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ON THE ORIGINS OF "A MONETARY HISTORY"

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ABSTRACT

This paper explores some of the scholarship that influenced Milton Friedman and Anna J. Schwartz's "A Monetary History". It shows that the ideas of several Chicago economists -- Henry Schultz, Henry Simons, Lloyd Mints, and Jacob Viner -- left clear marks. It argues, however, that the most important influence may have been Wesley Clair Mitchell and his classic book "Business Cycles" (1913). Mitchell, and the NBER, provided the methodology for "A Monetary History", in particular the emphasis on compiling long time series of monthly data and analyzing the effects of specific variables on the business cycle. A common methodology and the stability of monetary relationships produced similar conclusions about money. Friedman and Schwartz deemphasized Mitchell's "bank-centric" view of the monetary transmission process, but they reinforced Mitchell's conclusion that money had an independent, predictable, and important influence on the business cycle.

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1. Introduction[1]

Milton Friedman and Anna J. Schwartz's *A Monetary History of the United States* (1963) is arguably the most important book in economics since *The General Theory* (1936).[2] It has inspired and informed an enormous amount of research. Even footnotes have produced voluminous and contentious literatures.[3] This paper explores some of the work that influenced the *Monetary History*.[4] It shows that the ideas of several Chicago economists – Henry Schultz, Henry Simons, Lloyd Mints, and Jacob Viner – left clear marks. However, it argues that the most important influence may have been Wesley Clair Mitchell and his classic book *Business Cycles* (1913). Mitchell, and the NBER, provided the methodology, in particular the emphasis on long accurate time series of monthly data and the analysis of the effects of specific variables on the business cycle. Mitchell's book, as I try to show below, contained a preliminary exploration of the issues examined in more detail, and for a longer historical period, in the *Monetary History*. Mitchell concluded that money played an independent, predictable, and important role in shaping the business cycle. Friedman and Schwartz's work, although it differed on several particulars, most importantly on the role of bank-lending policies in the transmission of monetary impulses, strongly reinforced Mitchell's basic conclusions.

Perhaps the most important reason for exploring the origins of the *Monetary History* is that it facilitates understanding. Although the *Monetary History* is enormously influential, and although its analyses of particular events are often taken to be authoritative, its basic findings are often misunderstood because the book arose from a methodological tradition that differs markedly from the tradition underlying modern macroeconomics. Young economists often find the book especially challenging because they have not been trained in, perhaps not even exposed to, the historical methods used by Friedman and Schwartz.

The influences on the *Monetary History* that I stress have largely been ignored in the literature on the origins of the *Monetary History*. The initial reviews of the *Monetary History* ignored Mitchell and the Chicago economists and viewed the *Monetary History* as a response to Keynesian economics. Harry Johnson in his review of the *Monetary History* (1965), for example, does not mention Mitchell or other earlier work on business cycles. Johnson briefly summarizes Friedman and Schwartz's basic findings, indeed he accepts them, but he concentrates his attention on the absence of a formal theoretical model, and on the implicit assumption that he sees in Friedman and Schwartz that the demand for money was not a function of the rate of interest.[5] Johnson's (1971) evaluation of monetarism as a whole does not even mention the *Monetary History*. Don Patinkin in a series of papers (1969, 1972, and 1975), although more directly concerned with other aspects of Friedman's work, downplayed the contribution of the Chicago economists, ignored Mitchell and the NBER, and stressed Friedman's unacknowledged debt to Keynes.

Much of the subsequent literature focused on whether Friedman and Schwartz had properly acknowledged their debt to previous analysts of the Great Depression. David Laidler (1993), for example, showed that important elements of the work of the British economist Ralph Hawtrey and the Harvard economists Allyn Young and Laughlin Currie can be found in the *Monetary History* and related work by Friedman and by Friedman and Schwartz. Indeed, at various times Friedman and Schwartz have conceded that they did not give due recognition to the work of one or another previous scholars. I will consider one case, Jacob Viner, below. On the other hand, Friedman and Schwartz's treatment of previous scholars has it defenders. Frank G. Steindl (1995), notably, argued that while a number of economists in the 1930s, such as Currie, had produced elements of the Friedman-Schwartz interpretation of the Depression, none

produced a detailed analysis of the way decisions by banks and the public influenced the supply of money, and none produced an interpretation that integrated supply and demand.

Historians of economic thought interested in Mitchell, for example Howard Sherman (2001), have downplayed Mitchell's views about the role of money in the business cycle, or emphasized the differences between Mitchell and Friedman and Schwartz. Mitchell expressed doubts about the usefulness of monetary aggregates and about the quantity theory; Friedman and Schwartz embraced both. There is, of course, some truth in the initial reaction to the *Monetary History* and the more recent writings of historians of economic thought. Friedman and Schwartz were critics of the Keynesian orthodoxy of the day, and Friedman and Schwartz did compute monetary aggregates and use the quantity theory. However, ignoring Mitchell's *Business Cycles* or focusing on the differences between Mitchell and Friedman and Schwartz misses the agreement on the most important issues: on methodology and on conclusions about the role of money in the business cycle.

The institutional history of how the *Monetary History* came to be is relatively well known. Indeed, Friedman and Schwartz (1963b, xxi) provide the essentials in the *Monetary History*. And J. Daniel Hammond (1996) provides a carefully organized account of the origins of the National Bureau's monetary project and a description of how the book uses the National Bureau's methodology. The National Bureau's money project was one of many started at the Bureau to expand on issues that Mitchell had initially surveyed in *Business Cycles* (1913). The project had started before Friedman and Schwartz took over. Originally James W. Angell and Caroline Whitney, a student of Angell at Columbia who completed her degree at Columbia in 1935, were assigned to the project. Schwartz replaced Whitney, possibly because Whitney was experiencing health problems. And when Angell later left the project, Arthur Burns, who had

replaced Mitchell as director of the NBER, asked Friedman to take over.[6] The choice of Friedman made sense. Friedman had successfully completed several projects at the Bureau. He had already, moreover, developed an interest in the problem of inflation while working at the Treasury in World War II, although Friedman's work at that time was Keynesian in the sense that it emphasized taxes and spending rather than the stock of money. (Friedman 1943; Shoup, Friedman, and Mack 1943). Most important, as it would turn out, Friedman's skills as a theorist perfectly complemented those of Anna Schwartz, a superb economic historian. Burns also suggested that Friedman talk to Walter W. Stewart at Princeton about the project, and it was Stewart who suggested an "analytical narrative" as a background for the statistical study.[7]

Work on the *Monetary History* went on in two places. The statistical work was carried out in New York under Anna Schwartz's direction, while Friedman remained at Chicago. There Friedman recruited graduate students who participated in his workshop and who wrote dissertations related to issues being explored in the *Monetary History*. Much of the joint work of writing the *Monetary History* took place in correspondence between Friedman in Chicago and Schwartz in New York. Although the institutional origins of the *Monetary History* have been documented before, I believe there is more to be said about the basic conclusions about the role of money in the business cycle reached in the *Monetary History* and the relationship between those conclusions and the earlier work by Mitchell and by economists at Chicago.

The rest of the paper is arranged as follows. In section 2 I describe Mitchell's career and his interest in money. In section 3 I develop 5 conclusions about the role of money in the business cycle that summarize Mitchell's views as expressed in *Business Cycles*. I then describe how Friedman and Schwartz's work reinforced, modified, and in one case rejected Mitchell's conclusions. In section 4 I explore the Chicago influences on the *Monetary History*. While

Mitchell's work provided the methodology, it was the Chicago economists who provided much of the analytic framework. In section 5 I explore Friedman's (1950) mathematical model of Mitchell's views. The parallels between this model and Friedman's subsequent attempts to mathematical models of his views strengthen the case for seeing a close connection between *Business Cycles* (1913) and the *Monetary History* (1963). In section 6 I look at some of the influential criticisms of the *Monetary History.* Here I argue that by time the *Monetary History* appeared Mitchell's work, indeed the Bureau's approach in general, was unfamiliar to many economists, and that this partly explains the failure of many economists to understand what Friedman and Schwartz had accomplished. In section 7 I try to define more precisely the "clinical methodology" that I believe underlies the work of Mitchell and of Friedman and Schwartz. In section 8 I try to explain why Mitchell's conclusions about the role of money are so similar on so many fronts to the conclusions reached by Friedman and Schwartz. In section 9 I summarize the main conclusions.

2. Wesley Clair Mitchell

Wesley C. Mitchell is usually remembered today for his non-monetary contributions: his founding of the National Bureau of Economic Research and the stimulus it provided for research such as the early studies of national income. But Mitchell had a deep and continuing interest in monetary phenomenon. Mitchell studied at the University of Chicago as both undergraduate and graduate student during the 1890s, an exciting period in American monetary history that included several financial panics and the famous "Battle of the Standards" (gold or bimetallism?).[8] J. Laurence Laughlin, a professor at Chicago who supervised Mitchell's dissertation and encouraged Mitchell's career, was a prominent defender of the gold standard. Mitchell's

dissertation, *A History of the United States Notes* completed in 1899, discussed the issue of fiat paper money by the North during the Civil War. The University of Chicago Press published it as *A History of the Greenbacks* in 1903.[9] This volume covered the Civil War years (1861-1865) and addressed the effects of the greenbacks on interest rates, prices, wages, and related variables.

Mitchell's teaching career began at Chicago in 1900. In 1903 he moved to the University of California at Berkeley where he would spend a remarkably productive decade. In 1908 Mitchell published *Gold, Prices, and Wages under the Greenback Standard*. Here Mitchell covered the years 1862 to 1878, the entire period when the United States was on the "Greenback Standard" – the currency was not convertible into gold. Theory played a role in the shaping of this volume – Mitchell examined the relationship between the gold premium and prices, an item Friedman and Schwartz would take up in the *Monetary History*, and he discussed the patterns of lags among economic variables – but on the whole the emphasis was on data rather than analysis. Mitchell described the book as the "statistical apparatus of a book still to be written." (Quoted in Burns 1949, 17-18).

In 1907 Mitchell lectured at Harvard on money and business cycles. In 1908 he returned to California and began to work on his third classic: *Business Cycles*. Mitchell's enormous capacity for work served him well. Despite the huge scope of Mitchell's vision, and the length of the manuscript (600 pages), it was completed in record time. Burns (1949, 23) put it this way: "In the amazingly short time of three years, Mitchell had worked out and written one of the masterpieces in the world's economic literature."

Business Cycles (1913) consisted of three parts. The first was a review of existing theories of the business cycle. The second was a compendium and analysis of time series (prices, currency, profits, the condition of the banks, and so on) covering the United States, Britain,

France and Germany from 1890 to 1911. The third was a description and explanation of the typical business cycle combined with a comparison of the United States and Britain during the crisis of 1907. This final chapter, although cast as a description, comes closer than anything else Mitchell wrote to representing his "model" of the business cycle.

Here Mitchell described something approaching a true cycle: Expansion was followed by contraction and contraction by expansion, with some regularity. The expansion, which would diffuse widely through the economy, automatically produced forces – falling profits resulting from the pressure of costs on prices and tensions in the money market – that inevitably produced a contraction. The contraction, which would also diffuse widely, then produced changes in relative prices, such as a fall in costs relative to final product prices, which produced expansion. Friedman and Schwartz agreed that adjustments in relative prices would reverse contractions, so that in the end the economy would return to full employment, but they viewed the forces depressing the economy as random shocks, rather than as the inevitable products of expansions. A long-term statistical portrait of the economy, to put it somewhat differently, had the appearance of a cycle, and the language of cycles could be used to describe the economy, but only the forces making for recovery were truly inherent in the system.[10]

Mitchell did not argue that money was the central element in the business cycle. The cycle, according to Mitchell, was a complex and recurring phenomenon dependent on a wide range of factors, especially the interaction of wages, prices, and profits. Mitchell, however, did accord money an independent and important role in the cycle, and he cited historical events such as the increase in the world stock of gold after 1896 and the banking crisis of 1907 to make his point.

In future years Mitchell served as the first director of the National Bureau of Economic

Research, and became a major public economist. He also remained a highly productive scholar. In 1927 he published *Business Cycles, the Problem and its Setting*, which provided a much richer empirical description of the business cycle than the 1913 volume, but did not attempt to push the analysis of the causes of the cycle further. In 1946 Mitchell and Burns published *Measuring Business Cycles*. In 1951 the NBER published posthumously an unfinished manuscript that Mitchell was working on at his death in 1948, *What Happens During Business Cycles, a Progress Report*. Each of the studies completed after 1913 contained new empirical data, and new discussions of the timing of the business cycle. However, Mitchell never produced an explicit analysis of the causes of the cycle to replace his 1913 effort.

Each topic that Mitchell addressed in his 1913 volume – prices, wages, employment, national income, money, interest rates, and so on – became a project at the National Bureau of Economic Research. A researcher was assigned the task of accumulating data – the Bureau had a strong preference for monthly data – and turning points (from contraction to expansion and expansion to contraction) were identified in the economy as a whole (reference cycles) and in individual series (specific cycles). The specialists then analyzed the relationships between specific cycles and reference cycles. Friedman and Schwartz completed some work of this kind, Friedman and Schwartz (1963a), and they had intended to do an entire volume on money and business cycles. This goal, however, was never reached. In the end they completed three volumes: the *Monetary History* (1963), *Monetary Statistics of the United States* (1970), and *Monetary Trends in the United States and the United Kingdom* (1982).

In the *Monetary History* episodes were treated chronologically, and the focus in each discussion was on whether there was a relationship between changes in monetary policy and changes in the economy. Differences from one episode to another in the time between changes in

monetary policy and responses were, for the most part, not addressed in detail. This issue was addressed in some of the related work and surely would have been addressed in the volume on cycles.

The reasons for Mitchell's decision not to revise his 1913 description of the cycle are unclear. Perhaps he was simply waiting for the full flowering of the empirical work at the Bureau that he had set in motion. Money is a good example. Given the prominent role played by money in his 1913 account of the cycle, it made sense to wait until the Bureau's money project had clarified the role of money before revising his description. In the end, Mitchell's failure to produce a revised description of the business cycle, and his concentration on data led to a telling criticism of Mitchell and the Bureau. Burns and Mitchell (1946) was criticized by Tjalling Koopmans (1947) as "Measurement without Theory," a label that would trouble the Bureau and its workers for years to come. Friedman's (1950) essay "Wesley C. Mitchell as an Economic Theorist," which I will discuss below, was written partly to reverse the negative image of Mitchell that had developed from Koopmans's criticism.

In 1941, in lieu of a new theoretical treatment of the business cycle, Mitchell republished the third part of his 1913 book with "betterments in wording and correction of an arithmetical blunder." (1959 [1913], p. Vii) Therefore, the third part of this early volume contains the fullest available description of how Mitchell viewed the cycle and the role he assigned to money. The quotations here will be from the 1959 printing, the first paperback edition.

Friedman clearly held *Business Cycles (1913)*, if not all of Mitchell's later work, in high regard.[11] In a response to a letter from mathematician Edwin B. Wilson for a list of works Friedman considered "thoroughly good" in the sense that they used theory as a way of organizing facts, and facts as a way of modifying theory, Friedman composed a list of five. *Business Cycles*

(1913) was at the top list, although Friedman did not indicate that his choices were listed in order. (Stigler, 1994, 1200). The list also included, in second place, Burns and Mitchell's *Measuring Business Cycles (1946)*. Much later Friedman would say that the 1913 volume "was a book," but the 1927 volume was not, referring to the lack of a theoretical framework in the later volume. (Friedman 2002).

Friedman's understanding of Mitchell's approach recognized both strengths and weaknesses. In a letter to Burns offering comments on Burns's (1949) description of Mitchell's career, Friedman described Mitchell's approach as follows.

> In truth, Mitchell's great and overwhelming genius was in his unparalleled capacity for bringing together an enormous mass of material, putting it into systematic form, and giving an orderly, lucid, and meaningful account of it. This capacity is demonstrated no less in his expositions of empirical material – The History, Gold etc., – than in his expositions of theoretical literature – part I of both Business Cycle Volumes, The Role of Money in Economic Theory, Types of Economic Theory. But this virtue inevitably carried with it a vice. He did such a masterful job – both intellectually and in literary form and style – that he misled both himself and his readers into taking his descriptions as explanations. I cannot emphasize too strongly that this vice (surely too strong a word) was an inescapable a corollary of the virtue. Nor need I emphasize that this comment is a very different from saying that analysis or theory were absent. They played a crucial role. They provided the organizing principle for the descriptions; and the descriptions themselves were analytical descriptions.[12]

One quantitative index of the influence of particular authors on the *Monetary History* is the number of times they are cited in the text. Table 1 shows the top six authors, and by way of contrast, some prominent authors who were not cited. By this criterion Mitchell, the most frequently cited author (next to Milton Friedman) was the most influential. Citations to Mitchell significantly outdistance those to Irving Fisher and other monetarists. Hayek, Keynes, and Schumpeter were not cited. Citations, of course, are a crude index of influence. Ideas may be so widely accepted that no citation is necessary; a source of data may be cited repeatedly. A book in one's own library may be cited often; a book in the university library may be cited once. To go

deeper I develop 5 propositions that summarize Mitchell's main conclusions about the role of money in the business cycle and show that these propositions also encompass the main findings of Friedman and Schwartz.

3. From Mitchell to Friedman and Schwartz

Briefly these propositions can be described as follows: (1) there are changes in money that are independent of the business cycle, (2) these changes are regularly associated with changes in economic activity, (3) money therefore causes changes in economic activity, (4) while money influences the business cycle, so do many other things, and (5) changes in bank-lending rates are an important part of the transmission mechanism. Below I have placed my summary of Mitchell's conclusions in italics, and my summary of how Friedman and Schwartz extended Mitchell's conclusions underneath. I show that Friedman and Schwartz strongly reinforced Mitchell's conclusions on the first three issues with new data and arguments. Friedman and Schwartz did not break new ground on the fourth issue, the role of non-monetary forces, which was outside their assignment, although they discussed a wide range of non-monetary forces. On the last issue, the role of bank lending, Friedman and Schwartz reached a different conclusion from Mitchell. Friedman and Schwartz emphasized a direct channel running from changes in money to changes in national income, while Mitchell emphasized a channel that ran through bank lending and credit markets. It is interesting in this respect that one of the main differences of Friedman and Schwartz with their close allies Alan Meltzer and Karl Brunner was over whether credit market effects should be ignored, and that the most widely accepted addition to the Friedman and Schwartz interpretation of the Great Depression, the influential work of Ben Bernanke (1983) restores the role of bank lending to the central role in the monetary transmission

process.

My propositions, although designed for the purpose of comparing Mitchell with Friedman and Schwartz, are closely related to the propositions Friedman and Schwartz used to summarize their findings. In their concluding chapter Friedman and Schwartz (1963b, 676) identified 4 principal findings: (1) "changes in the behavior of the money stock have been closely associated with changes in economic activity," (2) "the interrelation between monetary and economic change has been highly stable," (3) "monetary changes have often had an independent origin," and (4) "in monetary matters, appearances are deceiving." My propositions (1) – (3) cover Friedman and Schwartz's (1) – (3). Their fourth proposition was also adumbrated by Mitchell (1959 [1913], 1-2) who noted that observers usually attributed the revival of business from a contraction to "happy accidents" and missed the underlying forces that tended to make for recovery.

Here then are what I believe to be Mitchell's main conclusions about the role of money in the business cycle.

(1) *There are different types of money – gold, paper money issued by the government, paper money issued by banks, and bank deposits. The amounts of gold and paper money issued by the government are relatively independent of the business cycle, while the amount of deposits is highly dependent on the business cycle. Paper money issued by banks is an intermediate case. (Mitchell 1959 [1913], 50-52).*

Mitchell was content to look at each component of the stock of money separately and was skeptical about the wisdom of adding them up. Friedman and Schwartz, on the other hand, were determined to add up assets and produce an estimate of the total quantity of money. Friedman and Schwartz, however, preserved the most important aspect of Mitchell's analysis of the supply

of money by introducing the distinction between high-powered money and low-powered money. This distinction preserved Mitchell's main point and clarified how gold and paper money issued by the government (high-powered money) differed from bank deposits (low-powered money). Friedman and Schwartz set out their formal analysis of the supply of money in Appendix B (1963b, 780-789) of the *Monetary History*, and included sections on the supply of money in most chapters, typically labeled "factors accounting for changes in the money stock."[13] I will consider their analysis in more detail in section 5.

Philip Cagan (1965) carried out much of the analysis of the supply of money in his *Determinants and Effects of Changes in the Stock of Money, 1875-1960.* Although published two years after the *Monetary History*, Cagan's manuscript was at hand when Friedman and Schwartz were writing the *Monetary History*. Friedman and Schwartz cite this unpublished study 9 times (see Table 1) and an earlier study by Cagan, "The Demand for Currency Relative to Deposits," three times. Clearly, Cagan deserves much of the credit for pushing forward this aspect of Mitchell's analysis.

(2) Changes in the stock of money have been associated with predictable changes in economic activity. Increases in money have been associated with economic expansions and inflation; decreases with contractions and deflation.

Mitchell based his findings in *Business Cycles* mainly on his reading of the period 1890 to 1911, although his earlier work on the Civil War and postbellum periods must have played a role as well. The events of most interest to monetary historians during the period 1890 to 1911 were the inflation from 1896 to 1911 and the financial panics in the United States in 1893 and 1907. His interpretations of these events were one of the main sources of his conclusions about the role of money in the business cycle.

Mitchell argued, for example, that increases in the supply of gold influences the business cycle. In Mitchell's words, (1959 [1913], 52)

> An abundant supply of gold favors a revival of business activity by giving the banks liberal reserves and thereby increasing their ability to lend credit at moderate rates of interest. This feature of the situation grows more important as the revival ripens into full prosperity. … That is, an increasing supply of gold favors the continuance of prosperity by retarding the accumulation of one of the stresses characteristic of one of its later stages --- namely, tension in the money market.

At a later point in his discussion Mitchell (1959 [1913], 138-39) was more explicit in referring to the period after 1896, and in pointing to an effect on prices.[14]

> Hence such an increase in the world's production of gold as has been going on in recent years tends to cut short and to mitigate depressions as well as to prolong and to intensify prosperity. By thus altering somewhat both the intensity and the relative duration of these two phases of business cycles, it tends to an upward direction to those long-period movements of the price curve in which the years of depression and of prosperity are averaged.

Friedman and Schwartz also analyzed the effect of the increase in world gold production at the turn of the century. They agreed with Mitchell that the gold discoveries increased long-term inflation.

> The proximate cause of the world price rise [from 1897 to 1914] was clearly the tremendous outpourings of gold after 1890 that resulted from discoveries in South Africa, Alaska, and Colorado, and from the development of improved methods of mining and refining. (Friedman and Schwartz 1963b, 137)

They were skeptical, however, that rapid growth of the stock of gold had increased the rate of growth of real income over a period of several business cycles, a claim that might be read into Mitchell's claim that economic expansions had been lengthened and contractions shortened. One piece of evidence was their comparison of the period 1903-1913, a period of "moderately stable growth" with 1882-1892. (Friedman and Schwartz 1963b, 186-7) There was a substantial difference in inflation: the national income deflator fell at a rate of 2.0 percent per year from

1882-1892, but rose at a rate of 2.0 percent per year from 1903 to 1913. There was not, however, a dramatic difference in the growth of the real economy: real income rose 3.5 percent per year from 1882 to 1892 and 3.3 percent per year from 1903 to 1913. (Friedman and Schwartz 1963b, 185).[15]

Mitchell also stressed that banking panics could disrupt economic activity, and he included (1959 [1913], 74-122) a detailed comparison of the panic of 1907 in the United States with the crisis – it never degenerated to the point where it could be called a panic – in Britain.[16] A banking panic, Mitchell argued, produced disarray in the normal means of settlement, and discouraged banks from making loans available in the normal way. Both developments produced the contraction in economic activity.

The close association of banking panics with severe economic declines was also important to Friedman and Schwartz. In the concluding chapter to the *Monetary History* they noted that there had been six severe economic contractions in the period they examined: 1873-79, 1893-94, 1907-08, 1920-21, 1929-1933, and 1937-38. Each was marked by a decline in the stock of money, and four – 1873-79, 1893-94, 1907-08, and 1929-33 – "by major banking and monetary disturbances." (Friedman and Schwartz 1963b, 677-78).

(3) *History provides persuasive evidence that the direction of causation can run from money to economic activity: History proves that money matters. This conclusion follows from proposition (1), that some monetary changes are independent of current or future changes in the economy, and proposition (2) that significant changes in economic activity are associated in a predictable way with changes in money.*

Mitchell, as far as I am aware, never developed this point explicitly. However, it is implicit in his description of the business cycle. His treatment of the supply of gold in the 1890s

is a good example. As we saw above, Mitchell (1941 [1913], 138-39) concluded "that an increase in the world's production of gold as has been going on in recent years tends to cut short and to mitigate depressions as well as to prolong and to intensify prosperity." Mitchell also noted that the increase in the gold supply was independent of the increase in prosperity. Mitchell (1941 [1913], 50-51) listed a number of ways that an economic expansion would decrease gold production, for example by raising costs of production while the final product price remained fixed, and a number of ways that an expansion would increase gold production, for example by encouraging people to buy risky stocks in gold mines. He concluded, however, that "in recent times" the net effect of cycle-related factors had

> certainly been overshadowed by the influence of other factors not directly dependent upon the condition of business – the progress of the mining and metallurgical technique, the discovery of new gold deposits, and the maintenance of order in the chief producing districts.

Clearly it followed that causation had run from the independent increase in the supply of gold to the mitigation of depressions.

Mitchell's contrast between the United States in 1907 and Britain in 1907 also points to a causative role for money. The United States suffered a severe banking panic and a severe contraction in economic activity. Britain suffered a milder monetary stringency and a milder decline in economic activity, mainly because of timely actions by the Bank of England. The Bank owed its commanding presence to a long historical process that was largely independent of business conditions in 1907. It followed that monetary policy was an independent influence on the economy.

In his concluding chapter Mitchell (1959 [1913], 159) gave forceful expression to the importance of a lender of last resort, and the need for one in the United States.

> That occasionally crises still degenerate into panics in America, but not in Great Britain, France, or Germany, arises primarily from differences in banking organization and practice. In each of the three European countries the prevalence of branch banking and existence of a central bank so organizes the banking system as a whole that reserves can be applied when and where needed although they constitute only a small percentage of aggregate demand liabilities of all the branches. In marked contrast to the policy of American banks, the central bank not only carries a reserve far in excess of immediate requirements in ordinary times, but also uses it boldly in times of stress.[17]

Friedman and Schwartz further developed the argument that historical details about monetary institutions and the origin of monetary changes would illuminate the direction of causation between money and economic activity. What is needed to show persuasively that money matters are natural experiments, crucial experiments as Friedman and Schwartz style them, occasions when the stock of money changed for reasons that were clearly independent of contemporaneous or future economic changes in real income or prices.

The gold-based inflation from 1897 to 1914, stressed by Mitchell, also appears as a major case study in the *Monetary History*. In fact, Friedman and Schwartz (1963b, 686) argue that this episode provides the best evidence from their array of episodes on the direction of causation.

> The *clearest example* [that the direction of causation may run from money to income; my italics] is perhaps the monetary expansion from 1897 to 1914, which was worldwide and reflected an increased output of gold. The increased output of gold was partly a consequence of earlier decades of declining prices, which encouraged gold production, and so speaks also for the mutual interaction between monetary and economic changes. But clearly the monetary expansion cannot be attributed to the contemporary rise in money income and prices. By itself, the rise in money income and prices made for a reduced output of gold in the world at large and for an outflow of gold from any single country in a gold standard world. If the common movement of money and income was not purely coincidental, the direction of influence must run from money to income.

Although natural experiments are the best evidence that history can provide, it is not the only evidence. Even in the absence of a natural experiment, historical episodes can be compared and contrasted, as Mitchell did when comparing the United States and Britain in 1907. In the

Monetary History Friedman and Schwartz confined themselves to the United States so for the most part this kind of comparison could not be made.[18] However, they could and did make comparisons across time as when they compared the contraction of 1907-08 with the contraction of 1929-1933.[19] They were also able to compare experiences during major wars, something Mitchell writing in 1913 could not do. Here Friedman and Schwartz (2963b, chapter 10, *passim*) noted that prices rose more in World War I than in World War II, a fact consistent with the degree of monetary expansion in the two wars, but not consistent with the degree of mobilization.

Although Friedman and Schwartz pressed the case for using history to prove causality running from money to income, it was Philip Cagan who developed the argument in greatest detail in *Determinants and Effects of Changes in the Stock of Money, 1875-1960*, especially in the penultimate chapter, "The Cause-and-Effect Relations Between Money, Prices, and Output." In his forward to Cagan's book Friedman (1965) summarizes Cagan's conclusions about the causal role of money, and reports (1965, xxvii-xxviii) that "originally, we did not expect the examination of the supply of money to provide evidence on such general issues as the causal relation between money and prices." Perhaps because Cagan's monograph was published two years after the *Monetary History*, Cagan's detailed development of the case for using history to prove that money influences the business cycle was frequently missed by the critics of Friedman and Schwartz. Friedman's remark in the introduction to the Cagan volume also suggests that Mitchell's recognition that some apparently important monetary changes were the result of forces independent of the business cycle was not the direct inspiration for Cagan's analysis, although Cagan (1965, 143-145) does discuss some of Mitchell's ideas about the money supply.

(4) The business cycle is the result of both monetary and non-monetary forces.[20] *Money matters, but so do many other things.*

Mitchell's list of important influences on the business cycle was long. Money was merely one actor on a crowded stage. Friedman and Schwartz also assumed that there were many factors affecting the business cycle. This statement may seem to contradict a commonly accepted view that Friedman and Schwartz claimed that money explains everything. The common wisdom, however, is simply mistaken. Like Mitchell, Friedman and Schwartz assumed that even in the absence of mistakes in monetary policy that there would be a business cycle, sometimes a very volatile business cycle, caused by other factors.

Friedman and Schwartz frequently describe the non-monetary forces influencing the business cycle explicitly. Agriculture provides one example. Mitchell (1959 [1913], 2), noted that in 1891 "Unusually large American crops of grain, sold at exceptionally high prices, cut short what was promising to be an extended period of liquidation after the crisis of 1890, and suddenly set the tide of business rising."[21] And Friedman and Schwartz noted three cases in which a "fortuitous combination" of favorable harvests in the United States and unfavorable harvests in Europe boosted economic activity in the United States: 1879-1882, 1891, and 1896.[22, 23] (Friedman and Schwartz 1963b, 97-98, 107, 140).

The stock market is another source of non-monetary forces addressed by both Mitchell and by Friedman and Schwartz. Mitchell (1957 [1913], 69-70) argued that a decline in stock prices during the last phase of an economic expansion would set in motion forces that contributed to the following recession. Costs would rise more rapidly than prices during the expansion, squeezing profits, and putting downward pressure on stock prices. Reduced valuations for firms, especially smaller firms, in turn would reduce the availability of credit, and force cutbacks in production. Similarly, Friedman and Schwartz argued that the Crash of 1929 had depressed economic activity by reducing the willingness of consumers and business

enterprises to spend because the Crash "spread uncertainty where dazzling hopes of a new era had prevailed." (Friedman and Schwartz 1963b, 306-07).

In most cases, however, Friedman and Schwartz simplified their task by assuming a non-monetary cycle that would continue even in the absence of monetary disturbances. The non-monetary disturbances that often drove the economy off its full employment trend, and the underlying tendency of a market economy to fight its way back to full employment, in other words, were often assumed rather than described in detail. The decision to abstract from non-monetary forces was justified by the division of labor at the Bureau. Friedman and Schwartz's assignment was to clarify the role of money in the business cycle; other forces were the province of other investigators. Friedman and Schwartz's basic formula then was to describe how monetary forces pushed or pulled a cycle determined by non-monetary forces.

In their discussion of the 1920-21 recession, for example, they write as follows. (Friedman and Schwartz 1963b, 237.)

> The extraordinary disturbances of the World War I period certainly induced national and international adjustments in the use of real resources on a far larger scale than is usual, and were unquestionably a source of uncertainty. Those disturbances might well have made it impossible to avoid a more than usually severe cyclical movement in this country, though our experience after World War II demonstrated that this result need not follow. But there can be little doubt that Federal Reserve policy [very large increases in the discount rate] was a further and not unimportant factor contributing to the severity of the movement.

In their discussion of the effects of monetary contraction in the early 1930s they write as follows. (Friedman and Schwartz 1963b, 313)

> All in all, the figures for the first four or five months of 1931, if examined without reference to what actually followed, have many of the earmarks of the bottom of a cycle and the beginning of revival.
>
> Perhaps if those tentative stirrings of revival had been reinforced by a vigorous expansion on the stock of money, they could have been converted into sustained recovery. But that was not to be.

The obvious implication is that there are other forces that make for recovery besides money, but those forces are abstracted from the discussion. Part of the story was simply the inherent tendency of a free market economy to right itself after a recession.

Another example of their treatment of the non-monetary cycle occurs in the discussion of the 1937-1938 recession. Friedman and Schwartz attributed a great deal of damage to the decision made by the Federal Reserve prior to the recession to double the required reserve ratios of banks and to sterilize the inflow of gold. Nevertheless, they do not claim that there would have been continued expansion in the absence of the change in monetary policy. Their statement (Friedman and Schwartz, 1963b, 544) is far more circumspect, and assumes a business cycle that is independent of monetary policy.

> Consideration of the effects of monetary policy on the stock of money certainly strengthens the case for attributing an important role to monetary changes as a factor that significantly intensified the severity of the decline and also probably caused it to occur earlier than otherwise.

And in discussing the recovery from this recession, Friedman and Schwartz (1963b, 545) write as follows.

> Recovery came after the money stock had started to rise. … Munich and the outbreak of war in Europe were the main factors determining the U.S. money stock in those years, as Hitler and the gold miners had been in 1934 to 1936. Doubtless, other factors helped to account for the onset of recovery and for its pace, but the rapid increase in the money stock certainly at the very least facilitated their operation.

Again, Friedman and Schwartz assume that "other factors," left unnamed, influenced the business cycle. Indeed, in this quotation the possibility that money was merely "facilitating" other forces, Mitchell's way of putting things, is acknowledged.

The continuity with Mitchell's views cannot be allowed, of course, to obscure the difference in emphasis. It is probably a fair conjecture that if Friedman and Schwartz and

Mitchell had been asked for a summary figures indicating the amount of business fluctuations accounted for by independent changes in money Friedman and Schwartz would have produced a higher figure than Mitchell.[24] Mitchell, moreover, was not directly concerned in his work at the Bureau with focusing on policy variables because in his day adherence to the gold standard and the absence of a central bank eliminated the potential for short-term monetary policies. Much of Friedman's later writings focus on money, not because other factors don't affect the business cycle, but rather because of his belief that of the policy instruments available to the government, money is by far the most important.

(5) *Bank lending plays a central role in the transmission mechanism. When banks are flush with reserves they extend loans, lowering interest rates and encouraging economic activity*.

Recall again the statement quoted from Mitchell above about the impact of gold during the period after 1897. In it Mitchell claimed that the way that an abundant supply of gold had ameliorated fluctuations was "by giving the banks liberal reserves and thereby increasing their ability to lend credit at moderate rates of interest." Friedman and Schwartz ignored the bank-lending channel emphasized by Mitchell. Instead, Friedman and Schwartz emphasized a direct channel running from a temporary excess of money holdings to increased spending.

Friedman and Schwartz do not explain why they rejected the bank-lending channel stressed by Mitchell and other early theorists. Part of the explanation may be that concurrent work on investment suggested that interest rate changes had little effect on investment spending. Another part of the explanation may be that Friedman and Schwartz had already begun to work out a description of the transmission mechanism from money to economic activity that relied on a more direct channel connecting changes in money with changes in income. Their report on their statistical findings published in the *Review of Economics and Statistics* (1963a) contained a

description of how an increase in money produced by an open market purchase would gradually produce changes in portfolio holdings and affect economic activity. Interest rates would be affected, but only as part of a larger process.

Friedman's famous essay "The Optimum Quantity of Money," published in 1969, (although circulating some time before) carried this sketch a step further. Here he began with a simple, and now famous, thought experiment: a helicopter flies over an economy that relies on fiat paper money and drops more. Friedman concludes that in this simplified economy spending will increase producing an increase in nominal income. If the economy is at full employment, the result will be higher prices. He argues, moreover, that this conclusion holds even as more sophisticated institutional arrangements are added to the model.[25]

Another reason that Friedman and Schwartz may have deemphasized the bank-lending channel was empirical. In 1893 and 1907, the banking crises that most concerned Mitchell, the crises were associated with high nominal interest rates, but in the early 1930s, a crisis of major importance to Friedman and Schwartz, the crisis was associated with low nominal rates. In 1893 the stock market crash came in May 1893 and the banking panic in June. (Friedman and Schwartz 1893, 108). The monthly commercial paper rate also reached its peak of 15 percent in June, after averaging about four per cent in 1892.[26] In 1907 the stock market slide began in March and the first major bank failure occurred in October. The monthly commercial paper rate hit its peak of 10 percent, nearly twice the 1906 rate, in November and December. In the 1930s, however, events unfolded differently. The stock market crash occurred in October 1929 when the commercial paper rate also peaked at a little over 6 percent. The onset of the first banking panic, however, did not occur until a year later. And the commercial paper rate, which then stood at about three percent, was in the midst of a long decline, that seemed to be unaffected by the

banking crisis.

To account for events in the early 1930s, therefore, Friedman and Schwartz were forced to broaden their interpretation of the determinants of nominal interest rates. First of all, the rate of interest was determined in the market for credit and therefore influenced by factors outside the banking sector. The recession that began in the late twenties (and was perhaps intensified by the stock market crash) reduced the demand for loanable funds and the rate of interest. The channels running from changes in the stock of money to interest rates, moreover, were complex. The supply of bank loans declined because of the runs on the banks and the associated increase in bank reserve ratios, but the decline in economic activity produced by the decline in the stock of money working directly on spending, reduced the demand for loans even more, and contributed to the fall in economic activity.

For Friedman and Schwartz the complexity of the relationships between money and interest rates argued against seeing interest rates as a simple indicator of the lending capacity of the banks and against using interest rates as a guide to monetary policy. In the crucial period 1929 to 1932 nominal rates had given the misleading signal that monetary policy was "easy." Avoiding the worst-case scenario, a repeat of the Great Depression, was a good reason for focusing on the stock of money rather than nominal interest rates.

There were, it is true, other ways that interest rates could have been used as indicators of monetary policy. Although not stressed in the *Monetary History*, the decline in the price level, the result in part of the decline in the stock of money, lowered nominal rates while disguising the increase in real rates. This argument suggests that real interest rates would have worked better than nominal rates as an indicator of monetary policy in the Depression. Anna J. Schwartz (1981) later drew attention to the rise in real rates, as Allan Meltzer (Meltzer 2003, 730) did more

recently. Meltzer also pointed to the rising spread between high risk and low risk assets, a development that was also noted by Friedman and Schwartz (1963, 312), as an indication of the need for monetary expansion.

Mitchell was not breaking new ground in emphasizing the bank-lending channel. The debate between monetary economists who emphasized the bank-lending channel and those who emphasized the quantity theory and a direct connection between increases in money and increases in economic activity was heated throughout the nineteenth century. (Laidler 1004 [1999]). Mitchell, of course, was well aware of this debate. Laughlin, his thesis advisor at Chicago, supported the banking school, and Mitchell had, as Friedman pointed out, an unprecedented command of the economic literature as a whole.

The neglect of the bank-lending channel proved to be one of the many controversial aspects of the *Monetary History*. Alan Meltzer and Karl Brunner, the most formidable allies of Friedman and Schwartz when it came to monetary policy – it was Karl Brunner who coined the term "monetarism" – consistently argued that the credit channel had to be included in models of how monetary policy influenced the economy. (Brunner and Meltzer 1988).[27]

One of the few modifications of the Friedman-Schwartz view of the Great Depression that has become widely accepted by economic historians, moreover, is the argument developed by Ben Bernanke (1983) that the banking crises in the early 1930s produced changes in the lending practices of banks that made the depression deeper and longer than it otherwise would have been. Bernanke did not reject the possibility of the direct channel from money to income stressed by Friedman and Schwartz, but he argued that the failure of banks and the destruction of the value of collateral raised the cost of credit intermediation and depressed economic activity. Bernanke (1983, 5) could find no “exact antecedents” for his work. While he is undoubtedly right

that there are no exact antecedents, one can point to a strong family resemblance between Bernanke's bank-centric description of the causes of the Great Contraction and Mitchell's bank-centric descriptions of earlier panics such as 1907. Of course, by the time Bernanke wrote, Mitchell's work had ceased to have any direct influence on the profession.

4. The Chicago Connection

The *Monetary History* was an NBER book, but also a Chicago book. Friedman had studied at Chicago, although his Ph.D. was awarded by Columbia, and he was a professor at Chicago during the time he was working on the *Monetary History*. In Friedman's time as a student at Chicago there were four professors whose work appears to have influenced the *Monetary History*: *(*1) Henry Simons and (2) Lloyd Mints, both monetary theorists; (3) Jacob Viner, an expert on international trade, history of economic thought, and other areas including money; and (4) Henry Schultz, a pioneer in the estimation of demand curves.

In his NBER interview (Friedman 2002) remembered that he had taken courses with both Mints and Viner, but not with Simons, although Friedman recalled that he knew Simons and had read his work. All three were quantity theorists and in that sense, Friedman remembered, their ideas provided the framework for the *Monetary History*. In Friedman's memory, the key figure was Viner, whose one-quarter course opened Friedman's mind to the power of economic theory. In the interview Friedman (2002) did not see much of a connection between his work with Shultz and his work for the Bureau, although Friedman again emphasized his lack of introspection.

We can, however, conjecture influences of all four. One has to use the qualified phrase "conjecture influences" because we do not have, for the most part, a trail of footnotes that leads from the work of these scholars to the *Monetary History*, and because Friedman, who as noted

above describes himself as not very introspective, has not attempted to construct the links in retrospect.

Simons's most important contribution to monetary economics was his famous distinction between "rules" and "authority" in the conduct of monetary policy (Simons 1936). Simons argued that it was better to bind the Federal Reserve with a specific rule, rather than follow the undemocratic course of allowing it to use its discretion in setting monetary policy. Simons's preferred rule was stabilizing the price level. He considered the rule of stabilizing the stock of money or its growth rate, but thought that this rule would not work as long as close substitutes for money were ubiquitous. Substantial institutional reforms would need to be undertaken, such as his proposal for 100% reserve banking, before a monetary rule would be preferable to a price rule. Friedman (1959), on the other hand, rejected a price rule, because it would be hard to hold the Federal Reserve responsible for prices when they were influenced by so many non-monetary variables, and preferred a monetary rule, discounting the problem posed by close substitutes for money.

The *Monetary History* supported the case for a "Friedman Rule" by showing that a single, consistently defined monetary aggregate, basically currency held by the public plus all deposits held by the public in commercial banks, could explain over 90 years of monetary history in the United States. The evidence, on this issue, was augmented in the companion volume, *Monetary Statistics of the United States* (Friedman and Schwartz 1970). Over six hundred pages in length, this volume not only described the methods by which Friedman and Schwartz estimated the stock of money, fulfilling the Bureau mandate for a detailed report on how numbers were estimated, but also presented detailed arguments and statistical evidence to show that their simple sum monetary aggregate worked well throughout the period they examined. Thus, the *Monetary*

History and *Monetary Statistics* showed that Simons's concern that variations in the volume of close substitutes for money, or variations in their degree of "moneyness," would undermine the effectiveness of a rule based on a monetary aggregate was unwarranted.[28]

I am only aware, however, of one passage in the *Monetary History* that speaks explicitly to the value of a monetary rule. In looking back at the Great Depression Friedman and Schwartz note that the stock of money rose at an unusually rapid rate during the expansions from 1934 to 1936 and 1938 to 1941. Those unusually rapid increases, however, were justified by the unusually rapid decline from 1929 to 1933. They conclude their analysis of the Great Depression with the following passage. (Friedman and Schwartz 1963, 545).

> How different the history of that fateful dozen years might have been if the money stock had grown steadily at its average rate of 2½ per cent per year, let alone at the higher long-term historical rate, instead of first falling by one-third from 1929 to 1933 and then doubling from 1933 to 1941.

A monetary rule whatever its failings, in other words, would avoid the worst-case scenario: a repeat of the Great Depression.

Lloyd Mints, whom Friedman did study with at Chicago, is remembered mainly for his analysis of the "real bills doctrine."[29] (Mints 1938) This doctrine held that the goal of monetary policy should be to increase or decrease the money supply in response to changes in the "needs of trade." Moreover, the doctrine held that this goal could be realized by making sure that banks confined their lending to the discounting of "real bills" – loans arising from the purchase of grain, not loans arising from the purchase of land for speculation. One of the main problems with the real bills doctrine, according to Mints, is that the nominal values of real bills will rise during inflations and fall during deflations. Hence a monetary policy built on the real bills doctrine will simply ratify inflation or deflation, rather than stabilizing the price level. Friedman and Schwartz

(1963b, 169) refer to Mint's critique of the real bills doctrine when they criticize the Federal Reserve Act for partially incorporating ideas arising from the real bills doctrine. Overall, Friedman and Schwartz cite Mint's book three times for various purposes.

Mints criticized the Federal Reserve for its handling of the Great Depression. (Mints et al., 1946, 62-63, and Mints 1950). He pointed out that the Federal Reserve had permitted a massive decline in the stock of money during 1929-1931 and argued that this decline had contributed to the severity of the Depression. This point became a major conclusion in Friedman and Schwartz's interpretation of the Great Depression, and one of the key points that generations of economists have taken from the *Monetary History*. Mints's views on the role of Federal Reserve in the Great Depression, it is possible, formed part of the background that shaped the narrative in the *Monetary History*, but his views about the Federal Reserve's behavior in the early 1930s are not cited explicitly.

Jacob Viner was also a sharp critic of the Federal Reserve's policy in the early 1930s. In a symposium on Friedman's work Friedman (1972, 939-40) expressed his admiration for Viner's criticism of the Federal Reserve in the Depression and quoted extensively from Viner's public lectures in the early years of the Depression. He notes, for example, that in a talk given in Minneapolis on February 20, 1933 Viner strongly criticized the Federal Reserve.

> It is often said that the federal government and the Federal Reserve system have practiced inflation [an increase in monetary assets in Viner's lexicon] during this Depression and that no beneficial effects resulted from it. What in fact happened is that they made mild motions in the direction of inflation, which did not succeed in achieving it, did not succeed even in accomplishing 'reflation'; but which probably did slow up somewhat the rate of price decline …At no time … since the beginning of the depression has there been for so long as four months a net increase in the volume of bank credit outstanding. On the contrary, the government and Federal Reserve bank operations have not nearly sufficed to countervail the contraction of credit on the part of member and non-member [of the Federal Reserve system] banks. (Viner quoted in Friedman 1972, 939-40)

Friedman (1972, 940) went on to point out the connection with the *Monetary History*.

> Can anyone who knows my work read Viner's comments and not see the direct links between them and Anna Schwartz's and my *Monetary History* (1963) or between them and the empirical *Studies in the Quantity Theory of Money* (1956)? Indeed as I have read Viner's talk for the purposes of this paper, I have myself been amazed to discover how precisely it foreshadows the main thesis of our *Monetary History* for the depression period, and have been embarrassed that we made no reference to it on that account.

Friedman's remarks it seems to me document the influence of Viner, and the other Chicago monetarists, on the *Monetary History*. They also suggest that there may be links between other works and the *Monetary History*, such as between *Business Cycles* and the *Monetary History*, which cannot be established simply by reading footnotes, especially when the role of the earlier work was not to provide specific facts but to set the stage for the latter work.

In addition to the monetary theorists at Chicago (Simons, Mints, and Viner) one can also point to another figure as a possible influence on the *Monetary History* and the related work by Friedman and Schwartz on money: Henry Schultz. To be sure, Schultz, whose magnum opus *The Theory and Measurement of Demand* (1938) consisted of a series of studies of the demand for agricultural products, may seem a strange choice as a major influence on monetary economics. Nevertheless, Friedman did work closely with Schultz. Friedman served as Schultz's research assistant in 1934, and at several points in *The Theory and Measurement of Demand* Schultz acknowledges Friedman's contribution. More importantly, one can detect a number of parallels between Friedman’s work and Schultz’s, especially in Friedman’s famous essay "The Quantity Theory of Money – A Restatement.” (Friedman 1956).

Schultz took the theoretical ideas of Alfred Marshall and other theorists of demand and restated them so that they could be tested using then current statistical tools. Friedman’s essay can be read as a similar effort to take the ideas developed by Simons and Mints at Chicago, and

by Irving Fisher and other quantity theorists, and restate them so that they could be tested using statistical methods.[30]

Perhaps the most important argument in Schultz's book was that ordinary least squares could be used to identify the demand curve for an agricultural product. The supply of an agricultural product, say corn, Schultz argued, varied a lot compared with demand because of the weather, so an ordinary least squares regression of quantity of corn on the price of corn would identify the demand curve.[31] Friedman's strictly analogous argument was that the supply of money varied a lot compared with demand, because, for example, governments frequently resort to inflationary finance, and that therefore one can identify the demand for money with ordinary least squares.

Schultz's decision to include only a the price of corn and the prices of a few close substitutes in his demand function is strictly analogous to Friedman and Schwartz's decision to include only a few variables including the own rate of return on money (for currency, the rate inflation) and the rates of return on bonds and equities in the demand function for money.

When faced with plausible alternative estimates of the supply of corn, Schultz solved the problem by choosing the estimate that was most highly correlated with the price of corn. Friedman and Schwartz applied the same empirical approach to choosing the best estimate of money. They constructed a series of plausible alternatives – M_1 which included currency and demand deposits, M_2 which included currency and all deposits at commercial banks, M_3 which included additional money-like assets, and so on – and chose the estimate M that was most highly correlated with income. This approached is spelled out in *Monetary Statistics*, and provides the basis for using M_2 in the *Monetary History*.

Finally, just as Schultz's ultimate purpose was to use his estimates to evaluate the effects of New Deal agricultural policies, such as output restrictions; Friedman and Schwartz's ultimate purpose was to use his estimates to evaluate the effect of alternative monetary policies.

The issue of what influence non-Chicago economists, especially Keynes, had on Friedman and Schwartz became extremely contentious. In the opening section Friedman claimed in his1956 paper that he was merely developing Chicago's "oral tradition;" Keynes was not mentioned. This contention was attacked by Don Patinkin (1969, 1972, and 1975) and Harry Johnson (1971) who saw the reference to an oral tradition that apparently included interest rates in the demand for money as a way of bringing in Keynesian ideas without mentioning Keynes. It would take us too far afield to go in to the details of this debate. However, we should note that Friedman had his defenders as well as detractors. Frank Steindl (1990) pointed out that Simons had referred to the oral tradition at Chicago in a review published in 1935 of a book by Currie on monetary conditions in the early 1930s. And George Tavlas (1998) showed that Friedman's approach was anticipated in the work of Knight and Mints.

Further evidence of the Friedman's deep understanding of Mitchell, and of the way that the Chicago emphasis on economic theory shaped that understanding, is to be found in Friedman's admiring (1950) essay "Wesley C. Mitchell as an Economic Theorist," to which we alluded above.

5. Friedman's Mathematical Restatement of Mitchell's Theory

In this essay Friedman provided a mathematical model of Mitchell's theory of the business cycle. If we look closely at this model, the starting point of the *Monetary History* is clear. In what follows I have used the symbols that Friedman and Schwartz used in later work

because these symbols are likely to be more familiar, rather than the symbols Friedman used in his original essay. I have also suppressed some of the nonmonetary equations in the Mitchell-Friedman model.[32] Friedman, for example, modeled Mitchell's conclusions about the important role of final product prices and costs in determining profits and the level of investment spending. Here, however, I will concentrate on Friedman's attempt to model Mitchell's conclusions about the relationships between the monetary variables and other sectors of the economy. I will, however, consider three equations Friedman included that describe a simple Keynesian multiplier model. The inclusion of these equations was a precursor to Friedman's (1970a) attempt to find a simple "common model" that could be specialized as a Keynesian or Monetarist model.

Define the following terms.

Y = total national income

C = consumption (induced expenditures)

I = investment (autonomous expenditures)

H = high-powered money (gold or paper money issued by the government)

$= C_p + R$

C_p = currency in the hands of the public

D = deposits in the hands of the public

M = money = $C_p + D$

M^d = money demanded

M^s = money supplied

R = bank reserves = currency held by banks

b = the bank's ratio of deposits to reserves = D/R

b* = the bank's desired ratio of deposits to reserves

p = the public's ratio of deposits to currency = D/ C_p

λ = a shift term for a financial panic

i = the rate of interest

Let us start with the banks. By definition the actual ratio of deposits to reserves is

(1) $b = D/R$

Based on Mitchell's empirical observations Friedman assumed the desired amount of deposits created by banks per dollar of reserves, b*, to be a function of the current level of income, and the presence or absence of a banking panic.

(2) $b^* = f_1(Y, \lambda)$

The variable λ is my addition, a shift term introduced to clarify how a banking panic would affect the economy. The partial derivative of b* with respect to income, f_1^1, is assumed to be positive. When income increases in a cyclical expansion, lending opportunities improve, and so banks reduce the amount of reserves they wish to keep behind each dollar of deposits. The partial derivative of b* with respect to a panic, f_1^2, is assumed to be negative: in a panic banks want higher reserves to protect themselves against runs, so they reduce lending.

The interest rate, i, is assumed to be dependent on the relationship between the actual and desired ratios of the banking system.

(3) $i = f_2(b^* - b)$

The derivative f_2^1 is assumed to be negative. If the desired bank ratio exceeds the actual bank ratio banks will increase lending, and increased lending will lower interest rates. Equation (3) is part of the bank-lending channel that Mitchell stressed, Friedman and Schwartz downplayed, and

recent research has again brought forward.

There is also a demand for money equation. This relationship became central to Freidman and Schwartz's thinking about the role of money.

(4) $M^d = f_3(Y, i)$

Friedman adopts the standard assumptions that f_3^1 is positive, because money is a normal good, and f_3^2 is negative, because the interest rate is the cost of holding money. In later work, for example (Friedman 1956) and (Friedman and Schwartz 1982), Friedman and Schwartz elaborated on this simple equation, and took into account wealth, the interest paid on deposits, inflation, and other variables. The estimation of demand for money equations for a time was a major cottage industry for economists.

The ratio of deposits to currency is a function of the level of income, and the panic shift term.

(5) $p = f_4(Y, \lambda)$

The partial derivative of p with respect to income, f_4^1, is assumed to be positive – as income increases the demand for deposits grows relative to currency because deposits require more sophistication, but are capable of handling larger and more complex transactions. And the partial of derivative of b with respect to the panic shift, f_4^2, is negative – in a panic people want high-powered money.

Finally if we combine the definitions of M^S, H, b, and p, from the initial list of variables we can derive an equation explaining the stock of money in terms of its "determinants."

(6) $M^S = H*b(1+p)/(b+p)$

This is, with some changes in symbols, is equation (12) of Appendix B of the *Monetary History*.

Friedman and Schwartz (1963b, 791).

On one level this equation is merely an identity derived by manipulating the definitions of M^S, H, b, and p.[33] However, equation (6) can also be viewed as an equation that explains the relationship between the stock of money and the business cycle. It elegantly separated the determinants of the stock of money during Mitchell's era into elements Mitchell identified as independent of the business cycle from elements he identified as dependent. H includes gold, greenbacks, and national banknotes. The amount of gold, although influenced by economic conditions, was also subject to important influences that were independent of the business cycle such as the discovery of new gold fields and the discovery of new technologies for securing gold from gold ore. The government determined the amount of greenbacks outstanding. The third component of high-powered money, national banknotes, is a more complex case.

Mitchell, as we noted above, thought that bank issued currency was an intermediate case because at the time he was writing national bank notes were backed by Federal government bonds. Friedman and Schwartz wrestled with this issue as well. Ultimately they decided to include bank-issued currency in their definition of high-powered money. They (1963b, 780-81) pointed out that national bank notes had to be backed by Federal government bonds, were guaranteed by the Federal government, and that after 1874 the Treasury had "control over the amount outstanding, through its decisions that determined the volume of bonds bearing the circulation privilege and the interest coupon on them."[34] The last consideration echoes Mitchell's emphasis on supply independence.

If we take the total derivative of (6) we get the following.

(6') $dM^S/M^S = dH/H + [p/(b+p)]db/b + [p(b-1)/(1+p)(b+p)]dp/p$

This equation – equivalent to equation (22) in Appendix B of the *Monetary History* (Friedman

and Schwartz 1963b, 794) – shows that the stock of money will increase if the amount of high-powered money increases, the amount of deposits banks create with each dollar of reserves increases, or the amount of deposits that the public wants to hold relative to each dollar of currency it holds increases.[35]

Suppose that we also take the total derivatives of equations (4) and (5), and combine with (6'). To keep things simple, suppose that we also assume that λ is zero (there is no panic), and that the interest rate i, the bank money ratio b, and the public's money ratio p are fixed. Then we have a quantity theory model specialized to show the effect of an increase in high-powered money.

(7) $dY/dH = [b(1+b)/(b+p)](1/f_3^1)$

A change in high-powered money, H, is multiplied by the money multiplier, [b(1+b)/(b+p)], to become a change in the total stock of money, and that amount in turn is multiplied by velocity, $1/f_3^1$, to become a change in income. An influx of gold, for example, would increase H and raise nominal incomes.

Similarly, we can derive the effect of a financial panic on the level of economic activity through its effects on the stock of money. To keep things simple in this case we can hold H, the amount of high-powered money, b, the bank ratio, and i, the interest rate constant. With those assumptions we have a simple quantity theory simplified to show the effect of a financial panic.

(8) $dY/d\lambda = f_4^2/\{f_3^1 - f_4^1[H(b-1)b/(b+p)^2]\}$

This expression is more complex. The sign of the numerator is negative because f_4^2, the effect of a banking panic on the demand for deposits relative to currency, is negative. The sign of the denominator, however, is in principle ambiguous because the effect of income on the demand for

money f_3^1 and the effect of the fall in income on the demand for bank money f_4^1 are both positive. If f_3^1 is sufficiently large compared with f_4^1, $dY/d\lambda$, the effect of the banking panic on income will be negative. Equations (6), (7) and (8), are the workhorses of the *Monetary History,* although only (6) is developed explicitly. Most of the discussions in the *Monetary History* are, in other words, descriptions of why the stock of money changed, and of how the changes in the stock of money produced changes in the economy.

To see the bank-lending channel that Mitchell stressed, we can combine equations (2) and (3) with the "Keynesian" part of the Mitchell-Friedman model. First we have a simple definition of income as a sum of consumption and investment.

(9) $Y = C + I$

Consumption is assumed to be a simple function of the level of income.

(10) $C = f_5(Y)$

where, of course, $f_5^1 > 0$. And investment is assumed to be a function of the rate of interest.

(11) $I = f_6(i)$

where, $f_6^1 < 0$.

Combining the equations (2) and (3) with (9), (10), and (11) yields a simple Mitchell-Friedman-Keynes model that emphasizes the bank lending channel. Perhaps the most interesting case is a financial panic. Taking the total differentials of equations (2), (3), (9), (10), and (11) and rearranging terms yields.

(12) $dY/d\lambda = (f_1^2 f_2^1 f_6^1)/(1- f_5^1 - f_1^1 f_2^1 f_6^1)$

This expression, although somewhat complex, is clearly negative; a panic reduces income. The numerator is negative because it is the product of the negative effect of the panic on the banking

system's desired ratio deposits to reserves (f_1^2), the negative effect of lower desired deposit-reserve ratios on interest rates (f_2^1), and the negative effect of higher interest rates on investment (f_6^1). All this is multiplied by 1/ (1- f_5^1 - f_6^1 f_2^1 f_1^1) which is simply an augmented form of the simplest Keynesian multiplier 1/(1- f_5^1).

The next step would be to derive the full impact of a financial panic on economic activity by combining the direct channel, emphasized by Friedman and Schwartz, and modeled in equation (8), and the bank-lending channel, emphasized by Mitchell, and modeled in equation (12). The resulting equation, however, is quite complex and does not suggest new insights.

To sum up, the elements that Friedman included in his later efforts to develop a formal model of the role of money – a supply of money function that distinguishes between high-powered and low-powered money, a set of general equations that can be specialized as a Keynesian or Quantity Theory model, and the direct connection between money and income – can be found in his 1950 model of Mitchell's theory of the business cycle. Mitchell's emphasis on a bank-lending channel, although modeled in the 1950 paper, however, never played a role in the *Monetary History* or related work.

6. The Critics of Monetarism

When looking at the early reception of the *Monetary History* it is hard to escape the realization that much of the difference between Friedman and Schwartz and their critics resulted from the willingness of Friedman and Schwartz to follow Mitchell in placing great weight on historical evidence; while the critics were looking for mathematical models and statistical tests. The critics were sure that Friedman and Schwartz had failed to understand that correlating

changes in money and changes in economic activity proved nothing about causation. Money might be correlated with national income because changes in money caused changes in national income, or because changes in national income caused changes in money, or because money and changes in national income were produced by some third factor, say changes in fiscal policy. In short, Friedman and Schwartz had confused correlation with causation. Their critics recognized that at some points Friedman and Schwartz had noted the hazards involved in attempting to read causation from charts of economic variables, but the critics insisted that Friedman and Schwartz had nevertheless fallen into this trap.

Perhaps the best known attack on the work of Friedman and Schwartz along these lines was James Tobin's 1970 paper, "Money and Income: Post Hoc Ergo Propter Hoc?" Tobin presented an "ultra Keynesian" model, in which money didn't matter, that fit Friedman and Schwartz's findings regarding the relationship between money and income better, according to Tobin, than did a "Friedman model." Tobin (1970, 301) mentioned the *Monetary History* as a source of evidence, but focused his firepower on the timing evidence. In his reply Friedman (1970b) stressed the important role that the historical evidence played in his conclusion that money mattered.

Peter Temin in his book *Did Monetary Forces Cause the Great Depression*? (1976) made a point similar to Tobin's. Temin's criticism has been especially influential among economic historians. Temin (1976, 3) argued that Chapter 7, "The Great Contraction, 1929-33," of the *Monetary History* could be discussed independently from other chapters. After a close reading Temin found that the chapter was merely a detailed description of how the supply of money and other variables had changed during the contraction. He then draws the following conclusion from this finding. (1976, 30-31).

> Friedman and Schwartz's main conclusions are that the level of income fell as sharply as it did in the early 1930s because of a massive fall in the stock of money. This stock in turn fell because of sustained effects of multiple banking crises, that is because of a restriction in the supply of money. But an account of the supply of money cannot be taken for an account of the stock of money unless it is known that demand plays no role. The *Monetary History* appears to have been designed to show just this – but it turns out to be a narrative based on such an assumption, not an argument for it. Friedman and Schwartz referred elsewhere [in other articles and books] to The *Monetary History* to show that the stock of money was historically determined independently of income and that the correlation between money and income therefore must be interpreted to mean that movements in the stock of money determine movements of income. The *Monetary History*, however, does not provide independent evidence for this proposition. It follows that the hypothesis about the cause of the Depression must be regarded as unproven as well.

Temin's preferred methodology for determining causative relationships is to look for the contrasting predictions of different hypotheses. What he terms the money hypothesis (the Friedman and Schwartz hypothesis), he argues, predicts a rise in short-term interest rates during 1929-33, and what he terms the spending hypothesis (the Keynesian hypothesis) predicts a fall in short-term rates. Since short-term rates fell, he concludes that monetary forces did not cause the Depression. The emptiness that Temin finds in the Friedman and Schwartz narrative of 1929-33 is the result, partly, I believe, of a decision to reject the Mitchell-Friedman-Schwartz method for determining causation. Friedman and Schwartz's chapter 7 is indeed essentially a narrative constructed on the assumption that money matters. It contributes to the case for believing that money matters only to the extent that it provides another data point. For Friedman and Schwartz the conclusion that the decline in the stock of money aggravated the contraction, and that it could have been prevented by appropriate policy measures, follows from the whole body of evidence.[36]

The independence of monetary change from the cycle that Temin is looking for is clearly useful for establishing that monetary change has an effect on the economy. Natural experiments such as the increase in gold at the turn of the century are more informative than the more

endogenous movements in the stock of money in the great contraction. However, every case adds or subtracts something from the persuasiveness of the overall generalization. The claim, to put it somewhat differently, that the patient would have perked up if given "Buck-You-Up-O" cannot be proven by looking at one case.[37] This claim can only be demonstrated by looking at a "large" number of cases.

7. Monetary Clinicians

Both Mitchell and Friedman and Schwartz can be likened, to push the analogy further, to medical researchers undertaking a clinical study. Case histories are written up and compared, and reasonable inferences are made. Patients who were treated with Buck-You-Up-O recovered more quickly from the influenza than patients who were not treated. A natural experiment may increase the persuasiveness of the argument. A pair of identical twins came down with the flu. The twin who was given Buck-You-Up-O recovered more rapidly than the twin who was not treated. Still, a clinical study cannot have the authority of a large-scale double-blind scientific experiment. Even twins may differ because of life experiences, because they contracted different versions of the flu, or for other reasons.

Each episode that Mitchell or Friedman and Schwartz investigate is a separate case study. The Great Contraction taken as a whole, as we noted above, is essentially a single case history.[38] By itself it proves little as Temin and others have noted. The patient was suffering from a severe case of the flu, the doctor failed to give the patient Buck-You-Up-O, and the patient took a long time to recover. This case is consistent with the idea that Buck-You-Up-O reduces the severity of the flu, but not much evidence in itself.

The natural point at which to draw conclusions about causation from case studies is at the

point when cases can be compared and contrasted. This is what Friedman and Schwartz do in their final chapter, "A Summing Up," although comparisons that bear on causation are scattered throughout their narrative. In the final chapter Friedman and Schwartz (1963b, 693-94) compare the 1929-33 contraction with the 1907 crisis.

> Comparison of the 1907 banking panic under the earlier system [before the Federal Reserve] and the closely similar liquidity crisis which began in late 1930 offers strong evidence for this judgment. [that the 1930 crisis was handled badly] If the earlier system had been in operation, and if everything else had proceeded as it did up to December 1930, the experience of 1907 strongly suggests the there would have been a more severe initial reaction to the bank failures than there was in 1930, probably involving concerted restriction by banks of the convertibility of deposits in to currency. The restriction might have had more serve initial effects toward deepening the economic contraction than the persistent pressure on the banking system that characterized late 1930 and early 1931 had. But it would have cut short the spread of the crisis, would have prevented cumulation of bank failures, and would have made possible as it did in 1908 recovery after a few months.

This, it seems to me, is a reasonable way for a clinician to proceed. The two patients, patient number 1907 and patient number 1930, appear to have been suffering from the same condition, a severe case of the flu. Patient number 1907 was given Buck-You-Up-O; patient number 1930 was not. Patient number 1907 suffered a sharp intensification of his condition, but then recovered quickly; patient number 1930 suffered a prolonged decline and slow recovery. Therefore, this comparison suggests that Buck-You-Up-O is effective. A reasonable argument, of course, is not a foolproof argument. Another clinician might conclude that patient 1907 was suffering from a less severe case of the flu. And the constitution of the two patients was not identical: Patient number 1930 may have been weaker. When looking at the Great Depression we can easily create reasons why the 1930 case may have been different from the 1907 case. For one thing, the 1930 case occurred after World War I had shaken confidence in the gold standard and other fundamental economic institutions.[39]

The next step in a clinical study is to summarize the weight of the evidence suggested by the individual cases. It is interesting that here Friedman and Schwartz (1963b, 689-90) turn explicitly to a medical analogy.

> The strength of the evidence furnished by those three quasi-controlled experiments [the adoption of restrictive monetary policies in 1920, 1931, and 1936] can be made clearer by an analogy. Suppose we had the medical records of 42 married couples (to match the 42 years of Federal Reserve history from 1919 to 1942 …) Suppose 3 men and 4 women were found to have a specified illness; suppose 3 of the 4 women turn out to be the wives of the 3 men with the same illness. The presumption that the illness was contagious would certainly be very strong.…

In fact the probability of this result by chance, according to Friedman and Schwartz would be 1 in 2,870.[40]

Although Friedman and Schwartz were more systematic and self conscious than Mitchell about using historical case studies as raw materials for drawing conclusions about the effectiveness of monetary policy, their inductive methodology is clearly an extension of the practices of Mitchell and the other business cycle researchers at the National Bureau.

8. Explaining the Continuity.

The institutional origins of the *Monetary History* explain the similarities in methodology, the clinical methodology. Mitchell sketched the forces producing a typical business cycle in *Business Cycles* (1913). At the Bureau researchers then explored in detail each area that Mitchell had subjected to a preliminary investigation. These studies followed the "Bureau Methodology." Long time series of monthly data (where possible) were constructed, the behavior of the time series during the Bureau-defined cycles was computed, and an explanatory narrative was constructed. Although the *Monetary History* is the best known of these studies, many became classics. Friedman and Schwartz themselves made use of Frederic Macaulay's (1938) study of

interest rates, Burns's (1960) study of production, John Kendrick's (1961) study of productivity, and of course, Simon Kuznets's studies of national income, such as Kuznets (1961).

The similarity in methodology between *Business Cycles* and the *Monetary History*, however, does not explain the similarity in conclusions. What then explains it? Could the similarity in conclusions have been the result of some sort of subtle pressure to reinforce the founder's conclusions? This seems unlikely. The atmosphere at the Bureau was one in which researchers were free to pursue their research wherever it led. Some of Friedman and Schwartz's early decisions, moreover, went against Bureau tradition. Friedman and Schwartz, as we noted, rejected Mitchell's bank-centric view of the transmission mechanism. Friedman and Schwartz, moreover, emphasized the growth rate of the stock of money in some of their work, and linked changes in the growth rate of money with turns in the business cycle. Anna Schwartz (2001) recalled that this decision did not receive a warm reception from Burns, the President of the Bureau, or Geoffrey Moore, the research director. Finally, in response to an earlier draft of this paper that had suggested a direct influence Anna J. Schwartz wrote that any influence "… must have been subliminal. I am unaware of any discussion I had with Milton linking Mitchell's work with ours."[41]

Friedman and Schwartz, moreover, were familiar with many ideas besides those produced by the National Bureau. Frank Steindl (2004) has shown, based on lecture notes from a course that Friedman gave at the University of Wisconsin, that from his earliest days in academic life Friedman was familiar with a remarkably wide range of literature on the business cycle. It seems unlikely then that a lack of the awareness of alternatives to Mitchell's vision prevented Friedman and Schwartz from moving away from his positions. Indeed, as we showed above, Friedman and Schwartz rejected Mitchell's emphasis on the credit channel, and incorporated ideas drawn from

the Chicago economists of the 1930s.

It was probably simply the findings uncovered in the course of their research that in the end reinforced the conclusions Mitchell had reached based on his exploration of the period 1890-1911. The underlying stability of monetary relations, in other words, explains the continuities between *Business Cycles* and the *Monetary History*. If a small clinical study showed that taking aspirin reduced the frequency of heart attacks, and if a larger longer study of the effects of taking aspirin on the frequency of heart attacks confirmed the results of the earlier study, the explanation for the similarity in findings would simply be that taking aspirin did in fact reduce the frequency of heart attacks.

9. Conclusions

One of the best ways to understand what Friedman and Schwartz accomplished in the *Monetary History* is to compare it with (what I argue) is its closest predecessor in methodology and in many of its most important conclusions: Wesley Clair Mitchell's *Business Cycles* (1913). Five propositions, I have argued, summarize the key similarities and differences.

(1) Like Mitchell, Friedman and Schwartz concluded that some changes in the stock of money could occur for reasons independent of current and future changes in economic activity. The increase in the supply of money beginning in the late 1890s that resulted from the discovery of new gold fields in South Africa and other countries and new methods of extracting gold from ore was cited both by Mitchell and by Friedman and Schwartz as a clear example of an independent change in the stock of money.

(2) Like Mitchell, Friedman and Schwartz found that changes in the stock of money had important effects on the economy. They agreed with Mitchell, for example, that the expansion of

the stock of gold from 1897 to 1914 had raised prices.

(3) Like Mitchell, Friedman and Schwartz argued that the combination of independent changes in money, which could be observed by looking closely at the historical circumstances in which changes in money occurred, and the resulting changes in the economy provided persuasive evidence that money mattered. Friedman and Schwartz, building on important work by Philip Cagan, however, were more systematic and self-conscious about this inference.

(4) Like Mitchell, Friedman and Schwartz worked on the assumption that money was merely one of many factors producing business cycles. Although Friedman and Schwartz did not attempt to provide a comprehensive discussion of the non-monetary factors that affected the business cycle, they discussed non-monetary forces when they believed them to be important. For example, they discussed the coincidence of good harvests in the United States with poor harvests in Europe on a number of occasions in the nineteenth century, the stock market crash of 1929, wartime mobilizations and demobilizations, and the rise of labor unions in the 1930s. Most importantly, they assumed that there was an inherent tendency for the economy to recover from recessions. In many cases, however, they simply assumed the existence of a nonmonetary cycle and described how it was affected by changes in monetary policy. The role of monetary policy during a recession, for example, might be described, as was the effect of the decrease in the stock of money in the 1937-38 recession as "a factor that significantly intensified the severity of the decline." (Friedman and Schwartz, 1963b, 544).

(5) Unlike Mitchell, Friedman and Schwartz ignored the bank-lending (credit) channel. Mitchell thought that changes in bank lending policies were the most important part of the transmission mechanism linking changes in money with changes in economic activity. An unexpected increase in bank reserves would produce an increase in bank lending, a fall in interest

rates, and an increase in investment that would generate increased economic activity. Friedman and Schwartz, on the other hand, stressed a direct channel from increased money to increased economic activity and downplayed the bank-lending channel. It is interesting to note that one of the major developments in monetary economics in the decades after Friedman and Schwartz wrote was a partial return to the bank-centric view of the role of money.

Both Mitchell in 1913 and Friedman and Schwartz in 1963, to sum up, can be described as monetary clinicians. They summarized cases in which the economic patient was given more or less money, and described the reasons for the change in the dosage and the consequences. They understood that one case study taken separately – the increase in the gold supply after 1896, the panic of 1907, or even the Great Contraction from 1929-1932 – could prove little by itself. They argued, however, that taken together the case studies suggested persuasive generalizations about the role of money in the business cycle.

The *Monetary History*, of course, was not simply an extension of *Business Cycles* (1913). Friedman and Schwartz employed their own ideas, and those of other economists including major figures at Chicago in the 1930s – Henry Schultz, Jacob Viner, Lloyd Mints, and Henry Simons – to sharpen the analysis. The influence of Schultz can be seen mainly in the statistical analysis that was not reported in the *Monetary History*, but rather in other places, such as *Monetary Statistics* (1970) and *Monetary Trends* (1982). However, the other Chicago economists left an imprint on the *Monetary History* itself. From Viner and Mints came, among other things, the sharp criticism of the Federal Reserve for failing to stop the contraction of the stock of money in the early 1930s – monetary policy had failed not because it was not effective, but because it was not tried. From Simons came the important distinction between "rules versus authorities" in the conduct of the monetary policy. Friedman and Schwartz did not follow Simons in opting for a

price-stability rule over a money supply rule. However, they addressed his concerns when they considered alternative policies in the depression. "How different the history of that fateful dozen years might have been if the money stock had grown steadily at its average rate of 2½ per cent per year, let alone at the higher long-term historical rate, instead of first falling by one-third from 1929 to 1933 and then doubling from 1933 to 1941." (Friedman and Schwartz 1963b, 545).

Although the ideas came from many sources, the methodology of the *Monetary History*, to reiterate, came from Mitchell and the NBER. As Bordo and Schwartz (2004) show, Friedman usually summed up the methodological differences between himself and his many critics by explaining that he was a Marshallian while his critics were Walrasians. He, like Marshall, preferred using simple abstract theories to explain facts, rather than complex general equilibrium models. The term Marshallian is undoubtedly the best simple description of the methodology underlying his work as a whole. When it comes to characterizing the methodology of the *Monetary History* taken separately, however, the best one-word term may be Mitchellian.

Table 1. Sources Cited in the *Monetary History*		
Author	Book or Article	Citations
The top six authors		
Friedman, Milton	Various articles and books	29
Mitchell, Wesley C.	On Civil War era (10), on business cycles (7)	17
Sprague, Oliver M.W.	*History of Crises under the National Banking Act*	14
Cagan, Phillip	*Determinants and Effects* (9) and "The Demand for Currency" (3)	12
Warburton, Clark	Various journal articles	11
Fisher, Irving	Various articles and books	4
Other		
Hayek, Friedrich A.	*Prices and Production, etc.*	0
Keynes, John Maynard	*The Treatise on Money, The General Theory, etc.*	0
Schumpeter, Joseph A.	*Business Cycles, etc.*	0

Note: I have excluded responses to criticisms or comments on earlier drafts. I have also excluded references to the diaries of Charles S. Hamlin and George L. Harrison that Friedman and Schwartz cited repeatedly in their analysis of monetary policy making during the Great Depression.

Source. Friedman and Schwartz (1963b)

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Endnotes

[1] I broached some of the ideas that I develop here in Rockoff (2000).

[2] A JSTOR search over all journals and years for the combination of Friedman and "Monetary History" produced 557 hits, while Keynes and "General Theory" produced 2,524. Since 1990 the figures are 192 and 369. A search of the Social Science Citation Index for the period from 1997 forward revealed 349 citations to the *Monetary History* and 960 to the *General Theory*.

[3] Robert E. Lucas (1994), Jeffrey Miron (1994), and Anna J. Schwartz (2004) discuss the continuing influence of the *Monetary History* on monetary economics.

[4]Although Friedman and Schwartz modestly styled their work *A Monetary History*, most commentators, friend and foe alike, refer to *The Monetary History*, a usage I will follow.

[5] Reviews by C. A. E. Goodhart (1964) and Harold T. Shapiro (1965) struck similar notes.

[6] Burns had been an influential teacher of Friedman at Rutgers University where Friedman had done his undergraduate work. It was Burns who initially brought Friedman into the Bureau. (Friedman 2002.)

[7] Stewart, an influential economist at the Federal Reserve Board and a director of the National Bureau, had many connections to Mitchell. Before WWI he had taught a course at Amherst on business cycles based on Mitchell's work, shortly afterwards he had worked with Mitchell on a project on wartime prices, and in 1922 he became Director of the Division of Research and Statistics at the Federal Reserve Board on Mitchell's recommendation. (Rutherford 2001).

[8] My summary of Mitchell's career is based on Burns (1949).

[9] Its full title is *A History of the Greenbacks, with Special Reference to the Economic Consequences of Their Issue: 1862-65*.

[10] Friedman developed this idea further in his "plucking" model – the economy was like a string on a musical that could be plucked but would return to its normal shape. (Friedman 1969 [1964], 1993).

[11] Friedman took Mitchell's course on business cycles at Columbia but remembered it as dull [Friedman 2002].

[12] Letter to Arthur Burns, April 12, 1949, Milton Friedman Papers, uncatalogued. Daniel Hammond located this important letter and shared it with me. It is suggestive that the term "analytical description" that Friedman used here to describe Mitchell's work is similar to the term "analytical narrative" that Friedman and Schwartz used to describe the suggestion made by Walter Stewart that led to the *Monetary History*.

[13] Friedman and Schwartz reserved the term “money supply" for the change in the stock of money.

[14] Here Mitchell was reaching a conclusion different from his advisor at Chicago, Laughlin, who was a critic of the quantity theory, and firmly rejected (Laughlin 1911) the notion that the increase in the stock of gold had produced the increase in prices. Laughlin preferred to attribute the increase in prices to increases in costs and increases in monopoly.

[15] A recent estimate of GDP preserves the contrast. Real GDP rose 3.6 percent per year from 1882 to 1892, but only 2.6 percent per year from 1903 to 1913. The GDP deflator fell .8 percent per year from 1882 to 1892, but rose 1.7 percent per year from 1903 to 1913. (Johnston and Williamson 2005).

[16] In Mitchell's view the crisis in Britain never degenerated into a panic.

[17] Mitchell’s claim that the Bank of England carried reserves far in excess of its immediate requirements was an exaggeration. In some cases, such as the Barings Crisis (1890), the Bank had to rely on the cooperation of private British banks or foreign central banks to make adequate reserves available.

[18] In the *Monetary Trends* Friedman and Schwartz (1982) compared American and British experiences, although in that volume the emphasis was more on statistical than historical analysis. In the *Monetary History* Friedman and Schwartz did not systematically examine events in other countries although they did, occasionally, refer to experiences in other countries. For example, they (1963b, 352) used a comparison of the U.S. and Canada in the Great Contraction to analyze the effects of bank failures. (There were many in the U.S. and none in Canada.)

[19] The conclusions in this case are somewhat different. Comparing the mildness of 1907-08 in England, which had a central bank, with the severity of 1907-08 in the United States, which lacked a central bank, supports the case for central banking. Comparing 1907-08 in the United States with 1929-33 in the United States raises doubts about central banking.

[20] One is tempted to write "real and monetary forces" and to describe a "real cycle" that would exist even in the presence of an optimal monetary policy. The term real business cycle, however, has come to identify a line of thinking that would exclude some of the mechanisms that Mitchell included in his list of non-monetary forces.

[21] Mitchell (1959 [1913], 115) also thought that a large harvest sold at high prices had mitigated the effects of the 1907 crisis in England.

[22] Although the combination may have been fortuitous from the point of view of the economy, there may be, of course, an underlying meteorological explanation.

[23] In the last case Friedman and Schwartz were looking at the balance of payments, so the effect on the economy they had in mind may have run from exports to the balance of payments to the supply of gold to economic activity; in other words, a monetary channel.

[24] Howard Sherman (2001) discusses many of the non-monetary components of Mitchell's analysis of the business cycle.

[25] Students from my generation at Chicago – I started in 1967 – may remember Professor Friedman asking his class in monetary economics, where the "optimum quantity" was a basic text, why people who collected the cash dropped from the helicopter would immediately spend it rather than hoard it. The answer that I was contemplating: "that's what I would do," although not completely irrelevant, was not the correct answer. The correct answer was that since by assumption the economy had already reached its long-run equilibrium, each person separately had already achieved their desired ratio of money to income and therefore would feel free to spend the additional cash.

[26] There was a run-up in the fall of 1892, with the monthly rate peaking at 10 per cent in December. The rate is the end-of-month 3-year commercial paper rate, available at http://www.globalfinancialdata.com.

[27] This paper sums up their views and contains references to some of their earlier writing on this issue. Laidler (1991) provides a good retrospective account of Brunner's work.

[28] In the postwar era there were changes in the banking system – the introduction of deposit insurance, and the heavy investment by commercial banks in government bonds during the war – that moved the system in the direction that Simons thought was necessary to make a money rule workable.

[29] In my time at Chicago, as I remember, the doctrine was always referred to as the "Fallacious Real Bills Doctrine."

[30] While working with Schultz, Friedman evidently read widely in the literature on the measurement of demand curves. It was this literature, I believe, that is brought to bear in the 1956 paper. I am grateful to Dan Hammond for making the need to be careful at this point clear to me.

[31] Alternatively, one could regress price on quantity. Schultz paid close attention to measurement

error, and how measurement error would affect the elasticity estimates derived from regressions of quantity on price compared with regressions of price on quantity. Friedman and Schwartz also paid close attention to this distinction in their subsequent empirical research.

[32] Five of the eleven equations in Friedman's model concern the monetary sector.

[33] By definition M=Cp+D and H = S+R. Dividing M by H, rearranging terms, and substituting b for D/R and p for D/Cp yields equation (6).

[34] Most of the bonds carrying the circulation privilege were held by banks.

[35] Normally b will exceed 1 because normally banks lend out part of their deposits and hold only fractional reserves.

[36] Friedman and Schwartz, as David Laidler pointed out to me, may have encouraged the idea that their discussion was intended as proof by itself that monetary forces had caused the Great Depression by publishing Chapter 7 as a separate book, *The Great Contraction, 1929-1933* (1965).

[37] From P.G. Wodehouse.

[38] In monetary history one can have cases within cases. In 1932 there was a brief time in which the Federal Reserve deliberately switched to an expansionary policy of open market purchases of bonds. Friedman and Schwartz drew attention to this case as a natural experiment on the effectiveness of an expansionary policy within the confines of the Great Contraction.

[39] This point was developed by Abraham Hirsch and Neil de Marchi (1990, 236-37).

[40] The probability of the first sick husband being paired with a sick wife if the pairing is random is 4/42. The probability of the second sick husband being paired with a sick wife if the pairing is random and if the first sick husband was paired with a sick wife is 3/41. The probability for the third sick husband being paired is 2/40. The probability of all three sick husbands being paired with sick wives is therefore (4/42)*(3/41)*(2/40) = 1/2870.

[41] Personal Email from Anna J. Schwartz, March 6, 2006.