



Federal Reserve Bank of Chicago

**Money, Banking, and Old-School  
Historical Economics**

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# Money, Banking, and Old-School Historical Economics

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## Abstract

We review developments in the history of money, banking, and financial intermediation over the last twenty years. We focus on studies of financial development, including the role of regulation and the history of central banking. We also review the literature of banking and financial crises. This area has been largely unaffected by the so-called new econometric methods that seek to prove causality in reduced form settings. We discuss why historical macroeconomics is less amenable to such methods, discuss the underlying concepts of causality, and emphasize that models remain the backbone of our historical narratives. Keywords: historical macroeconomics, money, banking, financial intermediation (JEL N01, N10, N20).

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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.

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Friedman and Schwartz (1963, 686)

## 1 Introduction

How has the quantitative research in monetary and financial history over the last twenty years changed our understanding of historical and economic processes? What methods have been used to achieve new results?

Our conclusions differ from the premises of the editors of this Handbook. First, monetary and financial historians continue to study financial development and investigate the past for the sake of historical knowledge rather than "looking at the effect of past events on the current situation." Second, they continue to use models, "structural economic reasoning" and narrative methods to address key issues in the field. Although monetary and financial history has also been revolutionized by access to new microeconomic data, it makes limited use of statistical inference methods associated with recent development in applied microeconometrics. Difference in differences, quasi-natural experiments or discontinuity regressions are sometimes used successfully, but they have not become the gold standard.

There are good reasons for this. The description of sequences of events and general equilibrium perspectives remain key methods, rather than the identification of marginal effects, for studying the course of history (development and crises), building narratives and examining how the present has been shaped by history. Recent work also shows that important political and economic conclusions can be drawn from historical studies, without necessarily studying the direct effect of past events on current outcomes.

The history of financial development and financial crises remains the cornerstone of this field. Although these are old research topics, the recent work reviewed in sections 2 and 3 has gone considerably beyond traditional perspectives.<sup>1</sup> The basic chronology and typology of financial development has been questioned, leading to an approach that gives much more weight to the diversity of development models, financial systems, or central bank frameworks. The literature now focuses more on documenting and explaining this variety, recognizing that previous studies obsessed with the question of efficiency were often subject to a strong survival bias and centered on US and UK perspectives. The history of financial crises has benefited above all from a renewal of approaches that view the financial system as a network, making extensive use of the mass digitization of information on financial intermediaries. Rather than simply comparing aggregates, monetary history now focuses on the microeconomic sources of aggregate fluctuations.

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<sup>1</sup>We have focused on money and financial intermediation while setting aside related but vast areas such as public finance, sovereign debt, and exchange rates.

In reviewing this literature, we identify three different types of causal statements used by economic historians. Section 4 compares them: (i) *process tracing* (causality is identified by the description of the chain of events), (ii) the "*experimentalist*" *view of causality*—familiar to applied microeconomists and revolving around the ideal of a random experiment—and (iii) *structural or model-based causality*, which has a long tradition in macroeconomics and relies on historically informed theoretical relationships to construct counterfactuals.

Section 5 expands on this structural approach, explaining why it is essential for historical macroeconomics and how models are used in this case. Models are used for their narratives, whether to inform current debates or to better understand the past. From this perspective, a key question is whether the model used by the researcher is—and should be—consistent with the model that agents had in mind when making decisions. We illustrate this point with a review of the literature on the roots of high inflation in the 1970s.

In the course of our investigation, we realize that most of the discussion of the research topics and methods examined here can be traced back to Friedman and Schwartz (1963). They studied most of the topics and formulated most of the hypotheses currently common in the literature. They alternately used the three different definitions of causality that we still observe. Whether one disagrees with their conclusions or not, they still shape the how and why of our writings today.

## 2 Financial development

Historians of finance and money have devoted much energy over the last twenty years to debunking myths about the causes and typology of financial development. If we must name a common characteristic of these different studies, it has been to highlight the diversity of modes of financial development and not to consider that what has survived today is a good starting point for investigating the past.

### 2.1 The impact of legal regimes

Institutions matter for economic and financial development, but it is challenging to identify which ones really made the difference. The danger of a-historical methods is to look for institutions that were essential to the economy in the past through the lens of contemporary views about what is or is not important. As recently restated by Lamoreaux (2015), this creates a major survival bias, also named retrospective illusion, where much of the course of history is attributed to institutions that have survived or whose effects are likely to still matter.<sup>2</sup>

Legal regimes change only slowly, which makes them a plausible candidate to explain cross-country variations in financial systems. The "law and finance" theory of La Porta

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<sup>2</sup>She writes (in a paragraph about the research that focuses on the long-term economic effects of institutions): "The literature ignores cultural practices and institutions that do not have lasting effects, even though they may have been important shapers of economic behavior for long periods of time. At the same time, the literature simply assumes that practices and institutions that have significant associations with outcomes in the present were always important." (Lamoreaux 2015, 1255).

et al. (1998) argued that legal systems that protect investors will foster financial development, and found that common law countries have the strongest investor protections while Civil law countries have the weakest. The ensuing debate had a great influence on the development of financial history over the last twenty years, although the initial argument has ended up being almost universally rejected (see the survey by Musacchio and Turner 2013). Detailed studies of the way legal systems framed business and financial practices have shown that the development of financial institutions did not conform to the predictions of the theory on law and finance. Sgard (2006), Coyle, Musacchio, and Turner (2019), and Acheson, Campbell, and Turner (2019) show that the English financial market took off and achieved a high level of development in the 18th and 19th centuries without formal shareholder protection, against the argument that common law was better suited for creditors' right and spurred financial development. Lamoreaux and Rosenthal (2005), Sgard (2006), Musacchio and Turner (2013), Coyle, Musacchio, and Turner (2019), and Acheson, Campbell, and Turner (2019) demonstrate that countries with civil law could in fact had greater flexibility in business laws and forms of corporations than countries with common law. Focusing on bond markets and creditor protections, Musacchio (2008) showed large variations over time in bond market size, creditor protections, and court enforcement of bond contracts, within the *same* legal system; too large to support a direct causal link between a legal system and future financial development. Social and economic factors that shaped financial systems historically may have disappeared, such that their effect might be captured today by legal regimes in econometric estimations, although there was no actual direct relationship between the legal regime and financial development. Consider, for example, that colonialism played a key role in the development of the London financial market in the 19th century, in a context of weak protection of property rights (despite the common law). While economists seeking to prove historical causal effects tend to focus on sample size and exogeneity, economic historians point out that exogeneity is not sufficient to prove causality if there is no evidence of the transmission of the effect over time.

## 2.2 The many paths of financial development

Another instance of survival bias was the implicit belief that extinct forms of financial intermediation must have been inherently inefficient. The work carried out over the last two decades to reassess the traditional distinction between bank-based and market-based financial systems (Gershenkron 1963; Goldsmith 1969; Kindleberger 1984) has highlighted the importance of certain forms of financial intermediation that had disappeared or been neglected by previous studies. The view that emerges emphasizes the variety of financial development paths without reducing it to a simple dichotomy between two opposing models. The literature not only documents the role of intermediaries that were neither banks nor financial markets. It also shows that systems that were thought to be bank-based relied heavily on markets (as in Germany), whereas the role of markets in market-based systems must be reconsidered (as in the US).

In this area, the work of Guinnane (2001) on German credit cooperatives and of Hoff-

man, Postel-Vinay, and Rosenthal (2000, 2019) on French notaries have provided seminal references for further studies (see Cull et al. 2006 for a mid-term review). Drawing on different perspectives and studying different periods and countries, these studies show that, in many cases, long-term loans to households and businesses were granted or negotiated by institutions that were neither commercial (or universal) banks nor stock markets. Numerous studies have followed: Wandschneider (2015) on Prussian mortgage cooperatives, Gelderblom, Jonker, and Kool (2016) on direct finance and notaries in the Netherlands, Colvin and McLaughlin (2014) on credit cooperatives in the Netherlands and Ireland, Monnet (2018) on public credit institutions in France, etc. As Cull et al. (2006), Monnet (2018), and Hoffman, Postel-Vinay, and Rosenthal (2019) and others point out, lending institutions different from banks and stock markets are still very important today in emerging markets (where peer lending and government credit still account for a large share of total lending, especially long-term lending), although their role has diminished in Western economies. It is also striking that this type of institution has been extremely different from country to country. Summarizing historical research on financial systems, Fohlin (2012) thus concludes that new studies written since the early 2000s (in contrast to previous literature on financial systems) have undoubtedly shown that there is no “one size fits all” solution for the design of financial systems. Various forms of institutions have developed in response to local needs and in a way that is strongly influenced by specific economic and political contexts. Financial institutions that have succeeded in one context would not have succeeded in another. German cooperative banking, for example, prospered in the Netherlands because they could operate in niche markets without long-established incumbents, whereas they failed in Ireland where other institutions already performed similar functions in rural areas Colvin and McLaughlin (2014).

A large part of this new literature developed to modify the standard narrative of financial development that was based primarily on the expansion of financial markets in England and the United States. For this reason, researchers have devoted much effort to searching elsewhere, that is, outside the US and British cases and outside stock markets. But recent research on stock markets and on the US have also emphasized institutional diversity and the limitations of the standard narrative on financial developments.

Thus Hautcoeur and Riva (2012) points out that a highly regulated monopolistic stock market (the official Paris stock exchange in the 19th century) provided security and transparency in spot transactions while a free, innovative secondary market focused on forward liquidity, and that the two markets provided complementarity for investors. In examining the development of the Amsterdam Stock Exchange in the 18th century, Gelderblom and Jonker (2004, 2011) challenges the widely held view—based on the English example—that a liquid market for public debt was necessary for the development of financial markets. Lehmann-Hasemeyer and Streb (2016) shows the importance of the Berlin Stock Exchange in financing innovative technologies in a country previously characterized as bank-based. In the 19th century, large banks and the development of the stock exchange were not contradictory. The German case—among others—also shows the role of banks in the development of stock and bond markets through underwriting of securities (Lehmann

2014). In the international private and public debt markets, underwriters were also key players—although, in contrast to the German example, they were mainly investment banks (prominently Rothschild) rather than commercial or universal banks (Flandreau and Flores 2009). As for the US stock exchange, O’Sullivan (2016) has contested its role in the direct financing of US industrial development before the First World War, showing that it was in fact limited to the financing of the railways. This contrasted sharply with the London Stock Exchange, where industrial securities were widely traded.

### **2.3 The role of regulation**

Calomiris and Haber (2015) remind us how banking systems have been shaped by politics. Financial systems differ in part because they are subject to different regulations. In turn, regulation and state intervention have shaped financial weaknesses or strengths, as well as responses to crises.

Bordo, Redish, and Rockoff (2015) investigate why the US suffered from more banking crises than Canada in the 1930s. They demonstrate that most of the differences between the Canadian and US banking systems during the Great Depression were explained by the structure of the banking system and its supervisory policy (large oligopolistic banks in Canada versus unitary branches in the US). Likewise, within the US, the differences in bank suspensions in the 1930s were also partly explained by the differences in bank supervision (Mitchener 2005). These differences in US banking supervision can themselves be attributed to the legacy of 19th century policies and crises (Mitchener and Jaremski 2015). On the contrary, outside North America, bank regulation and supervision rarely existed before the Great Depression. This can be explained by the politics of the banking system and especially by the fact that politics created central banks on the one hand—providing liquidity during crises (Grossman 2010; Toniolo and White 2015)—and savings institutions on the other—protecting long-term savings (Degorce and Monnet 2020). Until the Great Depression, this was considered sufficient to protect savings and ensure banking stability.

The absence of banking regulation is not only studied as an historical phenomenon to be explained. Braggion, Dwarkasing, and Moore (2017) study the almost unregulated English banking system in the 19th century in order to test predictions of economic theory on the impact of bank competition on bank lending. They compare bank lending in areas with high and low bank concentration. A theory of lending with asymmetric information led them to examine loan quality, in order to establish whether the observed lower level of bank lending in highly concentrated countries is the result of higher credit rationing or lower demand. They assess loan quality by reading the bank’s comments on each loan, which are kept in archival records. The finding that loan quality increases as loan volume decreases leads them to conclude that the correlation between higher bank concentration and lower lending volumes is the result of higher credit rationing.

The history of banking regulation continues to be a very active area of research. Numerous recent articles aim at estimating the effect these regulation on economic outcomes.

Carlson, Correia, and Luck (2018) exploit a discontinuity in bank capital requirements during the 19th century National Banking era in the United States to investigate how banking competition affect credit provision and growth. Anderson, Paddrik, and Wang (2019) use original data on bank balance sheets and a network model to show that the introduction of the reserve requirements by the National Banking Acts of 1863 and 1864 in the US changed the nature of the financial links between banks. Calomiris and Jaremski (2019) also use a discontinuity in regulation (i.e. difference in regulation between similar groups of banks) to study the impact of deposit insurance prior to the creation of the FDIC, and Park and Van Horn (2015) to study the effect of reserve requirements in the 1930s. The discontinuity approach is particularly well suited to the history of the US banking system because banking regulation in the US is much older than anywhere else and notoriously multilayered, with different supervisors and rules applied to different institutions and locations (Toniolo and White 2015).

Although the case of pre-World War II United States has received much more attention than other countries in this literature, there is also a growing body of work on the post-war period and heavy banking regulation in countries where banks were nationalized and credit was mainly state-directed (Calomiris and Haber 2015; Monnet 2018; Musacchio and Lazzarini 2014). This kind of work also comes back to general questions about the relationship between financial development, regulation and policy, which are usually left aside in the US literature that focuses on the marginal effect of specific regulations. Calomiris and Haber (2015) build an analytical framework to explain how differences in financial development and financial fragility correspond to different political regimes and relationships between these regimes and the domestic banking sector. They apply their framework to the history of banking systems in the US, UK, Canada, Brazil and Mexico since the 19th century. The nature of the government-banker partnership varies among different types of political systems defined by the authors as "weakly centralized" or "centralized" autocracies, "populist" or "liberal" democracies. Since "banks need states" and "states need banks", there is a conflict of interest inherent in government-banker partnerships that makes the banking systems of most countries "fragile by design". But the sources of fragility depend on the type of relationship between the government and the banks. From this point of view, regulating or bailing out banks can reduce risk in the short term, but increase overall financial fragility by changing the relationship between the government and the banks and creating, for example, moral hazard or vested interests.

## **2.4 Central banking**

The literature on central banking has changed considerably in the last two decades. Hitherto it was largely conducted through the lens of modern, interest rate-setting central banks. To the extent that the past central banks were investigated, the focus had been mostly on the Bank of England as a prototype, and the late 19th-century gold standard as a regime in which the banks' role was to set interest rates. By contrast, much effort has been devoted in recent years to documenting and understanding the different models and



operational frameworks of past central banks, and how they eventually influenced the fate of financial development.

As central banks entered a world of quantitative easing and credit policies after 2008, economic historians turned to the archives to show that central banking was not fully captured by interest rate management. Rather, quantitative tools and collateral policies were an integral part of central bank history (Cerretano 2009; Monnet 2014, 2018; Jaremski and Mathy 2018). This opened the door to examining the variety of practices and how monetary authorities adapted their balance sheet policies to the needs of banks with which they had relationships. A more functional view of central banking has also led to study institutions that might not be central banks in the modern sense, but exercised the functions that are now in central banks' remit. Likewise central banks have in the past played roles that differ from theirs today. For example, in many countries central banks drove financial development by setting up nationwide branches and fostering local credit markets (Bazot 2014; Klovland and Øksendal 2017).

The mid-2010s brought a wave of bicentennial celebrations of central banks founded after the Congress of Vienna in 1815 that renewed interest in case studies outside the UK and the US (Bordo et al. 2016; Edvinsson, Jacobson, and Waldenström 2018; Jobst and Kernbauer 2016). Several comprehensive surveys (Roberds and Velde 2016a, 2016b, 2016c; Ugolini 2017; Bindseil 2020) have adopted a broad view of central banks and their predecessors the public banks of Europe, as institutions fulfilling a bundle of economic functions: not just setting monetary policy, but also the provision of liquidity services and reliable means of payment to the private sector, and of financing to the public sector. Another important lesson of this long-term history is that this last function made relations with governments perilous; survival often required these institutions to maintain a "gentlemanly distance" with their sponsors (see Karaman, Pamuk, and Yıldırım-Karaman 2019 for similar considerations in the case of monetary debasements). But to appreciate the inventiveness of these proto-central banks requires in-depth archival research, guided by a sharp understanding of the bundle of functions. Thus Quinn and Roberds (2007, 2014, 2019) have shown how the Bank of Amsterdam, the most advanced of its time, provided an irredeemable central bank money in the late 17th century and managed its value through open-market operations and the issue of repos. Quinn and Roberds (2015) found that it provided liquidity and prevented bankruptcies of major actors during the European crisis of 1763 described by Schnabel and Shin (2004).

Our understanding of the variety of lender of last resort policies has also tremendously progressed in the last decade. Economic historians have especially highlighted the various institutional arrangements of these policies: collateral requirements (Bignon, Flandreau, and Ugolini 2011), lifeboat operations vs. liquidity provision (Hautcoeur, Riva, and White 2014), secrecy on the identity of rescued banks (Gorton and Tallman 2018), etc. Investigations into the political economy of these policies are still in their infancy, however, and deserve to be deepened, especially on how private interests and social networks may have dictated the choice of collateral and the identity of the rescued institutions.

The history of central banking does not only involve central banks. Costabile and Neal

(2018) show how early modern Naples had a small and stable group of banks owned by charitable institutions providing the payments services that public banks had been created elsewhere in Europe to provide. The banks weathered successfully two hundred years of vicissitudes and were later merged to become the central bank of the Two Sicilies. The function of lender of last resort was not restricted to central banks, although only the latter exercised it on a large scale. Moen and Tallman (2000) and Gorton and Tallman (2018) highlight the quasi-central bank function of clearing houses in the US before the creation of the Federal Reserve. They limited counterparty risk and provided liquidity during crises. Bernstein, Hughson, and Weidenmier (2019) examine the effect of a clearing house on the stock market by looking at the creation of a clearing house on the New York Stock Exchange (NYSE) in 1892. At that time, the NYSE's largest shares were also listed on the Consolidated Stock Exchange (CSE), which already had a clearing house. Using identical securities on the CSE as a control, they find that the introduction of clearing reduced the annualized volatility of NYSE returns and increased asset values. Jaremski (2018) presents a more mixed picture of the clearing houses, showing that their introduction in the US states created negative externalities for banks that were not members of the clearing houses.

### **3 Banking and financial crises**

In recent years, the literature on the sources and consequences of financial crises has undergone four important developments that are partly interrelated. First, financial networks have been the focus of interest in order to identify how crises become systemic. Second, new digital technologies have revolutionized access to historical financial data (Mitchener 2015), allowing a more global view of financial systems and their interconnections. Third, investigations of the cost of crises—and of central bank policies mitigating or aggravating them—have relied more extensively on econometric identification and large microeconomic datasets. Fourth, in contrast with the monetarist approach of Friedman and Schwartz (1963), recent studies examine both the assets and liabilities of the balance sheets of financial intermediaries. Broad lending conditions and interbank markets are investigated together. Studies on the microeconomic sources and transmission of financial and banking crises are therefore consistent with the "credit view" highlighted in long-term perspective by Schularick and Taylor (2012).<sup>3</sup>

For economic historians, financial crises are a familiar phenomenon. Even before the 2008 global financial crash, few historians could have been tempted by the illusion that "this time is different". On the contrary, an abundant literature flourished in the 1990s, drawing parallels between the crises of the first era of globalization (at the end of the 19th century) and those of the new era of financial internationalization that followed the collapse of the communist world in the 1990s (Bordo et al. 2001; Bordo and Flandreau 2003; Obstfeld and Taylor 2005; Meissner, Taylor, et al. 2006; Esteves and Khoudour-Castéras 2009) The upheavals of the 1990s, from the Latin American debt crisis to the dot-com bub-

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<sup>3</sup>Although the historical macroeconomic literature has highlighted the strong link between financial crises and public debt crises, we keep the history of public debt out of the scope of this paper. A survey of the international historical macro literature on financial and fiscal crises is already written by Bordo and Meissner (2016).

ble, confirmed that the world had returned to a world of frequent financial crises, even though—looking back to the early 2000s—none of them had yet become fully global and as severe as the Great Depression of the 1930s. Credit booms and busts, asset price bubbles, sovereign debt crises and international contagion were the subject of landmark studies in international economic history in the years leading up to the 2008 crisis (Bordo and Meissner 2006; Schnabel 2004; Schnabel and Shin 2004; Neal and Weidenmier 2003; Eichengreen and Mitchener 2004). The banking and stock market crises of the Great Depression of the 1930s, particularly those in the United States, continued to be the subject of lively research (Rappoport and White 1994; Klug, Landon-Lane, and White 2005; Calomiris and Mason 2003; Mitchener 2005; Richardson 2007a, 2007b). Yet, the 2008 financial crash has considerably renewed the approach of financial and monetary historians and broadened the scope of their investigations.

### **3.1 Network and contagion**

As in many fields, the past is read through the lens of the present. The global financial crash of 2008, as well as the European sovereign debt problems of 2010-2012, prompted researchers to shed new light on past crises (by focusing on mechanisms or institutions that had been neglected before), often with the intention of fueling current debates. Important works on the causes and transmission of crises mainly use financial documents to reconstruct the links in the chain that led from a spark in the financial system to a systemic crisis. Thus, the new literature on financial crises after 2008 devotes much attention to the importance of financial networks and contagion. This approach is mainly based on very detailed archival research on the financial exposures of major banks (Richardson and Van Horn 2009; Accominotti 2012; Postel-Vinay 2016; Straumann, Kugler, and Weber 2017; Richardson and Van Horn 2018), or on microeconomic data allowing to reconstruct the links between networks and financial flows (Richardson 2007b; Carlson and Wheelock 2018; Mitchener and Richardson 2019; Anderson, Paddrik, and Wang 2019; Baubeau et al. 2020).

The focus on international contagion has renewed the study of the 1931 crisis, which was the main driving force behind the Great Depression. By examining the balance sheets of English merchant banks and the archives of the Bank of England, Accominotti (2012) shows that the devaluation of the pound sterling in September 1931 followed an exchange rate crisis whose roots were the financial exposure of London merchant banks to German merchants. The banking and economic crisis in Germany thus caused the pound sterling crisis, which in turn was a huge shock to the world economy. In the same vein, Straumann, Kugler, and Weber (2017) note that the banking crisis in Sweden was also triggered by Germany's insolvency because Swedish banks had accumulated short-term foreign assets. By contrast, detailed archival research led Richardson and Van Horn (2009, 2018) to find that New York banks were not affected by the Eastern European banking crises in the same year. This body of research is both instructive on the mechanics of a crisis in general and valuable in understanding one of the most dramatic economic crises in history.

The study of the links in a crisis chain through a detailed examination of the financial network is not limited to international contagion. Mitchener and Richardson (2019) examine the key role of the interbank network in transmitting deposit withdrawals through the US banking system and amplifying the contraction of lending during the Great Depression. The peculiar structure of the US banking system prompted hinterland banks to deposit certain reserves in large correspondent banks in financial centers. When hinterland banks began to experience withdrawals from depositors, they also withdrew their funds from correspondent banks. Although banks in the major financial centers did not face a run of depositors, they did face withdrawals from other banks, forcing them to reduce their lending. The authors show that the reduction in lending was in fact from two distinct sources. First, there was a general reduction in the assets of correspondent banks from which hinterland banks withdrew funds. Second, in response to liquidity risk, correspondent banks changed the composition of their assets and reduced the share of loans relative to the share of safe assets (government securities, cash and central bank reserves). Because of this change in behavior and asset composition, the volume of loans did not resume after the panic, when funds were returned to the correspondent banks. The essence of their argument is based on a detailed examination of the financial mechanics and interconnectedness. It is complemented by econometric analysis showing that indeed, interbank deposits and loans reacted after a bank panic.

### 3.2 Mass digitization

Easier access to historical records and, more importantly, the lower cost of digitization and data management have significantly changed the way economic historians can assess the macroeconomic effects of financial crisis. An unprecedented collection of microeconomic data on financial institutions or individuals enables researchers to adopt a holistic view of the financial system.<sup>4</sup> The article by Mitchener and Richardson (2019) cited above is only one study among many showing how more data can help tracking the chain of events. It is only a first step because this article did not make a full use of the geographic information contained in the data (Mitchener 2015). The data revolution was not a necessary condition for the increasing attention being paid to the financial network, but it is changing the game. Rather than simply comparing aggregates, monetary history now focuses on the microeconomic sources of aggregate fluctuations.

Compared to previous studies of financial crises, wider access to data allows researchers to see the financial system as a network. It has at least two important implications for a renewed historiography. First, as Mitchener and Richardson (2019) clearly show, it is a means of decomposing the transmission channels, which makes it possible to understand why macroeconomic variables evolve together. In this case, the authors renew the literature on the Great Depression because data on interbank linkages lead them to reinterpret

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<sup>4</sup>In an area different from that of financial crises, Flandreau and Jobst (2005) had preliminary used a network approach—also relying on a vast amount of data—to graph the connections between financial centers within the international monetary system. They present data on each currency quoted in each stock market around the world in the 19th century.

the causal link between money (deposits), credit and real activity that was at the heart of landmark studies such as those of Friedman and Schwartz (1963) and Bernanke (1983). Second, the wealth of data give researchers a comprehensive view of the financial system while previous studies were often limited to one specific segment of the financial system.

The study of French banking crises during the Great Depression illustrates how this new holistic approach of the financial system can renew the historiography. Baubeau et al. (2020) start from a fundamental—but rarely addressed—question for the history of banking crises: where do deposits go when depositors in a bank run withdraw them? For this country, the use of statistics of bank balance sheets was all the more limited because there was no banking regulation. Absent a banking supervisor, the article relies on private sources to collect the balance sheets of all major French commercial banks. Without comprehensive statistics, previous studies usually adopted a biased perspective focusing on the few large banks that did not fail. The availability of new data overturns previous knowledge: while the country's four largest banks were not affected (in fact, like the major banks in New York during the US crisis), the rest of the banking system experienced severe waves of withdrawals and a sharp drop in activity.

The authors then track the fate of deposits that were withdrawn from banks by panicked depositors. The approach is similar to the reconstruction of a flow of funds accounts. A necessary first step is to estimate whether cash hoarding was the main counterpart of withdrawals from bank deposits. Using a wealth of data from court records on bank liquidation and bankruptcies, the article then finds that the vast majority of depositors managed to withdraw their deposits before the bank closed, so that “frozen deposits” (i.e., losses to depositors) were small. Only one type of financial institution saw a significant increase in the flow of deposits during the crisis: government-guaranteed savings institutions (*Caisses d'épargne*). Overall, the nominal increase in *Caisses d'épargne* deposits was greater than the nominal decrease in bank deposits, but the two phenomena may have been unrelated and had independent causes. A mixture of estimates based on balance of payments statistics and qualitative records on the identity of depositors finally led the authors to conclude that during the two years of banking panic (1930-1931), the decrease in bank deposits was almost entirely due to withdrawals and transfers to the accounts of the *Caisses d'épargne*.

It is hard to imagine how this type of study could have been achieved without the recent significant reduction in the cost of digitizing archival data. In many ways, this is the continuation of the work undertaken by Friedman and Schwartz (1963) on the monetary history of the US, although their data were still limited and especially missed microeconomic information that helps to understand why macroeconomic aggregates move. The monetary histories of many other countries that followed the seminal publication of Friedman and Schwartz usually relied on even more limited data and focused on estimating money aggregates. By contrast, the new generation of studies that use digitized microeconomic data on banks and financial institutions has the means to go back to the questions initially raised by Friedman and Schwartz on the causes of large movements in monetary and financial aggregates. This is done primarily through reconstructing the linkages between the different parts of the financial and monetary network.

### 3.3 The cost of crises

While the studies mentioned above construct a narrative of the crisis by dissecting the sources and transmission of financial risk, another line of research aims to assess the economic impact of banking or financial difficulties. Rather than working at a general equilibrium level, this often means focusing on a narrower window to estimate this impact by identifying differences between a group affected by the crisis and a group not affected. These identification methods, also popular in empirical finance (Rajan and Zingales 1998; Khwaja and Mian 2008), have mainly used the fact that some firms or households were exposed differently to the financial turmoil than others. The source of the financial turmoil is not seen as exogenous to the overall economy, but the reason why some borrowers are affected differently from others by the crisis is deemed to be unrelated to the characteristics of the borrowers. It remains difficult for this literature, however, to link estimates of well-identified marginal effects to broad narratives of the crisis that take a holistic view of the financial system.

Recent papers study the effect of the US banking crises during the Great Depression using the identification method of Rajan and Zingales (Benmelech, Frydman, and Papanikolaou 2019; Gorton, Laarits, and Muir 2019): firms that relied more on external financing were more affected by banking crises than others. The challenge of this empirical method is to control for all the characteristics of firms that can be jointly associated with the choice of the composition of liabilities and the performance of firms. Xu (2018) applies the same type of identification procedure to measure the consequences of the English banking crisis of 1866 on international trade: all other things being equal, countries exposed to banks with headquarters in London that failed in 1866 exported less in the following decades.

Frydman, Hilt, and Zhou (2015) go further by using information on financial networks as a source of identification. They study the consequences of a run on trust companies in New York during the 1907 panic. Their identification is based on the fact that the withdrawals were caused by the identity of the trust company but were not related to the financial health of the companies financed by the trust. Based on qualitative and quantitative archival information, they establish that much of the variation in deposit losses among the New York financial trusts was due to their associations with individuals involved in speculation in the shares of a mining company (United Copper Company). When stock prices of this company fell, households feared that their deposits were threatened and ran on trusts whose directors had been involved in the speculation. The cause of these withdrawals was therefore not related to the financial health of the trust companies' corporate clients. The companies that borrowed from the trusts that suffered from the rush are akin to a treated group, whose main access to long-term credit was suddenly cut off. The author find that companies affiliated with the most affected trusts made fewer capital investments in the years following the panic.

### 3.4 Central banks to the rescue

As the history of the 1930s reminds us, crises are all the more costly without emergency liquidity provision by a central bank. Richardson and Troost (2009) used a quasi-natural experiment to support this interpretation. The experiment is based on Mississippi (United States) being divided into two Federal Reserve districts, which followed different policies of lender of last resort during the banking panic of 1930. In the district of Atlanta, the central bank provided liquidity to banks in trouble, whereas the central bank in the district of St Louis did not. The two parts of Mississippi were similar and the distinct policies of Atlanta and St Louis were not motivated by different characteristics of these two parts. The authors find that the failing rate of banks was lower and commercial activity remained higher in the part of Mississippi that was in the district of Atlanta. This is a rare example of an historical policy that is akin to a controlled experiment with one group that receives a treatment while the other do not. In this case, econometrics evidence provides important robustness checks but the effect was immediately observable by comparing the means of the two groups.

In another article about the lender of last resort role of the Federal Reserve, Carlson, Mitchener, and Richardson (2011) study how the Federal Reserve Bank of Atlanta halted the spread of a banking panic in 1929 by rushing currency to member banks. The panic was caused by an exogenous event (a fruit fly infestation that created bank runs in citrus growing regions) so that the intervention of the central bank is viewed as independent from pre-crisis characteristics. The authors use simple models to predict the failure rate of banks during the panic. Building on these counterfactuals, they conclude that bank failure rates would have been twice as high in Florida without the Atlanta Fed's decision to provide funds to the key correspondents in Tampa that were facing runs. Contrary to Richardson and Troost (2009), this paper does not rely on a comparison between two groups but on a counterfactual, using a simple estimated model to simulate what would have happened without central bank intervention. This counterfactual has important policy implications because it implies that if the Federal Reserve System had reacted in 1930 and 1931 as the Atlanta Fed did in 1929, it could have mitigated the consequences of banking panics during the Great Depression.

## 4 Different types of causal identification

How do financial and monetary historians deal with causality? Most of the literature reviewed in the previous sections makes little use of the methods associated with the "credibility revolution" in applied microeconomics (Angrist and Pischke (2010)). This is sometimes the case when studying the effect of banking regulation or banking crises. In these cases, economic historians have successfully relied on instrumental variables, discontinuity regressions, quasi-natural experiments, or difference in differences to identify exogenous variations and estimate the marginal effect of a crisis or policy intervention on one specific group, relative to another. Yet this is not a dominant trend of research. Economic historians also use other methods to study the effect of policy interventions and crises,

such as narratives or model-based counterfactuals. More importantly, when it comes to key topics such as the sources and characteristics of financial development and financial crises, the literature relies on other methods and definitions of causality than those used in applied microeconomics.

Below we distinguish three main types of causality used in the literature in financial and monetary history (and more generally in economic history). We begin by "process tracing", which is a common method for introducing causal reasoning into narrative analysis. Then we compare this method to the "experimentalist" or "interventionist" definition of causality that is currently familiar to microeconomists. Third, we present the structural approach that is mainly defended in macroeconomics and that is still widely used by historians of money and finance. The following sections of this chapter will explore in more detail how this structural, or model-based, approach to causality is used in history and how it is combined with narratives.

## 4.1 Process tracing

The above-mentioned studies on financial development and financial contagion use causal statements when reconstructing the chain of events. Detailed qualitative and quantitative information on the timing and volume of financial flows as well as financial exposure, for example, allow researchers to assert that one event or set of events was instrumental in triggering other events. Other assumptions or confounding factors are considered, but are eventually rejected by information on how the process actually unfolded. This definition of causality is common in history and the social sciences and corresponds to what Salmon (1998) calls a theory of process causality, or what has recently been reframed as "process tracing" by political scientists (Collier 2011; Bennett and Checkel 2015). As explained by Salmon (1998), the identification of such causality does not imply a counterfactual. It relies on the description of the chain of transmission .

In the studies by Richardson and Van Horn (2009), Accominotti (2012), Straumann, Kugler, and Weber (2017), and Richardson and Van Horn (2018), the authors are thus able to determine whether a crisis in one country triggered a crisis in another country by examining financial linkages. In a very different context, Bordo, Monnet, and Naef (2019) examine the dual crisis of the pound sterling and the US dollar in 1967, by exploiting both series of foreign exchange interventions and archival information on buyers of gold at the US 'gold window' to demonstrate that sterling crisis triggered the dollar crisis. By contrast, standard studies on financial contagion that merely examine the correlation between asset prices are not able to distinguish between correlation and causality. Only a precise description of the sequence of financial and political events can assess the direction of causation in this case. Similarly, by reconstructing financial flows and establishing a detailed chronology of bank panics, Mitchener and Richardson (2019) show that bank panics in the US hinterland were one of the causes of bank distress in large cities (through the interbank system).

The most popular definition of causality in microeconomics is based on the identi-



fication of an exogenous shock and the maintenance of constant characteristics between groups (see below), without necessarily observing the sequence from shock to outcome. In contrast, process tracing is a descriptive inference where a precise characterization of the sequence is the key to asserting causality. The scientific criteria used to evaluate sequence description depend on the type of material available to researchers. In political science and international relations, for example, qualitative information on the decision-making process of policy makers is essential for adequate description and identification (Collier 2011; Bennett and Checkel 2015). In financial and monetary history, as the examples above show, evidence of financial linkages between institutions is central to identification. By examining these linkages, researchers are able to assess causality (i.e. the chain of events) rather than simply observing the correlation between macroeconomic aggregates. It can also be supplemented with information on policy decisions where relevant.

In their comparative study of politics and financial development in history, Calomiris and Haber (2015, 451) justify their approach to causality in the following way in order to distinguish it from the standard identification restrictions that guide causal inference in microeconomics:

At the same time, we believe that the country histories in this book illustrate the usefulness of an alternative, complementary approach, based on the study of the sequence of events in particular countries over long periods of time. Such narratives can be uniquely useful for identifying causal patterns, so long as they are more than a string of facts. [...] Our approach, therefore, is to develop “structural narratives,” which combine the logic of economics and political bargaining with a careful examination of the specific historical events in individual countries.

An interesting avenue for further research, raised by Calomiris and Haber (2015) among others, is to evaluate the use of economic theory in characterizing the chain of events. In other words, how much does process tracing need theory to order the facts? When examining a sequence of financial flows, economic historians often start from theoretical considerations. For example, both Mitchener and Richardson (2019) and Baubeau et al. (2020) have used standard monetarist theory as a starting point to guide their empirical research and test alternative hypotheses, even if they end up showing their limitations. In a paper that exploits individual data on the exposure of lenders to a syndicate that went bankrupt in Amsterdam in 1772, Koudijs and Voth (2016) used a model to interpret the change in investor behavior, namely the impact of risk on haircuts and interest rates. They show that only those who were at risk of losing money changed their behavior significantly (they lent at much higher haircuts). Although the links of the chain and individual financial exposure are adequately described, the authors need prediction of financial theory to infer the consequences of individual behavior from the data.

## 4.2 Credibility revolution and interventionist causality

When possible, detailed historical research on financial linkages also follows the definition and identification of causality put forward in the applied microeconomics literature (recently labeled “credibility revolution” by Angrist and Pischke (2010)), and known in the epistemology literature as “interventionist”, “manipulationist” or “experimentalist” causality (Reiss 2005; Heckman and Pinto 2014; Bourgeois-Gironde and Monnet 2017). The use of these terms is nevertheless sometimes confusing because other authors use the term “interventionist” to denote definitions of causality that are model-based (see below). Contrary to structural models, the “interventionist” or “manipulationist” definition of causality associated with applied microeconomics and natural experiments relies on the agency of the researcher rather than on model-based relationships. The randomized experiment ideal is the guiding principle of this definition of causality. In this perspective, causation can be identified because the researcher is able to design an experiment, give a treatment and observe its effects. In the absence of controlled or natural experiments similar to the one used in Richardson and Troost (2009), economic historians study the differences (all else being equal) between the institutions that were most likely to suffer from a crisis, for example. The main disadvantage of this approach is that if it is not complemented by a detailed account of the crisis, or supported by a structural model, there is no understanding of the mechanics of the crisis and thus no possibility of making it interesting for historians of a given period, nor any means of generalizing the conclusions.

This type of causal identification can nevertheless be combined with a detailed narrative of the sequence of events and, in this case, be illuminating for historians. The study of Frydman, Hilt, and Zhou (2015) mentioned above is a good example of microeconomic identification based first on a detailed analysis of the sequence of a financial crisis and the links between financial institutions and firms. It combines a causal analysis based on process tracing to highlight the cause of the crisis, and then uses an interventionist definition of causality to examine how the crisis had different effects on different groups.

## 4.3 Structural causality

Another definition of causality that is widely used in macroeconomics and economic history is structural or model-based causality. It will be discussed further in the next section. This type of causality relies on a model of structural equations that specifies the relationship between variables (for an introduction to the large literature on structural causality in philosophy of sciences, see Heckman and Pinto 2014; Hitchcock 2020). Although a variable is usually endogenous in the model, the researcher can simulate an exogenous shock to one of these variables. This is where the causal process originates. The model, through the structural equations, predicts the impact of this shock on other variables. Several events in history can be interpreted as “shocks” if it can be proven that they are not mere consequences of the changes in other variables of the model. The researcher can either use the model to assess the historical consequences of this shock, or, alternatively, compare the historical outcomes (if directly observable) to the ones predicted by the model. Counter-

factual reasoning is essential to this definition of causality.

Contrary to “process tracing”, the causality is assessed through theoretical relationships rather by a full description of the sequence. When the researcher does not have access to sufficient empirical material to document the sequence of events, it is by relying on a model and theoretical assumptions that she can fill in the gaps between unrelated a priori facts. Contrary to difference-in-difference or natural experiments, there is no control group: the counterfactual is specified by the model rather than by the existence of two groups (treated vs. untreated).

Anderson, Paddrik, and Wang (2019) provide a recent landmark investigation of the link between banking regulation and financial stability using a structural network model on the US banking system in the 19th century. Thanks to unique data on bank balance sheets, they reconstruct linkages between banks, before and after the passage of the National Banking Acts (NBAs) of 1863 and 1864. The NBAs established legal reserve requirements that allowed banks to maintain a large portion of interbank deposits in designated cities, thereby creating a reserve pyramid. In a second step, they build a structural network model of the system that they calibrate using historical data. Then, they simulate financial shocks in the model and assess how the transmission of liquidity shocks differs before and after the NBAs. Contrary to Mitchener and Richardson (2019), the authors do not reconstruct the precise sequence of events during a crisis. They do not in fact study actual crises. Instead, they use a calibrated model to simulate crises in order to study how banking regulation changed liquidity transmission. It provides important conclusions on both the process of US financial development and current issues for banking regulators: concentration of interbank deposits facilitated diversification but contagion was thus more likely when financial center banks faced large shocks.

As it will become clear in the next section, the application of structural models to history requires the use of a lot of qualitative information to justify the hypotheses and main structural relationships on which a model rely. For this reason, major works in monetary and financial history have combined the use of theory and of narratives based on archival evidence to make sense of a particular historical period of event.

## **5 Historical Macroeconomics and the Science of Story-Telling**

Our survey shows that the “modern methods” have found limited use in monetary, banking, and financial history. Broadly speaking, researchers in this subfield of historical economics have used the methods to shed strong but narrow light on a particular piece of a broader mechanism. There is very little emphasis on natural experiments and differences-in-differences estimation, as well as a general absence of long-run regressions stretching over centuries.

Why? Setting aside the hypothesis that those researchers respond less to obvious incentives, the rest of this chapter speculates on the reasons. Macroeconomics in general has embraced these methods to a lesser degree, and used them differently when it has, than the rest of economics. We think the reasons apply as well, if not more, to historical

macroeconomics where models have structured narratives for a long time and continue to do so.

## 5.1 The macroeconomic tradition

Modern macroeconomics is built on tradition. The econometric theory of Haavelmo (1944), Marschak (1953), and the Cowles Commission in the 1940s and 1950s gave the first scientific foundation for the analysis of economic data. The ultimate goal of the program, which began during the Great Depression, was to provide sound policy advice, that is, credible counterfactuals.

The first step is to separate the *explananda*, the endogenous variables we are in the business of explaining, from the *explanantia*, the exogenous variables we do not try to explain and take from engineers, psychologists, lawyers, historians.

For the economy as a whole, endogenous variables can be roughly identified with what are often called “economic variables.” These are usually the quantities (stocks or flows) and prices of goods and services, or their aggregates and averages, such as national income, total investment, price level, wage level, and so on. The exogenous variables and the structural parameters are, roughly, “noneconomic variables” (also called “data” in the economic literature) and may include the weather and technological, psychological, and sociological conditions as well as legal rules and political decisions. But the boundary is movable. Should political science ever succeed in explaining political situations (and hence legislation itself) by economic causes, institutional variables like tax rates would have to be counted as endogenous Marschak (1953, 10).

Macroeconomics differs from the rest of economics in its concern for economy-wide aggregates and its natural framework is the general equilibrium theory of Arrow and Debreu (1954), Debreu (1959), and McKenzie (1954)—indeed, it may be the last field of economics where general equilibrium theory is still cherished. In that framework Marschak’s “non-economic variables” are specified as preferences of agents defined over a space of goods, their endowments of goods, and the available means to turn goods into more or other goods (technology). His “legal rules and political decisions” can be described in the constraints on agents’ choices and transactions (taxes) or in the resource constraints (government spending, money creation, etc). The model is solved by a set of endogenous variables that satisfies the conditions expressing the purposeful behavior of agents and the coherence of their actions. These conditions involve the structural parameters  $\theta$  that characterize the exogenous objects of the model, and the endogenous variables  $p$  to be predicted. The equilibrium conditions  $F(p, \theta) = 0$  implicitly define the vector  $p$  as a function of the vector  $\theta$ .

What is causality in this framework? As Heckman (2000, 46) puts it:

Just as the ancient Hebrews were “the people of the book,” economists are “the people of the model.” Formal economic models are logically consistent systems

within which hypothetical “thought experiments” can be conducted to examine the effects of changes in parameters and constraints on outcomes. Within a model, the effects on outcomes of variation in constraints facing agents in a market setting are well defined. Comparative statics exercises formalize Marshall’s notion of a *ceteris paribus* change which is what economists mean by a causal effect.

Thus, while  $F(p, \theta)$  only defines a mapping from the complete vector  $\theta$  to the complete vector  $p$ , one can conceivably vary only one element of  $\theta$  to trace its effect on one or more elements of  $p$ . Causality is postulated by the equations of the model, and causal effects are the outcomes of thought experiments. But the general equilibrium framework, and its implication that all elements of  $p$  are simultaneously determined, places the problem of simultaneous equations (Haavelmo 1943) at the heart of econometric work.

Time and uncertainty do not change the notion of causality although they complicate the interpretation of observed outcomes (Lucas and Sargent 1981). Exogenous variables must now be specified as stochastic processes whose whole future paths enter into agents’ calculations, and the model needs to take a stand on agents’ information sets and beliefs.

Macroeconomics, whether in the DSGE tradition, or in the multivariate time series models restricted by economic reasoning (Sims 2010), has remained much more structural in nature than other areas of economics (see the survey in Nakamura and Steinsson 2018). To the extent that quasi-experiments are useful, it is to illuminate one particular “block” or portion of a model, or to derive estimates of a parameter that can be then used in a fully structural model. The tradition of using historical narrative to identify exogenous shocks in the spirit of Friedman and Schwartz (1963) continues (Romer and Romer 2004) and use them to evaluate competing models and discriminate between them. But “natural” experiments have their “natural” limits outside of models (Fuchs-Schündeln and Hassan 2016).

## 5.2 History and macroeconomics

When history and economics meet, particular challenges arise.

There are two broad, not mutually exclusive modes of interaction between history and economics. The first is to think of the historical record as a large warehouse of data on which we can draw to test our theories, more or less formally. This can be useful for macroeconomics, since observations about aggregate economies are necessarily fewer than for particular microeconomic questions, and particularly useful for infrequent phenomena such as large-scale financial crises. The confrontation with past experiences can also expand our thinking by presenting us with events, arrangements, or outcomes that we might not have thought of in the first place (Abramitzky 2015).

The two disciplines can interact in a second way. If historians want to be more than annalists, instill some order into apparent chaos, and shape a jumble of disconnected facts with a narrative then a causal process can serve as an organizing device for the flow of

history.<sup>5</sup> When the events are economic in nature, economic theory provides the logical structure of that process.

Historical analysis, however, creates particular challenges for the framework used by macroeconomics, and details that were set aside become prominent.

First, history unfolds in the flow of time, and in that flow agents look forward. Their expectations or beliefs become salient. Second, that flow must always be interrupted (the narrative has a starting point) but in truth there is no pure  $t = 0$ , endowments and beliefs are products of  $t < 0$ . In a simple Markovian world, we know all we need to know at  $t = 0$  to understand the future. But chains of causation can run through these products. The counterfactual that necessarily follows from quasi-experiments must be compatible with the chain of events for  $t < 0$ : e.g., what would Germany have been if history had been such that the French Revolution did not happen? This is particularly true when the quasi-experiment is in fact run by a purposeful agent such as the government, which often looms large in macroeconomic models (see also Chemla and Hennessy 2020).

Second, while the partition between endogenous and exogenous is fairly straightforward for the first elements in Marschak's list (preferences, endowments, technology, delivered by psychologists and engineers), matters become more complicated when we come to the offerings of the lawyers and historians. We have to specify details about information (who knows what and when), timing (who moves first), market structure (who trades with whom, outside of complete markets), which promises can be made and will be enforced. In the case of monetary, banking, and financial history this is simply because in a complete markets model there is neither need nor room for money, banks, or finance (except the anonymous trading of perfectly safe Arrow securities).

Much of this has been labeled "institutions," even outside of money, banking, and finance, for a natural heuristic reason. The concept of causality relies on generalization: as Hume famously said, "the same cause produces the same effects," but with historical macroeconomics we do not have myriads of instances where the "same" cause is readily identified. For the inductive method to work, a collection of events  $\{x_i\}$  must become a class  $X$ . The challenge is to do so while remaining sensitive to the specificity of each history—generalize just enough to function in the "middle terrain" between economics and history identified by Lamoreaux (2015). "Institutions" serve that heuristic purpose of generalization, without necessarily having recourse to a model.

But what are institutions? The simplest model is a set of constraints on agent behavior, or an exogenous collection of payoffs: this can be an explicit penal code or norms of behavior, that is, exogenously restricted sets of expectations about other agents' actions. A more subtle interpretation would make the constraints endogenous, i.e., spell out the game in which agents find it in their interest to abide what seem to be constraints when only equilibrium paths are observed (e.g., Chari and Kehoe 1990).

Remarkably, while many of the long-run effects studies in historical economics rely cru-

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<sup>5</sup>"On fait la science avec des faits comme une maison avec des pierres ; mais une accumulation de faits n'est pas plus une science qu'un tas de pierres n'est une maison" [Science is built up with facts, as a house is with stones; But an accumulation of facts is no more a science than a heap of stones is a house] Poincaré (1902), cited by Calomiris and Haber (2015, 451).

cially on institutions and have appealed to the so-called “New Institutional Economics” (NEI) to play up their importance, the concept plays an ambiguous role. NEI often burrows (perhaps too) deeply into the logic of institutions: why do they exist, what purpose do they serve, why do they change. For the purposes of quasi-experiments their role is exogenous: an institution is randomly changed, and the consequences are observed. Yet the motivation is typically interventionist in nature: policy-oriented conclusions are drawn on the merits of the institutions, implicitly at least to argue that some institutions are preferable to others. But if institutions can change as a result of the researcher’s policy advice because some are better than others, could they not have changed in the course of history, for the same reason? As Marschak noted in the quotation above, institutions are near the shifting boundary between exogenous and endogenous. The boundary can shift (permanently) because of scientific progress, but it will also shift depending on the topic and its time-frame.

Institutions are, by nature, stable: we wouldn’t call them by that name if they weren’t. Yet vary across space and change over time, slowly or abruptly: that’s why they are used as vectors of causation. Setting institutions as exogenous and taking small, partial variations as quasi-random may well be adequate for very short horizons. But the appropriate time frame for historical macroeconomics in money, banking, and finance is awkward: too short to ignore them altogether as background noise, too long to see them as fixed. In this time scale, institutions can both matter and change at the same time. If they change durably, the first suspicion must be that they change for some solid reason. Even when the change is prompted by an event that might look random if one doesn’t look too closely (say, being invaded by French armies), the next suspicion must be that the change stuck for some reason. It is not enough to drop a legal code onto a defeated nation to create a natural experiment. Nor can changes be always summarized by a crisp list of enacting dates, as if institutions were a panel of toggles and switches.

### **5.3 Models to structure a narrative: some examples**

The nature and tradition of macroeconomics, as well as the added challenges of historical macroeconomics, help understand which tools have been used or not. Historical macroeconomics has its own tradition as well.

Friedman and Schwartz (1963) remains one of the most important texts for modern macroeconomists. They used high-powered money and two ratios (the deposit-to-reserve ratio, and the deposit-to-currency ratio, broadly capturing the behavior of the banking system and the public respectively) as “proximate determinants” of the stock of money. The core theoretical principle that makes it possible to construct a ninety-year history of the US macroeconomy on this basis, is the non-neutrality of money in the short run, and its neutrality on the long run. Variations in the money stock are used to account for departures from a trend impervious to those variations, in other words business cycles, and the proximate determinants are used to trace these variations either to monetary authorities or to shocks in the private sector (such as banking panics). But the book is not just a col-

lection of tables and graphs: a detailed history is woven onto that frame, using as threads the documented motives and actions of the players. The normative implication is strong: whatever the source of monetary contraction (endogenous or exogenous), it would have been preferable for monetary authorities to counteract it. While the theoretical apparatus was incomplete, as Tobin (1965) pointed out, its impact was long-lasting because “it served the purpose that any narrative history must serve: it told a coherent story, and told it well” (Lucas 1994).

The study of hyperinflations by Sargent (1983) came at a time when the rational expectations hypothesis was increasingly used to account for agents’ expectations. In models with time and uncertainty, agents’ perceptions about future events could not be ignored, and assuming rational expectations was a way to specify them in a coherent way: agents know the model no less than the modeler. The implications for current debates about the costs of bringing inflation under control were crisply summarized by Paul Samuelson: the proponents of the rational expectations hypothesis “are optimistic that inflation can be wiped out with little pain if only the government makes credible its determination to do so. But neither history nor reason tempt one to bet their way” (cited by Sargent 1983). Sargent took up the challenge: what did history have to say? Cagan (1956) and Sargent (1977) had studied the four post-World War I hyperinflations of Austria, Hungary, Poland, and Germany: Sargent (1983) studied their ends, and also considered the absence of hyperinflation in Czechoslovakia. He showed that the hyperinflations stopped abruptly once fiscal regimes had been changed and central banks prevented from financing deficits. Money stocks continued to grow but exchange rates and price levels stabilized nearly overnight.

In another broad narrative in monetary history, stretching over centuries, Sargent and Velde (2002) use a model to understand why the dysfunctions of commodity money systems in medieval and early modern Europe. Money consisted of gold and silver coins of various sizes. The coins were supplied by mints operating under specific rules: coins were provided on demand in exchange for metal at a stated price, providing an upper bound on their value. The ability to melt coins in excess of the transaction needs provided a lower bound. The system did not seem to supply coins of various denominations in adequate quantities, as recurrent shortages of small change indicated. Yet the solution, making all coins but the largest into tokens and pegging their value, was not widely known and adopted by the end of the 19th century. The model has the fewest elements necessary: it relies on two cash-in-advance constraints to generate distinct demands for small and large coins for the purchase of small and large goods. A shortage of small change is modeled as a binding “penny-in-advance” constraint. The model generates surprising results: when a shortage occurs, the large coin appreciates: the capital gain on the large coin compensates for its reduced usefulness in transactions, relative to the small coin. The prediction accounts for a secular trend of large coin appreciation relative to the small coin that is borne out in the data. The model can also account for instances when the solution was partially implemented: small coins made into tokens but without pegging their value, with resulting inflation. The simplicity of the solution contrasts with the time it took to implement it, and directs attention to the possible impediments: the concept of token coinage (or fiat



money) and the technology to make counterfeit-proof tokens both had to be invented.

Velde and Weber (2000) study the existence and desirability of bimetallism as a monetary system, questions raised by Friedman (1990). Their model shows that bimetallism was not a knife-edge system oscillating between gold and silver, but allowed the two metals to circulate as money at an indeterminate (within bounds) parity. Their calibration suggests that bimetallism was not doomed to disappear when it did in 1873, in spite of the recent gold discoveries. The model they construct, in the tradition of Walras (1889) and Fisher (1911), also allows them to compute welfare, and they find that the steady state welfare cost of bimetallism (the value of metal in monetary form) is higher than that of monometallism.

## 5.4 Experiments?

The language of experiments permeates the tradition of historical macroeconomics. Friedman (1953, 11) (cited by Bordo and Rockoff 2013) uses it explicitly:

Occasionally, experience casts up evidence that is about as direct, dramatic, and convincing as any that could be provided by controlled experiments. Perhaps the most obviously important example is the evidence from inflations on the hypothesis that a substantial increase in the quantity of money within a relatively short period is accompanied by a substantial increase in prices. Here the evidence is dramatic, and the chain of reasoning required to interpret it is relatively short.

Likewise, in their conclusion (Friedman and Schwartz 1963, 696) write:

The varied character of U.S. monetary history renders this century of experience particularly valuable to the student of economic change. He cannot control the experiment, but he can observe monetary experience under sufficiently disparate conditions to sort out what is common from what is adventitious and to acquire considerable confidence that what is common can be counted on to hold.

And Sargent (1983) also concludes: “The four incidents we have studied are akin to laboratory experiments in which the elemental forces that cause and can be used to stop inflation are easiest to spot.”

The metaphor can be misleading: the process remains one of abstraction, in order to generate visible patterns. Sometimes the historical circumstances sharpen the focus and simplify the pattern, making abstraction easier. This is not, however, fundamentally different from the tradition of business cycle analysis from Burns and Mitchell (1946) through Prescott (1986) and Galí (2008), which abstracts from specific time paths to discern patterns: but the quest for patterns remains guided by a model.

Velde (2009), perhaps misleadingly, also uses the language of experiment in describing particular monetary policy actions taken in 1724 France.<sup>6</sup> Specifically, the actions formed a sequence of three overnight, unannounced reductions of the money supply in exact proportion to agents' money balances. The response of prices and output, and the reactions of contemporaries, are documented from archival sources. Although the word "experiment" is used repeatedly, there is no claim whatsoever that the 1724 episode is "natural", let alone plausibly exogenous or quasi-random: the policy was clearly not exogenous (it was a response to elevated prices) and it was unannounced but not necessarily unanticipated—indeed, expectations play a prominent role in contemporary explanations of price movements, and in the government's own rationale for the last of the three reductions. There is no control group and no cross-sectional variation to exploit: the whole country was subjected at once to the same policy, and the government took care that the policies be announced at exactly the same time everywhere.

The government's experiment was carefully designed, but not to simulate a pharmaceutical study. If it serves us, researchers, as an experiment, it does so in the sense of a thought experiment, such as those that Hume used to elucidate his reasoning about monetary non-neutralities (citing the same 1724 episode). What makes the story interesting is that the response of prices was not the one predicted by the government of the time, nor is it the one that follows from many standard models. Additionally, the circumstances do not satisfy assumptions (such as menu costs or rational inattention) commonly used to generate monetary non-neutralities. In short, the contribution, if any, is not to establish causation: it exists only within the context of models.

## 5.5 Off-the-rack or bespoke models

The examples of the previous section have in common that they use models of their time for their narratives, whether to shed light on current debates or to better understand the past. There are recent examples in this vein. Chen and Ward (2019) use a fairly standard macroeconomic approach to understand why economies under the gold standard (i.e., fixed exchange rates) responded to external shocks with little output loss. They include several possible channels (flexible prices, labor mobility, monetary policy) in an otherwise standard model, calibrate some parameters, estimate the others, and simulate the estimated model economy while counterfactually shutting down some channels to assess their relative importance. Price flexibility turns out to be the most important channel, and the primary sector, with the most flexible prices, dominated exports. Palma and Silva (2016) calibrate a general equilibrium model of the world economy to understand trade and metal flows between Asia and the rest of the world in the early modern period, and find that the metal discoveries in the New World vastly outweigh the lower trading costs to explain the increased trade.

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<sup>6</sup> Angrist and Pischke (2010, 19) say it is as close to their sort of quasi-experimental work and one of the "rays of sunlight" poking through the "grey clouds" of DSGE. The author might be allowed to plead that it is, in fact, a counter-example.

This strategy is not always available. A strong theme of our survey is the diversity of arrangements, institutions, policies, and outcomes; unsurprising given that money, banking, and financial intermediation all presume some market incompleteness. Lucas (2013, 272) famously paraphrased Tolstoy's observation about happy families to say that "complete market economies are all alike, but each incomplete market economy is incomplete in its own individual way." By "in its own way" Tolstoy, if not Lucas, meant that each has a different history.<sup>7</sup> There are examples of scholars using bespoke models in macroeconomic and financial history: Anderson, Paddrik, and Wang (2019) on bank networks in late 19th century US, Börner and Hatfield (2017) on medieval fairs, Koudijs (2015) on insiders in 18th century financial markets, The common approach is to tailor carefully a model built on standard theory in order to understand a set of events or a particular institution. The placement of such papers should be an encouraging sign for those not quite committed to the methods of quasi-experiments and long-run regressions. But more off-the-shelf, well-worn models of banks, markets, and financial systems will help.

## 5.6 Models arguing: the Great Inflation debate

In the 1990s and early 2000s there was an active debate about the so-called "Great Inflation" in the US, namely the rise and fall of inflation from the 1960s to the 1980s. While this might not qualify as quite historical (since macro-history seems to start with quarterly NIPA accounts in 1946), it involved some of the most prominent macroeconomists, yet its focus was a single sequence of events and its goal is to understand the process driving them.

Based on a broad narrative (mixing analyses of speeches, writings of policymakers and economists), De Long (1996) argued that the Great Inflation was due to the very high weight assigned to unemployment by monetary policy ("The memory left by the Depression predisposed the left and center to think that any unemployment was too much"). The measures to fight inflation starting 1979 are thus interpreted as a shift in beliefs.

Sargent (1999) took up De Long's idea, deepened the narrative and built a model to account for the change in policymakers beliefs and their impact on economic outcomes. Orphanides (2002) brought "archival material" into play to argue that policy actions can be explained by misinterpretations of the current data. Romer and Romer (2002) used extensively the archives of the Fed to provide evidence of the change in policymakers' beliefs over time. Based on records of the discussions at the Fed, they emphasized that it was not so much a too high weight on unemployment that led the Fed to run inflationary policies in the 1970s (because they adopted early the natural rate of unemployment theory) but an underestimation of the natural rate of unemployment.

In his discussion of Romer and Romer (2002), Sargent (2002) acknowledged the usefulness of their work but criticized the main conclusion based on the narrative approach

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<sup>7</sup>Tolstoy was inspired by the French proverb that happy people have no history, which in turn seems to derive from the dictum of D'Alembert (1779, vii): "happy [fortunate] are the people with a boring history!" Over time the implication was reversed; then Tolstoy, as a novelist, redirected the statement from aggregates to households and from history to stories.

because it lacked the kind of quantitative analysis that could take into account other parameters, especially the variety of shocks hitting the economy. That being said, Sargent included himself in the “Berkeley” view, together with the Romers and De Long, for emphasizing the importance of the changes in beliefs as an explanation of the Great Inflation. Cogley and Sargent (2005) responded to Romer and Romer (2002) by incorporating model uncertainty. They built a model where the central bank chooses (with uncertainty) at each period between several models of the world/Phillips curve and NAIRU.

Primiceri (2006) modeled and estimated directly the changes in beliefs of policymakers about the NAIRU over time. Sims and Zha (2006) criticized the literature by showing that, when accounting for a wide range of shocks, they did not estimate drastic regime shift in the coefficients of the monetary policy rule over time. In turn, this literature continued to influence economic historians working with a qualitative or narrative approach. For example, Meltzer (2010) in his history of the Fed, Bordo and Eichengreen (2013), Weise (2012) emphasized that, besides internal beliefs and NAIRU estimates of the Fed, the pressures from the Treasury to keep interest rates low were of key importance.

The debate died out with the Great Recession and was left somewhat unresolved. Three views remained: a story based on luck (Sims and Zha 2006); exogenous changes in policy that moved from a region of indeterminacy to determinacy (Lubik and Schorfheide 2004); and the learning story (also Orphanides and Williams 2013), either about parameters of a model as in Primiceri (2006) or between different models as in Cogley and Sargent (2005).

The question throughout this literature is a simple, historical one: “Why did  $X$  happen?” At first sight this history for history’s sake, arguing over a single data point without any attempt at establishing a causal relationship. But the debate was fruitful because models (more or less fleshed out, more or less confronted with observations) were arguing with each other and fighting to claim the data point. Whether or not a winner came out is of little importance: our knowledge and understanding of the (recent) past is deeper, our theoretical insights are sharper. Everyone gained.

## 6 Summary and Conclusion

Money and banking is an important subfield of historical economics: it represents twelve out of sixty-five chapters in the recent *Handbook of Cliometrics* (Diebolt and Hauptert 2019) and was an important part of the earlier cliometrics revolution. Its own “bible” (Friedman and Schwartz 1963) already contained the language of causality and “controlled experiments,” long before the so-called credibility revolution (Angrist and Pischke 2010). But this subfield has little embraced the two trends identified by the editors of the present volume, namely the search for long-run effects in persistence studies and the reduced-form empirics targeted at causality through quasi-random experiments. The latter method has been used in some papers to study local effects, but the subfield still relies on traditional approaches: long narratives, large data collection to document long-term trends, and the construction of arguments and narratives through monographs and not just sharply focused articles. It is also a subfield with, in our experience, meaningful interactions between

historians and economists, which may not be typical of historical economics in general (Lamoreaux 2015).

We speculated on the reasons for this difference. The new methods are ill-suited to answer the traditional questions of money and banking history: Why markets and financial institutions developed as they did? Why do monetary and financial institutions differ so much across time and space? Is capital efficiently allocated? Wherefore financial crises? There are no natural experiments to answer these questions. Other large questions, such as the impact of monetary shocks, have been addressed with different methods (Sims 2010).

The interesting studies that use methods of empirical microeconomics rely on group comparisons rather than genuine natural experiment, and examine local and short-term effects. They are context-specific and difficult to generalize. This is consistent with the ongoing tradition of macroeconomics (Leamer 2010; Sims 2010) that highlights the difficulties left aside in the new “historical economics” literature but cannot be evaded in a general equilibrium setting.

It does not mean that causation is ignored, but it is understood differently: causality identified with a chain of events or relations, hence particular attention to the provision of plausible transmission mechanisms. Better yet, it is understood within a structural model, where causality has been explicitly asserted by the modeler, and where counterfactuals, the ultimate meaning of causality, are truly meaningful.

In the end, says Leamer (2010, 44), “we seek patterns and tell stories.” He spoke as a macroeconomist, but those of the historical persuasion tend to agree (Antipa and Bignon 2018). Are we, then, any better than epic poets? Not a bad thing to be, in our opinion. We have our prosody too: writing models and testing against data is our rhyme and meter. And if indeed we are just telling stories, we shouldn’t fool ourselves that we aren’t, yet we should fool ourselves, constantly, to tell better ones.

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