Bank Entry Regulation and Financial Development: The US Free Banking Experience*

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Abstract

We examine how the relaxation of barriers to bank entry affects financial development by exploiting the introduction of free banking laws in US states during 1830-1860, a period when regulatory barriers to bank exit were more limited than today. Free banking laws led to an expansion of the banking sector. Further evidence on the exit probability and growth of incumbent banks, as well as the higher growth of industries for which external finance is presumably more relevant, reveal improvements in allocative efficiency. Our findings highlight the importance of both entry and exit barriers in the process of selection among banks.

Keywords: Bank Deregulation, Bank Competition, Economic Growth, Financial Development, Dynamic Efficiency, Free Banking.

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I. Introduction

The optimal level of competition among banks is a long-standing question. According to conventional wisdom, competition drives inefficient banks out of the market and allows efficient ones to expand. This virtuous selection effect leads to an overall improvement in the ability to allocate savings in the economy (e.g., Jayaratne and Strahan, 1996). On the other hand, regulators are concerned about bank competition because of its effects on the risk of failure. As firms might not be able to easily switch to another lender, the regulators' view implies that competition should be controlled in order to prevent banks from exiting the market (e.g., Vives, 2010). Since these two views have opposite predictions, the effect of competition on the size and efficiency of the banking system is ultimately an empirical question.

We shed new light on this question by focusing on the introduction of free banking laws in US states between 1837 and 1863. This is an excellent setup for three reasons. First, the introduction of free banking laws, which "*reduced entry costs*" according to Mitchener and Jaremski (2015), provides us with state-by-year variation in the relaxation of barriers to bank entry.[1] Even though their specific form differed across states,[2] all free banking laws established requirements applicable to any citizen who wished to open a bank, eliminating the power of governments to grant bank charters. Second, the bank regulatory framework in 1837-63 US allows us to compare the reaction of incumbents to entry under two bank chartering regimes, a liberal and a restrictive one. Third, barriers to bank exit in US states during 1837-1863 were much more limited than today. The riskiness of banks was not the main concern of US state authorities, who had little power to prevent defaults (Mitchener and Jaremski, 2015).[3] Hence, the 1837-63 US provides us with a unique setup to study the effects of measures to reduce bank entry barriers, the creation of new banks and their interaction in the absence of barriers to bank exit.

We examine the effects of free banking laws on the size and efficiency of the banking system by conducting three types of test. The first two tests exploit the state-by-year variation in the introduction of free banking laws using Weber's (2006, 2011a) datasets on antebellum banks. The first test focuses on the size of the banking system at the state level, as measured by the number of banks per capita, while the

[1] Historical accounts document that a wave of bank entries followed the introduction of free banking laws (Hammond, 1957).

[2] Free banking laws differed mainly in terms of minimum level of paid-in capital, the ratio of nominal bond value to banknote circulation, and eligible bonds (e.g., Rockoff, 1972; Hasan, 1987). See Section II for a detailed description of the US banking regulation during the antebellum period.

[3] Banks could mutually insure themselves by joining cooperative arrangements, but they would be expelled, rather than rescued, if they posed a threat to the stability of their peers (Calomiris and Kahn, 1996). See Section III.B for a discussion of the implications of private cooperative arrangements during the free banking era.

second test uses individual-level data on incumbent banks to examine allocative efficiency. As incumbent banks existed both before and after the introduction of free banking laws, we can investigate whether their reaction to the regulatory change, as well as to the entry of new banks, is consistent with a virtuous selection process. The third test combines industry data from the US manufacturing census (Atack et al., 2004) with information on whether states had a free banking law in a given year, rather than exploiting the timing of their introduction. In the spirit of Rajan and Zingales (1998), we then study how free banking laws affected growth across industries that differ in terms of characteristics related to the need for external finance. The rationale for this test is that the benefits of a virtuous selection effect or the costs in terms of higher agency problems are greater for industries that rely more on external finance.

Our results reveal a positive link between the relaxation of barriers to bank entry and the size and efficiency of the banking system. First, the introduction of free banking laws led to an increase in the number of banks per capita. Second, free banking reduced incumbents' probability of exit and led to a decrease of the growth rate of incumbents that exited the market. Under the reasonable assumption that those incumbents who survived the whole sample period were the most efficient, our evidence suggests that the introduction of free banking laws led to improvements of allocative efficiency. Third, free banking laws increased the growth rate of incumbents, both after the entry of new banks and in cities where no new banks were created. This novel aspect is consistent with a reaction of incumbents to both actual and potential competition. Finally, while we do not find, on average, a robust positive effect of free banking laws on the growth rate of industries that depend more on external finance, our results point to a growth-enhancing effect for these industries in states with more banks per capita. This result resonates with recent literature documenting a non linear relationship between financial development and growth (e.g. Rioja and Valev, 2004; Aghion et al., 2005).

Our evidence is based on two types of specifications. The first two tests are based on a difference-in-differences approach. Hence, we obtain our estimates as the difference between the averages in states with and without free banking laws, net of the same difference before the introduction of these laws. This approach allows us to rule out any fixed characteristics and time variation that could be correlated to free banking. The analysis in the spirit of Rajan and Zingales (1998), by contrast, abstracts from the timing of free banking laws and considers them as state characteristics. In this setup, our estimates represent the difference between the growth rate of sectors in states with and without free banking laws, as a function of the dependence on external finance. Since identification is based on sector-by-state variation, we are able

to control for any state and sector-specific time-invariant characteristics that are potentially correlated with free banking laws.

One concern with our results, especially as regards the size of the banking system, is that free banking laws may be driven by omitted variables. For example, why did New York introduce a free banking law in 1838, while Pennsylvania did not until 1860? Potentially, growth opportunities were more promising in New York than in Pennsylvania, and this led entrepreneurs in New York to lobby for a free banking law. Since our findings are based on a specification that does not control for any other state-by-year variation than the introduction of free banking laws, our estimated positive effect on the size of the banking system could be upward-biased, as it potentially reflects greater growth opportunities. By contrast, such endogeneity concerns are less severe at the individual level and when using the approach à la Rajan and Zingales (1998). As for the former, incumbent banks would presumably have incentives to preserve the status quo rather than facilitating the entry of new banks. Moreover, if free banking laws were correlated to unobservable growth opportunities, incumbent banks should exhibit a higher growth rate after their enactment, which we do not find on average. As for the approach à la Rajan and Zingales (1998), we are able to control for any state characteristics, including growth opportunities potentially correlated to both free banking laws and the growth rate of value added.

To further improve the identification of the state-level analysis, we employ the variation in the introduction of free banking laws across state borders to implement a "*regression discontinuity*" design.[4] The rationale behind this method is that the potential gap in average growth opportunities that might have led New York to introduce a free banking law 22 years before Pennsylvania is likely to be much smaller in two contiguous counties separated only by the border between these two states. Restricting the sample to state border counties, and controlling for any county pair-by-time variation, we are able to mitigate the concern that unobservable economic conditions correlated with the introduction of free banking could bias our estimates.

Another concern with our study is that our estimates could capture the effect of other state laws that were introduced at the same time as free banking laws. For example, during the 19^{th} century many US states used usury laws to impose a ceiling on the interest rate a bank could charge to a borrower. If the states

[4]Huang (2008) applies a similar strategy to investigate the local economic effects of relaxing bank-branching restrictions in the United States between 1975 and 1990. Danisewicz et al. (2013) use a similar regression discontinuity design to examine the effects of regulatory enforcement actions on local economic growth.

that introduced free banking laws relaxed usury laws at the same time, our results might be biased because a higher ceiling allows banks to serve a wider pool of borrowers, facilitating an increase in the size of the banking system. To rule out this type of concern, we collected a set of time varying state laws and added them as further controls to our specifications. The set of state law controls contains usury laws, incorporation laws, limited liability laws, state-specific liability insurance systems, and clearing arrangements.

Our paper relates to the literature on the finance-growth nexus and the financial history of the United States. A number of papers show that financial development stimulates economic growth (e.g., King and Levine, 1993; Rajan and Zingales, 1998; Levine, 2005). Within this literature, several papers exploit the removal of branching restrictions in US states during the 1980s. These papers document positive effects on economic growth (Jayaratne and Strahan, 1996; Huang, 2008), which are driven by improvements in the allocative efficiency of savings rather than an increase in the aggregate quantity of credit (Jayaratne and Strahan, 1996; Rice and Strahan, 2010). In particular, existing studies document a faster growth of the most efficient banks ex-ante (Jayaratne and Strahan, 1998), improvements in the productive efficiency of banks (DeYoung et al., 1998; Evanoff and Ors, 2002), and a growth in the entry of small businesses (Black and Strahan, 2002; Cetorelli and Strahan, 2006; Kerr and Nanda, 2009).[5] Our findings, based on a period when the absence of implicit state guarantees presumably reinforced the process of natural selection, confirm the positive link between bank competition and allocative efficiency. In contrast to studies of the relaxation of US intra-state branching restrictions in the 1980s, we document a positive effect on the size of the banking system and provide evidence of how bank entry barriers affect incumbents' reaction to potential competition.[6]

Our results provide new insights into the US free banking literature. Economic historians argue that free banking laws facilitated bank entry (e.g., Hammond, 1957), but the empirical literature provides conflicting evidence. Economopoulos and O'Neill (1995) and Bodenhorn (2008) document that more banks entered in free banking states, whereas Ng (1988) and Bodenhorn (1993) do not find any significant effect. At the same time, economic historians have investigated the instability of banks during the free banking period.

[5] Studies focusing on other countries or periods also find positive effects of competition on allocative efficiency. For example, Bertrand et al. (2007) document beneficial effects of bank deregulation on the industry structure in France, while Carlson and Mitchener (2009) show that bank branching increased competition and forced weak banks out of the market in California during the 1920s and 1930s.

[6] Using data on bank entry in the US over the 1994-2008 and 1977-1988 periods, Adams and Amel (2016) and Amel and Liang (1997) provide evidence supporting the potential competition hypothesis, but do not distinguish entry based on the regulatory regime.

There are various explanations, including shocks to the value of banks' bond holdings (Rolnick and Weber, 1984, 1985; Jaremski, 2010), lack of diversification (Economopoulos, 1990; Jaremski, 2010), and contagious bank runs (Hasan and Dwyer, 1994; Dwyer, 1996). Our contribution to this literature is twofold. First, we are interested in the net effect of free banking laws, that is whether there is an overall increase in the size of the banking system. Second, we examine how incumbents, which represented a relatively large size of the banking system, reacted to the introduction of free banking laws. This incumbent level analysis is an important contribution to the existing literature. The conventional view implicitly regards free banking laws as effective only if (free) banks were chartered under the new regime. This interpretation, however, lacks one important aspect: In order to prevent entry under a free banking system, incumbent banks might behave more competitively. Hence, the allocative efficiency of the banking sector might improve even if no free banks entered the market. Our empirical analysis supports this interpretation. Free banking laws increased, on average, the size of the banking sector and improved allocative efficiency.

There is a large amount of literature on the finance-growth nexus in US history (e.g., Bodenhorn, 2000; Rousseau, 2003; Rousseau and Sylla, 2005; Mitchener and Wheelock, 2013; Atack et al., 2014), but no study so far has investigated the role of free banking laws. Closely related is Jaremski and Rousseau (2012), who find that state-chartered banks enhanced economic growth at the county level, whereas free banks did not. Our contribution is to provide new evidence on the finance-growth nexus during the free banking period, using industry data from the manufacturing census. Compared to Jaremski and Rousseau (2012) our focus is not on the direct effects of banking activity but on whether free banking laws facilitated the growth of industries that depend more on external finance. In this respect, our empirical framework follows Mitchener and Wheelock (2013), who employ a similar identification in the spirit of Rajan and Zingales (1998) to study the effect of banking concentration on manufacturing sector growth across US states during the 1899-1929 period.

The rest of this paper is structured as follows. Section II provides an overview of the free banking era tailored to the purpose of our study. Section III describes our data. We present our main empirical results in Sections IV, V, and VI. Section VII presents our conclusions.

II. The Free Banking Era (1837-1863)

Our study investigates the effect of removing barriers to bank entry on the size and efficiency of the banking system. In this section, we consider three aspects of the free banking period that are particularly relevant for our study: bank chartering policies, bank regulation, and the role of banks and credit.

A. Bank Chartering Policies in the United States between 1837 and 1863

Bank chartering was one of the main elements of bank regulation in the United States between 1837 and 1863.[7] Crucially for the purpose of our study, bank chartering policies were decided at the state level and underwent reforms over the course of this period. Before 1837, bank chartering was the means through which states exerted their control over banks.[8] In order to open up a bank, the aspiring banker had to apply for a bank charter. The state government decided whether to grant the charter and, in case it did so, it set the requirements that the bank had to satisfy. Those requirements differed from bank to bank but generally consisted of an initial capital stock and constraints on the allocation of funds.[9] It was usually difficult to obtain a bank charter because states wanted to limit the number of banks in order to protect the interests of incumbent banks (e.g., Bodenhorn, 2006, 2008).

What leads us to consider the introduction of free banking laws as a relaxation of barriers to bank entry is the fact that governments gave up their discretionary power to grant bank charters. Starting from Michigan in 1837 and followed by New York and Georgia in 1838, eighteen US states introduced free banking laws (see Table 1). Free banking laws allowed any individual to open up a bank, subject to a set of legal requirements. Banks chartered under free banking laws were typically obliged to pay in a minimum amount of capital and to back their banknote circulation with government bonds or mortgages (e.g., Rockoff, 1972; Hasan, 1987). The decision to open a bank became an administrative rather than a political process (Bodenhorn, 2006), which presumably relaxed the barriers to bank entry. This could have fostered competition in two ways. First, any individual could open up a bank in profitable markets where the incumbent enjoyed monopoly

[7] See Bodenhorn (2002), Hammond (1957), and Schweikart (1987) for a description of banking in the antebellum US, for example.

[8] Few states had general banking laws. Banking laws usually defined managers' and shareholders' liability and tied banknote circulation to bank capital or specie. No state allowed individuals to open a bank that could issue banknotes without a charter. See, for example, Dewey (1910), Knox (1903), and Hendrickson (2011) for further information on banking regulation in the United States in the 19th century.

[9] Some charters required banks to lend to companies involved in the construction of railroads or canals, or to invest in state bonds (e.g., Knox, 1903).

rents. Second, the threat of new entrants could induce incumbent (state chartered) banks to act in a more competitive manner. Hence, the introduction of free banking laws could increase competition regardless of the number of new banks.

One concern with our study is that the introduction of free banking laws might not have been a random event. There are three arguments suggesting growth opportunities as the main source of endogeneity. First, Bodenhorn (2006) argues that the impulse for reform in New York arose from dissatisfaction with the existing banking system. This system was perceived as corrupt and inadequate to satisfy the increased demand for credit. Even though Bodenhorn notes a random component in the introduction of free banking laws,[10] his analysis suggests that the relaxation of the barriers to bank entry could be driven by a higher demand for credit. Second, Atack et al. (2014) reveal the tendency of banks to open in counties where new railroads connecting the US Midwest states were built during the 1837-63 period. Third, states might introduce free banking laws to facilitate the financing of infrastructures, such as railroads, through the creation of banks. Overall, these arguments raise the concern that the greater size of the banking system is not the effect of free banking laws, but rather a consequence of growing business opportunities. We will address this threat to identification in the empirical analysis.

B. Bank Regulation in the United States between 1837 and 1863

During the free banking period, relatively little attention was paid to the supervision of banks (Mitchener and Jaremski, 2015). The monitoring of risk was left to private creditors, who mainly relied on the limited data published on balance sheets. Very few states engaged in regular examinations, but they were generally unable to enforce actions to prevent the insolvency of banks. Hence, banks were "free" to go bankrupt and exit the market. This is notably different to modern financial systems, where prudential regulation is more sophisticated and banks can count on the implicit guarantee of being rescued in the event of default. For these reasons, we believe that the US free banking period is an ideal framework to study how the relaxation of barriers to bank entry affected the size and efficiency of the banking system.

Even though limiting the riskiness of banks was not among the main objectives of regulators, the existence of private cooperative arrangements might be a concern for our study. These arrangements represented a

[10] Bodenhorn (2006) refers to a series of events unrelated to the state of the economy that led to the formation of an organized opposition to the political class in power before 1838. In particular, he argues that the case of William Morgan, a Freemason who was kidnapped and disappeared after threatening to reveal the secrets of Freemasonry, unleashed a political movement that became increasingly critical of the dominant class.

form of coinsurance among banks in times of financial distress. Hence, bank failures might be the result of excessive risk-taking fueled by coinsurance.[11] Nevertheless, we argue that there are reasons to believe that private arrangements generate less severe distortions to bank incentives than public safety nets.

First, this hypothesis is consistent with the analysis of Gorton and Huang (2003) on the origins of central banking, which is based on the assumption that the regulator is worse than banks at monitoring.[12] If this is the case, it follows that banks' incentives to take risk will be stronger under a system of public safety nets. Second, Calomiris and Kahn (1996) argue that banks could join a private cooperative arrangement only if they complied with a set of rules, which aimed at limiting moral hazard. Calomiris and Kahn also maintain that a bank could be expelled from a private cooperative arrangement if its behavior represented a threat to the reputation of the coalition. It follows that the competitive distortions generated by private arrangements were likely to be minor, because insolvent banks were not rescued. Nonetheless, in our empirical analysis we control for the existence of private cooperative arrangements.[13]

A further concern is that states introduced other laws and institutional arrangements at the same time as free banking laws. If these measures could influence the size and efficiency of the banking system, this would then bias our estimates of the effect of free banking laws. To address this issue we collected information on usury laws, general incorporation laws, shareholder liability, branch-banking, state-owned banks, and suspensions of convertibility and included them as additional controls in our empirical analysis. In the data appendix we provide a detailed description of those state-by-year varying legislation controls and discuss their potential threat to our identification strategy.

C. The Role of Banks and Credit in the United States between 1837 and 1863

Economic historians have argued that the rapid growth over the 1837-1863 period was finance-led (Rousseau and Sylla, 2005). Sylla (1999) claims that the "Federalist Financial Revolution" promoted by the Secretary

[11] Examples of private cooperative arrangements were the Suffolk System, which was a clearing system of New England's banks, and the New York Safety Fund, an insurance fund for New York's banks. Banks in Indiana, Iowa, Michigan, and Vermont also set up forms of cooperative arrangements (e.g., Weber, 2011b; Klebaner, 2005).

[12] The authors demonstrate that banks can effectively monitor each other, but have the incentives to do so only under the threat of a panic. Panics have social costs that banks do not internalize. Hence, in spite of being a worse monitor than banks, a regulator finds it optimal to provide public debt insurance if the social costs of panics are large.

[13] We construct a binary variable, which equals one if a state had a liability insurance system in a given year. Since the Suffolk System was a regional clearing system operating only in New England, we control for it by adding region-by-year fixed effects to our estimating equation. Note that in some states only certain types of banks (e.g., in Ohio and Indiana state banks and their branches) were members of the insurance system. Other states (e.g., Vermont) required new chartered banks to join the system, but later decided to make membership voluntary. We refer to Weber (2011b) for more details of the antebellum liability insurance systems.

of the Treasury, Alexander Hamilton, in the 1790s was crucial to the development of a financial system that was able to finance firms' investments and innovations. Economic historians have recognized that banks played a specific role in the economic development of the antebellum US. For example, Bodenhorn (2000) emphasizes the importance of US banks in providing means of payments, in accumulating savings, and in the efficient allocation of funds. Bodenhorn (1999) demonstrates that banks encouraged invention and financed industrial development, while Atack et al. (2014) argue that the existing banking system in the American Midwest facilitated the diffusion of railroads. What emerges is a picture of banks performing what theory considers to be their main functions.

Nevertheless, some of the typical banking practices in the antebellum period are potentially a concern for our study. First, descriptive statistics reveal that antebellum banks financed roughly half of their (non risk-weighted) assets through capital, while banks today hold an amount of capital that is less than 10% of their assets (Hanson et al., 2011). Second, it was common to lend short-term, against safe collateral, and, at least in the Northeast, predominantly to bank insiders (Lamoreaux, 1994). These two practices suggest agency problems and asymmetric information (that is, the frictions impairing the allocative efficiency of savings) were limited even before states introduced free banking laws. At the same time, some firms were likely to be financially constrained during the antebellum period because loans were mainly granted to insiders and against collateral. Moreover, some banks could be inefficient, but able to survive because they enjoyed monopoly rents. Overall, there are reasons to expect a positive effect of free banking laws on the size and efficiency of the banking system.

III. Data

Our empirical analysis builds on two datasets. The first source is Weber (2006, 2011a), who collected bank balance sheets and reports of the census of state banks during the antebellum period. The balance sheets, which antebellum banks had to report to US state authorities, provide detailed information on assets and liabilities, such as loans, bonds, cash, deposits, and capital. The census of state banks contains location, name, and beginning and end dates of all banks that existed in the United States from 1789 to 1861. Weber's dataset also contains information on the type of bank charter (i.e., free or state chartered banks) and whether banks failed, closed or were still in existence in 1861. We combine the information from the reports of the

census of state banks with the bank balance sheet data by matching them at the bank and year level.

Our analysis of the link between free banking laws and the banking sector spans the period from 1830 to 1860.[14] Since our evidence relies on a difference-in-differences approach, it is necessary to have a sufficiently large pre-treatment window. Our choice is to use 1830 as the starting year (that is, seven years before the first state (Michigan) introduced a free banking law). This allows us to overcome problems with data availability for earlier years.[15] Our analysis ends in 1860, the year before the outbreak of the American Civil War. The 1861-1865 Civil War was an atypically large negative shock to the US economy that may have affected the banking sector in an unusual way.

Our evidence on the size and efficiency of the banking sector relies on both individual and aggregate data. Starting from the bank-level dataset, we match the location of each bank to its corresponding county and state, and then construct aggregates. Our focus is on the individual and state-level analysis, but we also use county-level aggregates to implement the "regression discontinuity" design described in Section IV. This empirical design restricts the analysis to pairs of counties located on the opposite sides of state borders, which we identify using county shape-files provided by NHGIS (National Historical Geographic Information System).[16]

The second source of data is the "National Samples from the Census of Manufacturing: 1850, 1860, and 1870", originally collected by Fred Bateman and Thomas J. Weiss and later extended by Jeremy Atack (Atack et al., 2004). This dataset contains a random sample from the Census of Manufacturing for US states and the District of Columbia, with data on employment, capital invested, and type and value of inputs and outputs, and a 3-digit SIC industry code. Jaremski (2014) uses this dataset to investigate the link between finance and industrial development in the US during the second half of the nineteenth century. While his focus is on evaluating how bank growth during the 1860s affects manufacturing activity at the county and establishment-level, our aim is to perform a test along the lines of Rajan and Zingales (1998). To this end, we aggregate the data from the 1850, 1860 and 1870 Census of Manufacturing at the industry and state level and link it to the dataset on banks. Even though the establishments included in the Census vary over time, aggregation should not induce any bias since the yearly samples are representative at the national level.

We use two measures to proxy for the external dependence on finance in antebellum US. The first measure

[14] We exclude Washington D.C. from our sample as it is a federal district.

[15] During the free banking era banks sent annual reports to the state authorities and the problem of missing data became less problematic (Jaremski, 2010).

[16] County shape-files are retrieved from https://www.nhgis.org/.

draws on Cetorelli and Strahan (2006, Table 2), who provide a measure of external financial dependence for mature US firms at (2-digit SIC) industry level. After converting the industry codes from 3 to 2 digits, we construct a dummy variable that equals one if an industry has a positive external financial dependence, according to Cetorelli and Strahan (2006, Table 2). Hence, we implicitly rely on the assumption that the external financial dependence of industries in antebellum US is the same as in modern times. This assumption is similar to Rajan and Zingales (1998), who use a measure of external dependence based on US data for countries at different stages of economic development. For the second measure, we follow Mitchener and Wheelock (2013) and use the average (output) size of the establishments in each industry in 1850.[17] The rationale for this measure is that the size and efficiency of the banking sector should affect small firms more than large ones, because the former are more likely to be financially constrained. At the same time, small firms may potentially depend less on external finance, because of technological reasons (Mitchener and Wheelock, 2013). Overall, using these two different measures for external financial dependence allows us to check the robustness of our results. Descriptive statistics of the main variables of interest are shown in Table 2.

IV. Free Banking and the Size of the Banking Sector

In this section we investigate how the introduction of free banking laws affected the size of the banking sector at the state level. We use the number of banks to measure the size of the banking sector. The interpretation of this measures is that if the introduction of free banking laws increased the size of the banking sector, we should observe an increase in the number of banks per capita. Hence, we are implicitly assuming that there was no financial intermediation before the creation of banks. However, there is evidence of an important role of trade credit in the US economic development.[18] This might suggest that banks represent an institutionalization of trade credit, but it did not lead to an increase in the volumes. Even though this was true, we argue that a higher number of banks improves allocative efficiency, mainly through their ability to better pool and mobilize savings, while the availability of trade credit is tied to a transaction between two parties.

[17] Mitchener and Wheelock (2013) also use the fraction of establishments incorporated and the ratio of bonds to total output as a measure of external dependence. Unfortunately, we cannot use these measures, because data are not available, to the extent of our knowledge.

[18] For example, Rousseau and Sylla (2006) argue that cotton factors, who were borrowing from banks located in the city, extended short and medium-term trade credit to cotton farmers by discounting their notes.

A. *Estimation Strategy: State-level Analysis*

Our estimation strategy exploits the state-by-year variation in the introduction of free banking laws using a difference-in-differences approach. Our estimation equation takes the following form:

$$y_{s,t} = \lambda_s + \lambda_s t + \lambda_s t^2 + \lambda_{r,t} + \beta FB_{s,t} + \Gamma X_{s,t} + \epsilon_{s,t}. \quad (1)$$

The dependent variable, $y_{s,t}$, is the number of banks (in logarithmic units).[19] The variable of interest, $FB_{s,t}$, is an indicator variable that takes the value one for all years t since state s introduced a free banking law. State fixed effects, λ_s, capture any state-specific time-invariant factors, such as geography and historical factors. The region-by-year fixed effects (i.e., every region fixed effect is interacted with year fixed effects, so that there is no need to control for year fixed effects separately), $\lambda_{r,t}$, control for any variation between US Census regions over time, such as regional business cycles and growth trends.[20] We also add a quadratic trend, $\lambda_s t$ and $\lambda_s t^2$, to estimating equation (1).

We further include a set of state varying control variables, $\Gamma X_{s,t}$. These include the lagged average age of banks, white population, and the state-by-year varying legislation variables mentioned in Section III to make sure that our results are not driven by other laws and institutional arrangements than free banking.[21] Following Bodenhorn (2008), we also include the interaction between year fixed effects and a set of initial state controls, such as population size, urbanization rate, manufacturing share, commercial share, number of banks per white population, and bank capital per white population in 1830. We add these covariates in order to mitigate the concern that the timing of states' decision to adopt free banking laws could be systematically related to state-level outcomes in 1830. We estimate equation (1) using least squares and cluster the error term, $\epsilon_{s,t}$, at the state level.

B. *Estimation Strategy: Identification Using Discontinuities at State Borders*

The state-by-year variation in the introduction of free banking laws, together with the federal structure of the United States, allows us to exploit state borders to implement a "regression discontinuity" design. Following the identification strategy of Holmes (1998), we restrict the sample to contiguous counties lying

[19] The number of banks in logarithmic units is measured as ln(banks +1).

[20] The US Census regions in our sample are: New England, Mid-Atlantic, Midwest, and the South.

[21] To calculate white population annually, we interpolate the white population series from the decennial US census assuming a constant growth rate within the decade.

on the border of states that passed a free banking law at different points in time (see Appendix Figure 1).[22] This approach facilitates the identification of the effect of free banking laws because counties that are geographically close are more likely to be similar in terms of unobservables, such as growth trends and economic shocks. For example, if our estimates were driven by the fact that New York introduced a free banking law 22 years before Pennsylvania because of better growth opportunities, we would not observe any significant effect using this approach, because two contiguous counties separated only by a state border are likely to share the same economic fundamentals.

The estimation strategy follows closely the regression discontinuity design of Dube et al. (2010). We estimate the following equation for the number of banks:

$$y_{cp,t} = \lambda_c + \lambda_s t + \lambda_s t^2 + \lambda_{p,t} + \beta FB_{s,t} + \Gamma X_{c,t} + \epsilon_{cp,t} \tag{2}$$

The important difference to equation (1) is the inclusion of pair-by-year fixed effects ($\lambda_{p,t}$), which implies that our estimates are based only on the variation within each contiguous border county pair. We also note that state fixed effects are replaced by county fixed effects, λ_c, in estimating equation (2). Our identifying assumption is that, within county pairs, differences in the treatment are uncorrelated with the differences in the error term (i.e., $E(FB_{s,t}, \epsilon_{cp,t}) = 0$). We use two-dimensional clustering to account for within-state over time and within-border segment over time correlations (a border segment includes all counties that are located at one side of the border).[23]

C. Results

Table 3 presents the estimates of the average effect that the introduction of free banking laws had on the size of the banking sector during the 1830-1860 period. Columns (1)-(2) report the results for the state-level analysis, while columns (3)-(4) show the results for the border counties. The dependent variable is the number of banks (in logarithmic units). Columns (1) and (3) report the effect of free banking laws without initial 1830 controls interacted with time, which are added to the estimating equation in columns (2) and

[22] Other studies that exploit policy discontinuities at the state border to investigate how regulatory changes affect bank performance are Huang (2008) and Danisewicz et al. (2013), for example.

[23] Two-dimensional clustering is required since with the border-county approach the same county can be included in multiple pairs inducing mechanical correlation. See, for example, Dube et al. (2010) for further details on using two-dimensional clustering for a contiguous border county pair sample.

(4). At the state-level, the coefficient on the free banking indicator is positive and statistically significant at the 1-percent level. The point estimate implies that introducing free banking increased the number of banks by around 25 percent.

One concern might be that our results are driven by the state of New York. This state could be regarded as a special case, since banking activity after the introduction of free banking laws in 1838 was more pronounced than in other states. Reassuringly, the point estimate on free banking remains almost unaffected ($\hat{\beta} = 0.25$; standard error = 0.08) if we exclude New York from the sample. Another concern might be that the construction of railroads in the Midwest influenced the creation of new banks and the introduction of free banking laws (Atack et al., 2014). While excluding Midwestern States (seven states in our sample) reduces the magnitude of the estimated coefficient, the result is still positive and highly statistically significant ($\hat{\beta} = 0.13$; standard error = 0.06).[24] Finally, we present the results of our border county identification strategy in columns (3) and (4). While there is a significant drop in the magnitude of the point estimates, the findings remain qualitatively unchanged when we use the more restrictive border county sample. The coefficient on the free banking indicator is 0.05 and statistically significant at the 5-percent level.

Overall, our empirical evidence suggests that the introduction of free banking laws increased the size of the banking sector (i.e, if more banks imply a higher credit volume). Since banks are able to pool and mobilize savings and allocate them across different firms, these findings support the view that relaxing barriers to bank entry increases the credit volume and improves the allocative efficiency of savings. Studies based on modern data (Jayaratne and Strahan, 1996) do not find an effect of the relaxation of branching restrictions on credit volume. This difference could be due to the fact that entry happened in areas not served by banks previously, or where the supply of credit, for example trade credit, was inefficiently low. While the existing literature on the free banking period found mixed evidence on bank entry and considered the free banking period as a period of instability with numerous bank failures, we document a positive net effect using the more comprehensive census of banks during the US antebellum period (Weber, 2006). Hence, the process of entry and exit following the introduction of free banking laws resulted in a larger banking sector.

[24] Results excluding New York and the Midwest states are available upon request.

V. Free Banking and Allocative Efficiency: Bank-Level Evidence

In this section we examine whether the introduction of free banking laws led to improvements in the allocative efficiency of savings. Previous literature has focused on the sources of risk for free and state banks but has neglected how the latter reacted to the introduction of free banking laws (e.g. Jaremski, 2010). We fill this gap, with the aim to test whether relaxing barriers to bank entry improves the allocative efficiency of incumbent (state chartered) banks. To this end, we focus on two outcomes, namely riskiness and growth.

A. Estimation Strategy

The first step of our individual analysis is to investigate the effects of free banking laws on the probability of failure and closure of incumbents. One would expect incumbent banks to become more likely to close if more efficient banks entered the market after the barriers to bank entry were lifted. It could also be argued that the higher degree of bank competition following the introduction of free banking laws led incumbents to take more risk and hence increased their likelihood to default. On the other hand, incumbent banks might become more efficient, and thus less likely to fail or close, in order to prevent other banks from entering the market. To test which effect dominates, we estimate the following model:

$$y_{i,t} = \alpha_s + \lambda_t + \beta FB_{s,t} + \Gamma X_{i,t-1} + \Theta Banks_{l,t-1} + \epsilon_{i,t}. \tag{3}$$

The dependent variable, $y_{i,t}$, is an exit dummy, which captures whether an incumbent bank failed or closed at time t. Our variable of interest is the free banking indicator variable, $FB_{s,t}$. We control for state fixed characteristics, α_s, lagged bank-level balance sheet ratios and age, $X_{i,t-1}$, the number of banks in each city, $Banks_{l,t-1}$, and year fixed effects, λ_t. We restrict the sample to incumbent banks, which we define as the banks that existed at least three years before the introduction of free banking laws. For states that never introduced free banking laws, we define as incumbents all the banks that existed during our sample period.

The second step of our individual analysis is to investigate whether the introduction of free banking laws affected the growth rate of incumbents. Our approach to test this hypothesis is twofold. First, we follow Jayaratne and Strahan (1998) and examine how the growth rate of the most and the least efficient banks, measured by their performance, changes after the introduction of free banking laws. In contrast to Jayaratne and Strahan (1998), we cannot use profitability as a measure of performance because we only have data on

profits after dividends were paid. Given the limitations of our bank balance sheet data, we focus on an ex-post measure of performance, that is whether a bank exited the market or survived during our sample period. The rationale for this measure is that a bank that exits the market is presumably less efficient than a bank that survives, either because of a lower screening and monitoring ability, or high operating costs. To test the effect of free banking laws on the growth rate of closed or failed incumbents relative to the survivors, we estimate the following specification:

$$y_{i,t} = \alpha_i + \lambda_t + \beta_1 FB_{s,t} + \beta_2 FB_{s,t} \times Exit_i + \Gamma X_{i,t-1} + \Theta Banks_{l,t-1} + \epsilon_{i,t}. \quad (4)$$

We consider the yearly growth rate of incumbents' assets as dependent variable.[25] In addition to the control variables in equation (3), we include an interaction term between $FB_{s,t}$ and $Exit_i$, which is a dummy that takes the value one if a bank failed or closed during our sample period.[26] We are interested in the free banking indicator, $FB_{s,t}$, and the interaction term, $FB_{s,t} \times Exit_i$. The former captures the difference between the growth rate of survivor incumbents after the introduction of free banking laws, relative to states without these laws. To estimate $FB_{s,t}$, we compare two types of banks that existed until the end of our sample period: One is a bank like the Steuben County Bank, which opened in New York five years before the enactment of a free banking law, and the other is a bank such as the Coventry Bank, which was established in Rhode Island in 1850.[27] The interaction term $FB_{s,t} \times Exit_i$, by contrast, reflects the difference in the growth rate of incumbent banks that exited the market in states with and without free banking laws. For example, our estimate compares a bank like the Madison County Bank, which was created in New York six years before the enactment of a free banking law and closed in 1858, to a bank such as the Rhode Island Agricultural Bank, which opened in Rhode Island in 1825 and closed in 1843.

Our second approach investigates the dynamics of asset growth after bank entry. Since both of the two chartering regimes during the 1837-63 US allowed the creation of new banks, we are able to test the reaction of incumbents to bank entry, the introduction of free banking laws and the interaction of the two. If free banking laws relaxed barriers to entry, we should observe incumbent banks growing faster, either to discourage potential competitors or keep their market share. By contrast, this effect should be less

[25] Results using market share as a dependent variable are available upon request.
[26] Note that the $Exit_i$ dummy is time-invariant and absorbed by the bank fixed effects α_i.
[27] Note that we control for age to account for the different growth rate of young banks.

pronounced in states that did not enact free banking laws, as barriers to entry protected the market power of the incumbent. To test these two hypothesis, we estimate the following specification:

$$\begin{aligned} y_{i,t} = \alpha_i + \lambda_t + \beta_1 FB_{s,t} + \beta_2 Entry_{i,l,t} + \beta_3 FB_{s,t} \times Entry_{i,l,t} + \\ + \beta_4 FB_{s,t} \times NoEntry_{i,l} + \Gamma X_{i,t-1} + \Theta Banks_{l,t-1} + \epsilon_{i,t}. \end{aligned} \tag{5}$$

The yearly growth rate of incumbents' assets, $y_{i,t}$, is regressed on the free banking indicator, $FB_{s,t}$, the dummy $Entry_{i,l,t}$, which is equal to one for incumbent banks after a new bank enters in the city where they are located, and their interaction. For example, let us consider the Bank of Genesee, that was established in Batavia, New York, in 1831. For this bank, the $Entry_{i,l,t}$ dummy turns to one in 1839, when, one year after New York passed a free banking law, the Exchange Bank of Genesee opened in Batavia, New York. Thus, the interaction $FB_{s,t} \times Entry_{i,l,t}$ captures the post-entry growth rate of an incumbent in a state with free banking, such as the Bank of Genesee, compared to another incumbent in a state without free banking, such as the Cranston Bank, Rhode Island. This bank was the only bank in Cranston until 1854, when the Elmwood Bank was established. Hence, the interaction $FB_{s,t} \times Entry_{i,l,t}$ indicates the effect of a new entrant on incumbents, depending on the severity of bank entry barriers.

Additionally, specification (5) includes the interaction between our free banking indicator and the dummy $NoEntry_{i,l}$. This dummy, which equals one for incumbent banks located in cities where no bank ever entered, is by definition orthogonal to $Entry_{i,l,t}$.[28] For example, the $NoEntry_{i,l}$ dummy is a constant equal to one for the Central Bank of Cherry Valley, that was the only bank in Cherry Valley, New York, until the end of our sample period. Thus, the interaction $FB_{s,t} \times NoEntry_{i,l}$ captures the growth rate of an incumbent located in a state with free banking and a city where no bank ever entered, such as the Central Bank of Cherry Valley, compared to a similar incumbent in a state without free banking, such as the Franklyn Bank. This bank was the only bank in Chepachet, Rhode Island, from its creation until the end of our sample period. Overall, the interaction $FB_{s,t} \times NoEntry_{i,l}$ reflects how incumbents react to the threat of a new entrant, depending on the severity of bank entry barriers.

Finally, notice that, in this specification, the free banking indicator $FB_{s,t}$ captures the pre-entry growth rate of incumbents located in states with free banking, relative to those incumbents in states without free banking. For example, $FB_{s,t}$ would pick up the difference in the growth rate of the Bank of Genesee before

[28] Specification (5) does not include the dummy $NoEntry_{i,l,t}$ because this is absorbed by the bank fixed effect.

1839 and the Cranston Bank before 1854. Hence, this effect provides further evidence on whether incumbents react to the threat of new entrants.

B. Results

Table 4 reports the marginal effects from a logit estimation of equation (3). In column (1), we show the effect of free banking laws after controlling for bank balance sheet ratios, age, number of banks, and year fixed effects. The point estimate indicates a 0.8 percentage point reduction in the probability of exit of incumbent banks, which is statistically significant at the 5-percent level. The coefficient on the free banking indicator becomes smaller and remains statistically significant at the 5-percent level, as we include state (column 2) and county fixed effects (column 3). Our maximum estimated effect indicates a 3 percentage point reduction in the probability of exit of incumbent banks.

In columns (1)-(3) of Table 5 we present the least squares estimates of equation (4) for the asset growth of incumbent banks. In all the three columns, we report the effect of free banking laws controlling for bank balance sheet ratios, the city-level number of banks, and bank and year fixed effects. In column (2) we add the county observables in 1830 interacted with year dummies, while column (3) also includes the legislation variables. Our point estimates indicate that the introduction of free banking laws did not significantly affect the growth rate of incumbent banks that survived during our sample period. The coefficient on the interaction term between the free banking indicator and the exit dummy is negative and statistically significant at the 5-percent level in column (1), but not in the other two specifications. Our main finding is a negative effect of free banking laws on the growth rate of the incumbent banks that exited the market. The cumulative effect of the free banking indicator and the interaction term is negative and statistically significant, at least at the 10-percent level, for all specifications. The estimates suggest that the introduction of free banking laws decreased the growth rate of incumbent banks that exited the market by 5-6 percentage points.

The link between the entry of new banks and the growth rate of incumbents provides additional insights into the evolution of the banking sector. Columns (4)-(6) of Table 5 report the estimates of specification (5), using the same set of controls as in columns (1)-(3). The sum of the coefficients on $FB_{s,t}$ and $FB_{s,t} \times Entry_{i,l,t}$ is positive but never statistically significant. The same holds for the linear combination of $FB_{s,t}$ and $FB_{s,t} \times NoEntry_{i,l}$, even though the p-value is roughly 0.15 in columns (4) and (5). This evidence indicates that, on average, free banking laws did not induce incumbent banks to expand, neither after new banks

opened nor in cities where no bank ever entered.

What is more interesting is the dynamics behind these average effects. In states with free banking laws, incumbent banks grew between 4.1 and 5.4 percentage points faster after the creation of a new bank, compared to the period between the regulatory change and entry. Relative to the same incumbents and time period, incumbent banks in cities with no bank entry also grew faster, as indicated by the coefficient on $FB_{s,t} \times NoEntry_{i,l}$. Moreover, the negative coefficient on $FB_{s,t}$ suggests a lower growth rate of incumbents between the introduction of free banking laws and the entry of a new bank. Even though we are unable to establish the direction of causality, this evidence is consistent with incumbents reacting to potential competition. New banks entered only in cities where incumbents did not expand after the introduction of free banking laws, but not where incumbents grew faster. In cities where entry occurred, incumbents reacted with a higher growth rate, presumably to keep their market share or prevent further entry.

Columns (4)-(6) of Table 5 also reveal a negative coefficient on the $Entry_{i,l,t}$ dummy. This coefficient indicates that, following the creation of a new bank in states without free banking laws, banks reduced their growth rate between 2.8 and 3.4 percentage points compared to those in cities with no entry. This result suggests no concern about potential competition for banks in states without free banking laws. Since bank entry in these states was subject to political rather than market forces, the new bank and the incumbent had incentives to share rents rather than compete with each other.

Overall, our evidence indicates that the introduction of free banking laws decreased the exit probability of incumbents and led incumbents that exited the market to grow less. Under the reasonable assumption banks exiting the market were the least efficient ones, our results are consistent with an improvement in the allocative efficiency of savings. Moreover, our evidence of more competition in cities without bank entry suggests that not only were the beneficial effects of free banking linked to new bank creation, but also to an expansion of incumbent banks.

VI. Free Banking and Allocative Efficiency: Sector-Level Evidence

This section examines the link between free banking laws and allocative efficiency using a test à la Rajan and Zingales (1998). If free banking laws induced a virtuous selection effect among banks, the wedge between the cost of internal and external finance should decrease because of the improvement in the ability to screen and

monitor firms. This selection effect should have larger benefits for firms relying on external finance, which should exhibit a disproportionately larger growth rate. By contrast, the growth rate of these firms should be lower if free banking laws impaired the efficiency of financial intermediation.

We use two measures to proxy for the external dependence on finance in antebellum US. The first is a measure of external financial dependence for mature US firms developed by Cetorelli and Strahan (2006). Along the lines of Rajan and Zingales (1998), Cetorelli and Strahan (2006) consider the sum of capital expenditures, net of operating cash flows, as an indicator of a firm's dependence on external finance. The actual measure used in Cetorelli and Strahan (2006) is obtained by aggregating at the (2-digit SIC) sector level, and constructing a dummy that equals one if an industry has a positive dependence on external finance. In this study, we follow the same approach as Cetorelli and Strahan (2006, Table 2) after converting the industry codes from 3 to 2 digits. Hence, we implicitly rely on the assumption that the external financial dependence of industries in antebellum US is the same as in modern times. This assumption is similar to Rajan and Zingales (1998), who use a measure of external dependence based on US data for countries at different stages of economic development.

For the second proxy of dependence on external finance, we follow Mitchener and Wheelock (2013) and use the average (output) size of the establishments in each industry.[29] The rationale for this measure is that the size and efficiency of the banking sector should affect small firms more than large ones, because the former are more likely to be financially constrained. Hence, average establishment size is an inverse measure of external finance dependence, which implies that the sign of the coefficients should be the opposite as those obtained using Cetorelli and Strahan (2006)'s measure.

Finally, it is important to mention that we are not able to make statements about the effect of free banking laws on economic growth. Instead, we are interested in comparing firms with different levels of external dependence, with the aim to provide evidence of a channel through which finance could affect growth.

[29] Mitchener and Wheelock (2013) also use the fraction of establishments incorporated and the ratio of bonds to total output as a measure of external dependence. We cannot use these measures due to missing data for our sample period.

A. *Estimation Strategy*

This section puts forward our specifications to test the link between free banking laws and allocative efficiency at the industry-level. The baseline estimating equation takes the following form:

$$y_{s,i} = \lambda_s + \lambda_i + \alpha Share_{s,i} + \beta FB_s \times ED_i + \epsilon_{s,i}. \tag{6}$$

The dependent variable, $y_{s,i}$, is the growth rate of value added in state s and industry i, calculated between 1850 and 1860.[30] The variable of interest, $FB_s \times ED_i$, is the interaction term between an indicator that equals one if state s had introduced a free banking law by 1850 (FB_s) and the measure of external dependence (ED_i). Since we use growth rates from 1850 to 1860, the evidence from our baseline specification uses New York, New Jersey, and Alabama as free banking states.[31] To allow for more states with free banking laws, we conduct the same test adding value added growth from the period 1860-1870 to the sample.

Note that our baseline specification does not include the free banking indicator, FB_s, nor the external dependence measure, ED_i, as we control for state fixed effects, λ_s, and industry fixed effects, λ_i. These fixed effects absorb any time invariant state and industry level characteristic, including whether a state installed free banking laws by 1850. Including state fixed effects also helps to mitigate endogeneity concerns as they absorb any other state specific law or growth trend (in levels) that is correlated with free banking laws. Finally, our baseline specification includes the 1850 share of value added of industry i in state s, to control for size effects.

In a second specification, we investigate whether the effect of free banking laws depends on other state-level characteristics of the banking system. The estimating equation is the following:

$$y_{s,i} = \lambda_s + \lambda_i + \alpha Share_{s,i} + \beta_1 FB_s \times ED_i + \beta_2 FB_s \times ED_i \times BK_s + \beta_3 BK_s \times ED_i + \epsilon_{s,i}. \tag{7}$$

Compared to the baseline version, the main difference is the triple interaction term, $FB_s \times ED_i \times BK_s$. This interaction term indicates whether the effect of free banking laws depends on the sector-level dependence on external finance and the number of banks per white population in 1850, BK_s. The intuition for including

[30] Value added growth is constructed as the aggregated value of output minus the aggregated value of inputs in logarithmic units).

[31] Georgia, another free banking state, is not included in our sample because data from the US Manufacturing Census are missing.

the triple interaction term is that the growth-enhancing effect of free banking laws could be stronger in more financially developed states. Finally, note that estimating equation (7) also includes the interaction term $BK_s \times ED_i$, as this interaction is not absorbed by the fixed effects.

Finally, we estimate a panel specification using the 1850-1860 and 1860-1870 value added growth rates, to ensure that our results remain unchanged including the states that installed free banking during the 1850s. The estimating equation is the following:

$$y_{s,i,t} = \lambda_{s,t} + \lambda_i + \beta FB_{s,t} \times ED_i + \alpha Share_{s,i,t} + \epsilon_{s,i,t}. \tag{8}$$

Compared to the baseline specification, the main difference is the time subscript t. Note, while value added growth is measured for the period 1850-1860 and 1860-1870, variation in the variables of interest are considered only from the period before the Civil War, such as whether a state had a free banking system in 1850 and 1860. By adding the state-by-year fixed effect, $\lambda_{s,t}$ we rule out any state-by-year variation, including a direct effect of free banking on value added growth. This implies that our evidence only relies on within state-by-year variation across different industries. We estimate equations (6), (7), and (8) using least squares, and cluster the error term both at the state level and industry level.

B. Results

Table 6 contains our results on the link between free banking laws and allocative efficiency at the sector level. The dependent variable is the growth rate of value added between 1850 and 1860, and the method of estimation is least squares. Columns (1) and (3) report the results from estimating equation (6), using the external dependence measure of Cetorelli and Strahan (2006) and the average establishment size as proxies for external dependence. The variable of interest is the interaction effect between the free banking indicator and the external dependence measure, $FB_s \times ED_i$. We further control for the industry share of value added as well as state and industry fixed effects. The coefficient on our variable of interest is negative and statistically significant at the 5-percent level, but only when using the average establishment size as the measure of external dependence. The coefficient implies that a firm at the 25^{th} percentile of the establishment size distribution, compared to another at the 75^{th} percentile, grows 50 percent faster in free banking states, relative to the sample mean. This is in line with the hypothesis that free banking laws relaxed financial

constraints, provided that raising outside finance was more difficult for small firms.

Columns (2) and (4) report the results from estimating equation (7). For this specification we are interested in the triple interaction term, $FB_s \times ED_i \times BK_s$, as well as the interaction between the free banking indicator and the external dependence measure, $FB_s \times ED_i$. The coefficients on these two variables are statistically significant at the 1-percent level and have opposite signs, depending on the measure of external finance. This is reassuring, as the sectors with smaller firms are those for which a large and efficient banking sector is more important. The estimates suggest that the effect of free banking laws on the growth rate of value added depends on both the sector's external dependence and on the number of banks per white population in a state.

At the bottom of columns (2) and (4) we report the combined effect of $FB_s \times ED_i \times BK_s$ and $FB_s \times ED_i$, evaluated at the median, mean, and maximum number of banks per white population in a state. Sectors depending more on external finance, as measured by the Cetorelli and Strahan (2006) proxy, grow less in states with free banking laws and the median number of banks. The combined effect, which is statistically significant at the 1-percent level, corresponds to a 76 percent slower grow rate, compared to the average growth rate of value added in 1850-1860. The effect of free banking laws is not statistically significant in states with the average number of banks, whereas it is positive and statistically significant at the 5-percent level if the number of banks is at the maximum. The combined effect corresponds to a growth rate larger by a factor of four compared to the average growth rate of value added in 1850-1860. When using average establishment size as external dependence measure, we obtain that smaller firms grow significantly more in states with free banking laws and an above average number of banks per capita. To summarize, these results suggest free banking laws improved the allocative efficiency in states with a high number of banks.

We obtain similar results when using panel regressions in Table 7, suggesting that our results are not driven by events such as the Civil War. The interaction terms, as well as the combined effects, retain the same sign and statistical significance when using the measure of Cetorelli and Strahan (2006). While the variables of interest lose statistical significance in the model with triple interaction when using average establishment size as the measure of external dependence, the combined effects are in line with the static specification. The effect at the maximum number of banks, which does not appear to be statistically significant, has a p-value equal to 0.15.

Overall, our empirical evidence indicates variation across states in the effects of free banking on industry

growth. Relaxing barriers to entry favored a virtuous selection process in states with a large number of banks, but impaired the efficiency of financial intermediation in the remaining states. One explanation for this finding is that bank exit entails lower social costs in states with many banks, because switching banks is easier. An alternative explanation is that industry growth depends both on the quantity of credit and its allocative efficiency. A certain availability of credit, as proxied by the number of banks, has a stronger growth-enhancing effect in states with free banking laws. At the aggregate industry-level, however, only states with free banking and a high number of banks grow faster. Finally, note that our results represent a comparison of industries based on their external dependence, but they do not allow us to draw conclusions about the effect of free banking laws on economic growth at the state level. This is beyond the scope of our paper, which focuses on a specific channel through which financial development affects economic growth.

VII. Conclusion

We have investigated how the introduction of free banking laws in US states between 1837 and 1863 affected the size and the efficiency of the banking system. With free banking laws, governments gave up their power over bank chartering and allowed any individual to establish a bank, provided that certain legal requirements were satisfied. This policy change, the entry of banks under both the old and new chartering regimes and the limited influence of regulators on bank exit makes the free banking era an ideal period to study the effects of relaxing barriers to bank entry. We studied the effects of these policies, like the literature on more recent pro-competitive measures in the US, and their interaction with bank entry in a setup with limited barriers to bank exit.

We have shown that the introduction of free banking laws significantly increased the size of the banking system and documented a positive effect on the allocative efficiency of savings, based on our incumbent-level analysis. Finally, using an approach à la Rajan and Zingales (1998), we have found that free banking laws fostered the growth of industries relying more on external finance, but only in states with a larger number of banks. Overall, our empirical evidence is consistent with a virtuous selection process of banks operating in a system with no barriers to bank entry and exit. We find support for the regulators' view only at the industry-level and for states with few banks, suggesting beneficial effects of bank entry and exit conditional on a certain degree of financial development. This interpretation resonates with Rioja and Valev (2004) and

Aghion et al. (2005), who document a weaker finance-growth nexus at low levels of financial development.

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Tables and Data Appendix

Laws and Institutional Arrangements

(a) *Usury Laws*: During the 19th century, US states had usury laws, which imposed a ceiling on the interest rate a bank could charge to a borrower.[32] If states that introduced free banking laws were also raising the interest rate ceiling, our results might be biased. The reason is that a higher ceiling allows banks to serve a wider pool of borrowers, which makes bank entry more attractive but also increases credit risk. To address this concern we collect the data reported in Holmes (1892) to control for the state-by-year variation in usury laws.

(b) *General Incorporation Laws*: The evolution of the chartering policy for non-financial corporations resembles that of banks. During the 19th century, US states gradually lifted barriers to entry for non-financial corporations by introducing general incorporation laws. If states introduced general incorporation laws besides free banking laws, our results could be biased. It might be that a wave of new firms spurred the demand for credit, which made bank entry more profitable and incentivized banks to take more risk. To address this threat we collect the data reported in Evans (1948) to control for the state-by-year variation in general incorporation laws.

(c) *Shareholders' Liability*: Prior to the Civil War, shareholders of US banks had either unlimited, double, or limited liability (Dewey, 1910). Legal differences in shareholders' liability may have affected banks' risk-taking incentives and the costs to establish a new bank (Grossman, 2001, 2007). A concern with our study might be that the states introducing free banking laws also gave shareholders the privilege of limited liability. To address this concern, we control for the state-by-year variation in shareholders' liability laws using the data collected from Dewey (1910) and Knox (1903).

(d) *Branch-Banking*: Calomiris and Schweikart (1991) argue that the banking system in the American South was more resilient than in the rest of the United States, because it was structured as a network of bank branches. This structure presumably deterred the creation of new banks, as the existing banks could establish new branches in locations where no bank existed. This raises the concern that states

[32] Rockoff (2003) provides a detailed examination of the economic history of usury laws in the United States. For a study of the political economy of US state usury laws we refer to Benmelech and Moskowitz (2010).

which retained the state bank chartering policies were those where bank branching was allowed. To address this concern, we use information provided by Weber's (2006, 2011) databases on branches to construct an indicator variable which equals one in the states and years in which branch banking existed.

(e) *State-Owned Banks*: Some US states established state-owned banks before the American Civil War. One may argue that state-owned banks were relatively safe, since they were often financing governments' investments in infrastructure and other public projects (Knox, 1903). It might also be that states imposed higher barriers to bank entry in order to shield the state-owned banks from bank competition. Hence, our results could be biased if states retaining the traditional bank chartering policies were also those establishing state-owned banks. To address this concern, we use information provided by Weber's (2006, 2011) databases on state-owned banks to construct an indicator variable which equals one in states and years in which state-owned banks existed.

(f) *Suspensions of Convertibility*: Suspending the convertibility of banknotes was a common measure to stop a panic and prevent bank failures. If states that retained the traditional bank chartering policy suspended convertibility in crisis periods, our evidence might not be driven by the introduction of free banking laws. To address this concern, we control for the state-by-year variation in suspensions of convertibility using the data compiled by Jalil (2015).

TABLE 1

THE EIGHTEEN UNITED STATES FREE BANKING STATES

States	Year
Michigan	1837/1857(a)
New York	1838
Georgia	1838
Alabama	1849
New Jersey	1850
Massachusetts	1851
Vermont	1851
Ohio	1851
Illinois	1852
Connecticut	1852(b)
Indiana	1852
Wisconsin	1852
Tennessee	1852(c)
Florida	1853
Louisiana	1853
Minnesota	1858
Iowa	1858
Pennsylvania	1860

Notes: Source: Rockoff (1972).

(a) Michigan suspendend the free banking law in 1838 and reenacted it in 1857. Source: Rockoff (1972).

(b) Connecticut repealed the free banking law in 1855. Source: Rockoff (1972).

(c) Tennessee repeald free banking in 1858. Soure: Schweikart (1987).

TABLE 2

DESCRIPTIVE STATISTICS

Variables	Obs	Mean	Sd	Min	Max	Interval
			Panel A: State-Level Analysis			
Dependent Variables:						
Banks (in logs)	992	2.49	1.527	0	5.76	Yearly
Independent Variables:						
Free Banking Dummy	758	0.189	0.391	0	1	Yearly
Branch Dummy	819	0.440	0.497	0	1	Yearly
State Owned Dummy	819	0.197	0.398	0	1	Yearly
Limited Liability	819	0.436	0.723	0	2	Yearly
Usury Rate	899	9.524	14.47	6	100	Yearly
Safety Fund Dummy	819	0.128	0.335	0	1	Yearly
Suspension of Convertibility Dummy	819	0.226	0.418	0	1	Yearly
Incorporation Law Dummy	819	0.127	0.333	0	1	Yearly
Urbanization Rate in 1830	837	0.0684	0.0915	0	0.312	1830
Banks per White Population in 1830	837	6.20e-05	9.70e-05	0	0.000513	1830
Bank Capital per White Population in 1830	837	0.0588	0.135	0	0.648	1830
Manufacturing Share in 1830	806	0.162	0.113	0.0331	0.463	1830
Commercial Share in 1830	806	0.0284	0.0200	0.00866	0.0980	1830
Total White Population in 1830	992	400,646	440,874	0	1.919e+06	1830
			Panel B: Individual-Level Analysis			
Dependent Variables:						
Market Share	21,256	0.323	0.342	0	1	Yearly
Asset Growth	17,663	0.0257	0.238	-4.956	3.819	Yearly
Bank Exit Dummy	27,233	0.196	0.397	0	1	Yearly
Independent Variables:						
Age	27,233	14.50	13.75	0	78	Yearly
Total Assets (in log)	19,596	8.160	1.054	3.281	11.93	Yearly
Cash to Assets Ratio	19,596	0.0614	0.0644	0	1	Yearly
Capital to Assets Ratio	19,596	0.459	0.152	0	1	Yearly
Bonds to Assets Ratio	19,596	0.0263	0.0992	0	1	Yearly
Deposits to Assets Ratio	19,596	0.156	0.116	0	0.795	Yearly
Banknotes to Assets Ratio	19,596	0.290	0.150	0	1	Yearly
Loans to Assets Ratio	19,596	0.745	0.183	0	1	Yearly
Incumbent Dummy	17,618	0.896	0.305	0	1	Yearly
			Panel C: Sector-Level Analysis			
Dependent Variables:						
Value Added Growth	465	0.248	1.145	-3.815	4.210	Decennial
Independent Variables:						
External Dependence Dummy	500	0.448	0.498	0	1	Decennial
Average Establishment Size	500	6.005	4.050	2.060	16.92	Decennial
Industry Share	500	0.0697	0.0967	0.00148	0.699	Decennial
Banks per White Population	500	0.0601	0.0447	0.00259	0.161	Decennial

TABLE 3

FREE BANKING AND THE SIZE OF THE BANKING SYSTEM

Dependent Variable:	Log Banks			
	State Level Analysis		Border County Analysis	
	(1)	(2)	(3)	(4)
Free Banking	0.255***	0.257***	0.053**	0.048**
	(0.062)	(0.079)	(0.022)	(0.021)
Observations	779	730	31,646	15,174
R-squared	0.901	0.926	0.962	0.972
Number of States	32	26	30	24
Number of Border Segments			57	36
Region FE x Year FE	Yes	Yes	Yes	Yes
Pair FE x Year FE	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	No	No
County FE	No	No	Yes	Yes
Quadratic State Trend	Yes	Yes	Yes	Yes
Legislation Controls	Yes	Yes	Yes	Yes
Avg. Age of Banking Sector	Yes	Yes	Yes	Yes
Ln White Population	Yes	Yes	Yes	Yes
Initial Controls 1830	No	Yes	No	Yes

Notes: The dependent variable is ln(banks+1). The sample spans the period 1830-1860. For columns 1-2 the unit of analysis is the state, while for columns 3-4 it is the county. Free Banking is an indicator variable that is one in state s and period t if a state introduced a free banking law and zero otherwise. In columns 1-2 (columns 3-4), all specifications include state fixed effects (county fixed effects), region-by-year fixed effects (pair-by-year fixed effects), and a quadratic state trend. Column 2 (4) further includes a set of initial (year 1830) controls fully interacted with year fixed effects (interacted with a linear trend); see Section IV for further details. In columns 1-2 (columns 3-4), Huber robust standard errors shown in parentheses are clustered at the state level (state and border segment level): *** p<0.01, ** p<0.05, * p<0.1.

TABLE 4

FREE BANKING AND PROBABILITY OF BANK EXIT

BANK-LEVEL ANALYSIS -- MARGINAL EFFECTS

	(1)	(2)	(3)
Free Banking	-0.0082**	-0.0151**	-0.03**
	(0.004)	(0.00736)	(0.0119)
Observations	11,805	10,675	6,985
County FE	no	no	yes
State FE	no	yes	yes
Year FE	yes	yes	yes
Bank-Level Controls	yes	yes	yes

Notes: The dependent variable is a dummy that equals one if bank i failed or closed at time t. Free Banking is an indicator variable that takes the value one for all years t since state s introduced a free banking law. Note that the coefficient reported in the table is the marginal effect from a logit estimation. Bank-level controls include the age of banks, the number of banks at the city level, and a set of lagged bank balance sheet ratios, such as the capital to assets, deposits to assets, circulation to assets, loans to assets, cash to assets, and public bonds to assets ratios (estimates not reported in the table). Huber robust standard errors (shown in parentheses) are clustered at the state level: *** p<0.01, ** p<0.05, * p<0.1.

TABLE 5
FREE BANKING LAWS, BANK ENTRY AND EXIT
BANK LEVEL ANALYSIS

	(1)	(2)	(3)	(4)	(5)	(6)
Free Banking	0.00320	-0.0046	-0.0055	-0.0112	-0.0273*	-0.0322*
	(0.0182)	(0.016)	(0.017)	(0.0206)	(0.0156)	(0.0176)
Free Banking x Exit	-0.0580**	-0.0634	-0.0535			
	(0.0269)	(0.041)	(0.038)			
Entry				-0.041***	-0.0354**	-0.036**
				(0.0130)	(0.0139)	(0.0148)
Free Banking x Entry				0.0415***	0.0450**	0.054***
				(0.0140)	(0.0163)	(0.0183)
Free Banking x No Entry				0.0338**	0.0459***	0.048***
				(0.0122)	(0.0119)	(0.0139)
Free Banking + Free Banking x Exit	-0.055***	-0.068*	-0.059*			
	(0.022)	(0.036)	(0.032)			
Free Banking + Free Banking x Entry				0.0302	0.0177	0.0215
				(0.0269)	(0.0215)	(0.0198)
Free Banking + Free Banking x No Entry				0.0226	0.0186	0.0155
				(0.0149)	(0.0137)	(0.0143)
Observations	11,910	10,683	10,683	8,445	7,551	7,551
R-squared	0.377	0.376	0.379	0.427	0.422	0.427
Number of id	894	756	756	793	663	663
Bank FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
Bank-Level Controls	yes	yes	yes	yes	yes	yes
County-Level Controls	no	yes	yes	no	yes	yes
State Legislation Controls	no	no	yes	no	no	yes

Notes: The dependent variable is asset size growth. Free Banking is an indicator variable that takes the value one for all years t since state s introduced a free banking law. Exit is a dummy that equals 1 if bank i failed or closed during our sample period. Entry is a dummy equal to 1 (0) for incumbent banks after (before) a new bank entry. No Entry is a dummy equal to 1 for incumbents in cities where no bank ever entered. Bank-level controls include the age of banks, the number of banks at the city level and a set of lagged bank balance sheet ratios, such as the capital to assets, deposits to assets, circulation to assets, loans to assets, cash to assets, and public bonds to assets ratios (estimates not reported in the table). County level controls include population size, urbanization rate, manufacturing share, agricultural share, banks and bank capital per capita in year 1830 interacted with year fixed effects (estimates not reported in the table). Finally, state legislation controls are usury laws, general incorporation laws, shareholders' liability, branch-banking, state-owned banks, suspensions of convertibility, liability insurance schemes and clearing house arrangements (estimates not reported in the table). Huber robust standard errors (shown in parentheses) are clustered at the state level: *** p<0.01, ** p<0.05, * p<0.1.

TABLE 6

FREE BANKING AND ALLOCATIVE EFFICIENCY

SECTOR-LEVEL EVIDENCE – STATIC SPECIFICATION

Measure of External Dependence:	Cetorelli and Strahan (2006)		Avg establishment size	
	(1)	(2)	(3)	(4)
External Dependence x Free Banking	-0.231	-1.039***	-0.0444*	0.223***
	(0.161)	(0.0902)	(0.0235)	(0.0845)
External Dependence x Banks pc		-9.842***		-0.959*
		(3.275)		(0.509)
External Dependence x Free Banking x Banks pc		16.51***		-4.845***
		(5.472)		(1.465)
Joint Effect: External Dependence x Free Banking + External Dependence x Free Banking x Banks pc				
Median # banks pc		-0.296***		0.004
		(0.097)		(0.235)
Average # banks pc		-0.0486		-0.067***
		(0.185)		(0.023)
Max # banks pc		1.602**		-0.552***
		(0.746)		(0.154)
Observations	250	250	250	250
R-squared	0.363	0.392	0.365	0.398
State FE	yes	yes	yes	yes
Industry FE	yes	yes	yes	yes
Industry Share	yes	yes	yes	yes

Notes: The dependent variable is the growth rate of value added in industry i and state s from 1850 to 1860. Value added is calculated as the difference between the aggregate value of outputs (OUTPUT_V) and aggregate value of inputs (INPUT_V). Free Banking is an indicator variable that takes the value one if a state s has a free banking law in 1850. External dependence is either the measure in Cetorelli and Strahan (2006) or the average establishment size, calculated as the average across states of the output in a given sector in 1850. Banks per capita is the number of banks per white population in state s in 1850. Industry share equals output in industry i and state s, divided by the total industry output in state s in 1850. Huber robust standard errors (shown in parentheses) are clustered at the state and industry level: *** p<0.01, ** p<0.05, * p<0.1.

TABLE 7

FREE BANKING AND ALLOCATIVE EFFICIENCY

SECTOR-LEVEL EVIDENCE -- DYNAMIC SPECIFICATION

Measure of External Dependence:	Cetorelli and Strahan (2006)		Avg establishment size	
	(1)	(2)	(3)	(4)
External Dependence x Free Banking	-0.324	-1.038***	-0.0662***	-0.0722
	(0.269)	(0.258)	(0.0251)	(0.0469)
External Dependence x Banks pc		-6.804*		-0.193
		(3.597)		(0.675)
External Dependence x Free Banking x Banks pc		11.74***		0.114
		(1.722)		(0.461)
Joint Effect: External Dependence x Free Banking + External Dependence x Free Banking x Banks pc				
Median # banks pc		-0.51**		-0.0671**
		(0.2378)		(0.029)
Average # banks pc		-0.334		-0.0654***
		(0.243)		(0.024)
Max # banks pc		0.841***		-0.053
		(0.291)		(0.035)
Observations	465	465	465	465
R-squared	0.396	0.410	0.403	0.403
State-by-Year FE	yes	yes	yes	yes
Industry FE	yes	yes	yes	yes
Industry Share	yes	yes	yes	yes

Notes: The dependent variable is the growth rate of value added in industry i, state s, and time t, with t indicating the periods 1850-60 and 1860-70. Value added is calculated as the difference between the aggregate value of outputs (OUTPUT_V) and aggregate value of inputs (INPUT_V). Free Banking is an indicator variable that takes the value one if a state s has a free banking law in 1850 or in 1860. External dependence is either the measure in Cetorelli and Strahan (2006) or the average establishment size, calculated as the average across states of the output in a given sector in 1850. Banks per capita is the number of banks per white population in state s and time t, with t indicating 1850 and 1860. Industry share equals output in industry i, state s, and time t, divided by the total industry output in state s at time t, with t equal to 1850 or 1860. Huber robust standard errors (shown in parentheses) are clustered at the state and industry level: *** p<0.01, ** p<0.05, * p<0.1.

Appendix Figure 1
Border County Sample