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History as Heresy: Unlearning the Lessons of Economic Orthodoxy

The Tawney Memorial Lecture 2021

Mary O’Sullivan

Paul Bairoch Institute of Economic History, University of Geneva, UniMail, bd du Pont-d'Arve 40, CH-1211 Genève 4. T: +41 22 379 81 92. Fax: +41 22 379 81 93

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Mary O'Sullivan
University of Geneva

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Abstract

In spring 2020, in the face of the covid-19 pandemic, central bankers in rich countries made unprecedented liquidity injections to stave off an economic crisis. Such radical action by central banks gained legitimacy during the 2008-2009 global financial crisis and enjoys strong support from prominent economists and economic historians. Their certainty reflects a remarkable agreement on a specific interpretation of the Great Depression of the 1930s in the United States, an interpretation developed by Milton Friedman and Anna Schwartz in *A Monetary History of the United States* (1963). In this article, I explore the origins, the influence and the limits of *A Monetary History*'s interpretation for the insights it offers on the relationship between theory and history in the study of economic life. I show how historical research has been mobilised to show the value of heretical ideas in order to challenge economic orthodoxies. Friedman and Schwartz understood the heretical potential of historical research and exploited it in *A Monetary History* to question dominant interpretations of the Great Depression in their time. Now that their interpretation has become our orthodoxy, I show how we can develop the fertile link between history and heresy to better understand our economic past.

JEL Codes: N0, N1, N2, B3, B4, B5

On March 26th, 2020, in the face of the covid-19 pandemic, the governor of the US Federal Reserve System, Jerome Powell, made an extraordinary declaration. "We're not going to run out of ammunition", he told Americans, signalling that the central bank stood ready to take any action necessary to stem the mounting economic crisis. Only three months later, the Fed had injected nearly $3 trillion dollars of liquidity into the US economy, with the aggregate assets of the Group of Ten (G10) central banks increasing by about $6 trillion over the same period. Such radical action by central banks -- quantitative easing (QE) as it is known -- has its critics on the right and the left of the political spectrum.[1] Just as striking, however, is that many prominent economists and economic historians have rallied in support of central banks' bold actions to cast quantitative easing (QE) as a *sine qua non* in responding to economic crisis, to any crisis, even one as different from past crises as the virus crisis seems to be.[2] Their remarkable certainty offers a string we can pull to unravel a story about how our understanding of present crises came to be dominated by lessons drawn from past crises.

That story is above all about one historical crisis, the Great Depression of the 1930s in the United States, and its interpretation by Milton Friedman and Anna Schwartz in a book they published in 1963. In 2002, Ben Bernanke offered a much-cited tribute to *A Monetary History of the United States* : "I would like to say to Milton and Anna: regarding the Great Depression. You're right, we did it. We're very sorry. But thanks to you, we won't do it again."[3] Bernanke was a member of the Board of Governors of the Federal Reserve System at the time, as well as an academic economist with an international reputation for his research on the Great Depression. His statement alluded to the widespread consensus around the claim advanced by Friedman and Schwartz that the Federal Reserve System was responsible for turning an ordinary economic downturn into the Great Depression of the 1930s. They argued that when a massive financial crisis led to a sharp decline in the stock of money in the US economy, the Federal Reserve failed to take action to mitigate the problem.[4]

By the end of the 20th century, their interpretation had become sufficiently dominant in economics and economic history to qualify as the orthodoxy of the Great Depression in the United States. In the early 21st century, this economic orthodoxy of a most unorthodox history leaped from academic minds into the policy sphere. When the global financial crisis struck in 2008, Bernanke was Chairman of the Federal Reserve System and he showed his determination not to "do it again" by proposing aggressive policies of monetary expansion. In the process a monetary policy that had been deemed unconventional and somewhat disreputable when employed by the Japanese central bank became the "new normal" for rich countries' monetary authorities.

The flood of liquidity into capitalism's financial system was remarkable in historical perspective, surpassing all previous records for monetary interventions, outside of wartime, since the beginning of the 20th century.[5] It defines our economic reality to such an extent that the story of

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[1] On the right, there are complaints that central banks are exceeding their narrowly defined mandate to control inflation with such radical monetary interventionism; for the left, it is the lack of democratic process in making liquidity injections, their bias towards the rich, and their stimulus to financial bubbles that has drawn criticism.

[2] Barry Eichengreen's admonishing of critics of quantitative easing a few months before the pandemic is illustrative of this attitude: "QE's opponents should consider the alternative. Absent this support from advanced-country central banks following the global financial crisis, a debilitating deflation *might have* set in, and the post-crisis recession *would have* been more severe.", "Critics of quantitative easing should consider the alternative", *The Guardian*, Tue 11 Jun 2019.

[3] "On Milton Friedman's Ninetieth Birthday", Remarks by Governor Ben S. Bernanke at the Conference to Honor Milton Friedman, University of Chicago, Chicago, Illinois, November 8, 2002, https://www.federalreserve.gov/BOARDDOCS/SPEECHES/2002/20021108/

[4] Milton Friedman and Anna Schwartz, *A Monetary History of the United States, 1867-1960*, (Cambridge, MA, 1963).

[5] Thomas Piketty, *Capital and Ideology*, Harvard University Press, 2020, Figure 13.14, 701.

a mysterious professor, who meticulously planned a raid on the Royal Mint of Spain to print billions of euros, became the basis for a wildly popular television series. The Professor explained that: "In 2011, the European Central Bank made €171bn out of nowhere. Just like we're doing. Only bigger….'Liquidity injections,' they called it. I'm making a liquidity injection, but not for the banks. I'm making it here, in the real economy."[6] And the Professor made these remarks long before central banks responded to the coronavirus crisis with an even greater flood of liquidity.[7]

What we have here is a story of how a specific interpretation of a crisis that occurred nearly a century ago became an economic orthodoxy of the Great Depression, before being mobilised to justify policies for dealing with crises today. We can reflect on this story from many perspectives but here I will explore it for the insights it offers on the relationship between theory and history in the study of economic life. I will emphasise how historical research can be mobilised to show the value of heretical ideas in order to challenge economic orthodoxies. We will see that Friedman and Schwartz understood the heretical potential of historical research and exploited it in *A Monetary History of the United States* to question dominant interpretations of the Great Depression in their time. Now that their interpretation has become our orthodoxy, I suggest that we explore the fertile link between history and heresy to better understand our economic past.

To make my case, I will focus on changing interpretations of economic cycles and crises, with particular attention to the Great Depression of the 1930s. My story dwells on the ideas of a few men and one woman. It focuses on a nanosecond in time, a slice of the 20th century. And my main concern will be that microcosm of our world that we call the United States. Whether we like it or not, debates about the economics of crises and cycles in the United States have had implications far beyond that country's borders. And that is not entirely surprising since the US lays claim to a system of capitalism that is distinctive for its history of affluence and instability.

1. The Historical Origins of Heretical Narratives

We have already seen the enormous influence of Americans' ideas about America in the reliance by a global policy elite on *A Monetary History of the United States*. As the book's original cover suggests, it was a contribution to the NBER's ambitious research programme on business cycles, a programme initiated in the early 1920s by Wesley Clair Mitchell and guided by him for decades. So I begin with Mitchell's research programme on business cycles, and its distinctive integration of theory and history, before dwelling on its successes and setbacks at the NBER. By understanding the intellectual context from which *A Monetary History* emerged, we can appreciate the continuities and ruptures in the way it brought theory and history together in its analysis of the Great Depression.

1.1 Heresies of Mitchell's Business Cycles

Wesley Clair Mitchell did more than any other economist to shape the study of business cycles in the United States from the publication of *Business Cycles* in 1913 until his death in 1948 as he struggled to complete a new synthesis.[8] But Mitchell's influence went far beyond his own country; as the *Encyclopaedia Britannica* notes, he was: "the world's foremost authority of his day on business cycles".[9] Mitchell began his career as a monetary economist at the University of

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[6] The Spanish television series, La Case de Papel (literally "The House of Paper" but called "Money Heist" in English), was created by Álex Pina and aired on Spanish television in 2017 before being acquired and streamed around the world by Netflix.

[7] The Federal Reserve System, for example, pumped 2.5 times more money into the US financial system in 2020 than it did in 2008 and its coronavirus QE programme, like those of other central banks, continued into 2021.

[8] Wesley Clair Mitchell, 1913, *Business Cycles*, University of California Press.

[9] https://www.britannica.com/biography/Wesley-C-Mitchell

Chicago's recently established Department of Economics.[10] There he met Thorstein Veblen and was inspired by the unconventional economist's criticisms of orthodox economic theory and his efforts to construct an alternative to it.[11]

As Milton Friedman later explained: "One of Veblen's chief criticisms of 'orthodox' economics was that it was not an 'evolutionary science', that it did not deal with the problem of 'cumulative change' ", and that "problem" was to become one of Mitchell's preoccupations in building his theory of business cycles.[12] Still, Mitchell developed his own version of economic heterodoxy that was distinctive for its commitment to systematic empirical, and more specifically, historical research. Some of this research was qualitative but even Mitchell's prolific statistical work was imbued with a temporal quality: in a letter to fellow economist, Irving Fisher, he asked: "When you speak of periods of equilibrium, are you not referring again to imaginary conditions instead of the historical conditions which our statistics reflect?"[13]

But Mitchell can be deemed heretical not only because he undertook systematic empirical research at a time when it was the exception in economics but also given the purpose to which he applied it.[14] Mitchell suggested that the growing accumulation of empirical evidence would lead to "the obsolescence of the older type of reasoning in economics". "If my forecast is valid", he wrote, "our whole apparatus of reasoning on the basis of utilities and disutilities, or motives, or choices, in the individual economy, will drop out of sight in the work of the quantitative analysts, going the way of the static state." He believed that the accumulation of empirical evidence could show that the theory generated while economics was still a qualitative science was unhelpful for understanding the flow of economic life.[15]

Mitchell did not expect "the rapid crystallization of a new system of economic theory built by quantitative analysis" nor could he predict its content. However, he was sure that one problem, which "has been sadly slurred over" by qualitative economics, would receive more attention: "the relation between business and industry, between making money and making goods, between the pecuniary and the technological phases of economic life".[16] That relation had animated other economic heretics, from Sismondi to Marx to Veblen, and it was to animate Wesley Clair Mitchell in his research on business cycles.[17]

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[10] The chair of the department, J. Laurence Laughlin, was a specialist of money and banking and he supervised Mitchell's dissertation, entitled *A History of the Greenbacks*. Laughlin was an advocate of the liberalism espoused by classical economists but he encouraged his students to chart their own intellectual course. Mitchell took advantage of that freedom early on, breaking new ground compared to his adviser in broadening the study of monetary dynamics to include their impact on wages (see, notably, Mitchell, *Gold Prices and Wages Under the Greenback Standard*, The University Press, 1908).

[11] Veblen was a guiding light for the distinctive US school of institutional economics, with which Wesley Clair Mitchell was associated; indeed, Veblen, Mitchell and John Commons can be seen as the "founding triumvirate" of this school (Geoffrey M. Hodgson, *The Evolution of Institutional Economics*, London, Routledge, 2004; Malcolm Rutherford, *The Institutionalist Movement in American Economics, 1918-1947: Science and Social Control*, Cambridge University Press, 2011; for the term 'founding triumvirate' see Jeff Biddle, "The Sources and Extent of Wesley Mitchell's Reputation: An Application of Citation Analysis to the Journal Literature of the Early Twentieth Century." *History of Political Economy*, Summer 1996.

[12] Friedman, "Wesley C. Mitchell as an Economic Theorist", *Journal of Political Economy*, 58(6), Dec., 1950, 465-493.

[13] Wesley Clair Mitchell to Irving Fisher, November 13, 1926, Papers of Wesley Clair Mitchell. Columbia Rare Book and Manuscript Library.

[14] For the importance of Mitchell's quantitative research, see Mary Morgan, *The History of Econometrics*, Cambridge University Press, 1990, 44-56.

[15] Wesley C. Mitchell, "Quantitative Analysis in Economic Theory", *American Economic Review*, 15(1), (Mar., 1925), 5.

[16] Ibid., 7.

[17] A mild-mannered, modest and generous man, Wesley Clair Mitchell is not as instinctively classified as a heretic as more colourful characters like Marx and Veblen. Moreover, his prominence in interwar US economics,

Mitchell's used "the money economy" to evoke a distinctive institutional form of economic activity, one whose "essential feature" was "not the use of money as a medium of exchange" but rather "the fact that economic activity takes the form of making and spending money incomes".[18] Mitchell emphasized that the pecuniary logic that dominated the functioning of a money economy might well conflict with the material requirements of man's well-being. Indeed, it was due to the "precarious dependence" of material well-being on an economy organized for profit seeking that business cycles occurred: "Where money economy dominates, natural resources are not developed, mechanical equipment is not provided, industrial skill is not exercised, unless conditions are such as to promise a money profit to those who direct production."[19]

For Mitchell, therefore, business cycles "make their appearance at that stage of economic history when the process of making and distributing goods is organized chiefly in the form of business enterprises conducted for profit".[20] And he located the root of cycles and crises in the dynamics of these enterprises' profit-making and, specifically, in "the factors which control present and prospective profits, together with present and prospective ability to meet financial obligations."[21] Expressed in these terms, Mitchell's theory of business cycles has obvious points of contact with Marx's rate of profit as a determinant of cycles and crises. Marx is usually seen as emphasising characteristics of the production process as the primary determinants of the profit rate.[22] Mitchell looked to the "system of prices" since "the margins between different prices within the system hold out that hope of pecuniary profit, which is the motive power that drives our business world."[23]

Mitchell's analytical perspective on the dynamics of business cycles fostered a distinctive methodological approach, based on an integration of theory and history, that he put into practice

during a period that has been characterised as "pluralist", raises questions about the significance of terms like "orthodox" and "heterodox" at the time. Mary Morgan and Malcolm Rutherford suggested: "it is especially difficult to define 'orthodox' or 'neoclassical' economics in the interwar context and to provide a grouping of individuals under these labels" (Mary Morgan and Malcolm Rutherford, *From Interwar Pluralism to Postwar Neoclassicism*, Duke University Press, 1998, 3). However, an orthodox-heterodox opposition inspired Veblen and played a significant role for Mitchell in his own work. Moreover, as J. Daniel Hammond showed, Mitchell's work on business cycles was read as heterodox, quoting Paul Homan as noting that "the introductory chapters of Business Cycles cannot be intelligently read without perceiving that they rest upon a groundwork of ideas incompatible with any of the variant statements of orthodox theory" (J. Daniel Hammond, p. 12). In a recent study of Mitchell's ideas and influence, Jeff Biddle suggested there were two Mitchells, a heterodox thinker who was a member of the "founding triumvirate" of US institutionalism, along with Veblen and John Commons, and a more "neutral" Mitchell, known for his work on business cycles and economic statistics. As is evident from my discussion of Mitchell's research on business cycles, that distinction is difficult to sustain given how much his business-cycle research was imbued with, and an expression of, his heretical ideas about theory and history.

[18] Ibid., 21. Later in his life, Mitchell would refer to 'capitalism' as a synonym for this concept but I will stick with his original formulation here.

[19] Mitchell, 1913, 21-22.

[20] Ibid., 585-6.

[21] Ibid., 26.

[22] In his analysis of the generation of surplus value, Marx emphasised characteristics of the production process and, specifically, the intensification of work and the role of technological change. However, it is important not to draw too sharp a contrast with Mitchell for two reasons. First, no student of Veblen could ignore technology and Mitchell was well aware of its importance but insisted the implications of technology on profits could be traced through its impact on costs or revenues or both. Second, Marx was attentive to the significance of prices for the realisation of profits and seems to have been at the heart of Marx's sketch of a possible resolution of the infamous "transformation" problem in his writing on capital.

[23] Mitchell described the system of prices as "a social mechanism" that he saw as necessary to "the elaborate exchanges, and the consequent specialization, which characterize the modern world". It was a "highly complex system of many parts connected with each other in diverse ways, a system infinitely flexible in detail yet stable in the essential balance of its interrelations, a system like a living organism in its ability to recover from the serious disorders into which it periodically falls" (Mitchell, 1913, 31).

in his 1913 book. Insofar as a theory of business cycles was concerned, Mitchell emphasised that "[t]he deepest-seated difficulty in the way of framing such a theory arises from the fact that while business cycles recur decade after decade each new cycle presents points of novelty".[24] Since "the recurrent phases presented by economic activity, wherever it is dominated by the quest of profits, grow out of and grow into each other", he insisted that an analysis of business cycles had to take account "of cumulative changes by which one set of business conditions transforms itself into another set".[25] Inherent in the process of cumulative change was the fact that "[e]very business cycle, strictly speaking, is a unique series of events" growing out of a "preceding series of events, likewise unique". For all the complexity of business cycles, however, Mitchell insisted that theory "need not be given up in despair" if it took as its focus the temporal logic of business cycles, the sequences among business phenomena", "a few which are substantially uniform."[26]. Crucial in this regard were systemic changes in price relations that he suggested were found in every period of revival, prosperity, crisis or depression.

To show that, Mitchell needed an empirical methodology to identify the different phases of cycles in the past that he could use as the basis for his study of the cyclical relations between prices and profits. There was no NBER to help him, of course, and historians offered little help since they were only slowly turning to economic phenomena.[27] So Mitchell relied on the contemporary business and financial press to construct a qualitative account of the historical rhythm of business cycles in different countries that he referred to as his "annals".[28] Then, using statistics, he analysed the changing relations among various types of prices during different phases of the cycle. His task was enormous since it meant, for example, collecting extensive data on wages, the prices of raw materials and processed inputs, little of which were available from official statistics.[29] Linking changes in the system of prices to the dynamics of profits was a still greater problem since, as Mitchell explained, "[s]tatistics both trustworthy and significant concerning profits are scarce".[30] But he was not easily daunted and embraced these difficulties, pioneering on many fronts in the compilation, presentation and analysis of economic data.[31]

And after all of this empirical effort, Mitchell kept his eye fixed on the theoretical questions with which he began. He returned in the final third of his book to spend 150 pages presenting what he called an "analytic description of the complicated processes by which seasons of business prosperity, crisis, depression, and revival come about in the modern world". There he laid out what happened to prices and profits in different phases of the business cycle. His insights in this regard have been seen, for example, as a precocious antecedent of the "profit-squeeze" theories of recessions that proliferated from the late 1960s.[32]

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[24] Ibid., 449
[25] Ibid., 449
[26] Ibid. 450
[27] Francesco Boldizzoni & Pat Hudson, *Routledge Handbook of Global Economic History*, Oxford and New York, 2016.
[28] To make his task feasible, Mitchell's annals covered cyclical developments in four countries – America, England [sic], France, and Germany – for the period from 1890 to 1911 (Mitchell, 1913, chapter III, 44-87).
[29] For the history of statistical thinking and collection, see Alain Desrosières, La politique des grands nombres: Histoire de la raison statistique, La Découverte, 2010; Theodore Porter, *The Rise of Statistical Thinking, 1820-1900,* Princeton University Press, 1986; Adam Tooze, *Statistics and the German State, 1900-1945: The Making of Modern Economic Knowledge*, Cambridge University Press, 2001.
[30] Mitchell, 1913, 422.
[31] Jeff Biddle, "The Sources and Extent of Wesley Mitchell's Reputation: An Application of Citation Analysis to the Journal Literature of the Early Twentieth Century." *History of Political Economy*, Summer 1996.
[32] Howard Sherman, "Profit-Squeeze (or Nutcracker) Theory of the Cycle: A Production-Realisation Hypothesis", chapter 13 in *The Business Cycle : Growth and Crisis under Capitalism*, Princeton University Press, 1991, 248-252.

1.2 Successes & Setbacks of the NBER Programme

Mitchell's approach to business cycles created links with another important debate that was gaining a great deal of attention at the time in the United States. It was a "Piketty moment" when searching questions were raised about the distribution of the fruits of economic activity among different classes in society. And it encouraged Mitchell to play an active role in establishing a non-partisan centre for economic research, the National Bureau of Economic Research (NBER), in 1920. The first subject to be investigated was the size and distribution of the national income of the United States. In February 1922, in his Annual Report as NBER Director of Research, Mitchell reported that the bureau's inaugural study was complete and announced the Executive Committee's choice for the next subject of investigation: business cycles.[33]

With the United States just emerging from the post-war depression of 1920-1921, the choice seemed a self-evident one for the fledgling NBER. For Mitchell himself, the launch of a major programme of empirical research on business cycles made sense since he considered his 1913 book to be out of date. A new book had to be written but Mitchell believed the task was too great for him to undertake alone: "[f]rom this quandary", as he put it, "I was rescued by the National Bureau of Economic Research".[34]

The NBER's programme of research was built on the same three pillars on which Mitchell's 1913 book had been constructed. First, Willard Thorp used contemporary press reports to generate historical annals of business conditions extending back to 1790 for a variety of countries.[35] Second, a massive statistical programme was undertaken to compile hundreds of price series, unprecedented data on profits, as well as the earliest estimates of national income for the United States, drawing in economists such as Ralph Epstein, Solomon Fabricant, Simon Kuznets, Frederick Mills, and many more. It was to continue over decades, broadening as it evolved to include all aspects of the US economy. The programme's extension to include monetary factors explains why the NBER commissioned the study that Friedman and Schwartz undertook from the late 1940s.[36]

In devising methods for interpreting and combining these data, the NBER programme offered inspiration for the reference points we use until this day in identifying business cycles in the US and elsewhere.[37] But the main purpose of the programme, throughout Mitchell's association with the NBER, was to feed into a theoretical analysis of business cycles. Integrating the vast outpouring of empirical evidence in a business cycle theory was always understood to be Mitchell's task and it was his "chief concern from 1923".[38] However, when he published a new book on business cycles in 1927, he explained that: "[d]espite the National Bureau's efficient aid, my resurvey of the field is taking more time than the first survey took."[39]

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[33] Annual Report of the Director of Research: Wesley C. Mitchell, February 1922, National Bureau of Economic Research, New York, 4.

[34] Mitchell, *Business Cycles: The Problem and Its Setting*, National Bureau of Economic Research, New York, ix.

[35] Willard Long Thorp, *Business Annals: United States, England, France, Germany, Austria, Russia, Sweden, Netherlands, Italy, Argentina, Brazil, Canada, South Africa, Australia, India, Japan, China*, National Bureau of Economic Research, New York, 1926.

[36] Given Mitchell's expertise in monetary economics, it is no surprise that his 1913 book contained an extensive discussion of, and considerable data on, monetary factors in the business cycle. When Friedman embarked on the NBER monetary study, Hammond noted: "[h]e stated that he and Schwartz were picking up where Mitchell left off in Chapter VI of his 1913 *Business Cycles*". (Hammond, 63)

[37] Biddle, 163-4.

[38] Mitchell, 1927, pn.

[39] Ibid., pn.

Understanding the theoretical implications of new evidence was a major challenge as it flowed out of the NBER, other US research centres, and the business cycle institutes mushrooming around the world.[40] The challenge attracted increasing numbers of creative scholars besides Wesley Clair Mitchell including Nikolai Kondratiev, Friedrich von Hayek and Michal Kalecki in what is sometimes described as a golden age of theoretical and empirical research on business cycles. A "golden age" it may have been but it was a confusing time too since there was so little theoretical agreement when it came to cycles and crises. Most students of the business cycle sought explanations for recurrent fluctuations in economic activity in the internal dynamics of the economic system but there was no consensus among them about the causal process involved.

And there was worse to come. That the onset of Depression would have a dramatic impact on business cycle theory hardly needs to be said. The depth and persistence of the crisis, especially in the country that seemed to embody capitalism in its most sophisticated form, gave the economics of cycles and crises a prominence that no leading economist could ignore. It became harder to sustain the longstanding belief, still expressed by prominent economists, that cycles were temporary aberrations that would be corrected by the normal functioning of a market economy.[41]

In giving further impetus to efforts to understand business cycles, the Depression reinforced existing programmes, such as Mitchell's research at the NBER, as well as Joseph Schumpeter's work, on the cumulative dynamics of capitalism.[42] But it also fostered new ideas about cycles, such as the growing interest in large enterprises and their implications for business cycles. Still, it was the novel perspective proposed by John Maynard Keynes that stimulated particular interest in the 1930s. Classifying Keynes' ideas is a task that is fraught with difficulty but the broad sweep of *The General Theory of Employment, Interest, and Money*, the reason that Keynes presented it as a general theory, was for his ideas about the economic importance of uncertainty and, as a consequence, sentiment or animal spirits, in driving economic behaviour.

These conceptual fault lines in the economic analysis of cycles and crises made it difficult to predict how research would develop after the Depression. And methodological fault lines further complicated that task. Here again the new was mixed with the old. Keynes can be seen as maintaining the tradition of deductive reasoning in economics[43] and there was continuity too in the integration of theory and history by Mitchell as well as Schumpeter.[44] But the Depression gave a major boost to a new entrant to the field of methodological possibilities as econometric approaches gained ground in the study of business cycles.

As Patricia Clavin explained, the Depression induced the League of Nations to assume a prominent role in promoting research on business cycles.[45] And it became a sponsor of

[40] Morgan, 1990, 64-68; Quinn Slobodian, *Globalists: The End of Empire and the Birth of Neoliberalism*, Harvard University Press, 2018, 55-73.
[41] See, for example, Irving Fisher, Fisher, Irving, "A Debt-Deflation Theory of the Great Depression", *Econometrica*, October 1933, 1, 337-57.
[42] Thomas K. McCraw, "Schumpeter's "Business Cycles" as Business History", *Business History Review*, 80(2), (Summer, 2006), pp. 231-261.
[43] It should be emphasised that statistics and history were not absent from this tradition or, for that matter, in Keynes's work but they were used, as Schumpeter put it, "for purposes of illustration and verification" (Schumpeter, 1946).
[44] For a discussion of Schumpeter's approach, see Lazonick, 1994 ; Thomas McCraw, *Prophet of Innovation : Joseph Schumpeter and Creative Destruction*, Harvard University Press, 2007.
[45] Clavin explained that the Depression created "rising expectations" that the League should do something as well as awareness that the crisis was 'not suitable for the advance of the liberal economic policy with which the League had hitherto been associated'. As a result, "[t]he business cycle marked the pithead of what became a

econometric research to determine the explanatory power of the various cycle theories that had proliferated. The Dutchman, Jan Tinbergen, played a pioneering role in this regard, constructing a 42-equation model for the U.S. economy, which was endorsed by the League.[46] During the war, the centre of gravity for this pioneering econometric work shifted from Europe to the United States, especially to the Cowles Commission, under the intellectual leadership of European economists like Jacob Marschak and Tinbergen's protégé, Tjalling Koopmans.[47]

Notwithstanding the rapid development of econometrics, there was enormous controversy at the time about its potential value in economics. Keynes was famously and profoundly sceptical of its potential, dismissing Tinbergen's work for the League of Nations as "a piece of historical curve-fitting and description".[48] Keynes emphasised that econometric models were useful only if we already had a correct and complete understanding of causal economic relationships, if the causal factors were measurable, if the relationship among them could be specified in simple mathematical terms, and if conditions observed in the past would persist into the future. Keynes' animosity was long dismissed as outdated and cantankerous but the substance of his critique has attracted renewed interest in recent years.[49]

The econometricians at the Cowles Commission had another critic, soon to be much closer to home when they moved to the University of Chicago. In a review of Tinbergen's research on the United States, Milton Friedman objected to the claim that econometric models could serve as the basis for "an empirically tested explanation of business cycle movements". He pointed out that the structural features of Tinbergen's model of the US economy – both the variables and the relations among them – were chosen because they fit well with the economic data at his disposal. But the goodness of their fit created "no presumption that the relationships they describe will hold in the future". Friedman cited Mitchell both on this point as well as the conclusion to be drawn from it: such models represented, rather than explained, statistical history and their explanatory power could be judged only when enough time had passed to generate new data to test them.[50]

The data that Tinbergen had at his disposal covered the period from 1919 to 1932 and owed much to Mitchell and the NBER. But even more than that, the Dutchman had been so struck by the patterns he observed in corporate profits that, as Friedman pointed out, he made it one of the "strategic variables" in his analysis of business cycles. For that reason, Friedman carried out a "modest experiment" that compared estimates using the profit equation in Tinbergen's model and actual profits for the period from 1932 to 1937 and concluded that the "[t]he degree of agreement is not impressive". Thus, if there was value in econometric approaches, Friedman suggested, it was: "for deriving tentative hypotheses about the nature of cyclical behaviour" and, as such, much more modest than its advocates claimed.[51] Friedman was working as a researcher

rich seam of intellectual engagement by the League as to the cause of and possible remedies for economic depression over the next fourteen years" (Patricia Clavin, *Securing the World Economy*, 2015, 73).

[46] Morgan, 1990, 101-132.

[47] There is an ample literature on the Cowles Commission written by economists who worked there and historians of economic thought. For a recent contribution of direct relevance to what follows, see Boumans, Marcel, "Friedman and the Cowles Commission", in Robert A. Cord and J. Daniel Hammond, eds., *Milton Friedman: Contributions to Economics and Public Policy*, Oxford, UK: Oxford University Press, 2016.

[48] Keynes, J.M. (1939). 'Professor Tinbergen's Method', *Economic Journal*, 49, pp. 558-568.

[49] For earlier interest, see Patinkin, 1976 and Hendry, 1980; more recent discussion include Leeson, R., 1998. "The Ghost I Called I Can't Get Rid of Now: The Keynes-Tinbergen- Friedman-Phillips Critique of Keynesian Macroeconometrics", *History of Political Economy* 30:1, 51-94; Garrone, Marchionnati & Bellofiore, "Keynes on econometric method : A reassessment of his debate with Tinbergen and other econometricians, 1938-1943", 2004.

[50] Milton Friedman, Review of *Business Cycles in the United States of America, 1919-1932* by J. Tinbergen, *American Economic Review*, 30(3), Sep., 1940, 657-660.

[51] ibid., cited at 659.

at the NBER when he wrote the review and NBER researchers on the business cycle programme took much the same line, expressing caution about overstating the scientific achievements of the econometric analyses being developed at the Cowles Commission.[52]

Given these methodological fault lines, a clash of titans was in the offing. The demands of wartime planning and then post-war economic management gave an impetus to systematic empirical research. That might seem to rule out Keynes, given his methodological stance, but his insights about the malfunctioning of the economic system, and the remedies for addressing it, seemed too attractive to give up. As a result, some Keynesian notions were incorporated in the new macro-models built on structural econometric equations but, as Keynes's own critique had highlighted, these models could incorporate only causal factors that were measurable. And since uncertainty and sentiment resisted quantification, they were abandoned by the wayside, prompting Joan Robinson's evocation of a "bastard Keynesianism".[53]

The main rival for structural econometrics as an empirical research programme for studying business cycles in the post-war years was the integration of theory and history that Mitchell and the NBER exemplified. And so the clash of the titans came down to a contest between these rival programmes in a series of increasingly vigorous exchanges from the late 1930s to the late 1940s. Matters came to a head at a NBER conference on business cycles in 1949 where the historical approach confronted the structural econometric approach.[54] There is no doubt about what happened in the wake of this conference, with victory for the structural econometric programme and defeat for Mitchell's alternative, but the question of why that happened does not lend itself to easy answers.

Since Mitchell died in 1948, and could not open the conference as planned, one might be tempted to appeal to circumstance. But the story is often told as if the decline in Mitchell's influence in studies of the business cycle was deserved. Following Koopmans' lacerating critique, "Measurement without Theory" in 1947, Mitchell and the NBER are characterised as offering history -- statistical history it is true -- but no theory, and economic measurement that was distinctive for, as Koopmans put it, "the pedestrian character of the statistical devices" it employed.[55] From this perspective, the structural econometrics programme dominated because it deserved to dominate, given its greater theoretical and empirical sophistication.

There are several reasons to challenge this interpretation.[56] We only need to look carefully at the two research programmes involved in the clash to see that Koopmans' characterisation suffered from obvious weaknesses. The claim that Mitchell was a-theoretical is hard to sustain, as Friedman soon pointed out, and it is a stretch to classify the simple economic relationships embodied in econometric models as some higher form of theory. But it is even more provocative to look forward to consider what happened after the clash of the titans was supposedly won by the proponents of structural econometrics against the advocates of a historical approach. For if there is one book that creates a problem for the usual explanations of who won and why they triumphed in the battle of ideas about crises and cycles, it is *A Monetary History of the United States*.

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[52] Hammond, 1996, 39-43; 18-23; Rutherford, 2011, 280ff

[53] Robinson, J. "What has become of the Keynesian Revolution?" In Robinson, J., editor, *After Keynes*, Basil Blackwell, Oxford, 1973.

[54] NBER, *Conference on Business Cycles*, NBER, New York, 1951.

[55] Tjalling C. Koopmans, "Measurement Without Theory", *Review of Economics and Statistics*, 29, No. 3 (Aug., 1947), pp. 161-172.

[56] Hammond, 1996, chapter 1, 5-25; Howard Sherman, "The Business Cycle Theory of Wesley Clair Mitchell," *Journal of Economic Issues*, Vol. 35, No. 1 (Mar., 2001), 85-97; Philip Mirowski, "Cowles Changes Allegiance: From Empiricism to Cognition as Intuitive Statistics", *Journal of the History of Economic Thought*, 2002, vol. 24, no 2, p. 174. Camila Orozco Espinel, "L'économie, une discipline en quête d'autorité scientifique (États-Unis, 1932-1957)", thèse de doctorat, Ecole des Hautes Etudes en Sciences Sociales, 2018, ch. 4, 131-166.

1.3 An Analytic Narrative of the "Great Contraction"

In his book on theory and measurement in Milton Friedman's economics, J. Daniel Hammond offered a rich analysis of the inspiration he drew from Wesley Clair Mitchell's work on business cycles. In "On the Origins of 'A Monetary History' ", Hugh Rockoff built on Hammond's work to suggest: "that the most important influence may have been Wesley Clair Mitchell and his classic book "Business Cycles" (1913)". Rockoff located that influence, above all, in *A Monetary History*'s emphasis on compiling long time series of monthly data and analysing the effects of specific variables on the business cycle. In fact, Mitchell's methodological influence on *A Monetary History* went much deeper since the book was based on a combination of statistics, historical narrative and theory that bears an uncanny resemblance to the work of Mitchell. However, the distance that Friedman and Schwartz marked with respect to Mitchell's work is just as significant and much greater than Rockoff allows.[57] As we shall see, Friedman and Schwartz employed their historical research to heretical effect to challenge not only what Mitchell believed, but what many US economists believed, about the causes of cycles.

That Mitchell's work would serve as an important inspiration for *A Monetary History* is not surprising given that Friedman had made it clear where his sympathies lay in the clash of the titans.[58] And in a lengthy eulogy to Mitchell in 1950, he offered a sympathetic and insightful review of the late economist's collected works, and an explicit refutation of Koopmans' claim that Mitchell's work on business cycles was a-theoretical. Indeed, Friedman went so far as to include an extensive appendix to show that the theoretical ideas in Mitchell's 1913 book on *Business Cycles* could be presented in a set of structural econometric equations. He concluded on a forceful note that: "Mitchell's striving for theoretical explanations of the phenomena he studied was an essential element in his scientific work". It led him to formulate a specific business-cycle theory, Friedman observed, and "focused his empirical work on meaningful problems, made it analytic as well as descriptive, and prevented him from engaging in empiricism for its own sake". If some economists had not understood the theoretical significance of Mitchell's work, Friedman speculated that it was because "[h]is theoretical work is throughout interwoven with his empirical work and made a part of an "analytic description" of the phenomena under study".[59]

Mitchell himself had been less generous in his assessment of Friedman and had urged Arthur Burns, his successor as NBER research director, to be cautious in dealing with him: "The kind of watching M. needs is not critical examination of his statistical methods + general reasoning, but detailed study of his data + the way he uses them. That is a time consuming job."[60] But Burns knew Friedman well, and had a higher opinion of him than Mitchell, and he commissioned him to work with Anna Schwartz on a monograph on money and the business cycle.

When Friedman and Schwartz began work on their project, they expected to produce a statistical study of monetary factors in the US business cycle. What emerged fifteen years later as

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[57] Rockoff focussed on the similarities and differences in the analysis of money between Mitchell's *Business Cycles* and *A Monetary History*.

[58] Thomas A. Stapleford, "Positive Economics for Democratic Policy: Milton Friedman, Institutionalism, and the Science of History", in Robert Van Horn, Philip Mirowski, Thomas A. Stapleford, eds., *Building Chicago Economics : New perspectives on the history of America's most powerful economic program*, Cambridge University Press, 2011; Hugh Rockoff, "On the Origins of 'A Monetary History' ", NBER Working Paper 12666, November 2006.

[59] Friedman, "Wesley C. Mitchell as an Economic Theorist", *Journal of Political Economy*, 58(6), Dec., 1950, 465-493. Friedman acknowledged that his mathematical model was not intended to "as a version of the theory that Mitchell would have accepted as his own" (490). It does seem unlikely that Mitchell would have seen the model as a faithful representation of his key arguments given Friedman's treatment of profits and time.

[60] Wesley Clair Mitchell to Arthur Burns, August 27, 1945. The letter is worth reading in its entirety and is available, thanks to Irwin Collier, at http://www.irwincollier.com/nber-mitchell-to-burns-about-friedman-1945/.

A Monetary History was an "analytical narrative" of post-Civil War monetary developments in the United States and as the authors explained: "[o]ur foray into analytical narrative has significantly affected our statistical analysis".[61] The book comprises 11 chronological chapters, which take the reader from the Civil War to 1960. By far the most important of these chapters is "The Great Contraction, 1929-1933", which at 121 pages was more than twice as long as most of the others. It was this chapter, more than any other interpretation of the Great Depression, which was to transform our understanding of the greatest crisis that capitalism has ever experienced. It offered an analytical narrative of the Great Depression, a narrative that was heretical both in the interpretation it offered and in the methodology it used to build it. And these qualities of *A Monetary History* had a great deal to do with the way the book both built on and challenged the economics of Wesley Clair Mitchell.

A first observation to be made about similarities to Mitchell can be gleaned from Friedman & Schwartz's use of the term "analytical narrative". For economists to acknowledge that their explanations were a type of narrative was uncommon at the time.[62] However, we have seen something very similar in Mitchell's notion of "analytic description" or "descriptive analysis" to refer to the integration of statistics, annals and theory in his explanation of business cycles and Friedman and Schwartz used "analytical narrative" to evoke a similar integration in their work.[63]

A second similarity to Mitchell is striking in *A Monetary History*'s heavy use of basic statistics and elementary methods for their analysis. Friedman and Schwartz relied on extremely simple statistical techniques, on averages and medians and ratios and indices, just the "pedestrian statistical techniques" that supposedly brought down Mitchell and the NBER in the domain of business cycles. There was no attempt to establish any correlations among different statistical series, no mathematical equations to capture structural relationships, indeed no econometrics of any kind.

And there is a third echo of Mitchell since the statistics quickly give way in Friedman and Schwartz to historical annals, where we find the same emphasis on temporality, on the importance of sequences in time. We find an especially long and detailed annal of the Great Depression and to generate it, Friedman and Schwartz relied, as Mitchell did, on published contemporary accounts. Indeed, they go further than Mitchell, using letters, diaries, and other primary sources to reconstruct how prominent actors in their account acted or failed to act in the face of the Great Depression.

So now we have statistics and annals but how did Friedman and Schwartz make sense of what happened? Or, to put it differently, where is the theory that provided the "analytic" to complement the "narrative"? A friendly critic, Robert Lucas, characterised Friedman and Schwartz as writing a descriptive history without theory but that was no more accurate for Friedman and Schwartz than it was for Mitchell.[64] It is true that they sometimes seemed quite

[61] Friedman & Schwartz, 1963 (hereafter F&S), xxi.
[62] Even today, after decades of post-modernism, when economists speak of narratives it is to refer to the stories that other people tell themselves. That is despite important contributions such as the articles and books by Deirdre McCloskey (see, for example, "The Rhetoric of Economics", The Rhetoric of Economics", *Journal of Economic Literature*, 21 (2), (Jun., 1983), 481-517. It is notable, therefore, that Robert Shiller chose "Narrative Economics" as the title of his recent presidential address to the American Economic Association. Although he spends most of his time talking about other people's narratives, Shiller does acknowledge that economists rely on narratives in their work (Robert Shiller, "Narrative Economics", *American Economic Review*, 107(4), (2017): 967-1004.
[63] When a group of social scientists and historians advocated the use of "analytic narrative" in the late 1990s, they seem to have invented the concept anew as a fusion of game theory and history without direct inspiration either from Mitchell or Friedman and Schwartz (Robert H. Bates, Avner Greif, Margaret Levi, Jean-Laurent Rosenthal, *Analytic Narratives*, Princeton University Press, 1998).
[64] Robert E. Lucas, "Milton Friedman as Teacher and Scholar" in Cord & Hammond, 2016.

casual in their causal reasoning but we soon encounter different statements, stronger statements of cause and effect, that show there is theory and make us wonder where it might be found.

To answer that question, to understand how Friedman and Schwartz made their causal argument, we need to look not to the similarities but to the differences between their analysis and the one that Mitchell proposed. Mitchell identified profits as the root of cycles and crises and used historical analysis to understand how profit making was linked to business cycles. Friedman and Schwartz focussed on the relationship between money and income but took a very different analytical stance to Mitchell. They posited that there is a stable long-term relationship between money and income over the long run but it is really in the background of their book. What they were interested in exploring is what happens when the long-run stable relationship breaks down as it does, most spectacularly, in the Great Depression (see Figure 1).

Figure 1 Money Stock, Income, Prices, and Velocity, in Reference Cycle Expansions and Contractions, 1867-1960

Already this tells us something important. Friedman and Schwartz conceived of the norm in capitalism as stability, as characterised by a harmonious covariance of money and income, interrupted only by cycles that are presented as aberrations. These aberrations were the focus of their analysis and it was during these unusual historical moments, they claimed, that money mattered a great deal. Specifically, insofar as the Great Depression was concerned, they argued that it was the drop in money that caused income to fall. This is far from self-evident, as Friedman and Schwartz acknowledged, since causality in such a relationship could just as easily go the other way.[65] And that begs the question of how Friedman and Schwartz went about showing that money mattered so much during crises?

Writing in the early 1960s, many economists would have turned to some kind of econometrics to do that but that is not the approach that Friedman and Schwartz pursued. Instead, they turned to history, noting that:

> A great merit of the examination of a wide range of qualitative evidence, so essential in a monetary history, is that it provides a basis for discriminating between these possible explanations of the observed statistical covariation. We can go beyond the numbers alone and, at least on some occasions, discern the antecedent circumstances whence arose the particular movements that become so anonymous when we feed the statistics into the computer.[66]

Based on historical research, they purported to reconstruct the temporal sequence of events that led to a "catastrophic contraction" during the Great Depression to show how waves of banking crises led to a decline in the stock of money in the US economy, precipitating the diminution of the country's national income. To the extent that they had evidence for their causal interpretation of the Great Depression, therefore, it was historical evidence.

Moreover, they used historical reasoning to go further, to transcend a story that would otherwise locate the collapse of the US economy in the failures of its private financial system. If they did not lay the ultimate blame there, it was because of a counterfactual history that they constructed:

> Throughout the contraction, the [Federal Reserve] System had ample powers to cut short the tragic process of monetary deflation and banking collapse. *Had it used* those powers

[65] F&S, 686..
[66] F&S, 686.

> effectively in late 1930 or even in early or mid-1931, the successive liquidity crises that in retrospect are the distinctive feature of the contraction *could almost certainly have been prevented* and the stock of money kept from declining or, indeed, increased to any desired extent. Such action *would have* eased the severity of the contraction and very likely *would have brought* it to an end at a much earlier date.[67]

Their use of the conditional is highlighted in italics to emphasise that what they were building was the impression of a crisis that did not have to occur. Indeed, Friedman and Schwartz were quite explicit about their exercise in "conjectural history -- the tale of 'what might have been' " and acknowledged "[t]here is no way to repeat the experiment precisely and so to test these conjectures in detail". Still, they claimed that: "all truly analytical history, history that seeks to interpret and not simply record the past, is of this character, which is why history must be continuously rewritten in the light of new evidence as it unfolds".[68]

Now you can write good books based on counterfactuals but imaginative literary devices are usually employed to avoid writing ugly phrases like the ones in the previous quotation. The phrase "would have" actually appeared 455 times in their book. Whatever the literary qualities of that choice, it tells us is that much of Friedman and Schwartz's history was counterfactual history and nowhere was it more important than in their interpretation of the Great Depression. It allowed them to make a bold claim, even a breath-taking one, which is that the greatest crisis that capitalism had ever confronted was allowed to go as deep and to last as long as it did because of the failure of government.

That their interpretation was ideologically loaded was blatantly clear in a book that Milton Friedman had published only a year earlier.[69] In *Capitalism and Freedom*, a veritable hymn to free markets and liberalism, Friedman devoted a whole chapter to the control of money and there was no subtlety about the target or the weapon in his attack:

> The fact is that the Great Depression, like most other periods of severe unemployment, was produced by government mismanagement rather than by any inherent instability of the private economy. A governmentally established agency- the Federal Reserve System- had been assigned responsibility for monetary policy. In 1930 and 1931, it exercised this responsibility so ineptly as to convert what otherwise would have been a moderate contraction into a major catastrophe.[70]

Here we find the interpretation of the Great Depression that Friedman and Schwartz were to roll out in all of its historical glory one year later. And Friedman used it to bolster his claim that "[t]he Great Depression in the United States, far from being a sign of the inherent instability of the private enterprise system, is a testament to how much harm can be done by mistakes on the part of a few men when they wield vast power over the monetary system of a country."[71]

2. An Economic Orthodoxy of an Unorthodox Past

When we turn to how *A Monetary History* became a classic, we should recognise how unlikely that seems when we take stock of what its authors did. They proposed a novel interpretation of the Great Depression that focussed on one variable – money – to explain why it was so deep and long. In doing so, they posited a role for money and a significance for monetary policy that was in stark opposition to the economic orthodoxy of their day. They built that interpretation on

[67] F&S, 11.
[68] F&S, 168.
[69] Milton Friedman, *Capitalism & Freedom*, 1962.
[70] Friedman, 1962, 38.
[71] Friedman, 1962, 50.

pedestrian statistical techniques and historical analysis that had been dismissed as old-fashioned by some of the leading economists of their day. And they were flagrant, at least Friedman was flagrant, in drawing out its ideological significance. But perhaps what makes it most improbable of all is that there were significant gaps in their historical analysis, gaps that made it difficult to be sure that the story they told was a convincing account of the economic dynamics of the Depression in the United States.

2.1 A Provocative Interpretation with Obvious Gaps

In laying out these gaps, I emphasise that these are not obscure holes in their account but obvious limitations of their evidence and arguments with respect to the Great Depression. To locate them, we can strip their interpretation down to its essential elements, as illustrated in Figure 2. Banking failures play a crucial role in their account in precipitating the US monetary mess. But the authors took only three pages to mull over why these failures occurred and, specifically, whether they stemmed "primarily from the financial practices of the preceding years?" or were "produced by the developments of the early thirties?".[72] These three pages offer the most unsatisfying musings in the whole chapter and, according to Hammond, were added only under pressure from inside the NBER.[73] Friedman and Schwartz alternated between the possibility that there was a deterioration in the quality of loans in the twenties -- a hypothesis they clearly did not favour -- and the limits of the evidence to prove or disprove it.[74] In the end, they did not reach a clear conclusion on the causes of banking failures and instead downplayed their importance: "If deterioration of credit quality or bad banking was the trigger, which it may to some extent have been, the damaging bullet it discharged was the inability of the banking system to acquire additional high-powered money to meet the resulting demands of depositors for currency…".[75]

Figure 2 Friedman & Schwartz's explanatory schema

Friedman and Schwartz emphasised that the importance of banking failures as such should not be overstated since it was their indirect, rather than direct, effects that were decisive in bringing about collapse of the US money stock. However, insofar as documenting the contagion of fear that the bank failures supposedly prompted among depositors, and that engulfed healthy and weak banks across the United States, their account is unsatisfying. Homespun wisdoms, like "such contagion knows no geographical limits", alternate with a few specific claims based on limited evidence. Friedman and Schwartz tell us about the Bank of the United States, for example, noting that it was "the largest commercial bank, as measured by volume of deposits, ever to have failed up to that time in U.S. history". But its real significance for the contagion of fear, they suggested, was that: "its name had led many at home and abroad to regard it somehow as official bank". How many depositors, one wonders, but there is no evidence, not even a footnote to evidence, to tell us.[76]

If the dynamics of the banking panic are obscure, its consequences seem clear enough in one of the two crucial graphs in their chapter on the "great contraction" (reproduced below as Figure

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72 F&S, 353.
73 The pressure was exerted by Geoffrey Moore, the associate director of research at the NBER. From the beginning of the monetary project, Moore had been pushing for a thorough exploration of the role played by credit quality in business cycles but, as Hammond explained, Friedman and Schwartz opted to exclude it. However, when the manuscript was completed, Moore objected to its distribution since he believed that the authors were holding the Fed responsible for problems that had to do with a deterioration in credit quality. The pages referred to above seem to have been added or at least expanded just before publication as a response to Moore's criticisms (Hammond, 78-83).
74 F&S, 354-356.
75 F&S, 356.
76 F&S, 308-311.

3). But 'seem' is the appropriate word since the rise in high-powered money at the bottom does not offer any straightforward explanation of the decrease in the money stock at the top. The curves are not drawn to the same scale but I have added numbers to get a sense of the magnitudes involved. Clearly, something is missing and anyone who knows money and banking will see it is banking. The reason that pulling a thousand dollars out of a banking system is so problematic, more specifically out of a fractional reserve system, is that money inside that banking system creates more money than money outside of it. The difference stems from the money multiplier: a fraction of each deposit is kept in a reserve and the rest is lent out, then deposited again, creating a cascade of loans and deposits, and then another one, and on and on until the money stock has risen by a multiple of the original deposit. If instead of adding a deposit, I withdraw it, then the cascade operates in reverse, which seems to be what Friedman and Schwartz had in mind.

Figure 3 The Stock of Money and its Proximate Determinants, monthly, 1929-March 1933

They wrote as if depositors were the primary actors in the collapse of the money stock but they do not show that. If depositors were the prime movers in the decline, banks must have played a passive role but we cannot observe the temporal characteristics of the decline in their loans. In fact, a startling feature of the 100-plus pages that Friedman and Schwartz offered on the Great Depression is that they hardly mentioned bank credit never mind offered systematic evidence on what happened to it between 1929 and 1933.[77] Resourceful readers will find a hint in a table much later in the book, which shows a veritable collapse in bank loans and will surely want to know more to be sure that banks were passive actors in the collapse of deposits.[78] But if they want to see what happened to loans as a share of deposits, they have to look to monthly data on bank deposits at the back of the book, compute annual averages of deposits for 1929 and 1933, and compare them to data on bank loans for these two years.[79] The results are unsettling, since they show a sharp decline in the loan-deposit ratio from 85.2 to 64.1 per cent, suggesting that banks may not have been passive actors in the collapse of deposits. If they were active players, the rhythm of monetary events that Friedman and Schwartz described would be wrong, but there is not enough evidence in their chapter to rule out that possibility.

So much for the gaps in their account of the rhythm of monetary events but to generate a new interpretation of the Great Depression, Friedman and Schwartz needed to go further, to show how that rhythm became an economic crisis. They wrote as if that link was evident, claiming that the chronology of monetary events "serves about equally well to demarcate distinctive behavior of the other economic magnitudes".[80] However, just eyeballing the charts that accompany this statement – the only possibility their analysis allows -- we can discern a dramatic decline in national income, industrial production and prices before the banking crises even began and no obvious change in the downward trends once these crises took effect.[81]

So the reader needs something more substantive, some evidence on the mechanisms for the transmission of the crisis from the monetary sphere to the rest of the economy. Do we imagine a decline in bank loans that made it harder to finance investment and consumption? That hardly seems plausible since the authors scarcely mentioned bank loans. So was there a change in portfolios of households that made them more cautious about spending? Perhaps. But anyone's

[77] The omission is all the more striking since, in light of criticisms received inside the NBER before *A Monetary History* was published, it was deliberate (see footnote 75 above).
[78] F&S, Table 17, 450.
[79] Data on monthly bank deposits are shown in Table A.1 (F&S, 712-714) and generate annual averages of $42.4 and $25.7 billion for 1929 and 1933 respectively, which can be compared to loan data of $36.1 and $16.5 billion (F&S, 450).
[80] F&S, 305.
[81] See F&S, Chart 28, 303.

guess is as good as this reader's speculations in the absence of any clear indication from Friedman and Schwartz themselves.

To conclude, let us come to the Fed, which has not been mentioned so far and for good reason. The Fed was not involved in any of the causal junctures that Friedman and Schwartz identified as leading to the Great Depression. It entered instead as a *deus ex machina* and, more specifically, as a god that stood by and let his people suffer by failing to counteract the collapse of the U.S. money stock. Since Friedman and Schwartz emphasised what the Fed could have done and did not do, their claims could not be proven. They understood that well, as we have seen, but knew they had to find a way to make the Fed's ineptitude seem plausible.

The devices they used to do so were partly evidentiary but mostly rhetorical. To the extent that there are living, breathing, human beings in their chapter on the Great Depression, most of them work for the Fed. In fact, so many people work for the Fed that they crowd in on the reader so that he ends up confused about what any specific individual thought and did in this story. What really matters, however, is the overall impression that there were lots of actors in the Fed, that anyone of them could have done the right thing, but that none of them did. Financial crises are described as one might describe the weather: people panic, they pull their money out of banks, in the way that clouds accumulate and rain falls. People in the Fed, in contrast, had the power of reason and the ability to act but they sat on their hands, thereby turning a normal recession into the Great Depression.

2.2 A "Monumental Scientific Work"

As we have seen, there are good reasons to anticipate some robust reactions to the heretical narrative of the Great Depression that Friedman and Schwartz constructed. But when we study the reception of the book in the leading journals in economics and history, it is to be sorely disappointed. Since their subject and methods were historical in nature, the place where we would expect to find a thorough assessment is in the leading journal of economic history in the US. And we do see a promising start in an extensive review in the *Journal of Economic History* by Robert Clower. He complimented Friedman and Schwartz on their careful work on the monetary history of the United States and emphasised that: "their historical judgments about this history are based on pain-staking examination of a fantastically large body of evidence and on thorough, honest, and closely reasoned analysis of its implications". Nevertheless, he concluded: "that subsequent researches, provoked by Friedman and Schwartz's pronouncements, will overturn some of their bolder judgements, but that is another story".[82]

Certainly, it should have been another story but it was not. What we observe in Figure 4 are all of the citations to *A Monetary History of the United States* in the *Journal of Economic History* in the 25 years following the book's publication. And what is striking is how few there were. When we go further to actually read these articles and reviews, we discover hardly any of the serious, penetrating engagement we would expect to find with the historical evidence and interpretations they presented. When we extend our scope to include other leading historical journals in the United States, the pickings are slimmer still.

Figure 4 Citations to A Monetary History, 1963-1986

What systematic vetting there was in the years following the book's publication took place in the leading economics journals where *A Monetary History* featured more prominently. Studying the content of reviews and articles there, we find that a significant number of them were written by economists who shared Friedman and Schwartz's monetarist views and were only too willing to

[82] Robert Clower, "Monetary History and Positive Economics", *Journal of Economic History*, 24 (3), 1964, 364-380.

talk about the book as if its evidence and claims were above reproach. Scepticism was expressed, as we might expect, by Keynesian economists. However, their sharpest criticisms were targeted at what they saw as an unsubstantiated theoretical model lurking behind the historical interpretations offered in *A Monetary History* and fit into a broader theoretical critique of Friedman's economic writing on money.[83] In contrast, when it came to the historical analysis in *A Monetary History*, economists like Harrod and Tobin expressed respect, even reverence, for what Friedman and Schwartz had achieved and a startling willingness to accept their historical claims including, as Tobin put it, of "the passive acquiescence of the Fed in the monetary contraction and banking collapse".[84]

As Rockoff observed, most economists did not do the kind of historical work that Friedman and Schwartz offered.[85] So, even if they had been so inclined, economists had little competence to vet the basis for their arguments or to build an historical alternative to it.[86] Their limits in this regard can be seen in the contrast between their reception of *A Monetary History* and Friedman's monetarist analyses based on econometric work. Once they were on their own territory, as Béatrice Cherrier observes, Friedman's economist critics were happy to round on him, with one accusing him of charlatanism, another of "distorting his results", and a third one comparing the clarity of his econometrics to ancient Greek oracles.[87] No mainstream economist used this kind of language in describing the history that Friedman and Schwartz constructed.

Writing thirteen years after *A Monetary History* was published, Elmus Wicker emphasised how little critical assessment there had been of the book's historical analysis. By then, there were signs that some historians were unhappy with the claims that Friedman and Schwartz made. When Charles Kindleberger published a book on the Great Depression in 1973,[88] he engaged directly with *A Monetary History* and made no bones about his view of it: "In my judgment, it is wrong".[89] But Kindleberger was soon pushed back on the defensive, with one reviewer noting: "Kindleberger's dismissal of Friedman-Schwartz is vigorous but imprecise". Anna Schwartz proved to be an especially brutal critic, casting Kindleberger's "journalistic account" as "inconsistent in detail and as loosely constructed in its broad outlines", as characterized by "*obiter dicta* on substantive issues without supporting evidence and casual dismissal of opposing views". And she went further to suggest that Kindleberger had "misread or not read" crucial parts of *A Monetary History*'s chapter on the depression. The truculent reader might suggest that she and Friedman were not above a few *obiter dicta* themselves and, if not a casual neglect, then a more deliberate exclusion of opposing views.[90] Yet, even less interested reviewers than Schwartz were critical of Kindleberger's efforts to dethrone *A Monetary History* with prominent economic historian, Stanley Engerman, suggesting that: "[w]hile Kindleberger attacks the Friedman and Schwartz position, it is more by sideswiping than by direct assault, and his basic propositions are often more assumed than demonstrated. Thus, while a useful contribution, it

[83] For an extended discussion of these criticisms, see Hammond, 105-123.
[84] James Tobin, "The Monetary Interpretation of History", *American Economic Review*, 55(3), Jun., 1965, 483.
[85] Rockoff. Hugh Rockoff, Review Essay on *A Monetary History of the United States*, eh.net
[86] Rockoff, 2006,
[87] Béatrice Cherrier, "The Lucky Consistency between Milton Friedman's Science and Politics, 1933-1963," in Van Horn, Mirowski, Stapleford, 2011, 335-367.
[88] Kindleberger argued that the world economy had become inherently unstable given the deep-seated international asymmetry that had built up by the 1920s. The only way in which such instability could have been overcome, he suggests, is if the international economc hegemon of the time – the United States – had been willing to display the type of international leadership that Kindleberger attributes to the British in the 19th century (Charles Kindleberger, *The World in Depression 1929-1939*, University of California Press, 1973.
[89] Kindleberger, 1973, 20.
[90] Anna Schwartz, *Journal of Political Economy*, 83, No. 1 (Feb., 1975), 231-237

still cannot be said that Kindleberger has gone very far to rehabilitate the case for the neo-Keynesian interpretations of the period".[91]

Wicker took much the same view a couple of years later in criticising economists and historians inspired by Keynesian ideas for failing to generate a comparable study that matched "in scope or analytical achievement, the study by Friedman and Schwartz", Since they had failed to do their "historical homework", Wicker claimed, "two questions continue to haunt" the reader: "Is the Friedman and Schwartz interpretation historically valid? And are their equally persuasive nonmonetary explanations of inflations and depressions?"[92] And so historians found themselves confronted with an interpretation of the Great Depression that they were not sure was correct but without any credible alternative to it.

By 1976, however, the world was not waiting for historians, economic or otherwise, to make up their minds. That year, Milton Friedman was awarded the Sveriges Riksbank prize in Economic Science and in the citation for this prize, *A Monetary History of the United States* was singled out as "[h]is major work" and described "as one of Friedman's most profound and also most distinguished achievements": "[m]ost outstanding is, perhaps, his original and energetically pursued study of the strategic role played by the policy of the Federal Reserve System in sparking off the 1929 crisis, and in deepening and prolonging the depression that followed. The critics agree that this is a monumental scientific work which will long stimulate the re-examination of the course of events during this epoch."[93] . Where was Anna Schwartz in this tribute, one might ask, given the praise it bestowed on her co-author, and who were the critics who agreed that *A Monetary History* was a monumental work?[94]

The question is especially apt since 1976 marked the publication of a full-frontal attack on *A Monetary History*'s interpretation of the Great Depression and with it the only rival that was to gain much credibility among economic historians. In *Did Monetary Forces Cause the Great Depression?*, Peter Temin offered a clear, negative answer to the provocative question in his book's title.[95] He acknowledged the quality and influence of *A Monetary History*'s account of the Great Depression, suggesting that it: "stands without peer among narratives of the early 1930s. It is scholarly, detailed, insightful, and fascinating. As might be expected, it has had an enormous influence on our views of the Depression. It has become something like the standard history of the Depression for students of economics".[96] But then his axe fell: "[w]hat evidence do Friedman and Schwartz muster to support [their] propositions? Their narrative is long and complex, but it offers far less support for these assertions than appears at first. In fact, it assumes the conclusion and describes the Depression in terms of it; it does not test it or prove it at all". [97] So Temin set out to test "the money hypothesis" himself, concluding there was little evidence to support it, and more for "the spending hypothesis": "it is more plausible to believe that the Depression was the result of a drop in autonomous expenditures, particularly consumption, than the result of autonomous bank failures."[98]

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[91] Stanley L. Engerman, "On Avoiding the International Economic Collapse of the 1930s", *Reviews in American History*, 2(3) (Sep., 1974), 425-429

[92] Wicker, Elmus, 1976. Review of *Did Monetary Forces Cause the Great Depression?* by Peter Temin, *American Historical Review*, 81, No. 4 (Oct., 1976), 993-994.

[93] https://www.nobelprize.org/prizes/economic-sciences/1976/summary/

[94] Friedman did pay tribute to Anna Schwartz in accepting the prize although one might well ask if he should have accepted it on his own account if it was awarded for the co-written book that was singled out as "his major work".

[95] Peter Temin, *Did Monetary Forces Cause the Great Depression*?, New York: W. W. Norton, 1976.

[96] Ibid.,14.

[97] Ibid.,16.

[98] Ibid.,178.

There was a significant theoretical opposition between these rival interpretations about the macroeconomic relationship between money and income. More striking still is Temin's methodological distance from Friedman and Schwartz, which is implicit in the prominence he gave to a so-called "autonomous" drop in consumption. Friedman and Schwartz built their interpretation on an historical reconstruction of the changing rhythm of monetary events. Temin, in contrast, relied on the specification of econometric models of consumer spending and applied them to historical data to identify a drop in consumption that could not be explained by the models. In claiming that this "autonomous" drop in consumption was a cause of the Depression, Temin assigned historical significance to an econometric artefact. But, in the spirit of Friedman's earlier critique of Tinbergen, that artefact could be interpreted as evidence of the difficulties of specifying a simple mathematical function to account for consumer behaviour in the changing US economy of the 1920s and 1930s. Certainly, the character of Temin's autonomous drop in consumption meant that he could not explain it in econometric terms and he did not go very far in offering an historical analysis to fill the gap, concluding that it derived "from a variety of diverse and as yet still incompletely delineated sources".[99] But, as Temin acknowledged himself, "[i]t is somewhat unsatisfactory to say that the Depression was started by an unexplained event".[100]

For that reason, Hyman Minsky suggested that the appropriate conclusion to be drawn from Temin's analysis about "the relative validity of two currently fashionable views" was that "[n]either hypothesis really passes the tests". Minsky's real concern with the book, however, was the limited scope of the theoretical explanations of the Depression that it considered, specifically the fact that Temin cast the monetarist and spending models as "the outer limits of the spectrum of theoretical explanation to be tested". Minsky saw himself as writing in a long tradition of economists "who held that the capitalist process is endogenously unstable" and his name is associated with "the financial instability hypothesis", the claim that "the normal functioning of a capitalist economy leads to conditions conducive to a financial crisis".[101] Writing in the 1970s, he emphasised just how heretical such ideas seemed: "Although financial instability, even onto crises, is a fact of capitalist economic life, it is a non-event, something which just cannot happen, insofar as the standard body of economic theory is concerned". But Minsky criticised Temin not just for his neglect of financial instability but of any economic dynamics that might generate endogenous instability: "[e]ven if Temin had chosen to ignore the work of his contemporaries who look beyond the narrow confines of neoclassical theory and had merely considered the explanations of the "ancients," his work would have been much stronger and much more relevant to an understanding of real world phenomena".[102]

But Temin was not alone among US economic historians in seeing possible economic explanations of the Great Depression as stemming from a monetarist school and whatever version of Keynesianism could be squeezed into an econometric model. That Engerman, Wicker and Temin could all conceive of Keynesians as the only likely rival to *A Monetary History*'s interpretation of the Great Depression suggested the intellectual shrinking of the debate about business cycles that had occurred in the 25 years since Mitchell and Schumpeter died. It is worth asking whether heterodox economists bear some responsibility for this narrowing of the scope of imaginable explanations of cycles. Minsky was an economic heretic but he was less of a historian than Mitchell or Schumpeter. Still, even when heterodox economists offered forceful criticism of *A Monetary History*'s historical analysis – as Anne Mayhew did in "Ideology and the

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99 Ibid.,172.
100 Temin, 1976, 83.
101 Minsky, 1979, *The Financial Instability Hypothesis : Capitalist Processes & the Behavior of the Economy*,
102 Hyman Minsky, "Did Monetary Forces Cause the Geat Depression : A Review", *Challenge*, October-November 1976.

Great Depression: Monetary History Rewritten" [103] – or generated their own interpretations of the causes of the Great Depression in the United States, they made hardly any impact on the debate among economic historians.[104]

Following the publication of *A Monetary History*, therefore, its only serious contender was Temin's autonomous drop in consumption. The consumption view attracted some further interest from economic historians such as Christina Romer and Martha Olney who tried to put more historical flesh on its econometric bones. But if we look at citation patterns, even before citations to *A Monetary History* really took off, we can see that the attention that Temin's book garnered fell far short of its target. And no further rivals emerged to challenge the money hypothesis head on.

Indeed, there was a marked and rapid retreat from confrontation even by one of *A Monetary History*'s grumpier readers. Kindleberger had picked up on Minsky's financial-instability hypothesis and used it to frame a new book, *Manias, Panics and Crashes*, published in 1978, but he took pains to cast himself as a modest historian with no claim to be an economic heretic. In a lecture on the book in honour of Paul Samuelson, Kindleberger hoped that nothing he said would be "taken to suggest that I believe markets don't work well at all. On the whole they do. My position is far from that of Socialists or planners or the New International Economic Order or whatever. While I recognize the arguments for second-best solutions based on monopoly and the like, I am less moved by the thought of market failure than by the possibility of occasional breakdown. On the usefulness of the market overall I am much closer to Friedman and Johnson than to say Prebisch or the late John Blair or Marglin".[105]

So much for those who thought we were talking about the historical dynamics of economic cycles and crises! And lest there be any doubt that what was at stake was akin to religious orthodoxy and heresy, Kindleberger offered the following analogy:

> Milton Friedman is to markets as Christian Science is to the human body. For the Christian Scientists the body cannot be sick. For Friedman, markets always function properly. At the other end of a wide spectrum are the hypochondriacs and pill-poppers and the planners who will replace the market. My position is much closer to Friedman, as I have just said, than to the planners, and much closer to the Christian Scientists, than to the hypochondriacs.

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[103] Anne Mayhew, "Ideology and the Great Depression: Monetary History Rewritten", *Journal of Economic Issues*, 17 (2), (Jun., 1983), 353-360.

[104] The work of Josef Steindl is a good example, specifically *Maturity and Stagnation in American Capitalism*, Monthly Review Press, 1952, as is the neglect by Robert J. Gordon of the work of his own brother, David M. Gordon, a prominent heterodox economist at the New School and a specialist of business cycles (see Robert Gordon, "Introduction: Continuity and Change in Theory, Behavior,and Methodology", *The American Business Cycle: Continuity and Change*, University of Chicago Press, 1986 for its neglect, for example, of David M. Gordon, "Up and Down the Long Roller Coaster" in URPE, ed., *U.S. Capitalism in Crisis*. New York: Union for Radical Political Economics, 22-35 & David M. Gordon, "Stages of Accumulation and Long Economic Cycles," in T. K. Hopkins & I. Wallerstein, eds., *Processes of the World-System*, Sage Publications, 1980, 9-45). It is worth noting too that Michael Bernstein, who surely offered the most serious heterodox account of the crisis, went to some lengths to present his work not as an analysis of the causes of the Great Depression in the United States, in other words, not as a challenge to Friedman and Schwartz, but as an explanation of why the Depression was as long as it was (Michael Bernstein, *The Great Depression: Delayed recovery and economic change in America, 1929-1939*, Cambridge University Press, 1987); see also Mark Wheeler, editor, *The Economics of the Great Depression*. Kalamazoo, MI: W.E. Upjohn Institute for Employment Research, l998).

[105] Kindleberger, 1978, "Manias, Panics, and Rationality", *Eastern Economic Journal*, 4, No. 2 (Apr., 1978), 103-112; Kindleberger, *Manias, Panics, and Crashes: A History of Financial Crises*, Macmillan, 1978.

It seems staggeringly inappropriate to invoke such an analogy in fawning over one of the leading Jewish intellectuals in the United States in a lecture to honour another – Kindleberger did acknowledge it "must be offered with particular delicacy or it may be thought offensive by some" – but it reveals a good deal about how economic heretics and hypochondriacs were kept at a distance from historical debate.

2.3 The Monetary Ties that Bind

Although there were few efforts to offer a comprehensive challenge to *A Monetary History*'s interpretation of the Great Depression, it would be a serious mistake to conclude that historians did not criticise the book. However, they increasingly entered the debate about the causes of the Great Depression by proposing extensions to the monetary analysis of the Great Depression or qualifying specific elements of it. There was little attempt, in contrast, to confront or even evaluate the core claims on which Friedman & Schwartz's monetary interpretation was constructed.

The extension of the money hypothesis to include the financial system was the most significant modification of *A Monetary History*'s interpretation and the most dramatic example of how monetarist ties bound historical research on the Great Depression. Of all the aspects excluded from thoroughgoing consideration in Friedman and Schwartz's account, the instability of the financial system was the most peculiar. Their interpretation depended on banking crises, and distinctly unorthodox notions of contagion or panic to precipitate a monetary collapse. However, as we have seen, they displayed a peculiar reluctance to engage in any systematic evaluation of the role of the banking system during the crisis.

Notwithstanding the book's obvious shortcomings, there was limited attention to "Financial Factors in the Great Depression", as Charles Calomiris noted in his survey of the topic, in the two decades after *A Monetary History* was published. He noted that there were "a few dissidents" interested in the theme, such as economist, Hyman Minsky, but emphasised that only economic historian, Charles Kindleberger, focussed on the Depression. And Calomiris suggested that "[h]is insistence on complex financial linkages and feedback across countries, without supplying formal modeling or measurement of these mechanisms, was welcomed with the enthusiasm accorded Banquo at Macbeth's feast." [106] That situation changed, however, with the publication of Ben Bernanke's enormously successful article in 1983 on "Nonmonetary Effects of the Financial Crisis in the Propagation of the Great Depression".[107]

Bernanke opened with a vivid statement of the chaotic situation in which the US financial system found itself during the Great Depression and he emphasised the close association between the financial crisis, especially bank failures, and "adverse developments in the macroeconomy". We might think, he said, that "the financial system simply responded, without feedback, to the declines in aggregate output" but he dismissed this view as "contradicted by the facts". The conclusion he drew from his assessment of the existing literature on the Great Depression was that the economic effects of the financial collapse of the 1930s were poorly understood.[108]

That it took so long for such an acknowledgement to be made in the leading economics journal of the United States offers food for thought but what really makes one wonder is where Bernanke went next. First, he identified his allies. He explained that there was "much support for

[106] Charles W. Calomiris, "Financial Factors in the Great Depression", *Journal of Economic Perspectives*, 7, No. 2 (Spring, 1993), 61-85.
[107] Ben Bernanke, "Nonmonetary Effects of the Financial Crisis in the Propagation of the Great Depression", *American Economic Review*, 73(3), (Jun., 1983), 257-276.
[108] Ibid., 257.

the monetary view" propounded by Friedman and Schwartz, but that "it is not a complete explanation of the link between the financial sector and aggregate output in the 1930's" since "the reductions of the money supply in this period seems [sic] quantitatively insufficient to explain the subsequent falls in output". He suggested that his argument "builds on the Friedman-Schwartz work", thus opting to present his work as a twist on, rather than a challenge to their interpretation. Second, he chose his enemies, ruling out contemporaries to whom he might have turned for insights on the instability of the US financial system: "Hyman Minsky (1977) and Charles Kindleberger (1978) have in several places argued for the internal instability of the financial system, but in doing so have had to depart from the assumption of rational economic behavior".[109] That was an odd way to take sides given that Friedman and Schwartz relied so heavily on a contagion of fear among depositors to bring about the collapse of the US money stock and it should make us curious about how Bernanke proposed to "build on" the work of Friedman and Schwartz without flouting his own rationality postulate.[110]

Reading Bernanke's discussion of the financial collapse, it becomes clear that he promised something that he could not deliver. He identified two major reasons for the financial collapse. First, the US system "historically suffered" from a "malign source of bank failures; namely, financial panics". The *Oxford English Dictionary* defines panic as "[a]n excessive or unreasoning feeling of alarm or fear leading to extravagant or foolish behaviour, such as that which may suddenly spread through a crowd of people".[111] If Bernanke thought that panic meant something different, something reasonable and reasoning, then he did not explain what it would be. Just as in *A Monetary History*, it was panic that precipitated the bank runs in Bernanke's explanation and it was panic that meant that: "the entire system was adversely affected" and not just the "marginal banks". The second reason that Bernanke evoked for the financial collapse went beyond Friedman and Schwartz to draw on Irving Fisher in emphasising the problem of debt deflation. Bernanke insisted that the problem stemmed not only from deflation – since prices had dropped before without leading to "mass insolvency" -- "but also to the large and broad-based expansion of inside debt in the 1920's". Bernanke offered a brief overview of the credit expansion that occurred in the 1920s, clearly suggesting it was excessive.[112] What he did not explain, however, is how over-indebtedness occurred in the rational world he was trying to coax out of a most irrational history.

Oddly enough, it was to this question that Irving Fisher turned in his classic article on "The Debt-Deflation Theory of Great Depressions" that Bernanke invoked. In a section entitled "Debt Starters" Fisher emphasised that "[t]he over-indebtedness hitherto presupposed must have had its starters" and then turned to an analysis of its causes. Insofar as "the over-indebtedness of 1929" was concerned, he emphasised that "[w]hen an investor thinks he can make over 100 per cent per annum by borrowing at 6 per cent, he will be tempted to borrow, and to invest or speculate with borrowed money". And he suggested that these conditions came about in the 1920s because, on the one hand, "[i]nventions and technological improvements created wonderful investment opportunities" and, on the other hand, money was "easy". But he went farther in his reflections to suggest that the "public psychology of going into debt for gain" passed through distinct phases from "the lure of prospective dividends or gains in income in the remote future" all the way through to "the development of downright fraud, imposing on a public which had grown credulous and gullible". Fisher's basic schema was taken up by Hyman Minsky and developed into a full-blown theory of financial instability but Bernanke chose to

[109] He went on to explain that "I do not deny the possible importance of irrationality in economic life; however, it seems that the best research strategy is to push the rationality postulate as far as it will go" (ibid., fn 5, 258).
[110] F&S, 1963, 308.
[111] Lesley Brown, ed., *The New Shorter Oxford English Dictionary*, volume 2, N-Z, Oxford, 1993, 2084.
[112] Bernanke, 259-261. His account is largely based on an article by Charles Person, published in November 1930.

"presuppose" over-indebtedness and overlook the financial dynamics that both Fisher and Minsky pinpointed as generating it.

He consigned the financial collapse to the background of his analysis in order to focus his attention on its impact on the quality of credit intermediation in the US economy.[113] In this regard, Bernanke looked for inspiration to models of imperfect information that were being developed in the late 1970s and early 1980s to suggest a story that went as follows. The financial collapse of the early 1930s precipitated "a contraction of the banking system's role in the intermediation of credit" but "[s]ome of the slack was taken up by the growing importance of alternative channels of credit". However, the "rapid switch away from the banks (given the banks' accumulated expertise, information, and customer relationships) no doubt impaired financial efficiency", increasing the cost of credit intermediation. And, with the diminished importance of banks, the real economy bore the burden of poorer information and, as a result, an increasingly inefficient allocation of financial resources.

It is worth reflecting on this argument for a moment to acknowledge its improbable character. Bernanke was talking about a banking system that he had just described as having plunged itself into over-indebtedness but he offered a model that assumed this same banking system had superior information. He offered no evidence in support of this claim and could not even show that a shift from banks to other credit channels drove up the cost of credit intermediation. As he explained: "It would be useful to have a direct measure of the *CCI* [cost of credit intermediation]; unfortunately, no really satisfactory empirical representation of this concept is available". Not to be deterred, however, he suggested that: "[w]hile we cannot observe directly the effects of the banking troubles on the *CCI*, we can see their impact on the extension of bank credit".[114] The only historical evidence that Bernanke added to what Friedman and Schwartz offered was monthly data on bank credit that he compiled from the widely used *Banking and Monetary Statistics* of the United States.[115] But as the basis for Bernanke's claims about the nonmonetary effects of the financial crisis in the propagation of the Great Depression, claims that turn on a theoretically specific and historically implausible story about banks' superior information, his evidence must be seen as approximate.

It is worth highlighting that Bernanke's 1983 paper is celebrated for its role in bringing about a paradigm shift in our understanding of the Great Depression in the United States. In Calomiris's words: "Bernanke's (1983) contribution was to combine theory and empirical evidence to argue that financial collapse was more than a symptom of economic decline; financial collapse deepened the Depression by hampering the efficient allocation of capital."[116] The reference to evidence seems like a stretch given that Bernanke presented no direct evidence on any concept

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113 Presumably the fact that Fisher did not delve into the undoubtedly messy roots of the overindebtedness that made him Bernanke's preferred choice of inspiration rather than Minsky who had extended Fisher's analysis to generate his theory of financial instability.

114 Bernanke, 264.

115 These data extended from July 1929 to March 1933 and confirm, as I suggested above, that there was a major change in the relationship between loans and deposits in the Great Depression, a relationship that *A Monetary History* overlooked. In Table 1 of his paper, it seems that Bernanke compared bank loans with monthly data on industrial production, deposits and liabilities of failed banks, and total bank deposits. Bernanke concluded from his scrutiny of these different series that "credit outstanding declined very little before October 1930", that "[t]he shrinkage of credit shared the rhythm of the banking crises", but that "[t]he fall in loans after November 1930 was not simply a balance sheet reflection of the decline in deposits". The limited data that Bernanke presented on bank loans as a share of deposits offers ambiguous support for these claims. In addition, he normalises the change in commercial bank loans either by personal income or industrial production but an unfortunate mistake in his table, text or both makes it difficult to know which one he used. Bernanke refers to an analysis of loans (L) and personal income (PI) in the text, and in a note to Table 1, but then shows data only for $\Delta L/IP$ where IP is the name he gives to his variable for industrial production. Perhaps he inadvertently reversed the letters so we are really looking at a series for $\Delta L/PI$ in the fifth column of Table 1 but it is hard to know (ibid., 263-4).

116 Calomiris, 68.

that featured in his theoretical analysis. And, as for theory, if Calomiris is right that Kindleberger's impact was limited by the fact that he offered no "formal model" of the mechanisms he emphasised, what accounts for Bernanke's influence? He did not present a formal model of the mechanisms he emphasised but that did not stop his paper becoming the only publication on the Great Depression in the United States that came close to attracting anything like the attention accorded to *A Monetary History*.

Figure 5 Citations to Friedman's most-influential contributions to monetarism versus Temin's *Did Monetary Forces Cause the Great Depression?*, 1956-2018

Bernanke's extension of the money hypothesis to include the financial system was the most blatant example of how researchers might fit what they saw in the past, no matter how unorthodox it seemed, into what was increasingly an economic orthodoxy of the Great Depression. For the most part, the influence of this orthodoxy on historical research was much less deliberate, and sometimes contested, and reflected more in the topics that researchers explored than what they discovered about them. Of particular importance in this regard were historians' preoccupation with the causes of US bank failures and the foundations of Federal Reserve System's policies in the Depression.

Following the publication of *A Monetary History*, it was Peter Temin who set a debate in motion about the causes of bank failures. He claimed that: "[t]he banking panics were a part of a larger process that started with the decline in autonomous spending". Put differently, they were a symptom of a Great Depression that was already underway rather than the result of liquidity shocks.[117] Some reviewers pointed out that Temin had little evidence for his claim but his critique did suggest just how little was known about the US banking crises of the Great Depression.[118] What ensued was an extended debate among economic historians about the causes and consequences of US banking failures in the early 1930s.

Early contributions came from Elmus Wicker and Eugene White on the banking crisis of 1930, and both of them challenged the Friedman and Schwartz story, arguing that the bank failures of 1930 did not mark a drastic change in US banking history and were unlikely to have had the macroeconomic impact that Friedman and Schwartz attributed to them.[119] Far from ending there, the discussion went on, with studies accumulating on both clusters of banks and specific banks that failed, with historians taking sides on the claims about banking crises in *A Monetary History*.

On one occasion, when one historian directly challenged its interpretation of the failure of the Bank of the United States, Friedman and Schwartz made an extraordinary intervention in the debate. Joseph Lucia offered evidence that the failure stemmed from the bank's insolvency and argued that: "[g]iven its regional nature, with a majority of loans in New York real estate, it is difficult to envisage its failure having a deflationary impact nationally". Friedman and Schwartz offered a ferocious reply in which, they questioned "the scientific integrity" of the author and "the scientific standards" of *Explorations in Economic History*. Their justification for such extraordinary accusations was that Lucia had distorted the views they expressed in *A Monetary History* and had failed to mention that he had been in correspondence with Friedman before his article was published or to use information that Friedman had sent him.

[117] 1976, 9-10.
[118] He presented some limited evidence to substantiate his claim, such as correlations between the geographical location of bank failures in 1930 and 1931 and agricultural incomes. CHECK Thomas Mayer, "Money and the Great Depression: A Critique of Professor Temin's Thesis", *Explorations in Economic History*, 15, 127- 145 (1978).
[119] Wicker, Elmus R., "A Reconsideration of the Causes of the Banking Panic of 1930," *Journal of Economic History*, September 1980, 40:3, 571-83 ; White, Eugene N., "A Reinterpretation of the Banking Crisis of 1930, *Journal of Economic History*, March 1984, 44:1, 119-38.

The brief article that follows stands in stark contrast to *A Monetary History* in its ratio of content to bluster but two important points can be discerned in the angry rant. First, Friedman and Schwartz acknowledged that Lucia's analysis of the failure of the Bank of the United States was far more extensive than the one they offered and based on historical evidence that they did not have at their disposal. Indeed, they suggested that if Lucia had focussed on the reasons for the failure of the Bank of the United States, there might have been no problem but instead "he could not resist the temptation to make a bigger splash in a bigger pond" by addressing the effect of the bank's failure "upon the banking crisis and its subsequent development". Second, and here they repeated what they had stated in *A Monetary History*, why specific banks or even clusters of banks failed was much less important than the impact of these failures on "monetary and economic developments in the country as a whole". And on this issue, they claimed that Lucia "offers essentially no evidence on this question, only unsupported assertions". They omitted to mention that they had offered limited evidence themselves and had nothing new to offer in this regard. Instead, they referred to "the sharp decline in the deposit-currency ratio in December 1930 – the clearest indication that the bank's failure had more than local effects" but to call this causal evidence is surely to stretch the logic of *post hoc ergo propter hoc* to unpersuasive limits.[120]

In the wake of this intervention, we might have expected some refocusing of the historical debate to place greater emphasis on tracing the impact of bank failures on monetary and economic variables. However, the causes of bank failures in the Great Depression continued to absorb most of the attention. Forty-five years after *A Monetary History* was published, Gary Richardson offered an extensive review of the literature on the causes of bank failures during the Great Depression that concluded that the illiquidity versus insolvency debate was inconclusive.[121] And so the debate goes on…

Another aspect of *A Monetary History*'s interpretation of the Great Depression that stimulated attention, and some modification in this case, was its explanation of the Fed's alleged ineptitude. Friedman & Schwartz posited a deterioration in the quality of the Fed's leadership following the death of Benjamin Strong in late 1928, going as far as to suggest that had Strong lived, the US depression might have ended in 1930, and the world economic crisis been averted.[122] That claim soon came under fire from historians who argued that there was little evidence of a marked shift in policy regime at the Fed following Strong's death. Attention shifted to other explanations of the Fed's alleged unwillingness to counter the decline in the money stock.

Around the same time, the debate about monetary factors in the Great Depression went international, with Temin (1989) and Eichengreen (1992) arguing that the commitment to the gold standard constrained central banks around the world from adopting expansionary policies.[123] Thus, it was not the stupidity of a particular generation of Federal Reserve officials that was highlighted as the problem but the grip that particular ideas about money -- a gold fetish --maintained over the minds of policymakers around the world. Thus the global crisis was no longer acknowledged as a crisis of capitalism but a recession that turned into a depression by a glitch in the system, a human error of government officials, one that was unnecessary and avoidable in a capitalist system that was inherently stable.

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[120] There was a reprise of this debate ten years later, although Joseph Lucia had died in the meantime.
[121] Gary Richardson, "Categories and causes of bank distress during the great depression, 1929–1933: The illiquidity versus insolvency debate revisited", *Explorations in Economic History*, 44 (2007), 588–607.
[122] F&S, 407-419 ; 692.
[123] Temin, Peter, *Lessons from the Great Depression*. Cambridge, Massachusetts: The MIT Press, 1989; Eichengreen, Barry, *Golden Fetters: The Gold Standard and the Great Depression, 1919-I939* (New York, I992).

Although there was disagreement about the specifics -- Anna Schwartz resisted the gold fetish interpretation for the United States[124] -- the consensus that emerged about the centrality of monetary factors as causes of the Great Depression was extraordinary.[125] Extraordinary not because Friedman and Schwartz's monetary interpretation of the Great Depression was inherently implausible but since there was still not enough historical evidence to evaluate the basic claims on which it was constructed. That is clear in a recent paper by Kris Mitchener and Gary Richardson. They note that: "[b]anking panics were a notorious feature of the Great Depression. Friedman and Schwartz famously described the panics, which began in the autumn of 1930, as "a contagion of fear [that] spread among depositors," leading to widespread runs that "had no geographical limits". But they go on to explain that "[s]urprisingly, empirical analyses of the effects of the banking panics of the 1930s on lending, the money multiplier, and the money supply surprisingly do not exist for the Great Depression". For that reason, "[t]he contagion-of-fear hypothesis rests on narrative evidence and time-series aggregates collected decades ago."[126] In other words, we still do not have any more evidence than the limited evidence that Friedman and Schwartz offered for the key mechanism in their interpretation of monetary events.

Much the same can be said about the relationship between these monetary events and the collapse in the US economy that defined the Great Depression. The title of a 2013 paper by Christina and David Romer – "The Missing Transmission Mechanism in the Monetary Explanation of the Great Depression" – conveys the main point. As the authors explain: "the book proves that monetary shocks caused the Depression is a stretch. Of the monetary shocks Friedman and Schwartz identify, those early in the Depression are arguably the most tenuous. And crucially, the book provides scant discussion of how monetary shocks affect the economy. This weakness is most pressing in the analysis of the Depression."[127] That weakness should have been evident to anyone who read the book when *A Monetary History* was published so it is telling that it still needs to be expressed fifty years later.

3. The U.S. Great Depression as a Real Problem

So where does that leave us insofar as historical research on the Great Depression is concerned? With a real problem, I would like to suggest, and a real problem in a double sense. First, it is a real problem that research on the economic history of the Great Depression in the United States has been trapped in an economic orthodoxy that turns on and around *A Monetary History*. Its interpretation of the crisis has blinded us to any serious contemplation of the possibility that a capitalist economy is inherently unstable, just as Friedman intended it to do. Second, we have strayed very far from grappling with the Great Depression as a problem of the real economy. And as a result, we still do not know why industrial production and investment and consumption and employment and wages collapsed in the United States between 1929 and 1933.

3.1 The scope for economic heresy

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[124] Anna J. Schwartz, Review of *Lessons from the Great Depression: The Lionel Robbins Lectures for 1989* by Peter Temin, *Economica*, 58 (232), Nov., 1991, 535-536.

[125] For an expression of that consensus, see Barry Eichengreen and Peter Temin, "The Gold Standard and the Great Depression", *Contemporary European History*, 9(2), 2000, 183-207 where they say "Writing about the United States, Friedman and Schwartz concentrated on policy actions (and inaction) by the Federal Reserve System, which they characterised as mistakes. More recent work has revealed that the Fed continued to act in the early 1930s according to patterns it had established in the previous decade…These patterns, as we will describe later, were designed to defend and maintain the gold value of the dollar against attack, not to stabilise the economy".

[126] Kris James Mitchener and Gary Richardson, Contagion of Fear, CEPR Discussion Paper Series, DP14510, March 19th, 2020.

[127] Christina Romer & David Romer, "The Missing Transmission Mechanism in the Monetary Explanation of the Great Depression", *American Economic Review: Papers & Proceedings* 2013, 103(3): 66–72

We could take the sanguine view that such heretical views will bubble up from historical research if they are worth considering. Indeed, there has been a veritable outpouring of historical research on the Great Depression in recent years and signs that the ties that bound the economic history of the Great Depression to a monetary orthodoxy may be loosening. Much of this research is concerned with historical precursors of the financial dynamics that led to the global financial crisis of 2008-2009. Ironically, it may be a financial crisis that generated unprecedented public attention for *A Monetary History* that is sowing the seeds of academic dissent about its basic premises.

Whatever hope there is of heretical insights from research on the financial dynamics of the Great Depression, there is less reason to be optimistic when it comes to the crisis as a real phenomenon. To imagine that an entrenched orthodoxy can be shaken by the accumulation of historical facts is naïve. Most of all it ignores the crucial lesson that Friedman taught us in referring to Mitchell: we need theory to focus our empirical work on meaningful problems, to make it analytic as well as descriptive, and to prevent us from engaging in empiricism for its own sake. And once we acknowledge the importance of theoretical reflection in our historical research, we can see the inadequacy of the standard theoretical approach used to generate alternative accounts of the Great Depression in the United States.

We have seen that inadequacy in Peter Temin's efforts to counter the money hypothesis by trying to capture the real dynamics of the Great Depression with the notion of an autonomous drop in consumption. We see a similar approach in a more recent study by David Greasley and Jakob Madsen that suggests that the decline in investment in 1930 has as much right as consumption to be treated as an autonomous cause of the Depression. But the problem with their account is the same as for Temin's study and, to their credit, they acknowledge that: "[o]f course, neither consumption nor the fixed investment slumps of 1930 were truly autonomous: rather, they are unexplained by conventional models".[128] The question that begs is whether the identification of autonomous shocks offers us insight into the historical dynamics of the US economy or the limits of simple mathematical functions for representing complex and changing economic relations?

Given the inadequacies of this approach, we might wonder why it persists? It is not the only game in town since we might appeal to real business cycle theory or theories of long waves of technological change. But that the only imaginable causes of cycles and crises are external to the economic system is the symptom of a specific way of thinking about its functioning. If we insist on studying our economies only in terms of their character as market economies, and market economies with strict conditions attached to the way their markets work, then we limit our capacity to imagine the possible causes of cycles and crises.

In the 1920s and 1930s, and for nearly a century before that, a common way to think about gluts and crises was as a problem of under-consumption or over-production. Such claims were always controversial since critics argued these features could not persist in a market economy. After World War 2, claims of under-consumption and over-production bit the dust among mainstream economists so their explanations of crises and cycles looked to exogenous factors instead. In the process, more consistency may have been achieved in our theories of market economies but only by losing sight of the intuition that something in the historical dynamics of the economic system might generate endogenous cycles.

And that suggests that the standard theoretical frameworks that are available to economic historians are not only unhelpful for explaining cycles but may be an obstacle to understanding

[128] David Greasley and Jakob Madsen, "Investment and Uncertainty: Precipitating the Great Depression in the United States", *Economica*, February 2006, 73(291): 393-412.

them. The US economy in the 1920s and 1930s was a market economy in which goods and services were exchanged at prices set in markets. But it was more than a market economy; it was a capitalist economy too. What difference does that make, one might ask ? Well, a great deal of difference, as it happens, and we could turn in several heretical directions to see that. But since we began with Wesley Clair Mitchell, what better way to end than by turning to him again?

3.2 A few leaves from classic books

Mitchell articulated a theory that located the rhythm of business cycles in the dynamics of enterprises' profit making. And his analysis of profit making was grounded above all in scrutiny of the changing system of prices in an economy and its impact on enterprises' revenues and costs. We could ask for more by pointing to determinants of profits that other heterodox economic traditions have emphasised. And in my own work on the history of profit I have highlighted issues that are not envisaged in Mitchell's analysis.[129] Nevertheless, there are still plenty of insights to be gained from what Mitchell offered and it is worth reflecting on what they might offer in more concrete terms.

In his 1913 book, Mitchell sketched an empirical methodology for making the links he posited between prices and profits but the possibilities for applying his methodology were sorely constrained by the limited data available in his day. At the NBER he hoped to overcome these constraints through the generation of new data and his success can be seen in the extraordinary outpouring of price series and profit data. Moreover, synthetic studies by Frederick Mills on the "anatomy of prices" in the US economy, and by Ralph Epstein and Florence Clark on industrial profits, offered strong signals of the potential that Mitchell had seen for trying to link one to the other. Whatever the promise of his research agenda, however, Mitchell never succeeded in bringing new data on prices and profits into sufficiently close association to develop his theory of business cycles.

But the potential of Mitchell's approach can be seen by engaging with a recent paper in the *Journal of Economic History* that focuses not on the Great Depression but on the major US recession of 1937-1938. In "What was Bad for General Motors was Bad for America", Joshua Hausman explains that "the most popular explanation for the 1937/38 recession is restrictive monetary policy", citing Friedman and Schwartz as an influential advocate of this view. Hausman takes issue with a monetary explanation on the grounds that the decline in the money supply is "ill-suited to explain a much more rapid decline in industrial production". He offers an alternative perspective, claiming that "labor-strife-induced wage increases and an increase in raw material costs" in the US automobile industry contributed to the recession's severity.[130]

Hausman's analysis seems to owe nothing to Mitchell except for the rich array of NBER price series it employs on raw materials and automobile prices. Moreover, his repeated reference to "an auto industry supply shock" is conventional and, as such, antithetical to Mitchell's emphasis on the internal dynamics of the capitalist system for explaining business cycles. But closer scrutiny shows that Hausman is proposing a version of Mitchell's profit-squeeze theory of recessions. Furthermore, by using Mitchell's framing, we can improve on the empirical analysis that Hausman offers.

To make his case, Hausman estimates the costs of the raw materials and hours of work required to make a small car in the US in 1936 and 1937. Since his results suggest that material and labour costs were about equally important, and that they both rose sharply in 1937, Hausman

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[129] Mary O'Sullivan, "The Intelligent Woman's Guide to Capitalism", *Enterprise and Society*, 19(4), December 2018, 751 – 802.

[130] Joshua Hausman, "What was Bad for General Motors was Bad for America : The Automobile Industry and the 1937/38 Recession", *Journal of Economic History*, 76, 2 (June 2016), cited at 433 and 435.

concludes that they contributed in roughly equal measure to rising cost pressures. He argues that consumers rushed to buy cars in anticipation of a price rise -- presumably as carmakers sought to avert a profit squeeze -- and stopped buying them when automobile prices actually rose, leading to a collapse in industry sales. Hausman never refers to profit, or any synonym for it, even though he quotes Alfred Sloan of General Motors as lamenting the decline in net income "in relation to unit and dollar sales volume". This is not just a question of semantics since thinking in terms of profit suggests new insights for this kind of analysis.

The selling price of a typical small car in 1937 was $575. Hausman estimates total factory costs at $202, implying an enormous gross profit of 65 per cent on the price of a small car.[131] That is far above the gross profit of 10 per cent of sales reported by the Federal Trade Commission's for GM's small car in 1937.[132] The report does not allow us to make similar calculations for other US automakers. However, data from the Biennial Census of Manufactures for the motor vehicle industry, shown in Table 1, confirm that its average gross profits were below 12 per cent of sales in 1937. In addition, they show that cost pressures were generating a profit squeeze in the car industry in 1937, as Hausman suggests, but they qualify his analysis of the sources of these pressures.

Table 1 Census Statistics for the U.S. Motor Vehicle Industry, 1923-1937

Crucially, we can see that rising labour costs were not the main source of a profit squeeze in 1937. Wages were too low as a share of costs to make as much difference as the much weightier expense in automakers' cost structure.[133] The rising costs of materials, fuel and purchased energy were a more significant source of a profit squeeze in 1937 but since their increased weight dates from the early 1930s, the problem did not stem, as Hausman suggested, from "actual and expected rearmament demand in Europe".[134] By the mid-1930s, rising input costs were pressing down on the percentage shown in the last column of Table 1, which is not a measure of net profits, since it includes salaries as well as other administrative and central expenses, but tells us what surplus was available from which net profits might be generated.

It is tempting to go back earlier, all the way back to the 1920s, to see if we can use this approach to say something about the role of the automobile industry in the Great Depression, a seemingly sensible route to take given the significance of the industry's collapse from 1929 to 1933. Whatever story there is to tell, it must be different from any tale of the 1937-38 recession. Indeed, there is no obvious sign of any profit squeeze in 1929, which seems to have been a record year for automotive profits. But Mitchell already warned us that profits might reach their peak just before a crisis, and suggested that we should look to the cumulative build-up of pressures that might threaten a profit squeeze in the future.

The temptation, therefore, is to dig a little deeper into the automobile industry to see if we observe such a build-up. But instead of doing that, I will offer an oblique perspective on the

[131] This estimate seems inconsistent with the contemporary estimate from DuBrul (1939) that Hausman quotes, which suggests that raw materials and labour represented about 85 per cent of a car's sales price. The problem seems to stem, at least in part, from Hausman's choice of price series for estimating material costs. For steel, for example, he uses a price index for composite furnished steel products (m04155) rather than auto body and rolled steel sheets (m04157). In terms of cents per pound, the latter was an average of 50 per cent higher than the former in 1937.

[132] The net sales revenue for a Chevrolet passenger car for 1937 was reported to be $556.10, factory cost of sales as $501.96, with no breakdown between materials and labour costs, and gross profit per car as $54.14 (Federal Trade Commission, *Report on Motor Vehicle Industry*, pursuant to Joint Resolution No. 87 (H. J. Res. 594) Seventy-Fifth Congress, Third Session, United States, Washington, 1939, 538).

[133] Moreover, the sharp increase in average hourly earnings of more than 20 per cent was partially compensated by a decline in average working hours.

[134] Hausman, 453-4.

automobile industry by taking a methodological leaf out of another classic book. *A Monetary History* taught us the power of counterfactual reasoning in historical analysis and, specifically, of thinking about how actors might have behaved before assessing how they did behave. We can apply that lesson here by looking at another industry that experienced a similar expansion to the automobile industry during the 1920s but behaved rather differently during the Great Depression.

3.3 *A provocative counterfactual*

The image below from March 1929 evokes a product that was just as much a symbol of the Roaring Twenties in the United States as the automobile. Silk stockings were one of the few US textile industries that enjoyed a boom in the 1920s; as the Jazz Age dawned, and skirt hems rose, so did the demand for silk stockings. As we learn from Philip Scranton and Sharon McConnell-Sidorick, the production of silk hosiery was much more dependent on skilled workers, some of whom were women, than the car industry. Expanding demand meant a keen competition for skilled workers as hosiery companies increased their production. And workers secured a rising share in the industry's prosperity with wages increasing as a share of the value produced by the industry.[135]

Figure 6 Advertisement for Silk Hosiery, March 1929

But how many pairs of stockings a woman is prepared to buy without wondering what she should pay for them? Prices were already falling by the late 1920s when talk of a hosiery price war gripped the industry. Sure enough, as Figure 7 shows, prices fell sharply and then plummeted in the wake of the stock market crash in October 1929. As a result, prices of silk stockings fell to less than a third of what they had been in the early 1920s. And, yet, when we look at Figure 7, we see something striking. Production rose all through the 1920s and even when silk hosiery prices came crashing down, production did not collapse with them.

Figure 7 Production & Prices of Full-Fashioned Hosiery

We can see what happened if we look at the census data again in Table 2. Wages as a share of value produced, already high by the automobile industry's standard in the early 1920s, were much higher by 1929. But what might seem to have spelt disaster for silk hosiery enterprises did not lead to a general profit squeeze. Why? Their material costs dropped even faster than the price of silk stockings so that the 1920s was an age of prosperity for US silk hosiery. Even as the lustre wore off, and the price of silk stockings collapsed, the last column of Table 2 remained surprisingly buoyant. Many producers of silk stockings found themselves in difficulty, of course, but the basic foundations of the industry's factory profits did not implode.

Hosiery workers experienced a sharp drop in their average wages, similar to what autoworkers faced, but the vast majority of them held on to their jobs in contrast to their counterparts. It was only from the mid-1930s, when hosiery workers demanded higher wages, given rising production and even prices, that we see the industry's economics coming undone. The problem by then was that material costs had been squeezed so much that they no longer acted as the buffer for rising wage costs that they had been in the 1920s.

The account of silk hosiery offered herein is so stylized that it ignores many features of the turbulent history of this fascinating industry. But it gives us just enough to see that silk stockings may have something to teach us about automobiles. Automakers, like silk hosiery companies,

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[135] Philip Scranton, *Figured Tapestry : Production, markets, and power in Philadelphia textiles, 1885-1941*, Cambridge University Press, 1989 ; Sharon McConnell-Sidorick, *Silk Stockings and Socialism: Philadelphia's Radical Hosiery Workers from the Jazz Age to the New Deal*, University of North Carolina Press, 2017.

faced softening demand for their products from the mid-1920s with a strong shift towards cheaper cars as the decade unfolded. However, declining costs, especially material costs, meant that average industry profits continued to rise. So automakers ramped up production, so much indeed that automakers themselves knew the increase could not be sustained. When production broke through all previous records in late 1928 and early 1929, there was talk of a pending collapse in sales and carmakers started to cut production schedules.

Table 2 Census Statistics for U.S. Silk Hosiery Industry, 1923-1935

So why did the car industry fail to react the way the silk hosiery industry did? Why didn't they slash the prices of automobiles instead of production? The answer, Mitchell's analysis suggests, may have something to do with the dynamics of their respective material costs. Reading the research of Debin Ma and Giovanni Federico on the global silk industry makes it clear that the US sourced most of its raw silk from Japan.[136] We have some idea too of how Japan's complex of traders and spinners and cocoon and mulberry tree farmers sold raw silk at such rapidly diminishing prices, as well as some sense of what it cost them. And then the question we should ask is whether the automobile industry could expect its suppliers to deliver the materials it needed at such rapidly diminishing prices?

I would suggest that they did not and thought it better to slash production, and material costs with it, to sustain their profits. That strategy worked out well for some, less so for others as we know from Timothy Bresnahan and Dan Raff, but it brought problems for the future.[137] Even to sell what they sold, car companies felt pressure to offer more, so they literally increased the heft of their cars, making their cost structures more vulnerable to changes in material costs. However, it is not the answer that's important here but the question, and it is a question that has resonance across all kinds of US industries in the 1920s and 1930s. That question points us in directions we have not adequately explored in research on the Great Depression, which suggests the following conclusion. If we want to dismiss the Great Depression as a real problem, we can do that, but only after we have made a much more serious effort to think about it, using any and all theories at our disposal.

4. Conclusion

This article's discussion of theory and history raises serious questions for economic history about its relationship to economics. The recent attention to *A Monetary History* in the global financial crisis gave some economic historians a sense that their time had come after years of struggling for attention from economists. Ran Abramitzky urged other economic historians to make the most of the opportunity by conveying history to economists using concepts and methodologies they understand. We heard more cautious voices with Barry Eichengreen speaking of the "misuses" of history and suggesting that what we retain about the Depression reflects what was acceptable and unacceptable to the mainstream of the US economics profession.[138]

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136 Debin Ma, "The Modern Silk Road: The Global Raw-Silk Market, 1850-1930", *Journal of Economic History*, 56(2), (Jun., 1996), 330-355; Giovanni Federico, *An economic history of the silk industry, 1830-1930*, Cambridge University Press, 1997.

137 Timothy Bresnahan and Daniel Raff, "Intra-Industry Heterogeneity and the Great Depression: The American Motor Vehicles Industry, 1929-1935", *Journal of Economic History*, 51(2), (Jun., 1991), 317-331.

138 "This view [the instability of financial markets] existed as well, but it resided mainly on the fringes of economics. Why it remained out of the mainstream is worth pondering further. If such powerful lessons for how policymakers should respond to a crisis were remembered, how could other equally powerful lessons about what could cause it be forgotten?" (Barry Eichengreen, *Hall of Mirrors: The Great Depression, the Great Recession, and the Uses -- and Misuses -- of History*, Oxford University Press, 2015, 380).

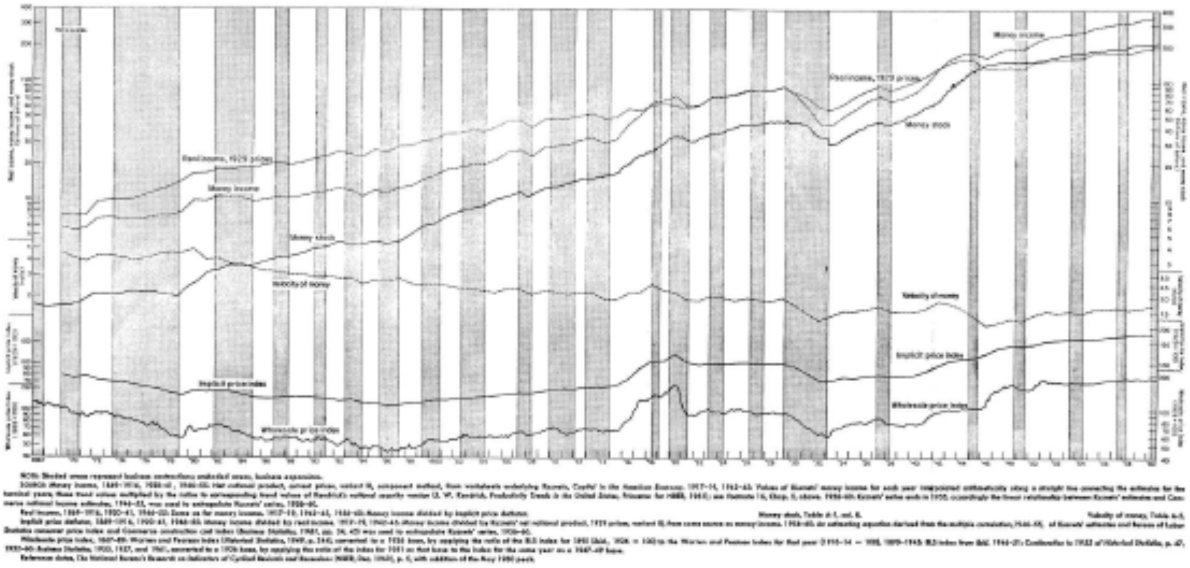
Some have gone further still to suggest that the most widely invoked lessons of the Great Depression have a stronger ideological than historical basis. Bradford DeLong observed that: "[a]dmitting that the monetarist cure was inadequate would have required mainstream economists to swim against the neoliberal currents of our age. It would have required acknowledging that the causes of the Great Depression ran much deeper than a technocratic failure to manage the money supply properly." Specifically, he suggested, that it would demand recognition "that the failure of markets can sometimes be a greater danger than the inefficiency of governments."[139]

The story I have recounted in this article has a good deal of politics in it. If I have chosen not to make politics the central theme of my address, the reason can be found in DeLong's words. Even economists who disagree about politics often agree that any alternative to the Friedman and Schwartz story must be about markets. The failure of markets, to be sure, but markets all the same. The story we might want to tell, I have suggested, is about capitalism. It would allow us to see patterns we did not see before and it might suggest a different story about the Great Depression. If it does, that story will not be about the economic failures of capitalism, it will be about how capitalism worked in the Great Depression. And because it worked, production and employment might be maintained or cut, and wages and prices might move with or against each other. The institutional foundations of capitalism might have come under threat in in the United States if the government had not stepped in to control the damage it wrought. But that damage was part and parcel of any society's Faustian deal with capitalism as a strikingly mild-mannered heretic pointed out more than a century ago.[140]

[139] Bradford DeLong, "The Monetarist Mistake", Mar 30, 2015, Project Syndicate.
[140] "Where money economy dominates, natural resources are not developed, mechanical equipment is not provided, industrial skill is not exercised, unless conditions are such as to promise a money profit to those who direct production." (Mitchell, 1913, 21-22).

Figures & Tables

Figure 1 Money Stock, Income, Prices, and Velocity, in Reference Cycle Expansions and Contractions, 1867-1960



Source: Friedman and Schwartz, 1963, between 678 & 679.

Figure 2 Friedman & Schwartz's explanatory schema

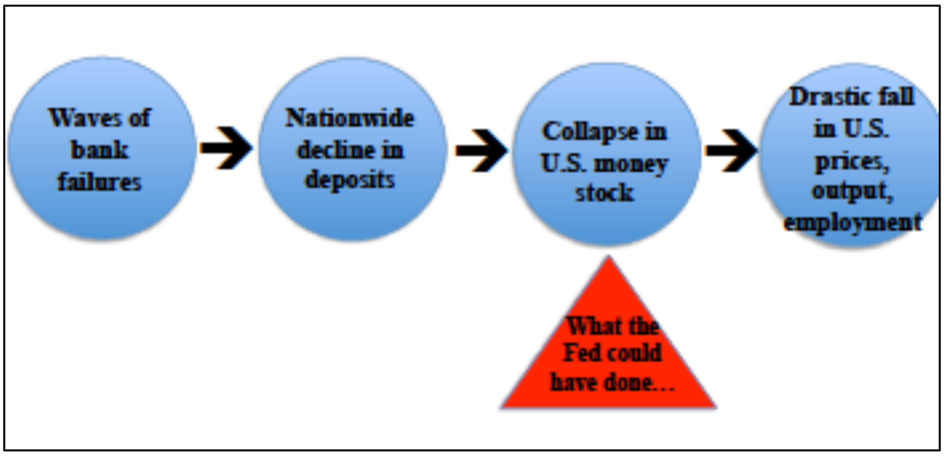
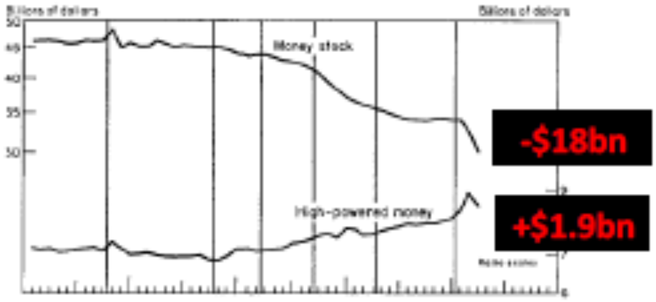
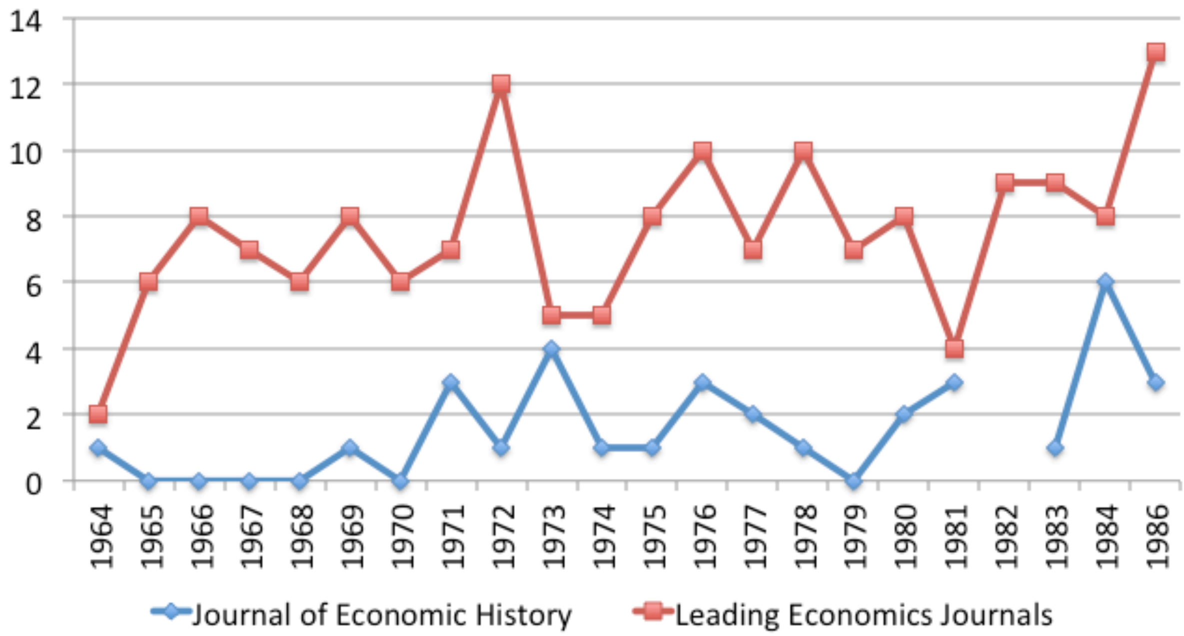


Figure 3 The Stock of Money and its Proximate Determinants, monthly, 1929-March 1933



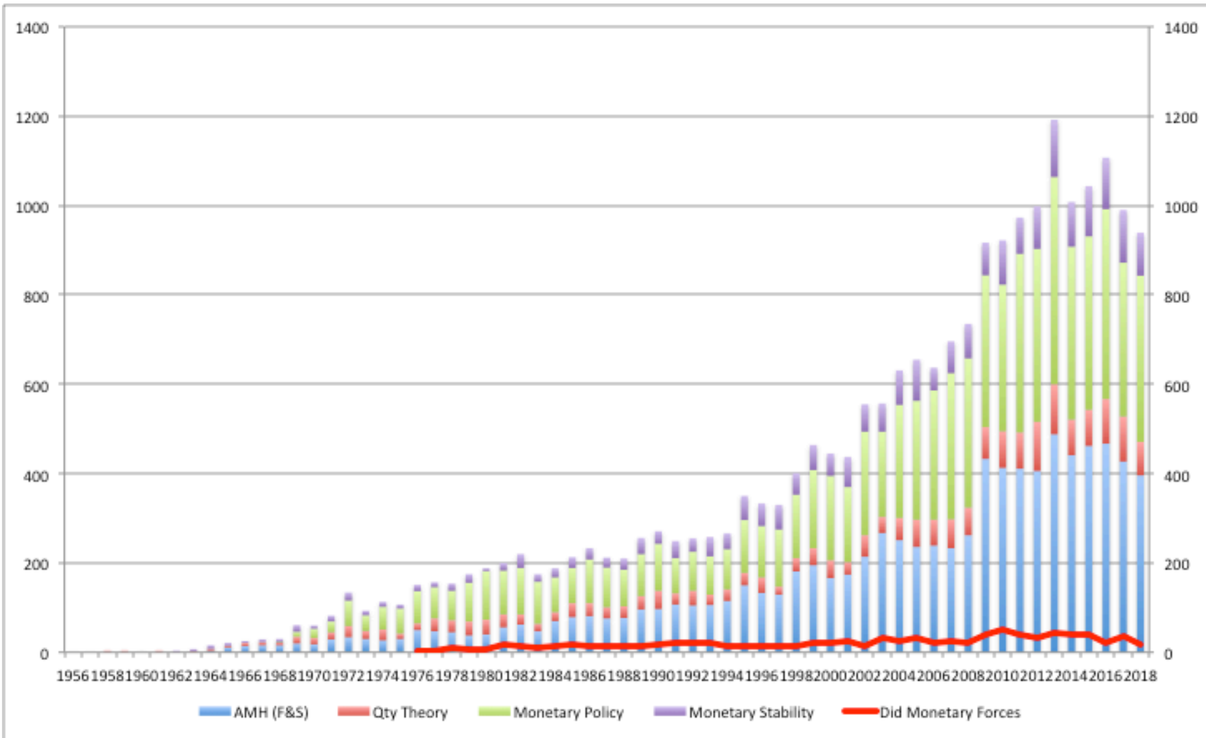
Source: Friedman & Schwartz, 1963, Chart 31, 333.

Figure 4 Citations to A Monetary History, 1963-1986



Note: Leading economics journals include JPE, AER, JME, JMCB, RECSTAT, JF, EJ, QJE
Source: author's analysis based on Michael Bordo, ed., *Money, history, and international finance: essays in honor of Anna Schwartz*, NBER, 1989, Table 1.1, 17.

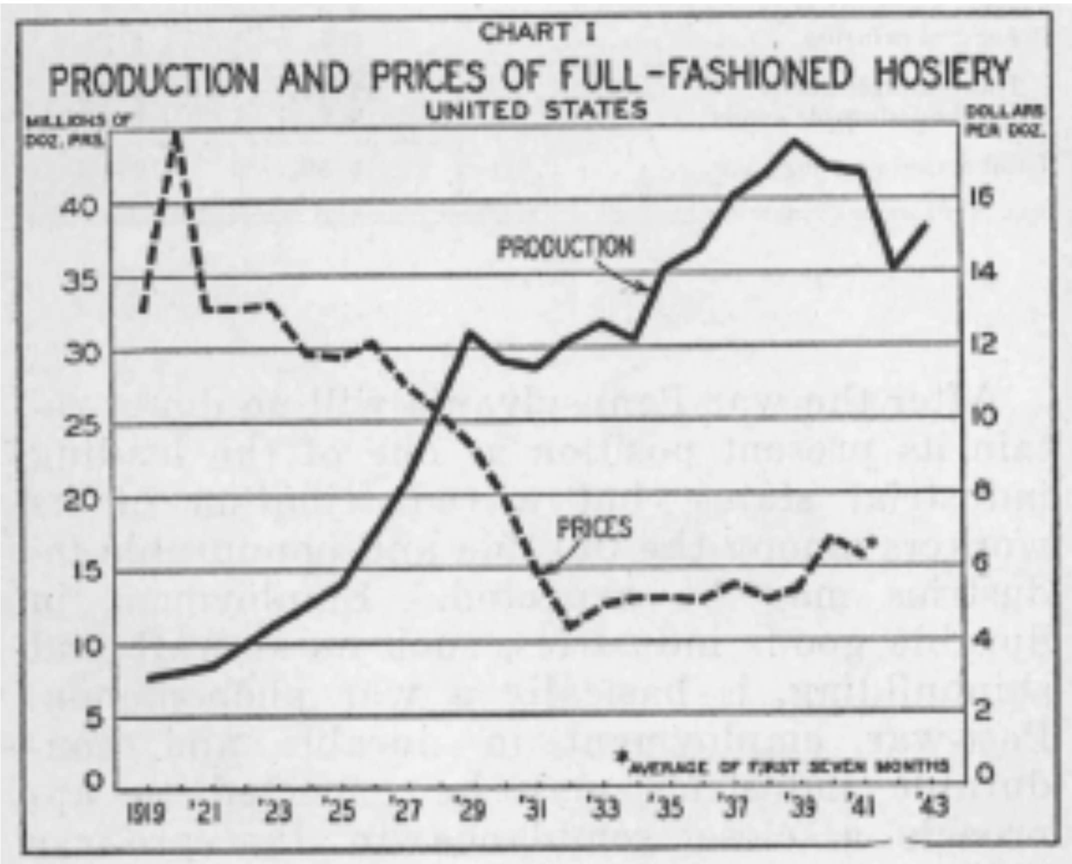
Figure 5 Citations to Friedman's most-influential contributions to monetarism versus Temin's *Did Monetary Forces Cause the Great Depression?*, 1956-2018



Source: author's analysis based on Google Scholar, consulted on June 30, 2019.

Figure 6 Advertisement for Silk Hosiery, March 1929

Figure 7 Production and Prices of Full-Fashioned Hosiery, 1919-1943



Source: Federal Reserve Bank of Philadelphia, *Business Review*, 1946.

Table 1 Census Statistics for the U.S. Motor Vehicle Industry, 1923-1937

Year	% of value of product		
	Wages	Cost of materials, fuel, purchased electricity	Other expenses & profit
1923	12.9%	67.9%	19.3%
1925	10.7%	65.9%	23.4%
1927	11.3%	66.3%	22.4%
1929	9.8%	64.5%	25.6%
1931	10.0%	66.6%	23.4%
1933	9.5%	70.0%	20.5%
1935	9.1%	75.9%	15.1%
1937	10.2%	77.3%	12.5%

Note: salaries are not reported separately for early years so they are included in other expenses and profit. In 1937, they amounted to $48,673,258 or 1.6 per cent of value of product
Source : U.S. Department of Commerce, *Census of Manufactures*, various years

Table 2 Census Statistics for U.S. Silk Hosiery Industry, 1923-1935

Year	% of value of product		
	Wages	Materials, fuel, purchased electricity	Other expenses & profit
1923	20.2%	57.0%	22.8%
1925	22.2%	54.2%	23.7%
1927	25.1%	50.3%	24.6%
1929	26.5%	47.0%	26.5%
1931	28.7%	44.9%	26.3%
1933	31.5%	43.1%	25.4%
1935	38.5%	41.8%	19.7%

Source : U.S. Department of Commerce, *Biennial Census of Manufactures*, various years