The Effects of Banking Competition on Growth and Financial Stability: Evidence from the National Banking Era*

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Abstract

How does banking competition affect credit provision and growth? How does it affect financial stability? In order to identify the causal effects of banking competition, we exploit a discontinuity in bank capital requirements during the 19th century National Banking Era. We show that banks operating in markets with lower entry barriers extend more credit. The resulting local credit boom, in turn, is associated with an expansion in real economic activity. However, banks in markets with lower entry barriers also take more risk and are also more likely to default. Thus, we provide causal evidence that banking competition can cause both, growth and financial instability.

JEL: G0, G1, G21, N0, N21

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1 Introduction

How does competition in banking affect credit provision and financial stability? How does it affect real economic outcomes? Despite the importance of these questions to academics and policy-makers, there is only limited consensus about their answers. In theory, it is plausible for competition among banks to either increase or decrease both credit provision and risk taking. Identifying the causal effect of bank competition empirically is generally challenging, as competition and concentration are typically endogenous. For instance, existing empirical studies for the U.S. focus mostly on the deregulation of branching restrictions (see, e.g., Jayaratne and Strahan, 1996, 1998; Black and Strahan, 2002; Jiang et al., 2016, 2017). However, isolating the effects of competition by studying the lifting of branching restrictions can be constrained by confounding factors such as the ability of banks to diversify (Goetz et al., 2016) and a complex interplay of bank mergers and political economic forces (Agarwal et al., 2012; Calomiris and Haber, 2014).

In this paper, we study the causal effects of banking competition on the behavior of individual banks by studying the National Banking Era. By exploiting peculiarities in the National Banking Era's capital regulation, we are able to identify the effect of changes in entry barriers for banks on bank behavior. We document that lower entry barriers induce banks to provide more credit. In particular, our evidence suggests that banks in more contestable markets increase market shares to deter potential entrants, reminiscent of evidence on the behavior of non-financial firms (see, e.g., Goolsbee and Syverson, 2008).

We further show that additional credit provision in more competitive markets is associated with higher real economic activity. However, this additional credit growth is also associated with higher bank risk-taking and banks in more contested markets were more likely to default. Hence, we identify that credit growth causally affects real economic outcomes (King and Levine, 1993; Levine and Zervos, 1998; Chodorow-Reich, 2014; Benmelech et al., 2017), but also contributes to the buildup of financial fragility. Our paper thus offers a causal interpretation of the relation of credit booms and busts (Rancière et al., 2008; Reinhart and Rogoff, 2009; Schularick and Taylor, 2012; Rajan and Ramcharan, 2015; Mian et al., 2017) in which a credit supply shock causes both, real economic activity and the likelihood of financial distress.

The National Banking Era constitutes a close-to-ideal empirical laboratory to study the causal effects of banking competition for three reasons. First, the absence of a central bank, deposit insurance, and potential bailouts imply that banks' behavior is not governed by the anticipation of government interventions. Second, the prevalence of unit banking ensures that banking markets are local, allowing us to compare different, arguably independent markets. Third, minimum capital requirements during the National Banking Era give rise to local exogenous variation in entry barriers, which we exploit for our identification strategy.

In particular, capital regulation during the National Banking Era dictated that shareholders had to raise a minimum dollar amount of equity at the founding of a bank. Capital requirements hence did not constrain a bank's equity-to-assets ratio, as in contemporary regulatory frameworks, but rather represented an entry barrier. Moreover, the minimum dollar amount of equity to found a bank varied with the population of a bank's place of operation as determined by the decennial census. For example, founding a bank in a town with a population of more than 6,000 inhabitants required the partners of the bank to invest twice the minimum capital that was required in a town with fewer than 6,000 inhabitants. Hence, fairly similar local markets above and below this cutoff had different requirements for national bank entrants. We are therefore able to use changes in the census population that altered the amount of regulatory capital required to start a bank to identify the effects of changes in entry barriers on bank behavior, credit provision, and risk-taking.

The regulatory framework further determined that changes in the required capital following a census publication only applied to newly founded banks, and not to incumbent banks. This feature is particularly attractive from the viewpoint of identification, as differential behavior of incumbent banks across markets with different entry barriers can only derive from changes in the requirements for new entrants and not from differential regulatory treatment of the incumbents themselves. Hence, we can isolate the change in bank behavior that stems from differences in the ease with which new banks can enter and contest a market.

To conduct our investigation, we construct a novel data set that consists of all national bank balance sheets from 1867 through 1904, and use the decennial census publications of 1870, 1880, and 1890 as the source of variation in entry barriers. In particular, we focus on towns that had fewer than 6,000 inhabitants as of the respective last census and thus had the same entry barriers in the decade preceding a census publication. We then study outcomes in the decade following a census publication and compare cities that cross the cutoff with those that stayed below.

Our identification strategy is subject to two main concerns. The most important concern is that the population growth that induces the increase in entry barriers may not be entirely exogenous. In that case, differences in outcomes might be driven by the same factors that pushed the population above the cutoff. For instance, town growth trajectories may be concave with smaller towns growing faster than larger towns.

To address this concern, we focus our analysis on towns in the vicinity of the 6,000 inhabitant cutoff for which the assignment of high and low entry barriers becomes quasi-random. We formally examine the discontinuity in entry barriers through tools developed for the analysis of regression discontinuity (RD) designs (Imbens and Lemieux, 2008; Lee and Lemieux, 2010; Cattaneo et al., 2019): Throughout our analysis, we estimate non-parametric local linear and quadratic regressions (Hahn et al., 2001) with optimally selected bandwidths around the cutoff (Imbens and Kalyanaraman, 2011; Calonico et al., 2014). To support this approach, we provide evidence that treated and non-treated cities around the cutoff are indistinguishable across a number of important observable characteristics. Further, we show that our results are robust for various types of data-driven bandwidth selection methods as well as to using parametric estimation techniques. Moreover, we provide evidence that the effect on bank entry is stronger and only statistically significant around the true cutoff.[1]

A second concern stems from the fact even though national banking is the most common form of banking during the period considered, banking services were also be provided by non-national, state-chartered banks. This is important as entering a market with a state bank charter allowed bankers to potentially avoid the relatively strict capital requirement for nationally chartered banks.[2] In order to ensure that our results are not simply driven by the substitution of national banks by state banks, we test and control for the existence and entry of non-national banks.

Our analysis then proceeds in three steps. First, we provide evidence that that bank entry drops discontinuously whenever a town becomes subject to higher entry barriers after a census publication. We find that markets just right to the 6,000 inhabitants cutoff and thus subject to higher capital requirements for entry after the census publication see on average 0.25-0.3 fewer national banks enter over the next decade than those markets just left of the cutoff. The magnitude of this effect is economically meaningful, as most of the markets in our samples are either monopolies or duopolies. Our finding is also in line with the hypothesis developed by Sylla (1969) and James (1978) that

[1]We also show that there is no evidence of manipulation around the 6,000 inhabitants cutoff (McCrary, 2008; Cattaneo et al., 2018), and that we find no discontinuities in predetermined covariates.

[2]As discussed in more detail below, state banks were subject to similar, but often lower, capital requirements as national banks. The relative strictness of these capital requirements varied across states.

capital requirements reduced bank entry during the National Banking Era. We also find that entry of state-chartered institutions is unchanged abound the cutoff, in line with the notion that state banks and national banks were not perfect substitutes (see, e.g., Barnett, 1911; White, 1983).

In the second and central part of our analysis, we compare the behavior of incumbent national banks across markets with different entry barriers. We start by considering indicators of credit availability. We estimate that, after the publication of the census and over the next 10 years, incumbent banks operating in markets with higher entry barriers grew their loan portfolio at a rate around 13-15 percentage points lower than that of their peers in markets with lower entry barriers. Again, the effect is economically meaningful when considering that average loan growth is around 27 percentage points. Our results are therefore consistent with the idea that banks with more market power restrict, rather than increase, credit provision.

A particular advantage of our empirical setting is that our data allow us to study whether differences in bank behavior are a response to changes in actual concentration or driven by changes in entry barriers only. We present two empirical facts that suggest that deterrence of potential entrants is a driver of bank behavior. First, we test our main empirical specification using a restricted sample of markets in which the number of competitors is unchanged throughout the decade following a census publication. We find a similar differential response of incumbents to changes in entry barriers, despite no change in actual competition—indicating that changes in entry costs alone can govern bank behavior. Second, when studying the dynamics in loan growth across markets with different entry barriers, we find that loan growth by less competitive banks slows down immediately after the publication of the census. This is surprising, as actual entry only occurs after time has passed. This finding suggests that banks started reducing credit supply upon learning that entry barriers had increased and thus reduced the need to deter potential entrants. Altogether, our evidence is thus in line with predictions from the theoretical literature on entry deterrence (see, e.g., Dixit, 1979; Milgrom and Roberts, 1982a,b; Klemperer, 1987) as well as more recent empirical evidence from the airline industry (see, e.g., Goolsbee and Syverson, 2008).

Considering banks' risk-taking behavior, we find that incumbent banks in markets with higher entry barriers take less risk than their peers in more competitive markets. In particular, we show that the levels of equity relative to assets and loans—the riskiest component of a bank's assets—are higher in markets with higher entry barriers. If loan portfolios had a similar risk profile across the different types of markets, this finding would imply that banks in towns with higher entry barriers indeed follow a safer business model.

As we cannot directly observe the risk characteristics of loan portfolios, we also consider ex-post measures of risk-taking. We therefore study bank failure rates during the decade following a census publication. We find that failure rates of incumbent banks were around 5 percentage points lower in the less competitive towns in the ten years following a change in entry barriers, an economically significant effect given the unconditional default probability of 4 percent.

The findings on bank failure rates are further supported by the fact that incumbent banks in cities with lower entry barriers tended to have, on average, more seized collateral on their balance sheets than banks in towns with higher entry barriers. This finding suggests a more conservative approach to lending by banks that face less competition. Altogether, our finding that the banks in areas with higher entry barriers took less risk is consistent with theories of market power increasing charter value (see, e.g., Keeley, 1990). Banks with higher charter value have less incentive to take risk and need not expand credit as rapidly—either because they are more cautious about their customers or less concerned about having to protect their market share.

Finally, in the third part of our analysis, we study real economic outcomes. In particular, we investigate whether farming and manufacturing outcomes vary across markets with different entry barriers. In line with existing findings that financial conditions matter for real economic outcomes, we find that lower credit provision by national banks is associated with an decrease in economic activity. We document that ten years after a census is published, markets that are subject to higher entry barriers exhibit a lower per capita farm output, farm value, and number of farms. Moreover, we also show that markets with higher entry barriers exhibit exhibit lower per capita manufacturing capital invested. However, in line with manufacturing becoming more important towards the end of the 19th century, the latter effect can only be detected between 1880 and 1900 and not during the 1870's.

Altogether, our results suggest that competition creates a tension between credit availability and financial stability: we find that banks in areas with more potential competition appear to have made credit more easily available, which appears to be associated with increased economic growth. However, these banks also appear to have taken more risk and were more likely to fail.

The rest of the paper proceeds as follows. We review the related literature in Section 2, before describing our data set in more detail in Section 3. We then provide background on how we use the capital regulation during the National Banking Era to identify the causal effects of banking competition in Section 4. We study the effect on entry in Section 5, the effect on bank behavior in Section 6, and the effects on the real economy in Section 7. Section 8 concludes.

2 Related literature

The effect of competition on bank behavior has been studied extensively, although no ultimate consensus has emerged. Theoretical predictions are sensitive to the assumptions made about the nature of banking. With respect to credit availability and lending volume, an increase in competition will also increase the volume of loans and deposits whenever banks face upward-sloping deposit supply curves and downward-sloping loan demand curves (Klein, 1971). However, if the nature of banking is more complex and the role of relationships more important, the opposite may be true, and competition among banks may decrease overall credit. For instance, if lending requires high initial monitoring efforts, competition will prevent banks from extracting future rents from borrowers, which might reduce lending or prevent it altogether (see, e.g., Petersen and Rajan, 1995).[3] When both forces are active at the same time, the net effect of banking competition on credit may vary with the degree of development of an economy (see, e.g., Cetorelli and Peretto, 2012) as well as the efficacy of regulation (see, e.g., Vives, 2016).

Likewise, theory has ambiguous predictions with respect to risk-taking. Competition potentially increases bank risk- taking, as it may decrease the charter value of banks and hence destroy the incentives of bankers to behave prudently (see, e.g., Keeley, 1990; Allen and Gale, 2004; Corbae and Levine, 2018).[4] By contrast, other theories predict that competition could decrease the overall riskiness of bank lending; if competition reduces interest rates on loans, then the incentives of bank borrowers to take riskier projects is reduced (see, e.g., Boyd and De Nicolo, 2005). Combining both arguments, Martinez-Miera and Repullo (2010) show that the relationship of competition and risk-taking can be U-shaped and vary across different economies.

In the light of the wide range of theoretical predictions, empirical evidence becomes especially important. A number of key contributions indicate that competition, while increasing the efficiency

[3] Another related argument is made by Marquez (2002), who shows that competition among banks increases information dispersion, which impacts banks' screening ability.

[4] See also Repullo (2004) and Matutes and Vives (1996).

of bank management and bank stability, does not necessarily increase credit provision. For example, classic empirical evidence by Petersen and Rajan (1994, 1995) shows that young firms can borrow at lower rates in more concentrated markets, which suggests a higher credit availability in less competitive markets. Further, a series of seminal empirical papers exploit the removal of branching restrictions to identify the effect of competition; see, in particular, Jayaratne and Strahan (1996, 1998). These papers show that the deregulation of branching increased the threat of takeovers and thereby induced bank managers to make more efficient lending decisions. However, the overall evidence also suggests that while the deregulation of branching restrictions leads to better bank management, it does not necessarily lead to more credit provision.[5]

In contrast, other works find an increase in lending as competition intensifies. Dick and Lehnert (2010) and Mian et al. (2017) find an increase in credit provision to households in the context of the lifting of branching restrictions. Moreover, additional evidence by Gissler et al. (2018) finds that more competition from credit unions leads to an increase in credit provision to households by banks.[6]

Similarly, existing evidence on the effects of banking competition on financial stability also varies across different empirical settings. Jayaratne and Strahan (1998) find that the lifting of branching restrictions led to an increase in the overall safety of the banking system. Similarly, Carlson and Mitchener (2009) find beneficial effects of increased competition on financial stability in the 1930s. In particular, they show that incumbent banks which faced competition from a large, diversified entrant either became more efficient—and thus more likely to survive a large shock—or exited the market. By contrast, Jiang et al. (2017)—exploiting variations in the interstate distance and the timing of the lifting of branching restrictions—show that an increase in market contestability increases bank risk taking. This is also in line with the finding from Berger and Hannan (1998) who show that monopolistic markets see fewer bank failures but argue that this is due to a lack of market discipline that, in turn, reduces overall efficiency of the banking system.

[5]Jayaratne and Strahan (1996) find some indications that credit supply may have increased, but argue that the finding is not robust.

[6]Moreover, the real economic effects of increased banking competition are studied by Black and Strahan (2002) and Cetorelli and Strahan (2006), who show that less concentration in the banking sector induces concentration to decline among banks' creditors. The importance for bank market power in monetary policy transmission is discussed in Drechsler et al. (2017). Further important papers on the real effects of branching restrictions in the U.S. are Stiroh and Strahan (2003), Zarutskie (2006), Rice and Strahan (2010), Beck et al. (2010), Cetorelli (2014), Jiang et al. (2019). Additional evidence from France on the real effect of banking competition is provided by Bertrand et al. (2007). Evidence from the UK is by Braggion and Ongena (2017). Finally, a set of recent papers use changes in local concentration resulting from bank mergers to instrument competition, (see, e.g., Scharfstein and Sunderam, 2014; Liebersohn, 2017) and also measure the effects of ownership structures (Azar et al., 2018).

Studying the effects of banking competition by exploiting the lifting of branching restrictions, while extremely useful and important, is limited by a series of factors. First, while the lifting of branching restrictions arguably increased local banking competition, it also changed the banking landscape through a number of other channels. It changes the ability of banks to diversify (Goetz et al., 2016) and thus potentially influences bank risk-taking. Moreover, and particularly in the U.S., it is associated with a wave of bank mergers that interacts in a complex way with other political economic forces (Agarwal et al., 2012; Calomiris and Haber, 2014). Second, the lifting of branching restrictions took place in an environment in which deposit insurance and the prospect of bank bailouts might have influenced bank behavior, potentially masking the effects of competition in absence of government interventions.

Therefore, we argue that our paper's empirical setting has two key advantages over existing studies on the effect of banking competition. First, local variations in entry cost during the National Banking Era do not coincide with variations in other market characteristics, such as the ability to diversify across markets. Second, we provide evidence on the effects of competition in the absence of any ex-ante and ex-post government interventions that might distort behavior.

In contrast to the general leaning in the literature on the lifting of branching restrictions, we find strong indications that banks in more competitive areas tend to provide more credit availability. In particular, we find that lower entry barriers lead to an increase in credit provision by incumbents in an apparent attempt to deter new entrants. Hence, our evidence suggests that banks react to changes in the competitive environment like non-financial firms (see, e.g., Goolsbee and Syverson, 2008).

Furthermore, we find that banks in more competitive areas tend to choose riskier balance sheets, resulting in more bank failures. Our findings lend support to the notion that competition among banks increases bank risk taking (consistent with Jimenez et al., 2013; Braggion et al., 2017; Gissler et al., 2018)[7] rather than increasing the safety of the banking system (as found in Jayaratne and Strahan, 1998; Schaeck et al., 2009; Carlson and Mitchener, 2009).

In line with existing findings that financial conditions matter for real economic outcomes (see, e.g., King and Levine, 1993; Levine and Zervos, 1998; Chodorow-Reich, 2014; Benmelech et al., 2017), we find that the additional credit provision resulting from lower entry barriers leads to faster real economic growth (also in line with existing evidence by, e.g., Cetorelli and Gambera, 2001). Thus,

[7] Additional cross-country evidence on bank failures is provided by Beck et al. (2006), who show that more monopolistic markets see fewer bank failures.

an important contribution of our paper is to provide micro-evidence on the causal connection of increases in credit and economic growth, as well as the lower financial resilience, which is more often debated in macroeconomics (see, e.g., Rancière et al., 2008; Reinhart and Rogoff, 2009; Schularick and Taylor, 2012; Rajan and Ramcharan, 2015; Mian et al., 2017; Jaremski and Wheelock, 2017).

3 Data

To implement our analysis, we assemble bank-level data that incorporate a wide variety of information. The first building block of our data set consists of a comprehensive, novel compilation of the annual balance sheets of all U.S. national banks between 1867 and 1904. Our source is the Comptroller of the Currency's Annual Report to the Congress which reports detailed balance sheet items for all national banks on an annual basis. The data are fairly granular, and include the amount of loans, securities, and reserves each bank held, as well as their levels of regulatory capital, surplus equity, undivided profits, interbank claims, and deposits outstanding. See Figure F.1 in the Appendix for an example of a balance sheet.

Second, we complement our data on national banks with information on the existence and location of state-chartered banks. This information comes from "Rand McNally's Directory of Bankers and Lawyers."[8]

Third, the information on city names, location, and population per decennial census is based on a novel dataset by Schmidt (2017), which is itself based on the decennial census reports digitized by Jacob Alperin-Sheriff and by the U.S. Census Bureau and Steiner (2017). In addition, corrections for city name changes and city mergers (and even relocations) were done manually.

Fourth, railroad data come from Atack (2013), who constructed digital maps of railroad lines as they were laid across the United States. This allows us to determine the year in which a city gains access to a railroad. A city is assumed to have access to a railroad if there is at least one railroad track passing within 10 miles of the city's center. As an additional statistic on railroad access, we also count the number of railroad connections each city had, which we measure as the number of railroad lines that cross the 10-mile-radius circle around each city's center.

Finally, we use real economic outcomes at the county-level from the 1870, 1880, and 1890 decennial

[8]These data were kindly shared with us by Matt Jaremski who documents the existence of state banks, trusts, and savings banks in Jaremski and Fishback (2018).

census, provided by Haines (2004). In particular, the census provides information on manufacturing capital invested, the value of manufacturing products produced, and the number of manufacturing establishments.

4 Background and identification strategy

We start by describing the details of capital regulation during the National Banking Era and how they can be used to identify the effect of bank competition on bank behavior.

4.1 Capital regulation and entry restrictions during the National Banking Era

During the National Banking Era, capital regulation was not intended to constrain banks' leverage ratios. Instead, regulators required a minimum *dollar amount* of equity investment (of "capital stock paid in") in order to establish a bank. Because this minimum was coded in the banks' charters and remained fixed through time, banks were free to arbitrarily increase their own leverage subject to the willingness of depositors to keep their deposits at the bank. Therefore, as several authors have argued before us (see, e.g., Sylla, 1969; James, 1978; Jaremski, 2013; Fulford, 2015), capital requirements were a barrier to entry rather than a restriction on leverage.[9]

Branching regulations restricted banks to operate a single office in a single location or "place". Moreover, capital regulation specified that the minimum amount of capital required to open a bank depended on the population of the bank's location. Specifically, in towns with up to 6,000 inhabitants, newly founded banks were required to maintain at least $50,000 in capital. After crossing this population cutoff, this requirement doubled to $100,000, and increased further to $200,000 in towns with more than 50,000 inhabitants.[10]

[9] Note that the Office of the Comptroller of the Currency itself saw the capital regulation governing entry of banks. In 1876, in a debate on lowering capital requirements, Jay Knox in his function as the Comptroller argued that: "*The organization of small institutions in the large cities has a tendency to weaken those already organized, and to so divide the business as to make them all more or less unprofitable to the shareholders.*", See Appendix, Section F for details.

Furthermore, note that were also other regulations related to capital. For instance, bank directors also had to be shareholders and were required to reside in the vicinity of the bank. Moreover, national banks were subject to a "double liability" rule: in case of a bank failure, shareholders were liable to lose not only their investments in the bank, but their own personal property up to the book value of their shares (see also Grossman, 2001; Koudijs et al., 2018).

[10] The selection of the 6,000 inhabitant cutoff appears to have been a political compromise. For instance, the proposed "Hooper bill" from 1862 suggested a $50,000 requirement for all locations. The "Sherman Act" of 1863 in contrast suggested to increase the capital requirement once a location's population exceeds 10,000 inhabitants. For details, see Davis (1910).

In 1900, the capital regulation was refined such that banks founded in towns with less 3,000 inhabitants were required to raise only $25,000 in capital paid-in, studied in more detail by Gou (2016). Moreover, banks were not allowed to pay out dividends until the bank had accumulated a surplus fund of at least 20 percent of the regulatory capital determined in the banks charter. See James (1978) and Champ (2007) for details.

$$\text{“Capital stock paid in”} \geq \begin{cases} \$50,000 & \text{if population } \leq 6,000 \\ \$100,000 & \text{if population } \in (6,000, 50,000] \\ \$200,000 & \text{if population } > 50,000 \end{cases}$$

Two additional details regarding this capital requirement turn out to be key for our identification strategy. First, the legal population of a place was determined by the most recently published decennial census.[11] Second, the regulatory capital requirement only applied to national banks that were entering the market, and not to incumbent national banks, i.e. incumbent banks did not have to increase their capital even if the towns in which they operated grew in population. These details are, for instance, described in the contemporary legal resource *"Pratt's Digest of the National Bank Act and Other Laws Relating to National Banks from the Revised Statutes of the United States"* (Pratt, 1886):

> *"The population of a place in the United States is legally determined by the last previous census. Thus a bank organized at any time between 1880 and 1890 would generally be bound by the census of 1880. Exceptions might of course arise, as, for instance, where new towns are started in the interval, and other proof of population might then be accepted by the Comptroller. Small variations in population between censuses, would not be regarded. A bank organized with $50,000 capital in a small place might continue with that capital if the population should increase to any number. It thus sometimes happens that we find banks in some towns and cities that appear to have less than the minimum capital required by law. They were either organized when the places were smaller, or were organized in villages absorbed by cities lying near." (page 12)*

The fact that the legal population is determined by the most recent census means that even if the population of every town is changing constantly, the minimum requirement for entrants only changes when the census is published. In line with the regulatory statutes, Figure D.1 shows that all banks in our sample that are founded between 1871 and 1899 fulfill the regulation: While banks can choose to have more capital than required, banks that are founded in cities with more than 6,000 inhabitants always have at least $100,000, whereas banks in cities with fewer than 6,000 inhabitants never have less than $50,000, but potentially do have less than $100,000. Moreover, more than two-thirds of

[11] The "place" could be a "city," a "town," a "village," or an incorporated place enumerated in the decennial census. Note that the census also reported information on civil townships (confusingly, called "towns" in New England, New York, and Wisconsin). Thus, in cases where two locations share the same name in a given state (e.g., Dunkirk, NY), we always select the city, town, or village, and not the civil townships.

all newly founded banks in our sample period opened with the exact minimum capital required, indicating that the constraint was binding in most cases.

The fact that changes in the capital requirement due to population growth only applied to entrants and not to incumbent banks is very attractive from the standpoint of identification, as any observed changes in the behavior of incumbent banks are therefore driven by changes in the local market structure, rather than by changes in the banks' own capital structure. This is particularly important, as a change in their own minimum amount of capital required may affect banks in ways other than through competition.[12]

Finally, note that the capital regulation described above only applies to banks that enter a local market under a national charter, but not under a state charter. Hence, we need to keep track of state bank entry as discussed in more detail below.

4.2 Identification

In order to study the effect of bank competition on bank behavior, we exploit that census publications changed entry barriers differentially around the 6,000 inhabitant cutoff. We use the census publications of 1870, 1880, and 1890 as the source of variation in entry barriers and study differences in bank behavior over the respective subsequent decade.

We define a local market as *treated* and hence subject to higher entry costs for national banks if it had fewer than 6,000 inhabitants as of the preceding census but more than 6,000 in a given census publication. The control group consists of all cities that had fewer than 6,000 inhabitants in both the last and the current census. Formally, we define $\mathbb{1}_{ct}^{\text{pop}>6{,}000}$ as an indicator variable equal to one if city c passes the 6,000 person threshold in the census of year $t \in \{1870, 1880, 1890\}$ and zero otherwise; i.e.,

$$\mathbb{1}_{ct}^{\text{pop}>6{,}000} = \begin{cases} 1 & \text{if } \text{pop}_{ct} > 6,000 \\ 0 & \text{if } \text{pop}_{ct} \leq 6,000 \end{cases}.$$

Because there is another step-up in capital requirements once a town has 50,000 inhabitants, we exclude from the study banks in towns that crossed this threshold, as there are not many such cities

[12]For instance, banks subject to the higher capital requirement may have a different ownership structure, as they may need to increase the number of partners to raise the capital required. In turn, differences in ownership structure are important for a bank's governance; see Calomiris and Carlson (2016).

and because this threshold also granted eligibility a reserve city status, [13] a potentially important confounding factor. Hence, we focus our analysis on the discontinuity around the 6,000 cutoff.

We restrict the sample to towns with fewer than 6,000 inhabitants according to the last preceding census. Thus, both control group and treatment group shared the same entry barriers through the preceding decade, but as the decennial census was released some markets became subject to higher entry barriers. Pooling different census years implies that the same town can be in our sample up to three times. For instance, the town of Charlotte, NC had a population of 2,265, 4,473, 7,094 and 11,557 as of the 1860, 1870, 1880 and 1890 census. Thus, Charlotte will be a control in 1870, treated in 1880, and excluded in 1890.[14]

Throughout all three census episodes, we assume that population estimates become available publicly in the year after the census has been conducted. For instance, the 1880 census in conducted in June 1880 and we treat 1881 as the year in which results of the 1880 census are published.[15]

We use towns that had at least one national bank by the time a decennial census was published, as we are interested in studying the response of incumbent banks to changes in entry barriers. This data restriction implies that our paper focuses on the effect of adding an additional bank to a market that already has one or more national banks, rather than studying the margin of having any national bank or not. Note that more than 95 percent of our sample consists of towns with one or two incumbent national banks. Hence, we study the effects of banking competition in markets in which a monopoly may become a duopoly, or a duopoly become an oligopoly. In line with existing theories and empirical evidence on competition, these are the margins at which the effect of competition can have the largest effects (see, e.g., Bresnahan and Reiss, 1991). Moreover, as most towns experience population increases during the episode considered, our study focuses on the effect of an increase in entry barriers following a census publication.

Finally, we exclude cities from the former Confederate states during the 1870's due to concerns that the population counts in the South may be unreliable before and after the Civil War. Moreover,

[13]The National Banking Era reserve requirements dictated that banks in locations that are not deemed reserve or central reserve cities, which included most small towns and cities, were required to hold their reserves with banks in reserve or central reserve cities. (Central) reserve city banks, in turn, would be subject to different reserve requirements. We exclude all cities that are or become reserve and central reserve cities from the sample.

[14]Once a town crosses the cutoff it will not reappear in the data. This is true even if the town experiences population decline and crosses below the threshold, a relatively rate event.

[15]The official publication of the census may have taken longer. For instance, the final results of the 1880 census were published on March 2, 1882. However, the Census Bureau provided some preliminary results to local newspapers as early as July 1880 and we thus choose 1881 as the relevant publication year. The bank-level outcomes are reported as of October. Therefore, the above amounts assume that all population estimates are publicly available by October 1871, 1881, and 1891.

it addresses concerns that results may be driven by peculiarities in the immediate aftermath of the Civil War during which large parts of the South had been destroyed (see, e.g., Feigenbaum et al., 2018). Cities from the South are included from 1880 onwards and our results are robust to including the South throughout all three decades.

We arrive at a sample of 2,864 city-census year observations with around 1,700 unique cities.[16] Of those 2,864 city-census year observations, 285 unique cities are *treated* and cross the 6,000 person threshold according at a census publication. Further, we identify more than 3,000 bank-census year observations with around 2,400 unique national banks, of which more than 400 banks are in *treated* markets that are subject to higher entry costs after a respective publication of the census. Our sample thus covers a significant part of the entire banking system. For instance, by 1881, around 2,000 national banks had been founded of which around 1,000 are in our sample.[17]

As noted above, our identification strategy is subject to two main concerns. First, variations in population growth and hence in entry barriers may not be purely random and exogenous. Second, an increase in entry barriers may change the type of bank entrant left and right of the cutoff and in particular national bank entry may be substituted by institutions that are not subject to the same regulations. In the following, we lay out how we address these types of concerns.

Starting with the first concern, note that in order to identify an effect of a variation of entry costs on banking behavior, this variation would need to be independent of other factors that affect banking. However, having more than 6,000 inhabitants in a given census year and thus being subject to higher entry costs may not be entirely exogenous. Cities that cross the threshold might already have a higher population in the preceding census, might have experienced a faster population growth over the preceding decade, or both. These differences in the evolution of a town's population could in turn cause differences in bank entry and bank behavior after a census publication, especially further away from the cutoff. For instance, if growth trajectories of towns are concave—i.e., if growth flattens out over time—we may be simply picking up that towns that grew faster in the past subsequently grow slower and hence have less bank entry.

To alleviate this first-order concern, we conduct our analysis only using towns that are in the immediate vicinity of the 6,000 inhabitants cutoff. The identifying assumption is thus that the

[16] Figure D.2 in the Appendix shows the spatial distribution of the sample and also shows that treated towns are fairly evenly spatially distributed across the sample and not clustered in one specific region.

[17] For comparison, in 1871, there are around 1,700 national banks, and around 600 of those are in our sample. In 1891, a total of around 3,700 national banks are in operation and around 1,700 are in our sample.

assignment of higher entry barriers is quasi-random in the immediate vicinity of the cutoff and that these towns are similar in all other aspects. Moreover, as is standard in regression discontinuity designs, we include the running variable (population) with a different slope both left and right of the threshold.

To support this empirical approach, we start out by providing evidence on observable characteristics for treated and non-treated towns prior to the publication of the census. In particular, Table 1 shows observable characteristics in the year of a census publication for cities with a population of +/- 1,000 inhabitants around the cutoff. By construction, there are clear differences in population levels—the running variable—and treated cities have, on average, around 1,000 more inhabitants than non-treated cities. However, reassuringly for our purposes, treated and non-treated cities are similar in most other important observable characteristics such as the number of banks, railroad access, credit growth since the last census publication, and per capita rates of manufacturing capital, establishments, and output. Further, note that panel (a) and (b) of Figure D.3 in the Appendix shows that there are also no visually detectable discontinuities in important covariates such as the past population growth and the number of banks. The only statistically significant difference between cities left an right of the cutoff is that smaller cities tend to be located in counties that tend to have more farms, in line with smaller towns being likely to be located in rural areas.[18]

[TABLE 1 ABOUT HERE]

Given the differences in city size, the national banks in treated and untreated cities, banks in treated cities tend to be larger and thus have more outstanding loans (see Table 2). However, panels (b) and (c) in Figure D.3 show that—reassuringly—there is no discontinuity in bank size around the cutoff; instead bank size increases linearly in town size. Moreover, other than differences in average bank size, there are no detectable differences. For instance, banks across both types of market have on average the same leverage and capital-to-assets ratios in the year of a census publication. Also note that bank age is about the same in these groups of cities alleviating concerns of preemptive entry in anticipation of the census.

[18] Also note that the average distance of cities in our sample to the next city with a bank is around 10 km, for both treated and non-treated cities. Note that these are considerable distances to travel in absence of the automobile. Even in modern times, the median distance between banks and firms lies only somewhere between 6 and 13 km (Petersen and Rajan, 2002; Brevoort et al., 2011). We are thus confident to assume that banking markets are local and best measured at the town-level.

As treated and non-treated towns and incumbent banks are very similar in observable characteristic, we identify the effects of increased entry barriers by formally exploit the discontinuity at the 6,000 inhabitant cutoff and making use of the toolkit developed for the analysis of regression discontinuity (RD) designs (Imbens and Lemieux, 2008; Lee and Lemieux, 2010; Cattaneo et al., 2019). Throughout our analysis, we estimate local as well as quadratic linear regressions (Hahn et al., 2001) with tight bandwidths right around the cutoff, applying a variety of different MSE-optimal bandwidth selection methods (Imbens and Kalyanaraman, 2011; Calonico et al., 2014). Further, we conduct a series of validation and falsification tests to study whether the effect can only be detected at the true cutoff (Ganong and Jäger, 2018) and also show that there is no evidence of manipulation around the cutoff (McCrary, 2008; Cattaneo et al., 2018).

[TABLE 2 ABOUT HERE]

The second important type of concern for our identification strategy is that entrants left and right of the cutoff may be different. On the one hand, entrants right of the cutoff may be more likely to be state banks that are not subject to the same regulatory requirements. On the other hand, national banks entering right of the cutoff may—due to the higher capital paid in—mechanically be larger than those entering left of the cutoff. Both observations raise the issue that higher entry barriers may not only affect competition per se, but also the *type* of competitors that incumbents are facing.

With respect to the former, note that national banks are not the only type of financial intermediary active during the period considered. Competition could also arise from other types of financial institutions that provide similar services, such as state banks or savings banks. These institutions also faced capital requirements imposed by state regulations, but these tended to be less stringent than those for national banks. Hence, higher entry barriers for national banks increase the incentive for bankers to enter a market with non-federal bank charters that are not subject to the higher regulatory requirements.

We address this concern in several ways. First, focusing on the behavior of incumbent national banks has the advantage that incumbent banks should react to both potential national and state bank entry. Hence, any differential behavior we observe across markets with different entry barriers for national banks is a reaction to different degrees of competition. Second, national banking is generally the predominant type of banking during the period considered—for instance, in 1881, more than

80 percent of banking assets were held by national banks. Third, we exploit that state bank entry barriers varied across states and identify a subset of states in which there is no differential impact of national bank regulation on state bank entry.[19] We then confirm our main results for this subset of states in which state bank entry barriers are high, indicating that our results are driven by a change in the competitive environment rather than a change in the type of banking.

Moreover, abstracting from state bank entry, national banks entering right of the cutoff are on average founded with a larger capital base and may hence by construction be larger, see Figure D.1. It may therefore be the case that incumbents not only react to a lower probability of getting an additional competitor but also to the prospect that, upon entry, the entrant will have a large capital base. We are not able to fully determine whether results are driven by competition per se or the type of competitor. As we argue below, in the light of our results, it seems more plausible that the probability of entry is the dominating margin. However, the data ultimately do not allow us to distinguish which of the two possible margins of banking competition is more important.

5 The effects of entry barriers on competition

In this section, we analyze the effect of increased entry barriers on bank entry and the degree of local competition. If, as argued above, an increase in the minimum capital required to open a national bank acted as a meaningful barrier to bank entry, then we would expect to observe less national bank entry in markets that crossed the 6,000 person cutoff in the years following a census publication. At the same time, founding a state-chartered bank may possibly become more attractive. Thus, we also test whether a lower number of national banks entrants is offset by newly founded state-chartered institutions.

We begin by providing visual evidence of the effect of higher entry barriers on the degree of local competition. Panel (a) and (b) of Figure 1 depict binned scatter plots of the number of new national bank entrants by city throughout the decade following a census publication. We use binned scatter plots as the outcome variable is a discrete number and a scatter would thus be uninformative. Bins are equal-sized and contain around 15 observations and are grouped by city population as of the respective census. In panel (a), we include linear fits left and right of the 6,000 inhabitant cutoff and

[19] In particular, we exploit that in some states, state bank entry was at the discretion of the bank regulator White (1983), raising the possibility for incumbents to bribe regulators to keep entrants out (see, e.g., Schwartz, 1947).

in panel (b) we apply quadratic fits.

Both figures show that there is a positive correlation between city size in the year of a given census and the number of national bank over the following decade. However, there is also a discontinuity in the linear fits right around the 6,000 person cutoff. In particular, in the decade following a census publication, about 0.35 national banks entered in towns just below the threshold, while only 0.1 national banks entered in cities just right of the threshold. Thus the visual evidence suggests that higher entry barriers due to higher capital requirements for entrants affected bank entry right around the population cutoff.

Additionally, Figure 2 shows a regression discontinuity plot, again using the number of bank entrants over a decade following a census publication as the outcome variable. Here, we follow Calonico et al. (2017)'s optimal data-driven methods for automatically selecting the number and spacing of bins. As discussed by Cattaneo et al. (2019), doing so avoids the need for potentially subjective and ad-hoc tuning parameters. Panels (a)-(c) show quantile–spaced bins with varying polynomial fits. Using quantile-spaced bins has the advantage of taking into account the increasing sparsity of the data as the population size increases. Panel (d) plots the same outcome variable using equally-spaced bins.

Including 95% confidence intervals for each bin, all four panels confirm the visual pattern discussed above of a discontinuous drop in bank entry right around the cutoff. Reflecting the fact that there are fewer towns with a larger population, confidence bands become wider for larger towns. However, most important for our purposes, the bins just right of the cutoff are outside of the confidence intervals just left of the cutoff, reinforcing the evidence on a discontinuity in entry right around the cutoff that affects entry barriers.

Overall, the visual evidence thus suggests that whenever national banking entrants face a higher capital requirement, entry of new national banks is less likely. Hence, an increase in the capital requirement for new entrants seems to represent an increase in the barriers to entry. The visual evidence is particularly important in the context of our setting as the sharp discontinuity around the cutoff makes it less implausible that our results are driven by a non-linear relationship between town size and banking market outcomes.

[FIGURE 1 AND 2 ABOUT HERE]

To formally test the effect of capital regulation on entry in a local market, we estimate local linear regressions (Hahn et al., 2001; Calonico et al., 2014) that allow for the functional form of the running variable to vary across treated and non-treated cities. Moreover, we use various bandwidth selection methods and different kernel functions to construct the local estimators.[20] Specifically, we estimate

$$y_{ct} = \alpha + \beta_1 \cdot \mathbb{1}_{ct}^{\text{pop}>6,000} + \beta_2 \cdot (\text{pop}_{ct} - 6,000) + \beta_3 \cdot \mathbb{1}_{ct}^{\text{pop}>6,000} \cdot (\text{pop}_{ct} - 6,000) + \varepsilon_{ct}, \qquad (1)$$

where y_{ct} is the number of banks that entered city c in the decade after the publication of the census conducted in year t, pop_{ct} is the running variable and given by city c's population in census year t, and $\mathbb{1}_{ct}^{\text{pop}>6,000}$ is as defined above and given by a dummy whether a city's population is larger than the 6,000 inhabitants cutoff. To best capture differences in economic and social trajectories across regions, we add control variables indicating whether a city is located in the South (former confederate states) or the West using the covariate-adjustment approach developed by Calonico et al. (2019). Note that results are robust to excluding covariates entirely.

In our main analysis, we calculate Mean Squared Error (MSE)-optimal bandwidths around the cutoff in two ways as suggested by Calonico et al. (2017).[21] One case allows the optimal bandwidths to differ below and above the cutoff, while the other uses a single symmetrical bandwidth. Moreover, while we allow the slope of the running variable to vary left and right of the cutoff, for robustness we also allow the running variable to enter quadratically in some specificiations.

Results from estimating Equation (1) are reported in Table 3. In line with the visual pattern of Figure 1 and Figure 2, there is a strong negative relationship between being above the 6,000 population threshold at a census publication and national bank entry in the subsequent decade. In particular, column (1) shows that when allowing the bandwidths to be different left and right of the cutoff, it is optimal to estimate the effect from using cities with around 3,500 to 7,700 inhabitants. Choosing different bandwidth on each side of the cutoff is justified in our setting as there are more data point left to the cutoff. Effectively, we are using 677 observations left of the cutoff and 174 observations right of the cutoff to construct the estimator. The effect is consistently between 0.25 and 0.26, independent of whether one considers a conventional RD estimate, a bias-corrected estimate, or a robust estimate. Further note that column (2) shows that the effect is robust to changing the kernel

[20] Note that all our results are robust to using parametric estimation techniques, see, e.g., Table E.1 in the Appendix.

[21] Further note that our results are generally robust calculating the optimal bandwidth according to Imbens and Kalyanaraman (2011).

function to construct the estimator being triangular or uniform rather than epanechnikov.

The effect is also robust to calculating the optimal bandwidth symmetrically around the cutoff. Column (4) shows that in this case, the MSE-optimal bandwidth becomes +/- 1,700. Under this bandwidth selection, to construct the estimator we effectively only use cities with 4,300 to 7,700 inhabitants and 374 observations to the left of the cutoff and 170 to the right. Again, we find that towns right of the cutoff see around 0.26-0.27 less entrants following a census publication. Finally, columns (4)-(5) also show that the results are robust in magnitude and precision when allowing the running variable to be quadratic as opposed to linear.

[TABLE 3 ABOUT HERE]

Altogether, our evidence suggests that higher entry barriers following a census publication led to a lower number of bank entries over the following decade.

5.1 Robustness

To further strengthen the identification, we conduct a set of validation and falsification tests and repeatedly estimate the main model with varying cutoffs (see, e.g., Ganong and Jäger, 2018). In Figure 3a we plot coefficients for $\mathbb{1}_{ct}^{\text{pop}>X}$ from estimating local linear regressions of the form:

$$y_{ct} = \alpha + \beta_1 \times \mathbb{1}_{ct}^{\text{pop}>X} + \beta_2 \times (\text{pop}_{ct} - X) + \beta_3 \times \mathbb{1}_{ct}^{\text{pop}>X} \times (\text{pop} - X) + \varepsilon_c \tag{2}$$

where y_{ct} is the number of bank entries in the decade following publication of the census from year t, and X defines a cutoff that we vary between between 4,000 and 8,000 inhabitants in incremental steps of 10. We estimate Equation (2) for a fixed bandwidth of $\pm$ 2,000 inhabitants around the cutoff X. We choose the manually selected symmetric bandwidths in order to ensure comparability of coefficients across regressions.

Figure 3a reveals that the effect on bank entry is estimated to be the strongest right around the true cutoff of 6,000. Once the cutoff is moved to a larger or smaller population level, the estimates cease to be statistically significant. Being able to show that the results only hold around the true cutoff is reassuring: if forces other than a change in entry barriers, such as concave growth trajectories of towns, were driving the result, the effect on bank entry should arguably also be detected farther away from the actual cutoff. As this is not the case, we are confident that our results are driven by a change in entry barriers rather than by economic forces that are independent of the competitive environment.

To further address the identification concerns based on non-linear effects of town growth, we conduct additional permutation tests by estimating Figure 3 for a sample of either only non-treated cities or only treated cities, again varying population cutoffs. For instance, we exclude all treated cities with more than 6,000 inhabitants and only compare cities that have the same, low entry barriers and then vary the cutoff between 3,000 and 6,000. Alternatively, we exclude all non-treated cities and vary the cutoff between 6,000 and 9,000. If more general trends in town growth would be driving our findings, one would expect to find an effect on bank entry in each of these restricted samples too, with relatively larger towns seeing less entrants. The results are presented in Figure 3 and indicate that there are no negative effects at any cutoff on bank entry in these restricted samples. This findings re-emphasizes that it is unlikely that our results are driven by town growth patterns rather than the change entry barriers.

[FIGURE 3 ABOUT HERE]

An additional important concern is that agents could engage in manipulation of their reported population around the cutoff, leading to bunching of towns on either side of the cutoff. In the context of our study, the most plausible worry would be that incumbent banks somehow influence the population count reported in a census in order to nudge it to the right of the threshold, and increase their market power. To this end, note that in Section A of the Appendix, we also show that there is no evidence of manipulation around the cutoff (McCrary, 2008; Cattaneo et al., 2018).

5.2 State bank entry

State banks were not subject to the same regulatory requirements as national banks. Prospective bankers unwilling or unable to raise the $100,000 required to open a national bank in a town above the 6,000 person cutoffmight instead opt to open state-chartered banks. If this was the case, and if state and national banks were perfect substitutes, then state banks might potentially fill the gap left by the lack of national bank entrants, leaving the local competitive environment unchanged.

We test this hypothesis explicitly and estimate Equation (1) using the number of national bank entrants, state bank entrants, and the combined number of new entrants as the dependent variable. Columns (7) and (8) of Table 3 show these results.[22]

We find that even though national bank entry becomes more costly and less likely when a town crosses the 6,000 inhabitants cutoff, state bank entry is estimated to be essentially unaffected: The coefficient on crossing the 6,000 inhabitants cutoff is positive, but relatively small and statistically insignificant—irrespective of the bandwidth selection method. This finding is in line with the notion that state banks and national banks were not perfect substitutes. There are a number of reasons to believe that this was the case, as state banks had a comparative disadvantage in issuing bank notes and were less integrated intro the interbank market. Moreover, given the relatively lax regulation at the state level, state banks were generally perceived as less safe institutions and not as well reputed as national banks (Barnett, 1911; White, 1983).

The impact of competition from state banks could nonetheless be a confounding factor when studying bank behavior, as not only the actual type of competition but also the prospect of the type of competition may vary across markets with different entry barriers. I.e., even though we cannot detect a statistically significant effect on state bank entry, the higher potential of state bank entrant right to the cutoff may nonetheless have affected bank behavior. To rule this out, we exploit the heterogeneity in state bank capital requirements across states. In particular, we focus in a subset of states where state bank entry either required either as much capital as national bank entry, or where initial capital levels were at the discretion of the state bank regulator, which made entry barriers potentially unpredictable and costly. Hence, this subset of states, as detailed in Section B of the Appendix, allows us to check whether our results hold when competition from state bank entry is unlikely in any scenario. As further detailed in the Appendix, we find that this is indeed the case.

Altogether, our evidence suggests that towns subject to higher barriers of entry had indeed a lower actual frequency of entry, as well as a lower total number of banks in the decade following the publication of a given census. Hence, an increase in capital requirements is a good predictor for the ease at which a local market can be contested.

[22] Note that we only have state bank data for a subset of all the cities in our main sample used above. Our findings on national bank entry are robust to using the sub-sample of cities for which we have state bank data.

6 The effect of entry costs on incumbent banks' behavior

Having verified that capital regulation affects actual entry and hence the competitive environment, we now study the behavior of incumbent national banks. In particular, we contrast how incumbents behave in markets with low and high entry barriers in the 10 years following a census publication. Focusing on incumbents—banks already in existence at a given census publication—has the key advantage of isolating the effects of changes in the degree of local competition, as opposed to changes in the banks' capital structure. As discussed earlier, incumbent banks were not subject to the new minimum capital requirements, and differential behavior between incumbents across different markets arguably stems from differences in entry barriers.

This section studies incumbents' behavior in three dimensions. First, we ask if higher entry barriers affected their credit provision and deposit issuance, and if other balance sheet components were affected. Second, we look at whether potential differences in credit provision appear to be driven by differences in actual entry or whether they might be the result of incumbents attempting to deter potential entry. Finally, we study whether indicators of banks' risk appetite differed based on local entry barriers and whether banks were more likely to fail if they were located in more contestable markets.

6.1 Loans, deposits, and total assets

To study the effect on loan growth more formally, we again estimate local linear regressions, now at the incumbent bank-level:

$$y_{bt} = \alpha + \beta_1 \cdot \mathbb{1}_{ct}^{\text{pop}>6{,}000} + \beta_2 \cdot (\text{pop}_{ct} - 6{,}000) + \beta_3 \cdot \mathbb{1}_{ct}^{\text{pop}>6{,}000} \cdot (\text{pop}_{ct} - 6{,}000) + \varepsilon_{bt}, \tag{3}$$

where y_{bt} is a bank-level outcome variable such as the growth rate of loans, deposits or assets in the ten years following the publication of the census of year t, and pop_{ct} and $\mathbb{1}_{ct}^{\text{pop}>6{,}000}$ are defined as above. Note that, also as above, we estimate the model using various bandwidth selection methods, kernel functions, and allowing the slope of the running variable to be linear as well as quadratic. Moreover, we include dummies for whether a city is located in the South or the West as controls. Further, given that banks in towns right of the cutoff tend to be larger, we also control for bank size using the covariate-adjustment approach developed by Calonico et al. (2019).

Table 4 reports results. In column (1) we calculate two different MSE-optimal bandwidths left and right of the cutoff. Here it is optimal to use towns with a population between 4,100 and 8,200 to estimate the effect of higher entry barriers on loan growth. The estimates indicate that loan growth is around 13 percentage points lower in the 10 years that followed the census publication if a bank operates in a market with high entry barriers. The difference is substantial given the unconditional ten-year growth rate of 27 percentage points. Further, columns (2) to (5) show that this is effect is estimated with the same magnitude and precision when varying the bandwidth selection method, kernel function, and the functional form of the running variable. Note across specifications, all bias-corrected results are significant at the 5% level, and all robust results at least at the 10% level.

[TABLE 4 ABOUT HERE]

Further, we investigate whether the additional loan growth in markets with lower entry barriers was financed by an expansion of the banks' balance sheet or by substitution of liquid funds into illiquid loans. To the extent that loan growth is driven by an expansion of the balance sheet, we can study whether additional loans are financed by raising additional equity or by expanding the deposit base. To understand this, we estimate Equation (3) using the growth of equity, deposits, reserves, cash, national bank notes, and total assets as outcome variables.[23]

[TABLE 5 ABOUT HERE]

In line with the lower credit provision in markets with higher entry barriers, Table 5 shows that these banks also have 11 percentage points lower growth in deposits and around 6-7 percentage points lower growth in overall assets, although the latter is statistically insignificant. There is also no statistically significant difference in the growth of reserves, cash, or equity. Hence, the lower credit provision of banks in cities with higher entry barriers coincides with a contraction of the banks' deposit base and overall assets rather than a decrease in equity finance or an increase in liquid assets.

Notably, there are also no differences in national bank note issuance. If at all, there are less notes issued right of the cutoff. This finding further emphasizes that our results are unlikely driven by

[23]Bank equity is defined as the sum of paid-in capital (regulatory capital) and the surplus fund. Reserves are defined as the sum of cash and due from reserve agents. Cash is the sum of specie, fractional currency and coins, and legal-tender notes.

national banks facing competition from state banks right of the cutoff. Recall that one of the major differences between national and state banks is that national banks can, after purchasing government bonds, issue bank notes to circulate. If competition with state banks would be driving the results, one would expect that national banks make relatively more use of their comparative advantages such as note issuance—a low-risk, low-return activity. However, we do not observe that national banks to the right of the cut-off differ in their degree of note issuance, see column (5). This further emphasizes that our results are more likely to be driven by a lower degree of market contestability right of the cutoff, rather than an increase in competition from state banks.

Naturally, it is of interest to learn more about the mechanism giving rise to this differential behavior. One possibility is that incumbent banks expanded their lending only in those markets that experienced actual entry as banks competed over market share. Alternatively, the additional credit provision could have resulted from incumbents being more expansive in an attempt to deter potential entrants—a possibility suggested by classic theories of entry deterrence in firm competition (see, e.g., Dixit, 1979; Milgrom and Roberts, 1982a,b; Klemperer, 1987).

In order to shed light on this question, we estimate Equation (3) with a reduced sample consisting only of cities in which no additional bank–national or state–entered between in the decade after a census publication. Restricting the sample in this way leaves us with 1,741 cities in which the number of competing banks is unchanged. Studying bank behavior in this restricted sample allows us to investigate whether entry barriers determine bank behavior alone or whether entry barriers determine bank behavior only through determining actual entry. In particular, observing differential behavior across markets with different entry barriers but with no changes in the number of competitions can be taken as evidence that entry barriers can alone determine bank behavior.

The results are shown in columns (6) and (7) of Table 4. We find that the effects of an increase in entry barriers on loan growth are robust when narrowing the sample to this specific subset of markets. If at all, the effects are even stronger in magnitude. These results indicate that there was a larger credit expansion by incumbent banks in markets with lower entry barriers even when there had not been any additional entrants. We interpret this evidence as consistent with the idea that incumbent banks, once they learn that entry barriers have increased, provide less credit and demand less deposits as the necessity to deter potential entrants by increasing market shares has been reduced. This interpretation is further supported by a dynamic analysis of the effect of the census publication around the 1880 census (Section C in the Appendix). When studying loan growth dynamics across markets with different entry barriers, we find that loan growth by banks in less competitive markets slows down immediately after the census publication. This is surprising, as actual entry only occurs over time and is consistent with entry deterrence driving bank behavior.

Altogether, the evidence in this section suggests that higher entry barriers lead to a lower degree of credit provision. Moreover, the incumbent bank behavior seems to be largely governed by changes in the probability of potential entry rather than being driven by actual entry. This is consistent with results in other industries and time periods, such as Goolsbee and Syverson (2008), who find that pricing in the airline industry is partly driven by attempting to deter entrants. Thus, it appears that the phenomena of entry deterrence is an important driver of firm behavior not only in non-financial but also in financial industries.[24]

6.2 Risk-taking and bank failures

To study the effect of competition on risk-taking and financial stability, we start by exploring two balance sheet ratios correlated with distance to default and ex-ante risk-taking: the equity-assets ratio and the equity-loans ratio, as loans are typically a bank's riskiest asset component. Assuming equally risky loan portfolios across banks, larger equity buffers relative to loans indicate that the bank was pursuing a more conservative investment strategy.

In particular, we estimate Equation (3) using various balance sheet ratios as dependent variables and two different MSE-optimal bandwidths on both sides of the cutoff. The results are reported in Table 6. We find that incumbent national banks in markets with higher entry barriers had a 4-5 percentage points higher equity-to-asset ratio and a 12-15 percentage points higher ratio of equity to loans, see columns (1)-(2). These findings suggest that incumbent banks in markets with higher entry barriers had more conservative business models. This is in line with the fact that the credit expansion in markets with lower entry barriers was financed by raising deposits rather than raising

[24] The finding that our results are robust to considering only markets in which no entry occurs also partially addresses the concern that results may not be driven by competition per se, but by whether the entrant is more or less likely to be a large bank; Entrants right of the cutoff are on average larger due to a higher required minimum capital so incumbents to the right of the cutoff may be reacting to having larger competitors. While this is generally a plausible argument when considering *actual* entry, it becomes a somewhat less plausible argument when considering *potential* entry as we observe that actual entrants to the left of the cutoff can also be large entrants with a high regulatory amount of capital paid-in (as evidenced in Figure D.1). Ultimately though, we cannot empirically distinguish whether incumbent behavior right of the cutoff is rather a response to a lower probability of getting an additional competitor or a reaction to the prospect of a relatively larger competitor.

equity, which increases leverage mechanically in absence of equity or notes issuance.

In addition, we find that incumbent banks with more market power after a census publication also had a lower deposits-to assets ratio (column (3)) but no difference in the cash-to-loans ratio (column (4)), which together indicate a relatively more conservative funding structure and less liquidity mismatch. Further, banks in less contested markets also maintained a 21 percentage points higher reserve-to-required-reserves ratio (see column (5)), another indication that these banks were taking less risk, though the t-statistics are close to but never in excess of 1.66.

[TABLE 6 ABOUT HERE]

Overall, the results on balance sheet ratios provide suggestive evidence that institutions in areas with lower entry barriers behaved in a riskier manner than institutions in areas with higher barriers. To provide additional corroborating evidence, we study alternative measures that can be seen as ex-post measures of risk-taking. On the asset side, we measure ex-post asset quality through banks' holdings of real estate that banks obtained when loans went bad, referred to as "other real estate and mortgages owned" (OREO). Assuming that banks have similar collateral requirement across markets, higher OREO holdings are indicative of a bank that had previously made riskier loans and had to seize collateral when the borrower defaulted.

On the liability side, we study differences in the use of bills payable and rediscounts. These instruments are indicative of a riskier funding base as they were short-term, high-interest-rate, secured transactions to which banks turned when other sources of funding were scarce; we test whether banks in more competitive environments were more or less likely to use these particular liabilities.[25]

Since both these variables have relatively skewed distributions, we calculate the outcomes variable as an indicator on whether OREO is held or rediscounts are used. Further note that OREO is only reported from 1891 onwards and thus we can only analyze the effect of entry barriers on OREO after the 1880 and the 1890 census, but not the 1870 census.

[25]Rediscounts and bills payable are a form of short-term, expensive, secured interbank funding. Banks typically used this form of funding to meet a surge in demand for funds, such as processing the autumn crop harvest; however, a number of studies have also found that this type of funding was used more extensively, and at higher cost, by banks that were experiencing difficulties (White, 1983; Calomiris and Mason, 1997; Calomiris and Carlson, 2018).

[TABLE 7 ABOUT HERE]

The results from estimating Equation (3) are in Table 7. With respect to OREO, columns (1) and (2) show that banks operating in a less competitive market had a 7.5 percentage point lower probability of holding collateral compared to untreated banks, though the coefficient is only significant at the 10% level. This result is consistent with the idea that banks with larger market power chose safer borrowers and were hence less likely to be required to seize collateral in case of default. Similarly, as columns (3) and (4) show, we find some evidence that banks are less likely to make use of costly short-term funding, though the effect is not statistically significant.

Complementary to balance sheet indicators of risk taking, we also analyze bank failure rates.[26] Whether the banks fail in the decade after a census publication naturally provides a further test of the riskiness of their business model. Thus, we construct an indicator variable for whether a receiver was appointed and a bank defaulted some time in the decade after a census publication.[27] We then estimate Equation (3) with the indicator variable of whether a bank failed as dependent variables, again applying various bandwidth selection methods. Columns (5) and (6) of Table 7 reports these results.

The coefficients in columns (5) and (6) indicate that there is a statistically significant difference in the probability of failure of incumbent banks across the different types of markets: incumbent banks in areas with entry higher barriers have around a 5 percentage points lower failure probability, which is considerable given an unconditional default probability of 4 percentage points. I.e., while banks just left of the cutoff fail with more than 5% chance in the decade following a census publication, banks just right of the cutoff essentially have a failure probability that is close to zero.

Altogether, our findings are consistent with the idea that banks in markets with higher entry barriers had a higher charter value and acted in ways to preserve their value, such as by making safer loans and being more cautious when making credit available (Keeley, 1990). In particular, banks with more market power made less loans and took less risk. This, in turn, reduced their probability

[26] Note that the our data covers two important financial crises, the Panic of 1873 and the Panic of 1893. Both were among the most severe financial disturbances of the National Banking Era. The earlier has been attributed to the end of a railroad boom and the latter to concerns about the commitment of the U.S. to the gold standard and the economy (Friedman and Schwartz, 1963; Carlson, 2013). During both panics, there were serious disruptions to the payment system and a significant number of bank closures–some temporary and some permanent. Both panics were followed by severe economic downturns (Romer, 1986; Davis, 2004).

[27] Banks judged by the examiners to be insolvent were placed in receiverships and are considered to have failed.

of failure during times of financial distress and allowed their charters to survive the crisis. Moreover, banks with more market power were less likely to voluntarily wind down their operations during times of distress, further indicating the relative higher value of their charter as they arguably faced prospects of higher future rents.

7 Evidence on real economic outcomes

After studying how competition affects credit availability and risk-taking, this section provides evidence on how it affects real economic output. In particular, we study whether cities that have been subject to higher entry barriers exhibit higher or lower degrees of real economic activity a decade after a census was published. In doing so we build on previous work looking at the role of national banks in fueling development in the National Banking Era, such as Jaremski (2014) and Fulford (2015).[28]

To study the real effects we use data on farm outcomes and manufacturing outcomes provided by the census. In particular, with respect to farming outcomes, we study the effect of entry barriers on the value of farms, the output produced by farms, and the number of farms. With respect to manufacturing outcomes, we study the effect on capital invested, the output value and the establishment counts in the manufacturing industry. We measure all outcome variables normalized by population and thus per capita. Note that these data are only available at the county level and on average we have around 1.7 towns with national banks per county in our sample. Thus, using the real economic activity at the county level is only a proxy for the activity in the respective town.

To test whether entry barriers affect real economic output, we again estimate local linear regressions of the form Equation (1), using the county-level per capita farming or manufacturing outcome as of the next census as the outcome variable. Note that as above, we include controls for whether a city is located in the West or the South.

[TABLE 8 ABOUT HERE]

[28]Jaremski (2014) uses institution level data on banks and county level data on manufacturing; identification in his setup comes from looking at a shock in the mid-1860s just as the country is returning to peace-time footing after the Civil War. By comparison, we are looking at a later period in which development is further along and less likely to be complicated by the end of the Civil War. Fulford (2015) looks at county-level bank data and manufacturing. He uses a similar identification strategy that focuses on the margin whether a town a receives a national bank or not rather than studying the competition among incumbent banks.

The regression results confirm that areas with lower entry barriers—which had higher growth in credit, but also more bank failures—tended to have more real economic activity ten years later. Considering farming outcomes, results in Table 8 indicate that cities with higher barriers to entry for national bank after a census publication are in counties that exhibit a lower per capita farming value and farming output ten years later. In particular, the per capita ratio of the value of output of farms falls by around 15-18 dollars when a town was subject to higher entry barrier—a considerable effect given the average ratio of around 60 dollars. In addition, the number of farms per capita is also estimated to be lower in areas with higher entry barriers. This indicates that a decrease in competition in the banking sector might also decrease competition in the non-financial sector. This is in line with existing evidence from Black and Strahan (2002) and Cetorelli and Strahan (2006).

The findings on manufacturing outcomes are not as clear cut. Table 9 shows results from estimating Equation (1) when only using outcomes only after the 1880 and 1890 census, i.e., when excluding the 1870's. Here we find that towns that had been subjected to higher entry barriers for national banks have a lower per capita manufacturing capital ten years later. This finding are largely in line with the evidence provided by Jaremski (2014) that suggests that areas more conducive to national bank entry tended to have faster manufacturing growth. However, there is no effect on manufacturing output nor is there an effect on the number of manufacturing establishments. Further, note that the results are not robust to including the 1870's, as is evidenced in Table E.2 in the Appendix. This is arguably driven that during the 19th century the majority of U.S. GDP is still stemming from agricultural output and manufacturing as an industry only becomes more important towards the end of the 19th century.

[TABLE 9 ABOUT HERE]

Altogether, our findings show that the credit growth induced from lower entry barriers matters for real economic outcomes and hence replicates contemporary findings (see, e.g., King and Levine, 1993; Levine and Zervos, 1998). This is important, as it points to a more general tension associated with a more competitive environment: In Section 6, we observed that banking competition leads to an increase in credit growth, risk taking, and ultimately bank failures. Here we find it is also associated with higher real economic growth. Thus, we provide evidence that a credit boom stemming from increased competition among banks causes real economic growth but at the same time leads to a buildup of financial fragility.

8 Conclusion

How does competition in banking affect credit provision and financial stability? How does it affect real economic outcomes? This paper tackles these important questions by providing evidence from the National Banking Era. Our empirical setting has two advantages over the existing empirical literature that allow us to broaden the understanding of the effect of banking competition. First, peculiarities of the National Banking Era capital requirements allow us to get a cleaner identification of the causal effects of competition. Second, studying bank behavior during the National Banking Era allows us to study the behavior of financial intermediaries in the absence of government backstops such as a lender of last resort and deposit insurance.

Our findings suggest that, in such an environment, banks provide more credit in markets with lower entry barriers. Moreover, we find that banks seem to do so in response to potential entry by competitors, possibly as a means of deterring entry. Such behavior resembles the behavior found for firms in different industries, in different times (see, e.g., Goolsbee and Syverson, 2008) and highlights the importance of entry barriers as a driver of behavior.

Further, we find evidence that more competitive environments may be both areas of greater credit availability that supports economic growth and areas of greater risk-taking associated with financial instability. Hence, our paper allows us to interpret evidence on credit booms and busts (Rancière et al., 2008; Reinhart and Rogoff, 2009; Schularick and Taylor, 2012) in a causal sense, where credit causes both growth (see, e.g., King and Levine, 1993; Levine and Zervos, 1998; Chodorow-Reich, 2014; Benmelech et al., 2017) and financial instability.

Further, our evidence also suggests that charter values play an important role in governing bank behavior (Keeley, 1990). Charter values may have been particularly important and influential in the time period considered in this paper with its relatively light level of regulation. Nevertheless, understanding how charter values shaped bank behavior in the National Banking Era may provide useful insights into how financial institutions behave today in the less regulated shadow banking system. In particular, our findings imply that regulatory policies that affect the charter values of less regulated financial intermediaries may in turn shape how much credit these institutions extend and how much risk that they are willing to take.

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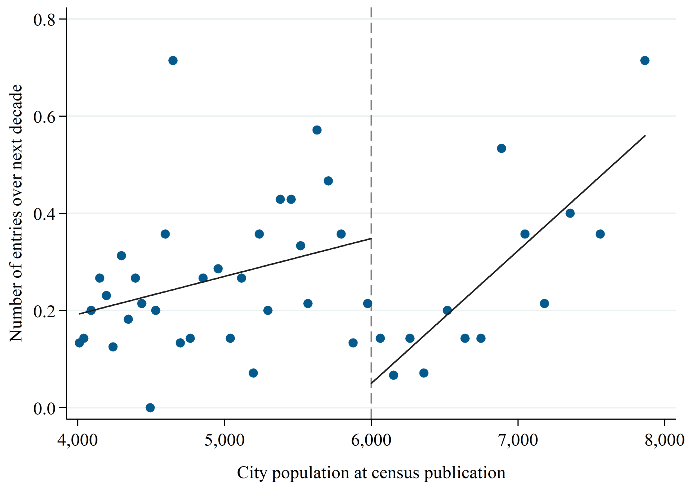
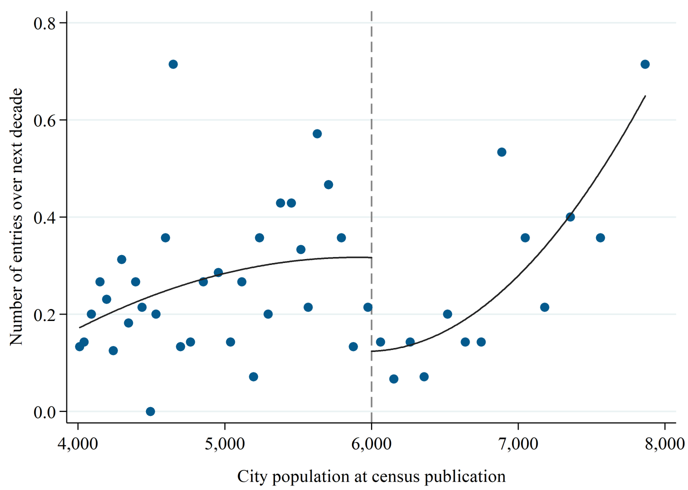
9 Figures and Tables

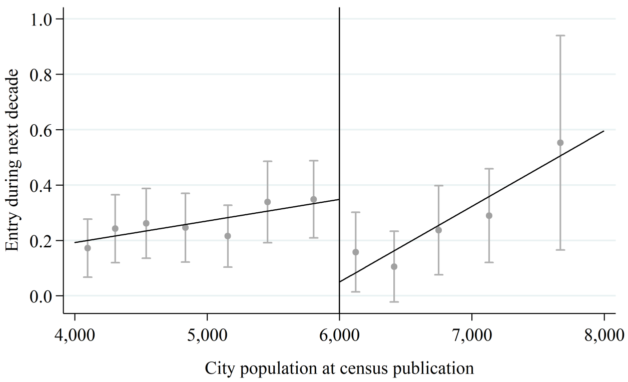


(a)

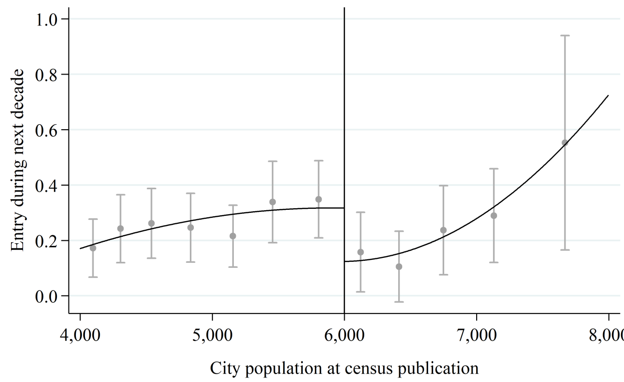


(b)

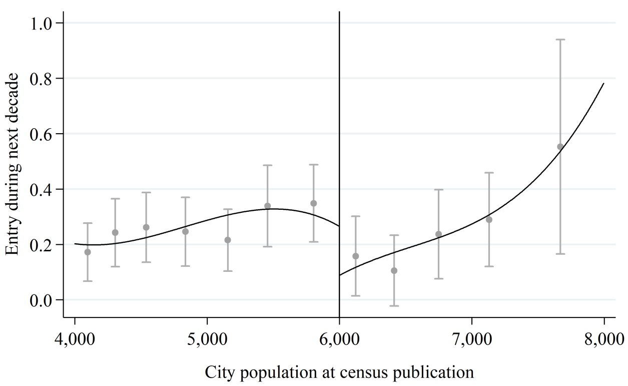
Figure 1: ***National bank entry after census publications.*** *Binned scatterplot of the number of national bank entrants over the decade following a census publication by city population at census publication. The figure pools data from the publications of the 1870, 1880, and 1890 census. This figure is created with the* `binscatter` *package (Stepner, 2013), using equal-sized bins with around 15 observations per bin, plus linear (panel (a)) and quadratic (panel (b)) fit lines left and right of the 6,000 population threshold.*



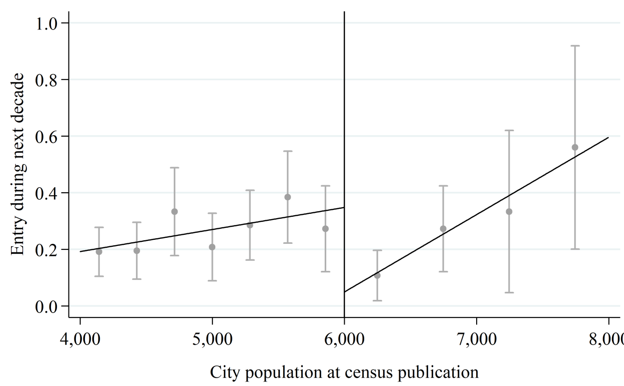
(a) *First-order polynomial; quantile–spaced*



(b) *Second-order polynomial; quantile–spaced*

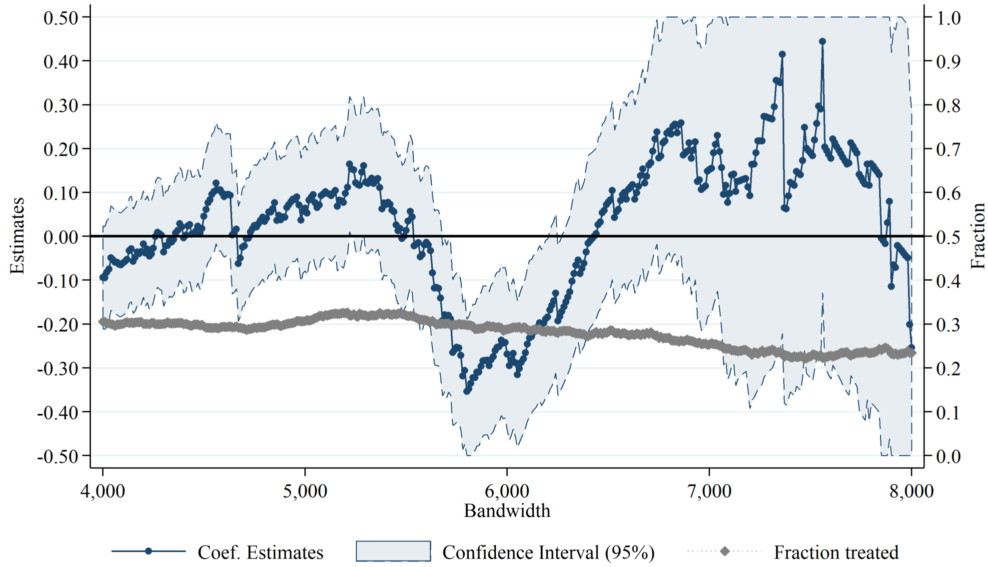


(c) *Third-order polynomial; quantile–spaced*

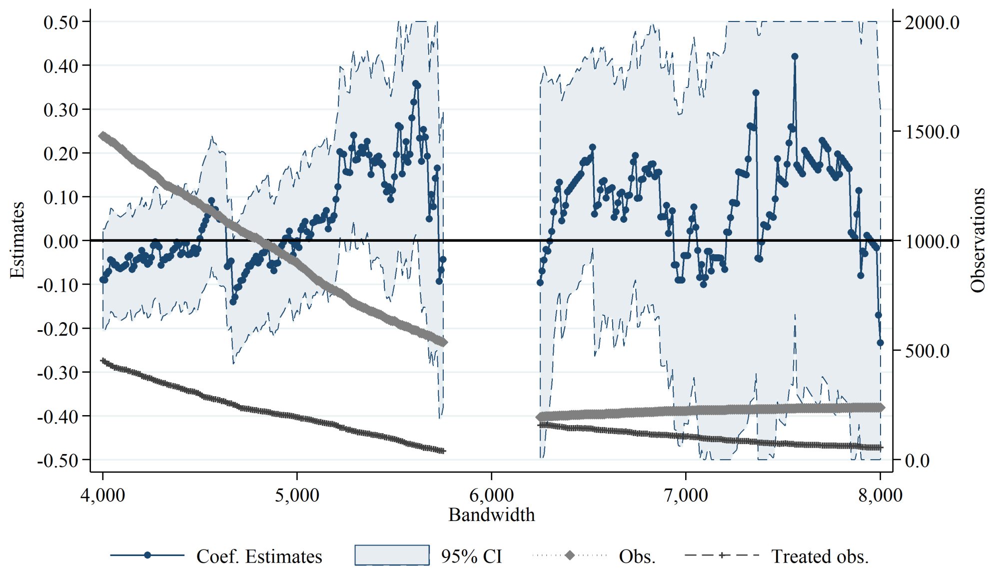


(d) *First-order polynomial; evenly–spaced*

Figure 2: ***Bank entry after census publications - varying choice of polynomial and bin selection method.*** *This figure shows regression discontinuity plots constructed with the optimal data–driven methods of Calonico et al. (2015), created through the* `rdplot` *package (Calonico et al., 2017). These methods automatically select the number of bins as well as the spacing between them, thus avoiding the need for potentially subjective and ad–hoc tuning parameters. In all cases, visual results suggest the presence of a discontinuity around the 6,000 population threshold, with fewer banks opening right of the threshold. Panels (a)-(c) show quantile–spaced bins with varying polynomial fits. Using quantile-spaced bins has the advantage of taking into account the increasing sparsity of the data as the population size increases. Panels (d) shows similar results when using equally-spaced bins. The figure pools data from the the publications of the 1870, 1880, and 1890 census. The running variable is the population of each town at each respective census publication; and the dependent variable is the number of new entrants in the decade following a respective census publication. Confidence bands at the 95% level.*



(a) *The figure plots the coefficient on varying cutoff dummies* $\mathbb{1}_{ct}^{pop>X}$*, with X varying from 4,000 to 8,000 in incremental steps of 10, when estimating Equation* (1) *(left y-axis) and the fraction of observations that are right of the varying cutoff (right y-axis).*



(b) *The figure plots the coefficient on varying cutoff dummies* $\mathbb{1}_{ct}^{pop>X}$*, when estimating Equation* (1) *with X varying from 3,000 to 5,750 and from 6,250 to 9,000, each in incremental steps of 10 (left y-axis) and the total and treated number of observations (right y-axis). When varying the cutoff between 3,000 to 5,750, we exclude all cities with more than 6,000 inhabitants. When varying the cutoff between 6,250 to 9,000, we exclude all cities with less than 6,000 inhabitants.*

Figure 3: ***Validation and falsifications test: sensitivity of the effect on bank entry to varying cutoffs.*** *In both panels, the dependent variable is the number of national bank entrants in the decade following a census publication. We use fixed bandwidths of +/-2,000 to ensure comparability across estimations and construct estimators using an epanechnikov kernel. As covariates, we include a dummy for whether the bank is located in the West or the South. Standard errors are clustered at the city level and 95% confidence bands.*

Table 1: *Descriptive statistics I*— *Observable characteristics for towns with more than 5,000 and less than 7,000 inhabitants as of the publication of the 1870, 1880, and 1890 census.*

	Population $\leq$ 6000			Population $>$ 6000			Difference	
	Mean	Std	N	Mean	Std	N	Diff	t-stat
Population	5,471.7	275.5	197	6,439.1	295.8	121	967.4	29.553
Δ Population during previous decade	57.8	95.1	197	68.8	116.8	121	10.9	0.913
Δ_{harm} Population during previous decade	35.0	30.2	197	39.5	31.9	121	4.5	1.265
Number of National banks	1.6	0.7	197	1.7	0.7	121	0.1	0.893
Bank entries in previous decade	0.8	0.8	197	0.8	0.9	121	-0.0	-0.172
Δ Capital during previous decade	15.7	42.1	105	15.5	54.5	73	-0.2	-0.030
Δ Loans during previous decade	44.3	48.1	105	45.3	57.6	73	1.0	0.126
Δ Assets during previous decade	24.6	40.9	105	28.7	46.8	73	4.1	0.620
Per capita bank capital	39.0	27.5	197	38.4	29.7	121	-0.6	-0.181
Per capita bank loans	63.8	48.5	197	63.0	44.8	121	-0.8	-0.145
Per capita bank assets	119.3	78.6	197	117.1	74.3	121	-2.2	-0.246
Number of manufacturing establishments	393.8	538.6	195	489.7	844.7	120	96.0	1.232
Per capita manufacturing capital	83.4	81.3	195	95.5	98.4	120	12.1	1.187
Per capita farm value	341.7	190.7	195	304.4	203.2	120	-37.3	-1.643
Number of farms	3,021.2	1,440.3	195	2,661.7	1,462.1	120	-359.5	-2.139
Years of railroad access	28.3	13.1	197	28.4	14.0	121	0.2	0.106
Railroad access	1.0	0.2	197	1.0	0.1	121	0.0	0.768
Number of railroad connections	6.0	4.3	196	6.3	4.2	121	0.3	0.643
Distance to New York City (in km)	849.7	702.9	197	853.1	682.3	121	3.4	0.042
Distance to next city with more than 50k inhabitants	92.5	77.0	197	100.8	98.2	121	8.3	0.841
Distance to next populated location	9.7	9.4	197	11.1	9.5	121	1.5	1.344

City-level data for towns that have more than 5,000 and less than 7,000 inhabitants as of the 1870, 1880, and 1890 census. Data is restricted to cities with at least one national bank at the respective census publication and less than 6,000 inhabitants in the respective previous census.
Δy describes a log growth rate, i.e., $\Delta y = log(y_t) - log(y_{t-1})$; Δ_{harm} describes a harmonized growth rate, i.e., $\Delta y = (y_t - y_{t-1})/(0.5 * (y_t + y_{t-1}))$.
Capital, total assets, and loans are from national banks only. Manufacturing and farming outcomes are per capita at the county-level.

Table 2: ***Descriptive statistics II***— *Bank-level data for incumbent national banks in towns that have more than 5,000 and less than 7,000 inhabitants as of the 1870, 1880, and 1890 census.*

	Population ≤ 6000			Population > 6000			Difference	
	Mean	Std	N	Mean	Std	N	Diff	t-stat
Total assets in (in th)	412.4	214.2	308	458.6	241.2	199	46.2	2.257
Capital paid in	108.1	58.5	308	119.1	78.7	199	11.0	1.807
Surplus fund	26.8	25.7	308	31.9	31.0	199	5.0	1.976
Deposits	189.7	131.5	308	209.3	142.3	199	19.7	1.593
National bank notes	63.1	57.1	308	70.6	68.7	199	7.5	1.338
Cash (specie and legal tender)	24.1	19.0	308	25.9	17.5	199	1.8	1.098
Liquid assets	71.3	56.9	308	79.2	63.1	199	8.0	1.472
Loans and discounts	221.0	127.7	308	246.8	151.2	199	25.8	2.061
Debt/Assets	66.0	10.1	308	66.6	10.9	199	0.6	0.685
Equity/Assets	34.0	10.1	308	33.4	10.9	199	-0.6	-0.685
Capital/Assets	28.0	9.9	308	26.9	10.1	199	-1.1	-1.237
Loans/Assets	53.7	14.1	308	53.7	14.7	199	-0.0	-0.002
Deposits/Assets	44.8	17.0	308	45.6	18.5	199	0.8	0.507
Cash/Assets	6.0	3.6	308	5.9	3.6	199	-0.2	-0.495
Liquid Assets/Assets	17.1	8.9	308	17.0	9.2	199	-0.1	-0.126
Reserves/(Required reserves)	253.9	236.2	308	232.6	143.1	199	-21.3	-1.146
Turnover in bank cashier (1872-1881)	9.7	29.7	226	9.5	29.5	105	-0.2	-0.060
Turnover in bank president (1872-1881)	8.5	27.9	224	4.7	21.3	106	-3.8	-1.229
President and cashier are family (1872-1881)	9.1	28.8	232	7.3	26.1	110	-1.8	-0.550
Age	12.1	8.3	308	12.7	8.1	199	0.5	0.703

Data restricted to information on incumbent national banks at the publication of the 1870, 1880, and 1890 census and to national banks that are located in cities with less than 6,000 inhabitants as of the respective previous census.

Table 3: ***Entry*** *— city-level evidence on entries of national banks in the respective decade following a census publication.*

Dependent Variable	Number of new national bank entrants						State bank entrants	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conventional	-0.26***	-0.25***	-0.28***	-0.26***	-0.30***	-0.27***	0.13	0.10
	[0.08]	[0.08]	[0.09]	[0.09]	[0.11]	[0.10]	[0.20]	[0.22]
Bias-corrected	-0.27***	-0.26***	-0.30***	-0.28***	-0.31***	-0.27***	0.10	0.07
	[0.08]	[0.08]	[0.09]	[0.09]	[0.11]	[0.10]	[0.20]	[0.22]
Robust	-0.27***	-0.26***	-0.30***	-0.28***	-0.31***	-0.27**	0.10	0.07
	[0.10]	[0.10]	[0.10]	[0.11]	[0.12]	[0.11]	[0.23]	[0.25]
BW Type	MSE Two	MSE Two	MSE Two	MSE Common	MSE Two	MSE Two	MSE Two	MSE Common
Kernel Type	Epanechnikov	Triangular	Uniform	Epanechnikov	Epanechnikov	Triangular	Epanechnikov	Epanechnikov
Order Loc. Poly. (p)	1	1	1	1	2	2	1	1
Order Bias (q)	2	2	2	2	3	3	2	2
Mean dep. var.	0.21	0.21	0.21	0.21	0.21	0.21	0.19	0.19
Num. counties	1,040	1,040	1,040	1,040	1,040	1,040	486	486
Num. cities	1,696	1,696	1,696	1,696	1,696	1,696	849	849
Observations	2,864	2,864	2,864	2,864	2,864	2,864	1,862	1,862
Obs. left of cutoff	2,579	2,579	2,579	2,579	2,579	2,579	1,727	1,727
Obs. right of cutoff	285	285	285	285	285	285	135	135
Left main bandwidth (h)	2,523	2,800	1,762	1,693	2,706	2,681	2,306	1,492
Right main bandwidth (h)	1,422	1,660	1,964	1,693	3,059	4,336	1,453	1,492
Effective obs. (left)	635	771	373	352	711	702	433	224
Effective obs. (right)	154	167	181	169	226	247	86	89

This table shows results from estimating a local linear regression of the form:

$$y_{ct} = \alpha + \beta_1 \cdot \mathbb{1}_{ct}^{\text{pop}>6,000} + \beta_2 \cdot (\text{pop}_{ct} - 6,000) + \beta_3 \cdot \mathbb{1}_{ct}^{\text{pop}>6,000} \cdot (\text{pop}_{ct} - 6,000) + \varepsilon_{ct},$$

where y_{ct} is either the number of national bank entries (columns (1) through (6)) or state bank entries (columns (7) and (8)) in the decade following the publication of the census from year $t \in \{1870, 1880, 1890\}$. We vary bandwidth selection methods and kernel functions (epanechnikov and triangular) across specifications. In particular, in column (1)-(3) and (5)-(7) we use two different MSE-optimal bandwidths selectors (below and above the cutoff) for the RD treatment effect estimator. In columns (4) and(8), we use a common MSE-optimal bandwidth selector for the RD treatment effect estimator. In columns (5) and (6), the running variable is quadratic as opposed to linear. Optimal bandwidth calculated using the `rdrobust` package (Calonico et al., 2017). As covariates, we include a dummy for whether the bank is located in the West or the South using the covariate-adjustment approach developed by Calonico et al. (2019). City-level data from the publication of the 1870, 1880, and 1890 census. Data is restricted to cities with at least one national bank at the respective census publication and less than 6,000 inhabitants in the respective previous census. Standard errors in parentheses; *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Table 4: ***Credit*** **—** *bank-level evidence on incumbent bank loan growth in the decade following a census publication.*

Dependent Variable	Δ Loans						
Sample	All cities					No new entrants	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Conventional	-10.10	-10.52*	-13.35**	-12.13*	-12.50*	-15.89**	-16.33**
	[6.27]	[6.22]	[6.76]	[7.20]	[7.20]	[7.18]	[7.21]
Bias-corrected	-12.70**	-13.01**	-15.31**	-14.05*	-14.27**	-17.81**	-18.56**
	[6.27]	[6.22]	[6.76]	[7.20]	[7.20]	[7.18]	[7.21]
Robust	-12.70*	-13.01*	-15.31*	-14.05*	-14.27*	-17.81**	-18.56**
	[7.11]	[7.09]	[7.83]	[7.96]	[7.94]	[8.35]	[8.38]
BW Type	MSE Two	MSE Two	MSE Common	MSE Two	MSE Two	MSE Two	MSE Two
Kernel Type	Epanechnikov	Triangular	Epanechnikov	Epanechnikov	Triangular	Epanechnikov	Triangular
Order Loc. Poly. (p)	1	1	1	2	2	1	1
Order Bias (q)	2	2	2	3	3	2	2
Mean dep. var.	27.02	27.02	27.02	27.02	27.02	23.82	23.82
Num. counties	1,043	1,043	1,043	1,043	1,043	797	797
Num. cities	1,703	1,703	1,703	1,703	1,703	1,305	1,305
Num. banks	2,391	2,391	2,391	2,391	2,391	1,741	1,741
Observations	3,104	3,104	3,104	3,104	3,104	2,494	2,494
Obs. left of cutoff	2,670	2,670	2,670	2,670	2,670	2,206	2,206
Obs. right of cutoff	434	434	434	434	434	288	288
Left main bandwidth (h)	1,912	2,156	1,741	3,039	3,271	1,877	1,954
Right main bandwidth (h)	2,209	2,385	1,741	3,940	4,036	1,525	1,678
Effective obs. (left)	537	646	471	1,112	1,249	415	430
Effective obs. (right)	285	296	256	349	354	189	194

This table presents results from estimating

$$y_{bt} = \alpha + \beta_1 \cdot \mathbb{1}_{ct}^{\text{pop}>6{,}000} + \beta_2 \cdot (\text{pop}_{ct} - 6{,}000) + \beta_3 \cdot \mathbb{1}_{ct}^{\text{pop}>6{,}000} \cdot (\text{pop}_{ct} - 6{,}000) + \varepsilon_{bt},$$

where y_{bt} is bank b's loan growth over the ten years after the publication of the census from year $t \in \{1870, 1880, 1890\}$. We vary bandwidth selection methods and kernel functions (epanechnikov and triangular). In particular, in columns (1),(2),(4)-(7) we use two different MSE-optimal bandwidths selectors (below and above the cutoff) for the RD treatment effect estimator. In column (3), we use a common MSE-optimal bandwidth selector for the RD treatment effect estimator. Optimal bandwidth calculated using the `rdrobust` package (Calonico et al., 2017). In columns (4) and (5), the running variable is quadratic as opposed to linear. In columns (6) and (7) the sample is restricted to cities with no new national bank entrants over the decade following the respective census. As covariates, we include a dummy for whether the bank is located in the West or the South as well as the bank size and bank loans (both in log) at the census publication using the covariate-adjustment approach developed by Calonico et al. (2019). Bank-level data from incumbent national banks at the publication of the 1870, 1880, and 1890 census. The sample is restricted to banks in towns with a population of less than 6,000 inhabitants as of the respective previous census. Standard errors in parentheses; *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Table 5: ***Equity, deposits, cash, reserves, notes, and assets*** — *bank-level evidence on growth rates of various balance sheets items in the decade following a census publication.*

Dependent Variable	Δ Capital	Δ Deposits	Δ Cash	Δ Reserves	Δ National bank notes	Δ Assets
	(1)	(2)	(3)	(4)	(5)	(6)
Conventional	-4.46	-10.34*	1.04	-4.36	-14.79	-4.80
	[4.11]	[6.16]	[9.17]	[9.94]	[12.55]	[5.13]
Bias-corrected	-5.71	-11.98*	1.77	-8.26	-18.08	-6.11
	[4.11]	[6.16]	[9.17]	[9.94]	[12.55]	[5.13]
Robust	-5.71	-11.98*	1.77	-8.26	-18.08	-6.11
	[4.70]	[7.17]	[10.91]	[11.53]	[14.28]	[5.99]
BW Type	MSE Two	MSE Two	MSE Two	MSE Two	MSE Two	MSE Two
Kernel Type	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov
Order Loc. Poly. (p)	1	1	1	1	1	1
Order Bias (q)	2	2	2	2	2	2
Mean dep. var.	-1.45	51.00	20.90	38.74	1.19	34.25
Num. counties	1,043	1,043	1,043	1,043	1,043	1,043
Num. cities	1,703	1,703	1,703	1,703	1,703	1,703
Num. banks	2,391	2,391	2,391	2,391	2,391	2,391
Observations	3,104	3,102	3,104	3,104	3,094	3,104
Obs. left of cutoff	2,670	2,668	2,670	2,670	2,661	2,670
Obs. right of cutoff	434	434	434	434	433	434
Left main bandwidth (h)	1,327	2,387	2,112	1,807	1,212	1,537
Right main bandwidth (h)	1,962	2,583	2,561	2,460	3,239	2,261
Effective obs. (left)	326	743	623	500	300	402
Effective obs. (right)	267	301	299	298	332	291

The table presents results from estimating

$$y_{bt} = \alpha + \beta_1 \cdot \mathbb{1}_{ct}^{\text{pop}>6{,}000} + \beta_2 \cdot (\text{pop}_{ct} - 6{,}000) + \beta_3 \cdot \mathbb{1}_{ct}^{\text{pop}>6{,}000} \cdot (\text{pop}_{ct} - 6{,}000) + \varepsilon_{bt},$$

where y_{bt} is bank b's growth deposits, assets, etc. over the ten years following the publication of the census from year t census. Throughout all specifications, we use two different MSE-optimal bandwidths selectors (below and above the cutoff) to construct the RD treatment effect estimator. Optimal bandwidth calculated using the `rdrobust` package (Calonico et al., 2017). As covariates, we include a dummy for whether the bank is located in the West or the South as well as the bank size and bank loans (both in log) at the census publication using the covariate-adjustment approach developed by Calonico et al. (2019). Bank-level data from incumbent national banks at the publication of the 1870, 1880, and 1890 census. The sample is restricted to banks in towns with a population of less than 6,000 inhabitants as of the respective previous census. Standard errors in parentheses; *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Table 6: ***Risk taking I***— *bank-level evidence on bank balance sheets characteristic ten years after a census publication.*

Dependent Variable	$\frac{\text{Equity}}{\text{Assets}}$	$\frac{\text{Equity}}{\text{Loans}}$	$\frac{\text{Deposits}}{\text{Assets}}$	$\frac{\text{Cash}}{\text{Loans}}$	$\frac{\text{Reserves}}{\text{Required Reserves}}$
	(1)	(2)	(3)	(4)	(5)
Conventional	4.20*	11.86**	-4.72**	-1.07	17.60
	[2.16]	[5.52]	[2.25]	[1.31]	[13.64]
Bias-corrected	4.95**	14.01**	-5.41**	-1.25	21.71
	[2.16]	[5.52]	[2.25]	[1.31]	[13.64]
Robust	4.95**	14.01**	-5.41**	-1.25	21.71
	[2.51]	[6.33]	[2.63]	[1.55]	[15.77]
BW Type	MSE Two	MSE Two	MSE Two	MSE Two	MSE Two
Kernel Type	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov
Order Loc. Poly. (p)	1	1	1	1	1
Order Bias (q)	2	2	2	2	2
Mean dep. var.	36.02	60.87	58.81	10.97	226.14
Num. counties	1,043	1,043	1,043	1,043	1,043
Num. cities	1,709	1,709	1,709	1,709	1,709
Num. banks	2,401	2,401	2,401	2,401	2,401
Observations	3,188	3,188	3,188	3,188	3,182
Obs. left of cutoff	2,745	2,745	2,745	2,745	2,739
Obs. right of cutoff	443	443	443	443	443
Left main bandwidth (h)	1,595	1,660	1,562	1,497	2,357
Right main bandwidth (h)	2,694	2,563	2,674	3,470	3,117
Effective obs. (left)	442	465	427	401	759
Effective obs. (right)	315	306	314	344	333

This table presents results from estimating

$$y_{bt} = \alpha + \beta_1 \cdot \mathbb{1}_{ct}^{\text{pop}>6,000} + \beta_2 \cdot (\text{pop}_{ct} - 6,000) + \beta_3 \cdot \mathbb{1}_{ct}^{\text{pop}>6,000} \cdot (\text{pop}_{ct} - 6,000) + \varepsilon_{bt},$$

where y_{bt} is a ratio of balance sheets items of bank b' ten years after the publication of the census from year $t \in \{1870, 1880, 1890\}$. We calculate two different MSE-optimal bandwidths (below and above the cutoff) for the RD treatment effect estimator across all specifications. Optimal bandwidth calculated using the `rdrobust` package (Calonico et al., 2017). As covariates, we include a dummy for whether the bank is located in the West or the South as well as the bank size (in log) at the census publication using the covariate-adjustment approach developed by Calonico et al. (2019). Bank-level data from incumbent national banks at the publication of the 1870, 1880, and 1890 census. The sample is restricted to banks in towns with a population of less than 6,000 inhabitants as of the respective previous census. Standard errors in parentheses; *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Table 7: ***Risk taking II***— *bank-level evidence on seized collateral (OREO), emergency funding and default in the decade following a census publication.*

Dependent Variable	OREO		Rediscounts		Default	
	(1)	(2)	(3)	(4)	(5)	(6)
Conventional	-0.07*	-0.07	-0.02	-0.03	-0.05***	-0.06***
	[0.04]	[0.04]	[0.03]	[0.03]	[0.02]	[0.02]
Bias-corrected	-0.07*	-0.07	-0.03	-0.04	-0.05***	-0.06***
	[0.04]	[0.04]	[0.03]	[0.03]	[0.02]	[0.02]
Robust	-0.07	-0.07	-0.03	-0.04	-0.05***	-0.06***
	[0.05]	[0.05]	[0.03]	[0.03]	[0.02]	[0.02]
BW Type	MSE Two	MSE Common	MSE Two	MSE Common	MSE Two	MSE Common
Kernel Type	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov
Order Loc. Poly. (p)	1	1	1	1	1	1
Order Bias (q)	2	2	2	2	2	2
Mean dep. var.	0.07	0.07	0.05	0.05	0.03	0.03
Num. counties	1,043	1,043	1,043	1,043	1,043	1,043
Num. cities	1,709	1,709	1,709	1,709	1,709	1,709
Num. banks	2,401	2,401	2,401	2,401	2,401	2,401
Observations	2,568	2,568	3,188	3,188	3,723	3,723
Obs. left of cutoff	2,206	2,206	2,745	2,745	3,197	3,197
Obs. right of cutoff	362	362	443	443	526	526
Left main bandwidth (h)	1,728	1,418	1,858	1,364	2,468	1,555
Right main bandwidth (h)	1,975	1,418	2,594	1,364	998	1,555
Effective obs. (left)	368	279	544	356	895	484
Effective obs. (right)	224	195	310	232	197	274

This table presents results from estimating

$$y_{bt} = \alpha + \beta_1 \cdot \mathbb{1}_{ct}^{\text{pop}>6,000} + \beta_2 \cdot (\text{pop}_{ct} - 6,000) + \beta_3 \cdot \mathbb{1}_{ct}^{\text{pop}>6,000} \cdot (\text{pop}_{ct} - 6,000) + \varepsilon_{bt},$$

where y_{bt} can be one of three possible outcome variables. In columns (1) and (2), the outcome variable is a dummy variable that takes the value 1 if a bank reports substantial holdings of other real estate owned (OREO) on the balance sheet (more than $15,000) ten years after the respective census has been published. The item is only collected from 1891 onwards and thus we restrict the analysis to the effect of the 1880 and the 1890 census. In columns (3) and (4), the outcome variable rediscounts is a dummy variable that takes the value one if a bank reports extensive use of emergency funding. Finally, in columns (5) and (6), the outcome is a dummy whether a bank defaults (receiver appointed) over the course of the decade following a census publication. We vary bandwidth selection methods. In particular, in column (1), (3),(5), we use two different MSE-optimal bandwidths selectors (below and above the cutoff) for the RD treatment effect estimator. In columns (2), (4), and (6), we use a common MSE-optimal bandwidth selector for the RD treatment effect estimator. Optimal bandwidth calculated using the `rdrobust` package (Calonico et al., 2017). As covariates, we include a dummy for whether the bank is located in the West or the South as well as bank size (in log) at the census publication using the covariate-adjustment approach developed by Calonico et al. (2019). Bank-level data from incumbent national banks at the publication of the 1870, 1880, and 1890 census. The sample is restricted to banks in towns with a population of less than 6,000 inhabitants as of the respective previous census. Standard errors in parentheses; *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Table 8: ***Real effects I*** — *Farming outcomes (per capita) ten years after the publication of the 1870, 1880, and 1890 census.*

Dependent Variable	Farm Value		Farm Output		Number of Farms	
	(1)	(2)	(3)	(4)	(5)	(6)
Conventional	-78.59*	-93.22**	-13.45**	-16.15**	-0.01**	-0.02**
	[41.90]	[45.12]	[6.08]	[6.72]	[0.01]	[0.01]
Bias-corrected	-96.44**	-111.86**	-15.55**	-18.57***	-0.02***	-0.02***
	[41.90]	[45.12]	[6.08]	[6.72]	[0.01]	[0.01]
Robust	-96.44**	-111.86**	-15.55**	-18.57**	-0.02**	-0.02**
	[46.66]	[49.57]	[6.86]	[7.37]	[0.01]	[0.01]
BW Type	MSE Two	MSE Common	MSE Two	MSE Common	MSE Two	MSE Common
Kernel Type	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov
Order Loc. Poly. (p)	1	1	1	1	1	1
Order Bias (q)	2	2	2	2	2	2
Mean dep. var.	399.81	399.81	62.86	62.86	0.09	0.09
Num. counties	1,043	1,043	1,043	1,043	1,043	1,043
Num. cities	1,716	1,716	1,716	1,716	1,716	1,716
Observations	2,857	2,859	2,859	2,859	2,859	2,859
Obs. left of cutoff	2,567	2,569	2,569	2,569	2,569	2,569
Obs. right of cutoff	290	290	290	290	290	290
Left main bandwidth (h)	1,518	1,436	1,436	1,306	1,460	1,578
Right main bandwidth (h)	2,922	1,436	2,640	1,306	2,411	1,578
Effective obs. (left)	304	277	277	244	283	318
Effective obs. (right)	226	160	216	148	210	171

This table shows results from estimating a local linear regressions of the form:

$$y_{ct} = \alpha + \beta_1 \cdot \mathbb{1}_{ct}^{\text{pop}>6{,}000} + \beta_2 \cdot (\text{pop}_{ct} - 6{,}000) + \beta_3 \cdot \mathbb{1}_{ct}^{\text{pop}>6{,}000} \cdot (\text{pop}_{ct} - 6{,}000) + \varepsilon_{ct}$$

where y_{ct} is the city c's county-level per capita farm value (columns (1) and (2)), per capita farm output (columns (3) and (4)) and the per capita number of farms (columns (5) and (6)) as of the census ten years after census t. We vary the bandwidth selection methods across specifications. In particular, in column (1), (3),(4) we use two different MSE-optimal bandwidths selectors (below and above the cutoff) for the RD treatment effect estimator. In column (2), (4), and (6), we use a common MSE-optimal bandwidth selector for the RD treatment effect estimator. Optimal bandwidth calculated using the `rdrobust` package (Calonico et al., 2017). As covariates, we include a dummy for whether the bank is located in the West or the South as well as the farm value (in logs) as of the census publication using the covariate-adjustment approach developed by Calonico et al. (2019). City-level data from the publication of the 1870, 1880, and 1890 census. Data is restricted to cities with at least one national bank at the respective census publication and less than 6,000 inhabitants in the respective previous census. Standard errors in parentheses; *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Table 9: ***Real effects II*** **—** *Manufacturing outcomes (per capita) ten years after the publication of the 1880 and 1890 census.*

Dependent Variable	Manuf. Capital		Manuf. Output		Manuf. Establishments	
	(1)	(2)	(3)	(4)	(5)	(6)
Conventional	-26.38*	-37.83**	-3.72	-4.98	-0.00	-0.00
	[14.13]	[18.40]	[13.61]	[14.30]	[0.00]	[0.00]
Bias-corrected	-29.89**	-44.52**	-3.04	-7.86	-0.00	-0.00
	[14.13]	[18.40]	[13.61]	[14.30]	[0.00]	[0.00]
Robust	-29.89*	-44.52**	-3.04	-7.86	-0.00	-0.00
	[16.62]	[20.10]	[15.55]	[15.83]	[0.00]	[0.00]
BW Type	MSE Two	MSE Common	MSE Two	MSE Common	MSE Two	MSE Common
Kernel Type	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov
Order Loc. Poly. (p)	1	1	1	1	1	1
Order Bias (q)	2	2	2	2	2	2
Mean dep. var.	98.74	98.74	122.21	122.21	0.01	0.01
Num. counties	1,005	1,005	1,005	1,005	1,005	1,005
Num. cities	1,626	1,626	1,626	1,626	1,626	1,626
Observations	2,269	2,271	2,271	2,271	2,252	2,252
Obs. left of cutoff	2,036	2,038	2,038	2,038	2,019	2,019
Obs. right of cutoff	233	233	233	233	233	233
Left main bandwidth (h)	2,129	1,212	2,161	1,755	1,522	1,437
Right main bandwidth (h)	2,067	1,212	2,590	1,755	2,163	1,437
Effective obs. (left)	372	168	384	276	228	208
Effective obs. (right)	157	115	172	144	161	129

This table shows results from estimating local linear regressions of the form:

$$y_{ct} = \alpha + \beta_1 \cdot \mathbb{1}_{ct}^{\text{pop}>6,000} + \beta_2 \cdot (\text{pop}_{ct} - 6,000) + \beta_3 \cdot \mathbb{1}_{ct}^{\text{pop}>6,000} \cdot (\text{pop}_{ct} - 6,000) + \varepsilon_{ct}$$

using the county-level per capita manufacturing capital (columns (1) and (2)), per capita manufacturing output (columns (3) and (4)) and the per capita number of manufacturing establishments (columns (5) and (6)) as of the next census as the outcome variable and varying bandwidth selection methods. In particular, in column (1), (3),(4) we use two different MSE-optimal bandwidths selectors (below and above the cutoff) for the RD treatment effect estimator. In column (2), (4), and (6), we use a common MSE-optimal bandwidth selector for the RD treatment effect estimator. Optimal bandwidth calculated using the `rdrobust` package (Calonico et al., 2017). As covariates, we include a dummy for whether the bank is located in the West or the South as well as the farm value (in logs) as of the census publication using the covariate-adjustment approach developed by Calonico et al. (2019). City-level data from the publication of the 1880 and 1890 census. Data is restricted to cities with at least one national bank at the respective census publication and less than 6,000 inhabitants in the respective previous census. Standard errors in parentheses; *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Online Appendix

Description of appendices

- Appendix A: Robustness I: Manipulation of the running variable
- Appendix B: Robustness II: State bank entry
- Appendix D: Additional Figures
- Appendix E: Additional Tables
- Appendix F: Historical Documents

A Robustness I: Manipulation of the running variable

An additional important concern is that agents manipulate around the cutoff leading to bunching of towns on either side of the cutoff. In the context of our study, the most plausible concerns would be that incumbent banks somehow influence the population count reported in a census in order to nudge the number to the right of the threshold and increase their market power. Given that our results show that there are indeed large effects from entry barriers on bank behavior, one may be inclined to believe that banks have strong incentives to increase the entry barriers in their respective market. If this is the case, we would expect there to be bunching of towns just right of the cutoff, i.e., there should be relatively more towns with just more than 6,000 inhabitants than with just less than 6,000 inhabitants.

To rule out systematic manipulation of the census counts — which could in turn be driving our results — Figure D.4 shows the distribution of population as of a census publication for all towns with banks, both left and right of the threshold. Panel A shows that while there are relatively fewer large cities with banks (reflecting Zipf's law), as well as very small cities with banks (reflecting insufficient scale), there is no detectable difference in the number of cities around the cutoff. Moreover, in Panel B we conduct a more formal manipulation test (Cattaneo et al., 2018), which fails to show any evidence of manipulation across the threshold. Similarly, we also used other standard tests such as McCrary (2008), which also show that there is no evidence of manipulation.

[FIGURE D.4 ABOUT HERE]

B Robustness II: State bank entry

In the body of the paper we examine the behavior of National Banks and how they responded to changes in entry barriers for other National Banks. However, as we noted in the text, banks could also choose to enter using a state charter and it is possible that potential competition from state banks could have affected the behavior of national banks. In this section, we provide additional evidence on potential state bank entry and show that it does not change the main results of the paper. In particular, we identify a set of states in which the entry barriers for state banks were especially high. This is attractive as it potentially provides a sub-sample that can be used to study the effects of the publication of the census on incumbent bank behavior when state bank entry is unlikely to substitute the lower competition from national banks in any way.

As documented by White (1983), some states had either stricter capital requirements, as in the case of Massachusetts, or the capital required to found a state-chartered bank would be at the discretion of the state regulator. In practice, the latter often required prospective bankers to lobby or bribe the local banking authorities, making entry cost for state banks unpredictable and potentially prohibitively costly. In particular, historians have argued that this type of regulation implied that entrants were required to bribe regulators to receive a charter while at the same time allowing incumbents to bribe regulators to prevent other banks from entering (see, e.g., Schwartz, 1947). States in which the capital required for state bank was at the discretion of the regulator are Connecticut, Delaware, Maine, New Hampshire, Kentucky, and Rhode Island, see (White, 1983).[29] We refer to these states as "high SB barrier" states.

We first estimate Equation (1) using the number of national and state bank entrants as the dependent variable and restricting the estimation to the sub-sample of cities in high SB barrier states.

Results are presented in Table E.3. First, in line with our main results on bank entry, we find that those markets that have stricter capital requirements for national banks entry see less new national bank entrants, see columns (1) and (2). Note that the results here are significant despite the relatively small sample and the small numbers of observations right to the cutoff. This again emphasizes the importance of entry barriers for entry. Second, in line with the findings discussed in the main body, we cannot detect an effect of entry barriers for national banks on state bank entry in the sub-sample of states in which state bank entry is arguably prohibitively costly, see columns (3) and (4).

[TABLE E.3 ABOUT HERE]

We can then go further and investigate whether our main results at the bank level hold for this smaller subset of high SB barrier states. To this end, we again estimate Equation (3) with the restricted sample of banks high SB barrier states, and using the growth in loans, deposits, and total assets as the dependent variable.

Table E.3 shows results. Comparable to the findings in our main analysis in Section 6, we find that incumbent banks have an around 15-21 percentage points lower loan growth in the ten years after the census publication. Moreover, deposits and assets grow exhibit at 12-14 and 6-8 percentage lower growth, respectively. Hence, we estimate very similar effects as for the main sample, even though the precision of the estimates is reduced by the smaller sample size.

[TABLE E.4 ABOUT HERE]

Altogether, this additional evidence provides reassurance that our results on entry are not driven by state bank entry substituting for the lack of national bank entry. Moreover, re-estimating our main bank-level specification for the high SB barrier states is re-assuring as it allows us to also address the concern that results are driven not by the effect of competition by another bank, but the type of competition—i.e., if state bank entry becomes more likely, one could expect national banks to emphasize their competitive advantages, which would then drive our results. Given that our estimates are essentially unchanged in the smaller sub-sample of high SB barriers states, we are able to rule out this alternative interpretation.

C Dynamics around the 1880 census publication

To further exploit the full richness of our data we estimate a panel regression around a given census publication. Thus, further information on the mechanism through which entry barriers shaped bank outcomes can also be obtained by studying the timing of the effect in more detail. To this end we extend study a parametric regression around the publication of the 1880 census. In particular, using bank-level data for all years between 1872 and 1891, we estimate a model in which we interacting the

[29] White (1983) shows this for 1895. We verified that the same rule was in place during the 1880's.

treatment indicator with year indicators:

$$y_{bt} = \tau_t + \beta_t \times \tau_t \times \mathbb{1}_{ct}^{\text{pop}>6{,}000} + \delta X_{bt} + \varepsilon_{bt}, \tag{4}$$

where y_{bt} is the loan growth of bank b from year $t-1$ to t, and X_{bt} is a set of time-varying city and bank-level controls. Note that we normalize β_t to 0 for 1880, the last year before the publication of the census.[30] Again we the restricted sample of towns that do not see new entrants after a census publication.

Panel (a) of Figure D.5 shows the coefficients across time, with annual loan growth as the dependent variable. The effect of entry barriers on loan growth appears right around the publication of the census even though the sample is restricted to towns in which no actual entry occurs. Figure D.5 hence provides further indication that the effects result from attempts to deter entry. Hence, credit expands slower in markets in which the threat of entry is lower.

Moreover, panel (b) of Figure D.5 shows the coefficients resulting from estimating Equation (4) on annual deposit growth. In contrast to the pattern in Figure D.5, the adjustment in deposits appears to be less sharply timed compared to the adjustment in loan growth. While the estimates are consistently negative after the publication of the census, the fall in deposit growth is not as immediate and rather attenuated compared to loan growth.

We interpret this as an indication that banks' margin of adjustment to changes of entry barriers is through changes in credit supply, as opposed to adjustments in banks' deposit demand. This interpretation is consistent with the notion that national banks exert relatively more market power on the asset side of their balance sheets than on the liability side. In particular, while savers could always hold specie or currency, firms that desire credit for conducting their business had few alternatives for getting external finance at the time.

[30] Recall that localities received preliminary estimates as soon as July 1880 and bank level balance sheet variables are reported in October.

D Appendix Figures

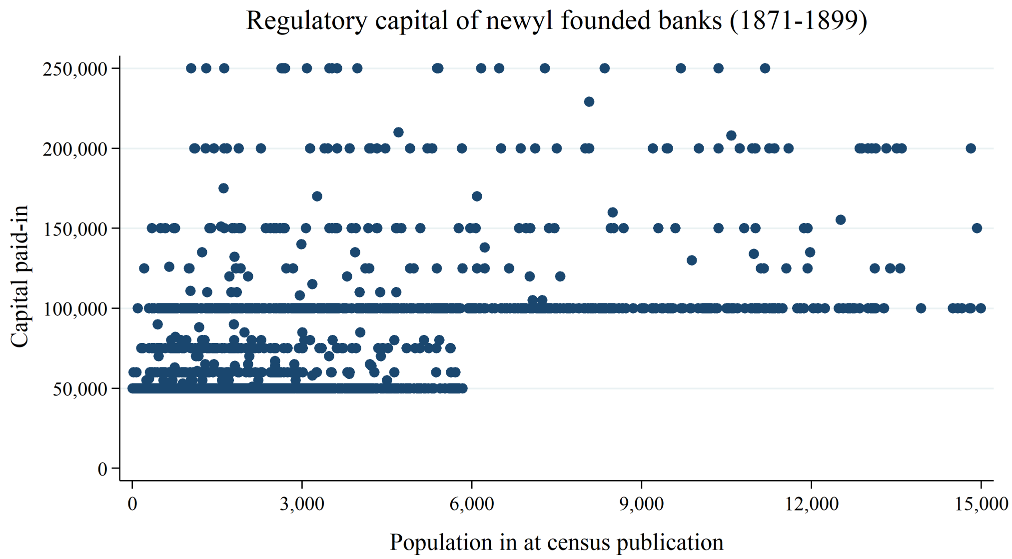


Figure D.1: *Scatterplot of capital paid-in in the year after the founding year for national banks founded between 1871 and 1899 by population of bank location. Sample restricted to towns with less than 15,000 inhabitants and capital paid-in less or equal than $250,000.*

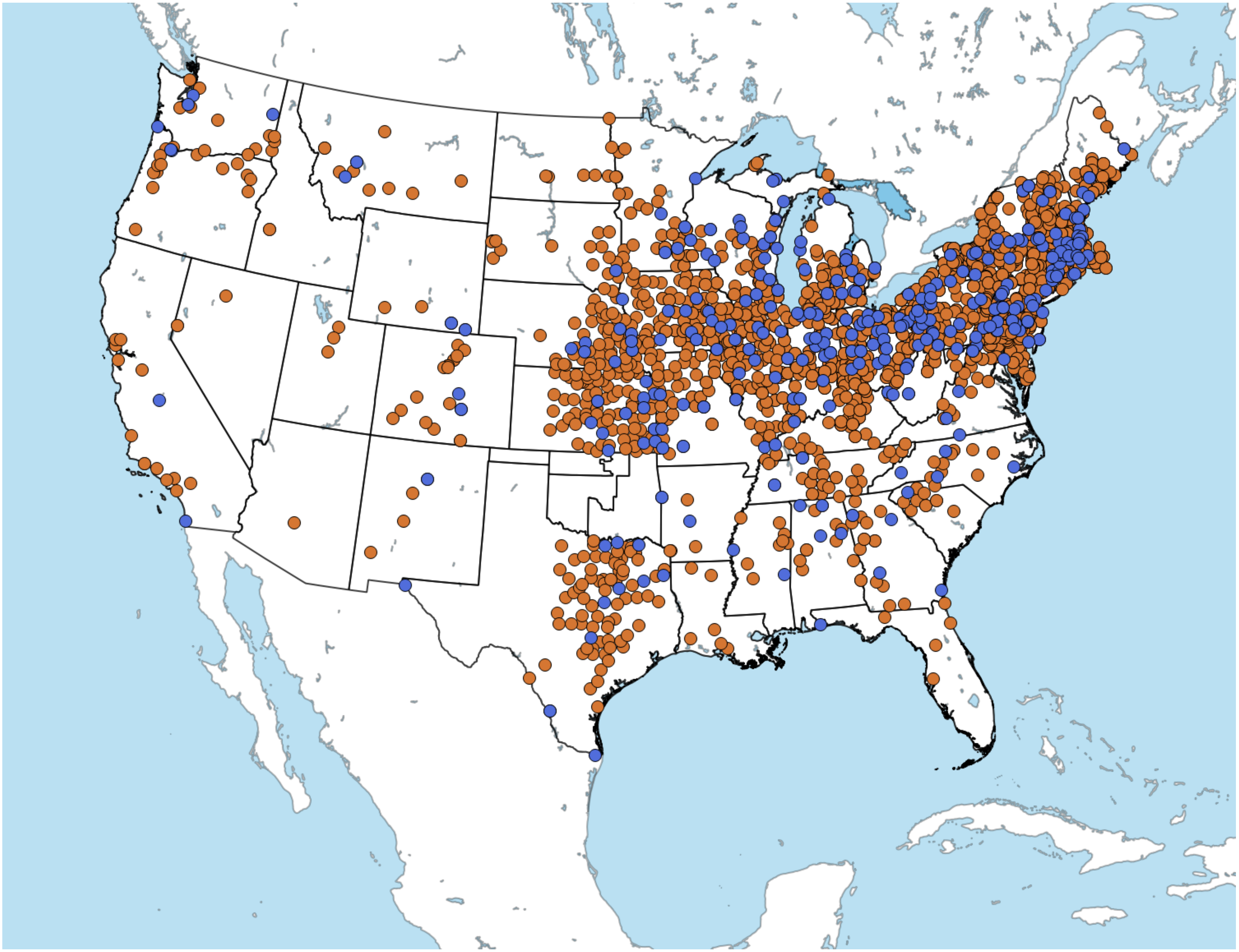
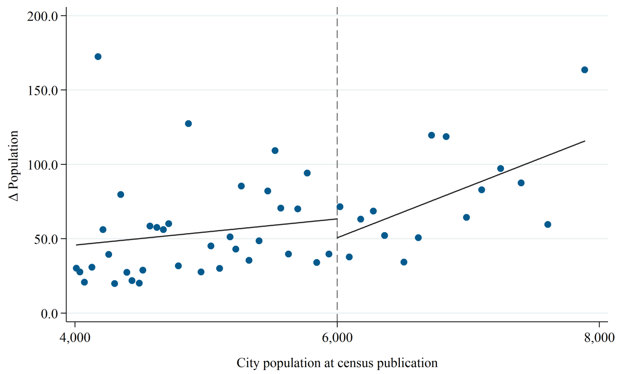
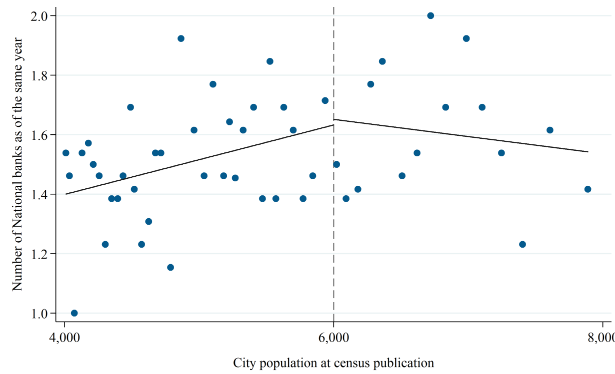


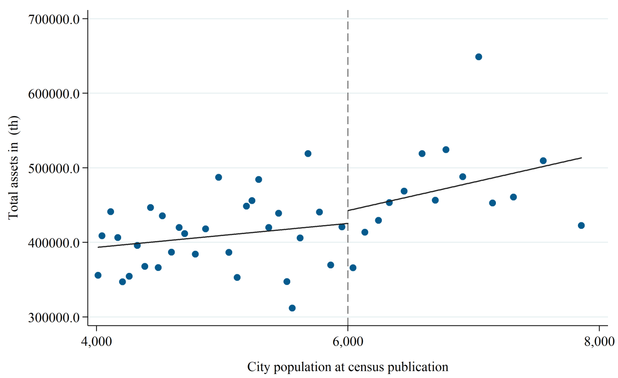
Figure D.2: ***Spatial distribution of main sample.*** *This figure maps the spatial distribution of the main sample, i.e., cities with one national bank or more at either the publication of the 1870, 1880, or 1890 census. Cities in blue are those that have more than 6,000 inhabitants as of a census publication and cities in orange are those that have less than 6,000. State borders as of 1890.*



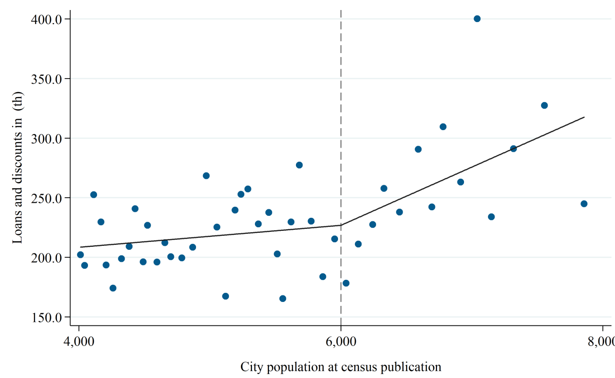
(a)



(b)

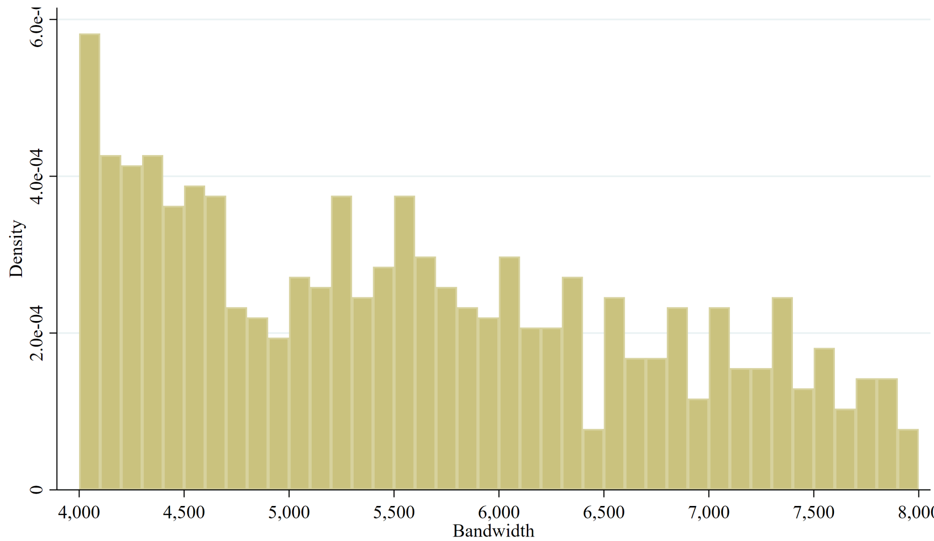


(c)

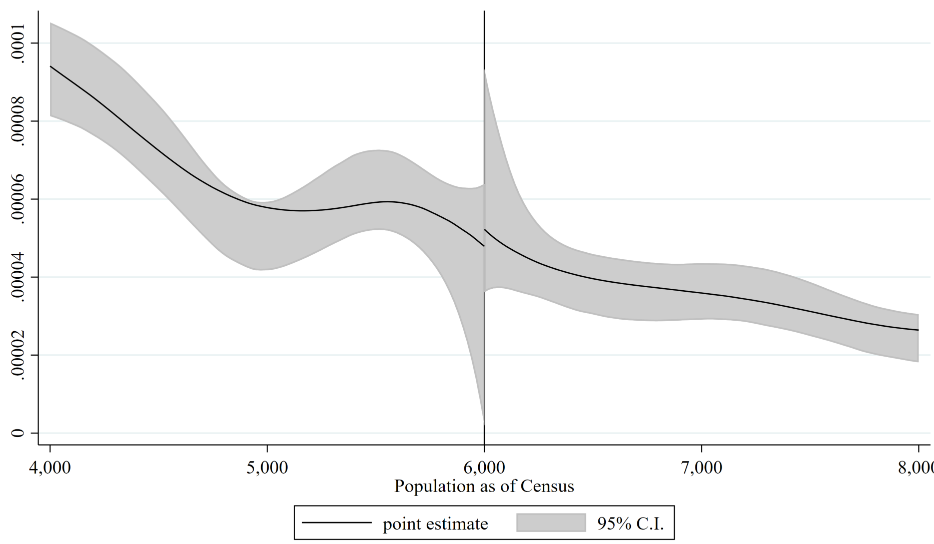


(d)

Figure D.3: ***Predetermined covariates*** *Binned scatterplot of various covariates in the year of a census publication by city population at census publication. The figure pools data from the the publications of the 1870, 1880, and 1890 census. This figure is created with the* `binscatter` *package (Stepner, 2013), using 45 equal-sized bins, plus linear fit lines left and right of the 6,000 population threshold.*



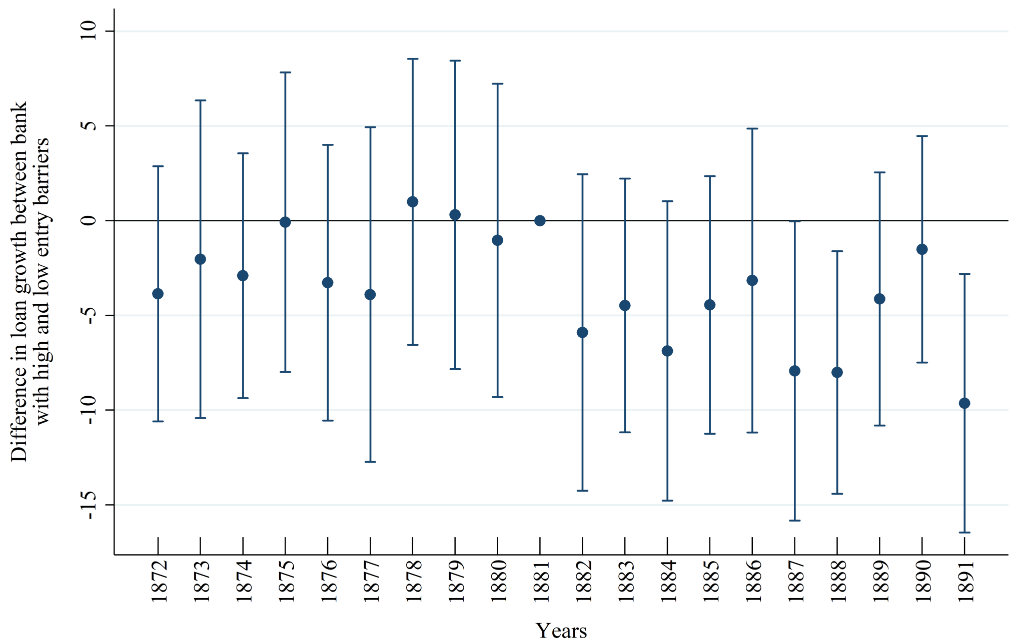
(a) ***Histogram of town population.*** *This figure shows a histogram of the 1880 census population of all U.S. towns with at least one national bank in the census year. Notice there is no apparent discontinuity around the 6,000 inhabitants treshold. Histogram used bins of 500 inhabitants; figure truncated at 12,000 inhabitants.*



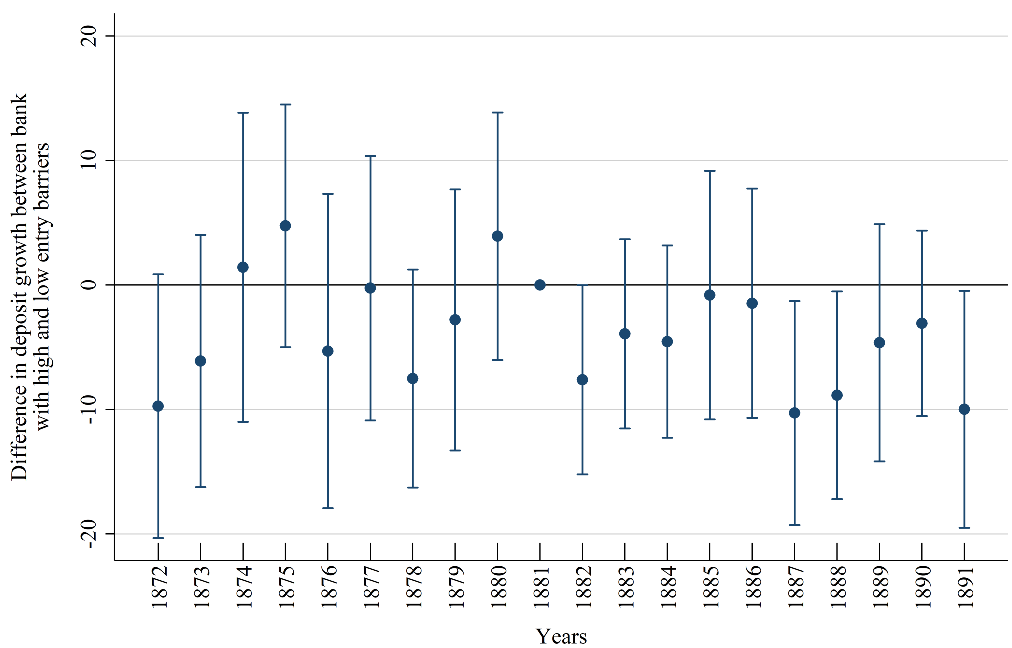
(b) ***Manipulation test.*** *This figure shows the results of a manipulation test using local polinomial density estimation (Cattaneo et al., 2018), implemented in Stata through the* `rddensity` *command. This test, using the sample of all U.S. towns with at least one national bank, fails to detect any evidence of manipulation of the 1880 population census results. It was constructed using the default options of a local quadratic approximation and a triangular kernel, but results are robust to other approximations (linear, cubic) and kernels (Epanechnikov, uniform).*

Figure D.4: ***Density of the running variable.***

E Appendix Tables



(a) *Loan growth from $t-1$ to t.*



(b) *Deposit growth from $t-1$ to t.*

Figure D.5: ***The effect on entry barriers on loan and deposit growth.*** *The figure shows coefficient from estimating $y_{bt} = \tau_t + \beta_t \times \tau_t \times \mathbb{1}_{ct}^{pop>6,000} + \delta X_{bt} + \varepsilon_{bt}$ where y_{bt} is the loan (Panel (a)) or deposit (Panel (b)) growth of bank b from $t-1$ to t. We normalize coefficients to 0 in the year prior to the census publication, 1880. The sample is restricted to using data from 1871 through 1891 for banks that were founded before 1881 and are located in towns with less than 6,000 inhabitants as of 1870. Further, the sample is restricted to banks in cities that experience additional net entry of banks between 1881 and 1891. Standard errors are clustered at the city level, and 95% confidence bands.*

Table E.1: ***Entry II (Poisson)***— *city-level evidence on entries of national banks over decade following a census publication. Sample restricted to towns with $+/-2,000$ inhabitants around the cutoff. Poisson estimation with average marginal effect reported.*

Dependent Variable	Entries_{nb}					
	Linear Population		Linear Flexible Population			
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}_{\text{pop1880}>6000}$	-0.161**	-0.191**	-0.170***	-0.195***	-0.165**	-0.194***
	(0.079)	(0.088)	(0.059)	(0.065)	(0.065)	(0.069)
Population (in th)	0.121***	0.107**	0.124***	0.110**	0.090**	0.089**
	(0.043)	(0.043)	(0.043)	(0.044)	(0.044)	(0.045)
Pop growth past decade					0.072***	0.061***
					(0.016)	(0.019)
Mean of dependent variable	0.31	0.34	0.31	0.34	0.31	0.34
Pseudo R^2	0.01	0.10	0.02	0.10	0.04	0.11
No. of Cities	653	592	653	592	653	592
No. of Counties	354	318	354	318	354	318
State FE	No	Yes	No	Yes	No	Yes
Market Structure FE	No	No	No	No	No	No

This table show results from estimating a Poisson model of the form:

$$y_{ct} = \exp\left(\alpha_s + \beta \cdot \mathbb{1}_{ct}^{\text{pop}>6,000} + \gamma_1 \cdot \text{pop}_{ct} + \mathbb{1}_{ct}^{\text{pop}>6,000} \cdot \gamma_2 \text{pop}_{ct} + \varepsilon_c\right)$$

where y_{ct} is the number of entries in the decade after the census from year $t \in 1870, 1880, 1890$ has been published. We estimate a Poisson model as the outcome variable is a count variable and using a linear regression in this type of setup can lead to *inconsistent estimates* under heteroskedasticity, as discussed by Santos Silva and Tenreyro (2006). We implement all Poisson regressions through Stata's `ppmlhdfe` command (Correia et al., 2019), which allows for multiple levels of fixed effects and adjusts standard errors accordingly. City-level data from the publication of the 1870, 1880, and 1890 census. Data is restricted to cities with at least one national bank at the respective census publication and less than 6,000 inhabitants in the respective previous census. The bandwidth is manually narrowed to +/-2,000 inhabitants around the cutoff. The running variable is linear town population. In columns (3)-(6) town population is also interacted with the treatment. Average marginal effect reported and robust standard errors in parentheses; *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Table E.2: *Real effects II* — *Manufacturing outcomes (per capita) ten years after the publication of the 1870, 1880, and 1890 census.*

Dependent Variable	Manuf. Output		Manuf. Capital		Manuf. Establishments	
	(1)	(2)	(3)	(4)	(5)	(6)
Conventional	-8.18	-9.74	6.47	8.39	0.00	0.00
	[11.37]	[12.45]	[12.81]	[14.07]	[0.00]	[0.00]
Bias-corrected	-8.85	-12.80	8.20	11.99	0.00	0.00
	[11.37]	[12.45]	[12.81]	[14.07]	[0.00]	[0.00]
Robust	-8.85	-12.80	8.20	11.99	0.00	0.00
	[13.13]	[13.92]	[14.68]	[16.03]	[0.00]	[0.00]
BW Type	MSE Two	MSE Common	MSE Two	MSE Common	MSE Two	MSE Common
Kernel Type	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov
Order Loc. Poly. (p)	1	1	1	1	1	1
Order Bias (q)	2	2	2	2	2	2
Mean dep. var.	93.84	93.84	121.97	121.97	0.02	0.02
Num. counties	1,043	1,043	1,043	1,043	1,043	1,043
Num. cities	1,716	1,716	1,716	1,716	1,716	1,716
Observations	2,843	2,845	2,845	2,845	2,824	2,824
Obs. left of cutoff	2,553	2,555	2,555	2,555	2,534	2,534
Obs. right of cutoff	290	290	290	290	290	290
Left main bandwidth (h)	2,290	1,716	1,615	1,653	1,367	1,443
Right main bandwidth (h)	1,949	1,716	3,129	1,653	2,232	1,443
Effective obs. (left)	543	356	332	338	260	281
Effective obs. (right)	187	176	231	172	201	160

This table shows results from estimating local linear regressions of the form:

$$y_{ct} = \alpha + \beta_1 \cdot \mathbb{1}_{ct}^{\text{pop}>6,000} + \beta_2 \cdot (\text{pop}_{ct} - 6,000) + \beta_3 \cdot \mathbb{1}_{ct}^{\text{pop}>6,000} \cdot (\text{pop}_{ct} - 6,000) + \varepsilon_{ct}$$

using the county-level per capita manufacturing capital (columns (1) and (2)), per capita manufacturing output (columns (3) and (4)) and the per capita number of manufacturing establishments (columns (5) and (6)) as of the next census as the outcome variable and varying bandwidth selection methods and using an epanechnikov kernel. In particular, in column (1), (3),(4) we use two different MSE-optimal bandwidths selectors (below and above the cutoff) for the RD treatment effect estimator. In column (2), (4), and (6), we use a common MSE-optimal bandwidth selector for the RD treatment effect estimator. Optimal bandwidth calculated using the `rdrobust` package (Calonico et al., 2017). As covariates, we include a dummy for whether the bank is located in the West or the South as well as the farm value (in logs) as of the census publication. City-level data from the publication of the 1870, 1880, and 1890 census. Data is restricted to cities with at least one national bank at the respective census publication and less than 6,000 inhabitants in the respective previous census. Standard errors in parentheses; *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Table E.3: ***Entry in states with high entry barriers for state banks***— *city-level evidence on entries of national and state banks in the respective decade following the publications of the 1870, 1880, and 1890 census for a sample of states with high state-bank entry barriers.*

Dependent Variable	National bank entrants		State bank entrants	
	(1)	(2)	(3)	(4)
Conventional	-0.23*	-0.33	1.05	0.82
	[0.14]	[0.21]	[0.85]	[0.96]
Bias-corrected	-0.26*	-0.38*	0.95	0.79
	[0.14]	[0.21]	[0.85]	[0.96]
Robust	-0.26	-0.38	0.95	0.79
	[0.17]	[0.26]	[1.04]	[1.18]
BW Type	MSE Two	MSE Common	MSE Two	MSE Common
Kernel Type	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov
Order Loc. Poly. (p)	1	1	1	1
Order Bias (q)	2	2	2	2
Mean dep. var.	0.10	0.10	0.94	0.94
Num. counties	99	99	99	99
Num. cities	274	274	274	274
Observations	596	596	596	596
Obs. left of cutoff	552	552	552	552
Obs. right of cutoff	44	44	44	44
Left main bandwidth (h)	2,572	1,009	2,718	1,056
Right main bandwidth (h)	1,177	1,009	1,034	1,056
Effective obs. (left)	169	46	182	48
Effective obs. (right)	29	27	28	28

This table shows results from estimating a local linear regression of the form:

$$y_{ct} = \alpha + \beta_1 \cdot \mathbb{1}_{ct}^{\text{pop}>6{,}000} + \beta_2 \cdot (\text{pop}_{ct} - 6{,}000) + \beta_3 \cdot \mathbb{1}_{ct}^{\text{pop}>6{,}000} \cdot (\text{pop}_{ct} - 6{,}000) + \varepsilon_{ct},$$

where y_{ct} is either the number of national bank entries (columns (1) and (2)) or state bank entries (columns (3) and (4)) over the decade following the publication of the census from year $t \in \{1870, 1880, 1890\}$. We vary bandwidth selection methods and kernel functions (epanechnikov and triangular) across specifications. In particular, in columns (1) and (3) we use two different MSE-optimal bandwidths selectors (below and above the cutoff) for the RD treatment effect estimator. In columns (2) and (4), we use a common MSE-optimal bandwidth selector for the RD treatment effect estimator. Optimal bandwidth calculated using the `rdrobust` package (Calonico et al., 2017). City-level data from the publication of the 1870, 1880, and 1890 census. Data is restricted to cities with at least one national bank at the respective census publication and less than 6,000 inhabitants in the respective previous census. Further, the sample is restricted to cities in "high SB barrier states", i.e., to cities from Massachusetts, Connecticut, Delaware, Maine, New Hampshire, Kentucky, and Rhode Island. Standard errors in parentheses; *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Table E.4: ***Loans, deposits, and assets*** *— bank-level evidence on growth rates of various balance sheets items over the decade following a census publication in states with high entry barriers for state banks.*

Dependent Variable	Δ Loans		Δ Deposits		Δ Assets	
	(1)	(2)	(3)	(4)	(5)	(6)
Conventional	-15.90	-16.95	-11.15	-12.52	-6.55	-7.24
	[18.16]	[17.26]	[14.80]	[14.08]	[7.72]	[7.62]
Bias-corrected	-19.54	-21.14	-12.37	-14.93	-6.49	-7.49
	[18.16]	[17.26]	[14.80]	[14.08]	[7.72]	[7.62]
Robust	-19.54	-21.14	-12.37	-14.93	-6.49	-7.49
	[22.09]	[21.33]	[18.17]	[17.26]	[9.52]	[9.36]
BW Type	MSE Two	MSE Two	MSE Two	MSE Two	MSE Two	MSE Two
Kernel Type	Epanechnikov	Triangular	Epanechnikov	Triangular	Epanechnikov	Triangular
Order Loc. Poly. (p)	1	1	1	1	1	1
Order Bias (q)	2	2	2	2	2	2
Mean dep. var.	10.67	10.67	37.51	37.51	17.06	17.06
Num. counties	100	100	100	100	100	100
Num. cities	281	281	281	281	281	281
Num. banks	364	364	364	364	364	364
Observations	676	676	675	675	676	676
Obs. left of cutoff	606	606	605	605	606	606
Obs. right of cutoff	70	70	70	70	70	70
Left main bandwidth (h)	2,398	2,624	1,909	2,170	2,463	2,577
Right main bandwidth (h)	805	944	1,037	1,307	1,052	1,170
Effective obs. (left)	204	221	143	183	211	218
Effective obs. (right)	38	44	46	51	46	50

The table presents results from estimating

$$y_{bt} = \alpha + \beta_1 \cdot \mathbb{1}_{ct}^{\text{pop}>6{,}000} + \beta_2 \cdot (\text{pop}_{ct} - 6{,}000) + \beta_3 \cdot \mathbb{1}_{ct}^{\text{pop}>6{,}000} \cdot (\text{pop}_{ct} - 6{,}000) + \varepsilon_{bt},$$

where y_{bt} is bank b's growth deposits, assets, etc. over the ten years following the publication of the year t census. We construct the estimators by using two different MSE-optimal bandwidths selectors (below and above the cutoff).Optimal bandwidth calculated using the `rdrobust` package (Calonico et al., 2017). As covariates, we include a dummy for whether the bank is located in the West or the South as well as the bank size and bank loans (both in log) at the census publication using the covariate-adjustment approach developed by Calonico et al. (2019). Bank-level data from incumbent national banks at the publication of the 1870, 1880, and 1890 census. The sample is restricted to banks in towns with a population of less than 6,000 inhabitants as of the respective previous census. Further, the sample is restricted to cities in "high SB barrier states", i.e., to cities from Massachusetts, Connecticut, Delaware, Maine, New Hampshire, Kentucky, and Rhode Island. Standard errors in parentheses; *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

F Historical Documents

REPORT OF THE COMPTROLLER OF THE CURRENCY. 399

PENNSYLVANIA.

First National Bank, Mifflintown.

JEREMIAH LYONS, *President*. No. 4039. EZRA C. DOTY, *Cashier*.

Resources.		Liabilities.	
Loans and discounts	$172, 227. 74	Capital stock paid in	$50, 000. 00
Overdrafts	107. 91		
U. S. bonds to secure circulation	50, 000. 00	Surplus fund	6, 000. 00
U. S. bonds to secure deposits	....	Undivided profits	2, 038. 62
U. S. bonds on hand	....		
Stocks, securities, etc	....	National-bank notes outstanding	45, 000. 00
Due from approved reserve agents	27, 187. 99	State-bank notes outstanding	....
Due from other national banks	1, 059. 41		
Due from State banks and bankers	886. 19	Dividends unpaid	....
Bank'g house, furniture, and fixtures	9, 400. 00		
Other real estate and mortg's owned	....	Individual deposits	173, 934. 15
Current expenses and taxes paid	811. 99	United States deposits	....
Premiums on U. S. bonds	6, 400. 00	Deposits of U.S. disbursing officers	....
Checks and other cash items	508. 41		
Exchanges for clearing house	....	Due to other national banks	8, 826. 66
Bills of other national banks	1, 015. 00	Due to State banks and bankers	....
Fractional currency, nickels, cents	140. 04		
Specie	8, 721. 75	Notes and bills rediscounted	....
Legal-tender notes	5, 083. 00	Bills payable	....
U. S. certificates of deposit	....		
Redemption fund with Treas. U. S.	2, 250. 00		
Due from Treasurer U. S.	....		
Total	285, 799. 43	Total	285, 799. 43

Figure F.1: *Excerpt from the Appendix of 1891 OCC annual report. In the paper, we extract all balance sheet information from the report between 1867 and 1904. To do so, we applied a combination of optical character recognition (OCR) and layout recognition techniques to the Annual Report. We flagged potential errors through multiple checks, including the application of balance sheet identities and legal constraints on the balance sheet. All flagged observations were hand-checked. We also extracted the charter number, the president and cashier name, state, county, and city of each bank, geo-located the cities, and recorded the dates of all relevant events for each bank (entry, receivership, liquidation, rechartering, etc.).*

THE ORGANIZATION OF SMALL BANKS.

A bill was introduced in the Senate in February by Mr. Ingalls, of Kansas, which proposed to allow National Banks to be organized with a capital as small as $50,000 in any place, regardless of its population. In reporting adversely upon this bill, the Finance Committee of the Senate cite the following letter from the Comptroller of the Currency:

TREASURY DEPARTMENT,
OFFICE OF COMPTROLLER OF THE CURRENCY,
WASHINGTON, February 24, 1876.

SIR: I have the honor to acknowledge the receipt of your letter of the 1st instant, transmitting, for my views, Senate bill No. 75, which provides that section 5138, Revised Statutes United States, be so amended as to read as follows: "No association shall be organized under this title with a less capital than $50,000." The section referred to, which it is proposed to amend, provides that banks with a capital of not less than $50,000 may, with the approval of the Secretary of the Treasury, be organized in any place the population of which does not exceed 6,000 inhabitants; and that no association shall be organized in a city the population of which exceeds 50,000, with a less capital than $200,000. The Act of February 25, 1863, which was superseded by the National Bank Act of June 3, 1864, provided that "the capital stock of National Banks shall not be less than $50,000, and in cities whose population is over 10,000, the capital stock shall not be less than $100,000."

The object of the proposed amendment would seem to be to authorize the organization of banks in cities and villages containing a greater population than 6,000 inhabitants, which are now supposed to be excluded from these privileges of the National Bank Act, namely, those authorizing the organization of banks with as small a capital as $50,000 in places the population of which does not exceed 6,000. I find from the census returns of 1870 that the State of Ohio had at that time 20 cities and villages exceeding 6,000 inhabitants, and that Indiana had 12, Illinois 18, Michigan 10, Wisconsin 7, Iowa 8, Missouri 4, Minnesota 3, Kansas 3 and Nebraska 2. In all of these cities, with the single exception of Newburg, a suburb of Cleveland, there are at the present time National Banking Associations, having in each instance a capital exceeding $100,000. In most of these cities and villages two or more National Banks exist under the provisions of the present act. It follows, therefore, that National Banks may be organized in all the cities and villages of the Western States with the exception of those enumerated with a capital of $50,000, and it will be found upon an examination of the last annual report of this office that many banks having a capital of $100,000 and upward have been organized in villages having a population of much less than 6,000. Experience has shown that in almost every instance where two or more banks have been organized in small towns, with a capital of $50,000, they have been so organized for the purpose of providing positions for stockholders or friends of stockholders of the several organizations, and in numerous instances, after such organizations have been perfected, applications have been made to this office for the consolidation of two or more of them into one with a large capital, in order to save expenses; and it rarely happens that applications are made for the organization of banks of a less capital than $100,000 in any of the larger towns of the country. The organization of numerous small institutions in the large cities has a tendency to weaken those already organized, and to so divide the business as to make them all more or less unprofitable to the shareholders. Very few applications are on file in this office for the organization of banks, which cannot be organized under the existing law.

I am of the opinion, therefore, that the passage of Mr. Ingalls' proposed amendment would be injurious rather than beneficial to the National Banking system.

I am, very respectfully, JNO. JAY KNOX,
Comptroller.

Hon. JOHN SHERMAN,
Chairman, Committee on Finance, United States Senate.

Figure F.2: *Evidence that the Comptroller of the Currency saw the capital regulation as means to provide barriers to entry to allow banks to make more profits.*