



## ORIGINAL ARTICLE

# History as heresy: Unlearning the lessons of economic orthodoxy

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[Correction added on 14 April 2022, after  
first online publication: Funding state-  
ment has been added.]

**Abstract**

Unprecedented liquidity injections by central bankers have gained legitimacy in recent years to stave off economic crisis and enjoy strong support from prominent economists and economic historians. Such radical action by central bankers is underpinned by a remarkable agreement on a specific interpretation of the Great Depression of the 1930s in the US, an interpretation proposed by Milton Friedman and Anna Schwartz in *A monetary history of the United States* (1963). This article explores the origins, the limits, and the influence of *A monetary history's* interpretation of the Great Depression for the insights it offers on theory and history in the study of economic life. The book was inspired by Wesley Clair Mitchell's mobilization of historical research to insist on the inherent instability of capitalism. Friedman and Schwartz exploited the heretical potential of historical research to dismiss any claim that the crisis reflected a systemic dysfunction of the US economy. Now that their interpretation has become our orthodoxy, this article shows how we can develop the fertile link between history and heresy to open up new lines of research on the causes of the greatest crisis in the history of capitalism.

**KEYWORDS**

business cycles, capitalism, Friedman and Schwartz, Great Depression, profit, theory and history, Wesley Clair Mitchell

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On 26 March 2020, in the face of the coronavirus 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.<sup>1</sup> Only three months later, the Fed and other rich countries' central banks had injected trillions of dollars of liquidity into the global economy. Such radical action by central banks has its critics on the right and the left of the political spectrum. Just as striking, however, is that many prominent economists and economic historians have rallied in support of central banks' bold actions to cast massive liquidity injections as a *sine qua non* in responding to economic crisis.<sup>2</sup> 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 US, 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'.<sup>3</sup> Bernanke's statement alluded to the widespread consensus about the claim advanced in *A monetary history* that the Federal Reserve System was responsible for allowing an ordinary economic downturn to turn into the Great Depression of the 1930s. Friedman and Schwartz argued that when a massive banking 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.<sup>4</sup> By the end of the twentieth century, their interpretation had become sufficiently dominant in economics and economic history to qualify as the orthodoxy of the Great Depression in the US.

In the early twenty-first century, that 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, even disreputable, 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 peacetime monetary interventions since the beginning of the twentieth century;<sup>5</sup> and that was before central banks responded to the coronavirus crisis with an even more spectacular flood of liquidity.

Thus, a specific interpretation of a crisis that occurred nearly a century ago became an economic orthodoxy of the Great Depression, before being mobilized to justify policies for dealing with crises today. In this article, I explore the origins, the limits, and the influence of *A monetary history's* interpretation of the Great Depression for the insights it offers on the relationship between theory and history in the study of economic life. The book owes an intellectual debt to US

<sup>1</sup> Powell on whether the Fed has enough firepower: "We're not going to run out of ammunition", *CNBC News*, 26 March 2020, <https://www.cnbc.com/2020/03/26/fed-chair-jerome-powell-on-when-to-restart-the-economy-we-would-tend-to-listen-to-the-experts.html> (accessed on 21 Oct. 2021).

<sup>2</sup> Barry Eichengreen's admonishing of critics of quantitative easing a few months before the pandemic is illustrative of this attitude: B. Eichengreen, 'Critics of quantitative easing should consider the alternative', *Guardian*, 11 June 2019, <https://www.theguardian.com/business/2019/jun/11/quantitative-easing-qe-recession> (accessed on 25 Aug. 2021).

<sup>3</sup> Bernanke, 'On Milton Friedman's ninetieth birthday'.

<sup>4</sup> Friedman and Schwartz, *A monetary history* (hereafter *AMH*).

<sup>5</sup> Piketty, *Capital and ideology*, fig. 13.14, p. 701.



economist Wesley Clair Mitchell, and in the first section I show how Mitchell mobilized historical analysis to develop an economic theory of business cycles that broke with orthodox thinking in its insistence on the inherent instability of capitalism. Friedman and Schwartz exploited the heretical potential of historical research in *A monetary history* but they did so, as I illustrate, to generate a provocative interpretation of the Great Depression in the US that challenged any claim that it reflected a systemic dysfunction of the US economy. In section II, I explain how the monetarist story that Friedman and Schwartz constructed in *A monetary history*, despite the limits of its analysis and some effort to challenge its interpretation, became the economic orthodoxy of the Great Depression in the US. Now that *A monetary history*'s interpretation of the Great Depression has become our orthodoxy, I suggest in section III that we develop the fertile link between history and heresy to understand our economic past better, returning to Mitchell's work for inspiration about the rich opportunities to be exploited in historical research on the Great Depression.

## I | 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*, but the US was not always so prominent in discussions of crises and cycles in political economy. If there is one economist to whom we can point as contributing to a significant increase in US research on business cycles, it is Wesley Clair Mitchell. As *A monetary history*'s original cover suggests, it was a contribution to the research programme of the National Bureau of Economic Research (NBER) on business cycles, initiated in the early 1920s by Wesley Mitchell and guided by him for decades. By focusing on the distinctive integration of theory and history that characterized Mitchell's research on business cycles, and the successes and setbacks of his approach at the NBER, we can understand the continuities and ruptures in the way *A monetary history* brought theory and history together in its analysis of the Great Depression in the US.

Wesley Mitchell did more than any other US economist to shape the study of business cycles from the publication of *Business cycles* in 1913 until his death in 1948 as he struggled to complete a new synthesis. His influence extended far beyond his own country, with the *Encyclopaedia Britannica* casting him as 'the world's foremost authority of his day on business cycles'.<sup>6</sup> Mitchell began his career at the University of Chicago's recently established Department of Economics, where he worked with J. Laurence Laughlin, a prominent monetary economist, and benefited from the influence of scholars such as John Dewey and Thorstein Veblen.

Veblen's criticisms of orthodox economic theory were especially important in inspiring Mitchell and in stimulating his efforts to contribute to the development of an alternative to it.<sup>7</sup> Veblen's influence can be seen in Mitchell's insistence that 'the older type of reasoning in economics' was unhelpful for understanding the flow of economic life and his interest in a theme that had been 'sadly slurred over' in economics: 'the relation between business and industry, between making money and making goods, between the pecuniary and the technological phases of economic life'.<sup>8</sup> That relation had animated Veblen, and other economic heretics including Sismondi and Marx,

<sup>6</sup> Editors of *Encyclopaedia Britannica*, 'Wesley C. Mitchell'.

<sup>7</sup> Veblen was a guiding light for the distinctive US school of institutional economics. Veblen, John Commons, and Mitchell are sometimes seen as the founders of this school although there is some debate on the matter; Hodgson, *Evolution*; Rutherford, *Institutionalist movement*; Biddle, 'Sources and extent'; for the debate, see Kaufman, 'Origins'.

<sup>8</sup> Mitchell, 'Quantitative analysis', pp. 5, 7.



and it was to animate Mitchell in his research on business cycles.<sup>9</sup> Mitchell criticized economists who failed to see that 'economic rationality was largely the product of the money economy', a term he used to evoke a distinctive institutional form of economic activity, one whose 'essential feature' is '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'.<sup>10</sup>

Mitchell emphasized that the pecuniary logic that dominated a money economy might well conflict with the material requirements of human well-being: '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'.<sup>11</sup> For Mitchell, therefore, business cycles occurred due to the 'precarious dependence' of material well-being on an economy organized for profit-seeking and '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'.<sup>12</sup> The causes of cycles and crises, he insisted, were to be found in the dynamics of enterprises' profit-making and, specifically, in 'the factors which control present and prospective profits, together with present and prospective ability to meet financial obligations'.<sup>13</sup> In this regard, Mitchell looked to the 'system of prices' as the crucial determinant of profits, 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'.<sup>14</sup>

At the core of Mitchell's theory of business cycles, therefore, was the evolving relationship between the system of prices and the money profits of enterprise. Noting that 'the recurrent phases presented by economic activity, wherever it is dominated by the quest of profits, grow out of and grow into each other', Mitchell 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'.<sup>15</sup> 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'.<sup>16</sup> Here again we see Veblen's influence since, as Milton Friedman later explained:

<sup>9</sup> 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, during a period that has been characterized as 'pluralist', raises questions about the significance of terms like 'orthodox' and 'heterodox' at the time; see, notably, [Morgan and Rutherford](#), *Interwar pluralism*, p. 3. However, Mitchell explicitly used the expression 'orthodox economic theory'; see, for example, [Mitchell](#), 'Prospects', p. 4. More substantively, he was quite clear about his dissatisfaction with 'our whole apparatus of reasoning on the basis of utilities and disutilities, or motives, or choices, in the individual economy' (*ibid.*; [Mitchell](#), 'Quantitative analysis', p. 5) and his enthusiasm for institutional economics, which he cast as 'an unorthodox type of economic theory' inspired by economists such as Sismondi, Marx, and Veblen, which 'deals with a range of problems undreamt of in the philosophy of value and distribution'; [Mitchell](#), 'Prospects', pp. 17–19, cited at p. 17. It is hardly a surprise, therefore, that Mitchell's work on business cycles was read as heterodox by his contemporaries; [Hammond](#), *Theory and measurement*, p. 12.

<sup>10</sup> [Mitchell](#), *Business cycles*, p. 21. There is an echo here of another economist who Mitchell admired—Karl Marx—and his argument that the circuit M-C-M' replaces C-M-C with the emergence of a capitalist mode of production.

<sup>11</sup> *Ibid.*, pp. 21–2.

<sup>12</sup> *Ibid.*, pp. 585–6.

<sup>13</sup> *Ibid.*, p. 26.

<sup>14</sup> *Ibid.*, p. 31.

<sup>15</sup> *Ibid.*, p. 449.

<sup>16</sup> *Ibid.*, p. 450.



‘[o]ne 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”’.<sup>17</sup>

That ‘problem’ was to become one of Mitchell’s preoccupations in building his theory of business cycles. As he put it: ‘[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’.<sup>18</sup> For all the historical 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’.<sup>19</sup> Crucial in this regard were systemic changes in price relations that he suggested were found in every period of revival, prosperity, crisis, or depression.

For Mitchell, the theoretical task of understanding business cycles needed to be integrated with the empirical challenge of studying the cycles as they unfolded over time. Identifying the different phases of cycles in the past was vital to studying the changing relationships between prices and profits during these phases. Historians offered little help in this regard since they were only slowly turning to the systematic study of economic phenomena.<sup>20</sup> So Mitchell relied on the contemporary business and financial press to construct his own historical ‘annals’ of business cycles in order to characterize their temporal rhythm.<sup>21</sup> Then, he turned to statistical analysis to uncover the relations among various types of prices, and their implications for profits, during different phases of the cycle, seeing his prolific statistical work as another vantage point from which to observe the unfolding of an historical process. As he observed to fellow economist, Irving Fisher: ‘[w]hen you speak of periods of equilibrium, are you not referring again to imaginary conditions instead of the historical conditions which our statistics reflect?’.<sup>22</sup>

The statistical task that Mitchell assigned himself was enormous since it meant collecting extensive data on wages, the prices of raw materials and processed inputs, and more, with only limited help from official statistics.<sup>23</sup> 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’.<sup>24</sup> But he was not easily daunted and embraced these difficulties, pioneering on many fronts in the compilation, presentation, and analysis of economic data.<sup>25</sup>

Notwithstanding all of his empirical effort, Mitchell kept his eye fixed on the theoretical questions with which he began. In the final third of his book, he spent 150 pages presenting what he called an ‘analytic description of the complicated processes by which seasons of business

<sup>17</sup> Friedman, ‘Mitchell as an economic theorist’.

<sup>18</sup> Mitchell, *Business cycles*, p. 449.

<sup>19</sup> *Ibid.*, p. 450.

<sup>20</sup> Lamoreaux, ‘Beyond the old and the new’, p. 36.

<sup>21</sup> 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, *Business cycles*, ch. III, pp. 44–87.

<sup>22</sup> Columbia Rare Book and Manuscript Library, New York, Papers of Wesley Clair Mitchell, Wesley Clair Mitchell to Irving Fisher, 13 Nov. 1926; for a comparison between the two economists, see Friedman, *Fortune tellers*, pp. 174–5.

<sup>23</sup> For the history of statistical thinking and collection, see Desrosières, *La politique des grands nombres*; Porter, *Rise*; Tooze, *Statistics*.

<sup>24</sup> Mitchell, *Business cycles*, p. 422.

<sup>25</sup> For the importance of Mitchell’s quantitative empirical research, see Morgan, *Econometric ideas*, pp. 44–56. His historical annals, in contrast, have not received the attention they deserve at a time when research in economic history in the US was in its infancy; Lamoreaux, ‘Beyond the old and the new’, pp. 35–9.



prosperity, crisis, depression, and revival come about in the modern world'.<sup>26</sup> There he laid out what happened to prices and profits in different phases of the business cycle, with his insights in this regard being seen, for example, as an antecedent of the 'profit-squeeze' theories of recessions that proliferated from the late 1960s.<sup>27</sup>

Given Mitchell's active role in establishing the NBER as a non-partisan centre for economic research in 1920, and his role as its research director, it is hardly surprising that the NBER soon launched a major programme on the study of business cycles.<sup>28</sup> Mitchell had laid out a distinctive methodological approach to business cycles, based on a close integration of theory, annals, and statistics, in his 1913 book, *Business cycles*. However, as the US emerged from its depression of 1920–1, he believed his book needed to be updated, but thought the task too great to be undertaken alone.<sup>29</sup> So Mitchell looked to the NBER for assistance, building its research on business cycles on the same methodological pillars as his 1913 book.

First, Willard Thorp used contemporary press reports to generate historical annals of business conditions extending back to 1790 for a variety of countries.<sup>30</sup> Second, a massive statistical programme was undertaken, drawing in economists including Ralph Epstein, Solomon Fabricant, Simon Kuznets, and Frederick Mills to compile hundreds of price series and unprecedented data on profits, as well as the earliest estimates of national income for the US. The programme was to continue over decades, broadening as it evolved to include all aspects of the US economy, and its incorporation of monetary factors explains why the NBER commissioned the study that Friedman and Schwartz undertook from the late 1940s.<sup>31</sup> Third, there was the theoretical analysis of business cycles, which was always understood to be Mitchell's task, but 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'.<sup>32</sup>

There is no great surprise here since understanding the theoretical implications of new evidence on cycles was a major challenge as it flowed out of the NBER, other US research centres, and the business cycle institutes mushrooming around the world.<sup>33</sup> The challenge attracted increasing numbers of creative scholars besides Mitchell, including Nikolai Kondratiev, Friedrich von Hayek, and Michał Kalecki, during a golden age for theoretical and empirical research on business cycles. 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 only worse to come.

That the onset of Depression would have a dramatic impact on the analysis of business cycles 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, made it harder to sustain the long-standing belief, still expressed by prominent economists, that cycles were temporary aberrations

<sup>26</sup> Mitchell, *Business cycles*, p. vii.

<sup>27</sup> Sherman, 'Profit-squeeze', pp. 248–52.

<sup>28</sup> Mitchell, *Annual report*, 4.

<sup>29</sup> Mitchell, *Problem*, p. ix.

<sup>30</sup> Thorp, *Business annals*.

<sup>31</sup> 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, *Theory and measurement*, p. 63, noted: '[h]e stated that he and Schwartz were picking up where Mitchell left off in Chapter VI of his 1913 *Business Cycles*'.

<sup>32</sup> Mitchell, *Problem*, p. ix.

<sup>33</sup> Morgan, *Econometric ideas*, pp. 64–8; Slobodian, *Globalists*, pp. 55–73.





that would be corrected by the normal functioning of a market economy.<sup>34</sup> In stimulating research on cycles, the Depression reinforced existing work, including Mitchell's programme at the NBER and Joseph Schumpeter's study of the cumulative dynamics of business cycles at Harvard.<sup>35</sup> The crisis fostered new ideas too, such as theories of cycles that emphasized the implications of the increased prevalence of large enterprises. Still, it was the novel perspective proposed by John Maynard Keynes that stimulated particular interest from the 1930s, with his emphasis on the economic importance of uncertainty and, as a consequence, sentiment or animal spirits in shaping economic behaviour.<sup>36</sup>

These conceptual fault lines in the economic analysis of cycles made it difficult to predict how research would develop in the decades after the Depression. Methodological fault lines further complicated that task. Keynes can be seen as maintaining the tradition of deductive reasoning in economics and there was continuity too in the integration of theory and history by Mitchell as well as Schumpeter.<sup>37</sup> 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 Clavin explained, the Depression induced the League of Nations to assume a prominent role in promoting research on business cycles.<sup>38</sup> The League became a sponsor of 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 US economy, which was endorsed by the League.<sup>39</sup> During the Second World War, the centre of gravity for this pioneering econometric work shifted from Europe to the US, especially to the Cowles Commission, under the intellectual leadership of European economists like Jacob Marschak and Tinbergen's protégé, Tjalling Koopmans.<sup>40</sup>

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'.<sup>41</sup> Keynes emphasized 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 relationships among them could be specified in simple mathematical terms, and if conditions observed in the past would persist into the future.

But econometricians of business cycles had another critic, closer to the Cowles Commission's home in the US. In a review of Tinbergen's research on the US, 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

<sup>34</sup> See, for example, Fisher, 'Debt-deflation theory'.

<sup>35</sup> McCraw, 'Schumpeter's "business cycles"'.

<sup>36</sup> Keynes, *General theory*.

<sup>37</sup> For a discussion of Schumpeter's methodological approach, see Lazonick, 'Integration'; McCraw, *Prophet*.

<sup>38</sup> 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 rich seam of intellectual engagement by the League as to the cause of and possible remedies for economic depression over the next fourteen years'; Clavin, *Securing*, p. 73.

<sup>39</sup> Morgan, *Econometric ideas*, pp. 101–32.

<sup>40</sup> 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, 'Friedman'.

<sup>41</sup> Keynes, 'Tinbergen's method', p. 566.



economy were chosen because they fit well with the economic data at his disposal. However, the goodness of their fit created 'no presumption that the relationships they describe will hold in the future'. Friedman cited Mitchell on this point as well as his conclusion: such models represented, rather than explained, statistical history and their explanatory power could only be judged when enough time had passed to generate new data.<sup>42</sup>

Given these methodological differences, a clash of titans was in the offing. The demands of wartime planning, and then postwar 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 into new macro-models built on structural econometric equations. Still, as Keynes's own critique had highlighted, these models could incorporate only causal factors that were measurable. Since uncertainty and sentiment resisted quantification, they were abandoned by the wayside, prompting Joan Robinson's subsequent evocation of a 'bastard Keynesianism'.<sup>43</sup>

The main rival for structural econometrics as an empirical research programme for studying business cycles in the postwar years was the integration of theory and history that Mitchell and the NBER exemplified. And so the clash of 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 and there is no doubt that what ensued was victory for the structural econometric programme and defeat for Mitchell's alternative.<sup>44</sup> However, the question of why that happened does not lend itself to easy answers.

The story is often told as if the decline in Mitchell's influence in the study of business cycles were deserved. Following Koopmans's lacerating critique in 1947, Mitchell and the NBER are characterized as offering measurement but no theory, and measurement that was distinctive for, as Koopmans put it, 'the pedestrian character of the statistical devices'.<sup>45</sup> There are compelling reasons to challenge this interpretation for what it implies about the substance of both Mitchell's methodological approach and the econometrics of business cycles.<sup>46</sup> But there is an even more flagrant challenge to the conventional explanation of the *dénouement* of the battle of ideas about cycles and it is *A monetary history of the United States*.

In his book on Milton Friedman's economics, J. Daniel Hammond offered a rich analysis of the inspiration that Friedman drew from Mitchell's work on business cycles. Building on Hammond's insights in his article 'On the origins of "*A monetary history*"', Hugh Rockoff suggested: '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 long accurate time series of monthly data and the analysis of the effects of specific variables on the business cycle'.<sup>47</sup> In fact, Mitchell's methodological influence on *A monetary history* went much deeper since the book was based on a combination of statistics, annals, and theory that bears an uncanny resemblance to the work of Mitchell. Yet the distance that Friedman and Schwartz

<sup>42</sup> Friedman, 'Review', pp. 659–60.

<sup>43</sup> Robinson, 'Keynesian Revolution?'.

<sup>44</sup> National Bureau of Economic Research, *Conference*.

<sup>45</sup> Koopmans, 'Measurement without theory'.

<sup>46</sup> Hammond, *Theory and measurement*, pp. 5–25; Sherman, 'Business cycle theory'; Mirowski, 'Cowles changes allegiance'; Orozco-Espinel, 'L'économie'.

<sup>47</sup> Rockoff, 'Origins', p. 1.





marked with respect to Mitchell's work is just as significant and much greater than Rockoff allows.<sup>48</sup> As we shall see, Friedman and Schwartz employed their historical research to heretical effect to challenge not only what Mitchell believed, but also what many 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 business-cycle titans.<sup>49</sup> In a lengthy eulogy for Mitchell in 1950, Friedman went further, offering a sympathetic and insightful review of the late economist's collected works, and an explicit refutation of Koopmans's claim that Mitchell's work on business cycles was atheoretical. 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'.<sup>50</sup>

Mitchell 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'.<sup>51</sup> However, Burns thought highly of Friedman and commissioned him in 1948 as director of an NBER study of monetary factors in business cycles, assigning NBER researcher Anna Jacobson Schwartz to work with him.

Schwartz brought valuable experience of her own to the NBER monetary project. As an undergraduate at Barnard College, she had been drawn to monetary and financial history by one of her teachers, Archie Gayer.<sup>52</sup> Gayer had written his doctoral dissertation at Oxford on industrial fluctuations and unemployment in Britain in the early nineteenth century but wanted to undertake a more ambitious study. He secured a research grant, allowing him to hire Schwartz in 1936 and Walter Rostow a few years later in 1939. Despite their youth—Schwartz joined the project at 21 years old and Rostow at 23 years old—they were to play important roles in bringing Gayer's project to fruition and they were listed as his co-authors of *The growth and fluctuation of the British economy, 1790–1850*.<sup>53</sup>

The book's subtitle—'a historical, statistical, and theoretical study of Britain's economic development'—hints at another important influence. In his 1941 preface, Gayer was effusive in his acknowledgement of Mitchell for 'his unfailing encouragement and interest in the study, and

<sup>48</sup> Rockoff focused on the similarities and differences in the analysis of money between Mitchell's *Business cycles* and *A monetary history*.

<sup>49</sup> Stapleford, 'Positive economics'; Rockoff, 'Origins'.

<sup>50</sup> Friedman, 'Mitchell as an economic theorist'. Friedman acknowledged that his mathematical model was not intended 'as a version of the theory that Mitchell would have accepted as his own'; *ibid.*, p. 490.

<sup>51</sup> Wesley Clair Mitchell to Arthur Burns, 27 Aug. 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/> (accessed on 25 Aug. 2021).

<sup>52</sup> White, 'Schwartz', p. 78.

<sup>53</sup> The book manuscript was completed in 1941 but published only in 1953 shortly after Gayer's untimely death in a car accident.



for his consistently helpful advice and kindly guidance'.<sup>54</sup> It was Mitchell who had helped Gayer to secure research funding and he was actively involved in the study as it progressed. 'Mitchell certainly was an influence', Schwartz explained, noting that his interest 'influenced our adoption of the NBER method of cyclical analysis' and that '[p]eriodically, we would meet with him to report on what we were doing'. When asked about her early sources of inspiration, Schwartz explained that 'Mitchell's integrity and commitment to research that was thorough made a lasting impression on me. What [Arthur] Burns achieved was indoctrinating me to insist on checking not only numerical data but every statement, in a manuscript'.<sup>55</sup> It is no surprise, therefore, that Anna Schwartz would join the NBER in 1941, when Gayer's project came to an end, nor that Burns might think she would be a good influence on Friedman. The influence ended up being mutual, with Schwartz emphasizing that Friedman's impact on her was 'profound'.<sup>56</sup>

When Friedman and Schwartz began work on their project, they expected to produce a statistical study of monetary factors in the US business cycle but what emerged 15 years later as *A monetary history* was an 'analytical narrative' of post-Civil War monetary developments in the US.<sup>57</sup> The interpretation of the Great Depression that it offered, in a chapter that ran to more than 100 pages, was to transform our understanding of the greatest crisis that capitalism has ever experienced. And that had a great deal to do with the way the book both built on and challenged the economics of Mitchell.

A first observation to be made about similarities to Mitchell can be gleaned from Friedman and Schwartz's use of the term 'analytical narrative', which clearly evokes Mitchell's notion of 'analytic description' or 'descriptive analysis' to refer to his explanation of business cycles.<sup>58</sup> A second similarity to Mitchell is striking in *A monetary history*'s heavy use of basic statistics and elementary methods, just the sort of 'pedestrian statistical techniques' that supposedly brought down Mitchell and the NBER in the domain of business cycles. 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.<sup>59</sup>

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 'analytical' to complement the 'narrative'? A friendly critic, Robert Lucas, characterized 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.<sup>60</sup> It is true that they sometimes seemed quite casual in their

<sup>54</sup> Gayer, 'Director's preface', p. xvi.

<sup>55</sup> White, 'Schwartz', p. 78.

<sup>56</sup> Ibid.

<sup>57</sup> Friedman and Schwartz, *AMH*, p. xxi.

<sup>58</sup> For economists to acknowledge that their explanations were a type of narrative was uncommon at the time. 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; Bates, Greif, Levi, Rosenthal, and Weingast, *Analytic narratives*. In economics, despite important contributions by scholars like Deirdre McCloskey (see, for example, 'Rhetoric'), it is still unusual for economists to speak of economic arguments as narratives. It is notable, therefore, that Robert Shiller chose 'Narrative economics' as the title of his recent presidential address to the American Economic Association. Shiller focuses mostly on other people's narratives but he does acknowledge that economists rely on narratives in their work; Shiller, 'Narrative economics'.

<sup>59</sup> See, notably, the discussion of 'the course of events' in Friedman and Schwartz, *AMH*, pp. 305–32, and 'the course of monetary policy', pp. 362–91.

<sup>60</sup> Lucas, 'Friedman'.



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 through the dynamics of prices. Friedman and Schwartz focused 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 did, most spectacularly, in the Great Depression.<sup>61</sup>

Already this tells us something important. Friedman and Schwartz conceived of the norm in capitalism as stability, as characterized 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. That is far from self-evident, as Friedman and Schwartz acknowledged, since causality in such a relationship could just as easily go the other way.<sup>62</sup> 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. They made no attempt to establish any correlations among different statistical series, to employ mathematical equations to capture structural relationships, or indeed to use econometrics of any kind. 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.<sup>63</sup>

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 due to the counterfactual history that they constructed:

<sup>61</sup> See Friedman and Schwartz, *AMH*, chart 62, insert between pp. 678–9.

<sup>62</sup> *Ibid.*, p. 686.

<sup>63</sup> *Ibid.*



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 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.<sup>64</sup>

I have highlighted their use of the conditional in italics to emphasize that they were building 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’.<sup>65</sup>

Certainly, good books are written based on counterfactuals but imaginative literary devices are usually employed to avoid writing ugly phrases like the italicized ones in the previous quotation. And there is nothing exceptional in them since the phrase ‘would have’ actually appeared 455 times in *A monetary history*. Whatever the literary qualities of that choice, it tells us that much of Friedman and Schwartz’s history was counterfactual history. 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—that the greatest crisis that capitalism had ever confronted was allowed to go as deep and to endure as long as it did because of the failure of government.

That their interpretation was ideologically loaded was blatantly clear in a book that Friedman had published only a year earlier.<sup>66</sup> 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 either the target or the weapon in his attack. Here we find the interpretation of the Great Depression that Friedman and Schwartz were to roll out in its scholarly glory one year later. 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’.<sup>67</sup>

## II | AN ECONOMIC ORTHODOXY OF AN UNORTHODOX PAST

When we turn to how *A monetary history* became a classic, we should recognize how unlikely that seems when we take stock of what its authors did. They proposed a novel interpretation of the Great Depression that focused on one variable—money—to explain why it was so deep and long. In doing so, they posited a role for money, and a potential significance for monetary policy, that was in stark opposition to the economic orthodoxy of their day. They built their interpretation on pedestrian statistical techniques and historical analysis that had been dismissed as old-fashioned by leading economists of cycles. And they were flagrant—at least Friedman was

<sup>64</sup> Ibid., p. 11.

<sup>65</sup> Ibid., p. 168.

<sup>66</sup> Friedman, *Capitalism and freedom*.

<sup>67</sup> Ibid., p. 50.



**FIGURE 1** Friedman and Schwartz's explanatory schema

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 US.

In laying out these gaps, I emphasize that they are not obscure holes in their account of Great Depression but obvious limitations of their evidence and arguments. To locate them, we can strip their interpretation down to its essential elements, as illustrated in figure 1. Banking failures played a crucial role in precipitating the US monetary mess but the authors took only three pages to mull over why these failures occurred.<sup>68</sup> There we find some of the most ambiguous musings in the whole chapter with Friedman and Schwartz alternating between the possibility of 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.<sup>69</sup> The unsatisfactory character of these pages surely reflects the fact, as Hammond explained, that they were added shortly before publication and only under pressure from inside the NBER.<sup>70</sup>

Failing to reach any clear conclusion on the causes of banking failures, Friedman and Schwartz downplayed the importance of understanding them, insisting it was the indirect, rather than direct, effects of bank failures that were decisive in bringing about collapse of the US money stock: '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'.<sup>71</sup> Here is the 'contagion of fear' that supposedly enveloped depositors across the US, but its discussion is characterized by homespun wisdoms like 'such contagion knows no geographical limits' and a few specific claims based on limited evidence. They tell us about the Bank of the United States,

<sup>68</sup> Friedman and Schwartz, *AMH*, p. 353.

<sup>69</sup> *Ibid.*, pp. 354–6.

<sup>70</sup> The pressure was exerted by Geoffrey Moore, the associate director of research at the NBER, who 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, *Theory and measurement*, pp. 78–83.

<sup>71</sup> Friedman and Schwartz, *AMH*, p. 356.



for example, suggesting that 'its name had led many at home and abroad to regard it somehow as an official bank'.<sup>72</sup> How many depositors, one wonders, but there is no evidence, not even a footnote to evidence, to tell us.<sup>73</sup>

Lacking such evidence, *A monetary history* relied on aggregate data on the amount of currency held by the public to suggest that 'the monetary character of the contraction changed dramatically' from October 1930 as the first banking panic broke.<sup>74</sup> Currency holdings by the public did increase from \$3.6 to \$3.8 billion between October and December 1930, but from a low base and remaining below the average level of \$3.9 billion from 1923 to 1929, which can hardly be seen as a dramatic change. From May 1931, in contrast, the public's currency holdings shot up to unprecedented levels, reaching \$4.9 billion by January 1932, but it is difficult to know how much of the increase was fuelled by depositors' panic about US banks' prospects.<sup>75</sup> Certainly, the timing suggests another possibility, since May 1931 marked the failure of the *Kreditanstalt*, Austria's largest private bank, and the onset of the European banking crisis. The result, as Friedman and Schwartz themselves acknowledged, was 'a flight of capital to the United States' and '[c]urrency in the hands of the public increased, absorbing the increase in gold'.<sup>76</sup> That suggests that currency holdings by the public are a rather noisy indicator of the contagion of fear they evoked, especially during a period of international financial turbulence like the early 1930s.

If *A monetary history* offered little direct evidence of widespread panic among US depositors, its monetary consequences seem clear enough in the book. Drawing on the notion of a money multiplier, Friedman and Schwartz identified three 'proximate determinants' of the money stock—the amount of high-powered money (H), the ratio between deposits and cash (D/C), and the ratio between reserves and cash (R/C)—and tracked their movements with historical data.<sup>77</sup> What they discovered was that '[t]he factors accounting for changes in the stock of money' from 1929 to 1933 were 'strikingly different from those in the other periods we have examined'. Whereas in the past, high-powered money 'has impressed itself most strongly on the total stock of money', the money stock collapsed in the early 1930s, despite an increase in high-powered money. The reason, they emphasized, is that 'the two deposit ratios take command', holding steady until the first banking crisis, and then plummeting.<sup>78</sup> The crucial question their analysis suggested, therefore, is what drives these deposit ratios, especially the deposit–currency ratio, whose decline was especially dramatic?

The money multiplier is generated by manipulating accounting identities and, as Friedman and Schwartz noted, offers only an 'arithmetic breakdown' of the components of the money stock.<sup>79</sup> Thus, it is necessary to specify the economic behaviour that generates its various components and Friedman and Schwartz certainly wrote as if depositors were the primary actors in bringing about the collapse of the money stock. Panicked depositors in their account applied pressure on

<sup>72</sup> Ibid., pp. 310–11.

<sup>73</sup> Ibid., pp. 308–11.

<sup>74</sup> Ibid., p. 308.

<sup>75</sup> Ibid., pp. 710–14.

<sup>76</sup> Ibid., pp. 314–15.

<sup>77</sup> For the origins of this approach—today the monetary workhorse of undergraduate economic textbooks—see Humphrey, 'Multiple expansion'.

<sup>78</sup> Friedman and Schwartz, *AMH*, pp. 332–3. The visual impression was confirmed with a numerical analysis of the relative importance of the proximate determinants of the money stock (ibid., pp. 333–4), which identifies the 'deposit ratios', especially the deposit–currency ratio, as crucial in driving down the money stock.

<sup>79</sup> Ibid., p. 274.





the banking system by pulling their money out of unhealthy and healthy banks alike so that 'the banking system as a whole was in a position to meet the demands of currency only by a multiple contraction of deposits, hence of assets'.<sup>80</sup> Banks, from this perspective, played a passive role in money creation, waiting for deposits to be made, setting aside a fraction in reserves, then lending out the remainder in monies that were deposited again, creating a cascade of loans and deposits. Conversely, when money was withdrawn from the banking system to be held in cash, the cascade operated in reverse, creating what Friedman and Schwartz described as a multiple contraction of deposits.

Thus, Friedman and Schwartz turned an accounting identity into a theory of money creation. It was a theory that suited their purposes since it implied that the only actor, besides depositors, that had a significant role to play in monetary creation or collapse was the Fed. Had the Fed used its powers to create sufficient high-powered money in the early 1930s, they suggested, the money stock could almost certainly have been 'kept from declining or, indeed, increased to any desired extent'.<sup>81</sup> Their assumption, once again, was that banks were passive players, and would follow where the central bank led.<sup>82</sup>

The characterization of money creation we find in Friedman and Schwartz is common among economists who rely on the money multiplier, but an alternative logic of money creation finds long and distinguished expression in economics. It characterizes banks as playing a much more active role in money creation, creating deposits through the act of lending rather than waiting for deposits to be placed with them. Whichever way one conceives of the process of money creation, it is possible to imagine it going into reverse, to generate a 'multiple contraction of deposits'. However, in one case, contraction would be precipitated by depositors converting their deposits into cash and, in the other case, banks would provoke that contraction by reducing their loans.

Where the balance lies is surely an empirical question, but Friedman and Schwartz offered surprisingly limited evidence to justify their own emphasis on depositors' behaviour and *A monetary history* does not allow us to observe the rhythm of US commercial banks' loans during the Great Depression.<sup>83</sup> One might be tempted to argue that Friedman and Schwartz captured changes in bank behaviour through the ratio of deposits to bank reserves. Yet just as for currency holdings by the US public, the measure of bank reserves that Friedman and Schwartz employ—cash held in bank vaults and bank deposits at Federal Reserve Banks—is a muddy proxy for banks' behaviour. Changes in the willingness of banks to lend may have affected this ratio but it was subject to other influences too, such as shifts from time to demand deposits or from country banks to banks in central reserve cities, since they were subject to different reserve requirements in the US at that time. Moreover, the meaning to be assigned to movements in the deposit–reserve ratio depends on our understanding of money creation and the relationship it implies between bank deposits, reserves, and loans. In this regard, Friedman and Schwartz offered little explicit analysis to justify their views, relying on assertion rather than argument in making claims such as 'additional [bank] reserves would almost certainly have been put to use promptly'.<sup>84</sup>

<sup>80</sup> Ibid., p. 355.

<sup>81</sup> Ibid., p. 11.

<sup>82</sup> For a recent analysis that raises serious questions about this assumption, see Marglin, *Raising Keynes*, pp. 293–9.

<sup>83</sup> Resourceful readers will find a hint in a table much later in the book, which shows a collapse in bank loans that was much sharper than the decline in deposits; Friedman and Schwartz, *AMH*, tab. 17, p. 450. Thus, they might well ask for more to be convinced that banks were the passive actors in the collapse of deposits that Friedman and Schwartz imply they were.

<sup>84</sup> Speaking of the period from Aug. 1929 to Oct. 1930; *ibid.*, p. 341.



So much for the gaps in *A monetary history's* 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 this rhythm generated an economic crisis. They wrote as if the link was evident, claiming that the chronology of monetary events 'serves about equally well to demarcate distinctive behavior of the other economic magnitudes'.<sup>85</sup> 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 well before the banking crises even began, with no clear change in the character of the downward trends once these crises struck.<sup>86</sup> Thus, 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, to be convinced of *A monetary history's* interpretation of the Great Depression. Without it, we cannot dismiss the possibility that the rhythm of monetary events so painstakingly described by Friedman and Schwartz was less important than they claimed and maybe even an outcome as much as a cause of the broader economic crisis.

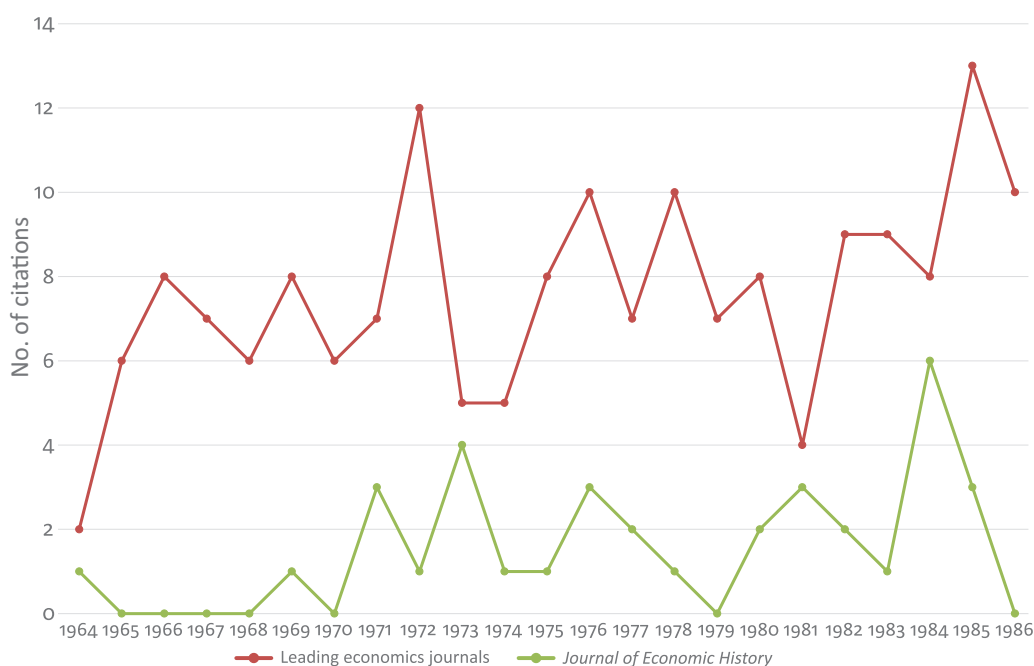
To conclude, let us come to the Fed, which has hardly been mentioned so far and for good reason. The Fed was not involved in any of the causal junctures that Friedman and Schwartz identified as leading to the Great Depression. It entered instead as a *deus ex machina* and, more specifically, as a god that stood by and let his people suffer by failing to counteract the collapse of the US money stock with liquidity injections. To believe that claim, as I noted above, a reader would have to accept the way Friedman and Schwartz characterize the process of money creation but, even then, their claims about the Fed's possible options and their likely effects could not be proven. So they had to find a way to make the Fed's ineptitude seem plausible and the devices they used to do that 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, generating confusion about what any specific individual thought and did in this story. What really matters is the overall impression that any of them could have done the right thing but that none of them did. People with deposits panicked; they pulled their money out of banks in the way that clouds accumulated and rain fell. People in the Fed, in contrast, had the power of reason and the ability to act but they proved 'inept' and allowed the US money stock to collapse, thereby turning a normal recession into the Great Depression.

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 their 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 the leading journal of economic history in the US. And we do see a promising start there in an extensive review by Robert Clower. He complimented Friedman and Schwartz on their historical research, noting that their 'judgements' about the monetary history of the US were 'based on pain-staking examination of a fantastically large body of evidence and on thorough, honest, and closely reasoned analysis of its implications'. Still, Clower concluded that 'subsequent researches, provoked by Friedman and Schwartz's pronouncements, will overturn some of their bolder judgements, but that is another story'.<sup>87</sup>

<sup>85</sup> Ibid., p. 305.

<sup>86</sup> See *ibid.*, chart 28, p. 303.

<sup>87</sup> Clower, 'Monetary history', p. 379.



**FIGURE 2** Citations of *A monetary history*, 1963–86. *Note:* Leading economics journals include *Journal of Political Economy* (JPE); *American Economic Review* (AER); *Journal of Monetary Economics* (JME); *Journal of Money, Credit and Banking* (JMCB); *Review of Economics and Statistics* (RECSTAT); *Journal of Finance* (JF); *Economic Journal* (EJ); *Quarterly Journal of Economics* (QJE). *Source:* Author's analysis based on Bordo, ed., *Money, history, and international finance*, tab. 1.1, 17 [Colour figure can be viewed at [wileyonlinelibrary.com](https://onlinelibrary.wiley.com/doi/10.1111/ehr.13117)]

Certainly, it should have been another story but it was not. What we observe in figure 2 are the citations of *A monetary history of the United States* in the *Journal of Economic History* in the 25 years following the book's publication. What is striking is how few there were, and when we 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 other leading historical journals in the US, the pickings are slimmer still.

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. A significant number of reviews and articles were written by economists who shared Friedman and Schwartz's monetarist views and spoke of the book as if its evidence and claims were above reproach. Scepticism was expressed, as we might expect, by Keynesian economists, but their sharpest criticisms were targeted at what they saw as an unsubstantiated theoretical model lurking behind the historical interpretations offered in *A monetary history*.<sup>88</sup> With respect to the historical analysis in *A monetary history*, in contrast, economists like Roy Harrod and James Tobin displayed a startling willingness to accept the book's claims including, as Tobin put it, of 'the passive acquiescence of the Fed in the monetary contraction and banking collapse'.<sup>89</sup>

<sup>88</sup> For an extended discussion of these criticisms, see Hammond, *Theory and measurement*, pp. 105–23.

<sup>89</sup> Tobin, 'Monetary interpretation', p. 483.



As Rockoff observed, most economists did not do the kind of historical work that Friedman and Schwartz offered so they had little competence to vet *A monetary history's* claims or build alternatives to them.<sup>90</sup> There was a sharp contrast, therefore, between their reception of *A monetary history* and Friedman's econometric research. Once Friedman's critics were on their own territory, as Cherrier observed, they 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.<sup>91</sup> In contrast, they expressed respect, even reverence, for his historical analysis with Schwartz.

Thus, writing 13 years after *A monetary history* was published, Elmus Wicker emphasized the dearth of thorough critical assessment of the book's historical analysis.<sup>92</sup> It is true that when Charles Kindleberger published a book on the Great Depression in 1973, he engaged directly with *A monetary history* and made no bones about his view of it: 'In my judgment, it is wrong'.<sup>93</sup> But Kindleberger was soon pushed back on the defensive, with one reviewer noting: 'Kindleberger's dismissal of Friedman-Schwartz is vigorous but imprecise'.<sup>94</sup> Schwartz proved to be an especially tough critic, casting Kindleberger's 'journalistic account' as 'inconsistent in detail and as loosely constructed in its broad outlines', and characterized by '*obiter dicta* on substantive issues without supporting evidence and casual dismissal of opposing views'.<sup>95</sup> 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. Yet even less interested reviewers than Schwartz were critical of Kindleberger's efforts to dethrone *A monetary history*, with the 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 still cannot be said that Kindleberger has gone very far to rehabilitate the case for the neo-Keynesian interpretations of the period'.<sup>96</sup>

Wicker took much the same view in criticizing 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?'.<sup>97</sup> 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

<sup>90</sup> Rockoff, 'Review essay'.

<sup>91</sup> Cherrier, 'Lucky consistency'.

<sup>92</sup> Wicker, 'Review', pp. 993–4.

<sup>93</sup> Kindleberger, *World*, p. 20.

<sup>94</sup> Shaw, 'Review', p. 400.

<sup>95</sup> Schwartz, 'Review of *The world in depression*'.

<sup>96</sup> Engerman, 'Avoiding', p. 427.

<sup>97</sup> Wicker, 'Review', pp. 993–4.



‘[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.<sup>98</sup>

Where was Anna Schwartz in this tribute, one might ask, and who were the critics who agreed that *A monetary history* was a monumental work?<sup>99</sup>

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 the only rival that was to gain significant credibility among economic historians. In *Did monetary forces cause the Great Depression?* Peter Temin offered a clear, negative answer to his provocative question.<sup>100</sup> He acknowledged that *A monetary history*’s account of the Great Depression ‘stands without peer among narratives of the early 1930s. It is scholarly, detailed, insightful, and fascinating’.<sup>101</sup> But then his axe fell: ‘[w]hat evidence do Friedman and Schwartz muster to support [their] propositions?’<sup>102</sup> Very little, he suggested, since the book ‘assumes the conclusion and describes the Depression in terms of it; it does not test it or prove it at all’.<sup>103</sup> 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’.<sup>104</sup>

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, applied econometric models of consumer spending to historical data to identify changes in consumption that could not be explained by the models. In claiming that an ‘autonomous’ drop in consumption was a cause of the Depression, therefore, Temin assigned historical significance to

<sup>98</sup> ‘The Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel 1976’, <https://www.nobelprize.org/prizes/economic-sciences/1976/summary/> (accessed on 25 Aug. 2021). In fact, the study was the only work mentioned by name in the citation. It alluded to Friedman’s argument ‘that monetary policy should be simplified and that its goal should be to ensure a long-range stable growth rate of the supply of money’ but made no explicit mention of his presidential address to the American Economic Association which was to become his other most-cited monetary publication (for a discussion of its reception, see Forder and Sømme, ‘Explaining’).

<sup>99</sup> Friedman himself paid 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’. In an interview with Eugene White decades later, Schwartz was asked about the impact of *A monetary history*, she added the following aside: ‘(Incidentally, *A Monetary History* served as my [Ph.D.] dissertation at Columbia).’

<sup>100</sup> Temin, *Monetary forces*.

<sup>101</sup> Ibid., p. 14.

<sup>102</sup> Ibid., p. 15.

<sup>103</sup> Ibid., p. 16.

<sup>104</sup> Ibid., p. 178.



an econometric artefact. But, in the spirit of Friedman's earlier critique of Tinbergen, that artefact could just as easily be interpreted as evidence of the difficulties of specifying a simple mathematical function that could 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 evidence to show its historical character, concluding that it derived 'from a variety of diverse and as yet still incompletely delineated sources'.<sup>105</sup> As he acknowledged himself, '[i]t is somewhat unsatisfactory to say that the Depression was started by an unexplained event'.<sup>106</sup>

Indeed, Hyman Minsky concluded from Temin's book that '[n]either hypothesis really passes the tests' but his main criticism focused on the limited scope of the theoretical explanations of the Depression that it considered, specifically the fact that Temin cast 'two currently fashionable views' as 'the outer limits of the spectrum of theoretical explanations to be tested'.<sup>107</sup> Minsky saw himself as writing in a long tradition of economists 'who held that the capitalist process is endogenously unstable'.<sup>108</sup> In his own work, he argued that 'the *normal functioning* of a capitalist economy leads to conditions conducive to a financial crisis'.<sup>109</sup> Writing in the 1970s, he emphasized just how heretical such ideas seemed with financial instability as 'a non-event, something which just cannot happen, insofar as the standard body of economic theory is concerned'.<sup>110</sup> However, Minsky criticized Temin not just for overlooking financial instability but for a more general neglect of economic dynamics that might generate endogenous instability in capitalist systems.<sup>111</sup>

Temin was not alone among US economic historians in conceiving of such a narrow spectrum of explanations of the Great Depression. That Engerman, Wicker, and Temin could all conceive of a Keynesian alternative as the only 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 bore some responsibility for this narrowing of the scope of imaginable explanations of cycles. Minsky may have been an economic heretic but he was much 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—or generated their own interpretations of the Great Depression in the US—as Josef Steindl attempted—they made hardly any impact on the debate about the causes of the Depression among economic historians.<sup>112</sup>

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 with Romer and Olney putting more historical flesh on its econometric

<sup>105</sup> Ibid., p. 172.

<sup>106</sup> Ibid., p. 83.

<sup>107</sup> Minsky, 'Review', p. 45.

<sup>108</sup> Minsky, 'Financial instability hypothesis', p. 1.

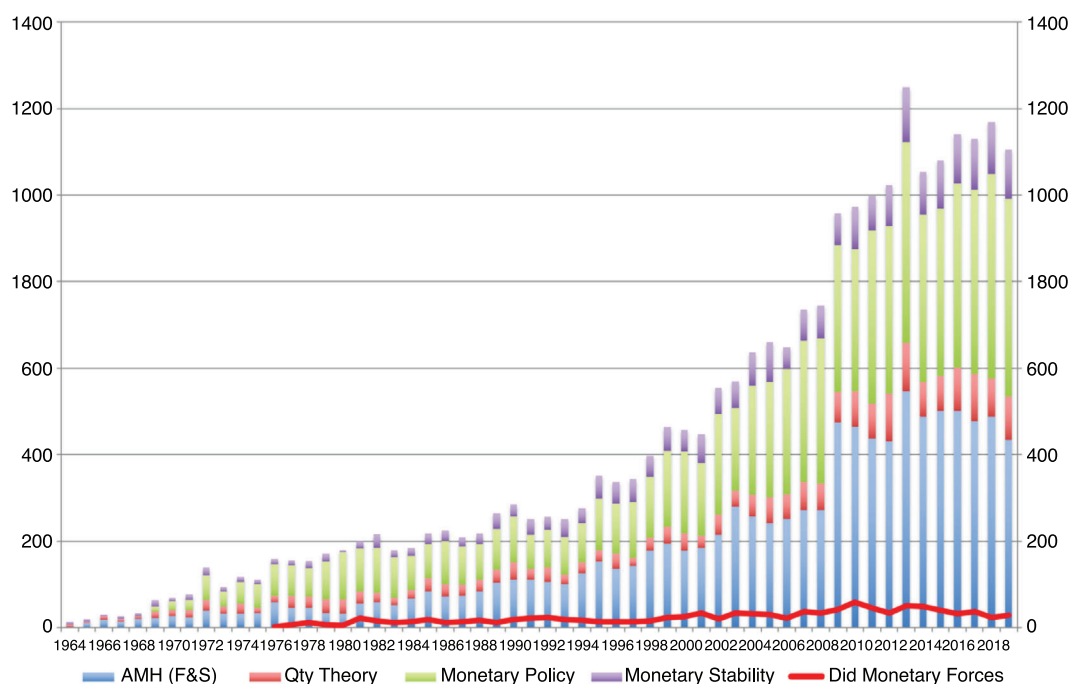
<sup>109</sup> Ibid., p. 9 (original emphasis).

<sup>110</sup> Ibid., pp. 1–2.

<sup>111</sup> Minsky, 'Review', p. 45.

<sup>112</sup> Mayhew, 'Ideology'; Steindl, *Maturity*. It is worth noting that the work of economic historian Michael Bernstein, which was acknowledged by some historians although it built critically on the work of Steindl and other heterodox economists, was presented not as a challenge to Friedman and Schwartz but as an explanation of why the Depression was as long as it was; Bernstein, *Great Depression*; see also Wheeler, ed., *Economics*.





**FIGURE 3** Citations of Friedman's most influential contributions to monetarism versus Temin's *Did monetary forces cause the Great Depression?*, 1956–2020. Qty Theory: Friedman, 'Quantity theory of money'. Monetary Policy: idem, 'Role of monetary policy'. Monetary Stability: idem, *Program for monetary stability*. Did Monetary Forces: Temin, *Monetary forces*. Source: Author's analysis based on Google Scholar, consulted on 22 July 2021 [Colour figure can be viewed at [wileyonlinelibrary.com](https://onlinelibrary.wiley.com/doi/10.1111/ehr.13117)]

bones.<sup>113</sup> But if we look at citation patterns in figure 3, even before citations of *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, allowing *A monetary history* to gain extraordinary influence not only in economic history but far beyond, to become a crucial pillar of monetarism, as figure 3 suggests.

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. Duly admonished by the earlier fracas, he took pains to cast himself as a modest historian with no claim to being an economic heretic. In a lecture on his 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 ... 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.'<sup>114</sup> Lest there be any doubt that what was at stake had a religious character, Kindleberger offered the following analogy:

<sup>113</sup> Romer, 'Great Crash'; Olney, 'Avoiding default'.

<sup>114</sup> Kindleberger, 'Manias, panics, and rationality', p. 110; idem, *Manias, panics, and crashes*.



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.<sup>115</sup>

Kindleberger acknowledged that his analogy 'must be offered with particular delicacy or it may be thought offensive by some' but it certainly reveals a good deal about how economic heretics were marginalized as hypochondriacs and pill-poppers in historical debate.<sup>116</sup>

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 criticize 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 to confront, or even evaluate, the core claims on which Friedman and 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. Yet, as we have seen, Friedman and Schwartz displayed a marked reluctance to engage in any systematic evaluation of the role of the banking system during the crisis.

As Charles Calomiris explained, there was limited attention to 'Financial Factors in the Great Depression' in the two decades after *A monetary history* was published. He acknowledged that there were 'a few dissidents' like Minsky who were interested in the theme but only Kindleberger focused on the Depression and: '[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'.<sup>117</sup> That situation changed, Calomiris noted, with the publication of an important article by macroeconomist Ben Bernanke in 1983.<sup>118</sup>

Bernanke's article opened with a vivid statement of the financial chaos that characterized the Great Depression in the US. We might think, Bernanke said, that 'the financial system simply responded, without feedback, to the declines in aggregate output' but he dismissed that view as 'contradicted by the facts' and insisted that the economic effects of the financial collapse of the 1930s were poorly understood.<sup>119</sup> That it took so long for such an acknowledgement to be made in the leading economics journal of the US offers food for thought, but what really makes one wonder is where Bernanke went next.

<sup>115</sup> Kindleberger, 'Manias, panics, and rationality', p. 110.

<sup>116</sup> Ibid.

<sup>117</sup> Calomiris, 'Financial factors', p. 63.

<sup>118</sup> Bernanke, 'Nonmonetary effects'.

<sup>119</sup> Ibid., p. 257.



First, he identified his allies. Noting that there was ‘much support for the monetary view’, Bernanke emphasized that ‘it is not a complete explanation’ since ‘the reductions of the money supply in this period seems [*sic*] quantitatively insufficient to explain the subsequent falls in output’.<sup>120</sup> Nevertheless, he opted to ally with the monetarist view, rather than challenge it, noting that his article ‘builds on the Friedman-Schwartz work’.<sup>121</sup> Second, Bernanke 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’.<sup>122</sup> That was an odd way to take sides, given that Friedman and Schwartz relied so heavily on a contagion of fear 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.<sup>123</sup>

Reading Bernanke’s discussion of the financial collapse, it becomes clear that he promised something that he could not deliver. Inspired by Irving Fisher, Bernanke claimed that the US economy was sucked into a vicious dynamic of debt deflation in the Great Depression. He insisted 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’.<sup>124</sup> Bernanke offered a brief overview of credit expansion in the 1920s, and clearly suggested it was excessive.<sup>125</sup> As to what happened next, Bernanke said that the US financial system ‘historically suffered’ from a ‘malign source of bank failures, namely financial panics’ and in the 1930s ‘the entire system was adversely affected’ and not just the ‘marginal banks’.<sup>126</sup> What Bernanke did not explain, however, is how over-indebtedness and panic could occur in the rational world he was trying to coax out of a seemingly irrational history.

Indeed, it was to this question that Fisher had turned towards the end of his classic article on ‘The debt-deflation theory of great depressions’. In a section entitled ‘Debt starters’ Fisher emphasized that ‘[t]he over-indebtedness hitherto presupposed must have had its starters’ and then turned to an analysis of its causes.<sup>127</sup> Fisher claimed 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’.<sup>128</sup> His basic schema was taken up by Minsky and developed into a full-blown theory of capitalism’s inherent financial instability; Bernanke, in contrast, chose to presuppose over-indebtedness, and overlook the financial dynamics that generated it, to focus on the impact of the subsequent financial collapse.

<sup>120</sup> Ibid.

<sup>121</sup> Ibid.

<sup>122</sup> He went on to explain: ‘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.*, n. 5, p. 258.

<sup>123</sup> Friedman and Schwartz, *AMH*, p. 308.

<sup>124</sup> Bernanke, ‘Nonmonetary factors’, p. 261.

<sup>125</sup> Bernanke’s summary of the expansion of inside debt in the 1920s was based entirely on an article in 1930 by Charles Persons whose emphasis was on the ‘new lessons respecting the evils of credit inflation’ to be learned from the experience of the 1920s; Bernanke, ‘Nonmonetary factors’, pp. 259–61; Persons, ‘Credit expansion’, pp. 45, 128.

<sup>126</sup> Bernanke, ‘Nonmonetary effects’, pp. 259–60.

<sup>127</sup> Fisher, ‘Debt-deflation theory’, p. 57.

<sup>128</sup> *Ibid.*, p. 58.



Bernanke's main concern was the impact of financial collapse on the quality of credit intermediation in the US economy. He sought inspiration in models of imperfect information to suggest a story in which the financial implosion of the early 1930s precipitated 'a contraction of the banking system's role in the intermediation of credit' with '[s]ome of the slack ... 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. The real economy bore the burden of that poorer information, Bernanke claimed, through an inefficient allocation of financial resources.<sup>129</sup>

Although Bernanke was talking about a banking system that he believed had plunged into over-indebtedness, his model was premised on this same banking system having superior information. Unfortunately, he offered no evidence in support of this premise 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, Bernanke 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', pointing to monthly data on bank loans that he compiled from the *Banking and Monetary Statistics* of the US.<sup>130</sup> Certainly, that was more evidence than Friedman and Schwartz offered on bank loans but as the main basis for Bernanke's claims about the nonmonetary effects of the financial crisis in the propagation of the Great Depression, it is hardly a smoking gun.

It is worth highlighting that Bernanke's 1983 paper has been celebrated for bringing about a paradigm shift in our understanding of the Great Depression in the US. 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'.<sup>131</sup> The reference to evidence seems exaggerated since Bernanke presented no direct evidence for any concept that featured in his theoretical analysis. 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 emphasized, what accounts for Bernanke's influence? He did not present a formal model of the mechanisms he emphasized but that did not stop his paper becoming the only publication on the Great Depression in the US that came close to attracting anything like the attention accorded to *A monetary history*.

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 an increasingly orthodox view of the Great Depression. For the most part, the influence of this orthodoxy on historical research was much less deliberate and was reflected as much in the topics explored as what was discovered about them. Of particular importance in this regard is the attention that economic historians gave to the causes of US bank failures and the Federal Reserve System's policies in the 1920s and 1930s.

Following the publication of *A monetary history*, it was Temin who set off a major debate about the causes of US bank failures, claiming that: '[t]he banking panics were a part of a larger

<sup>129</sup> Bernanke, 'Nonmonetary factors', p. 264.

<sup>130</sup> Ibid., p. 264. These data, shown on p. 262, extended from July 1929 to March 1933 and suggested to Bernanke that '[t]he fall in loans after November 1930 was not simply a balance sheet reflection of the decline in deposits'; ibid., pp. 263–4.

<sup>131</sup> Calomiris, 'Financial factors', p. 68.



process that started with the decline in autonomous spending'. Put differently, they were a symptom of a Depression already underway rather than the result of liquidity shocks.<sup>132</sup> Some reviewers pointed out that Temin had little evidence for his claim, prompting further investigation into the US banking crises of the Great Depression.<sup>133</sup> Early contributions came from Wicker and Eugene White on the banking crisis of 1930: 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.<sup>134</sup> Far from ending there, the discussion about banking crises went on, with a wide variety of studies accumulating, and historians taking sides for and against *A monetary history's* claims.

On one occasion, when a hapless historian took aim at a crucial element of their historical interpretation, Friedman and Schwartz made an extraordinary intervention in the debate. Joseph Lucia offered evidence that the failure of the Bank of the United States stemmed from its 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'.<sup>135</sup> Friedman and Schwartz issued a ferocious reply, questioning Lucia's 'scientific integrity' and the 'scientific standards' of *Explorations in Economic History* where his article was published. They claimed that Lucia had distorted their claims in *A monetary history* and failed to use information that Friedman had sent him before his article was published.<sup>136</sup>

Their intervention represented a stark contrast to *A monetary history* in its ratio of content to bluster but two important points can be discerned in the furious prose. 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 had offered and that it was based on historical evidence that they did not have at their disposal. Indeed, they claimed that if Lucia had focused on the reasons for the failure of the Bank of the United States, there would 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'.<sup>137</sup> Second, and here they repeated what they had stated in *A monetary history*, the reasons for the failure of specific banks or clusters of banks were much less important than the impact of these failures on 'monetary and economic developments in the country as a whole'. On this issue, Friedman and Schwartz claimed that Lucia 'offers essentially no evidence on this question, only unsupported assertions'. They omitted to mention that they had offered limited evidence in *A monetary history* and had nothing new to offer in this regard. Instead, they returned 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.<sup>138</sup>

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 specific bank failures on broader

<sup>132</sup> Temin, *Monetary forces*, pp. 9–10.

<sup>133</sup> Mayer, 'Money'.

<sup>134</sup> Wicker, 'Reconsideration'; White, 'Reinterpretation'; for an even more critical perspective, see Hamilton, 'Causes'.

<sup>135</sup> Lucia, 'Failure', p. 415.

<sup>136</sup> Friedman and Schwartz, 'Failure', p. 204.

<sup>137</sup> Ibid., pp. 202, 204.

<sup>138</sup> Ibid., p. 199. Ten years later, there was a fresh contribution to the debate about the Bank of the United States, which clearly came down on Joseph Lucia's side, although he had died in the meantime; O'Brien, 'Defense'.



monetary and economic variables. Nevertheless, it was the causes of bank failures in the Great Depression that continued to absorb most of the attention. Forty-five years after *A monetary history* was published, Richardson offered an extensive review of the literature that had accumulated and concluded that the illiquidity versus insolvency debate remained unresolved.<sup>139</sup>

More problematic than the debate's inconclusiveness is that it distracted historians from scrutinizing the key mechanism in *A monetary history*'s causal explanation: the contagion of fear among depositors and its impact on the money stock. Even when historians claimed to be scrutinizing that contagion of fear, they often used bank deposits as a proxy even though they reflected a process of money creation that involved more than just depositors' decisions. And insofar as currency holdings by the public were concerned, there were few efforts to look beneath the aggregate trend presented in *A monetary history* despite long-standing evidence that currency holdings increased during the Great Depression according to patterns that bore no straightforward relationship to bank failures.<sup>140</sup>

An important exception is Wicker's book on *The banking panics of the Great Depression*, in which he analysed regional data on bank suspensions and currency holdings. Wicker concluded there was no national contagion of fear before the onset of the third banking crisis in September 1931. That crisis coincided with the suspension of gold convertibility of the British pound, creating financial turbulence that prompted Wicker to make an important methodological point. Currency holdings by the public, he observed, were 'not a reliable indicator of depositor distrust' since some of the increase in Federal reserve notes was being shipped abroad.<sup>141</sup>

If Wicker's analysis seemed like an open invitation to go further in exploring *A monetary history*'s contagion of fear and its macroeconomic consequences, historians have been slow to accept it. As Mitchener and Richardson noted as recently as 2020: '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, as they observed, '[t]he contagion-of-fear hypothesis rests on narrative evidence and time-series aggregates collected decades ago'.<sup>142</sup>

Besides the US banking crises of the 1930s, the other aspect of *A monetary history*'s interpretation of the Great Depression that stimulated considerable interest among historians was the book's explanation of the Fed's alleged ineptitude. Friedman and 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.<sup>143</sup> 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, and attention shifted to other explanations of the Fed's alleged unwillingness to counter the decline in the money stock.<sup>144</sup>

Around the same time, the debate about monetary factors in the Great Depression went international, with Temin and Eichengreen arguing that the commitment to the gold standard constrained central banks around the world from adopting expansionary policies.<sup>145</sup> Although there

<sup>139</sup> Richardson, 'Categories and causes'.

<sup>140</sup> Reed, 'Distrust'.

<sup>141</sup> Wicker, *Banking panics*, pp. 77–8.

<sup>142</sup> Mitchener and Richardson, 'Contagion of fear', p. 1.

<sup>143</sup> Friedman and Schwartz, *AMH*, pp. 407–19, 692.

<sup>144</sup> See, for example, Wheelock, *Strategy*; Eichengreen and Temin, 'Gold standard'; Meltzer, *History*.

<sup>145</sup> Temin, *Lessons*; Eichengreen, *Golden fetters*.





was disagreement about the specifics—Schwartz resisted the ‘gold fetish’ interpretation for the US<sup>146</sup>—the consensus that emerged about the centrality of monetary factors as causes of the Great Depression was extraordinary.<sup>147</sup> Thus, the global depression of the 1930s was no longer acknowledged to be a crisis of capitalism but a recession that turned into a depression because of central bankers’ and political leaders’ mentalities about money.

This consensus is 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. As we have seen, we remain dependent on the modest 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. As Romer and Romer explain:

saying the book proves that monetary shocks caused the Depression is a stretch. Of the monetary shocks Friedman and Schwartz identified, 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.<sup>148</sup>

It is telling that such a statement still needs to be made 50 years after *A monetary history* was published.

### III | THE US 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 US has been trapped in an economic orthodoxy that turns on and around *A monetary history*. Its interpretation of the Great Depression 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 so we still do not know why industrial production and investment and consumption and employment and wages collapsed in the US between 1929 and 1933.

We could take the sanguine view that heretical views will bubble up from historical research if they are worth considering. Indeed, there has been a veritable outpouring of research on the Great Depression in recent years, much of it animated by the search for historical precursors of the financial dynamics that caused the global financial crisis of 2008–9. The flurry of intellectual activity has led to a much more careful scrutiny of financial dynamics in the 1930s and there are tentative signs of some loosening of the ties that bound historical research to the economic orthodoxy spawned by *A monetary history*. Even more striking, perhaps, is the fact that central banks’ frustration at their inability to anticipate banks’ responses to liquidity injections has led to a concerted attack on the money multiplier as a guide to money

<sup>146</sup> Schwartz, ‘Review of *Lessons*’.

<sup>147</sup> For an expression of that consensus, see Eichengreen and Temin, ‘Gold standard’.

<sup>148</sup> Romer and Romer, ‘Missing transmission’.



creation.<sup>149</sup> Ironically, a financial crisis that generated unprecedented public attention for *A monetary history* may lead to a long overdue scrutiny of its basic premises.

Whatever hope there is of a heretical turn in exploring the financial and monetary dynamics of the Great Depression, there is much 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 naive. 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 analytical as well as descriptive, and prevent us from engaging in empiricism for its own sake. 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 US.

We have seen that inadequacy in 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 Greasley and Madsen that suggested that the decline in investment in 1930 had 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 acknowledged it: 'neither consumption nor the fixed investment slumps of 1930 were truly autonomous: rather, they are unexplained by conventional models'.<sup>150</sup> We might well ask 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, why does it persist? 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 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 the Second World War, claims of under-consumption and over-production bit the dust in mainstream economics, encouraging a search for exogenous forces to explain crises and cycles. 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.

This 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 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

<sup>149</sup> See, for example, [European Central Bank](#), 'Supply of money'; [McLeay, Radia, and Thomas](#), 'Money creation'; [Deutsche Bundesbank](#), 'Role of banks'. The claim they make is succinctly expressed by Charles Goodhart: 'the old pedagogical analytical approach that centred around the money multiplier was misleading, atheoretical and has recently been shown to be without predictive value'; [Goodhart](#), 'Money', p. F82.

<sup>150</sup> [Greasley and Madsen](#), 'Investment and uncertainty', p. 409.



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?

Mitchell articulated a theory that located the rhythm of business cycles in the dynamics of enterprises' profit-making. His analysis of profit-making was grounded above all in scrutiny of the changing system of prices in an economy and its implications for enterprises' revenues and costs. We would do well to ask for more when it comes to the determinants of profits and in my own work on the history of profit I have highlighted issues that are not envisaged in Mitchell's analysis.<sup>151</sup> Still, there are plenty of insights to be gained from Mitchell's approach, which offers as good a place as any to start an analysis of profit and cycles.

In his 1913 book, Mitchell sketched an empirical methodology for linking 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. Whatever the promise of his research agenda, however, Mitchell never succeeded in drawing out the implications of all of the new data on prices and profits for his theory of business cycles.

Nevertheless, the potential of Mitchell's approach can be seen by engaging with a recent paper in the *Journal of Economic History*, which focuses not on the Great Depression but on the major US recession of 1937–8. In 'What was bad for General Motors was bad for America', Hausman explained that 'the most popular explanation for the 1937/8 recession is restrictive monetary policy', citing Friedman and Schwartz as an influential advocate of this view. Hausman took issue with a monetary explanation on the grounds that the decline in the money supply was 'ill-suited to explain a much more rapid decline in industrial production'. He offered 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.<sup>152</sup>

Hausman's analysis seems to owe nothing to Mitchell except for the rich array of NBER price series it employed 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 proposed 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 offered.

To make his case, Hausman estimated 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 suggested that material and labour costs were about equally important, and that they both rose sharply in 1937, Hausman concluded that they contributed in roughly equal measure to rising cost pressures. He argued that consumers rushed to buy cars in anticipation of a price rise—presumably motivated by car makers' efforts to avert a profit squeeze—and stopped buying them when automobile prices actually rose, leading to a collapse in industry sales. Hausman never referred to profit in his article, or any synonym for it, except to quote Alfred Sloan of General Motors as lamenting the decline in net income 'in relation to unit and dollar sales volume'.<sup>153</sup> The absence of the concept is not just a question of semantics since thinking in terms of profit suggests new insights for Hausman's analysis.

<sup>151</sup> Given the historical context in which it was developed, it is not surprising that Mitchell's analysis is best suited to an analysis of the determinants of the return on sales; for explaining profit as a return on capital, in contrast, it excludes the important role of capital turnover or 'turns'; see O'Sullivan, 'Confusion', pp. 146–52; *eadem*, 'Intelligent woman's guide'.

<sup>152</sup> Hausman, 'What was bad', cited at pp. 433, 435.

<sup>153</sup> *Ibid.*, pp. 455–6.



**TABLE 1** Census statistics for the US motor vehicle industry, 1923–37

Year	% of value of product		
	Wages	Cost of materials, fuel, and purchased electricity	Other expenses and 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% of the value of product.

*Source:* Author's analysis based on US Department of Commerce, Bureau of the Census, *Biennial Census of Manufactures* (1923–37).

The selling price of a typical small car in 1937 was \$575, and Hausman estimated total factory costs at \$202, implying an enormous gross profit of 65 per cent on the price of a small car.<sup>154</sup> That is far above the gross profit of 10 per cent of sales reported by the Federal Trade Commission for GM's small car in 1937.<sup>155</sup> Although the report does not allow us to make similar calculations for other US automakers, data from the Biennial Census of Manufactures for the motor vehicle industry, shown in table 1, confirm that profits before the deduction of salaries and other administrative and central expenses were below 12 per cent of sales in 1937. In addition, these data show that cost pressures were generating a profit squeeze in the car industry in 1937, as Hausman suggested, but show the need to qualify his account of the sources of these pressures.

Crucially, we can see that rising labour costs were not the main source of pressure on US car makers' profits in 1937. Wages were too low as a share of their costs to make as much difference as the rising costs of materials, fuel, and purchased energy. Moreover, since the latter's increased weight in automakers' cost structures dated from the early 1930s, the problem did not stem, as Hausman suggested, from 'actual and expected rearmament demand in Europe'.<sup>156</sup> By the mid-1930s, rising input costs were already pressing down on profits, as table 1 shows.

It is tempting to go back further, 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 1937–8 recession. Indeed, there is no obvious sign of any profit squeeze for automakers in 1929, which seems to have been a record year

<sup>154</sup> Hausman, 'What was bad', pp. 453–4. Hausman does not explicitly generate an estimate of gross profit using the cost estimates he provides although he does say that his estimated costs 'can be compared to a typical small car price' (p. 453). He cites a contemporary estimate of 'variable costs (mostly raw materials and labor costs)' as 85% of the sales price for an 'unnamed company' (p. 453), which would imply a gross profit of 15%, rather different from what Hausman's cost estimates imply but similar to the Federal Trade Commission estimate in the text.

<sup>155</sup> 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](#), p. 538.

<sup>156</sup> [Hausman](#), 'What was bad', pp. 453–4.



for 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.

There is an urge, therefore, 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 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.

The image from September 1929, shown in figure 4, evokes a product that was just as much a symbol of the Roaring Twenties in the US 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 Scranton and 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 keen competition for skilled workers as hosiery companies increased their production and workers secured a rising share in the industry's prosperity, as table 2 shows, with wages increasing as a share of the value produced by the industry.<sup>157</sup>

But how many pairs of stockings is a woman prepared to buy without wondering what she should pay for them? Silk hosiery prices were already falling by the late 1920s when talk of a price war gripped the industry. Sure enough, prices fell sharply and then plummeted in the wake of the stock market crash in October 1929, falling to less than a third of what they had been in the early 1920s. Yet production of silk stockings rose through the 1920s, and even when prices of silk hosiery plummeted, production did not collapse with them.<sup>158</sup>

We can see what happened if we look again at 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 profit squeeze for the industry. Why? Material costs dropped even faster than the price of silk stockings so that the 1920s were an age of prosperity for US silk hosiery. Moreover, even as the lustre wore off, and there was a collapse in the value of sales of silk stockings, the last column of table 2 remained surprisingly buoyant. Many producers of silk stockings found themselves in difficulty—some in grave difficulty, given the need to buy materials months before finished stockings were sold—but the basic foundations of the industry's economic model did not implode.

Hosiery workers experienced a sharp drop in their average wages, similar to that faced by autoworkers, but the vast majority of them held on to their jobs in contrast to their counterparts as the production of silk hosiery held steady. It was only from the mid-1930s, as production of silk stockings, and even their prices, began to rise, that the industry's economics came undone. The industry's improved fortunes prompted hosiery workers to demand higher wages but by then material costs had been squeezed to such an extent that they could no longer act as the buffer 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. What we cannot see, for example, are the sustained efforts by US silk hosiery companies to keep the prices of stockings ahead of material costs by

<sup>157</sup> Scranton, *Figured tapestry*; McConnell-Sidorick, *Silk stockings*.

<sup>158</sup> Federal Reserve Bank of Philadelphia, 'Full-fashioned hosiery industry', *Business Review* (2 Oct. 1944), pp. 6–11.





September, 1929

LADIES' HOME JOURNAL

# Beautiful Legs

appear MORE BEAUTIFUL in perfectly woven Phoenix

BECAUSE they make graceful legs seem still more beautiful, Phoenix Silk Stockings have become the choice of all women of taste and fashion. This deserved popularity results from two things: Phoenix Hosiery is made to the proportions of the most beautiful legs in America and it is accurately woven by the most perfect process known . . . Florenz Ziegfeld, world-famous authority on feminine loveliness, selected the ideal legs which provide the pattern for Phoenix. Three hundred girls from his Broadway shows, representing the country's fairest, were entered in the contest which ended in the choice of Barbara Newberry as the Phoenix Girl.

*Typical of Lovely Women*  
Average in size, this young ingenue's legs are considered typical of the majority of attractive women. Stockings made to her measurements distinguish themselves by their excellent fit. . . . Through the use of the recently patented *Accurator*, which measures and counts every stitch in knitting, Phoenix stockings are now woven with exactness never before thought possible. Used only

by Phoenix, this magical device creates full fashioned hosiery which will delight you with its evenness, sheer quality and perfect shape . . . Phoenix colors, also, keep step with the increased beauty of the knitting. In many shades—Paris-chosen—there is a smart choice for every Autumn costume. \$1.50, \$1.95, \$2.95.

Florenz Ziegfeld's booklet, "How I Selected America's Most Beautiful Legs," will be sent you free. Address Room 501, Ziegfeld Theatre, 6th Avenue and 34th Street, New York City.

MISS BARBARA NEWBERRY  
painted from life by R. John Holmgren

# P H O E N I X

SILK Hosiery

ACCURATOR PROCESS  
PATENTED MARCH 12, 1929

**FIGURE 4** Advertisement for silk hosiery, September 1929. Source: *Ladies' Home Journal*, vol. 46, Sept. 1929 [Colour figure can be viewed at [wileyonlinelibrary.com](http://wileyonlinelibrary.com)]

investing in machines to produce finer and finer stockings.<sup>159</sup> Still, we understand enough to see that silk stockings may have something to teach us about automobiles. Automakers, like silk hosiery companies, faced softening demand for their products from the mid-1920s, with a marked shift towards cheaper cars as the decade unfolded. However, declining costs—both labour and

<sup>159</sup> The importance of such efforts generates a major problem in tracking the prices of silk stockings through the 1920s. Price estimates for the 1920s are often based on stockings produced on 39-gauge knitting machines but as early as 1926 the vast majority of new machines were 42- and 45-gauge, which produced finer stockings that sold at higher prices; *ibid.*, p. 8; *Scranton, Figured tapestry*, pp. 430–4. A similar difficulty has been more widely recognized for the automobile industry since it was the focus of early studies of hedonic pricing in the US; *Court, 'Hedonic price indexes'*.





**TABLE 2** Census statistics for the US silk hosiery industry, 1923–35

Year	% of value of product		
	Wages	Materials, fuel, and purchased electricity	Other expenses and 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: Author's analysis based on US Department of Commerce, Bureau of the Census, *Biennial Census of Manufactures* (1923–35).

material costs—meant that the industry's operating profits rose in the late 1920s as US car production broke all previous records. Yet, as early as 1930, US car production plummeted by nearly 40 per cent compared to 1929 and it continued its decline to only 25 per cent of its 1929 output by 1932. So why did the car industry fail to react like the silk hosiery industry by slashing 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. The US sourced the vast majority of its raw silk from Japan and we have some idea 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 may have cost them.<sup>160</sup> Then the question we should ask is whether the automobile industry could have bet on a similarly drastic diminution of its material costs to maintain production without sustaining crippling losses.

To sell more in the late 1920s, US car makers felt pressure to offer more to consumers, with closed-car models becoming the norm even for cheap cars that became heavier and more powerful over time.<sup>161</sup> Many of these improvements in quality implied more or better materials and although US car makers enjoyed some success in squeezing their suppliers to keep their material costs down, they witnessed nothing like the decline in material costs that we see in silk hosiery. This surely reflected the fact that some of the car industry's suppliers, notably steel producers, proved more resistant to pressure for lower prices than Japanese suppliers of raw silk.

The pressure to offer more for less only increased on the eve of the Great Depression when Ford flooded the US small car market with the Model A, a heavier and more powerful replacement for the Model T. It is not difficult to imagine why some US car makers thought it better to cut back on production, and material costs with it, to sustain their profits rather than slashing their prices in the hope that they could negotiate substantially lower material costs to compensate. That strategy worked out well for some, less so for others, but it brought problems for the future.<sup>162</sup> Even to sell much-reduced numbers of cars, producers felt pressure to offer more, so they further increased

<sup>160</sup> Federico, *Economic history*; Hunter and McNaughton, 'National histories'; Lockwood, 'Japanese silk'; Ma, 'Modern silk road'; Nakabayashi, 'Rise'.

<sup>161</sup> Gordon, *American growth*, pp. 152–7.

<sup>162</sup> Bresnahan and Raff, 'Intra-industry heterogeneity'.



the heft of their cars in the early 1930s, making their cost structures increasingly vulnerable to changes in material costs, as table 1 shows.

To offer a complete analysis of the vagaries of revenues and costs and their implications for profits in the silk-stocking and car industries would require more historical research. But what is important for this article is that the questions I have posed for these industries have resonance across the US economy for the 1920s and 1930s.<sup>163</sup> Those questions point us in directions that we have not adequately explored in historical research on the Great Depression in the US and, therefore, bring me to an important conclusion. If we want to dismiss the US Great Depression as a real problem, let us do that, but only after we have made a much more serious effort to think about it. And to do that, why not use the panoply of economic theories at our disposal, especially theories that grapple with what it means to speak of the emblematic example of a capitalist economy, for its enthusiasts and opponents, on the eve of the Great Depression?

#### IV | CONCLUSION

In this article, I have explored the origins, the limits, and the influence of *A monetary history's* interpretation of the Great Depression in the US for the insights it offers on theory and history in the study of economic life. What I have found suggests serious questions for economic historians about their relationship to the discipline of economics. The recent attention to *A monetary history* in the global financial crisis has given some economic historians a sense that their time had come after years of struggling for attention from economists. Abramitzky, for example, urged other economic historians to make the most of the opportunity by conveying history to economists using concepts and methodologies they understand.<sup>164</sup> Other economic historians have suggested more caution, with Eichengreen pointing to the possible 'misuses' of history when we remember only what is deemed acceptable to the mainstream of the US economics profession.<sup>165</sup> Some have gone further still, with DeLong claiming that the most widely invoked lessons of the US Great Depression have a stronger ideological than historical basis, reflecting a refusal of 'mainstream economists to swim against the neoliberal currents of our age' to acknowledge 'that the failure of markets can sometimes be a greater danger than the inefficiency of governments'.<sup>166</sup>

The story I have recounted here is suffused with ideology, but DeLong's own words suggest it goes deeper than he allows. Even economists who see themselves as ideological opponents of Friedman and Schwartz seek alternative interpretations of the Great Depression in the behaviour of 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, since it would allow us to see patterns we have not seen before and, for that reason, might suggest a different interpretation of the Great Depression.

If such an interpretation is forthcoming, it will not turn on the economic failures of capitalism but on how capitalism worked in the Great Depression. And because it worked, prices and wages might move with or against each other, and production and employment be maintained or slashed. The institutional foundations of capitalism might have come under threat

<sup>163</sup> For a contemporary study of the changing role of material costs in US manufacturing, see [US Department of Commerce, Materials](#).

<sup>164</sup> [Abramitzky](#), 'Economics', p. 1241.

<sup>165</sup> [Eichengreen](#), *Hall of mirrors*, p. 380.

<sup>166</sup> B. DeLong, 'The monetarist mistake', *Project Syndicate*, 30 March 2015.



in the US if its government had not stepped in to control the damage wrought by the system's economic dynamics. But that damage was part and parcel of any society's Faustian deal with capitalism as, indeed, a strikingly mild-mannered heretic pointed out more than a century ago.<sup>167</sup>

## ACKNOWLEDGEMENTS

I developed the themes in this address during my participation in the Uses of the Past in International Economic Relations (UPIER) project, coordinated by Prof. Catherine Schenk at the University of Oxford, with financial support from the HERA Joint Research Programme 3 Uses of the Past, which was co-funded by AHRC, AKA, BMBF via DLR-PT, CAS, CNR, DASTI, ETAG, FWF, FRS-FNRS, FWO, FCT, FNR, HAZU, IRC, LMT, MIZS, MINECO, NWO, NCN, RANNÍS, RCN, SNF, VIAA, and the European Commission through Horizon 2020. I benefitted greatly from colleagues' comments on earlier versions of the text, presented at the Schweizerische Geschichtstage/Journées suisses d'histoire, Universität Zürich (7 June 2019) and the 9ème Colloque international de l'AFEP et de l'IIPPE (5 July 2019). I am grateful too for encouragement and advice from Per Hansen, my colleagues in the Department of History, Economics and Society, Steve Marglin, and the Bachelor students in my course on 'Economie et histoire' at the University of Geneva. A special word of gratitude to John Turner and the other editors of the *Economic History Review* for their invaluable comments in preparing the address for publication.

Open Access Funding provided by Universite de Geneve.

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<sup>167</sup> '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, *Business cycles*, pp. 21–2.



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**How to cite this article:** O'Sullivan, M. A., 'History as heresy: Unlearning the lessons of economic orthodoxy', *Economic History Review*, 75 (2022), pp. 297–335.  
<https://doi.org/10.1111/ehr.13117>