried. Indeed, the whole investigation would be crude and superficial if we did not seek help from all quarters. But the help wanted is help in making a fresh examination into the facts (1913, pp. 19–20).

The distinction that Mitchell made between his and the conventional approach suggests that the issue was one of emphasis and orientation rather than an either/or choice between theoretical and atheoretical, or a priori and a posteriori, approaches. His approach was different because its primary focus was on the concrete phenomena to be explained rather than on the extant theory.

There was an evolution of Mitchell's views on causality, which is evident through the various editions of *Business Cycles*. In the 1913 edition Mitchell was not at all self-conscious about writing on the causes of business cycles. He directed readers who desire a quick reading on the causes of cycles to the final chapter, which includes a section entitled, "Diversities Among Business Cycles and Their Causes." His review of the history of business-cycle analysis in Chapter 1 suggests nonetheless that he saw causal problems in traditional methods. From the economic crisis of 1825 through the remainder of the nineteenth century, students of the phenomenon developed a number of plausible theories of business cycles. They typically identified a single causal factor that disturbed the economic equilibrium. *The* cause of each crisis was taken to be the event that precipitated the crisis – its genesis. The theories traced out the cause–effect chains emanating from the disturbance, proceeding from the crisis's beginning to its end. As Mitchell became increasingly self-conscious about causal analysis, he took this unidirectional, unicausal explanation to be its archetype.

Mitchell defined the aim of his work as developing "a descriptive analysis of the cumulative changes by which one set of business conditions transforms itself into another set" (1913, p. 449).[5] Because he thought of business cycles as cumulative processes, he expected that a complete theory would be out of reach: "Business history repeats itself, but always with a difference" (1913, p. 449). This was for two reasons. First, because every individual phase (such as prosperity or depression) is the cumulation of an indefinitely long history, pursuit of a complete explanation leads to an infinite regress. Second, because each phase has its own uniqueness, there is a necessary trade-off between completeness and generality in a theoretical account.

In the 1927 revision of Part I of the 1913 volume, Mitchell pondered the role of causality in business-cycle analysis. He questioned whether in light of the complex interdependence of the institutions and processes from which he saw business cycles arising, there could be scientific warrant for a search for causes. Part of the problem was that which he discussed in the 1913 volume – the association of causal analysis with identification of a single disturbing cause. But rejection of unicausal explanation need not imply rejection of causal analysis altogether. Mitchell had developed another concern. At the time, philosophers were suggesting that in scientific explanation, mathe-

[5] There was a trend at the time toward framing the problem in terms of cyclical phenomena rather than discrete crises. See Mitchell (1913, ch. 1, especially p. 5).

matical functions *replace* causal connections.[6] For example, Bertrand Russell wrote that "the law of gravitation will illustrate what occurs in any advanced science. In the motions of mutually gravitating bodies, there is nothing that can be called a cause, and nothing that can be called an effect; there is merely a formula" (1917, p. 194). The following passage suggests that Mitchell was influenced by this philosophical climate:

> As our knowledge grows wider and more intimate, our attitude toward the discussion of causes undergoes a subtle change. When we have accounted in causal terms for each stage in a lengthy series of actions and reactions, we find that our analysis deals with many causes, each one of which is logically indispensable to the theory we have elaborated. On reflection, we see the application to our work of the old contention that the idea of causation has pragmatic, rather than scientific, warrant. . . .
>
> In the progress of knowledge, causal explanations are commonly an early stage in the advance toward analytic description. The more complete the theory of any subject becomes in content, the more mathematical in form, the less it invokes causation (1927, pp. 54–5).

Mitchell's concern with the role of causality also reveals a sensitivity to a not inconsequential limitation of the new reliance on empirical statistical material – that the statistics could not in themselves reveal any causal mechanism. Mitchell was well aware of the pitfalls of spurious correlation and *post hoc ergo propter hoc* explanation. This created something of a quandary for him. He saw causality as an extrascientific metaphor, but nevertheless one that is useful in accounts of cyclical behavior.

> In business-cycle theory, the transformation from causal explanations into analytic description is being hastened by free use of statistical materials and methods. What time series can be made to show are functional relationships. We are always reading something into statistics, when we assert that the process represented by one series exercises a causal influence upon the process represented by a second series. Yet a stiff refusal to employ causal expressions in the detail of our investigation might often hamper us. In the present stage of our knowledge, we can probably make more rapid progress toward attaining insight into business cycles, by using the thought-forms of daily life than by trying to express ideas at which we are grasping in the form which may ultimately prevail (1927, p. 55).

[6] Mitchell's teacher Thorstein Veblen traced an evolution of the scientific use of causal analysis from final cause to efficient cause to process cause. But he thought the urge to *replace* causal relations with mathematical functions to avoid the anthropomorphism of causal imputation was futile. See Veblen (1906, 1908).