Bank Deregulation, Competition and Economic Growth: The US Free Banking Experience

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25 November 2013

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November 2013

Abstract: We exploit the introduction of free banking laws in US states during the 1837-1863 period to examine the impact of removing barriers to bank entry on bank competition and economic growth. As governments were not concerned about systemic stability in this period, we are able to isolate the effects of bank competition from those of state implicit guarantees. We find that the introduction of free banking laws stimulated the creation of new banks and led to more bank failures. Our empirical evidence indicates that states adopting free banking laws experienced an increase in output per capita compared to the states that retained state bank chartering policies. We argue that the fiercer bank competition following the introduction of free banking laws might have spurred economic growth by (1) increasing the money stock and the availability of credit; (2) leading to efficiency gains in the banking market. Our findings suggest that the more frequent bank failures occurring in a competitive banking market do not harm long-run economic growth in a system without public safety nets.

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

JEL: G18, G21, G28, N21

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I. Introduction

The optimal level of competition among banks is a long-standing question. It has been argued that bank competition promotes economic growth by improving efficiency in the banking market (see e.g. Jayaratne and Strahan, 1996, 1998). On the other hand, bank regulators are concerned about bank competition for its effects on the risk of bank failure. As bank failures might have huge social costs in terms of GDP growth, competition should be controlled in order to guarantee the stability of the financial system (see e.g Vives, 2010). As these two views have opposite predictions, the effect of bank competition on economic growth is ultimately an empirical question.

We shed new light on this question by testing how the introduction of free banking laws between 1837 and 1863 affected bank competition and economic growth in US counties. With the introduction of free banking laws, US states gave up their power over bank chartering and allowed any individual to establish a bank subject to a set of legally defined requirements.[1] Historical accounts suggest that free banking laws lifted barriers to bank entry, as a wave of bank entry followed their introduction (Hammond, 1957). Using the state-by-year variation in free banking laws, we are able to test three hypotheses on the effects of lifting barriers to bank entry. First, we examine whether the relaxation of barriers to bank entry affected bank competition and bank exit in US counties using Weber's (2006, 2011a) databases on antebellum bank balance sheets and census of state banks. Second, we merge these two databases with the decennial US Census data to investigate whether the relaxation of barriers to bank entry had an impact on local economic growth.[2] Third, we provide evidence on specific channels through which bank competition might have affected economic growth. In particular, we investigate whether the relaxation of barriers to bank entry spurred local financial development and led to dynamic efficiency gains in the banking market.

One concern with studies of bank competition and economic growth based on modern data is the confounding effect of state implicit guarantees. Regulators adverse to bank failures, for example, will likely prefer to bail-out an inefficient bank rather than letting it out of the market. As inefficient banks are able to continue operating, there will be presumably efficiency losses.[3] Disentangling these effects from those of

[1] Typical requirements were a minimum level of capital and bond-secured banknote circulation (see e.g. Rockoff, 1972; Hasan, 1987). See Section 2 for a more detailed description of the US banking regulation during the antebellum period.

[2] The US Census data are retrieved from the ICPSR file 2896 (Haines, 2010).

[3] Hoshi and Kashyap (2004) and Caballero, Hoshi, and Kashyap (2008), for example, document efficiency losses in the real sector due to the misallocation of credit by ailing banks that the regulator allowed to continue operating during the Japanese banking crisis in the 1990s.

competition might be difficult in studies based on modern data. Focusing on the US free banking period (1837-1863) allows us to circumvent this problem. As systemic stability was not among the objectives of bank regulation at that time (Mitchener and Jaremski, 2012), we are able to isolate the effects of bank competition from those of state-implicit guarantees.

Our main finding is that the introduction of free banking laws significantly affected the banking sector in US counties. Our county-level estimates show that the introduction of free banking laws caused a 35% rise in the share of new banks and a 137% increase in the fraction of failed banks, relative to the unconditional mean of these variables. There is no statistically significant effect on bank closures due to the introduction of free banking laws. Our interpretation of these findings is that the introduction of free banking laws relaxed the barriers to entry and allowed more banks to enter the market. Along with a higher degree of bank competition, the introduction of free banking laws caused more bank failures. Since these two effects have opposite implications for the real economy, we assess whether the introduction of free banking laws had an overall beneficial or detrimental effect on economic growth. Our empirical evidence suggests that there is a positive and statistically significant link between the relaxation of barriers to bank entry and economic growth during the 1830-1860 period. Our estimates indicate that counties in states that adopted free banking laws experienced a 20% increase in output per capita.

Since our empirical analysis exploits the state-by-year variation in the introduction of free banking laws, there could be a concern that omitted variables drive our results. For example, it might be that the states who introduced free banking laws were those with the most promising growth opportunities. To address this concern, we exploit the within-county variation of our panel dataset, which allows us to control for any time-invariant county characteristics, state-specific linear trends, and any region-by-year variation, such as business cycles and growth trends at the regional level. Another concern with our study is that we might erroneously consider our results as the effect of free banking laws rather than changes in other state-by-year varying laws that occurred at the same time. To address this concern, we collected data on a set of state laws that could affect bank competition, bank exit and economic growth. These state laws include usury laws, incorporation laws, limited liability laws, state-specific liability insurance systems and clearing arrangements. In our empirical analysis, we control for these laws in order to isolate the effect of free banking laws.

To further improve identification, we provide alternative estimation strategies that are tailored to deal with unobserved heterogeneity. We repeat our empirical analysis restricting the sample to contiguous coun-

ties lying on the border of states that passed a free banking law at different points in time.[4] The main advantage of this approach is that geographically close counties are more likely to be similar in terms of unobservables factors such as growth trends and local economic shocks. We also employ Altonji, Elder, and Taber (2005)'s method to assess how large the selection effect on unobservables compared to observables must be in order to explain away the estimated effect of free banking laws. As a further robustness check, we test for anticipation effects and whether the introduction of free banking laws was systematically related to any observable pre-existing county characteristics. We show that all our findings are robust to the different specifications and estimation techniques discussed above.

The final contribution of this paper is to examine through which channels the fiercer bank competition caused by free banking laws may have affected economic growth. The first channel we consider is financial development. Using the same estimation method as for the economic growth regressions, we find that counties in states that adopted free banking laws experienced a 62% increase in loans per capita and an expansion in the stock of money per capita by 149%, relative to the unconditional mean of these variables. Our findings suggest that the introduction of free banking laws facilitated bank entry, which led to an expansion in credit and money supply and allowed firms that were credit-constrained under the state-chartering regime to finance their investment opportunities.

The second channel we consider is dynamic efficiency. One may argue that the introduction of free banking laws enhanced bank competition, which drove the inefficient banks out of the market and allowed the efficient banks to grow faster. We test the dynamic efficiency hypothesis by performing a bank-level analysis on the sample of banks existing prior to the introduction of free banking laws (incumbent banks). We first provide evidence that incumbent banks became more efficient after the introduction of free banking laws, as their probability of closure decreased by 39% of its unconditional mean, while their probability of failure remained unaffected. We then show that the introduction of free banking laws led to a reallocation in the banking market. The banks who closed during our sample period grew 9.8 percentage points less, those who failed grew 2.7 percentage points more, while those who survived our sample period remained unaffected. We argue that the estimated redistribution effect improved the efficiency of the banking system, as the banks who closed were presumably the inefficient ones and those who failed were likely those lending

[4]Huang (2008) proposed a similar strategy that uses contiguous county-pairs separated by state borders to investigate the local economic effects of relaxing bank-branching restrictions in the US between 1975 and 1990. Danisewicz, McGowan, Onali, and Schaek (2013) uses a similar regression discontinuity design to examine the effects of regulatory enforcement actions on local economic growth.

more or financing high risk-return investment opportunities.

Our findings have implications for the literature on bank competition and financial stability, the finance-growth nexus, and the US financial history literature. Our results on the structure of the banking market provide new evidence in support of the "competition-instability" view, which postulates that fiercer competition makes banks more likely to go bankrupt (see Marcus, 1984; Chan, Greenbaum, and Thakor, 1986; Keeley, 1990, for example). This view is widely accepted among bank regulators, even though other scholars have raised doubts,[5] and empirical tests based on contemporary data have not found conclusive evidence.[6] Focusing on a period where state implicit guarantees were absent, we are able to rule out the confounding effect of bank moral hazard on risk taking and isolate the effect of bank competition.

Our empirical evidence on economic growth is in line with the finance-growth nexus literature, which argues that finance leads growth (see e.g. King and Levine, 1993; Rajan and Zingales, 1998; Rousseau and Sylla, 2005).[7] In an exercise similar to ours, Jayaratne and Strahan (1996) find that the relaxation of branching restrictions in the US between 1978 and 1992 had a positive effect on economic growth. Jayaratne and Strahan (1998), DeYoung, Hasan, and Kirchhoff (1998) and Evanoff and Ors (2002) document dynamic efficiency gains following the relaxation of branching restrictions between 1978 and 1992, while Carlson and Mitchener (2009) find similar results examining bank branching in California during the 1920s and 1930s. We confirm these findings using the state-by-year variation in the introduction of free banking laws in the US during the 1837-1863 period.

Our results suggest that the more frequent bank failures caused by the introduction of free banking laws did not harm economic growth, at least over the 1830-1860 period that we are considering. It is important to stress that we are not making any claims on the short-run impact of bank failures on output growth, which might be as well significant. We rather interpret our findings along the lines of Rancière, Tornell, and Westermann (2008), who show that countries experiencing occasional financial crises have an average growth rate higher than countries with a stable financial system. The authors argue that credit-constrained firms become able to raise funds and invest after countries liberalize their financial sector. As a result, the

[5]The competition-stability view developed by Boyd and DeNicolo (2005) suggests that banks with greater market power charge higher interest rates which in turn induces borrower to choose riskier projects. Martinez-Miera and Repullo (2010) show there is a U-shaped relationship between competition and banks' default risk.

[6]Examples of recent empirical studies are Berger, Klapper, and Turk-Ariss (2009) and Anginer, Demirguc-Kunt, and Zhu (2012). See also the surveys of Beck (2008) and Vives (2010) and the references therein for further information on the link between competition and financial stability.

[7]For further information on the finance-growth nexus we refer to the excellent surveys of Levine (2005) and Papaioannou (2008) and the references therein.

economic activity increases, but credit risk does as well. Besides our county-level evidence, this narrative might also explain why the incumbent banks who failed during our sample period grew faster following the introduction of free banking laws. It might be that banks' default was the consequence of increased competition, which pushed these banks to expand their exposure to new or previously credit-constrained firms.

Our results also provide new insights on the US free banking literature. Economic historians have argued that free banking laws facilitated bank entry (e.g. Hammond, 1957), but the empirical literature provides conflicting evidence (see Ng, 1988; Bodenhorn, 1990, 1993, 2008; Economopoulos and O'Neill, 1995). We document that free banking led to more bank entry employing alternative identification strategies and using a more comprehensive data set. There is a large literature on the causes of the instability of free banks, i.e. the banks chartered under free banking laws (see Rolnick and Weber, 1984, 1985; Economopoulos, 1990; Jaremski, 2010, for example). In contrast to these studies, we do not investigate the sources of instability of free banks per se, but we examine how the introduction of free banking laws affected bank failures, both at the county and bank-level. To the best of our knowledge, our study is the first investigating the effect of free banking laws on bank closures in the US, both at the county and bank-level.

There is a large literature on the history of financial development and economy growth in the US (see e.g. Sylla, 1999; Bodenhorn, 2000; Rousseau, 2003), but only a few papers investigating the role of finance in local economic growth during the free banking era. Jaremski and Rousseau (2012) study the relationship between banks and economic growth in the US at the county level, whereas Bodenhorn and Cuberes (2010) examine the effect of banking activity on city growth in the Northeastern United States between 1790 and 1870. In contrast to those papers, we do not analyze the direct growth effect of banks per se, but we assess whether counties in the states that adopted free banking laws grew more than their counterparts in states that retained state-chartering policies. We believe this is an hypothesis worth studying as the positive effects of free banking might operate not only through the size of the banking activity, but also at a more aggregate level through the efficiency gains arising in a more competitive banking market.

The remainder of our paper is organized as follows. Section 2 provides an overview of the free banking era tailored to the purpose of our study. Section 3 describes the data. In Section 4, we study the effect of free banking laws on bank competition and bank exit. Section 5 presents the results on the link between free banking laws and local economic growth. Section 6 presents the results on the channels. Section 7 provides further robustness checks to address potential threats to our identification strategy. Section 8 concludes.

II. The Free Banking Era (1837-1863)

Our study investigates the effect of removing barriers to bank entry on bank competition and economic growth during the US free banking era. In this section, we explain why focusing on this period is appropriate for the purpose of our study. We also identify some potential concerns and discuss how we are able to address them. We consider three aspects of the free banking period that are particularly relevant for our study: Bank chartering policies, bank regulation, and the finance-growth nexus.

A. Bank Chartering Policies in the US between 1837 and 1863

Bank chartering was one of the main aspects of bank regulation in the US between 1837 and 1863.[8] Crucially for the purpose of our study, bank chartering policies were decided at the state rather than federal-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.[9] 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, the regulator set the requirements the bank had to satisfy. Requirements differed from bank to bank, but generally consisted of an initial capital level and constraints on the allocation of funds.[10] 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 incumbents (see e.g. Bodenhorn, 2006, 2008).

Starting from Michigan in 1837, New York and Georgia in 1838, 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 hold a minimum amount of capital and back their banknote circulation with government bonds or mortgages (see e.g. Rockoff, 1972; Hasan, 1987; Jaremski, 2010).

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 in granting bank charters. The decision to open a bank became an administrative rather than a political process, as the aspiring banker needed only

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

[9] Few states had general banking laws. Banking laws usually defined managers and shareholders liability and tied banknote circulation to bank capital or specie. In no state the law allowed individuals to open a bank without a charter; see Dewey (1910), Knox (1903), and Hendrickson (2011) for banking regulation in the 19th century US.

[10] 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).

to fulfill some legal requirements (see Bodenhorn, 2004). Hence, the introduction of free banking laws could have fostered competition in two ways. First, it gave any individual the opportunity to open up a bank in profitable markets where the incumbent enjoyed monopoly rents. Second, it incentivized incumbent (state chartered) banks to act in a more competitive manner in order to prevent bank entry.

One concern with our study is that the introduction of free banking laws might not have been a random event. Bodenhorn (2004) examines the reform process that led to the introduction of free banking laws in New York. He argues that the reform impulse arose from the dissatisfaction with the existing banking system, which was perceived as corrupt and inadequate to satisfy the increased demand for credit. Bodenhorn also maintains that a random component strongly influenced the introduction of the 1838 Free Banking Act, as the demand for credit in New York was likely to be as high as in other states. The author points out 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.[11]

As we are not aware of other case studies similar to Bodenhorn's, we discuss the concerns on the endogeneity of free banking laws taking New York as a benchmark. Even though Bodenhorn remarks a random component in the introduction of free banking laws, his analysis suggests that the relaxation of the barriers to bank entry could be driven by a higher demand for credit. Hence, the higher growth rate, the creation of new banks and the more frequent bank failures would not be the effect of free banking laws, but the consequences of growing business opportunities. We will address this threat to identification in sections IV, V and VII.

B. Bank Regulation in the US between 1837 and 1863

During the free banking period, US states were not concerned about systemic stability (see Mitchener and Jaremski, 2012). This is a notable difference from modern financial systems, where banks can count on the implicit guarantee of being rescued in case of default. Hence, the incentive of banks to take excessive risk was presumably lower than nowadays, since banks were "free" to go bankrupt and exit the market. For these reasons, we believe that the US free banking period is an ideal setup to study how the relaxation of bank entry barriers affects bank competition and bank exit.

[11] Bodenhorn claims 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 against the dominant class.

Even though systemic stability was not among the objectives of bank regulators, the fact that banks in some states set up private cooperative arrangements might be a concern for our study. These arrangements represented a form of coinsurance among banks in times of financial distress. Hence, bank failures might be the result of excessive risk taking fueled by coinsurance rather than bank competition.[12] 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 Gorton and Huang (2002)'s analysis of the origins of central banking, which is based on the assumption that the regulator is worse than banks at monitoring.[13] 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 participate to a private cooperative arrangement only if they complied with a set of rules, which aimed at limiting moral hazard. Third, Calomiris and Kahn (1996) also maintain that a bank could be expelled from a private cooperative arrangement if its behavior represented a threat for 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 and allowed to continue their business. Nonetheless, in the empirical analysis we control for the existence of private cooperative arrangements.[14]

A further concern is that states introduced other regulatory measures at the same time as free banking laws. As long as these measures could influence bank competition and economic growth, our estimates of the effect of free banking laws would be biased. There are a number of laws and institutional arrangements that could represent a threat to our identification strategy. These include:

(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.[15] If states that introduced free banking laws were also raising

[12]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, which was an insurance fund for New York banks. Banks in Indiana, Iowa, Michigan, and Vermont also set up forms of cooperative arrangements (see e.g. Weber, 2011b; Klebaner, 2005).

[13]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 cost 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.

[14]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 decided later on to base membership on a voluntarily basis. We refer to Weber (2011b) for more details about the antebellum liability insurance systems.

[15]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).

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 US South was more resilient than in the rest of the US, 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 the states who retained the state bank chartering policies were those where bank branching was allowed. To address this concern, we use the information in Weber (2011a)'s database 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' investment 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 the information in Weber (2011a)'s database to construct an indicator variable which equals one in states and years in which a 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 contained in Jalil (2012).

C. The Finance-Growth Nexus in the US between 1837 and 1863

Economic historians have argued that the rapid growth over the 1837-1863 period was finance-led (see Rousseau and Sylla, 2005). Sylla (1999) claims that the "Federalist Financial Revolution" promoted by the Secretary 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 also recognized a specific role of banks in the economic development of the antebellum US. For example, Bodenhorn (2000) emphasizes the importance of US banks in the provision of means of payments, in the accumulation of savings and in their efficient allocation. Bodenhorn (1999) demonstrates that banks encouraged invention and financed industrial development. What emerges is a picture of banks performing what theory considers to be their main functions. Hence, we believe that the US free banking period provides an excellent opportunity to analyze the link between bank competition and economic growth following the relaxation of barriers to bank entry.

Some typical banking practices in the free banking period might cast doubts on our study. First, it might be that bank failures were due to mismanagement rather than economic fundamentals. Descriptive statistics reveal that US antebellum banks financed roughly half of their (non risk-weighted) assets through capital, while nowadays banks hold an amount of capital lower than 10% of their assets (Hanson, Kashyap, and Stein, 2011). Moreover, the common practice in the antebellum period was to lend short-term, against safe collateral, and, at least in the Northeast, mainly to bank insiders (see Lamoraux, 1994). Nonetheless,

we believe that bank failures were too frequent to be attributable only to mismanagement and that banks were actually exposed to risks. On the liability side, banks bore the risk of sudden withdrawals of funds by their investors. The reason is that a consistent source of funds for US antebellum banks was represented by banknotes, which entitled the holder to demand redemption in specie at any time (Gorton, 1999). On the asset side, the practice of lending mainly to insiders or to local firms could have exposed banks to a high default risk because of the lack of diversification.

Second, the personal nature of local credit markets in the US might raise concerns on the growth enhancing role of banks during the free banking era. If loans were mainly granted to bank insiders, bank entry might not have relaxed firms' credit constraints. Lamoreaux and Glaisek (1993), however, find evidence that banks were promoting economic growth by providing new firms the funds to finance their investment opportunities. This evidence also provides support to our interpretation of the finding that incumbent banks who failed were those growing more after the introduction of free banking laws.

III. Data

This section discusses the data we use to examine how the introduction of free banking laws affected bank competition and economic growth at the county-level. The focus of our empirical analysis is on the period from 1830 to 1860.[16] We choose 1830 as starting year, that is seven years before the first state (Michigan) introduced a free banking system, in order to have a sufficiently large pre-treatment window to implement a difference in differences (DID) estimation. Choosing 1830 as starting point, we also avoid data availability problems of earlier years.[17] Our empirical 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.

As customary in the banking literature, we consider counties as the unit of analysis. Scholars have often used county-level data to study the impact of bank activities on economic outcomes (e.g Ashcraft, 2005; Calomiris and Mason, 2003; Gilber and Kochin, 1998) and to analyze local banking markets (e.g. Berger, Klapper, and Turk-Ariss, 2009; Black and Strahan, 2002; Huang, 2008). In the historical context, Bodenhorn (2008) argues that banking in the free banking era was generally a local affair, both in legal and economic

[16] We exclude Washington D.C. from our sample, since it was a federal district.

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

terms. He uses county-level data to study bank entry in the nineteenth century New York, Massachusetts and Pennsylvania. Following Bodenhorn, we take the county as the appropriate unit of analysis to study the effects of free banking laws on bank competition and real sector's growth.

Our analysis builds on Warren Weber's (2006, 2011a) collection of bank balance sheets and census of state banks during the antebellum US. The census of state banks contains the location, name, the beginning and ending dates of all banks that existed in the US 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 still existed in 1861. The balance sheets, that antebellum banks had to report to the US states' banking authorities, provide detailed information on banks' assets and liabilities. We merge the census of state banks with the bank balance sheet data, match the location of each bank to its corresponding county, and then construct county aggregates. We use also Weber's data set at the bank level to study the dynamic efficiency gains in the banking market following the introduction of free banking laws (see Section VI.B).

For the real sector analysis we combine our banking dataset with the Inter-University Consortium for Political and Social Research (ICPSR) 2896 data file (Haines, 2010). The ICPSR 2896 file contains detailed decennial county-level data on demographic, economic, and social variables which were collected by the US Bureau of the Census for the period 1790-2000.[18] We use the Census data for the decades from 1830 to 1860 to investigate the link between free banking and economic growth in US counties.

Table 2 shows the summary statistics of the variables that we use in our empirical analysis. We provide a detailed description of these variables in the Appendix.

[18] See http://www.icpsr.umich.edu/icpsrweb/ICPSR/studies/02896 for more details.

IV. Free Banking, Bank Competition and Bank Exit

A. Estimation Strategy

In this section we use a difference-in-differences approach (DID) to estimate how the introduction of free banking laws affected bank competition and bank exit at the county level. Our measure of bank competition is the share of new banks in county c at time t. The share of failed banks and the share of closed banks in county c at time t are our measures of bank exit. Our estimation equation takes the following form:

$$y_{c,t} = \lambda_c + \lambda_s t + \lambda_{r,t} + \beta FB_{s,t} + \Gamma X_{c,t-1} + \Theta LEG_{s,t} + \Psi t Z_{c,1830} + \Phi t^2 Z_{c,1830} + \epsilon_{c,t} \quad (1)$$

Our variable of interest is the free banking indicator, $FB_{s,t}$, that takes the value one for all the years t since state s introduced a free banking law. The county fixed effects, λ_c, capture any county specific time-invariant factors, such as, geography and historical factors. The region-by-year fixed effects, $\lambda_{r,t}$, control for any variation between US Census regions over time, such as, regional business cycles and growth trends,[19] and $\lambda_s t$ captures a linear state specific time trend.

We include a set of lagged banking sector control, $X_{c,t-1}$, such as, the number of banks and the average age of banks to control for the size and average experience of counties' banking sector. We also add a set of lagged bank balance sheet ratios, averaged at the county level, to the bank exit estimating equation. This set includes the capital to assets, deposits to assets, circulation to assets, loans to assets, cash to assets, and public bonds to assets ratios. These ratios control for banks' asset and liability structure, which could represent another source of risk besides the increased competition due to the introduction of free banking laws.[20]

We further add the set of state-by-year varying legislation variables, $LEG_{s,t}$, that we have described in Section II, to make sure that our results are not driven by other laws than free banking. Following Bodenhorn (2008), we also include a set of initial county observables, $Z_{c,1830}$, interacted with a linear and squared time trend. This set of observable variables include counties' population size, the urbanization rate, the manufacturing share, the commercial share, the number of banks per capita, and bank capital per capita.

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

[20] We use the lag for the control variables, $X_{c,t-1}$, in order to mitigate the reverse causality concern.

We add these covariates, $Z_{c,1830}$, in order to mitigate the concern that the timing of states' decision to adopt free banking laws could be systematically related to county-level outcomes in 1830 (see also Section VII.A).

As a benchmark, we estimate equation (1) by least squares and, as a robustness check, by a heteroskedastic fractional probit model (see Wooldridge, 2010). We cluster the error term, $\epsilon_{csr,t}$, at the state level to ensure that the standard errors of our estimates are robust to arbitrary correlation across counties in each US state. In Section VII we address further threats to identification, such as, pre-free banking differences in the sample and anticipation effects, and we provide alternative estimation strategies to deal with unobserved heterogeneity.

B. Results: Free Banking and Bank Competition

Table 3 shows the within-county level response of the share of new banks to the introduction of free banking laws using estimating equation (1). The method of estimation is least squares. Column (1) reports the results from regressing the share of new banks on the free banking indicator variable after controlling for county fixed effects, region-by-year fixed effects and a linear state specific time trend. 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 is associated with an increase in the share of new banks by around 3.5 percentage points.

In column (2) of Table 3 we add the lagged number of banks and the lagged average age of banks to the estimating equation. The coefficient on the free banking indicator remains positive and statistically significant at the 1-percent level. The point estimate implies that, after controlling for the size and average experience of county c's banking sector, introducing free banking is associated with an increase in the share of new banks by around 4 percentage points. In column (3), we include the legislation measures discussed in Section 2 as additional controls. The coefficient on the free banking indicator variable remains positive and statistically significant at the 1-percent level. This result is quite reassuring, since it confirms that our positive effect is not driven by changes in other state laws that could also potentially affect bank entry. The result in column (4) shows that there is still a positive effect of free banking laws on bank entry that is statistically significant at the 1-percent level after controlling for a set of initial county observables (see Section IV.A for details). The point estimate implies that the introduction of free banking laws is associated with an increase in the rate of bank entry by around 3.25 percentage points.

Since the share of new banks is a fractional response variable, which is bounded between zero and one by definition, our least square estimates provide only an approximation. It might well be that the predicted share of new banks is outside the unit interval. To address this problem we follow the suggestion of Wooldridge (2010) and estimate a heteroskedastic fractional probit model adjusted to deal with unobserved heterogeneity in an unbalanced panel.[21] Appendix Table 1 shows the results. The point estimates are statistically significant at the conventional levels and quantitatively larger than the least square estimates. Our estimates indicate that the share of new banks increases by up to 8 percentage points following the introduction of free banking laws.

Our results on bank entry are in line with the empirical evidence in Economopoulos and O'Neill (1995) and Bodenhorn (2008), who suggest that free banking laws led to more bank entry. Since the mean share of new banks for the entire sample is 9.38%, our least squares estimates represent an increase ranging between 34.75% and 42.8% relative the unconditional mean. Hence, we find evidence of an economically significant impact of free banking laws on bank competition.

C. Results: Free Banking and Bank Exit

Table 4 reports the effect of introducing free banking laws on bank exit using estimating equation (1). The method of estimation is least squares. Columns (1)-(4) contain the results on the share of failed banks, while columns (5)-(8) show the estimates for the share of closed banks. Column (1) shows the results from regressing the share of failed banks on the free banking indicator variable after controlling for county fixed effects, region-by-year fixed effects and a linear state specific time trend. The result is a positive coefficient on the free banking indicator variable that is statistically significant at the 1-percent level. The point estimate implies that introducing free banking is associated with an increase in the share of failed banks by more than 2 percentage points.

In column (2) of Table 4 we add to our estimating equation the lagged number of banks, the lagged average age of banks, and the lagged balance sheet ratios mentioned in Section IV.A.[22] The coefficient on the free banking indicator increases slightly and remains statistically significant at the 1-percent level. The point estimate implies that, after adding the set of lagged banking sector controls, introducing free banking

[21] We describe the estimation approach for the heteroskedastic fractional probit model in Appendix Table 1; for more details see Wooldridge (2010).

[22] Note, that there is a drop of observations, because we do not have information on the balance sheet controls available for all counties in the sample. This drop in observations does not change our result, qualitatively.

laws is associated with an increase in the share of failed banks by around 2.5 percentage points. In column (3), we include the legislation measures discussed in Section 2 as additional controls. We still find a positive coefficient on the free banking indicator variable, which is statistically significant at the 5 percent level. This confirms that our results are not driven by changes in other state laws that could also potentially affect bank failures. Column (4) presents the results after controlling for the set of initial county observables mentioned in Section IV.A. The effect of free banking laws on the share of failed banks remains positive and statistically significant at the 5-percent level. The point estimate implies that the introduction of free banking laws is associated with an increase in the share of failed banks by up to 3 percentage points.

Appendix Table 2 presents the results for the share of failed banks using, as in Section IV.B, a heteroskedatic fractional probit model. The estimated coefficient on the free banking indicator variable is positive and statistically significant at the conventional levels in three out of four specifications. The point estimates are quantitatively a bit larger compared to the least squares estimates and indicate that the share of failed banks increased up to 3.5 percentage points after states decided to introduce free banking laws.

Overall, we provide evidence that counties in states that introduced free banking laws experienced more bank failures. The estimated effect is quite strong, as our least squares estimates correspond to an increase up to 155.5% relative to the unconditional mean. As the absence of state-implicit guarantees allows us to rule out the confounding effect of moral hazard on risk taking, this finding provides new evidence in support of the competition-instability view (see e.g. Marcus, 1984; Chan, Greenbaum, and Thakor, 1986; Keeley, 1990), which claims that a more competitive banking sector leads to a higher probability of bank failure. As long as these additional banks failing were free banks, our finding is also consistent with the strand of literature showing that free banks had a significantly higher probability of failure than state-chartered banks (e.g. Rolnick and Weber, 1984, 1985; Economopoulos, 1990; Jaremski, 2010).

Columns (5)-(8) report the results of the same exercise as in columns (1)-(4), but using the share of closed banks as dependent variable. We do not find any robust and statistically significant association between the introduction of free banking laws and the share of bank closures. Even if there is no statistically significant effect at the county level, the type of banks that decided to close their business after states introduced free banking laws could differ fundamentally. We investigate this hypothesis in Section VI.B, where we use bank-level data to test whether the introduction of free banking laws affected the probability of closure of incumbent banks.

Our evidence on bank competition and bank failures raises the question whether the introduction of free banking laws yielded a net benefit for the real sector. The entry of new banks in a market has presumably beneficial effects for the real sector, as the credit supply increases and firms have stronger incentives to undertake investment opportunities. On the other hand, bank failures could lead to sharp contractions in the credit supply, which negatively affects output as firms might become unable to access external finance. To investigate which of these two effects dominate, we investigate whether free banking laws spurred economic growth over the decades 1830 to 1860.

V. Free Banking and Economic Growth

A. Estimation Strategy

In this section we investigate the link between the introduction of free banking laws and counties' real sector performance over the decades 1830 to 1860. Our preferred county-level measure of real sector performance is output per capita in logarithmic units. In order to shed light on the changes in the sectoral composition of output and in capital accumulation following the introduction of free banking laws, we also examine counties' agricultural and manufacturing output and manufacturing capital. More formally, we regress our outcome variable in county c at time t, $y_{c,t}$, on a free banking indicator variable, $FB_{s,t-1}$, that equals one if state s had introduced free banking laws in $t-1$.

Regressing output per capita, $y_{c,t}$, on a lagged free banking indicator, $FB_{s,t-1}$, is well suited to circumvent potential reverse causality concerns that higher output per capita levels could lead to the introduction of free banking laws. Hence, we are able to capture the existence of a lagged effect between the introduction of free banking laws and its impact on real sector performance. The estimating equation takes the following form:

$$y_{c,t} = \lambda_c + \lambda_s t + \lambda_t + \beta FB_{s,t-1} + \Gamma X_{c,t-1} + \Theta LEG_{s,t-1} + \epsilon_{c,t} \tag{2}$$

Our method of estimation is least squares. The county fixed effects, λ_c, account for counties' long-run steady state characteristics. The year fixed effects, λ_t wipe out any nationwide time varying unobserved

heterogeneity, such as national business cycles, that could otherwise confound our findings. The linear state specific time trend, $\lambda_s t$, allows for linear unobserved heterogeneity across states in the time trend of output per capita. We add a set of control variables, $X_{c,t-1}$, such as counties' urbanization rate, population size, the manufacturing share, the agricultural share, the share of blacks, the labor force participation rate, the number of banks and assets per capita to account for cross-county differences in $t-1$. In order to rule out the confounding effect of other state laws, we also control for the state-by-year varying legislation variables, $LEG_{s,t-1}$, discussed in Section II. We cluster the error term, $\epsilon_{csr,t}$, at the state level to ensure that the computed standard errors of our estimates are robust to arbitrary correlation across counties in each US state.

As an alternative model specification, we estimate equation (2) in first differences. After differencing out time-invariant heterogeneity across counties (λ_c), controlling for a linear time trend of output per capita across states, $\alpha_s t$, and year fixed effects, λ_t, in the level-form representation, our first-difference estimation equation takes the following form:

$$\Delta y_{c,t} = \alpha_s + \Delta\lambda_t + \beta \Delta FB_{s,t-1} + \Gamma \Delta X_{c,t-1} + \Theta \Delta LEG_{s,t-1} + \Delta\epsilon_{c,t} \tag{3}$$

In equation (3), the β coefficient represents the effect of the introduction of free banking laws between $t-2$ and $t-1$ on output growth between $t-1$ and t. Our identification strategy relies on the assumption that changes in the explanatory variables ($FB_{s,t-1}$, $X_{c,t-1}$, $LEG_{s,t-1}$) are not correlated with changes in the error term. The error term, $\Delta\epsilon_{c,t}$, is clustered at the state level to ensure that the computed standard errors of our estimates are robust to arbitrary correlation across counties in each US state. Estimating equation (2) in first differences has the advantage that we are able to add county fixed effects, α_c, to our first-difference estimating equation, which account for a county specific linear trend in the level of output per capita.[23]

As further robustness checks, we show in Section VII that the timing of free banking is not systematically related to initial county-level outcomes in 1830, and we provide alternative estimation strategies to deal with the concern of unobserved heterogeneity.

[23]Note that the time-invariant county fixed effects of equation (2), λ_c, drop out by taking first differences.

B. *Results*

Table 5 contains our results on the link between introducing a free banking system in $t-1$ and output per capita in t. Our estimating equation is based on (2) for columns (1)-(3), and on (3) for columns (4)-(6). The method of estimation is least squares. Column (1) reports the results from regressing the log of output per capita in t on a free banking indicator in $t-1$ after controlling for county fixed effects, year fixed effects, a state-specific linear time trend and the set of control variables, $X_{c,t-1}$, mentioned in Section V.A. The link between the free banking indicator and output per capita is positive and statistically significant at the 1-percent level. The point estimate implies that states that introduced free banking laws in $t-1$ experienced in t an increase in output per capita by around 14 percent.

In column (2) of Table 5, we add the set of state-by-year varying legislation variables, $LEG_{s,t-1}$, to our estimating equation (see Section II for details). The link between free banking laws and output per capita remains positive and statistically significant at the 5-percent level. This result indicates that free banking laws have a growth-enhancing effect which goes beyond the effect of other state laws that could influence output per capita. Column (3) shows the results when we include region-by-year fixed effects, $\lambda_{r,t}$, to our estimating equation. The coefficient on the free banking variable increases slightly and remains statistically significant at the 1-percent level. The point estimate implies that states that had a free banking system in $t-1$ experienced an increase in output per capita in t by more than 25 percent.

Columns (4) and (5) report the first difference estimates of the level-form specification used in columns (1) and (2). The link between the lagged change in free banking, $\Delta FB_{s,t-1}$, and changes in output per capita, $\Delta y_{c,t}$, is positive and statistically significant at the 5-percent level. The point estimate in column (4) implies that counties' output growth would increase by roughly 14 percent if a state introduced free banking laws in the previous decade. When we add the changes in other state laws, $\Delta LEG_{s,t-1}$, in column (5) the size of the estimated coefficient on free banking increases slightly, and remains statistically significant at the 5-percent level.

Column (6) presents our preferred specification, where we add region-by-year effects, $\Delta\lambda_{r,t}$, and county fixed effects, α_c to estimating equation (3). The estimated coefficient on free banking increases not only in its size, but also becomes statistically significant at the 1-percent level. The point estimate implies that counties' output growth would increase up to 22 percent if a state introduced free banking laws in the previous decade. Put it differently, the yearly output growth during the 1830-1860 period would be about 2

percent higher following the introduction of free banking.[24]

Table 6 presents the results on the sectoral composition of output and on capital accumulation in the manufacturing sector. We use the same specifications as in Table 5. The method of estimation is least squares. Panel A of Table 6 presents the results on manufacturing output per capita. The coefficient on the free banking indicator is positive and statistically significant at the 1-percent level in four out of six specifications. The point estimates imply that counties' manufacturing output per capita increased between 33 and 52 percent in the states that had introduced free banking laws in the previous decade. Panel B of Table 6 reports the results on agricultural output per capita. The coefficient on the free banking indicator is positive and statistically significant at the conventional levels in four out of six specifications. The point estimates imply that counties' agricultural output per capita increased by around 20 percent in the states that had introduced free banking laws in the previous decade. Panel C of Table 6 presents the results on manufacturing capital per capita. The link between free banking laws and manufacturing capital is positive and statistically significant at the 1-percent level. The point estimates imply that counties' manufacturing capital increased between 23 and 32 percent in states that had introduced free banking laws in the previous decade.

Overall, our empirical evidence suggests that the introduction of free banking laws may have spurred economic growth over the decades 1830 to 1860. Broadly speaking, our results are in line with the large literature on the finance-growth nexus that has argued that finance led growth (e.g. King and Levine, 1993; Rajan and Zingales, 1998; Jayaratne and Strahan, 1996). Our findings resonate with Rousseau and Sylla (2005)'s view that finance led growth in the US before the American Civil War, and suggest that the introduction of free banking laws may have significantly contributed to the rapid industrialization process over the course of that period.[25]

Our findings indicate that the increased bank competition following the introduction of free banking laws had overall beneficial effects in spite of the more frequent bank failures during the 1830 to 1860 period. We interpret this evidence along the lines of Rancière, Tornell, and Westermann (2008), who show that countries experiencing occasional financial crises grow more in the long run. We point out that our results do not allow

[24] We note that our results are qualitatively in line with Jayaratne and Strahan (1996), who find annual growth rates to increase by 0.51 to 1.19 percentage points following intrastate branch deregulation in the US during the late 20th century.

[25] We note that our objective is to analyze the economic consequences of free banking laws rather than investigating the effect of (free) banks on economic growth per se. Our results are therefore not at odds with Jaremski and Rousseau (2012)'s county-level study on the link between banks and economic growth during the free banking era.

us to conclude that the costs of bank failures in terms of output growth were negligible, but they suggest that bank competition might be beneficial in the long run. In the next section, we examine two channels through which this beneficial effect might have operated: Financial development, and dynamic efficiency gains in the banking market.

VI. Channels

A. Financial Development

In this subsection, we investigate the hypothesis that free banking laws spurred economic growth by enhancing financial development. To measure financial development in the US over the decades 1830 to 1860, we use loans per capita, as suggested by Bodenhorn (2000), and the real money stock per capita, as suggested by Rousseau and Sylla (2005). We use the same right-hand-side variables as in the previous section, but further add lagged output per capita to the set of control variables, $X_{c,t-1}$. The estimating equation is based on (2) in columns (1)-(3) , and on (3) in columns (4)-(6). The method of estimation is least squares.

Table 7 contains our county-level results on the link between free banking laws in $t-1$ and financial development in t. Panel A of Table 7 presents the results on loans per capita. The estimated coefficient on free banking is positive and at least statistically significant at the 5-percent level. The point estimates imply that loans per capita increased by around 4 percentage points following the introduction of free banking laws in the previous decade. This corresponds to a 61.7% increase relative to the mean loans per capita for the entire sample. In Panel B of Table 7 we report the estimates of the link between the introduction of free banking laws and the real money stock per capita. The coefficient on the free banking indicator is positive and statistically significant at the 1-percent level in five out of six specifications. The point estimates imply that county c's money stock increases by 2 to 6 percentage points following the introduction of free banking laws in the previous decade. These effects represent a 50% to 150% increase relative to the mean money stock per capita for the entire sample.

Our empirical evidence suggests that the growth-enhancing effect of introducing free banking laws may have worked through the financial development channel. The fiercer bank competition after states introduced free banking laws might have led banks to expand their supply of loans and money, which allowed new or previously credit-constrained firms to finance their investment opportunities.

B. *Dynamic Efficiency*

In this subsection we examine whether the introduction of free banking laws led to efficiency gains in the banking market. In particular, we investigate how the relative performance of banks changed following the introduction of free banking laws. To address this question, we need to focus on individual banks that existed before the introduction of free banking laws (i.e. incumbent banks), and rank them according to a measure of efficiency. In the remainder of this section, we first put forward our estimation strategy and then show the results.

B.1 Hypothesis and Estimation Strategy

We test for efficiency gains in the banking market following two steps. As a first step, we examine how the introduction of free banking laws affected the probability of failure and closure of incumbent banks. 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 of these two effects dominate, we estimate the following model:

$$y_{i,t} = \alpha_i + \lambda_t + \beta FB_{s,t} + \Gamma X_{i,t-1} + \Theta Banks_{c,t-1} + \epsilon_{i,t} \tag{4}$$

The dependent variables, $y_{i,t}$, are two dummy variables: One dummy variable captures whether an incumbent bank failed, the other whether an incumbent bank closed at time t. Our variable of interest is the free banking indicator variable, $FB_{s,t}$. We control for bank time-invariant characteristics, α_i, lagged bank-level balance sheet ratios and banks' age, $X_{i,t-1}$, the number of banks in each county, $Banks_{c,t-1}$, and include year fixed effects, λ_t. We restrict the sample to incumbent banks, which we define as the banks existing at least four years before the introduction of free banking laws.[26] For the states that never introduced free banking laws, we define as incumbents all the banks that existed during our sample period, as the level of competition in

[26] We also check the robustness of our results to other definitions of incumbent banks. In particular, we consider as incumbents banks that existed three and five years before the introduction of free banking laws. These alternative definitions of incumbents do not significantly change our results.

those banking markets was likely to be the same at any point in time.

As a second step, we examine the effect of free banking laws on the relative performance of incumbent banks. Ideally, we would like to perform an exercise in the spirit of Jayaratne and Strahan (1998) and rank incumbent banks on the basis of an efficiency measure. Unfortunately, we cannot use the same measures as Jayaratne and Strahan (1998), because of data limitations.[27] Our choice is to rank incumbent banks according to whether they failed, closed, or survived during our sample period.

During the free banking period, banks closed down their business either because their charter was repealed by the state, because of the violation of some state banking laws, or a voluntary choice by their shareholders (see e.g. Knox, 1903; Dewey, 1910). We believe these three cases are signals of banks' inefficiency and low profitability, which might have led shareholders not to comply with state laws in order to gamble for resurrection, or to take the decision to interrupt banks' activities. By contrast, bank failures did not necessarily occur because of inefficiency. Most likely, they were caused by an increase in risk taking together with lack of diversification or a negative systemic shock. Hence, we formulate the following hypothesis:

(a) The introduction of free banking laws led incumbent banks that closed during our sample period to grow less than their counterparts.

(b) The introduction of free banking laws led incumbent banks that failed during our sample period to grow more than their counterparts.

We test the relative performance of incumbent banks using the following specification:

$$y_{i,t} = \alpha_i + \lambda_t + \beta_1 FB_{s,t} + \beta_2 FB_{s,t} \times Failed_i + \beta_3 FB_{s,t} \times Closed_i + \Gamma X_{i,t-1} + \Theta Banks_{c,t-1} + \epsilon_{i,t} \quad (5)$$

We consider the yearly growth rate of incumbents' assets and the yearly growth rate of incumbents' market share as outcome variables. In addition to the control variables in equation (4), we include an interaction term between $FB_{s,t}$ and $Failed_i$, and between $FB_{s,t}$ and $Closed_i$. $Failed_i$ and $Closed_i$ are dummies that equal one if a bank failed or closed during our sample period, respectively.[28] We are interested in the free banking indicator and its interaction terms, which capture the marginal effect of free banking laws on the

[27] We do not have data on charge offs, loan loss provisions, and non performing loans. We have information on profits, but only net of dividends. Hence, we are not able to identify whether a bank had low profits because it was inefficient or distributed high dividends.

[28] Note that the $Failed_i$ and $Closed_i$ dummies are time-invariant and absorbed by the bank fixed effects.

banks who failed or closed during our sample period. As for estimating equation (4), we restrict the sample to incumbent banks.

B.2 Results

Table 8 presents the least squares estimates of equation (4). We find that the introduction of free banking laws reduced the probability of closure of incumbent banks by roughly 0.7 percentage points, but did not significantly affect their probability of failure. Relative to the sample mean, the reduction in the probability of closure equals 38.6%. Appendix Table 3 shows the results from estimating equation (4) using a heteroscedastic probit model (see Wooldridge, 2010). The probit model confirms the negative and statistically significant coefficient on the probability of closure.[29] As long as the banks that closed were the least efficient ones, our results are consistent with the hypothesis that incumbent banks became more efficient after the introduction of free banking laws.

In column (1) of Table 9 we present the least squares estimates of equation (5) for the asset growth of incumbent banks. The estimated coefficient on the free banking indicator variable is negative and statistically significant at the 5-percent level. We find that, after the introduction of free banking laws, incumbent banks that closed grew roughly 10 percentage points less than surviving banks. The overall effect of free banking laws on asset growth of incumbent banks that closed (i.e. $\beta_1 + \beta_3$) roughly equals -10 percentage points and is statistically significant at the 1-percent level. There is no statistically significant difference in the asset growth of banks who failed, but free banking had an overall positive and statistically significant effect on the asset growth of these banks (i.e. $\beta_1 + \beta_2$), which increased by roughly 3 percentage points.

The growth rate of incumbents' market share provides additional insights on the evolution of the banking sector. Column (2) of Table 9 shows that the interaction and overall effects of free banking laws are negative for the incumbent banks who closed, and positive for the incumbent banks who failed. The point estimates are statistically significant at the conventional levels.[30] Our evidence suggests that free banking laws reduced the growth rate of incumbents closing during our sample period, both in absolute terms and relative to the market. The effect on the failing incumbents is the opposite, while those on the surviving incumbents is not statistically significant.

[29] We now find also a positive and statistically significant effect of free banking on the probability of failure.

[30] These results continue to hold if we define banks that existed three or five years before the introduction of free banking laws as incumbents, respectively.

Our findings point to a redistribution effect of free banking laws. Banks who failed gained market share at the expense of those who closed during our sample period, while the surviving banks did not benefit nor lose from the introduction of free banking laws. As long as the banks who closed were the least efficient ones, we can conclude that this redistribution effect led to efficiency gains if the failed incumbent banks were making the most productive use of funds. This would be a difficult argument to defend if bank failures were due to poor lending practices or mismanagement.

However, in Section II, we argued that these were not the most likely causes of bank failure, because of the high capital ratios banks had at that time. It might well be that incumbent banks failed even though they were financing productive firms. We interpret our findings along the lines of Rancière, Tornell, and Westermann (2008) and argue that the introduction of free banking laws increased the competitive pressure, which led incumbent banks to lend to new or previously credit constrained firms. As a result, aggregate investment and economic activity increased, as well as banks' credit risk. The increased credit risk led to a greater fragility and might explain the evidence that the incumbent banks failing were those growing more.

VII. Robustness

A. Pre-Free Banking Differences in the Sample

In this subsection, we test whether the introduction of free banking laws was systematically related to county-level outcomes in 1830. We construct three dummy variables. The first dummy captures whether a state adopted free banking laws during the "first free banking wave" (1837-1838). The second dummy captures whether a state adopted free banking laws either during the "first free banking wave" or the "second free banking wave" (1849-1853). The third dummy captures whether a state ever introduced free banking laws during the 1830 to 1860 period. We regress the county-level outcomes in 1830 on each of the dummy variables (see Table 10 for more details). Ideally, we should find that none of the dummy variables have an explanatory power for any of the county-level outcomes in 1830.

Table 10, Panel A, shows the results on the link between county-level outcomes in 1830 and states adopting free banking laws during the "first wave" of free banking. All county-level outcomes in 1830 except loans per capita are statistically insignificant. The results for the "first and second wave" of free banking in Panel B of Table 10 are qualitatively the same. Reassuringly, the timing of free banking does not seem to be driven

by county-level differences in outcomes in 1830. The significant difference in loans per capita suggests that the "early" free banking states, on average, were less financially developed in 1830. This finding reveals that the states that adopted free banking laws earlier were not the most economically and financially developed, but it confirms our concern that the inadequate supply of credit may have influenced the decision to relax the barriers to bank entry. However, we believe that the identification strategy in Section IV and V, and the robustness checks in the remainder of this section, can address this concern.

Panel C of Table 10 presents the results on whether states that ever implemented a free banking system systematically differed in county-level outcomes in 1830. The only significant effects are those on the manufacturing, commercial and agricultural shares. The point estimates suggest that the states that introduced free banking laws were generally those with a more developed industrial and commercial sector than the states who retained the state-chartering policy. To account for these pre-free banking differences, we control for the initial manufacturing and commercial share interacted with a linear and squared time trend in the bank entry and exit analysis of Section IV, while we include these shares as lagged control variables in the growth regression of Section V.

B. Anticipation Effects

A major concern when using a DID approach is that anticipation effects could drive the results. If banks change their behavior anticipating the regulatory change, or in case there are any differences between the treatment and control group prior to the regulatory change, our estimated effects would not be driven by the introduction of free banking laws. To address this concern, we estimate a dynamic specification of equation (1), where we include five leads and lags of the free banking indicator. We test the significance of the cumulative sum of the coefficients, $\sum_{k=-5}^{T} \beta_k$, in order to examine whether anticipation effects contaminate our findings. We use the following estimating equation:

$$y_{c,t} = \lambda_c + \lambda_s t + \lambda_{r,t} + \sum_{k=-5}^{5} \beta_k FB_{s,t} + \Gamma X_{c,t-1} + \Theta LEG_{s,t} + \Psi t Z_{c,1830} + \Phi t^2 Z_{c,1830} + \epsilon_{c,t} \tag{6}$$

Figure 1 and 2 show the cumulative sum of the effects of free banking laws on the share of new and failed banks. The cumulative effect of the free banking laws on the share of new banks shows no pre-existing

trend, but displays a 4.6 percentage point increase in the year when states introduced free banking laws. The cumulative effect on the share of failed banks is qualitatively similar. Also for this variable, there is no pre-existing trend, but a 6.4 percentage point increase in the year following the introduction of free banking laws. Overall, our evidence indicates that the estimated effect of free banking laws is not driven by any anticipation effects.

C. Border-Counties: A Regression Discontinuity Design

The state-by-year variation in the introduction of free banking laws together with the federal structure of the US, allows us to use a regression discontinuity design. Following Holmes (1998)'s identification strategy, we restrict the sample to contiguous counties lying on the border of states that passed a free banking law at different points in time.[31] This approach facilitates the identification of the effect of free banking laws, because geographically close counties are more likely to be similar in terms of unobservables, such as, growth trends and economic shocks.

The estimation strategy, which we call *border-county approach*, follows closely the regression discontinuity design of Black (1999). We estimate the following equation:

$$y_{cb,t} = \lambda_c + \lambda_b t + \lambda_s t + \lambda_t + \beta FB_{s,t} + \Gamma X_{cb,t-1} + \Theta LEG_{s,t} + \Psi t Z_{cb,1830} + \epsilon_{cb,t} \tag{7}$$

The important difference to equation (1) is the inclusion of a border segment specific linear trend, $\lambda_b t$, which captures any unobserved heterogeneity across border segments in the linear time trend of the outcome variable, $y_{cb,t}$. Our identifying assumption is that any within-border segment difference in the treatment is uncorrelated with the within-border segment difference in the error term, that is $E(FB_{s,t}, \epsilon_{cb,t}) = 0$. We use two-dimensional clustering to account for within-state over time and within-border segment over time correlations.[32]

Table 11, Panel A, presents the within-county level response of the share of new banks to the introduction

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

[32] Note that with the border-county approach the same county can be in multiple pairs. We do not include counties multiple times, but choose the selection of the county into its corresponding border segment randomly to preserve the panel structure of the sample. Allowing counties to be included multiple times would not affect our findings qualitatively.

of free banking laws using estimating equation (7). The method of estimation is least squares. As in Section IV.B, we find that introducing free banking laws led to a significantly higher share of new banks. The point estimates are positive, statistically significant and qualitatively similar to the estimates reported in Table 3. In Table 11, Panel B, we repeat the estimation for the share of failed banks using estimating equation (7). As in Section IV.C, we find that introducing free banking laws led to a significantly higher share of failed banks, even though the estimated effect of the introduction of free banking laws is slightly smaller.

We also use the border-county approach to improve the identification of the effect of free banking laws on output per capita. We add a border segment specific linear trend to estimating equation (2) and add border segment fixed effects to the first differences equation (3). Table 12 presents the results. The estimating equation is based on (2) in columns (1)-(3), and on (3) in columns (4)-(6). The method of estimation is least squares. The link between free banking laws and output per capita is positive and statistically significant at the 1-percent level. The point estimates range between 16 to 41 percent, and thus are quantitatively a bit larger than the point estimates reported in Table 5. Overall, the border-county approach confirms our main results of Section IV and V, and mitigates the concern that unobservables could drive our main findings.

D. Selection Effect on Unobservables

In this subsection we perform an exercise in the spirit of Altonji, Elder, and Taber (2005) to assess the size of the omitted variable bias. Altonji, Elder and Taber provide a measure of this bias assuming that selection on observables can be used to assess the size of the selection on unobservables.

We follow Bellows and Miguel (2008)'s approach to construct Altonji, Elder, and Taber (2005)'s measure. We consider two regressions: One with a restricted set of control variables, and one with a full set of controls. We obtain the coefficients on the free banking indicator variable and calculate the ratio $\frac{\hat{\beta}^u}{\hat{\beta}^r - \hat{\beta}^u}$, where $\hat{\beta}^r$ ($\hat{\beta}^u$) is the coefficient obtained from the (un)restricted regression. This measure indicates how much stronger selection on unobservables, relative to selection on observables, must be to explain away the full estimated effect. The intuition is that, if selection on observables can be used to assess selection on unobservables, we should be concerned about the omitted variable bias if including observable controls substantially changes the coefficient on the variable of interest. Hence, the lower the absolute value of the ratio, the greater the size of the bias from unobservables.

We calculate this ratio for our bank competition, bank exit and economic growth variables. The unre-

stricted specification includes the full set of controls, while the restricted specification only the controls listed in the specification shown in the first column of the results on each outcome variable. All the ratios, but the one on the share of new banks, are negative.[33] Given our estimated $\hat{\beta}^u$ coefficients are positive, a negative ratio can only be obtained when the coefficient on free banking increases after we control for observables. Under the assumption that observables and unobservables are positively correlated, a negative ratio indicates that our coefficient on free banking is downward biased. Hence, the omitted variable bias does not seem to be a major concern. As regards the share of new banks, the ratio is roughly equal to sixteen. This means that selection on unobservables must be greater than selection on observables by a factor of sixteen to explain away the full estimated effect of free banking. Taken together with the evidence of the previous subsections, we believe that it is quite unlikely that unobserved heterogeneity drives our main results.

VIII. Conclusion

We have investigated how the introduction of free banking laws in the US between 1837 and 1863 affected bank competition and economic growth. With the introduction of 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 change in bank-chartering policy, together with the fact that systemic stability was not among the objectives of bank regulators at that time, makes the free banking period an ideal setup to study long-standing questions like the effect of higher bank competition on economic growth. Compared to studies based on contemporary data, a major advantage is that we are able to isolate the effects of bank competition from those of state implicit guarantees.

We have shown that the introduction of free banking laws significantly increased bank competition, as measured by the share of new banks, and led to more bank failures in US counties. This evidence is consistent with a strand of literature maintaining that fiercer bank competition increases the probability of bank failures. Our results also indicate a growth-enhancing effect of free banking laws, since output per capita turns out to be significantly higher in counties where free banking laws were introduced. Our results suggest that the growth-enhancing effect of free banking laws is consistent with two explanations. First, bank competition promoted counties' financial development, as measured by loans per capita and money stock

[33] The ratios can be easily calculated using the coefficients shown in Tables 3-7. The restricted coefficient is the one shown in the column with the least controls, while the unrestricted coefficient is the one shown in the column with the full set of controls.

per capita. Second, bank competition determined efficiency gains in the banking industry. In particular, our estimates show that free banking laws decreased the probability of closure of incumbent banks, and led the inefficient incumbent banks to grow less than their more efficient counterparts. These findings are consistent with the literature on the finance-growth nexus, which argues that finance led growth.

An interesting implication of our empirical evidence is that, in a banking system without public safety nets, more frequent bank failures do not harm economic growth in the long run. We cannot infer from this finding that bank failures were costless, nor do we claim that removing public safety nets would be a Pareto-improvement. We do believe, however, that this result might provide some guidance to regulators on the reform process that has started in the aftermath of the 2007-2009 financial crisis. A lot of effort has been directed to addressing the "too-big-to-fail" problem and avoiding the use of taxpayers' money to rescue ailing banks.[34] As a result, the future banking system will probably suffer less from the distortions engendered by public safety nets and, in this sense, will become more similar to a "free-banking system". In order to have a banking system that stimulates economic growth, however, it is crucial to make additional efforts in promoting competition among banks. These efforts should be directed both to the resolution of banks in financial distress, which might hinder the growth of healthier banks, and to limit the risk of excessive concentration of banking activities, especially in those countries where a consolidation process took place in the aftermath of the 2007-2009 financial crisis.

[34]For example, bank living wills and the ring-fencing of banking activities aim at resolving banks' distress in a more efficient way. Requiring banks to hold "bailinable" debt will guarantee the use of taxpayers' money only after forcing losses on bank creditors.

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Appendix: Data Appendix

VARIABLE	Interval	DESCRIPTION
Share of New Banks	Yearly	Fraction of newly established banks out of the total number of banks in each county (Source: Weber, 2006).
Share of Failed Banks	Yearly	Fraction of failed banks out of the total number of banks in each county (Source: Weber, 2006).
Share of Closed Banks	Yearly	Fraction of closed banks out of the total number of banks in each county (Source: Weber, 2006).
Output per capita	Decennial	Total output is the sum of manufacturing and agricultural output in per capita terms at the county level (Source: ICPSR 2896 file).
Manufacturing Output per capita	Decennial	We calculate manufacturing output at the county level as the difference between manufacturing output and the cost of materials used in manufacturing (Source: ICPSR 2896 file). We had to impute manufacturing output for the year 1830 because no manufacturing data were reported in the 1830 Census at the county level. See the ICPSR 2896 codebook for more details.
Agricultural Output per capita	Decennial	Agricultural output per capita at the county level (Source: ICPSR 2896 file). We had to impute agricultural output for the year 1830 because no agricultural data were reported in the 1830 Census at the county level. See the ICPSR 2896 codebook for more details.
Manufacturing Capital per capita	Decennial	Manufacturing capital per capita at the county level (Source: ICPSR 2896 file). We had to impute manufacturing capital for the year 1830 because no manufacturing data were reported in the 1830 Census at the county level. See the ICPSR 2896 codebook for more details.
Loans per capita	Decennial	Real bank loans (in terms of 1830 US Dollars) divided by population (excluding slaves) at the county level (Source: Weber, 2011a).
Real Money Stock per capita	Decennial	Sum of real bank deposits and circulation (in terms of 1830 US Dollars) divided by population (excluding slaves) at the county level (Source: Weber, 2011a).

VARIABLE	Interval	DESCRIPTION
Market Share	Yearly	The ratio of a bank's assets to the total amount of banks' assets in a county (Source: Weber, 2011a).
Age of Banks	Yearly	Average age of banks in business at the county level (Source: Weber, 2006).
Urbanization Rate	Decennial	Fraction of counties' population living in urban areas. The Census declared a county population as urban, if at least 2500 inhabitants lived in urban places (Source: ICPSR 2896 file).
Manufacturing / Agricultural / Commercial Share	Decennial	Fraction of persons engaged in manufacturing / agriculture / commerce (Source: ICPSR 2896 file). We had to impute the manufacturing / agricultural / commercial share for the year 1830 because no manufacturing / agricultural / manufacturing data were reported in the 1830 Census at the county level.
Labor Force Participation Rate	Decennial	Total number of workers divided by total population at the county level (Source: ICPSR 2896 file).

Tables and Figures

Table 1: The Eighteen US 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	1851
Connecticut	1852[b]
Indiana	1852
Wisconsin	1852
Tennessee	1852[c]
Florida	1853
Louisiana	1853
Minnesota	1858
Iowa	1858
Pennsylvania	1860

Source: Rockoff (1972)

(a) Michigan suspended 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 repealed free banking in 1858. Source: Schweikart (1987).

TABLE 2

DESCRIPTIVE STATISTICS

Variables	Interval	Obs	Mean	Sd
	Panel A: County-Level Analysis			
Share of New Banks	Yearly	10,113	0.0938	0.257
Share of Failed Banks	Yearly	10,113	0.0180	0.120
Share of Closed Banks	Yearly	10,113	0.0210	0.123
Free Banking Laws	Yearly	10,113	0.336	0.472
ln(Output p.c.)	Decennial	5,286	4.136	0.547
ln(Manufacturing Output p.c.)	Decennial	4,874	2.343	1.327
ln(Agricultural Output p.c.)	Decennial	5,259	3.792	0.610
ln(Manufacturing Capital p.c.)	Decennial	4,819	1.891	1.296
Loans p.c.	Decennial	5,219	0.0650	0.411
Money p.c.	Decennial	5,219	0.0402	0.227
Number of Banks	Yearly	10,113	2.825	4.533
ln(Population)	Decennial	5,589	8.805	1.228
Banks Capital p.c.	Decennial	9,344	0.1423	0.3665
Urbanization Rate	Decennial	5,589	0.0357	0.126
Manufacturing Share	Decennial	5,053	0.125	0.161
Agricultural Share	Decennial	5,175	0.828	0.198
Commercial Share	Decennial	5,175	0.0568	0.113
	Panel B: Bank-Level Analysis			
Closure Dummy	Yearly	28,569	0.0178	0.132
Failure Dummy	Yearly	28,569	0.0129	0.113
Asset Growth	Yearly	18,451	0.0291	0.238
Market Share Growth	Yearly	18,452	0.0565	0.475
Asset Size (log)	Yearly	22,077	8.139	1.059
Loans to Assets Ratio	Yearly	22,077	0.739	0.195
Public Bonds to Assets Ratio	Yearly	22,077	0.0298	0.110
Cash to Assets Ratio	Yearly	22,077	0.0608	0.0649
Capital to Assets Ratio	Yearly	22,077	0.461	0.155
Circulation to Assets Ratio	Yearly	22,077	0.290	0.153
Deposits to Assets Ratio	Yearly	22,077	0.156	0.118
Age	Yearly	28,569	14.41	13.61

TABLE 3

FREE BANKING LAWS AND BANK COMPETITION

– COUNTY-LEVEL ANALYSIS –

Dependent Variable: Share of New Banks				
	(1)	(2)	(3)	(4)
Free Banking	0.0346***	0.0402***	0.0380***	0.0326***
	(0.0116)	(0.0123)	(0.0117)	(0.0114)
Observations	8732	8732	8732	6569
R^2	0.080	0.103	0.112	0.113
County FE	yes	yes	yes	yes
State Trend	yes	yes	yes	yes
Region-by-Year FE	yes	yes	yes	yes
Banking Sector Controls	no	yes	yes	yes
State Legislation Controls	no	no	yes	yes
Initial Controls (1830)	no	no	no	yes

The dependent variable is the share of new banks between 1830 and 1860. The estimating equation is (1) and the method of estimation is least squares. *Free Banking* is an indicator variable that takes the value one for all years t since state s introduced a free banking law. We use the number of banks and the average age of banks in $t-1$ as controls for county c's banking sector (estimates not reported in the table), see Section IV.A for more details. State legislation controls are usury laws, general incorporation laws, shareholder's liability, branch-banking, state-owned banks, suspensions of convertibility, liability insurance schemes and clearing house arrangements (estimates not reported in the table); see Section II for more details. Initial controls are county c's population size, the urbanization rate, the manufacturing share, the commercial share, the number of banks per capita, and bank capital per capita in 1830 interacted with a linear and squared time trend (estimate not reported in the table); see Section IV.A for more details. Huber robust standard errors (shown in parentheses) are clustered at the state level: *** p<0.01, ** p<0.05, * p<0.1.

TABLE 4

FREE BANKING LAWS AND BANK EXIT
– COUNTY-LEVEL ANALYSIS –

Dependent Variable:	Share of Failed Banks				Share of Closed Banks			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Free Banking	0.0247***	0.0253***	0.0259**	0.0280**	-0.0170	-0.0194	-0.0204	-0.0242
	(0.00841)	(0.00813)	(0.00980)	(0.0123)	(0.0133)	(0.0134)	(0.0150)	(0.0163)
Observations	6325	6325	6325	4852	6325	6325	6325	4852
R^2	0.046	0.052	0.054	0.053	0.078	0.094	0.096	0.113
County FE	yes	yes	yes	yes	yes	yes	yes	yes
State Trend	yes	yes	yes	yes	yes	yes	yes	yes
Region-by-Year FE	yes	yes	yes	yes	yes	yes	yes	yes
Banking Sector Controls	no	yes	yes	yes	no	yes	yes	yes
State Legislation Controls	no	no	yes	yes	no	no	yes	yes
Initial Controls (1830)	no	no	no	yes	no	no	no	yes

In columns (1)-(4) the dependent variable is the share of failed banks between 1830 and 1860, while in columns (5)-(8) it is the share of closed banks. The estimating equation is (1) and the method of estimation is least squares. *Free Banking* is an indicator variable that takes the value one for all years t since state s introduced a free banking law. We use the number of banks, the average age of banks and a set of bank balance sheet ratios in $t-1$ as controls for county c's banking sector. The bank balance ratios included are 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), see Section IV.A for more details. State legislation controls are usury laws, general incorporation laws, shareholder's liability, branch-banking, state-owned banks, suspensions of convertibility, liability insurance schemes and clearing house arrangements (estimates not reported in the table); see Section II for more details. Initial controls are county c's population size, the urbanization rate, the manufacturing share, the commercial share, the number of banks per capita, and bank capital per capita in 1830 interacted with a linear and squared time trend (estimate not reported in the table); see Section IV.A for more details. 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 AND ECONOMIC GROWTH

– COUNTY-LEVEL ANALYSIS –

Dependent Variable:	ln(Output per capita)			Δln(Output per capita)		
	(1)	(2)	(3)	(4)	(5)	(6)
Free Banking $_{t-1}$	0.141***	0.199**	0.256***			
	(0.0508)	(0.0753)	(0.0556)			
Δ*Free Banking* $_{t-1}$				0.136**	0.197**	0.218***
				(0.0586)	(0.0872)	(0.0740)
Observations	2907	2907	2907	1537	1537	1537
R^2	0.433	0.438	0.445	0.063	0.070	0.120
County FE	yes	yes	yes	no	no	yes
State Trend	yes	yes	yes	no	no	no
State FE	no	no	no	yes	yes	no
Year FE	yes	yes	no	yes	yes	no
Region-by-Year FE	no	no	yes	no	no	yes
Controls	yes	yes	yes	yes	yes	yes
State Legislation Controls	no	yes	yes	no	no	yes

The dependent variable is the ln(output per capita). In columns (1)-(3) the estimating equation is (2), while for columns (4)-(6) the estimation equation is (3). The method of estimation is least squares. *Free Banking* is an indicator variable that equals one if state s has introduced free banking in $t-1$. Further control variables are county c's population size, the urbanization rate, the manufacturing share, the agricultural share, the share of blacks, the labor force participation rate, the number of banks and assets per capita (estimate not reported in the table); see Section V.A for more details. State legislation controls are usury laws, general incorporation laws, shareholder's liability, branch-banking, state-owned banks, suspensions of convertibility, liability insurance schemes and clearing house arrangements (estimates not reported in the table); see Section II for more details. 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 LAWS AND THE MANUFACTURING AND AGRICULTURAL SECTOR

– COUNTY-LEVEL ANALYSIS –

Dependent Variable:	ln(Mfg Output per capita)			Δln(Mfg Output per capita)		
PANEL A	(1)	(2)	(3)	(4)	(5)	(6)
Free Banking $_{t-1}$	0.454***	0.390***	0.524***			
	(0.127)	(0.136)	(0.106)			
Δ*Free Banking* $_{t-1}$				0.451***	0.382**	0.328*
				(0.118)	(0.160)	(0.179)
Observations	2731	2731	2731	1389	1389	1389
R^2	0.310	0.313	0.315	0.062	0.068	0.107
Dependent Variable:	ln(Agr Output per capita)			Δln(Agr Output per capita)		
PANEL B	(1)	(2)	(3)	(4)	(5)	(6)
Free Banking $_{t-1}$	0.0832	0.194*	0.204**			
	(0.0999)	(0.106)	(0.0854)			
Δ*Free Banking* $_{t-1}$				0.0825	0.212*	0.217*
				(0.106)	(0.117)	(0.108)
Observations	2903	2903	2903	1534	1534	1534
R^2	0.304	0.314	0.323	0.099	0.117	0.079
Dependent Variable:	ln(Mfg Capital per capita)			Δln(Mfg Capital per capita)		
PANEL C	(1)	(2)	(3)	(4)	(5)	(6)
Free Banking $_{t-1}$	0.227***	0.298**	0.274***			
	(0.0726)	(0.113)	(0.0992)			
Δ*Free Banking* $_{t-1}$				0.245***	0.322***	0.285**
				(0.0553)	(0.0933)	(0.107)
Observations	2718	2718	2718	1377	1377	1377
R^2	0.213	0.223	0.224	0.045	0.059	0.094
County FE	yes	yes	yes	no	no	yes
State Trend	yes	yes	yes	no	no	no
State FE	no	no	no	yes	yes	no
Year FE	yes	yes	no	yes	yes	no
Region-by-Year FE	no	no	yes	no	no	yes
Controls	yes	yes	yes	yes	yes	yes
State Legislation Controls	no	yes	yes	no	yes	yes

The dependent variable is the ln(manufacturing output per capita) in Panel A; the ln(agricultural output per capita) in Panel B; the ln(manufacturing capital per capita) in Panel C. In columns (1)-(3) the estimating equation is (2), while for columns (4)-(6) the estimation equation is (3). The method of estimation is least squares. *Free Banking* is an indicator variable that equals one if state s has introduced free banking in $t-1$. Further control variables are county c's population size, the urbanization rate, the manufacturing share, the agricultural share, the share of blacks, the labor force participation rate, the number of banks and assets per capita (estimate not reported in the table); see Section V.A for more details. State legislation controls are usury laws, general incorporation laws, shareholder's liability, branch-banking, state-owned banks, suspensions of convertibility, liability insurance schemes and clearing house arrangements (estimates not reported in the table); see Section II for more details. Huber robust standard errors (shown in parentheses) are clustered at the state level: *** p<0.01, ** p<0.05, * p<0.1.

TABLE 7

FREE BANKING LAWS AND THE FINANCIAL DEVELOPMENT CHANNEL
– COUNTY-LEVEL ANALYSIS –

Dependent Variable:	ln(Loans per capita)			Δln(Loans per capita)		
PANEL A	(1)	(2)	(3)	(4)	(5)	(6)
Free Banking $_{t-1}$	0.0401**	0.0418***	0.0490***			
	(0.0189)	(0.0141)	(0.00904)			
Δ*Free Banking* $_{t-1}$				0.0395**	0.0381***	0.0352***
				(0.0146)	(0.0128)	(0.00733)
Observations	2766	2766	2766	1441	1441	1441
R^2	0.110	0.116	0.122	0.102	0.108	0.478
Dependent Variable:	ln(Real Money Stock per capita)			Δln(Real Money Stock per capita)		
PANEL B	(1)	(2)	(3)	(4)	(5)	(6)
Free Banking $_{t-1}$	0.0441	0.0607***	0.0636***			
	(0.0259)	(0.0158)	(0.0134)			
Δ*Free Banking* $_{t-1}$				0.0364***	0.0519***	0.0224***
				(0.0126)	(0.0112)	(0.00593)
Observations	2766	2766	2766	1441	1441	1441
R^2	0.075	0.081	0.082	0.075	0.078	0.138
County FE	yes	yes	yes	no	no	yes
State Trend	yes	yes	yes	no	no	no
State FE	no	no	no	yes	yes	no
Year FE	yes	yes	no	yes	yes	no
Region-by-Year FE	no	no	yes	no	no	yes
Controls	yes	yes	yes	yes	yes	yes
State Legislation Controls	no	yes	yes	no	yes	yes

The dependent variable is loans per capita in Panel A, and the real money stock per capita in Panel B. In columns (1)-(3) the estimating equation is (2), while for columns (4)-(6) the estimation equation is (3). The method of estimation is least squares. *Free Banking* is an indicator variable that equals one if state s has introduced free banking in $t-1$. Further control variables are county c's population size, the urbanization rate, output per capita, the manufacturing share, the agricultural share, the share of blacks, the labor force participation rate, the number of banks and assets per capita (estimate not reported in the table); see Section V.A for more details. State legislation controls are usury laws, general incorporation laws, shareholder's liability, branch-banking, state-owned banks, suspensions of convertibility, liability insurance schemes and clearing house arrangements (estimates not reported in the table); see Section II for more details. Huber robust standard errors (shown in parentheses) are clustered at the state level: *** p<0.01, ** p<0.05, * p<0.1.

TABLE 8

THE PROBABILITY OF BANK FAILURE & CLOSURE

– BANK-LEVEL ANALYSIS –

Dependent Variable:	Failure Dummy	Closure Dummy
	(1)	(2)
Free Banking Laws	-0.00005	-0.00688*
	(0.00341)	(0.00337)
Observations	13,632	13,632
R-squared	0.018	0.017
Bank FE	yes	yes
Year FE	yes	yes
Banking Sector Controls	yes	yes

The dependent variable in column (1) is a dummy that equals one if incumbent bank i fails in year t, while in column (2) the dummy equals one if incumbent bank i closes in year t. *Free Banking* is an indicator variable that takes the value one for all years t since state s introduced a free banking law. We use the lagged number of banks at the county level, the lagged age of banks and its logarithm, and a set of bank balance sheet ratios at the bank level as controls. The bank balance ratios included are 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), see Section VI.B.1 for more details. Huber robust standard errors (shown in parentheses) are clustered at the state level: *** p<0.01, ** p<0.05, * p<0.1.

TABLE 9

FREE BANKING LAWS, BANKS' ASSET AND MARKET SHARE GROWTH
– BANK-LEVEL ANALYSIS –

Dependent Variable:	Asset Growth	Market Share Growth
	(1)	(2)
Free Banking	0.00545	0.00140
	(0.0183)	(0.0277)
Free Banking× *Failed*	0.0212	0.0837***
	(0.0230)	(0.0289)
Free Banking× *Closed*	-0.104**	-0.102*
	(0.0397)	(0.0506)
Joint Effect		
Free Banking + Free Banking× *Failed*	0.0267**	0.0851***
	(0.0106)	(0.0138)
Free Banking + Free Banking× *Closed*	-0.098***	-0.100**
	(0.0318)	(0.0449)
Observations	11,696	11,697
R-squared	0.371	0.083
Bank FE	yes	yes
Year FE	yes	yes
Banking Sector Controls	yes	yes

The dependent variable is asset size growth in column (1), while in column (2) we use the market share of incumbent bank i in year t. *Free Banking* is an indicator variable that takes the value one for all years t since state s introduced a free banking law. *Failed* and *Closed* are dummies that equal one if bank i failed or closed during our sample period. We use the lagged number of banks at the county level, the lagged age of banks and its logarithm, and a set of bank balance sheet ratios at the bank level as controls. The bank balance ratios included are 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), see Section VI.B.1 for more details. Huber robust standard errors (shown in parentheses) are clustered at the state level: *** p<0.01, ** p<0.05, * p<0.1.

TABLE 10
PRE-FREE BANKING DIFFERENCES IN THE SAMPLE

	INTRODUCTION OF FREE BANKING LAWS DURING THE FIRST WAVE (1837-1838)									
PANEL A, Dependent Variable:	ln(Output p.c.)	ln(Mfg Capital p.c.)	Loans p.c.	Money p.c.	Asset p.c.	ln(Banks)	Mfg Share	Agr Share	Com Share	Urb Rate
Free Banking (1^{st} Wave)	0.0261	0.0420	-0.0305**	-0.00844	-0.0137	0.0529	0.0139	-0.0159	0.00719	0.00759
	(0.0946)	(0.665)	(0.0128)	(0.0106)	(0.0267)	(0.101)	(0.0495)	(0.0697)	(0.00842)	(0.0155)
Observations	749	738	897	897	897	989	749	738	738	989
R^2	0.000	0.000	0.001	0.000	0.000	0.002	0.001	0.002	0.003	0.001
	INTRODUCTION OF FREE BANKING DURING FIRST OR SECOND WAVE (1837-1838/1849-1853)									
PANEL B, Dependent Variable:	ln(Output p.c.)	ln(Mfg Capital p.c.)	Loans p.c.	Money p.c.	Asset p.c.	ln(Banks)	Mfg Share	Agr Share	Com Share	Urb Rate
Free Banking ($1^{st}/2^{nd}$ Wave)	-0.0306	-0.0851	-0.0322**	-0.0115	-0.0218	0.0404	0.0152	-0.0140	0.00593	0.00485
	(0.0798)	(0.521)	(0.0134)	(0.00993)	(0.0241)	(0.0891)	(0.0407)	(0.0557)	(0.00660)	(0.0128)
Observations	749	738	897	897	897	989	749	738	738	989
R^2	0.001	0.001	0.002	0.000	0.000	0.001	0.002	0.002	0.003	0.000
	FREE BANKING EVER INTRODUCED									
PANEL C, Dependent Variable:	ln(Output p.c.)	ln(Mfg Capital p.c.)	Loans p.c.	Money p.c.	Asset p.c.	ln(Banks)	Mfg Share	Agr Share	Com Share	Urb Rate
Free Banking	-0.0195	0.287	-0.0250	-0.0115	-0.0251	0.0156	0.0513*	-0.0572*	0.00971*	0.00192
	(0.0485)	(0.263)	(0.0221)	(0.0136)	(0.0288)	(0.0743)	(0.0261)	(0.0307)	(0.00536)	(0.0105)
Observations	749	738	897	897	897	989	749	738	738	989
R^2	0.000	0.020	0.001	0.001	0.001	0.000	0.028	0.043	0.012	0.000

The dependent variables, sorted by columns, are the ln(output per capita), ln(manufacturing capital per capita), loans per capita, real stock of money per capita, assets per capita, the ln(number of banks), the manufacturing share, the agricultural share, the commercial share and the urbanization rate for the year 1830. The method of estimation is least squares. In Panel A, *Free Banking (1^{st} Wave)* is a dummy variable that captures whether a state introduced a free banking system during the "first wave" of free banking (1837-1838). In Panel B, *Free Banking ($1^{st}/2^{nd}$ Wave)* is a dummy variable that captures whether a state introduced a free banking system either during the "first wave" (1837-1838) or "second wave" of free banking (1849-1853). In Panel C, *Free Banking* is a dummy variable that captures whether a state ever introduced a free banking law. Huber robust standard errors (shown in parentheses) are clustered at the state level: *** p<0.01, ** p<0.05, * p<0.1.

TABLE 11

BORDER COUNTIES: FREE BANKING LAWS AND BANK ENTRY & FAILURES

Dependent Variable: Share of New Banks				
PANEL A	(1)	(2)	(3)	(4)
Free Banking	0.0383***	0.0402***	0.0354***	0.0294**
	(0.0128)	(0.0143)	(0.0127)	(0.0136)
Observations	3662	3662	3662	3039
R^2	0.156	0.198	0.208	0.200

Dependent Variable: Share of Failed Banks				
PANEL B	(1)	(2)	(3)	(4)
Free Banking	0.0208**	0.0222**	0.0252**	0.0210**
	(0.00799)	(0.00879)	(0.00955)	(0.00983)
Observations	2780	2780	2780	2357
R^2	0.229	0.256	0.257	0.259
County FE	yes	yes	yes	yes
State Trend	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Border Segment Trend	no	yes	yes	yes
Banking Sector Controls	no	yes	yes	yes
State Legislation Controls	no	no	yes	yes
Initial Controls (1830)	no	no	no	yes

The dependent variable in Panel A is the share of new banks between 1830 and 1860, while in Panel B it is the share of failed banks. The estimating equation is (7) and the method of estimation is least squares; see Section VII.C for more details. *Free Banking* is an indicator variable that takes the value one for all years t since state s introduced a free banking law. In Panel A we use the number of banks and the average age of banks in $t-1$ as controls for county c's banking sector, while for Panel B we also add the same set of bank balance sheet ratios as in Table 4 (estimates not reported in the table), see Section IV.A for more details. State legislation controls are usury laws, general incorporation laws, shareholder's liability, branch-banking, state-owned banks, suspensions of convertibility, liability insurance schemes and clearing house arrangements (estimates not reported in the table); see Section II for more details. Initial controls are county c's population size, the urbanization rate, the manufacturing share, the commercial share, the number of banks per capita, and bank capital per capita in 1830 interacted with a linear time trend (estimate not reported in the table); see Section IV.A for more details. Huber robust standard errors (shown in parentheses) are clustered at the state level: *** p<0.01, ** p<0.05, * p<0.1.

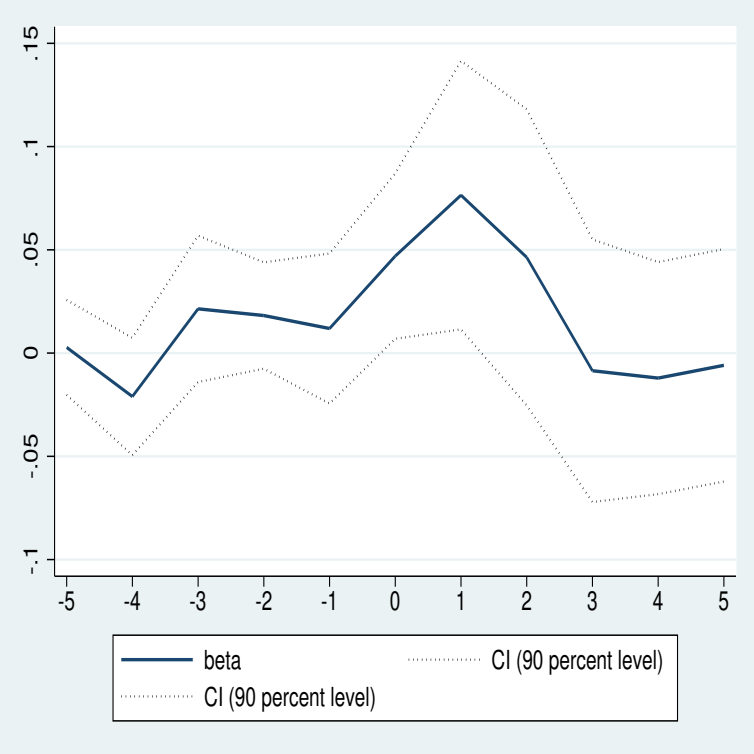
TABLE 12

BORDER COUNTIES: FREE BANKING LAWS AND ECONOMIC GROWTH

Dependent Variable:	ln(Output per capita)			Δln(Output per capita)		
	(1)	(2)	(3)	(4)	(5)	(6)
Free Banking $_{t-1}$	0.159*	0.325**	0.346***			
	(0.0825)	(0.130)	(0.0962)			
Δ*Free Banking* $_{t-1}$				0.162***	0.375***	0.409***
				(0.0613)	(0.0879)	(0.151)
Observations	1137	1137	1137	607	607	607
R^2	0.894	0.897	0.898	0.153	0.183	0.609
County FE	yes	yes	yes	no	no	yes
State Trend	yes	yes	yes	no	no	no
State FE	no	no	no	yes	yes	no
Year FE	yes	yes	no	yes	yes	no
Region-by-Year FE	no	no	yes	no	no	yes
Border Segment Trend	yes	yes	yes	no	no	no
Border Segment FE	no	no	no	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
State Legislation Controls	no	yes	yes	no	yes	yes

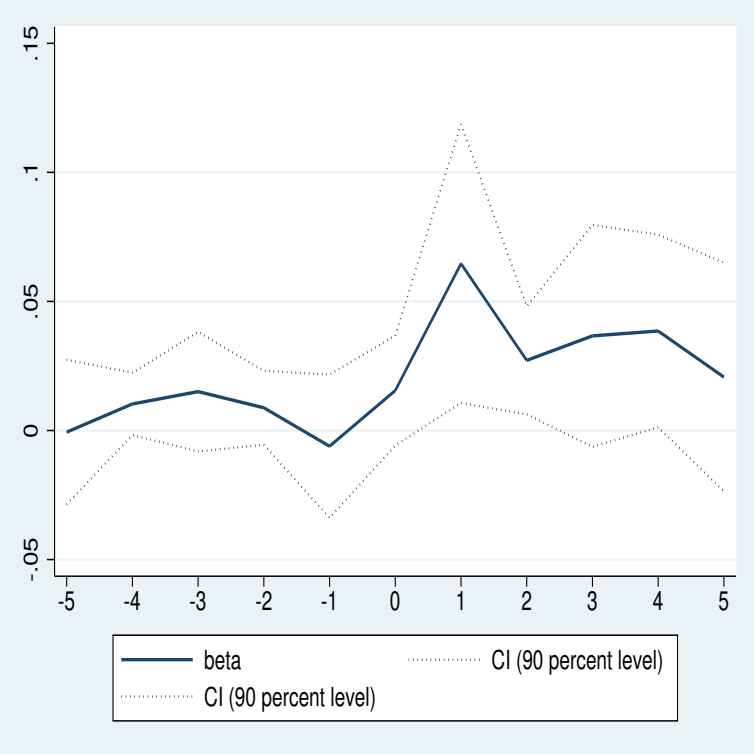
The dependent variable is the ln(output per capita). In columns (1)-(3) the estimating equation is (2), while for columns (4)-(6) the estimation equation is (3). The method of estimation is least squares; see Section VII.C for more details. *Free Banking* is an indicator variable that equals one if state s has introduced free banking in $t-1$. Further control variables are county c's population size, the urbanization rate, the manufacturing share, the agricultural share, the share of blacks, the labor force participation rate, the number of banks and assets per capita (estimate not reported in the table); see Section V.A for more details. State legislation controls are usury laws, general incorporation laws, shareholder's liability, branch-banking, state-owned banks, suspensions of convertibility, liability insurance schemes and clearing house arrangements (estimates not reported in the table); see Section II for more details. Huber robust standard errors (shown in parentheses) are clustered at the state level: *** p<0.01, ** p<0.05, * p<0.1.

Figure 1:
Dynamic Effect of Free Banking Laws on the Share of New Banks



Notes: Coefficients from the dynamic estimating equation (6), discussed in Section VII.B.

Figure 2:
Dynamic Effect of Free Banking Laws on the Share of Failed Banks



Notes: Coefficients from the dynamic estimating equation (6), discussed in Section VII.B.

APPENDIX TABLE 1

FREE BANKING LAWS AND BANK COMPETITION

– HETEROSKEDASTIC FRACTIONAL PROBIT MODEL –

Dependent Variable: Share of New Banks				
	(1)	(2)	(3)	(4)
Free Banking	0.0620*	0.0825***	0.0813***	0.0543**
	(0.0331)	(0.0297)	(0.0272)	(0.0230)
Observations	9344	9344	9344	6943
Quinquennial Year FE	yes	yes	yes	yes
Banking Sector Controls	no	yes	yes	yes
State Legislation Controls	no	no	yes	yes
Initial Controls (1830)	no	no	no	yes

The dependent variable is the share of new banks between 1830 and 1860. The method of estimation is a heteroskedastic fractional probit model; see Wooldridge (2010) for more details. *Free Banking* is an indicator variable that takes the value one for all years t since state s introduced a free banking law. The statistics reported in the table are the marginal effects of *Free Banking* and their standard errors. We use the number of banks and the average age of banks in $t-1$ as controls for county c's banking sector (estimates not reported in the table), see Section IV.A for more details. State legislation controls are usury laws, general incorporation laws, shareholder's liability, branch-banking, state-owned banks, suspensions of convertibility, liability insurance schemes and clearing house arrangements (estimates not reported in the table); see Section II for more details. Initial controls are county c's population size, the urbanization rate, the manufacturing share, the commercial share, the number of banks per capita, and bank capital per capita in 1830 interacted with a linear time trend (estimate not reported in the table); see Section IV.A for more details. Following Wooldridge (2010, pp.10-11), we add the time average of county c's right-hand side variables over the years we observe the full set of data on the control and outcome variables. We further add to the estimating equation and to the variance function the number of years observed for each county in our sample (see Wooldridge, 2010, pp.28-29). Huber robust standard errors (shown in parentheses) are clustered at the state level: *** p<0.01, ** p<0.05, * p<0.1.

APPENDIX TABLE 2

FREE BANKING LAWS AND BANK FAILURES

– HETEROSKEDASTIC FRACTIONAL PROBIT MODEL –

Dependent Variable: Share of Failed Banks				
	(1)	(2)	(3)	(4)
Free Banking	0.0353***	0.0302**	0.0296***	0.0170
	(0.0121)	(0.0120)	(0.0113)	(0.0106)
Observations	6681	6681	6681	5051
Quinquennial Year FE	yes	yes	yes	yes
Banking Sector Controls	no	yes	yes	yes
State Legislation Controls	no	no	yes	yes
Initial Controls (1830)	no	no	no	yes

The dependent variable is the share of failed banks between 1830 and 1860. The method of estimation is a heteroskedastic fractional probit model; see Wooldridge (2010) for more details. *Free Banking* is an indicator variable that takes the value one for all years t since state s introduced a free banking law. The statistics reported in the table are the marginal effects of *Free Banking* and their standard errors. We use the number of banks, the average age of banks in $t-1$ and a set of bank balance sheet ratios as controls for county c's banking sector. The bank balance ratios included are 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), see Section IV.A for more details. State legislation controls are usury laws, general incorporation laws, shareholder's liability, branch-banking, state-owned banks, suspensions of convertibility, liability insurance schemes and clearing house arrangements (estimates not reported in the table); see Section II for more details. Initial controls are county c's population size, the urbanization rate, the manufacturing share, the commercial share, the number of banks per capita, and bank capital per capita in 1830 interacted with a linear time trend (estimate not reported in the table); see Section IV.A for more details. Following Wooldridge (2010, pp.10-11), we add the time average of county c's right-hand side variables over the years we observe the full set of data on the control and outcome variables. We further add to the estimating equation and to the variance function the number of years observed for each county in our sample (see Wooldridge, 2010, pp.28-29). Huber robust standard errors (shown in parentheses) are clustered at the state level: *** p<0.01, ** p<0.05, * p<0.1.

APPENDIX TABLE 3

FREE BANKING LAWS AND PROBABILITY OF BANK FAILURE & CLOSURE

– HETEROSKEDASTIC PROBIT MODEL –

Dependent Variables:	Failure Dummy	Closure Dummy
	(1)	(2)
Free Banking Laws	0.013**	-0.0369**
	(0.005)	(0.015)
Observations	10,850	11,051
Year FE	yes	yes
Banking Sector Controls	yes	yes

The dependent variables are the failure and closure dummies, which captures respectively whether bank i failed and closed in year t. The method of estimation is a heteroskedastic probit model; see Wooldridge (2010) for more details. *Free Banking* is an indicator variable that takes the value one for all years t since state s introduced a free banking law. The statistics reported in the table are the marginal effects of *Free Banking* and their standard errors. We use the lagged number of banks, the lagged age of banks and its logarithm, and a set of bank balance sheet ratios as controls. The bank balance ratios included are 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), see Section VI.B.1 for more details. Following Wooldridge (2010, pp.10-11), we add the time average of county c's right-hand side variables over the years we observe the full set of data on the control and outcome variables. We further add to the estimating equation and to the variance function the number of years observed for each county in our sample (see Wooldridge, 2010, pp.28-29). Huber robust standard errors (shown in parentheses) are clustered at the state level: *** p<0.01, ** p<0.05, * p<0.1.