Bank Loans and the Real Economy: Evidence from the 19th century*

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Abstract

What is the effect of bank lending on the real economy? This project studies several ways that bank loans have real effects, through an episode in 19th century US history. In 1875, the US revalued its currency. I use this as a policy shock, and find that it generated exogenous regional variation in bank lending. Revaluation affected banks both by changing the value of their assets directly, and by changing their abilities to attract depositors – I thus quantify two channels by which monetary policy affects bank lending. Then, in a second stage analysis, I exploit this shock to study the effects of bank loans on three outcomes. I find that bank lending increased the probability of bank failure, but that it also increased regional investment. The latter is unsurprising: 19th century regulations directed bank loans towards industrial investment and away from mortgage and agricultural lending. Were these regulations distortionary? I find evidence that they were not. Bank lending improved capital allocation, which contributed nonnegligably to TFP growth. This suggests that regulatory controls on credit do not need to come with misallocation.

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1 Introduction

What is the effect of bank lending on the real economy? Bank loans can fund productive investment and growth, or they can lead to financial crises and unhealthy levels of debt. When does one or the other occur? This project studies these questions through an episode in 19th century US history. In 1875, the US revalued its currency. I use this revaluation as a policy shock, and find that it generated plausibly exogenous regional variation in bank lending. I then exploit this shock to study several aspects of the real effects of bank loans.

Compared to today, banks in the 1870s were tightly regulated: they were forbidden from opening multiple branches, they had to be run by local residents, they could not take real estate as collateral, and they could not make loans exceeding a certain amount. In this setting, I find that increased lending by national banks funded regional investment in industrial fixed capital. Indeed, regulations meant that bank lending was in some ways *forced* to fund these investments – it could go little elsewhere. One might as such expect these regulations to have been distortionary: banks could not lend entirely as they wished. Did this mean that bank loans were poorly allocated? I find that this does not appear to have been the case. In this setting, bank lending did not decrease, and in fact *increased* the efficient allocation of capital across industries. I find that the effect of improved allocation on TFP growth was positive and nonnegligable. In the late 19th century's regulatory environment, bank lending was a boon for US industrialization. This paper thus shows the 'bank lending channel' at work, efficiently allocating capital in a setting very different from the post-1980 liberalizations that are the usual focus of this literature (Favara and Imbs, 2015; Larrain and Stumpner, 2017; Bai et al., 2018; Bau and Matray, 2023). It suggests that regulatory controls on bank lending do not need to come with misallocation.

Studying 19th century banks allows me to examine several other questions of interest. While bank lending funded investment, improved capital allocation, and generated productivity gains, I also find that more lending increased the risk of bank failure. I find a significant and sizable causal relationship: 15% more loan growth (the size of the treatment effect of the currency revaluation shock) led the probability of bank failure to increase by 5%. Numerous recent papers have demonstrated a robust correlation between increased credit supply and increased crisis risk at the aggregate level (Schularick and Taylor, 2012; Jordà et al., 2013, 2016; Baron and Xiong, 2017; Mian et al., 2017; Baron et al., 2020; Greenwood et al., 2022). This paper provides well-identified evidence that the relationship is causal at the level of individual banks.[1]

This setting provides insight into two additional areas. First, it provides evidence for the ways monetary policy affects the real economy through the banking sector. Recent research has proposed two possible channels by which monetary policy passes through the banking sector. One is by revaluing banks' assets directly, leading them to lend more or less (Gomez et al.,

[1]Is increased failure probability in tension with the finding that bank lending improved capital allocation and contributed to growth? It's possible that loans could have generated more bank-level instability while also putting the entire economy on a higher growth path. There is more work I could do to examine the link between these two results, but I have not done so in the present draft. In concluding, I'll suggest some of these future directions.

2021). The second is by changing banks' abilities to attract depositors, based on the spread between deposit account interest rates and interest rates in the rest of the economy (Drechsler et al., 2017). Banks that attract more depositors can lend more; this provides a second way that monetary policy affects bank lending, and thereby the real economy. The currency revaluation I study gives an opportunity to quantify both channels. Revaluation directly affected bank balance sheets, increasing the asset values of some banks more than others. It also allowed banks that benefited to attract more depositors. I find that both channels contributed to increased bank lending, and the deposits channel ultimately accounts for a larger share (70-80%) of the increase.

Finally, this project is relevant for understanding the historical development of the United States in its own right. The decades after the Civil War were pivotal for American industrialization; I find that a tightly regulated financial sector played a nonnegligable role in allocating credit to fund this development. (My estimate is that 15% of increased loan growth nationwide would have caused around 3-4% of the 20% TFP growth between 1875 and 1885.) In addition, the coexistence of paper and gold currency is an interesting monetary phenomenon that itself deserves to be studied. Anna Schwartz and Milton Friedman for instance devoted significant attention to this period in their *Monetary History of the United States* (Friedman and Schwartz, 1963; Friedman, 1994). They ask questions about the implications of the issuance and retirement of paper currency for the money supply. To my knowledge, there has been no quantitative study of the effects of this monetary moment on the supply of *credit*. Following the recently renewed interest in studying private lending, this project thus provides a small chapter in the *credit* history of the United States.

Setting and empirical strategy

The setting of this project is the following (I provide more detail in section 3 below). During the US Civil War, the federal government began issuing paper currency – called 'greenbacks' – that was unbacked by gold. When the war ended in 1865, paper-backed currency coexisted with gold-backed money, and paper and gold traded against each other at a floating rate. After several years of political debate, the government passed a bill in 1875 promising that 'specie resumption' – the resumption of 1:1 convertibility between paper and gold – would occur in 1879. Despite more debate and technical uncertainty, gold convertibility did ultimately resume successfully as promised. After the government's 1875 decision, the value of paper greenbacks suddenly increased relative to gold. Banks held both forms of currency on their balance sheets; banks who held more greenbacks as such saw their assets become more valuable. Did this cause them to lend more?

My empirical strategy is to use the government's 1875 promise to resume convertibility as a shock to bank balance sheets. I define the intensity of individual banks' 'treatment' by the shock as the share of greenbacks held on their balance sheet in 1874. I use a dataset consisting of the balance sheets of every nationally chartered bank to study the effect of specie resumption on bank lending. In the first part of the paper, I use simple difference-in-differences comparisons between more- and less-treated banks.[2] I find first that revaluation caused more highly treated banks to accumulate more gold, as they traded in their newly convertible greenbacks and attracted more deposits. I then find that this caused them to lend more.

In the second part of the paper, I use pre-1875 exposure to paper currency as an instrument for bank loans, to study the effects of credit on the real economy. I study the effects of lending on bank failure and on productive investment. Increased lending raised the probability of a bank's failure, but it also led to increases in regional capital accumulation. Finally, in a third part, I model the supply side of the 19th century economy to study the effect of lending on misallocation. Bank loans helped the average industry accumulate more capital – but they also appear to have improved allocation across different industries. I use a simple aggregation framework to quantify the impact of this improved allocation on total factor productivity growth.

My empirical strategy makes several identification assumptions. To identify the causal effect of specie resumption on bank lending, I require that banks that were more and less exposed to paper currency would have been on parallel lending trends, had the shock not occurred. I'll demonstrate with descriptive evidence and placebo tests that pre-shock lending behavior between differently-exposed banks was largely parallel, even without conditioning on other bank characteristics. But banks that held different amounts of paper may have done so for specific reasons, which could themselves have affected their lending after 1875. Indeed, balance checks show that banks' 1874 greenback holdings were correlated with their exposure to other assets and liabilities. What caused banks to hold different amounts of greenbacks in 1874?

Banks held greenbacks in order to satisfy their reserve requirements. These requirements varied by bank, based on their balance sheets and their locations (I provide more detail in section 3). Banks could meet their reserve requirements with only three specific forms of currency; two of these were gold-backed, and the third was greenbacks. Reserve requirements thus explain some, but not all, of the variation in bank greenback holding: banks with higher requirements most likely held more greenbacks, but there was variation in whether they met their reserve requirements with greenbacks or with gold.

What caused a bank to meet their reserve requirement by holding greenbacks rather than by holding gold? I find that controlling for reserve requirements, holding more greenbacks is uncorrelated with domestic economic conditions and with most other important balance sheet items. This suggests to me that the decision to meet reserve requirements one way or the other may have been plausibly exogenous to relevant economic conditions and bank characteristics.

Reserve requirements thus allow me to extract a quasi-random element of greenback holding. I use inverse-propensity-weighting (IPW) and matching strategies in my difference-in-differences estimations, to compare variation only between banks that faced similar reserve requirements. In concluding, I'll suggest some ways that I think reserve requirements could be used to extract a more truly random component of greenback exposure; I haven't explored this in the current

[2] Following a recent literature on the reliability of difference-in-difference estimators (de Chaisemartin and D'Haultfœuille, 2020; de Chaisemartin and D'Haultfœuille, 2023; Sant'Anna and Zhao, 2020), I ensure that these estimates are robust to treatment effect heterogeneity using a variety of recently-proposed estimators and diagnostic tests.

draft.

My two second stage analyses rest on several other identifying assumptions. To identify the causal effect of bank lending on bank failure, I use greenback exposure as an instrument for bank lending. The relevance of this instrument is thoroughly demonstrated in the first part of my analysis. I also need pre-shock greenback holding to satisfy the exclusion restriction. It is unclear why greenback holding would have increased failure probability except by increasing lending. If anything, banks with more greenback exposure would arguably have been *less* likely to fail: federally backed greenbacks were among the safest assets a bank could hold. If holding greenbacks did itself affect failure probability, it would probably have decreased it. This would work against my finding, meaning that my estimates in this part are if anything conservative.

To identify the effect of bank lending on regional economic outcomes, I require two conditions to hold. First, I need bank lending markets to have been regionally segmented. Existing research on this period argues that this was the case in the 19th century (Xu and Yang, 2022; Carlson et al., 2022). Financial and communications technologies would have made long-distance lending more difficult than it is today; in addition, regulations likely made lending a local activity. Banks were forbidden from operating multiple branches, and regulations required that at least 75% of a bank's directors were local residents. All of this suggests that bank lending in the 1870s indeed operated in a regionally segmented market, making the 19th century a good setting for this project.[3]

Finally, my second stage analysis also requires that regions with different levels of greenback exposure were economically comparable. In an appendix, I repeat the first stage analysis at the regional level, showing that treated and untreated regions were on parallel lending trends prior to 1875 and that total lending increased by more in highly treated regions. I also use economic data from the 1870 census of manufacturing to show that, prior to the shock, treated and untreated regions had comparable populations, numbers of manufacturing establishments, total manufacturing output and input values, and total levels of capital and labor investment.

Overview of results

Ultimately, I find a significant, sizable, and robust first-stage causal relationship between greenback exposure and lending. Holding 10% more greenbacks on their balance sheet in 1874 caused banks to increase their lending by 10-20% after specie resumption. The revaluation of greenbacks grew these banks' non-loan assets by about 5% directly, and by another 10% indirectly (through a deposits channel). With this increase in assets, banks increased their lending while maintaining constant loan-to-asset ratios. A lending increase of this size (15%) however increased a bank's failure probability by over 5%. Aggregating across banks, I find that specie resumption was itself responsible for 12-15% of the total increase in lending between 1878 and 1884.

[3] In my analysis, I define banks' regional markets first as their county, and then as comprising a certain distance radius around the bank. I show that my results are robust to either definition as well as to several different choices of radii.

In my second stage analysis, I find that each percentage point of increased lending funded about 1% of increased capital investment. Bank lending allowed the average industry to increase its capital stock. How did lending allocate capital across industries? I find that it allocated capital primarily to industries with *higher* ex-ante marginal productivities. Treatment led these industries to accumulate three times as much capital as lower-productivity industries. This suggests that heavily regulated 19th century lending was able to improve capital allocation: it sent capital to the industries that needed it most. With an aggregation framework, I estimate the importance of this improved allocation for TFP growth. I find that, holding the rest of the economy constant, a nationwide 15% increase in bank loans would itself have contributed something like 3-3.5% to TFP growth over ten years. TFP growth was probably around 20% per decade in the 1870s and 1880s, meaning that the contribution of increased lending to this growth would have been nonnegligable.

The rest of the paper proceeds as follows. Section two reviews related literature, and section three introduces the data I use, providing historical context on 19th century banking. Section four studies the effect of specie resumption on bank lending, and section five analyzes the effect of lending on bank failure probability and on productive investment. Finally, section six introduces a simple supply-side model to estimate the effect of lending on efficient allocation. It then combines this with an aggregation framework to quantify the total impact on TFP. Section seven concludes.

2 Literature

This project is related to four strands of literature: research on the 'bank lending channel' and misallocation; research about the relationship between private credit and crises; research about the pass through of monetary policy to the real economy via bank loans; and finally, historical research into the origins of US growth.

Research on the 'bank lending channel' studies the real effects of bank loans. Several papers find that reductions in bank lending lead firms to reduce employment or output, suggesting that firms are credit constrained (Chodorow-Reich, 2014; Benmelech et al., 2019; Herreno, 2023). Other papers find that modern expansions in bank lending fund household borrowing (Mian et al., 2017), which can lead to crises (Jordà et al., 2013, 2016) or even to economic stagnation in non-crisis times, due to high levels of indebtedness (Mian et al., 2017, 2020, 2021). In contrast to results demonstrating the negative effects of private credit, other papers argue that financial deregulation and credit expansions improve the efficient allocation of capital. Larrain and Stumpner (2017) find this in Eastern European countries following capital account liberalization; Bai et al. (2018) for state-level bank deregulation in the US; and Bau and Matray (2023) for the liberalization of foreign investment controls in India. I contribute to this literature by showing the bank lending channel at work in a historical setting. I also combine my empirical results with a novel aggregation technique proposed by Sraer and Thesmar (2023), to measure the effect of improved allocation on TFP.

Ultimately, this literature is asking whether and when bank lending is good or bad for an economy. Müller and Verner (2022) suggest that the allocation of credit matters, and propose that 'good' credit funds investment in tradable goods, rather than funding household borrowing or nontradables. I contribute to this question by studying a very specific regulatory environment. In a setting where regulation kept banks small, local, and unbranched, and where it limited and at times even specified the types of loans banks could make, bank lending funded investment in tradable production. These regulations did not seem to cause loans to misallocate capital; if anything, bank lending increased efficient allocation and raised TFP in this environment. My project thus shows improvement in allocation in a regulatory setting very different from the post-1980 liberalizations that are the usual focus of this literature (Larrain and Stumpner, 2017; Bai et al., 2018; Bau and Matray, 2023). It suggests that regulatory controls do not need to come with misallocation.

Second, this project contributes to understanding the relationship between credit and crises. A large strand of research finds a robust correlation between bank lending and systemic crises at the aggregate level (Schularick and Taylor, 2012; Jordà et al., 2013, 2016; Baron and Xiong, 2017; Greenwood et al., 2022). Along with Carlson et al. (2022), who also study the 19th century national banking system to make a similar contribution, I use microdata to provide evidence that at the bank level, this relationship is a causal one.

Third, this project contributes to understanding the operation of monetary policy through its effects on bank balance sheets. Seminal papers in this literature showed that interest rate increases cause banks to contract their lending (Kashyap and Stein, 1995, 2000). More recent work proposes specific channels by which this happens. One channel is through bank asset revaluation: interest rates directly affect bank balance sheets, leading them to lend more or less depending on their exposure to interest rate risk (Gomez et al., 2021). Another is the deposits channel: interest rates affect banks' abilities to attract depositors, which in turn affects their lending behavior (Drechsler et al., 2017). In my setting, I observe both of these channels at work. The government's decision to revalue greenbacks directly affected the value of banks' assets. It also affected banks' abilities to attract depositors. I show that the deposits channel appears to have been responsible for 70-80% of the increase in lending, and the asset revaluation channel for the remainder. This setting thus offers a chance to observe and quantify the banking channels of monetary policy.

Finally, this project contributes to work in economic history. Economic historians have studied the development of the banking sector in the period I examine, often focusing on the role of deposits (Jaremski, 2014; Jaremski and Rousseau, 2018) or the money supply (Friedman and Schwartz, 1963; Friedman, 1994). I contribute a study of the role of *credit* in funding American industrialization. In addition, the coexistence of greenbacks and gold and the resumption of convertibility in 1879 is an interesting moment in US history in its own right. Contemporary observes often remarked on the importance of specie resumption for the nation's economic outcomes.[4] As far as I know, I provide the first modern, quantitative study of the effects of specie resumption on bank lending and on the real economy.

[4]See section 3 and appendix A for some examples; see also Barreyre (2015).

Two recent papers are most closely related to mine. Carlson et al. (2022) and Xu and Yang (2022) both use the same individual bank balance sheet data as I do, and they also study the relationship between banks and the real economy in this period. Both papers use a common identification strategy, exploiting a discontinuity in bank entry requirements to compare the effects of opening a new bank in a region. This identification strategy leads Carlson et al. (2022) to focus on the effects of bank competition, while Xu and Yang (2022) focus on the effects of currency provision (since this was one of the roles played by 19th century national banks). In contrast, my identification strategy uses the revaluation of greenbacks as a policy shock. This leads my analysis to differ from these papers in two ways. First, it means that I focus directly on bank lending, and study the effects of increased lending at an existing bank, rather than the effects of opening a new bank. With this different strategy, I confirm that their findings largely hold when looking at bank lending.[5] Second, the policy shock I study is interesting in itself. As mentioned above, resumption was a policy that both revalued bank assets and affected bank deposits. This allows me to identify and to quantify the importance of these two channels.

My paper makes two additional contributions to these existing studies. I build a new hand-collected dataset that allows me to observe investment decisions at specific iron factories biannually. Both Carlson et al. (2022) and Xu and Yang (2022) rely on decadal data from the Census of Manufactures to analyze the effect of banks on real activity. My higher frequency data allow me to study this link more closely. Finally, I use a simple partial equilibrium model of the economy's supply-side and a novel aggregation technique from Sraer and Thesmar (2023) to study the effect of bank lending on misallocation and TFP. This takes the analysis in these other two papers one step further, and allows me to connect the study of 19th century bank lending with the literature on efficient allocation.

3 Data, history, and descriptive evidence

3.1 Data

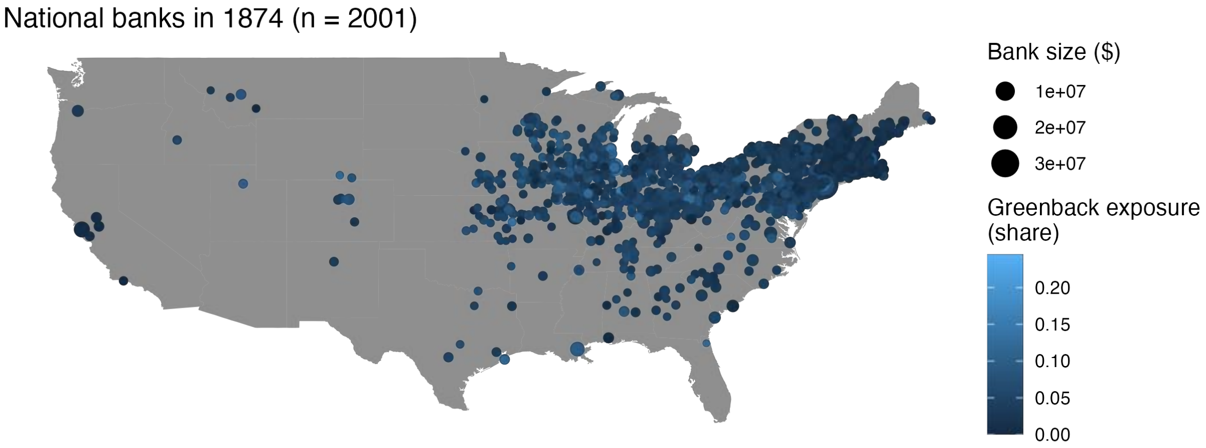
I use three main sources of data. To study the effect of specie resumption on bank activity, I use individual bank balance sheets collected by the federal 'Office of the Comptroller of the Currency.' These data were used in part in two earlier banking history papers (Jaremski, 2014; Jaremski and Rousseau, 2018), and were more recently digitized and released by Carlson et al. (2022).[6] They are also used by Xu and Yang (2022). Example pages and more information about this source is given in Appendix A.

The OCC reports allow me to study the behavior of every nationally chartered bank – a sample of 2,001 banks in 1874 (the year before the policy shock). Figure 1 shows the spatial distribution of banks in my sample, plotting banks according to the size of their balance sheets and the share

[5] One difference is that Xu and Yang (2022) find that increased currency provision led regional industries to hire more workers and purchase more inputs to production, but *not* to increase capital investment. I find that in contrast, lending did appear to lead to increased investment.

[6] For this project, I began a digitization effort of these sources myself. Carlson et al. (2022) released their data in March 2023, and I have since benefited from their dataset.

Figure 1: Sample of national banks



Notes: Banks are sized according to their size (the nominal value of their total assets) and colored according to the share of greenbacks in total assets. There is no obvious clustering of greenbacks, although banks in the northeast (Massachussetts, Connecticut, and Vermont) held fewer greenbacks than banks elsewhere. Section four explores this in more detail.

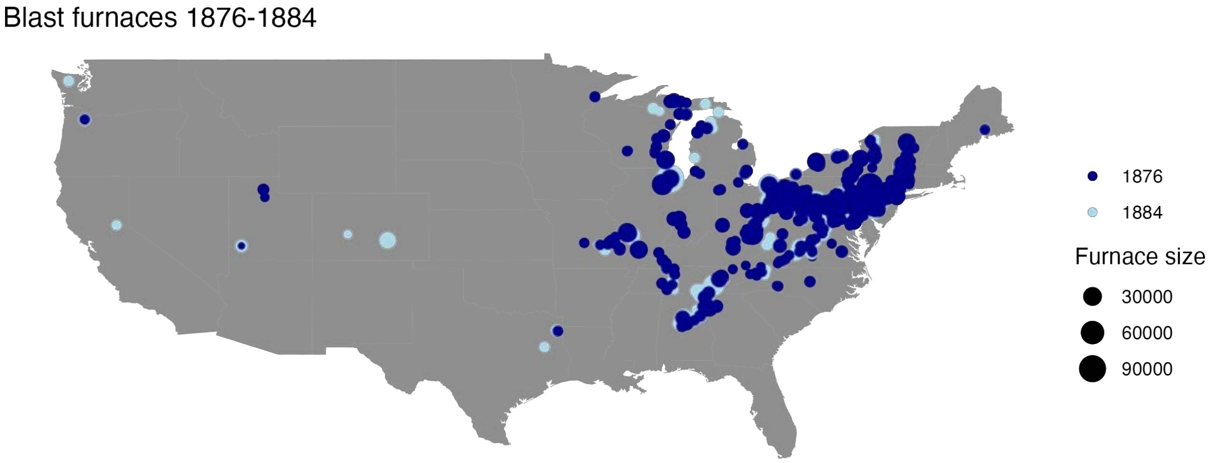
of greenbacks on their balance sheet. The banking system in 1874 was both far smaller and less concentrated than today: the total assets of the US commercial banking sector are currently around 75% of GDP, while in 1874 they were only 22%. In 1874, the top 5% of banks held 38% of the banking system's assets; today, they hold about 75% (Baron et al., 2022). Relative to GDP, the largest bank in 1874 was only 2% of the size of the largest bank today.[7]

The national banking system was established in 1862, during the Civil War. Before that, each individual US state chartered their own banks. State banks decreased in importance after the standardization of the national banking system, but state banks did continue to coexist alongside national banks in the 1870s. Historical evidence on state bank activity suggests that they played a relatively marginal role in the decade I study (Jaremski, 2014; Jaremski and Rousseau, 2018); following existing literature on this period (Carlson et al., 2022; Xu and Yang, 2022; Payne et al., 2022), I assume that the existence of state banks did not majorly interfere or compete with nationally chartered banks, and restrict my analysis to these latter banks.

To study the effect of bank loans on firm investment, I construct a new dataset measuring

[7]The largest bank in 1874 (in NYC) controlled $35 million in assets (nominal), or 0.4% of GDP. Currently, the assets of JP Morgan Chase are over 15% of GDP.

Figure 2: Sample of iron furnaces



Notes: Dark blue dots are active furnaces in 1876, sized according to the volume (in cubic feet) of their furnace 'stacks.' The light blue dots show furnaces in 1884 (n=663), sized similarly; comparing the dots thus shows both the construction of new furnaces and the expansion in size of existing furnaces due to furnace renovation.

fixed investment in iron factories. Iron manufacturing comprised a significant share of US capital investment: it was responsible for about 10% of the capital stock of all manufacturing industries in 1880. As a first step, I thus focus on the effect of bank loans on investment in the construction and renovation of iron furnaces. I hand collected furnace-level data from a directory of iron furnaces, published biannually by the 'American Iron and Steel Association.' This allows me to observe the dates and amounts of capital invested in the construction and renovation of all active iron blast furnaces across the country. The nation's 663 operational furnaces are plotted in figure 2. More information about my construction of this dataset is in Appendix A; to my knowledge, this is the first time this source has been used in modern quantitative analysis.

Finally, to study the effect of bank lending on other sectors and to measure the effects on misallocation and TFP, I use decadal data from the 1870 (pre-shock) and 1880 (post-shock) Census of Manufacturing. The census collected information on the value of outputs, inputs, capital investment, and labor for 35 industries at the county level. I use a cleaned and harmonized version of these data collected by Hornbeck and Rotemberg (2019).

3.2 Historical setting

During the Civil War the federal government passed the National Banking Act, forming the US's first nationally unified banking system. To fund the war effort, they also began printing greenbacks, paper money unbacked by gold. After the war ended, multiple forms of currency coexisted. Greenbacks, issued by the federal government, were one form of currency. National banks also issued their own bank notes, which were used locally. These notes were redeemable for greenbacks (backed by a federal guarantee), and were thus considered equivalent to them. At the same time, gold coins continued to circulate and were used especially by people and firms who made international transactions. Payments of both principal and interest on government

Figure 3: Aggregate lending by national banks, 1874-1884

Quarterly observations on aggregate lending from the Annual Report of the Comptroller of the Currency, 1884.

debt were also made in gold.[8] As a result, gold and greenbacks coexisted, with greenbacks trading at a floating discount against gold.[9] This remarkable arrangement continued for 19 years after the war – as Anna Schwartz and Milton Friedman observed, "[this] was certainly the only period in which two kinds of money exchanging at a fluctuating rate–greenbacks and gold–were used domestically side by side to any considerable extent" (Friedman and Schwartz, 1963).

In 1875, ten years after the end of the war, Congress passed a law promising to return greenbacks to gold convertibility on January 1, 1879. Despite continued uncertainty, convertibility did resume as promised on that date. Figure 3 shows aggregate lending of the banking sector: shortly after specie resumption occurred, bank loans increased. In October 1880, the Comptroller of the Currency observed the effect that he believed resumption was having on the banking sector:

> "The movement of the currency and the operations of the banks have never been more interesting than during the months which have intervened since the resumption of specie payments. [...] The coin in the banks has increased [...] The merchant, the manufacturer, and the farmer are alike prosperous [...] The deposits of the banks have everywhere increased, and money has been abundant wherever business or investment has invited capital" (United States. Office of the Comptroller of the Currency, 1880).

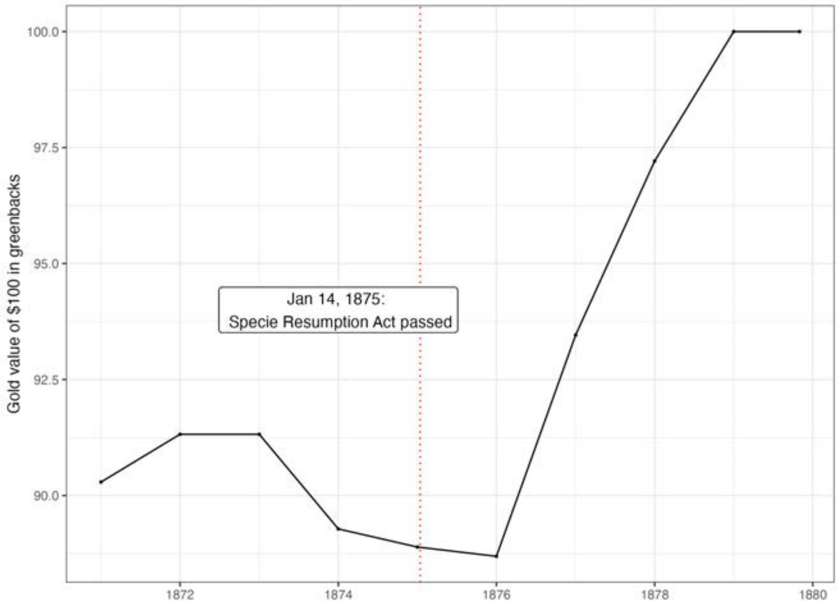
Is the Comptroller's observation causal – did specie resumption cause a credit boom?

The first stage of my analysis seeks to answer. Figure 4 shows the floating exchange rate

[8]Some forms of government debt were denominated in greenbacks, but especially after the Public Credit Act was passed in 1869, the majority of government debt payments were promised in gold. See Payne et al. (2022) for more details; see also Barreyre (2015).

[9]Some silver currency was in use, but the free exchange rate between silver and gold was more favorable to gold than the fixed domestic rate, which drove silver currency almost entirely out of circulation during the period I study (Friedman, 1994; Friedman and Schwartz, 1963; Barreyre, 2015).

Figure 4: Observed exchange rate between gold and greenbacks, 1871-1880



Greenbacks and gold currency coexisted, and traded against each other at a floating exchange rate. This rate was reported in the Annual Report of the Comptroller of the Currency, from which these data are taken.

between greenbacks and gold. Greenbacks traded at a discount against gold; after resumption was promised, their value quickly began rising until it hit par in 1879. The Specie Resumption Act thus affected the real value of greenbacks; banks who held more greenbacks got richer. Did this cause them to lend more?

3.3 Exogeneity

To identify the effects of specie resumption, two concerns are relevant. First, was the government's decision plausibly exogenous to trends in bank lending? Second, was it anticipated by banks? Work by political and economic historians suggests that the answers are yes, and no; I'll also provide suggestive quantitative evidence that the decision was unanticipated.

After the end of the war, a long debate began on the fate of the two currencies (a complete timeline is provided in appendix A). Political and economic historians have shown both that the outcome of this debate was uncertain, and that it was politically contentious. Various coalitions formed on all sides of the issue, with some advocating for the continued issuance of unbacked greenbacks, some calling for immediate resumption, and others pushing for an extension of the status quo. The debate was often framed in moral, religious, and ideological terms. As Barreyre (2015) argues, in the fraught post-reunification political climate, the money question quickly came to stand in for concerns about national unity and the country's political future. After much debate, and after the November 1874 election shifted the political balance of power, Congress passed the 'Specie Resumption Act' in January 1875. This promised to resume the gold convertibility of greenbacks on January 1, 1879. Overall, historical study of this debate (Barreyre, 2015; Caires, 2014; Thomson, 2022) presents the Specie Resumption Act as a political event driven by ideological concerns and the balance of political power. It was certainly not a rational decision undertaken by a technocratic governing body in response to proximate economic conditions.[10] This suggests exploiting the promise for specie resumption as a plausibly exogenous shock to banks.

Did banks anticipate the policy decision? Figure 4 provides a first piece of evidence that they did not. If they had, the value of greenbacks should have risen ahead of the decision. Instead, their value declined – the market was if anything *pessimistic* about specie resumption before 1875.[11] Indeed, it wasn't until 1876 that the value of greenbacks began rising at all (perhaps reflecting continued political uncertainty – there were several attempts to repeal the Specie Resumption Act – or widespread doubts about the government's technical capacity to ensure resumption, to which the 1880 OCC report referred).[12]

Figure 5 provides an additional piece of suggestive evidence that the policy decision was unanticipated. The green line shows aggregate greenback holdings by the banking sector. If banks expected a revaluation, they should have bought up cheap greenbacks before 1875 in anticipation. Figure 5 shows that they did not do this.

Did specie resumption cause a lending boom? I'll first present suggestive, descriptive evidence in the next subsection, before advancing to formal analysis in the following section.

3.4 Desriptive evidence

Banks that held more greenbacks before the Specie Resumption Act would have been more affected by the increase in greenback's value afterwards. Figure 6 shows the distribution of greenback holdings across the banking sector, and heuristically splits banks into three groups. About 50% of banks held less than 3% of their assets in greenbacks. Banks with between 3% and 8% (average 5%) of greenbacks comprised roughly the 50-90th percentiles of banks; the remaining banks had over 8% (average 10%) exposure.

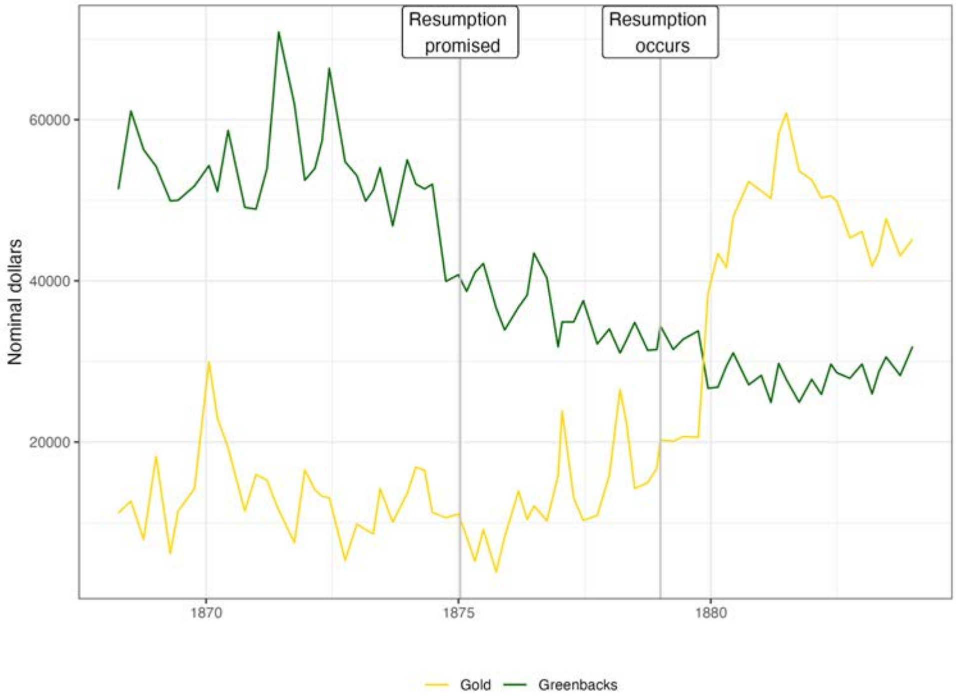
What generated this variation in greenback exposure? Some of it came from reserve requirements, which are explained in greater detail in appendix A. Banks could use a combination of three types of assets as reserves: federally-issued greenbacks, gold, and gold-backed government debt. Thus, while reserve requirements shaped a bank's level of greenback holdings, different

[10] One exchange makes clear that Congressional decisionmakers were by no means economic technocrats – at times, they didn't even understand what they were deciding between. During a debate on monetary policy in February 1867, one representative exclaimed: "It ought to be distinctly understood that the proposition of my colleague is for the direct expansion of the currency." To which another responded: "On the contrary, I regard it to a certain extent as a measure of contraction." See chapter 2 of Barreyre (2015).

[11] Smith and Smith (1997) validate the use of the greenback-gold exchange rate as a measure of the market's expectations of whether resumption would occur.

[12] The timing of the rise in greenbacks' value makes sense: it wasn't until the Republicans won the 1876 election – which was decided by a tiebreak – and Rutherford B. Hayes was inaugurated and appointed his Treasury Secretary in 1877, that the Democrats' attempts to repeal the Specie Resumption Act could be taken less seriously. Technical doubts were prevalent as well. The 1880 OCC report wrote: "To most of the political economists of this and other countries the resumption of coin payments by the United States at the time fixed by law, and its successful maintenance, were deemed almost impossible. [...] Even those who were known to be earnestly in favor of resumption, both in and out of Congress, doubted the ability of the government and of the banks to commence and continue coin payments."

Figure 5: Average bank holdings of greenbacks and gold



On average, banks gradually decreased their greenback holdings in this period. Following specie resumption in 1879, gold holdings suddenly increased. This jump in gold holdings was enabled in part by the real increase in the value of greenbacks, and in part by a 'deposits' channel (see section 4.2).

Figure 6: Distribution of greenback exposure across banks in 1874

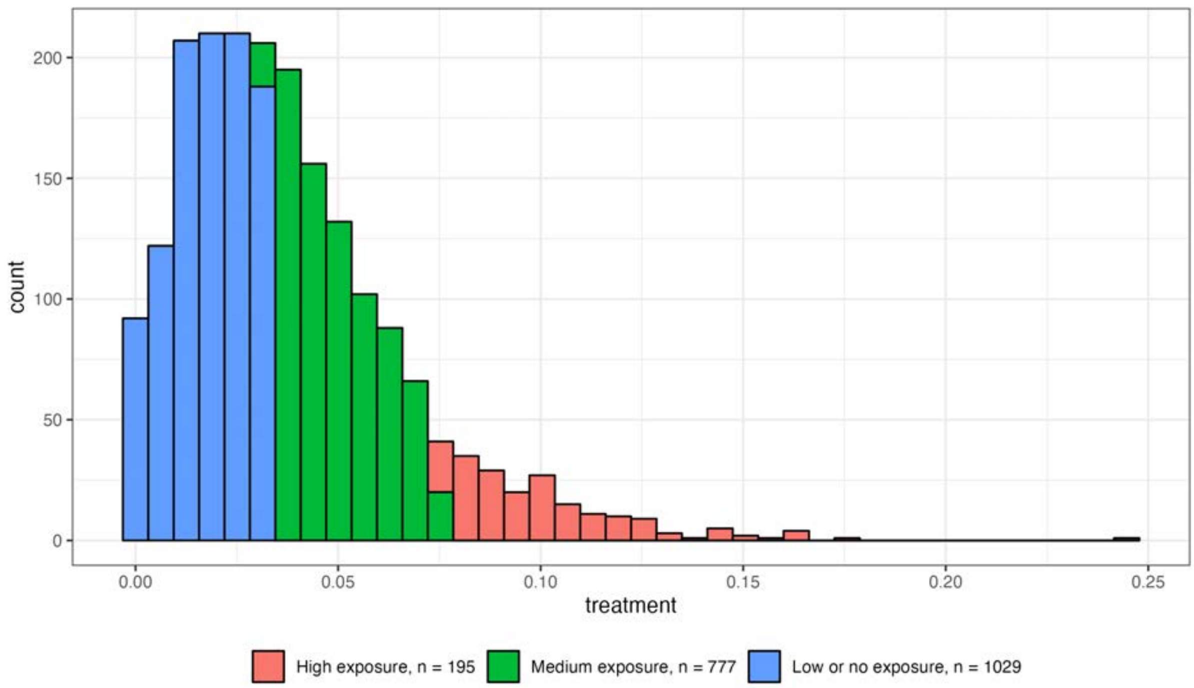
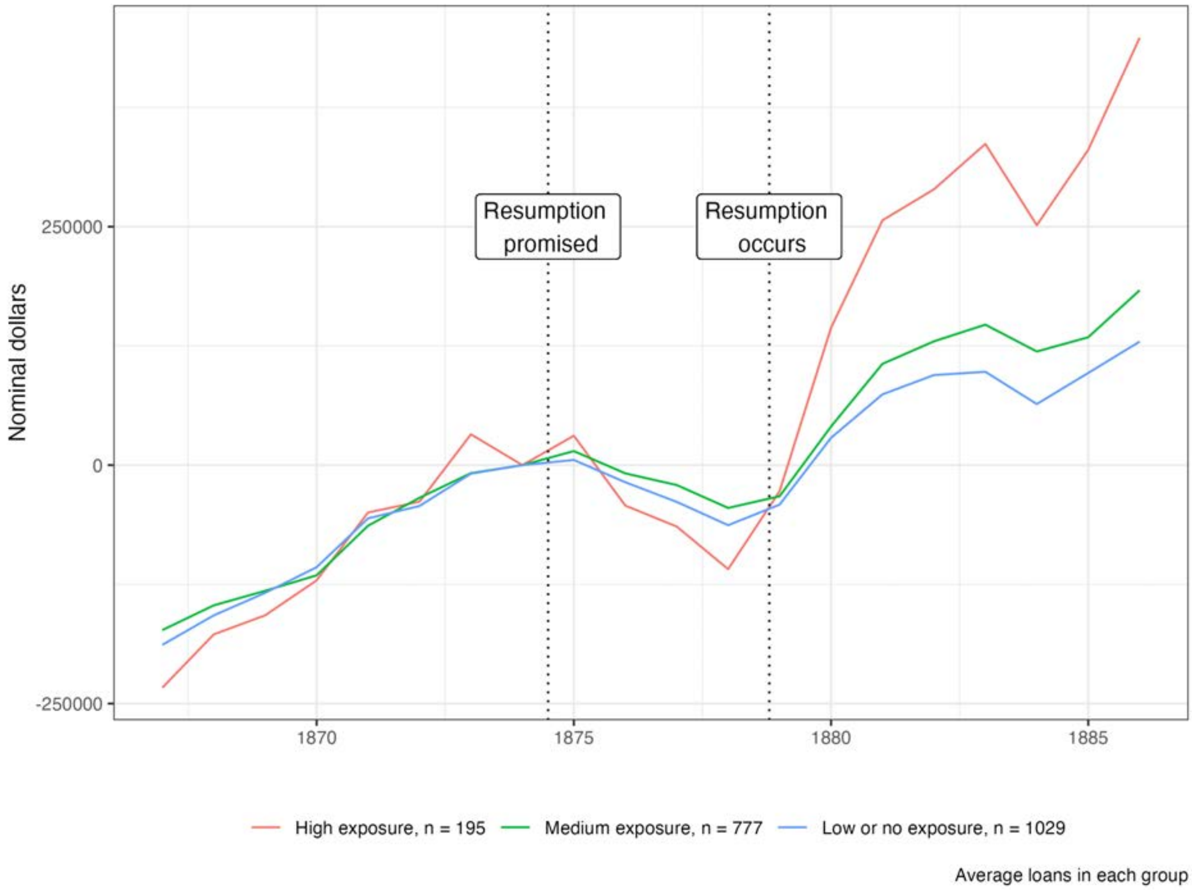


Figure 7: Lending trends across treatment groups



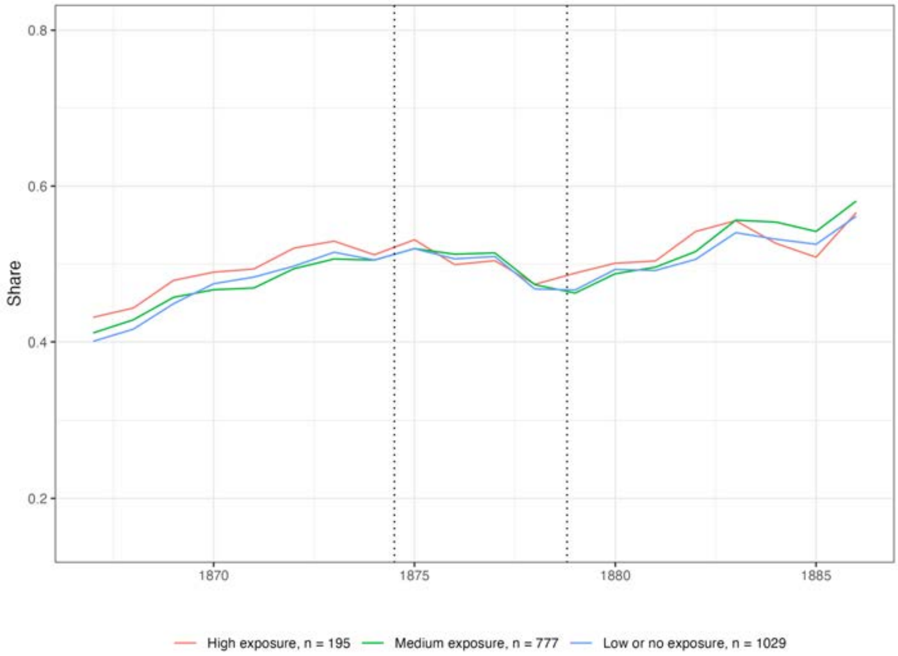
banks ended up meeting their reserve requirements with different amounts of paper and gold. In the formal analysis in the next section, I'll show that while greenback exposure was unconditionally correlated with several bank characteristics (figure A5), conditional on a bank's reserve requirements, greenback exposure was more balanced and appeared more random (figure 12). This suggests that there was some degree of randomness involved in how banks with the same level of reserve requirements met these requirements. I'll exploit this observation in later analysis, comparing variation between banks with similar reserve requirements.[13]

How did differently treated banks lend? Figure 7 shows changes in average nominal lending in each of the three groups defined in figure 6. Before resumption was promised, banks in all three treatment groups appeared to exhibit parallel trends in their lending behavior. After the passage of the Specie Resumption Act, banks continued to lend similarly, although treated banks may have somewhat decreased their lending. Then, when resumption actually occurred in 1879, more treated banks suddenly began to increase their lending, at a faster rate than less treated banks. This descriptive observation is formally confirmed in the next section.

Why did bank lending patterns diverge? Figure 8 shows that both before and after the policy shock, differently-treated banks maintained similar loan-to-asset ratios. This ratio is a measure

[13]In a possible extension to this project, I could explore using reserve requirements in greater detail to improve the design. Reserve requirements changed in 1874, which I think may allow me to extract a more random component of greenback exposure. I discuss this in the appendix.

Figure 8: Loan to asset ratios across treatment groups



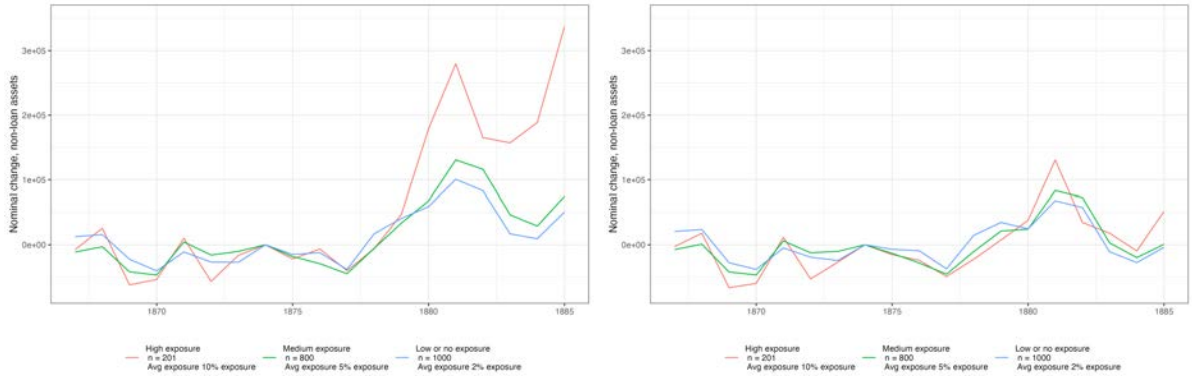
of bank liquidity. Loans are longer maturity assets that are not immediately realizable as cash; a bank with a larger share of its assets in loans is as such less liquid. In the 19th century, there were no regulations requiring banks to maintain these ratios at a certain level (regulations targeted other aspects of bank security). Banks instead adjusted their loan-to-asset ratio based on how they and depositors perceived risk; figure 8 shows that differently-treated banks appeared all to target similar ratios. More highly treated banks did *not* start lending a greater portion of their assets after 1879. Their divergent lending in figure 7 must have come from a relative increase in their other assets, enabling them to increase lending while maintaining similar loan-to-asset ratios.

Indeed, the left panel of figure 9 shows that assets other than loans grew more at more-treated banks than at less-treated ones. This enabled the trends observed in the previous two figures. What accounted for this divergence in assets? The right panel of figure 9 shows that it was almost entirely driven by the accumulation of gold. When gold is removed from the picture, highly treated banks exhibit almost no asset growth relative to less treated banks. This is suggestive evidence for the channel by which resumption caused exposed banks to increase their lending: highly exposed banks appear to have traded in their newly-revalued greenbacks for gold, growing their balance sheets and lending more as a result.

Is there any evidence that this occurred? Figure 10 shows the banking sector responding to revaluation and resumption. Once greenbacks began appreciating in value in 1876, the distribution of greenback holdings jumped to the left: banks began selling off their newly-valuable paper money.[14] When specie resumption occurred in 1879, the distribution jumped again: banks increasingly sold off their greenbacks, presumably for gold (figure 9).

[14] The jump in the distribution between 1873 and 1874 is explained by a shift in reserve requirement regulations. I do not consider this change in the present draft, but would like to exploit it in future analysis.

Figure 9: Changes in bank assets other than loans, with and without specie



Notes: the left panel shows the nominal change in non-loan assets across treatment groups. Highly treated (red) grew their assets relative to others, enabling them to increase loans while maintaining loan-to-asset ratios. The right panel shows that this asset growth was entirely driven by specie accumulation: once specie is removed, there is no difference between the treatment groups.

After presenting results, I'll examine in greater detail the sources of banks' accumulation of gold after resumption. This will allow me to study the channels more precisely. I'll also make several back-of-the envelope calculations, which demonstrate how the magnitudes of the effect can be accounted for. The next section confirms the story presented above with formal estimation.

4 First stage: Resumption and bank lending

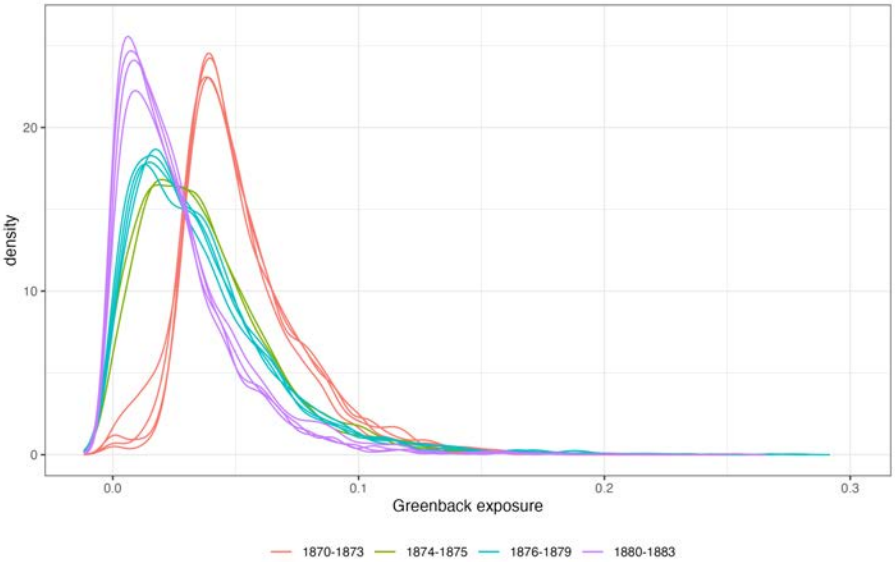
4.1 Balance checks

This section tests the suggestive evidence presented above. Banks that held different amounts of greenbacks exhibited similar lending trends prior to 1875. But did they differ in other ways? I'll first present evidence that, conditional on bank size and reserve requirements, more and less treated banks did not differ in ways that would confound my analysis.

My first stage analysis requires a parallel trends assumption. Banks appear to have been on parallel lending trends prior to 1874 (figure 7). One concern is that there are differences between more- and less-treated banks (other than greenback exposure), which *didn't* cause them to lend differently before resumption, but which *did* cause their divergent trends afterwards. I'll present balance checks to compare more- and less-treated banks. They show that treatment was not fully balanced – but that it was somewhat balanced conditional on bank size and reserve requirements.

Figure 11 shows unconditional correlations between greenback exposure and bank size (top left), between grenback exposure and lending behavior (top right), and between greenback exposure and the population of each bank's city (bottom panel). Bigger banks do not seem to have held any more greenbacks than smaller banks. Nor did banks who held more greenbacks have different lending behavior in 1874. Banks in larger cities may have been more exposed (lower left panel), but this relationship disappears after excluding the three largest cities (lower right

Figure 10: Distribution of greenback exposure over time



Notes: this figure shows kernel density estimations of the distribution of bank greenback exposure over time. It shows that banks did indeed respond to the policy changes I study: the distribution of greenback holdings jumped after a change in reserve requirements in 1874, once greenbacks began appreciating in value in 1876, and again after specie resumption occurred in 1879. See text.

panel). Figure B6 in the appendix suggests that greenback holdings were not clustered in certain states, either.[15]

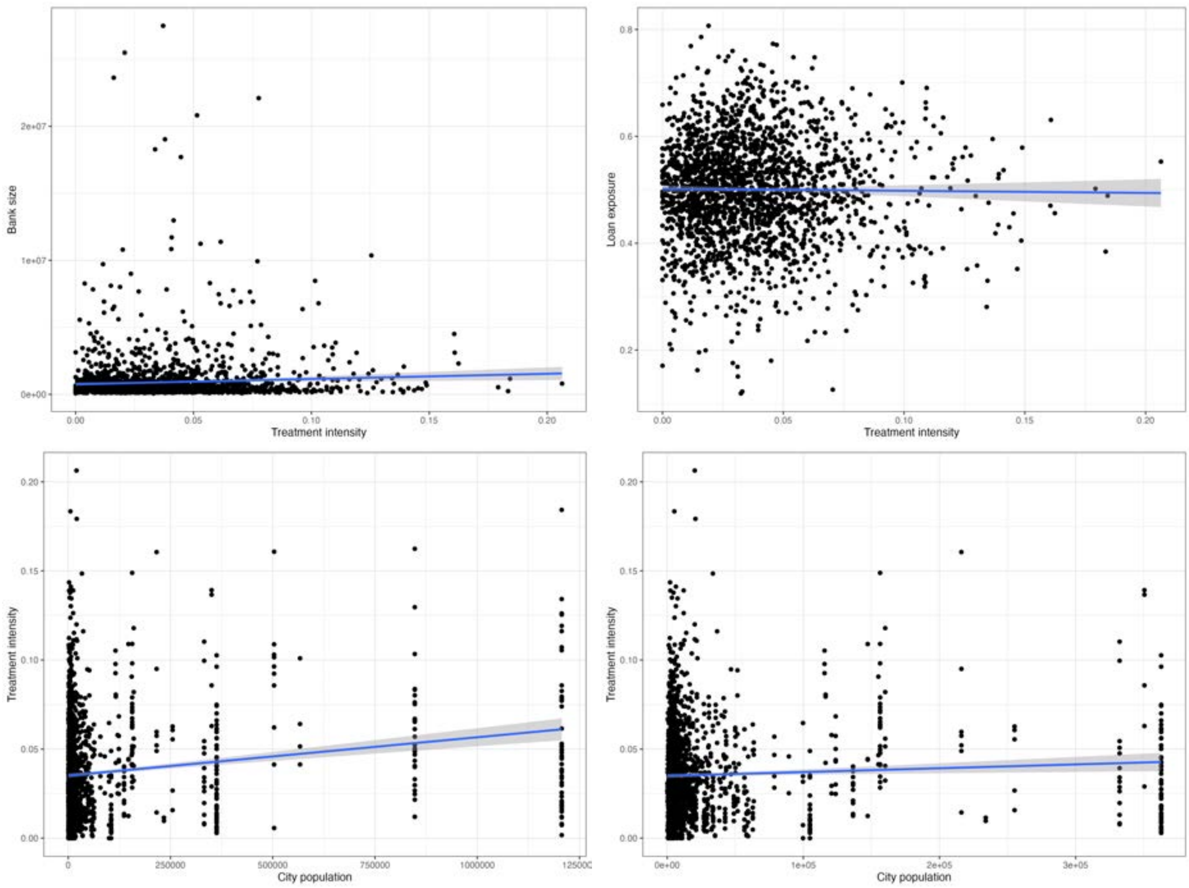
Figure 12 presents a more complete balance check. Several balance sheet items were mechanically or semi-mechanically correlated with greenback exposure, due to bank regulations. Banks held greenbacks because reserve requirements forced them to, and reserve requirements were set based on bank balance sheet characteristics. This means that banks with high greenback exposure had different balance sheet compositions than banks with low greenback exposure.[16] Treatment is however more balanced among banks with similar reserve requirements. To show this, I regress greenback exposure in 1874 on (normalized) holdings of each balance sheet item, controlling for bank size and for reserve requirements. Figure 12 plots the coefficients from these regressions; it shows the correlations between a one standard-deviation change in a covariate and greenback exposure, at banks of similar sizes and with similar reserve requirements.

For similarly sized banks that faced similar reserve requirements, treatment looks somewhat balanced. Banks who met their reserve requirements with greenbacks rather than with gold were located in counties with similar economic conditions to counties of less-treated banks (the coefficients in green). They also held similar liabilities. The assets of more- and less-treated banks with similar reserve requirements look similar, although some assets are significantly

[15]Banks in several New England states did hold systematically lower amounts of greenbacks. In robustness checks, I thus include state-fixed effects and controls for longitude; I also try dropping these states from the sample to ensure that their lower greenback exposure does not bias the control group.

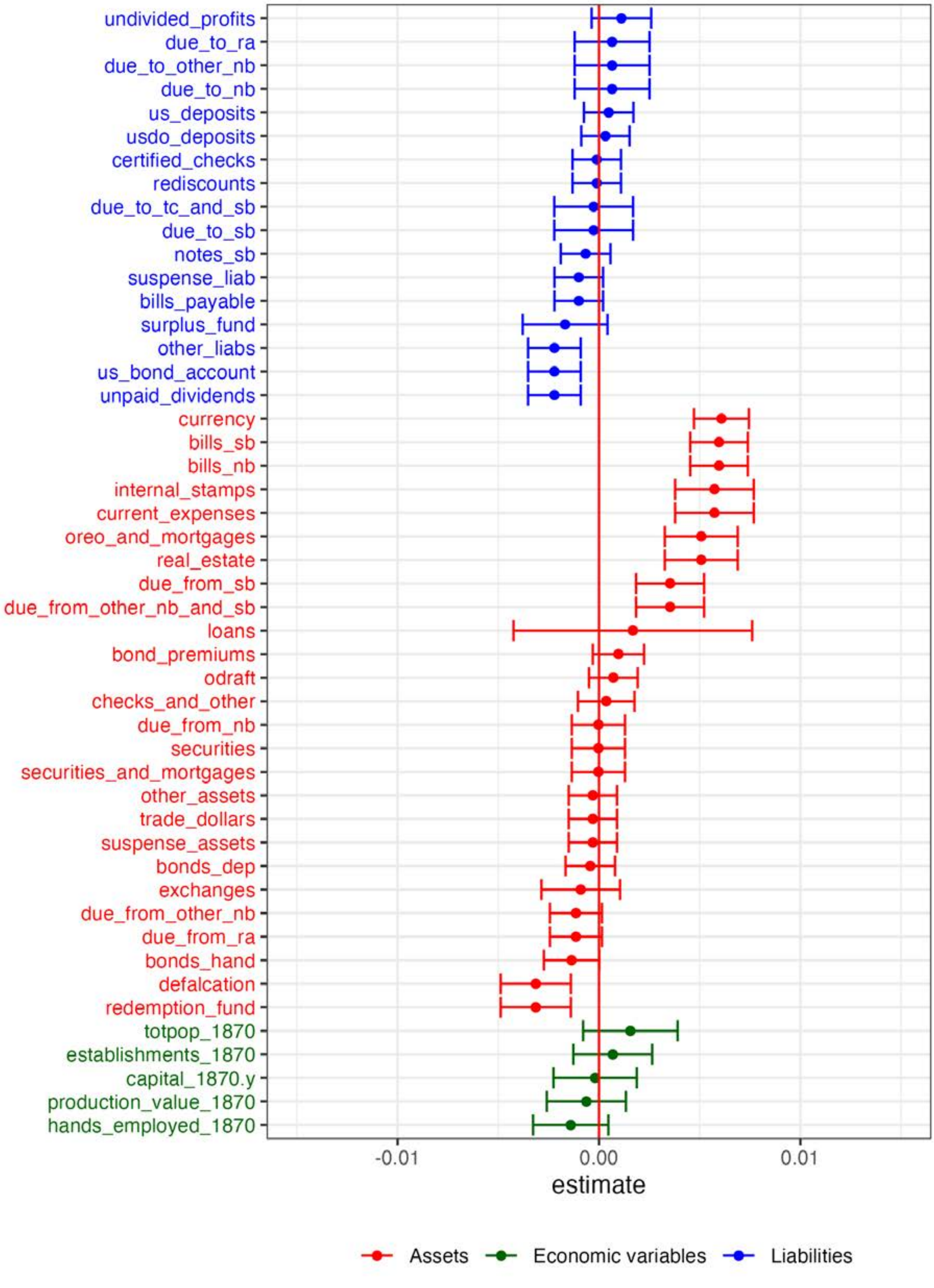
[16]Figure A5 in the appendix shows the unconditional correlations between balance sheet items and greenback exposure, revealing these mechanical correlations. The appendix explains in detail the reserve requirements that gave rise to these correlations.

Figure 11: Balance check: correlations with treatment



This figure shows unconditional correlations between 1874 greenback exopsure and 1874 bank size (top left) 1874 log loans (top right) and the population of a bank's city (bottom). The slight positive correlation in the bottom left panel disappears when excluding the three largest cities (bottom right).

Figure 12: Balance check, conditioning on bank size and reserve requirements



Notes: Each covariate is normalized so that the estimates represent the correlation between a one standard-deviation change and greenback exposure. Thus, for instance, one standard deviation more fractional currency (silver) is correlated with half of a percentage point more greenback exposure. Average 'high' greenback exposure is 10%, so this is a relatively small correlation. Balance sheet items (liabilities and assets) are in blue and red; green variables are economic conditions observed in the county of a bank, taken from the 1870 Census of Manufactures.

correlated with treatment. Banks who held one more standard deviation of fractional currency (silver), bank notes, or real estate tended to hold about half a percentage point more greenbacks on their balance sheets.[17]

Overall, the unconditional lending trends in figure 7 and the balance check in figure 12 suggests making a parallel trends assumption of some form. Using the potential outcomes notation from Rubin (1974), the form of this assumption is:

$$\begin{aligned} \forall (D_i, D_i', t) \qquad & \mathbb{E}\left[Y_{it}(0) - Y_{i,t-1}(0) \mid \mathbf{X}_i, D_i\right] \\ = \quad & \mathbb{E}\left[Y_{it}(0) - Y_{i,t-1}(0) \mid \mathbf{X}_i, D_i'\right] \end{aligned} \tag{1}$$

$Y_{it}(0)$ is counterfactual lending (or another outcome variable of interest) at bank i in time t had specie resumption not occurred. D_i and D_i' are two different treatment intensities (levels of greenback exposure in 1874). $\mathbf{X}_i$ are pre-treatment characteristics of bank i. This assumption thus says that trends in lending at banks with the same characteristics $\mathbf{X}$ in 1874, but different levels of greenback exposure in 1874, would have continued on parallel paths had specie resumption not occurred. In my baseline specification, Y_{it} are expressed in logs, and $\mathbf{X}_i$ is bank size in 1874. Below, I'll show that my results are reasonably robust to expressing Y_{it} in levels, and to using different control variables in $\mathbf{X}_i$.

4.2 Estimation results

This section uses a series of difference-in-differences estimators to test for the effect of specie resumption on banks. I'll first test for a direct effect: did specie resumption cause banks with more greenbacks to accumulate more gold? I'll then move on to test whether it caused these banks to lend more.

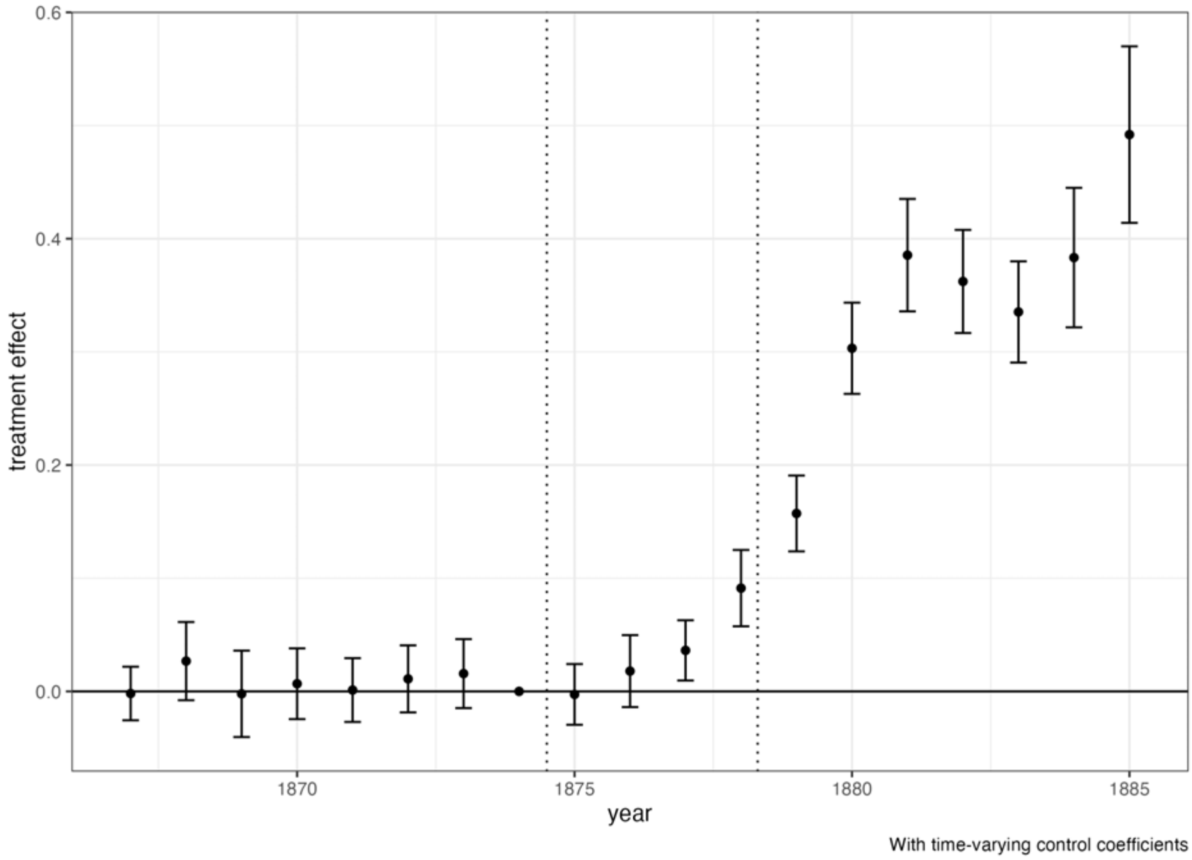
I'll begin by assuming that the treatment effect is homogeneous and linear across all banks, though it may be dynamic over time. That is, an outcome in time t for bank i with observed pre-shock greenback exposure of d_i can be written as:

$$Y_{it}(d_i) = Y_{it}(0) + \tau_t d_i$$

Under this assumption, a simple two-way fixed effects regression can identify the treatment

[17] Is this a concern for my first-stage identification? Bank notes were redeemable for greenbacks, meaning that they also increased in value after resumption. If increased bank note holding also affected banks, it arguably would have done so in the same way that greenback holding did: these two currencies functioned similarly and were similarly revalued. Since these two currencies were positively correlated, it suggests that treatment could just as well have been defined as greenback exposure plus bank note exposure. Further, bank notes were a very minor item on most banks' balance sheets (80% of banks held less than 2% of their assets in bank notes), meaning that the difference of defining treatment in this way is small. This correlation is thus unlikely to confound the identification of the effect of specie resumption. The role of real estate is unclear, but it is not obvious to me why real estate holdings would not have created divergent lending trends before 1874 and would have created them afterwards.

Figure 13: Effect of specie resumption on gold accumulation (TWFE)



effects τ_t:

$$Y_{it} = \alpha_i + \alpha_t + \beta_0 D_i + \sum_{t \neq 1874} \tau_t \left(\mathbb{1}_{t=t'} \times D_i \right) + \Gamma_t \mathbf{X}_i + \varepsilon_{it} \tag{2}$$

Y_{it} is the outcome of interest. D_i is greenback exposure in 1874 at bank i, and $\mathbf{X}_i$ are controls for pre-treatment bank characteristics.[18] Newey-West standard errors are used to account for serial correlation.

To begin, I use for Y_{it} the share of gold on banks' balance sheets, and I use bank size and reserve requirements in 1874 as X_i. Figure 13 plots the coefficients $\widehat{\tau}_t$ in this estimation, showing the treatment effect of specie resumption on gold accumulation. Banks with different levels of greenbacks before 1874 had no significant difference in specie exposure before that date. Beginning after 1876 – as the value of greenbacks began to rise – more exposed banks began accumulating more specie. Once resumption actually occurred, more treated banks quickly accumulated more specie. Since treatment is a continuous variable, these coefficients are elasticities: they say that a bank with 20% more greenback exposure in 1874 would have increased the share of gold on its balance sheet by 8-10% in the 5 years following resumption.

[18] Note that $\mathbf{X_i}$ is interacted with year indicator variables to give time-varying coefficients. While many TWFE implementations do not do this, it is in fact required: in the conditional parallel trends assumption above, the effect of $\mathbf{X}_i$ on bank lending trends is allowed to differ in each year. Results are similar (and the estimates are slightly larger) if these coefficients are not allowed to vary over time.

If the treatment effect is not homogeneous across all banks, this two-way fixed effects specification may not reliably identify average treatment effects (de Chaisemartin and D'Haultfœuille, 2020). In the appendix, I run a series of diagnostic tests to show that heterogeneity in the treatment effect is likely not a major problem. To further support this, I'll bin banks into three treatment groups according to their treatment intensities. I use the same intuitive bins shown in section 3: banks with below-median exposure are considered the control group (an average of 2% exposure); banks between the 50th and 90th percentile are considered medium-treated (an average 5% exposure), and banks above the 90th percentile are considered highly-treated (average 10% exposure). I do not have an econometrically principled way of selecting these bin-widths, but results are robust to defining these bins differently. Further, a procedure for optimally defining *two* treatment groups (treated and control) yields a comparable result.[19]

To notate the treatment bins, let G_{ig} (for $g \in \{C, M, H\}$) be an indicator variable for whether bank i is in group g (control, medium, or high treatment). I then calculate the non-parametric "doubly-robust" difference-in-differences estimator from Callaway and Sant'Anna (2021) for each group at each time period, comparing only banks with similar covariates. This will allow me to detect heterogeneity across treatment groups as well as the dynamic effect across time. The estimand is (supressing the i subscripts):

$$\forall t, \quad g \in \{M, H\} \qquad \tau_{gt} = \mathbb{E}\left[\left(\frac{G_g}{\mathbb{E}\left[G_g\right]} - \frac{\frac{p_g(X)G_C}{1-p_g(X)}}{\mathbb{E}\left[\frac{p_g(X)G_C}{1-p_g(X)}\right]}\right)\left(Y_t - \mathbb{E}\left[Y_t \mid X, G_C\right]\right)\right] \tag{3}$$

Y_t is now the change in outcome from a base year to year t, and G_g is an indicator for a bank's treatment group. X are controls (bank size and reserve requirements in my baseline specification). $p_g(X) \equiv P\left(G_g = 1 \mid X, G_g + G_C = 1\right)$: this is a bank's propensity to be in treatment group g, given covariates X and given that the bank is either in group g or the control (since these are the two groups being compared against each other in a given estimand). Thus, estimating τ_{gt} compares changes in lending for banks in group g (either medium or high exposure) against changes in lending for similar banks in the control group.[20]

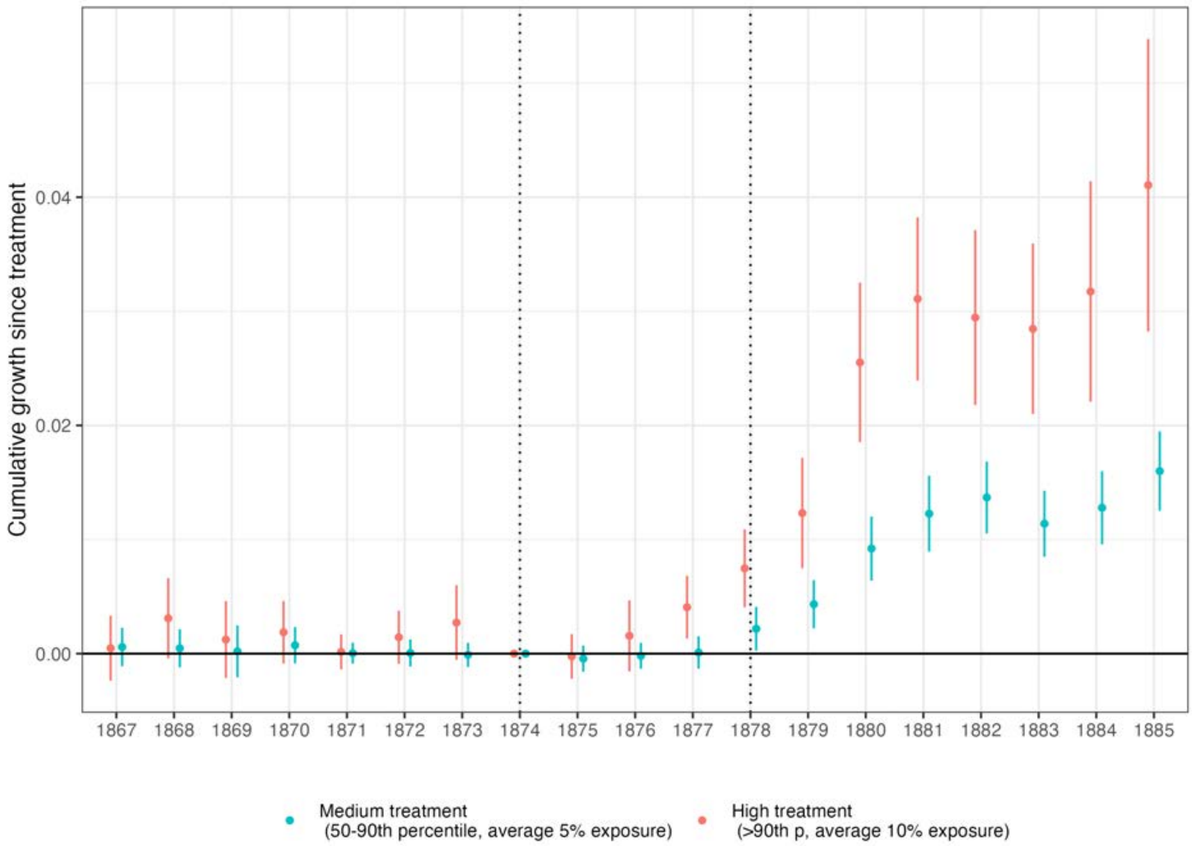
Figure 14 plots the estimates of each τ_{gt}, when Y_t is the change in the share of specie on a bank's balance sheet from 1874 to t.[21] The treatment groups have averages of 5% and 10% greenback exposure respectively; the estimates thus measure the treatment effect of moving from little or no exposure (less than 2%) to one of these two levels of exposure. Members of the 5% greenback exposure group have ATTs of around 1-2% more specie exposure, and members of the 10% group have ATTs of 3-4% exposure. This accords with the elasticity of 0.4 estimated in

[19] de Chaisemartin and D'Haultfœuille (2022) derived the optimal bandwidth for this setting when grouping units into two bins. In the appendix, I calculate the optimal bandwidth derived in de Chaisemartin and D'Haultfœuille (2022), and the corresponding estimators.

[20] The estimator is 'doubly robust' because it combines an outcome regression approach with an inverse-propensity-weighting approach; Callaway and Sant'Anna (2021) find that this is robust to misspecification in either approach.

[21] Analytic standard errors are computed following Callaway and Sant'Anna (2021).

Figure 14: Effect of specie resumption on gold accumultion (heterogeneity-robust DID)



the TWFE specification above. Together with the diagnostic tests in the appendix, this suggests that a homogeneous, linear treatment effect may not be a bad assumption in this setting.

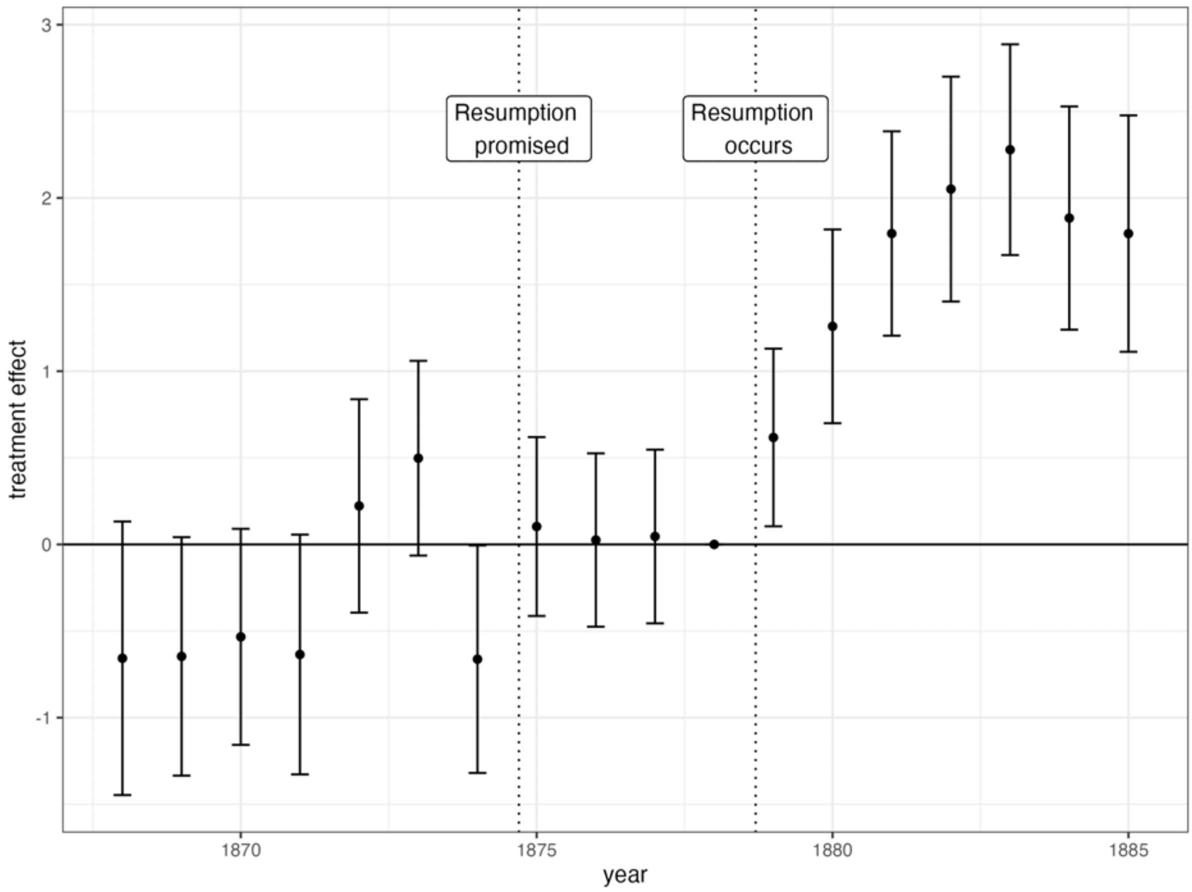
Banks within each treatment bin had different treatment intensities however – they held different amounts of greenbacks – and banks in the control group did experience some small nonzero treatment. As such, one may still be worried about treatment-effect heterogeneity within treatment groups as defined here, which could distort the estimates. In the appendix, I use results on fuzzy difference-in-differences from de Chaisemartin and D'HaultfŒuille (2018) to show that heterogeneity is unlikely to distort this estimator.

Figures 13 and 14, and the heterogeneity robustness checks presented in the appendix, demonstrate that specie resumption caused banks with more greenbacks to accumulate more gold. Did this cause them to lend more? I next repeat the steps in the above exercise to study bank lending.

Now, the outcome variable is log lending. I use 1878 as the reference year, so that the coefficients can be interpreted as the percent increase in loans after resumption occurred.[22] Figure 15

[22] I use 1878 and not 1874 as the base year because the credit expansion began after resumption occurred in 1879. The economic question of interest is the divergence in bank lending after resumption, i.e. after 1878. 1874 is the treatment year because banks could have anticipated that resumption would occur beginning in 1875, but I am most interested in comparing the change in bank lending after 1878. Using 1874 as the base year in this regression would of course just renormalize the coefficients, so that the coefficient in 1874 would be shifted up to

Figure 15: Effect of specie resumption on bank loans (TWFE)



Estimates of τ_t in equation (2), using bank size and reserve requirements in 1874 as $\mathbf{X}_i$. The outcome variable is log-lending, and 1878 is used as the reference year.

plots the estimates of τ_t in the TWFE specification, showing the dynamic effect of specie resumption on bank lending. More- and less-treated banks appear to have been on parallel lending trends before 1874, excepting a possible fluctuation from 1872 to 1873 (this fluctuation is not statistically significant, however). After resumption was promised, more exposed banks seem to have slightly increased their lending, but lending remained parallel until 1878. Then, once resumption actually occurred, more treated banks increased their lending. Since treatment is a continuous variable, these coefficients estimate elasticities: they say that specie resumption caused a bank with 10% more greenback exposure in 1874 to increase its lending by about 15-25% in the 4-5 years following resumption.

Figure 16 then calculates the binned doubly-robust difference-in-differences estimators from equation (3) for the effect on bank lending. The estimate is that 10% more greenback exposure in 1874 caused banks to increase their lending by 10-20% following resumption, while 5% more exposure caused an increased of 5-10%. This is mostly consistent with the elasticities of 1.5-2 estimated in the TWFE specification, although there may be some concavity in the treatment effect (the elasticity in the high group is slightly lower than in the medium group). The appendix shows that within-group heterogeneity is unlikely to be a severe problem, but

zero.

that some concavity may exist. This motivates adopting the binned treatment group approach as the preferred specification.

The bottom panels of figure 16 show that the results are robust to making different identifying assumptions. The top panel of figure 16 uses bank size in 1874 in the conditional parallel trends assumption. The bottom left panel uses both bank size and bank reserve requirements. Results are similar in both cases: 10% exposure in 1874 caused around a 15% increase in lending in the years after 1878.[23] The bottom right panel measures the outcome variable as changes in levels, rather than changes in logs. My estimation strategy relies on a (conditional) parallel trends assumption, which is sensitive to whether the outcome is measured in levels or in logs. In both cases however, parallel pre-treatment trends appear to hold reasonably well.[24]

4.3 Channels and magnitudes

How did specie resumption cause this increase in lending? In section 3, I found that highly treated banks increased their lending without increasing their loan-to-asset ratios. (Figure A4 in the appendix confirms this with formal estimation.) They lent more because they gained more assets; and the assets they gained were primarily specie. How did resumption cause treated banks to accumulate more specie? This subsection gives a suggestive quantification of the channels involved.

The average highly treated bank had \$1.3 million in assets, and held an average of \$150,000 in greenbacks in their vaults. Greenbacks made up 80% of the reserves kept at banks. As discussed in the appendix, banks could also hold some of their reserves on deposit with other banks in designated "reserve cities"; on average, highly treated banks held an additional \$60,000 in reserves on deposit. While I do not observe the paper vs. gold composition of these deposited reserves, assuming that they were composed similarly to the directly-held reserves brings the total nominal value of greenbacks held by these banks to about \$200,000.

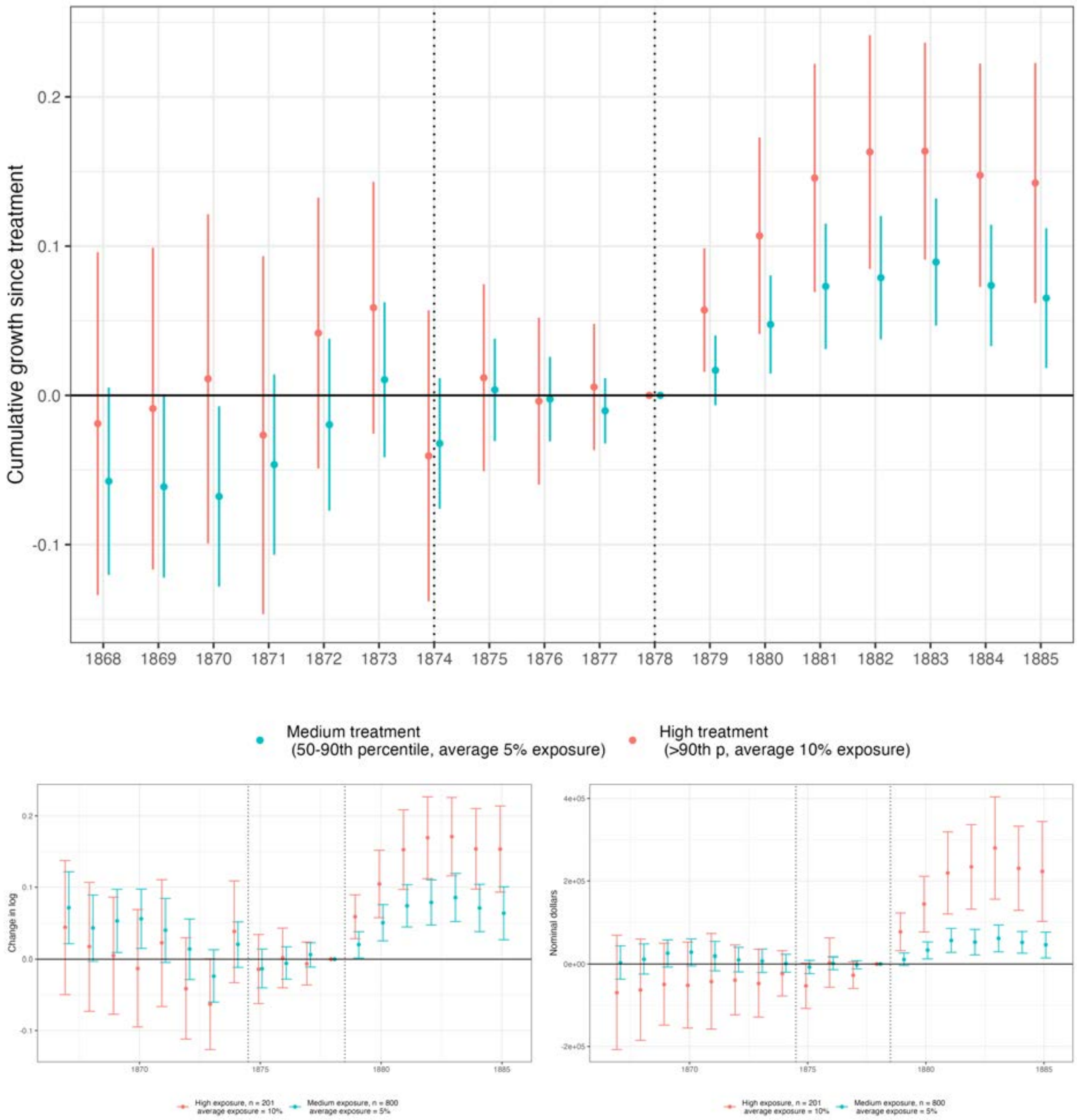
The difference-in-differences estimate is that resumption caused highly-treated banks to accumulate \$120,000 more in specie. Where did this accumulation come from? After the promise to resume specie payments, greenbacks appreciated in value by about 11%, meaning that the direct effect of resumption was to make these banks \$22,000 richer. About one fifth of their specie accumulation can thus be explained by the direct revaluation of the greenbacks they owned: when banks converted greenbacks into gold, they gained \$22,000 due to revaluation. Where did the rest come from?

Examining bank deposits can answer this question. Figure 17 shows changes in nominal deposits for each treatment group, demonstrating that specie resumption led highly treated banks to attract \$200,000 more in deposits than untreated banks. Banks in the sample save an average

[23]The appendix shows additional robustness checks: results are robust to making an unconditional parallel trends assumption, or to assuming parallel trends only conditional on bank size, city population, and also county-level pre-shock economic conditions.

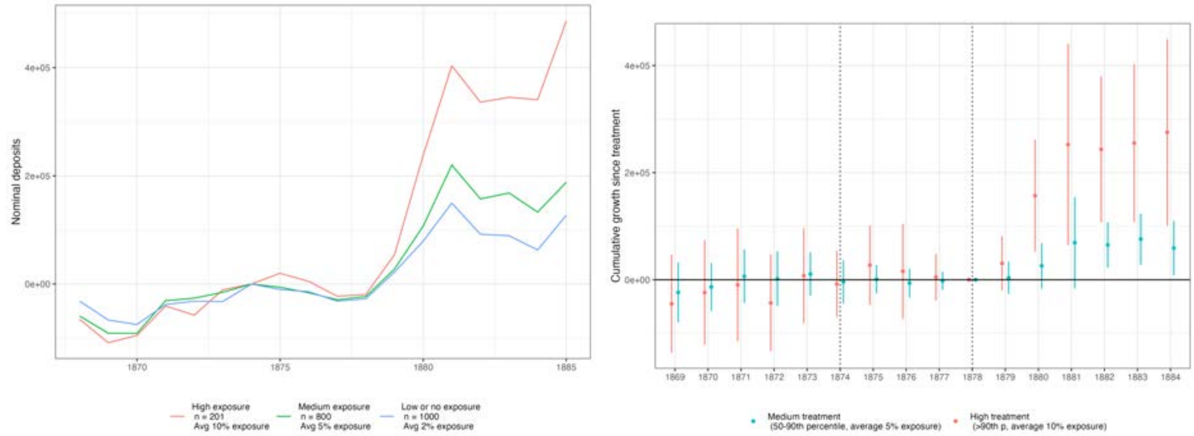
[24]However, when measured in levels, 10% exposure causes an increase in lending of about \$200-300,000. Average baseline lending for highly treated banks was \$700,000, so this estimate is of a 30-40% increase, which is larger than the estimated effect on log-lending.

Figure 16: Effect of specie resumption on bank loans (heterogeneity-robust DID estimates)



Notes: these estimates implement the doubly-robust inverse propensity weighted estimator from Callaway and Sant'Anna (2021). The top panel shows changes in log loans, assuming that bank lending was parallel across groups conditional on pre-shock bank size. The bottom left panel assumes parallel trends conditional on both size and reserve requirements. The bottom right panel shows changes in nominal loans, conditional on both bank size and bank reserve requirements. The results using logs are compatible with results in figure 15. The appendix includes additional robustness checks.

Figure 17: Change in deposits for different treatment groups



Notes: The left panel shows average deposits over time in each treatment group. The right panel implements the doubly-robust difference-in-differences estimator described in the previous subsection in the same three groups. Motivated by the left panel, it assumes unconditional parallel trends and measures the outcome in nominal dollars.

of 40% of their deposits as reserves, so this would have accounted for an additional $80,000 in specie accumulation. Combining this with the $22,000 in specie due to the direct revaluation effect thus accounts for the majority of treated banks' extra specie accumulation, which in turn accounts for the relative asset growth that allowed them to increase their lending. This exercise thus suggests that 20% of the increase in specie accumulation was via the revaluation channel, and 80% was due to the deposits channel.[25]

This exercise thus provides a suggestive quantification of the channels proposed in Drechsler et al. (2017) and Gomez et al. (2021): in my setting, a policy-induced revaluation of bank assets accounts for around 20% of the change in lending, while the deposits channel accounts for the rest.

Finally, how much of the post-1878 credit expansion (shown in figure 3) was caused by specie resumption? The estimates in the previous section suggest that it was between 12-15%: average greenback exposure in 1874 was just under 4%, which according to the estimates in this section would have led the average bank to increase its lending by 6% in the years following resumption (as discussed above, a homogeneous linear treatment effect across all banks appears to be a reasonable assumption). Aggregate lending in 1878 was $802 million, meaning that resumption was responsible for a $46 million increase in lending across all banks. The total increase in lending from 1878-1884 was $272 million, so resumption itself was responsible for around 15% of the total increase.

The results in this section suggest using greenback exposure in 1874 as an instrument for bank lending. In the next section, I use this instrument to study the causal effect of loans on the likelihood of bank failure. I then turn to studying the effect of loans on regional economic outcomes.

[25] Jaremski and Rousseau (2018) observe the rise in aggregate deposits after specie resumption, and find evidence that the increase was due to increased depositor confidence. Figure 17 suggests that this increase in confidence was differentially larger at highly treated banks. This makes sense – these banks and their depositors were those who gained the most from the reduction in greenback risk.

5 Second stage: the real effects of bank loans

5.1 Loans and bank failure

Does increased bank lending raise the probability of bank failure? A number of recent papers find a strong and robust correlation between private credit and financial crises at the aggregate level (Schularick and Taylor, 2012; Jordà et al., 2013, 2016; Baron and Xiong, 2017; Mian et al., 2017; Mian and Sufi, 2018; Baron et al., 2020; Greenwood et al., 2022). This section provides identified micro-evidence for a causal relationship.

To identify the effect of lending on bank failure, I use greenback exposure in 1874 as an instrument. This strategy requires that greenback exposure did not change bank failure probability, other than by increasing bank loans. Is this feasible, in light of the balance check in figure 12? Among banks with similar reserve requirements, those with more greenbacks also held more bank notes. But since both of these assets were safe and liquid currencies, it is unlikely that they would have increased failure probability – if anything, they would have reduced it, meaning that using greenbacks as an instrument for lending will give conservative estimates.

The OCC reports I use in this paper contained complete lists of all banks that failed in each year (these were banks that were either liquidated or placed into the hands of 'receivers,' federal agents tasked with managing insolvent banks). I use these lists to code a variable for whether a bank failed in a given year. 173 banks failed between 1879 and 1884 – were these failures caused by increased lending?

To answer, I estimate linear and probit models, predicting whether a bank failed at some point between 1879 and 1884 by the percentage increase in loans issued between 1878 and the bank's last year in the panel:

$$
\begin{aligned}
P\left(F_i \mid \Delta y_i, \mathbf{X}_i\right) &= \alpha_1 + \beta_1\left(\Delta y_i\right) + \Gamma_1' \mathbf{X}_i \\
P\left(F_i \mid \Delta y_i, \mathbf{X}_i\right) &= \Phi\Big(\alpha_2 + \beta_2 \Delta y_i + \Gamma_2' \mathbf{X}_i\Big)
\end{aligned}
$$

F_i is the indicator for whether bank i failed between 1879 and 1884; Δy_i is the change in log-loans in bank i between 1878 and its last year in the panel; and $\mathbf{X}_i$ are controls measured at bank i before treatment. In the reported results, I include controls for bank size and for the population of a bank's city in 1870.[26] Φ is the CDF of the standard normal distribution. I estimate these two regressions first without instrumenting, and then using greenback exposure in 1874 as an instrument for Δy_i.[27]

Table 1 reports the results. Columns 1-2 and 5-6 show the uninstrumented regressions. The coefficients on lending are negative: a 15% increase in loan growth is correlated with a 2% decrease in failure probability. This is unsurprising: unobserved variables that make a given

[26] Results are robust to also including a control for bank reserve requirements.

[27] To instrument a probit regression, I follow the procedure first proposed in Newey (1987). I use an indicator for whether a bank failed at all between 1879 and 1884, rather than predicting bank failure in each year, because the number of bank failures in each year is very low. This prevents me from studying bank failure using a proper event study; using an indicator for bank failure over the entire credit expansion gives the analysis more power.

bank's failure more likely (such as worsening regional economic conditions) would likely also make the bank decrease its lending.

Using greenback exposure as an instrument corrects for this ommitted variable bias. In both the linear and probit models, the relationship becomes positive (and is statistically significant) when using the instrument – these results are reported in columns 3-4 and 7-8.

The bottom rows of the table interpret the coefficients: a 15% increase in loan growth causes the probability of bank failure to increase by 4.8-6.1%. This is a statistically significant, large effect. The greenback instrument shows that the well-established correlation between loan growth and failure at the macroeconomic level is a causal relationship at the bank level – at least in the 19th century US.[28]

5.2 Lending and capital investment

Did increased bank lending fund productive investment? In this section I examine the effects of regional lending by assuming that loan markets were regionally segmented. This follows Xu and Yang (2022) and Carlson et al. (2022): banks in this period were prohibited by law from opening multiple branches, and regulations required that 75% of a bank's directors were local residents. These laws, combined with the more limited nature of financial and communications technology in the 1870s, made it likely that bank lending was regionally segmented. If a firm wanted to borrow, they likely would have needed to do so from a nearby bank.

I focus on the construction and renovation of iron furnaces. Iron manufacturing accounted for 5% of all manufacturing output, as well as for 10% of the US capital stock, in 1880. Building and renovating iron furnaces was important for US industrialization. To what extent was this construction enabled by bank lending?

To answer, I built a dataset measuring investment in construction and renovation at every operational iron blast furnace in the US. These data are from the biannual directory of furnaces described in section 3. The directory lists the overall size (volume of the blasting stacks) and capacity (annual tons of output) of each of the country's 663 operational furnaces. It also notes the dates that construction and/or renovation occurred at each furnace. By combining the changes in observed sizes and capacities with the dates of renovations, I can quantify the amount of construction that occurred at each furnace. Then, using a cross section from the 1880 Census of Manufactures, I am able to estimate the dollar cost of expanding furnace capacity.[29] This allows me to observe the dollar amount invested in each furnace from 1876 to 1884.[30]

[28]In an extension of this paper, I'd like to look more closely at this result. If I can find more annual or biannual data on regional economic outcomes, I could see whether bank failures generate regional economic busts, for instance. Also, so far I have only examined economic outcomes on the upswing of the credit cycle, from 1878 to 1884 (this is what I do in the following subsection). I'd like to see what happens after 1884, when there was a slight downturn. Regions with more greenback exposure experienced positive economic outcomes from 1875 to 1880 and from 1878 to 1884; did they experience downturns afterwards? I have not explored this question in the current draft.

[29]The appendix provides the details of this estimation.

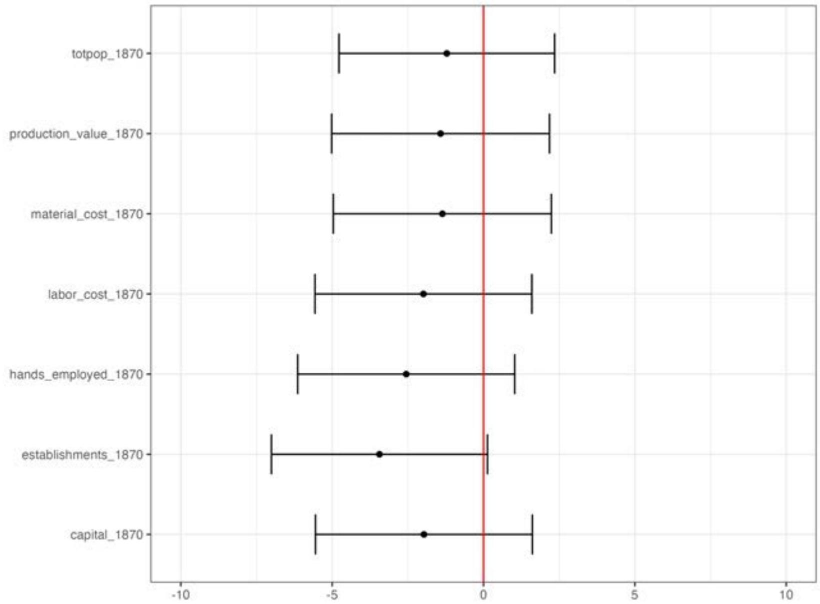
[30]The standard errors reported in my results in this section do not take into account the fact that my outcome variable is the result of this first stage estimation procedure. This is a limitation, but because the first stage estimate is very precise, I do not think results would be greatly affected by accounting for this in the standard errors. In an extension, I could attempt to derive an expression for the second stage standard errors that take

Table 1: Effect of loan growth on failure probability

	Dependent variable:							
	Bank failure							
	Linear		IV Linear		Probit		IV Probit	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δ log(Loans)	-0.134^{***}	-0.149^{***}	0.318^{***}	0.405^{**}	-1.037^{***}	-1.174^{***}	2.220^{**}	2.760^{**}
	(0.016)	(0.016)	(0.123)	(0.176)	(0.139)	(0.148)	(0.939)	(1.331)
log(Bank size)	-0.030^{***}	-0.056^{***}	-0.031^{***}	-0.002	-0.250^{***}	-0.451^{***}	-0.255^{***}	-0.065
	(0.006)	(0.010)	(0.007)	(0.021)	(0.059)	(0.093)	(0.065)	(0.170)
log(Bank city population)		0.017^{***}		-0.018		0.141^{***}		-0.111
		(0.005)		(0.013)		(0.047)		(0.102)
Effect of 15% increase in loan growth	−2%	−2.2%	4.8%	6.1%	−1.4%	−1.5%	4.8%	6.2%
Effect of one-s.d. increase in loan growth	−5.1%	−5.7%	12.1%	15.4%	−2.9%	−3.1%	16.9%	23.4%
Observations	1,992	1,914	1,992	1,914	1,992	1,914	1,992	1,914

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Figure 18: Balance check: correlation between county economic characteristics and county-level greenback exposure



Notes: results from regressing county-level greenback exposure in 1874 on county-level variables from the 1870 Census of Manufactures. Regressors are normalized so that the coefficients measure the correlation between each standard-deviation increase of the variables and greenback exposure in a county.

With these data, I can ask whether inreased bank lending funded productive investment in furnace construction. I calculate both the 1874 greenback exposure and the change in lending in a furnace's region. I begin by defining the region of each furnace to be a 50-mile radius around it. In robustness checks, I try radii of 25, 75, and 100 miles. Then using regional greenback exposure as an instrument, I estimate the causal effect of a regional increase in bank lending on investment.

This empirical strategy requires that the first stage result presented in section 4 holds betwee a *region's* greenback exposure and its lending. In an appendix, I repeat the analysis of section 4 at the regional level. Aggregating bank lending and greenback exposure dilutes the variation in the sample, and the first stage relationship is less strong than at the bank level. However, the relationship is still sizable and significant, and as I'll show below, using regional greenback exposure as an instrument for regional bank loans passes the weak instrument test.

Most importantly, this analysis requires that regions with more and less greenback exposure were comparable before resumption. Figure 18 shows that they were: there is no significant correlation between a region's greenback exposure and any of the economic variables observed in the 1870 Census of Manufacturing.[31]

into account both the greenback instrument and this additional imputation, but I'm not sure if that's possible.

[31] These variables are: total population, the value of manufacturing output, the value of inputs to manufacturing, the total amount spent on labor, the number of workers employed, the number of manufacturing establishments, and the value of capital stock.

To estimate the effect of loans on investment, I estimate the following equation:

$$\Delta_{1876}^{1884}\left(y_{ir}\right) = \beta\left(\Delta_{1878}^{1884}\widetilde{\log \text{loans}_r}\right) + \gamma x_i + \varepsilon_{ir}$$

The outcome variable is the change in the value of the capital stock (measured in both levels and logs) between 1876 and 1884 at furnace i in region r due to investment in construction. I regress this on the change in log-loans in a furnace's region between 1878 and 1884, instrumenting for the change in lending with regional greenback exposure in 1874. I also include a control for the furnace's initial size in 1876 (which is zero if the furnace was unbuilt). Standard errors are clustered at the regional level.

This regression thus asks whether a larger increase in lending caused more capital accumulation. Table 2 reports the results. Each percentage point increase in lending is estimated to have caused a 1.5% increase in investment (columns 1-2). Columns 3-4 give a sense of the magnitudes involved. The average lending in a region with a furnace was about \$3 million. A 1% increase in regional lending is thus a \$30,000 increase. The estimates in columns 3-4 are that this lending increase would have generated \$1,200 in furnace investment. This is reasonably compatible with columns 1-2: the average value of a furnace's fixed capital was about \$125,000 in 1876; if a 1% increase in regional lending caused a 1.7% increase in capital investment between 1876 and 1884, the magnitude of this increase would be about \$2,000.

Table 2: Greenback IV: effect of regional loans on factory investment

	Dependent variable:			
	ΔInvestment (pct.)		ΔInvestment (\$1,000)	
	(1)	(2)	(3)	(4)
Δ log(Loans) within 50 miles	1.609**	1.739**	1.234**	1.222**
	(0.756)	(0.759)	(0.557)	(0.554)
Furnace size in 1876		−0.007***		0.001
		(0.002)		(0.002)
Weak Instruments	0***	0***	0***	0***
Observations	451	451	451	451
R^2	−0.215	−0.232	−0.269	−0.263
Note:				*p<0.1; **p<0.05; ***p<0.01

The appendix reports results for alternate definitions of furnace regions. The estimates remain significant and similarly sized when considering bank lending within 25, 75, and 100 miles of each furnace.

Bank lending funded investment in iron furnaces. Does this result generalize to other industries? I'll provide suggestive evidence that it might, using data at the county-industry level from the decadal Census of Manufactures. The Census collected data on 200 detailed industries in every county, and aggregated these into 35 broad industry categories. Using these broader industries, I estimate:

$$\Delta_{1870}^{1880}(y_{jc}) = \alpha + \beta\left(\Delta_{1874}^{1880}\widetilde{\log \text{loans}_c}\right) + \Gamma'\mathbf{X}_{jc} + \varepsilon_{jc}$$

The outcome variable is the change in log capital in industry j in county c. I regress this on the total change in log-lending in county c from 1874 to 1880, instrumenting with the county's greenback exposure in 1874. I include controls for a county's baseline input material costs, capital, labor costs, and output value (all measured in log dollars) as well as its population in 1870. I also estimate this specification including industry and state fixed effects, both separately and together.

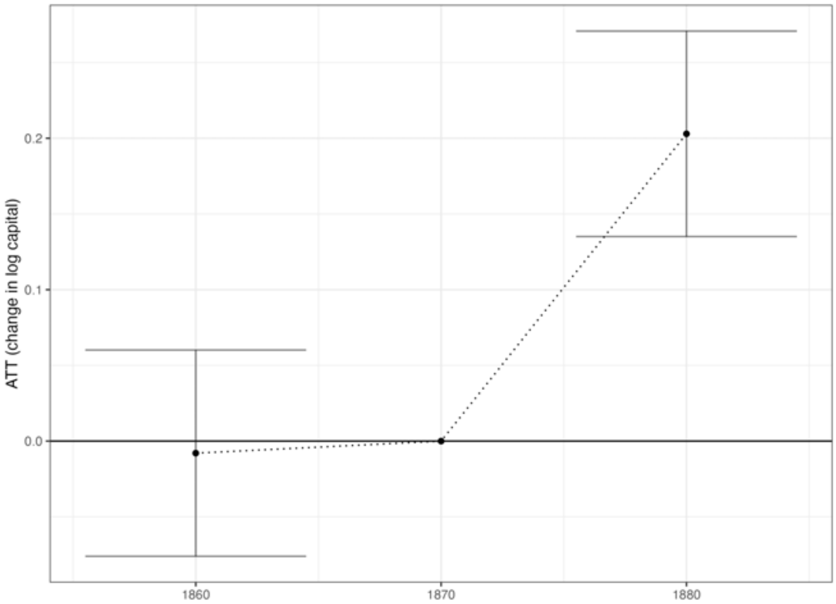
Table 3: DID: Effect of high exposure across all industries

	Dependent variable:			
	Δ log(Capital)			
	(1)	(2)	(3)	(4)
Δ log(Loans)	0.796***	0.673**	3.119**	2.286**
	(0.219)	(0.342)	(1.548)	(0.921)
Input cost (1870)	−0.030	−0.052	−0.041	−0.062
	(0.036)	(0.069)	(0.032)	(0.079)
Labor (1870)	−0.075***	0.134**	−0.092*	0.099*
	(0.020)	(0.060)	(0.048)	(0.054)
Capital (1870)	−0.516***	−0.700***	−0.525***	−0.714***
	(0.021)	(0.032)	(0.041)	(0.034)
Output value (1870)	0.373***	0.302***	0.415***	0.337***
	(0.053)	(0.072)	(0.053)	(0.076)
Population (1870)	0.184***	0.242***		0.217***
	(0.018)	(0.052)		(0.076)
Fixed effects	None	Industry	State	Industry-state
Observations	6348	6348	6348	6348

Note: *p<0.1; **p<0.05; ***p<0.01
Test notes

Table 3 shows the results. They appear mostly to be compatible with the above findings for iron furnaces. Within the same industry, a 10% increase in regional lending is estimated to cause a 6% increase in the capital stock. Within the same state, industries in counties that experienced more lending are estimated to have raised investment by 2 percentage points for every percentage of increased lending. The low-frequency nature of these data mean that these results are largely suggestive. They should be taken only to indicate that it is plausible that my findings above generalize to other industries.

Figure 19: Capital accumulation in industries in treated vs. untreated counties



Difference-in-differences estimation comparing treated and untreated counties with similar characteristics. This estimation implements a conditional parallel trends assumption: it conditions on county population, and industry capital, labor, and output in 1860 and 1870, following Sant'Anna and Zhao (2020). See text.

An alternative approach is to estimate a difference-in-differences between treated and untreated counties, rather than instrumenting for loans. There are many economic variables that affect capital accumulation across industries and counties.[32] I thus again use the approach of Callaway and Sant'Anna (2021) (the estimator in equation (3)) to compare only treated and untreated industries that were ex-ante economically similar. As covariates on which to condition, I use the values of an industry's capital, labor, and output in 1870, as well as the population of its county in both 1860 and 1870 (to make sure results are not driven by industries in counties that were on a high ex-ante growth path). Figure 19 computes the difference-in-differences estimator in equation (3), conditioning on these covariates. The treatment effect of 10% greenback exposure (which caused a 15% increase in lending) is about a 20% increase in capital, which is compatible with the elasticities estimated previously. The pre-treatment placebo coefficient is zero, as it would need to be if the industries I compare were indeed on parallel trends prior to specie resumption.

Bank lending funded investment in fixed capital in the 1870s. As discussed above, banks could only operate locally, and they were also forbidden from taking real estate as collateral. This prevented banks from making loans to mortgages or to agricultural production – farmers usually borrowed against their land.[33] Regulations thus channeled bank loans to industrial investment. Was this distortionary? In the next section, I introduce a simple model of the supply-side of the 19th century US industrial economy to suggest that it was not.

[32] Nonetheless, figure C3 in the appendix shows that even unconditionally, treated and untreated counties seemed to be on parallel trends of capital accumulation from 1860 to 1870.

[33] As a result, state governments chartered their own institutions to lend to farmers, at times setting explicit farmer-loan quotas for these institutions.

6 Lending and efficient allocation

Did bank regulations lead bank lending to misallocate capital? And what was the effect of changes in misallocation on growth? Answering the first question requires specifying production functions for the 19th century economy. Answering the second question requires considering a counterfactual: how much of TFP growth was due *only* to increased bank lending, rather than to other technological changes?

In this section, I continue using county-level data on 35 industries from the Census of Manufactures. I use these 35 industries to model the supply side of the US economy around 1880, based on the aggregation framework from Hsieh and Klenow (2009) and Sraer and Thesmar (2023). The model is as follows.

Each sector s produces output Y_s, by combining the output from each county with a CES technology. Output of sector s in county i is y_{is}. Each county's output is itself produced with Cobb-Douglas technology, using capital and labor. All 35 sectors then aggregate their intermediate outputs into a final good, according to a Cobb-Douglas aggregation. The output of the final good is Y. That is:

$$\underbrace{Y = \prod_s Y_s^{\phi_s}}_{\text{Total output (GDP)}} \qquad \text{and} \qquad \underbrace{Y_s = \left(\int_i y_{is}^{\theta_s}\right)^{\frac{1}{\theta_s}}}_{\text{Aggregate output, sector } s} \qquad \text{and} \qquad \underbrace{y_{is} = e^{z_{is}} k_{is}^{\alpha_s} l_{is}^{1-\alpha_s}}_{\text{Output of sector } s \text{ in county } i}$$

k_{is} and l_{is} are capital and labor inputs, and z_{is} is the unobserved log-productivity, of county-sector is. α_s, θ_s, and ϕ_s are sector-specific parameters. Price optimization and CES production give an expression for county-sector revenue:

$$p_{is} y_{is} = \frac{Y}{Y_s^{\theta_s}} \left(e^{z_{is}} k_{is}^{\alpha_s} l_{is}^{1-\alpha_s}\right)^{\theta_s}$$

As Bau and Matray (2023) observe, it is standard in the literature on capital misallocation to assume a Cobb-Doubglas revenue production function of this form, in which capital and labor are measured in value terms. With this functional form, the marginal revenue product of capital is

$$MRPK_{is} \equiv \frac{\partial p_{is} y_{is}}{\partial k_{is}} = \alpha_s \theta_s \left(\frac{p_{is} y_{is}}{k_{is}}\right)$$

This conveniently allows me to estimate $MRPK$ at the county-sector level, because I observe $p_{is} y_{is}$ and k_{is}. I then obtain estimates of each α_s and θ_s from the data. Cobb-Douglas revenue production gives

$$\log(p_{is} y_{is}) = C_s + \alpha_s \log(k_{is}) + (1 - \alpha_s) \log(l_{is}) + z_{is}$$

and so regressing log revenue on log capital and labor for a each industry (using the cross-section available from the census data) gives estimates of α_s. Then, θ_s are the elasticities of substitution across counties for each sector; I estimate these using a method proposed in Ahmad and Riker

(2019):

$$\widehat{\theta}_s = \frac{P_s Y_s - (\text{labor costs}_s + \text{input costs}_s)}{P_s Y_s}$$

Aggregate sectoral revenue, labor costs, and input costs are all observed in the census for each of the 35 sectors. With these parameter estimates in hand, I can estimate the marginal revenue product of capital for each county-sector. In addition, I obtain estimates of the ϕ_s (for later use in aggregation). To do this, observe that price optimization in the final good gives $P_s Y_s = \phi_s P Y$. Then ϕ_s are easily estimated as the share of each sector's output value in GDP.

Sraer and Thesmar (2023) note that, in standard macroeconomic models that follow this set up, the distribution of log-MRPK should be normal. Observing the distribution of the estimates I obtain (in appendix figure D2) confirms that this is indeed the case with the above calibration.

Specie resumption caused an exogenous increase in lending in some counties. The previous section shows that it also increased the average amount of capital in those counties. But what happened to capital *allocation* in those counties? Were loans allocated to higher productivity industries? To answer, I set up a difference-in-differences between counties that were and were not treated by specie resumption. Specifically, I compare highly treated counties (those with above-90th-percentile exposure) to a control group of below-median counties; I drop the 'medium-treated' counties from the sample. With decadal observations, there are only two periods (pre- and post treatment). In this simplified design, regression can identify the treatment effects of interest. Following Bau and Matray (2023), I estimate two equations:

$$\Delta y_{is} = \alpha_1 + \beta_1 I_{\text{treated}_i} + \Gamma_1 \mathbf{X}_{is} + \alpha_{1s} + \varepsilon_{is}$$

$$\Delta y_{is} = \alpha_2 + \beta_{21} I_{\text{treated}_i} + \beta_{22} \left(I_{\text{treated}_i} \times I_{\text{High MRPK}_{is}} \right) + \Gamma_2 \mathbf{X}_{is} + \alpha_{2s} + \nu_{is}$$

The outcome variables are measured in changes in logs from 1870 (pre-treatment) to 1880 (post). I'll use both log-capital and log-MRPK as outcomes. I_{treated} is an indicator for whether county i was treated by specie resumption (i.e., whether county i had an above-90th-percentile exposure to greenbacks). Included controls are pre-treatment county-sector capital, labor, and output, and pre-treatment county population. Standard errors are clustered at the county level. I estimate equations both with and without industry-fixed effects.

In the first equation, β_1 gives the treatment effect on industries in treated counties.[34] Table 4 shows regression estimates of β_1. Treated counties had an average of 10% greenback exposure, which would have caused an exogenous increase in lending of about 15%. The first two columns show that this led treated counties to increase their capital by 15-20%, which is compatible with the estimates in the previous section. Columns 3-4 suggest that there was no or minimal change in the average treated industries' log MRPKs.

The second equation allows me to study the effect of treatment on capital allocation. $I_{\text{High MRPK}}$ is an indicator variable for whether a county-sector had high ex-ante MRPK – in the reported

[34]It is comparable to the ATT estimated in figure 19.

Table 4: Effect of treatment, all industries

	Dependent variable:			
	Δ log Capital		Δ log MRPK	
	(1)	(2)	(3)	(4)
$I_{\text{treatment}}$	0.195***	0.170***	0.002	0.009***
	(0.063)	(0.049)	(0.003)	(0.003)
Observations	4423	4064	4064	4064
Industry fixed effects	No	Yes	No	Yes
Note:		*p<0.1; **p<0.05; ***p<0.01		

results, it is coded as one if and only if the MRPK was above 0.3 (the 75th percentile) in 1870.[35] If the lending induced by specie resumption reduced misallocation, it would have helped allocate capital first and foremost to industries with high ex-ante MRPKs. These are the industries that would use the capital most efficiently. β_{22} tests whether this was the case: it measures the differential effect of resumption on high relative to low ex-ante MRPK firms. When capital is the outcome variable, a positive β_{22} means that resumption caused more capital accumulation at industries with the highest pre-treatment MRPKs.

Table 5 reports the estimates of β_{21} and β_{22}. In columns 1-2, the coefficient on the interaction term is significant, and is almost twice as large as the coefficient on I_{treated}. This means that industries with higher ex-ante marginal products were indeed the ones that resumption caused to accumulate more capital. All treated industries increased capital – low-MRPK industries increased their capital stocks by 15% – but the industries who 'should' have received capital increased their stocks more (by around 45%). Columns 3-4 provide further evidence that misallocation was reduced: the negative coefficients mean that the MRPKs of ex-ante highly productive industries fell following treatment. Because the Cobb-Douglas function has diminishing returns, this is a sign of reduced misallocation. Capital was allocated to industries that needed it, allowing their MRPKs to fall as they became less capital-scarce. The inverse occurred as well, as MRPKs in less-productive industries rose slightly. The variance of the economy's MRPK fell as such. In the misallocation literature, this is a telltale sign of an improvement in allocative efficiency (Bau and Matray, 2023).

Was this improvement in allocation economically significant? Sraer and Thesmar (2023) provide an aggregation framework that can give a suggestive answer. The framework studies the following counterfactual. In my setting, only some counties within each industry were treated. What is the change in aggregate TFP that would have been caused by treating *all* counties, had this been the *only* change in the economy? This counterfactual thus measures the impact that a nation-wide 15% increase in bank lending would have had on TFP in the 1870s. Sraer and Thesmar (2023) show that, for a large class of macroeconomic models with a supply side as described above, this change can be estimated using only two sufficient statistics: $\Delta\Delta\mu(s)$, the

[35]Results are robust to using alternative thresholds.

Table 5: Interaction effect of treatment on ex-ante low vs. high productivity industries

	Dependent variable:			
	Δ log(Capital)		Δ log(MRPK)	
	(1)	(2)	(3)	(4)
$I_{\text{treatment}}$	0.163***	0.169***	0.007**	0.009***
	(0.046)	(0.048)	(0.003)	(0.003)
$I_{\text{treatment}} \times I_{\text{High MRPK}}$	0.299*	0.294*	−0.113***	−0.107***
	(0.179)	(0.174)	(0.021)	(0.014)
Observations	4064	4064	4064	4064
Industry fixed effects	No	Yes	No	Yes

Note: *p<0.1; **p<0.05; ***p<0.01
Outcomes regressed on a treatment indicator, the interaction term, and controls for capital, labor, output, and population in 1870 (not shown in table). See text.

difference-in-differences change in average log-MRPK between treated and untreated counties in each sector s, and $\Delta\Delta\sigma^2(s)$, the difference-in-differences change in the variance of log-MRPK. The authors then derive an expression for the aggregate effect on TFP, if every county had been treated with the exogenous increase in bank lending:

$$\Delta\log(\widehat{TFP}) \approx -\frac{\widehat{\alpha}^*}{2}\sum_{s=1}^{35}\widehat{\kappa}_s\left(1+\frac{\widehat{\alpha}_s\widehat{\theta}_s}{1-\widehat{\theta}_s}\right)\widehat{\Delta\Delta\sigma^2}(s)$$
$$-\sum_{s=1}^{35}\left(\widehat{\phi}_s\widehat{\alpha}_s-\widehat{\kappa}_s\widehat{\alpha}^*\right)\left(\widehat{\Delta\Delta\mu}(s)+\frac{1}{2}\frac{\widehat{\alpha}_s\widehat{\theta}_s}{1-\widehat{\theta}_s}\widehat{\Delta\Delta\sigma^2}(s)\right)$$

The parameters and difference-in-differences are as described above. In addition, $\alpha^* = \sum_s \phi_s\alpha_s$ is a weighted average of capital shares, and κ_s is the share of industry s capital stock in the total capital stock (pre-treatment). Sraer and Thesmar (2023) further show that the first term measures the change in TFP due to *across-sector* reallocation, while the second term measures the effect of *within-sector* reallocation. Computing the relevant difference-in-differences estimates and using the parameter values from the data, I find that the first term is 1.0%, and the second term is 2.3%. I thus find a 3.3% increase in TFP from 1870 to 1880, the majority of which is due to across-sector reallocation.

A common estimate of annual TFP growth in the late 19th century US seems to be around 2%. (Shackleton, 2013). This is about 19.5% over 10 years, of which the additional 3.3% estimated here would be a nonnegligable portion. In this setting, bank lending thus appears as an important contributor to US growth.

7 Conclusion

This project analyzes several ways that bank lending affects the real economy. In a relatively tight regulatory environment, bank lending funded regional investment, improved the allocation of capital, and contributed to US growth. The literature on misallocation usually studies the benefits of financial liberalization; in contrast, this paper suggests that tighter control of credit may not have impeded US industrialization. Perhaps credit controls do not need to come with misallocation.

This paper also finds that increased lending raised the probability of bank failure. I thus provide evidence that bank loans can cause both instability and growth.

Finally, to my knowledge this paper is the first well-identified, quantitative study of the effects of specie resumption. While historically important in its own right, the US government's revaluation of its unbacked paper currency was also a sort of 19th century monetary policy shock. This shock affected banks both by revaluing their assets directly, and by changing their abilities to attract depositors. Both of these channels in turn affected bank lending, with the depositor channel accounting for a larger share of the effect. This setting thus allows for quantification of two channels by which monetary policy affects the banking sector, and thereby the real economy.

Future extensions

There are two possible extensions to the project I'm considering. The first would use the 1874 change in reserve requirements to improve identification of randomly treated banks. The change in requirements means that I can observe banks that had a high preference for greenbacks: those for whom the pre-1874 reserve requirements were not binding, and who as such did not decrease their greenbacks when given the chance. I can then decompose bank greenback holding each year into a non-random, preference-based component, and a random component. This could allow me to extract a more truly-random component of treatment and improve my design.

The second extension would attempt to study the link between my results. I find that bank lending increased capital accumulation,improved allocation, and helped growth, while also increasing instability. However, I only study the effect on capital accumulation and allocation on the upswing of the credit cycle, from 1874 to 1884. There was an economic downturn in 1884 – was it worse in places that had seen larger increases in credit beforehand? In addition, I would be interested to see whether counties that saw banks fail experienced localized downturns. This could connect my result more convincingly to the literature on credit and crises. Both of these extensions would involve further data collection, ideally at a high enough frequency to detect the difference between the upswing into 1884 and the downturn afterwards. I could extend my iron dataset to do this, or look for additional higher frequency data on regional observations. I could use the 1890 publication of the Census of Manufactures to study this as well.

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Figure A1: Sample page from OCC balance sheet reports.

REPORT OF THE COMPTROLLER OF THE CURRENCY. 307

NEW JERSEY.

First National Bank, Paterson.

JOHN J. BROWN, *President.* No. 329. JOHN SWINBURNE, *Cashier.*

Resources.	
Loans and discounts	$1, 074, 744 62
Overdrafts	21 92
U. S. bonds to secure circulation	182, 500 00
U. S. bonds to secure deposits	....
U. S. bonds on hand	....
Other stocks, bonds, and mortgages	55, 470 00
Due from approved reserve agents	271, 098 68
Due from other banks and bankers	170 10
Real estate, furniture, and fixtures	89, 332 14
Current expenses and taxes paid	5, 671 27
Premiums paid	....
Checks and other cash items	13, 378 33
Exchanges for clearing-house	....
Bills of other banks	9, 862 00
Fractional currency	21 52
Specie	33, 390 00
Legal-tender notes	34, 801 00
U. S. certificates of deposit	....
Due from U. S. Treasurer	12, 212 50
Total	1, 782, 674 08

Liabilities.	
Capital stock paid in	$400, 000 00
Surplus fund	100, 000 00
Other undivided profits	33, 113 09
National bank notes outstanding	164, 250 00
State bank notes outstanding	....
Dividends unpaid	470 00
Individual deposits	954, 479 04
United States deposits	....
Deposits of U. S. disbursing officers	....
Due to other national banks	123, 628 10
Due to State banks and bankers	6, 733 85
Notes and bills re-discounted	....
Bills payable	....
Total	1, 782, 674 08

A More on history and sources

A.1 Bank balance sheet data

Figure A1 shows an example bank balance sheet. These data come from the Office of the Comptroller of the Currency's annual reports. The Office collected the individual balance sheets of every nationally chartered bank, in order to assess the overall health of the banking system and to enforce regulatory requirements. The office randomized the call dates each year, which insured that banks were likely unable to cheat on these reports (Jaremski, 2014). As argued in Jaremski and Rousseau (2018) and Carlson et al. (2022), the importance of state-chartered banks declined significantly after the passage of the National Banking Act in 1863, and remained low in the period I study.

A.2 Furnace data

For the analysis in section 5.2, I hand collected furnace-level data from a biannual directory of iron furnaces. This directory listed the size and capacity of every operational furnace in the country (there were 663 in 1874), and they recorded the year of construction as well as the years in which any renovations or new construction occurred on each furnace. This allows me to observe the total increase in furnace size due to construction and renovation (see the example in figure A2). Often, I can observe the change in annual capacity as well; when I cannot, the observed relationship between furnace size and capacity makes it possible to impute this (see figure A3 and the example in figure A2).

As explained in the text, I then use the 1880 Census of Manufactures to convert observed expansions in capacity into dollar amounts invested. I do this by examining a state-level cross-section of iron furnaces, which reports both furnaces' annual capacities and the values of their capital stock. These data show a stable, linear relationship, which I use to impute the dollar amount invested at each furnace. Those relationships are shown in figure A3, and figure A2

Figure A2: Examples from the biannual directory of iron furnaces

Lebanon Furnaces, G. Dawson Coleman, Lebanon, Lebanon county. Two stacks, 50 x 14 and 55 x 16, built in 1846 (reconstructed in 1868) and 1872–3, respectively; the new furnace was put in blast in August, 1873. A third stack, 36 x 12, built in 1847, has recently been torn down for the purpose of rebuilding, but work on it has been discontinued. Charles B. Forney, Manager.

Lebanon Furnaces, Arthur and Horace Brock, Lebanon, Lebanon county. Two stacks: one 50 x 13, built in 1846, reconstructed in 1868; the other, 65 x 17, built in 1872–3, put in blast in August, 1873; both stacks remodeled in 1883; one Whitwell fire-brick and one iron hot-blast stove; fuel, anthracite coal and coke; ore, Cornwall; product, Bessemer pig iron; a third stack, 36 x 12, built in 1847, was torn down to rebuild, but work on it has been discontinued. The combined capacity of the two furnaces in operation is about 1,000 net tons a week.

Notes: This example demonstrates the dataset construction from the furnace directory. Comparing directory entries from 1876 (top) and 1884 (bottom) enables me to observe furnace expansion. Here, Lebanon Furnaces' second stack was expanded from 55x16 to 65x17 in 1883. The stacks are measured in height x diameter, which enables computing the total furnace volume. Then, I find that annual capacity is highly predictable by furnace volume (figure A3). In this case, Lebanon Furnaces' grew their volume by 1767.146 cubic feet. This would have expanded their annual capacity by about 3,000 tons per year, which meant that this renovation involved an investment of about $45,000.

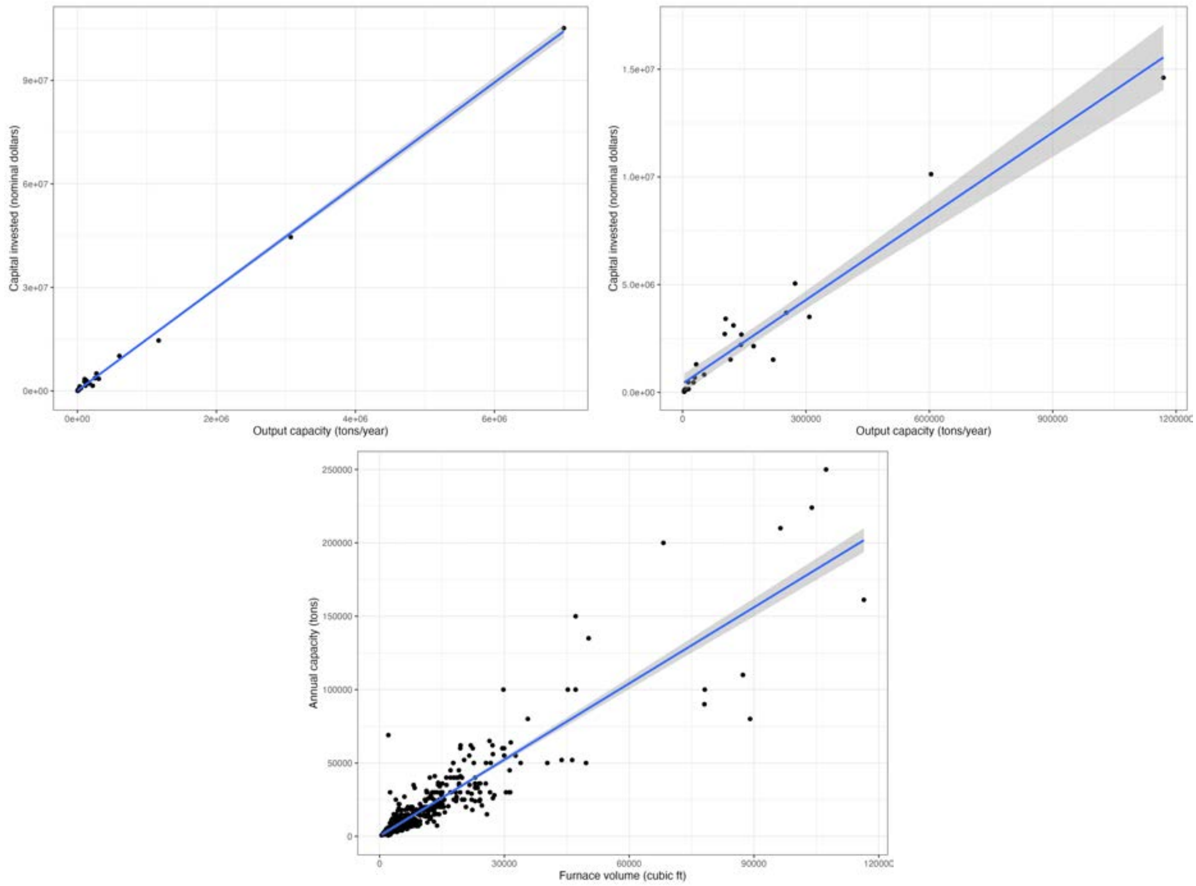
demosntrates an example imputation using these relationships.

A.3 The specie resumption debate

This appendix provides a timeline of greenback debate, in order to motivate my characterization of the Specie Resumption Act as a policy shock. After the Civil War ended, there was an initial consensus that Greenbacks should be retired and gold convertibility restored. The government began retiring greenbacks with this in mind. As Barreyre (2015) illustrates, the initially uncontroversial technical question of what to do with greenbacks quickly became a national political issue, which took on religious and moral significance. Popular pamphlets promoting resumption wrote that "the prolonged use of Paper Money [. . .] has done even more harm to the morals of the country than to its commerce." Labor leaders promoting the continued use of greenbacks wrote in newspapers that "The question is whether the government shall furnish the paper issues, which shall have all the qualities of money [...] into the hands of a small class of men [...] to engross and speculate upon the products of labor, to swindle and cheat without restraint." (Barreyre, 2015). Others spoke about unity, the trustworthiness of the federal government, and the fact that gold had been "prepared by the Almighty" to serve as currency. As Barreyre (2015) summarizes: "The money question inflamed the American political imagination because it embodied antagonistic moral visions projected onto a society undergoing rapid change following the end of slavery and the transformation of the economy."

National debate of resumption began around 1869, and continued for several years. In general,

Figure A3: Converting furnace volumes into value of capital stock



Notes: the top panel shows the relationship between furnace capacity and the value of their capital stocks. Data is from a cross-section of states in the 1880 Census of Manufactures. The right panel excludes outliers. These data suggest that it cost \$15 to expand annual capacity by one ton, and that this relationship was linear. The bottom panel shows the relationship between the volume of a furnace's stacks, and its annual capacity. It suggests similarly that annual capacity is predictable by stack volume. Combining these allows imputation of the value of a furnace's capital stock from its size.

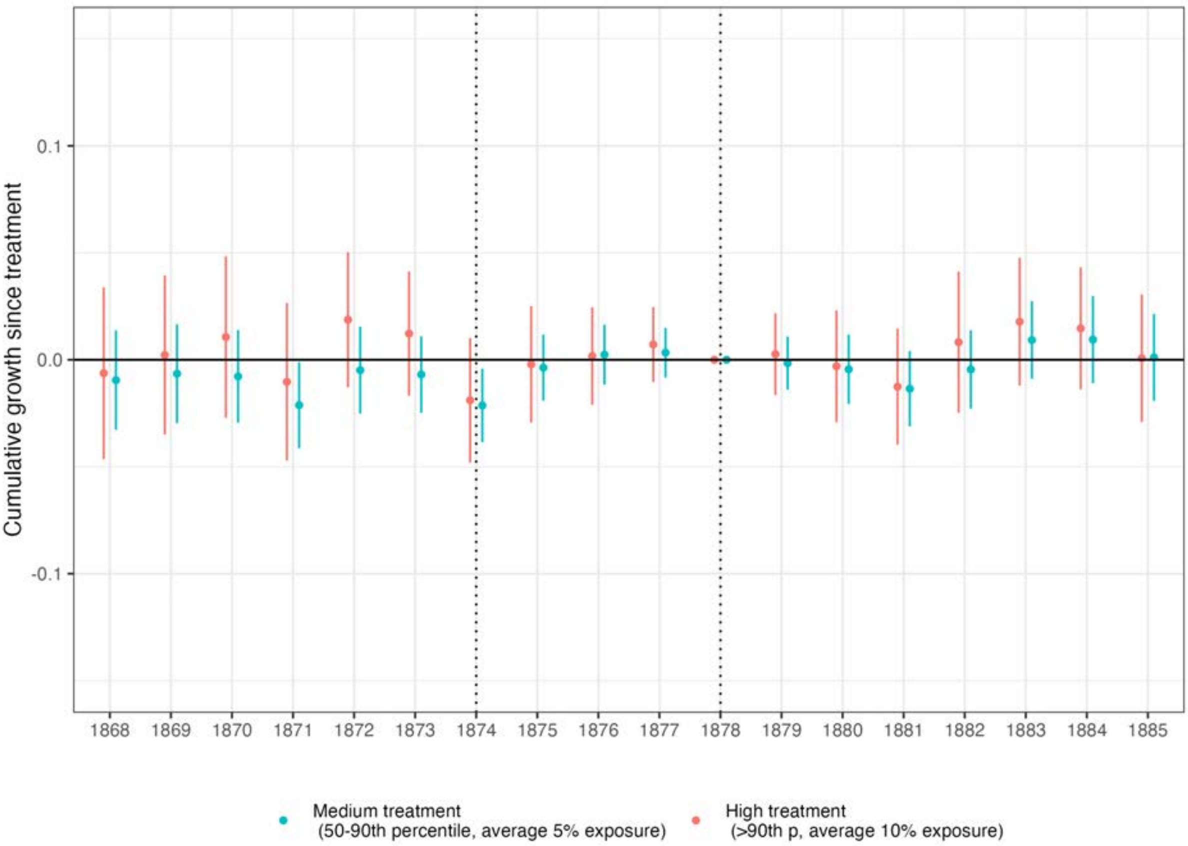
bankers, merchants, and industrialists in the Northeast supported resumption, while farmers, workers, and some industrialists in the midwest supported continued use and expansion of Greenbacks. After a financial crisis in September 1873, Congress (and especially the Republican party in power) felt an urgency to resolve the 'money question.' A couple of months after the panic, Congress passed a promise to resume convertibility in 1876. In April 1874, they repealed the promise, passing a bill to print more greenbacks instead. This bill was vetoed by President Grant. The veto surprised many, angered midwestern and southern voters, and, observers felt, ended up costing the Republican party the midterm elections in 1874. Republicans attempted to save face by passing a compromise bill in June 1874, which did slightly enable an increased money supply. Nonetheless, they lost the 1874 midterm elections. Following the loss, Republicans took advantage of the 'lame duck' session of Congress to pass the Specie Resumption Act in January of 1875 (the new congress wasn't sworn in until March of that year). When the Democratic majority convened, they considered attempting to repeal the Specie Resumption Act; pro-greenback sentiment even led to the creation of a third 'Greenback Party' that ran in the 1876 presidential election. Despite the passage of the Specie Resumption Act, uncertainty thus continued through that year – perhaps explaining the fact that Greenbacks did not begin to rise in value, and banks did not begin accumulating gold, until sometime in 1877. In 1876, the Republican Rutherford B. Hayes pulled off a surprising victory in the presidential election (which was decided by a tiebreak). Democratic opposition to resumption continued – in 1877, Democrats in the House of Representatives successfully repealed the Specie Resumption Act, but their repeal was overturned by the Senate later that year. With Hayes confirmed, his Treasury Secretary appointed, and a Republican majority in the Senate, the fate of Resumption finally began to look more certain – although as mentioned in the text, doubts about its technical feasibility remained. Ultimately, Resumption occurred as promised on January 1 1879.

What happened to bank holdings of greenbacks during this debate? Figure 10 shows the distribution of greenback holding. Greenback holdings were stable from 1870-1873, as the debate was somewhat at a stalemate. In June 1874, as part of the bill that Republicans passed to expand the money supply and pacify midwestern voters, they reduced banks' reserve requirements. This caused the jump in the distribution between 1873 and 1874. Then, in 1876, once the value of greenbacks began to appreciate and Specie Resumption appeared increasingly likely, banks began selling off their revalued greenbacks, and the distribution shifted to the left. Finally, after resumption occurred in 1879, the distribution shifted left again, as banks sold of much of their remaining greenbacks.

A.4 Loan-to-asset ratios

The text argues that treated banks increased their lending while maintaining loan-to-asset ratios that were comparable to other banks. Figure A4 estimates the effect of specie resumption on bank loan-to-asset ratios across treatment groups. It confirms that there was no significant difference between treatment groups. Increased lending at treated banks must therefore have come from an increase in their assets.

Figure A4: Difference-in-differences estimate for changes in loan-to-asset ratios



A.5 Bank functions and regulation

This subsection provides more background on the national banking system. It also describes the reserve requirement laws that I use in order to match banks on the level of requirements they faced.

National banks served two purposes after the Civil War: they collected deposits, as modern banks do, but they also each issued their own forms of currency ('national bank notes'). Banks were required to buy government bonds equal to 111% of the value of the notes they issued. In case the bank failed, the government would then seize these assets, and use them to repay anyone who held money that the bank had issued. Bank notes were thus indirectly guaranteed by the government, and they were used interchangeably with greenbacks. The difference was that bank notes were not technically legal tender, and could not be used by banks to meet their reserve requirements (explained below).

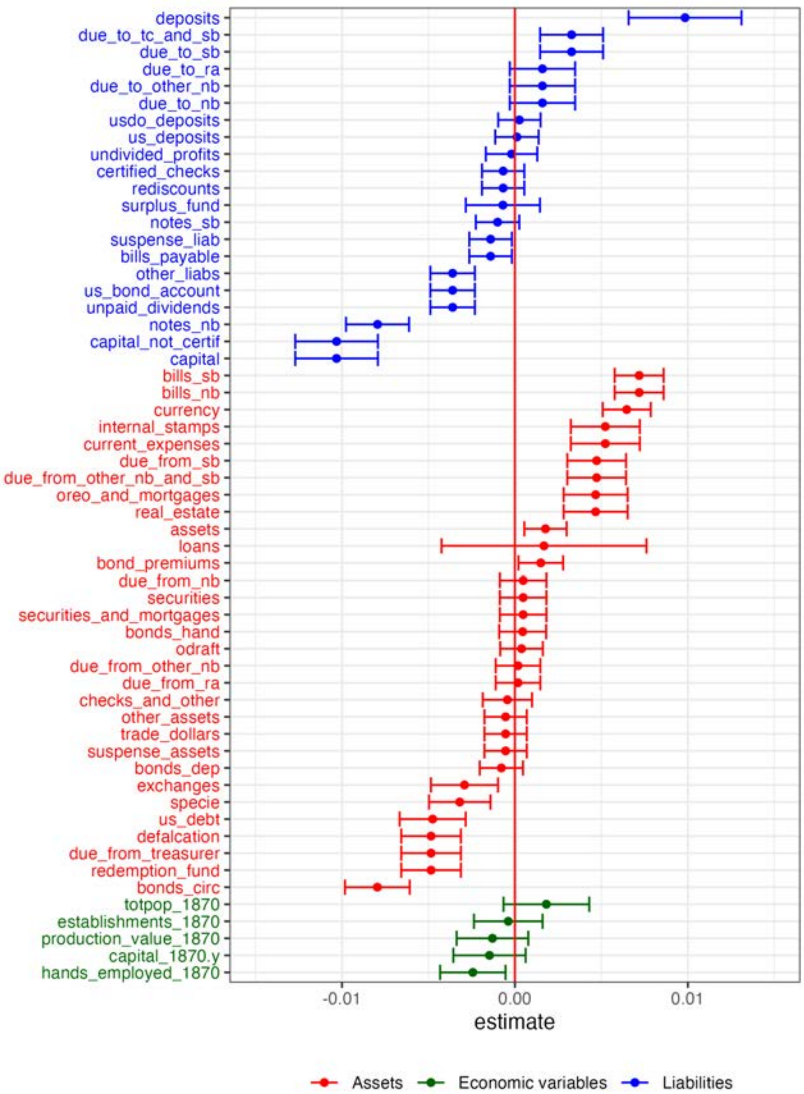
Banks faced several other restrictions alluded to in the text. National banks could not operate multiple branches. 75% of their directors were required to reside locally. They could not take real estate as collateral, and they were in addition forbidden from making any single loan that exceeded 10% of their capital. The restriction on mortgage lending meant that many state governments chartered separate institutions, on which they set minimum quotas of loans that had to go to farmers.

Regulations required banks to cover both the notes they issued and the deposits they held with reserves. Banks outside of 17 designated 'reserve cities' were required to secure 15% of these liabilities (deposits plus issued circulation) with reserves. Of their reserves, at least 40% needed to be held directly at the bank, and up to 60% could be deposited in banks in the reserve cities. Banks in the 17 reserve cities needed to hold 25% of their liabilities in reserves, and they could deposit up to half of these reserves in banks in New York City.

Banks could only use three assets to meet their reserve requirements: greenbacks, gold, or US government debt certificates that were payable in gold. Reserve requirements were thus correlated with greenback exposure. Running an unconditional balance check, using normalized balance sheet items to predict treatment, gives the coefficients in figure A5. Most of these correlations are mechanically due to reserve requirements.

These correlations are explained as followed. Deposits needed to be backed by reserves, and so are positively correlated with greenbacks. Banks face a trade-off between deposits and capital, since these are their two major liabilities. These two liabilities are thus negatively correlated, which explains the negative correlation between capital and greenback exposure. An additional regulation stipulated that a bank could only issue circulation worth up to 90% of its capital. In the data, this regulation appears to have been binding for most banks. This perfectly explains the negative correlation between circulation (`notes_nb`) and greenback exposure. Circulation was mechanically tied to the 111% in bonds that banks needed to hold in order to issue it, which thus explains the correlation of `bonds_circ` and greenbacks. Finally, since banks could choose to meet reserve requirements with greenbacks, specie, or gold-denominated US debt, the

Figure A5: Balance check: bank characteristics and greenback exposure



Notes: Greenback exposure (share of greenbacks on balance sheet) is regressed on normalized balance sheet items and a control for total assets. The plotted coefficients thus show the correlation between a one standard deviation increase in a bank's holdings of a certain asset or liability, controlling for bank size, on greenback exposure. Treatment is not balanced, but the strongest correlations are due to mechanical reserve requirements. See text.

negative correlations between greenbacks and specie or `us_debt` are also unsurprising.

This understanding of the legally-binding relationships between these balance sheet items thus motivate an additional balance check: many of the significant correlations in figure A5 do not mean that banks with different levels of greenbacks were necessarily very different. They all stem from the fact that banks with more deposits were required to hold more greenbacks. This suggests that reserve requirements should be used as a covariate in my difference-in-differences estimations. When controlling for reserve requirements (and removing the mechanically-correlated items), more- and less-treated banks appear far more similar, as shown in the text in figure 12.

On June 20th 1874, Congress changed the reserve requirements. Seeking to appease midwestern voters by expanding the money supply, without compromising on their northeastern, hard-money principles, Republicans passed a law that would slightly increase the circulation of bank notes. Banks no longer needed to protect both circulation and deposits, but only deposits. This caused some banks to reduce the amount of reserves they held (this is visible in the shift in the distribution between 1873 and 1874 in figure 10). In the current draft, I use 1874 exposure – i.e., post this reform – as treatment. This change in requirements does not seem to be a huge problem for my results: with treatment defined this way, banks appear to have been on parallel specie accumulation trends, and also on parallel lending trends; their economic conditions and other balance sheet characteristics are also somewhat balanced. In an extension however, I'd like to take this reform more seriously. It's possible that it allows me to identify banks' preferences for holding greenbacks, as I'll be able to see which banks reduced their greenback holdings when given the opportunity, and which banks remained at a certain level. Identifying bank preferences could in turn allow me to account for them, and to extract a more random component of greenback exposure. I do not explore this further in the current draft.

B Diagnostic tests for heterogeneity

B.1 Heterogeneity in TWFE specifications

The two-way-fixed-effects specifications I use in section 4.2 take the form:

$$y_{it} = \alpha_i + \alpha_t + \beta_0 G_i^{1874} + \sum_{t' \neq 1874} \tau_t \left(\mathbb{1}_{t=t'} \times G_i^{1874} \right) + \Gamma_t' \mathbf{X}_i + \varepsilon_{it}$$

The outcome variable is either the share of specie on a bank's balance sheet, or log lending. G_i^{1874} is a bank's treatment intensity (the share of greenbacks on a bank's balance sheet in 1874). de Chaisemartin and D'Haultfœuille (2023) find that in a regression of this form, the estimates τ_t are a weighted average of the ATTs of each treatment intensity, with weights equal to:

$$w_i = \frac{G_i^{1874} \left(G_i^{1874} - \overline{G^{1874}} \right)}{\sum_{i':G_{i'} \neq 0} G_{i'}^{1874} \left(G_{i'}^{1874} - \overline{G^{1874}} \right)}$$

$\overline{G} = \frac{1}{N} \sum_{i=1}^{N} G_i$ is the average treatment intensity. Computing these weights shows that 881 are positive and 1120 are negative, but the sum of the positive weights is 1.1875 while the sum of the negative weights is only -0.1875. The standard deviation of the weights is only 0.0016. A concern is that heterogeneity in the treatment effect among banks makes unreliable the interpretation of the estimates $\widehat{\tau}_t$; the fact that the negative weights are relatively unimportant makes this less concerning.

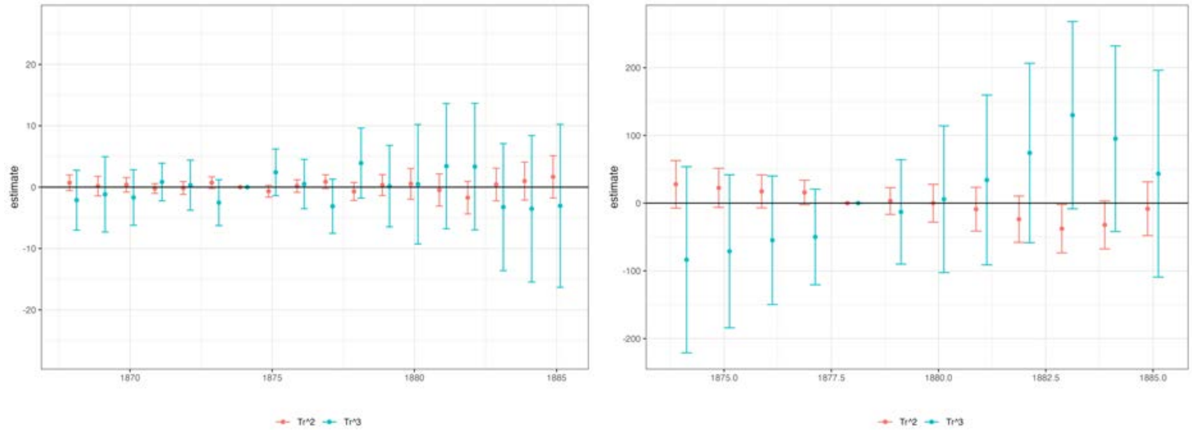
An additional diagnostic is to test for nonlinearities in treatment effect. This test regresses

$$y_{it} - y_{i,1874} = \alpha + \beta_1 D_i + \beta_2 D_i^2 + \beta_3 D_i^3 + \varepsilon_i$$

separately for each year t, where D_i is treatment exposure of bank i. If the coefficients on the higher-degree polynomial are significant, than TWFE may be biased due to nonlinearities. Figure B1 runs this test and plots the coefficients in each year, using both outcome variables examined in section 4.2. The results retain the null that treatment effect on the change in specie exposure is linear (left). They largely retain it for the effect on the change in log lending (right). In the right panel, the coefficients are insignificant except for the second-degree term in 1883. There is overall some evidence that a slight amount of nonlinearity exists after resumption: the coefficients on the second-degree terms may be lower than zero after 1878. The treatment effect as such may be slightly concave, as suggested by the multiple-groups estimations in figure 16. This would bias the TWFE estimates, and is part of why I introduce the grouped treatment. However, even in this case the amount of nonlinearity appears very small.

These diagnostics indicate that heterogeneous treatment effects are likely not a large problem in my setting. Nonetheless, in section 4.2 I adopt a discrete treatment group design to ensure that heterogeneity across groups does not affect the results. Heterogeneity may be a problem even in this design, since the control units are themselves slightly treated. I address this in appendix B.3.

Figure B1: Diagnostic test for a nonlinear treatment effect



This figure plots the higher-degree coefficients that result from regressing the change in an outcome variable from the baseline year to year t on first, second, and third powers of treatment intensity. The outcome variable in the left panel is change in specie exposure (the outcome variable analyzed in figure 13), and in the right panel change in log lending (as analyzed in figure 15). Neither case appears to exhibit strong nonlinearities.

B.2 Optimal bandwidth for treatment groups

As mentioned in the text, I do not have an econometrically principled way of defining my three treatment groups. de Chaisemartin and D'Haultfœuille (2022) derive an optimal bandwidth for a setting like mine in which only two groups – treated and control – need to be defined. This amounts to choosing a level of greenback exposure below which banks are considered untreated, and above which they are considered treated. I initially did not think that this was ideal for my setting: I have relatively few highly treated banks, which are the ones who give me the greatest power to identify a treatment effect; however, comparing these banks against all others would mean using a control group that is contaminated by banks who received medium levels of treatment. This is why, in the text, I segment the sample and compare highly treated banks (above 8% exposure) to barely-treated banks (less than 2% exposure). Nonetheless, in this section I implement the two-group optimal bandwidth in my setting as an exercise. Interestingly, the results do not seem very different from the ones I obtain in the text. In an extension of this project, I could try to derive a similar optimal bandwidth for choosing larger numbers of treatment groups, as I do in the paper, but I'm not sure how difficult this would be.

The setting in which de Chaisemartin and D'Haultfœuille (2022) derive an optimal bandwidth is the following. There are two periods (pre and post treatment), and all units receive a positive, continuously-valued treatment dosage (D_{i2})between the first and second period (greenback exposure in my setting). The problem is to choose a level of D_{i2} below which units are considered as untreated, and to choose this level to minimize mean-squared-error of the resulting estimator. The two-period design is equivalent to splitting my setting up into a variety of comparisons: each one takes 1874 as period 'one' (pre-treatment), and compares it to some year $t > 1874$ (post-treatment). In notation, I'll thus use $t = 1$ for 1874 and $t = 2$ for the chosen post-treatment period being studied.

The estimand is $\mathbb{E}\left[Y_2(D_2) - Y_2(0)\right]$. This is the average treatment effect of moving from no treatment to the actual value of treatment. It thus represents a sort of average of ATTs. The authors propose estimating this via a difference-in-differences strategy that considers banks below a certain threshold h to be untreated.

The optimal value of h that minimizes the mean-squared error of the estimator is:

$$h^* = \left[\frac{144 \cdot \sigma^2(0)}{n \cdot f''(0)^2 \cdot f_{D_2}(0)}\right]^{1/5}$$

h^* is the level of treatment dosage below which units should be considered as untreated. $\sigma^2(0) \equiv V(Y_2 - Y_1 \mid D_2 = 0)$ is the variance of the change for truly untreated units. We don't observe any truly-untreated units, but $\sigma^2(0)$ is estimable. First, regress $(Y_{i2} - Y_{i1})^2 \sim 1 + D_{i2}$ and regress $(Y_{i2} - Y_{i1}) \sim 1 + D_{i2} + D_{i2}^2$. Then, $\sigma^2(0)$ can be consistently estimated as the difference between the intercept of the first regression, and the square of the intercept of the second regression (this follows from setting D_{i2} to zero and the definition of variance).

$f(0) \equiv \mathbb{E}[Y_2 - Y_1 \mid D_2 = 0]$ is the expected change in outcome for truly-untreated units. Again, these are unobserved. To estimate $f''(0)$, regress $Y_{i2} - Y_{i1} \sim 1 + D_{i2} + D_{i2}^2$ and estimate $f''(0)$ as the coefficient on D_{i2}^2.

Finally, $f_{D_2}(\cdot)$ is the density of treatment intensity. This function can be estimated using kernel density on the observed distribution of treatment intensities, giving an estimate of $f_{D_2}(0)$.

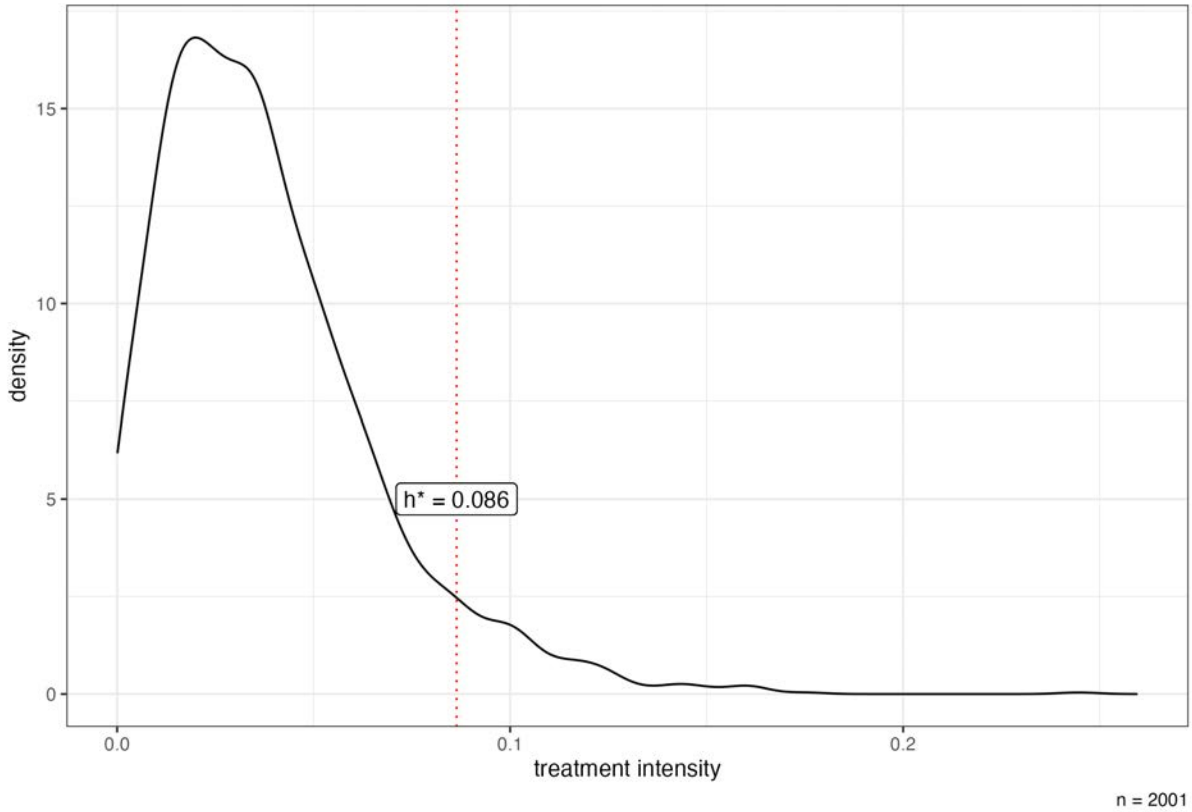
Following the above steps for my data, I estimate the optimal bandwidth in each post-treatment year. Figure B2 shows the density of treatment dosages with the optimal bandwidth calculated for the outcome year 1883. The optimal bandwidth considers most banks untreated, and only banks with above 8.6% exposure (which corresponds to the 93rd percentile of banks in my sample) as treated. This corresponds somewhat to the 'highly treated' group I use in the text.

Figure B3 calculates the optimal bandwidths separately for each post-treatment year. It plots the optimal bandwidth (y-axis) and also prints the percentiles in the treatment distribution at which the optimal bandwidth falls. For years immediately following treatment, the optimal bandwidth is to consider almost all banks (98-99% of them) in the control group. The analysis in section 4.2 shows that there is no treatment effect between when resumption was promised in 1874 and when it took effect in 1879; this is perhaps why the bandwidth takes such extreme values. Once the effect becomes more pronounced after 1879, the bandwidth begins to fall and include more banks in the treated group. Still, it remains very high, never considering more than the 7% most-treated banks as in the treatment group. However, I calculated bootstrap standard errors for the optimal bandwidths, and they are very large. The 95% confidence intervals for the optimal bandwidths tend to cover 0 through 0.3, which is the entire distribution of treatment intensities in my sample.

Then using these optimal bandwidths, I calculate the associated estimator in each year. I compute standard errors of each of these estimators via 100 bootstraped repititions for each year.[36] The resulting estimates are plotted in figure B4. They largely agree with the binned

[36]To bootstrap panel data, I resample at the bank level: each bootstrapped observation is a draw of one of the 2,001 banks in the sample; after being drawn, I observe all years that the bank is in the panel. The result is a bootstrap sample with the same number of units, but potentially a different number of units x years, than the

Figure B2: Treatment density and optimal bandwidth for outcome year 1884



estimates calculated elsewhere. The expected ATT after moving from no treatment to the actual treatments received by the banks whose dosage is above $\widehat{h}^*$ is about 0.125. Since $\widehat{h}^*$ is usually around 0.09, this is estimating something very similar to the ATT for my highly-treated group (whose treatments were all above 0.08). Indeed, the estimates of both methods agree.

B.3 Heterogeneity in treatment-group specifications

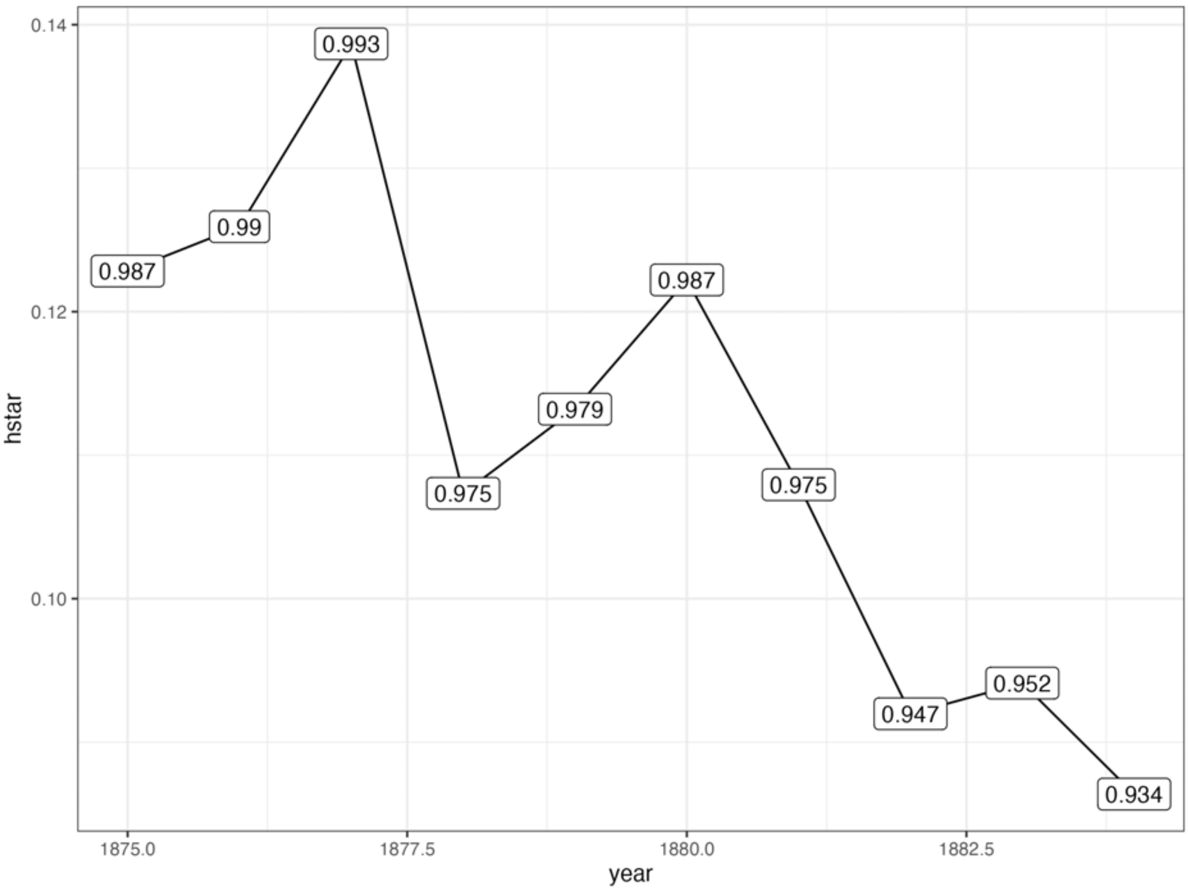
In the text and in appendix B.1, I observed that within-group heterogeneity may still be a problem, even in the grouped difference-in-differences estimator from equation (3).

To address this concern, observe that this difference-in-differences setting is equivalent to a "fuzzy difference-in-differences" estimation, in which the control group receives a treatment dose of 2%, the medium treated group receives a treatment dose of 5%, and the highly treated group receives a treatment dose of 10%. Then, I can use results about fuzzy difference-in-differences estimators from de Chaisemartin and D'HaultfŒuille (2018) to determine whether in-group treatment heterogeneity is a problem. Denote Y_{iGt} and D_{iGt} to be outcome and treatment of bank i in treatment group G at time t. Then, rescale an unconditional parallel trends estimator (the one used in 14 and in the bottom left panel of figure B5) to compute the fuzzy-DID Wald estimator:

$$\frac{E\left(Y_{Gt} - Y_{G,1874}\right) - E\left(Y_{Ct} - Y_{C,1874}\right)}{E(D_{Gt} - D_{G,1874}) - E(D_{Ct} - D_{C,1874})}$$

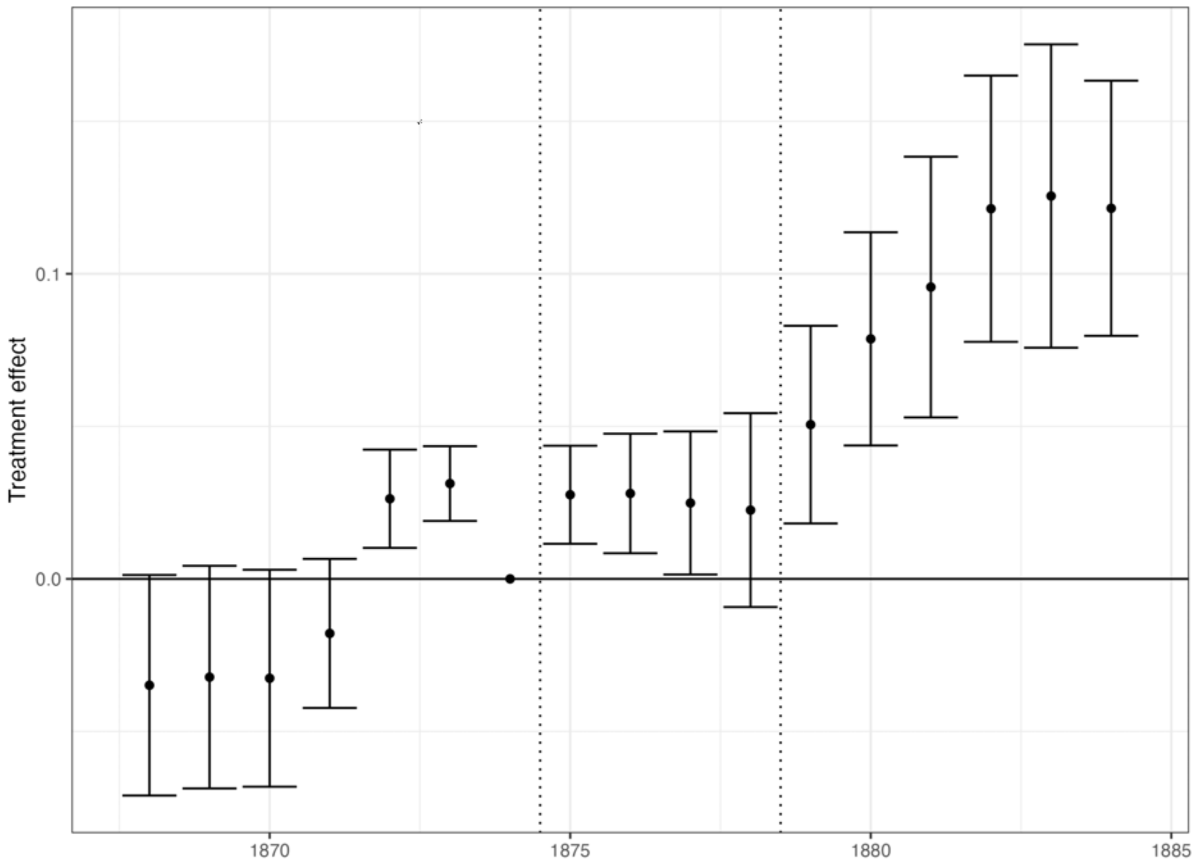
original.

Figure B3: Optimal bandwidths for each post-treatment year



Optimal bandwidths are computed separately for each post-treatment year, using 1874 as the year of treatment. The printed numbers are the percentile in the treatment distribution at which the optimal bandwidth falls. Bootstrap standard errors for the optimal bandwidth are very large, however – the 95% confidence intervals of the optimal bandwidth usually cover 0 through 0.3, which is the entire distribution of treatment intensity.

Figure B4: Difference-in-differences according to optimal bandwidths



Notes: Estimates for the average ATT according to the optimal bandwidth procedure described in the text. This estimator assumes an unconditional parallel trends assumption. Standard errors are calculated via 100 bootstrap repititions. The magnitude of the estimate largely agrees with the ATT estimated in the binned specification I use in the text.

Everyone is untreated in 1874, so $D_{G,1874} = 0$. Groups' expected treatments at time t for $t > 1874$ is equal to their average greenback exposure in 1874 (0.1 for $G = H$, 0.05 for $G = M$, and 0.02 for $G = C$). This divides the estimates in figure 14 for highly treated banks by .08, and for medium-treated banks by .03 (the difference between each group's average treatment intensity and the control group's average treatment intensity). Then, de Chaisemartin and D'HaultfŒuille (2018) show that this estimator is equivalent to a weighted average of four treatment effects, two of which are zero in this setting. Of the remaining two, one is positive and one is negative. The positive weight is $\frac{E(D_{Gt})}{E(D_{Gt})-E(D_{Lt})}$ and the negative weight is $\frac{E(D_{Ct})}{E(D_{Gt})-E(D_{Ct})}$.

For highly treated banks ($G = H$), the estimated slope of the treatment effect on specie accumulation (figure 14) is around $0.04/0.08 = 0.375$ between 1880 and 1884. For the effect on loan growth, it is around $0.15/0.08 = 1.875$. The positive weight is equal to $0.1/0.08 = 1.25$, and the negative weight is equal to $-0.02/0.08 = -0.25$.

For medium-treated banks ($G = M$), the estimated slope of the treatment effect on specie accumulation is around $0.015/0.03 = 0.5$ between 1880 and 1884. For the effect on loan growth, it is around $0.075/0.03 = 2.5$. The positive weight is equal to $0.05/0.03 = 0.017$ and the negative weight is equal to $-0.02/0.03 = -0.007$.

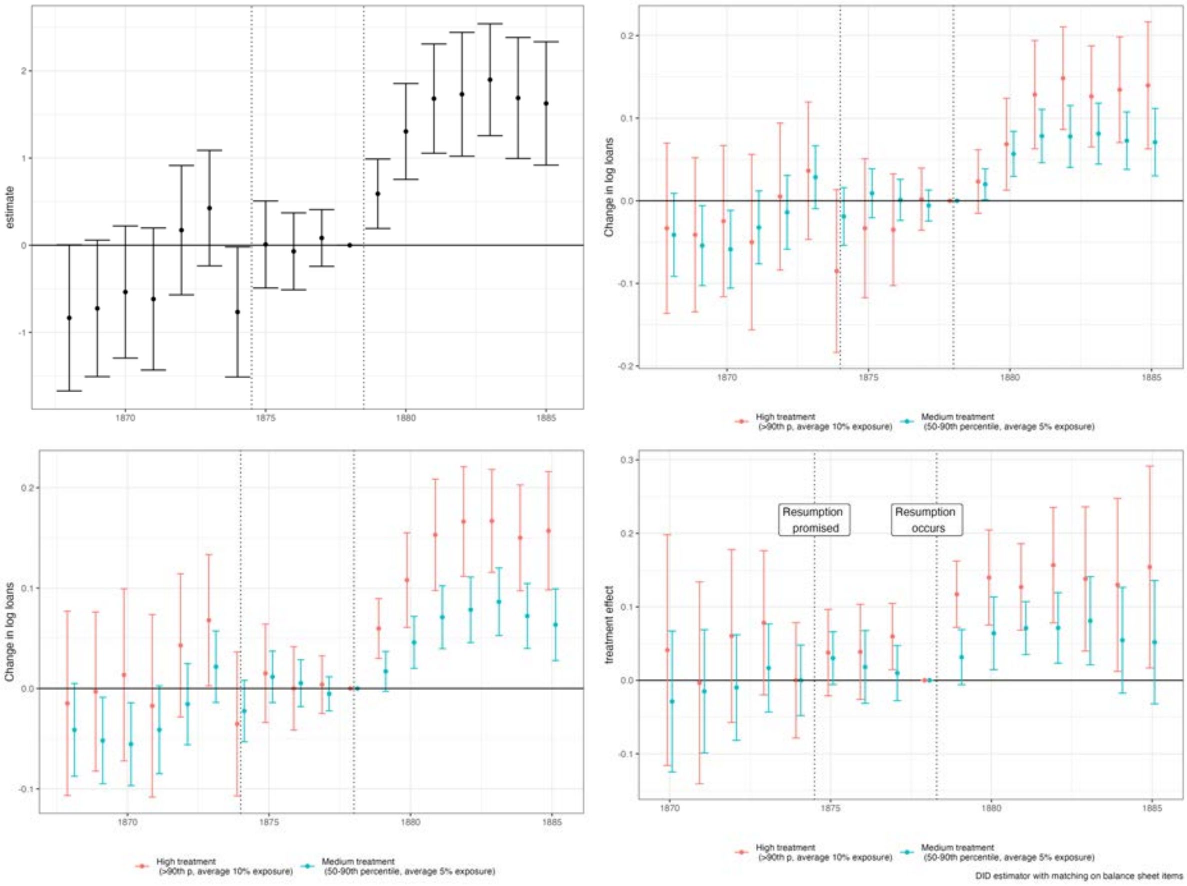
In all cases, the negative weight is lower than the estimate and much smaller than the positive weight, and so it seems unlikely that within-group heterogeneity is severely distorting the results in figures 14 or 16.

B.4 Additional first-stage robustness checks

Figure B5 tests for the effect of specie resumption on bank lending using a variety of controls. Motivated by the balance check in figure 12, the estimates in the text considered banks of the same size and with the same level of reserve requirements to be comparable. It computed different-in-differences estimators that made conditional parallel trends assumptions for banks of the same sizes and with the same reserve requirements. Notably, figure 12 showed that these banks were located in counties with similar economic conditions. To ensure robustness, figure B5 includes an additional set of covariates in the conditional parallel trends assumption. The estimates in the top panel include variables for bank size and reserve requirements in 1874, the 1870 population of a bank's city, and the value of manufacturing output, labor, and capital, as well as the number of manufacturing establishments in a bank's county in 1870. The bottom left panel makes an unconditional parallel trends assumption and includes no covariates in the propensity-weighting. Finally, the bottom right panel uses propensity-score matching rather than IPW weighting to implement conditional parallel trends. This implements the matching difference-in-differences estimator proposed in Smith and Todd (2005), and calculates bootstrapped standard errors.

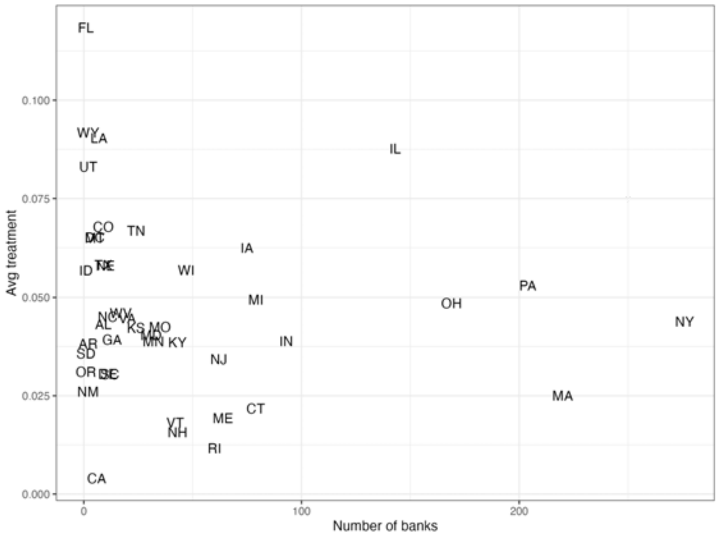
Figure B7 zooms in on states with highest densities of banks (a swath of states including, from East to West, New Hampshire, Vermont, Massachussets, Connecticut, New York, New Jersey, Pennsylvania, Ohio, Indiana, and Illinois). It shows that highly-treated banks are not clustered in any obvious way, but that banks in the Northeastern-most states almost all held very few

Figure B5: Effect of greenback exposure on lending: additional robustness checks



Notes: this figure explores the robustness of results using a variety of different estimators. The top two panels recalculate the TWFE and doubly-robust difference-in-differences estimators from section 4.2, including additional controls. These controls are: bank size and reserve requirements in 1874, the 1870 population of a bank's city, and pre-treatment economic conditions in a bank's county (total value of output, capital, and labor, and number of manufacturing establishments in 1870). The bottom left panel implements an unconditional parallel trends assumption. The bottom right panel uses propensity-score matching rather than the inverse-propensity-weighting used elsewhere.

Figure B6: State-level greenback exposure



This compares exposure to greenbacks in states by the number of banks. There is no obvious relationship, and states with more developed banking sectors do not appear to necessarily have held more greenbacks. Visual inspection does not reveal any obvious geographic clustering either (although the map in figure B7 shows that banks in the northeastern-most states did tend to hold fewer greenbacks than banks elsewhere).

greenbacks. This motivates attempting to control for geography. Figure B8 thus adds state fixed effects (left) and a control for longitude (right) to the specification in figure B5. It also tries dropping banks in New England from the sample, to ensure that the control group is not disproportionately made up of banks in these states.

C Second stage robustness checks

C.1 First-stage analysis at the regional level

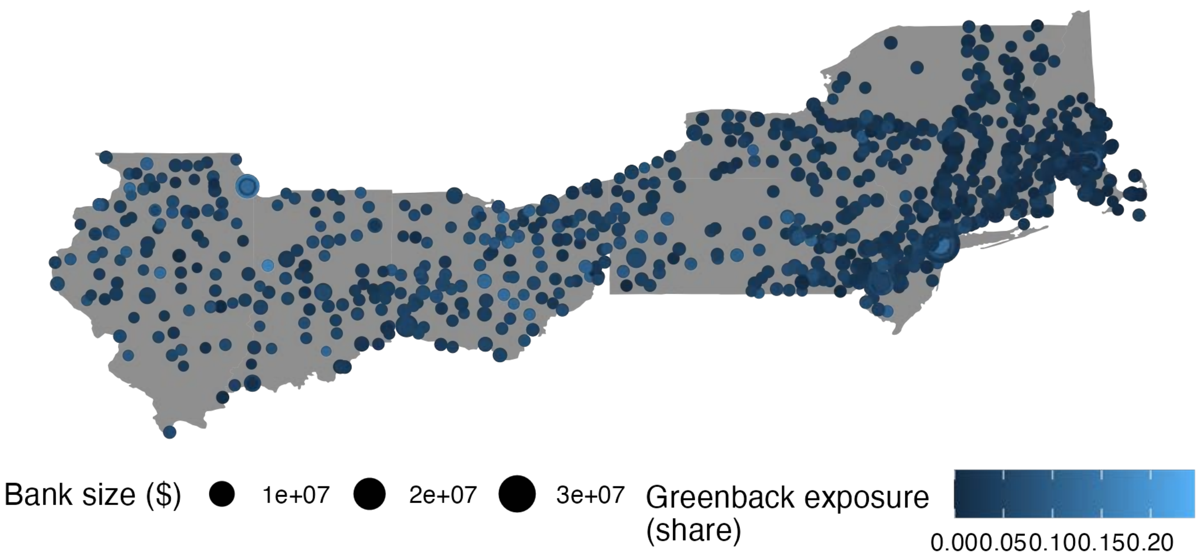
To conduct the county-level analyses in sections 5.2 and 6.1, it is important that the first-stage relationship between greenback exposure and lending holds at the county level as well. Aggregating bank balance sheets by county dilutes variation and reduces power, but the first stage relationship is still sizable and significant.

Figure 18 in the text shows that differently-treated counties were economically similar prior to specie resumption. Figure C1 and C2 repeat the first-stage analysis of section 4 at the county level, confirming that the relationship between greenback exposure and a post-resumption lending boom holds at the county level as well.

C.2 Alternate definitions of 'region'

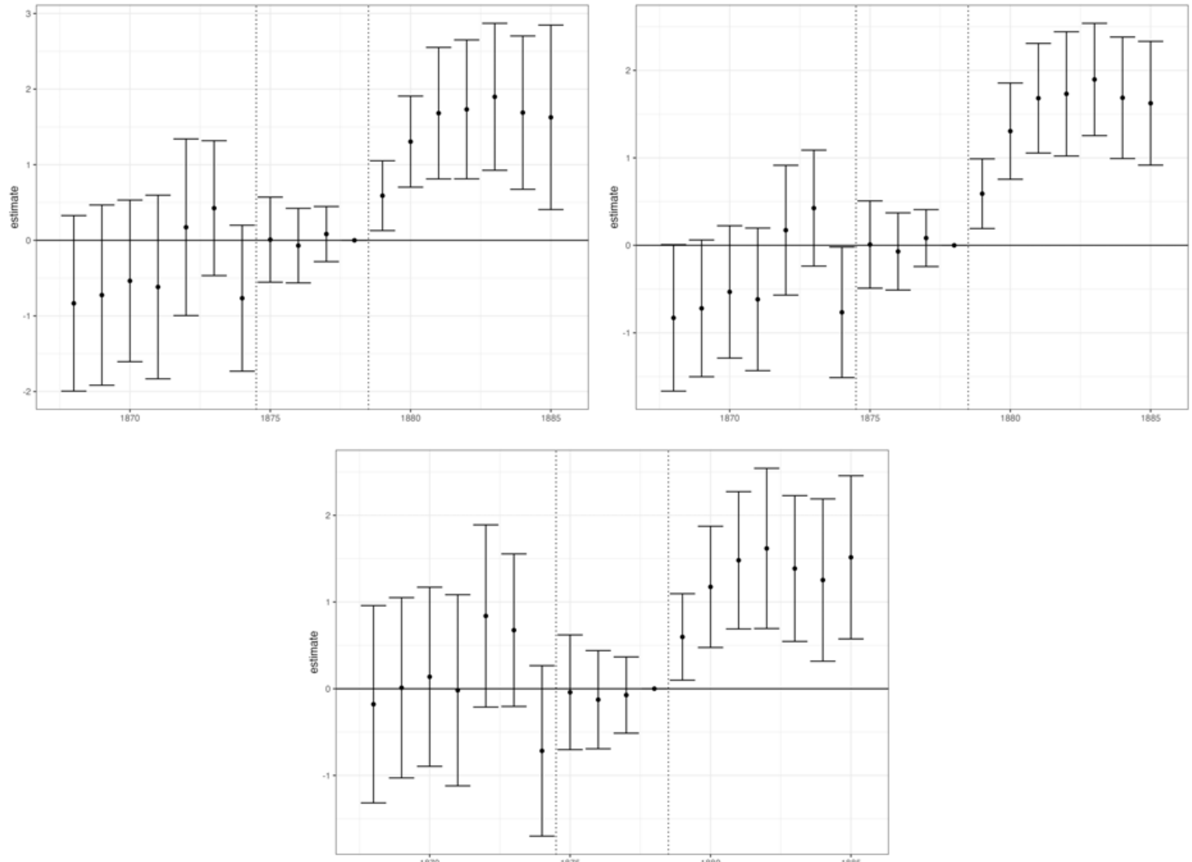
In section 5.2, I considered banks in a 50-mile radius around each furnace to be those from whom the furnace could secure loans. Tables C1 through C3 show that the results in section 5.2 are robust to considering alternative radii: restricting to only banks within 25 miles, or expanding to include banks 75- or 100-miles away gives similar results.

Figure B7: Banks in 1874 (detailed view)



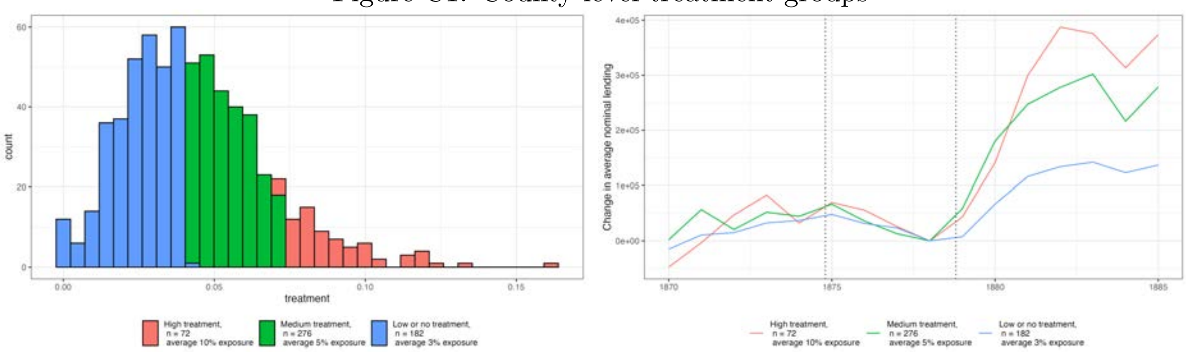
Detail of the states with the highest densities of banks. Greenback holdings are not obviously clustered, though they do appear to increase as one moves further west. This motivates including geographic controls in the robustness check in figure B8.

Figure B8: Geographic robustness checks



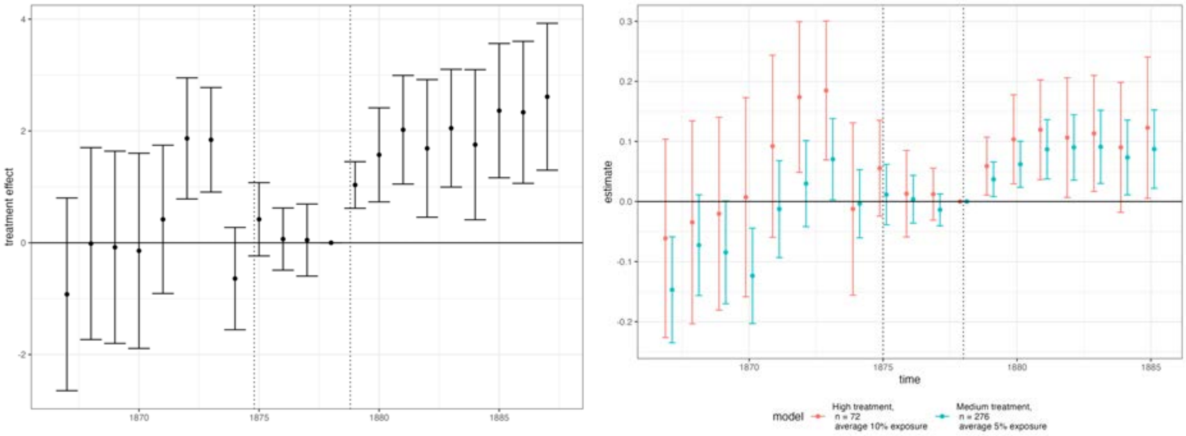
This figure adds state fixed-effects (left) and a control for longitude (right) to the specification in figure B5. This checks that the fact that banks further west tended to hold more greenbacks does not confound the results. The bottom panel excludes banks in states that systematically held very few greenbacks (New Hampshire, Vermont, Massachusetts, Connecticut, Rhode Island, and New York) to check whether results are distorted by these states' overrepresentation in the control group.

Figure C1: County-level treatment groups



The left panel shows the distribution of treatment exposure at the county level, and defines three treatment groups; the right panel compares lending trends at the county level. Both suggest that the first-stage relationship found in section 4 holds at the county level as well; figure C2 confirms this with formal estimation.

Figure C2: County-level first stage analysis



This figure repeats figures 15 and 16 at the county level, confirming that the first-stage relationship found in section 4 holds at the county level.

Table C1: Greenback IV: effect of regional loans on factory investment

	Dependent variable:			
	ΔInvestment (pct.)		ΔInvestment ($1,000)	
	(1)	(2)	(3)	(4)
Δ log(Loans) within 25 miles	2.531**	2.504**	1.792**	1.797**
	(1.059)	(1.050)	(0.791)	(0.794)
Furnace size in 1876		−0.006**		0.001
		(0.003)		(0.002)
Weak Instruments	0.0014***	0.0015***	0.0014***	0.0015***
Observations	408	408	408	408
R^2	−0.499	−0.464	−0.498	−0.500
Note:				*p<0.1; **p<0.05; ***p<0.01

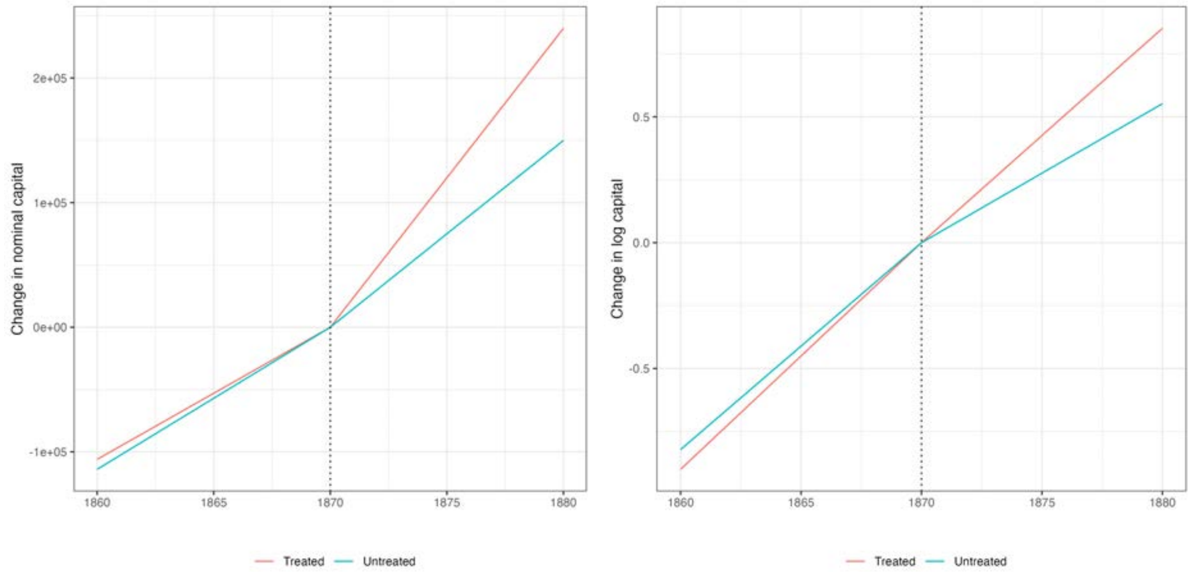
Table C2: Greenback IV: effect of regional loans on factory investment

	Dependent variable:			
	ΔInvestment (pct.)		ΔInvestment ($1,000)	
	(1)	(2)	(3)	(4)
Δ log(Loans) within 75 miles	1.515**	1.698**	1.157**	1.202**
	(0.764)	(0.778)	(0.550)	(0.563)
Furnace size in 1876		−0.008***		0.00001
		(0.002)		(0.002)
Weak Instruments	0***	0***	0***	0***
Observations	482	481	482	481
R^2	−0.087	−0.087	−0.129	−0.142
Note:				*p<0.1; **p<0.05; ***p<0.01

Table C3: Greenback IV: effect of regional loans on factory investment

	Dependent variable:			
	ΔInvestment (pct.)		ΔInvestment ($1,000)	
	(1)	(2)	(3)	(4)
Δ log(Loans) within 100 miles	2.656***	2.744***	1.668***	1.696***
	(0.854)	(0.852)	(0.590)	(0.597)
Furnace size in 1876		−0.010***		−0.001
		(0.002)		(0.002)
Weak Instruments	0***	0***	0***	0***
Observations	490	489	490	489
R^2	−0.258	−0.230	−0.214	−0.222
Note:				*p<0.1; **p<0.05; ***p<0.01

Figure C3: Trends in capital accumulation at sectors in treated and untreated counties



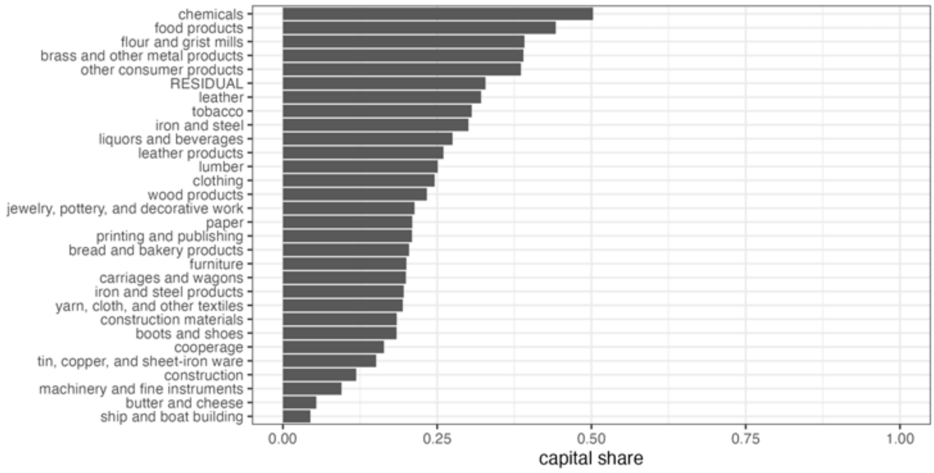
This figure compares average capital at industries in counties with above-median greenback exposure to those in counties with below-median greenback exposure in 1874. Counties appear to have been on parallel trends from 1860 to 1870, and diverged thereafter. The left panel measures changes in nominal capital, the right panel measures changes in log capital.

D Appendix to section 6

Figure D1 shows the estimates of α_s obtained for each sector. The estimates are usually between .2 and .5, which is consistent with the modern literature; the heterogeneity is also somewhat intuitive, wich industries we think of as more capital intensive (metalworking) receiving a higher share than others (shoemaking or butter and cheesemaking).

In the text, I note that Sraer and Thesmar (2023) observes that MRPK should be log-normally distributed. To validate the calibration of the model I make in the text, figure D2 shows the empirical distribution of log-MRPKs in the data. It is normally distributed as desired.

Figure D1: Estimates of α_s



Notes: this figure shows the estimates of the capital shares α_s for each sector. Estimates are usually between .2 and .5, which accords with the modern literature. The heterogeneity is somewhat intuitive – more capital-intensive industries like metalworking tend to have larger α_s than crafts like shoemaking or butter and cheesemaking.

Figure D2: Estimated county-industry MRPKs from the 1870 census of manufactures