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

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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 nineteenth-century National Banking Era. We show that banks operating in markets with lower entry barriers

We thank Chad Syverson and four anonymous referees for valuable suggestions. Special thanks to Matt Jaremski for sharing data on the existence of state banks and to Mark Dregson, Peter Huntoon, and Andrew Pollock for their help in standardizing bank president and cashier names. We also thank our discussants, Narly Dwarkasing, Florian Heider, Matt Jaremski, Peter Koudijs, Francesco Manaresi, Ralf Meisenzahl, Mitchell Petersen, Richard Rosen, Glenn Schepens, and Constantine Yannelis. Further thanks to Sriya Anbil, Effi Benmelech, Allen Berger, Michael Bordo, Fabio Braggion, Claudia Buch, Charles Calomiris, Matias Cattaneo, Nicola Cetorelli, Ian Dew-Becker, Carola Frydman, Stefan Gissler, Eric Hilt, Kilian Huber, Ben Hyman, Sasha Indarte, Victoria Ivashina, Simon Jäger, Elizabeth Klee, Divya Kirti, Arvind Krishnamurthy, Andreas Lehnert, Ross Levine, Joe Mason, Filippo Mezzanotti, Atif Mian, Kris Mitchener, Mark Morley, Christopher Palmer, Amiyatosh Purnanandam, Marcelo Rezende, Hugh Rockoff, Phil Strahan, David Thesmar, Wilbert van der Klaauw, David Wheelock, and

Electronically published December 16, 2021

Journal of Political Economy, volume 130, number 2, February 2022.

https://doi.org/10.1086/717453

extend more credit. The resulting credit expansion, in turn, is associated with additional real economic activity. However, banks in markets with lower entry barriers also take more risk and are more likely to default. Thus, we provide causal evidence that banking competition can cause both growth and financial instability.

I. Introduction

How does banking competition affect credit provision and growth? How does it affect financial stability? In this paper, we exploit peculiarities in the National Banking Era's capital regulation 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. 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 are more likely to default. Hence, we identify that credit growth affects real economic outcomes (King and Levine 1993; Levine and Zervos 1998; Chodorow-Reich 2014; Benmelech, Frydman, and Papanikolaou 2019) 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, Tornell, and Westermann 2008; Reinhart and Rogoff 2009; Schularick and Taylor 2012; Rajan and Ramcharan 2015; Mian, Sufi, and Verner 2020) in which credit supply shocks cause both real economic activity and an increased chance of financial distress.

Despite the importance of banking competition for economic policy (see, e.g., Drechsler, Savov, and Schnabl 2017; Cox, Liu, and Morrison 2020), there is limited consensus on its effect on growth and financial stability. In theory, it is plausible for competition among banks to either increase or decrease credit provision and risk taking. Identifying the

Eugene White, as well as seminar and conference participants at the Federal Reserve Board, FRB (Federal Reserve Bank) Philadelphia, FRB New York, Kellogg, the National Bureau of Economic Research Corporate Finance meeting, the Financial Intermediation Research Society (Savannah), the Economic History Association (Montreal), the European Finance Association (Warsaw), the Federal Deposit Insurance Corporation/Journal of Financial Services Research Banking Research Conference, the American Economic Association (San Diego), the Virtual Western Finance Association (San Francisco), German Economists Abroad (Frankfurt), the European Economic Association (Cologne), the International Monetary Fund Research Seminar, Banco de Portugal, Princeton University, Rutgers University, the University of California Davis, the European Macroeconomic History Webinar, the Barcelona Graduate School of Economics Summer Forum, Swiss Winter Finance, University College Dublin, and CESifo for useful comments. Last but not least, we would like to thank Harry Cooperman, Sarah Ngo Hamerling, Candy Martinez, and Tyler Wake for excellent research assistance. This paper expresses the views of the authors and not necessarily those of the Board of Governors of the Federal Reserve or the Federal Reserve Bank of New York. Data are provided as supplementary material online. This paper was edited by Chad Syverson.

causal effects of banking competition empirically, however, is challenging. Existing empirical studies either equate measures of concentration, such as the Herfindahl-Hirschman index, with competition or focus on events, such as the deregulation of branching restrictions (see, e.g., Jayaratne and Strahan 1996, 1998; Black and Strahan 2002; Jiang, Levine, and Lin 2016, 2017). However, measuring market structure by concentration is problematic, as concentration is itself a market outcome and is sensitive to the definition of what constitutes a market. 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, Laeven, and Levine 2016) and a complex interplay of bank mergers and political-economic forces (Agarwal et al. 2012; Calomiris and Haber 2014).

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 implies that banks' behavior is not influenced by the anticipation of government interventions. Second, the prevalence of unit banking ensures that banking markets are local and well defined, which allows 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, so that we are able to observe the precise source of the variation in market power.

Capital regulation during the National Banking Era required that shareholders had to raise a minimum dollar amount of equity at the founding of a bank. Capital requirements did not constrain a bank's equity-to-assets ratio, as is the case in contemporary regulatory frameworks, but rather represented an entry barrier. Importantly for our study, 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 with a charter from the federal government (national 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, 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 entry, bank behavior, and real economic outcomes.

The regulatory framework further determined that changes in the required capital following a census publication applied only 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 derive only from changes in the requirements for new entrants and not from a 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 could 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 publication of the 1870, 1880, and 1890 censuses as the source of variation in entry barriers. 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 banks in towns that crossed the cutoff with those that stayed below.

Our identification strategy is subject to two main concerns. The most important one 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-inhabitants cutoff and 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, Idrobo, and Titiunik 2019): Throughout our analysis, we estimate nonparametric local polynomial regressions (Hahn, Todd, and Van der Klaauw 2001) with optimally selected bandwidths around the cutoff (Imbens and Kalyanaraman 2011; Calonico, Cattaneo, and Titiunik 2014). To support this approach, we provide evidence that treated and nontreated towns around the cutoff are indistinguishable across several important observable characteristics. Further, we show that our results are robust to various types of data-driven bandwidth selection methods as well as to using parametric estimation techniques. Moreover, we conduct a set of permutation tests and provide evidence that the effect on bank entry is stronger and is statistically significant only around the true cutoff.[1]

A second concern stems from the fact that, even though national banking was the most common form of banking during the period considered, banking services were also provided by banks with charters from state governments (state 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] To ensure that our results

[1] We also tested for, and found no evidence of, manipulation around the 6,000-inhabitants cutoff (McCrary 2008; Cattaneo, Jansson, and Ma 2018) or discontinuities in the predetermined covariates.

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

are not simply driven by the substitution between charter types, we test and control for the existence and entry of state banks.

Our analysis then proceeds in three steps. First, we provide evidence that bank entry drops discontinuously whenever a town becomes subject to higher entry barriers after a census publication. We find that markets subject to higher capital requirements see less entry: on average, 0.21–0.27 fewer banks enter in markets just right of (i.e., above) the 6,000-inhabitants cutoff than in markets just left of (i.e., below) 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 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 portfolios at a rate around 12–15 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. Overall, our results are consistent with the idea that banks with more market power restrict, rather than increase, credit provision. However, we provide additional evidence that suggests that the positive effects of higher banking competition on credit growth may be attenuated when bank-borrower relationships are stronger (Petersen and Rajan 1995).

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 entry barriers alone, as opposed to a change in actual market concentration that follows changes in market structure. We present two empirical facts that suggest that banks take actions to deter entrants. 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 concentration—indicating that changes in entry barriers alone can govern bank behavior. Second, when studying the dynamics around census publication, we find that incumbents contract loans and deposits immediately after learning that entry barriers have increased. Importantly, actual entry occurs only relatively infrequently and responds only slowly to changes in entry barriers. The abrupt response thus indicates that banks start reducing credit supply immediately upon learning that the need to deter potential entrants has receded. Altogether, our evidence is in line with predictions from the theoretical literature on entry deterrence (see, e.g., Dixit 1979; Milgrom and Roberts 1982a, 1982b; Klemperer 1987) as well as more recent empirical evidence on entry deterrence and economic outcomes from the airline industry (see, e.g., Goolsbee and Syverson 2008).

To consider banks' risk-taking behavior, we first study bank failure rates during the decade following a census publication. We find that failure rates of incumbent banks were around 8 percentage points lower in the less competitive towns in the 10 years following a change in entry barriers, an economically significant effect, given the unconditional default probability of 4%. Importantly, for robustness, we also show that our results are driven not by region-specific asset shocks in western lands during the 1890s (Calomiris and Gorton 1991) but indeed by changes in the competitive environment. Moreover, the findings on bank failure rates are supported by the fact that incumbent banks in towns with lower entry barriers tended to have, on average, more seized collateral from loans where the borrowers defaulted on their balance sheets than banks in towns with higher entry barriers.

Further, we also find evidence that incumbent banks in markets with higher entry barriers took less risk than their peers in more competitive markets. For instance, 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 followed a safer business model.

Altogether, our findings suggest a more conservative approach to lending by banks that faced less competition, which 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 risks and need not expand credit as rapidly—because they are either 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. We investigate whether farming and manufacturing outcomes varied 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 a decrease in economic activity. We document that 10 years after a census is published, markets that were subject to higher entry barriers exhibited a lower per capita farm output, farm value, and number of farms. Moreover, we also show that markets with higher entry barriers exhibited lower per capita manufacturing capital invested. However, the latter effect can be detected only between 1880 and 1900 and not during the 1870s, which is in line with disruptions from the Civil War lasting well into the 1870s and manufacturing becoming more important toward the end of the nineteenth century, especially in the smaller towns considered in our sample.

The rest of the paper proceeds as follows. We review the related literature in section II, before describing our data set in more detail in section III. We then provide background on how we use capital regulation during the National Banking Era to identify the causal effects of banking competition in section IV. We study the effect on entry in section V, the effect on bank behavior in section VI, and the effects on the real economy in section VII. Section VIII concludes.

II. 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 borrowers have fewer incentives to take on riskier projects (see, e.g., Boyd and De Nicolò 2005). Combining the two arguments, Martinez-Miera and Repullo (2010) show that the relationship between competition and risk taking can be U-shaped and vary across different economies.

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

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

In the light of the wide range of theoretical predictions, empirical evidence becomes especially important. Several key contributions indicate that competition, while increasing the efficiency 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, Sufi, and Verner (2020) find an increase in credit provision to households in the context of the lifting of branching restrictions. Moreover, additional evidence by Gissler, Ramcharan, and Yu (2020) finds that more competition from credit unions leads to an increase in household credit.[6]

Similarly, existing evidence on the effects of banking competition on financial stability also varies across different empirical settings. Jayaratne and Strahan (1996) 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. They show that incumbent banks that 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, Levine, and Lin (2017)—exploiting variations in

[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 borrowers. Drechsler, Savov, and Schnabl (2017) discuss the importance of bank market power in monetary policy transmission, and Cox, Liu, and Morrison (2020) study the effect on the pass-through of fiscal measures such as loan guarantees. Further important papers on the real effects of branching restrictions in the United States are by Stiroh and Strahan (2003), Zarutskie (2006), Beck, Levine, and Levkov (2010), Rice and Strahan (2010), Cetorelli (2014), and Jiang, Levine, and Lin (2019). Additional evidence from France on the real effect of banking competition is provided by Bertrand, Schoar, and Thesmar (2007). Evidence from the United Kingdom is by Braggion and Ongena (2019). Finally, a set of recent papers use changes in local concentration resulting from bank mergers to instrument competition (see, e.g., Scharfstein and Sunderam 2013; Liebersohn 2017) and measure the effects of ownership structures (Azar, Schmalz, and Tecu 2018).

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 the overall efficiency of the banking system.

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 other channels. It changes the ability of banks to diversify (Goetz, Laeven, and Levine 2016) and thus potentially influences bank risk taking. Moreover, and particularly in the United States, 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, our empirical setting offers temporal and cross-sectional variation in market power that allows us to observe the precise source of the variation in market power: barriers to entry. Thus, unlike existing empirical work on banking competition, our measurement of market power does not rely on market outcomes such as concentration, and the variation in entry barriers does not coincide with other changes, such as the ability to diversify across markets, as in the literature on the lifting of branching restrictions. 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 make credit more readily available. 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 change their behavior in ways that affect real economic outcomes in response to changes in the competitive environment, like nonfinancial firms (Goolsbee and Syverson 2008). Our evidence is also in line with the findings in Tomy (2019) that suggest that banks—as a response to falling entry barriers—change their accounting standards and report lower profits to deter potential entrants.

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 Jiménez, Lopez, and Saurina 2013; Braggion, Dwarkasing, and Moore 2017; Gissler, Ramcharan, and Yu 2020),[7] rather than increasing the safety of the banking system (as found in Jayaratne and Strahan 1998; Carlson and Mitchener 2009; Schaeck, Cihak, and Wolfe 2009). However, as we highlight in our discussion in section VIII, it is important to be cautious when generalizing from historical experience, as the institutional details of the National Banking Era we exploit for identification also complicate the comparison to modern banking systems.

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, Frydman, and Papanikolaou 2019), 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, an important contribution of our paper is to provide microevidence 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, Tornell, and Westermann 2008; Reinhart and Rogoff 2009; Schularick and Taylor 2012; Rajan and Ramcharan 2015; Mian, Sufi, and Verner 2020; Jaremski and Wheelock 2020).

III. 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 US national banks between 1867 and 1904. Our source is the Comptroller of the Currency's Annual Report to 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 G.1 (figs. A.1, B.1, B.2, C.1, C.2, D.1, E.1, G.1, and G.2 are available online) for an example of such a balance sheet. The Office of the Comptroller of the Currency (OCC) also reported bank charter numbers, which allow us to track banks across time. Finally, the Annual Reports also contain the names of bank presidents and cashiers, allowing us to collect information on managerial turnover.[8]

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

[8] To identify managerial turnover, it is crucial to correctly track standardized information on bank officer names. However, the OCC Annual Reports do not standardize officer names, which thus vary in spelling quite a lot across time. For instance, the name of the

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 Attorneys*.[9]

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

Finally, we use real economic outcomes at the county level from the 1870, 1880, and 1890 decennial censuses, provided by Haines (2004). In particular, the censuses provide information on manufacturing capital invested, the value of manufacturing products produced, and the number of manufacturing establishments. To track county borders across each census, we use the geographic crosswalk provided by Manson et al. (2017).

IV. 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.

A. *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.[10] Once the banks had opened, they were

cashier of the Lawrence National Bank (charter 3849) is listed in 1889 as "H. C. Vaughn," in 1890 as "H. C. Vaughan" (typo), and in 1891 as "Hiram C. Vaughn" (unabbreviated). To address these concerns, we instead rely on the "Bank and Bankers Database" (Dregson, Huntoon, and Pollock 2021), which painstakingly verified and standardized all the officer names used in our sample. This data set also addresses other issues, such as family members with the same name succeeding each other (e.g., in the Mystic River National Bank, "Henry Byron Noyes Jr." succeeded his father "Henry Byron Noyes Sr.," but both are noted only as "Henry Byron Noyes").

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

[10] There 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. Further, 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 also their own personal property up to the book value of their shares (see also Grossman 2001; Koudijs, Salisbury, and Sran 2021). Next to their standard banking business, national banks also operated an additional line of business, in which they issued bank notes (Calomiris and Mason 2008). Note

free to increase their 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),[11] capital requirements were a barrier to entry rather than a restriction on leverage.[12]

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, a minimum of $50,000 in capital was required to charter a bank. When towns reached 6,000 inhabitants, this requirement doubled to $100,000, and it increased further to $200,000 in towns with more than 50,000 inhabitants:[13]

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

issuance could typically not exceed 90% or 100% of a bank's capital, depending on the year. See Calomiris and Mason (2008) for details on the exact requirements and a comprehensive study of the determinants of note issuance.

[11] Among these papers, Fulford's (2015) is the most similar to ours, in that he uses the discontinuity in required capital for entrants to look at the impact on economic development, a question similar to the one we consider in the final section of our paper. Fulford does not study the impact of the entry barrier on loan growth and risk taking, which is the main focus of our paper. Moreover, there are three important differences between our paper and his concerning how the entry barrier related to economic development. First, Fulford does not distinguish between the effect of having access to finance and the effect of banking competition, while our paper focuses on identifying the effects of the latter. Second, our data are much richer, allowing us to study outcomes beyond the real effects and show that there are actual differences in entry and that banks change their behavior when market contestability changes. Third, given that we have more data, we are able to apply RD techniques and nonparametric regressions, whereas Fulford estimates a structural model.

[12] Moreover, the OCC itself saw the capital regulation governing bank entry. In 1876, in a debate on lowering capital requirements, Jay Knox, in his function as the Comptroller, argued that "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" (Knox 1876). See sec. G of the appendix.

[13] The selection of the 6,000-inhabitants cutoff appears to have been a political compromise. For instance, the proposed "Hooper Bill" of 1862 suggested a $50,000 requirement for all locations. The "Sherman Act" of 1863, in contrast, suggested increasing the capital requirement once a location's population exceeded 10,000 inhabitants. For details, see Davis (1910). In 1900, the capital regulation was refined such that banks founded in towns with fewer than 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% of the regulatory capital, as determined in the bank charter. See James (1978) and Champ (2007) for details.

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.[14] Second, the regulatory capital requirement applied only to national banks that were entering the market and not to incumbent national banks; that is, 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*:

> 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. (Pratt 1886, 12)

The fact that the legal population was determined by the most recent census means that even if the population of every town was changing constantly, the minimum requirement for entrants changed only when the census was published. In line with the regulatory statutes, figure E.1 shows that all banks in our sample that were founded between 1867 and 1899 fulfilled the regulation. While banks could choose to have more capital than required, banks founded in towns with more than 6,000 inhabitants always had at least $100,000, whereas banks in towns with up to 6,000 inhabitants never had less than $50,000 but potentially did have less than $100,000. Moreover, more than two-thirds of all newly founded banks in our sample period opened with the exact minimum capital required, indicating that the constraint was binding in most cases.

[14] 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.

The fact that changes in the capital requirement due to population growth applied only 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.[15]

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

B. *Identification*

To study the effect of bank competition on bank behavior, we exploit that census publications changed entry barriers differentially around the 6,000-inhabitants cutoff. We use the publication of the 1870, 1880, and 1890 censuses 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 banks in towns that had fewer than 6,000 inhabitants in both the preceding and the current census. Formally, we define $\mathbf{1}_{ct}^{\text{pop}>6{,}000}$ as an indicator variable equal to 1 if city c passed the 6,000-inhabitants cutoff in the census of year $t \in \{1870, 1880, 1890\}$ and 0 otherwise; that is,

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

There is another step-up in capital requirements once a town has 50,000 inhabitants. As this higher cutoff also granted eligibility for reserve city status,[16] a potentially important confounding factor, we focus our analysis on the discontinuity around the 6,000-inhabitants cutoff.

[15] 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).

[16] 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.

We restrict the sample to towns with fewer than 6,000 inhabitants according to the last preceding census. Thus, we ensure that both control group and treatment group shared the same entry barriers through the preceding decade, but because a 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 Annapolis, Maryland, had a population of 4,529, 5,744, 6,642, and 7,604 as of the 1860, 1870, 1880, and 1890 censuses. Thus, Annapolis will be a control in 1870, treated in 1880, and excluded in 1890.[17]

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 is conducted in June 1880, and we treat 1881 as the year in which results of the 1880 census are published.[18]

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 a 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% 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 necessarily

Anderson, Paddrik, and Wang (2019) discuss the network structure and its importance for financial stability in detail.

[17] 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 cutoff, a quite rare event. Further, note that constructing the sample by pooling multiple census years gives rise to the concern that we could be selecting a sample of treated markets that are fundamentally different from the control group. If competition induces banking growth and real growth, it possibly also affects population growth and thus endogenously increases the probability of getting treated and thus being subject to higher entry barriers. Additional evidence in the online appendix (see table C.2 [tables B.1, C.1–C.8, and F.1–F.4 are available online]) shows that our main estimates have roughly the same magnitude across different census publications, although the statistical precision varies across decades. Our results are also robust to including decade fixed effects in the pooled specification; see table C.1.

[18] 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.

focuses on the effect of an increase, as opposed to a decrease, in entry barriers following a census publication.

Finally, we exclude towns from the former Confederate states during the 1870s because of concerns that the population counts in the South may be unreliable before and after the Civil War. Moreover, it addresses concerns that results may be driven by peculiarities in the immediate aftermath of the Civil War, during which large parts of the Southern economic infrastructure had been destroyed (see, e.g., Feigenbaum, Lee, and Mezzanotti 2018). Towns from the South are included from 1880 onward, though our results are robust to including or excluding the South in all three decades. Note also that our results are robust to excluding the 1870s altogether.

We arrive at a sample of 2,844 city–census year observations, with 1,686 unique towns. Figure 1 shows the spatial distribution of the sample and shows that treated towns are fairly evenly spatially distributed and not clustered in one specific region. Of those 2,891 city–census year observations, 296 unique towns are treated and cross the 6,000-inhabitants cutoff at a census publication. Further, we identify more than 3,600 bank–census year observations, with around 2,400 unique incumbent national banks, of which more than 500 are in treated markets that are subject to higher entry costs after a respective publication of the census. Our sample thus covers a

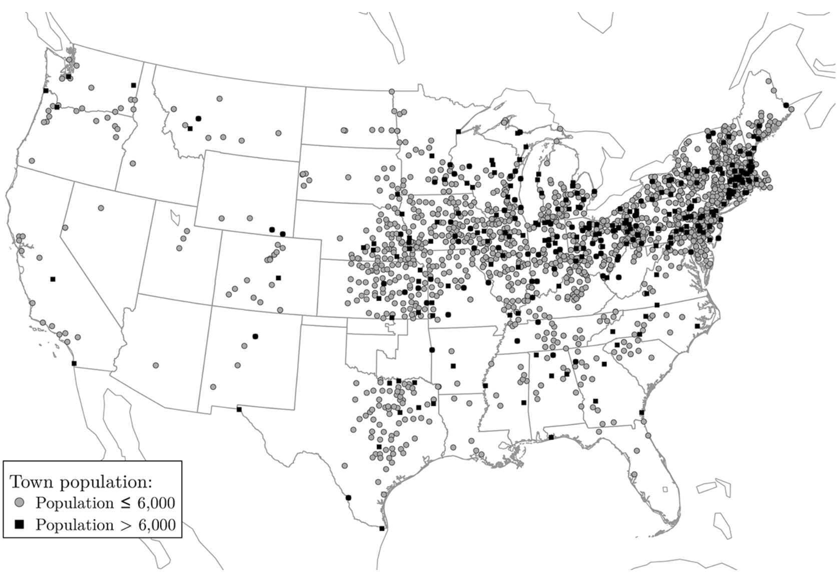

FIG. 1.—Spatial distribution of main sample. This figure maps the spatial distribution of the main sample, that is, cities with one national bank or more at the publication of the 1870, 1880, or 1890 census. Cities indicated by boxes are those that have more than 6,000 inhabitants as of a census publication, and cities indicated by circles are those that have fewer than 6,000. State borders as of 1890.

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.[19]

As noted above, our identification strategy is subject to two main concerns. First, variation in population growth and hence in entry barriers may not be purely random and exogenous. Second, an increase in entry barriers may change whether a bank entrant prefers to enter as a state or nationally chartered bank. In the following, we lay out how we address both concerns.[20]

Starting with the first concern, note that in order to identify an effect of a variation of entry costs on banking behavior, variation in whether or not a town crosses the population cutoff would have 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. Towns that crossed the cutoff might already have had a higher population in the preceding census, might have experienced 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 farther away from the cutoff. For instance, if growth trajectories of towns are concave—that is, 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 using only towns with a population level close to the 6,000-inhabitants cutoff. The identifying assumption is that towns are similar in all other aspects, that is, that covariates are continuous with respect to town size around the cutoff (Hahn, Todd, and Van der Klaauw 2001). To support this empirical approach, we consider a set of observable characteristics for treated and nontreated towns before the publication of the census. Table 1 shows observable characteristics in the year of a census publication for towns with a

[19] For comparison, in 1871, there are around 1,700 national banks, and around 700 of those are in our sample. In 1891, a total of around 3,700 national banks are in operation, and around 2,000 are in our sample.

[20] An additional concern is that entry can potentially also affect the behavior of incumbent banks through an increase in merger activity and overall consolidation of banks in a given town. To explore this possibility, we constructed an exhaustive data set of all national bank mergers during our sample period, using data from Van Belkum (1968) augmented with information from the monthly issues of the *Banker's Magazine and Statistical Registry*. We found mergers to be extremely rare in our sample, and thus accounting for mergers had no material effect in our results. This can be due to the relative difficulty in executing a merger, which, as discussed by White (1985), until 1918 required the voluntary liquidation of at least one of the merging banks. Moreover, mergers were more common in larger cities (Lamoreaux 1991) and less so in cities around the 6,000 population threshold we study. For instance, of the 121 mergers we find in our sample period, only 13 took place in cities with a population between 4,000 and 8,000 inhabitants.

TABLE 1

Descriptive Statistics I: Observable Characteristics for Towns with More than 5,000 and Fewer than 7,000 Inhabitants as of the Publication of the 1870, 1880, and 1890 Censuses

	Population ≤ 6,000			Population > 6,000			Difference	
	Mean	Standard Deviation	N	Mean	Standard Deviation	N	Difference	t-Statistic
Population	5,472.8	276.1	196	6,435.3	291.4	120	962.5	29.448
Δpopulation during previous decade	58.0	95.3	196	62.3	83.1	120	4.3	.409
Δ_{harm}population during previous decade	35.1	30.3	196	39.0	29.9	120	3.9	1.127
No. of national banks	1.6	.7	196	1.6	.8	120	.1	.819
National bank entries in previous decade	.7	.7	196	.7	.8	120	.1	.745
No. of state banks	.6	.8	196	.8	.9	120	.1	1.401
State bank entries in previous decade	.2	.7	142	.2	.7	90	−.0	−.222
Δcapital during previous decade	15.1	41.9	98	16.8	58.6	67	1.7	.216
Δloans during previous decade	43.3	47.5	98	46.9	59.8	67	3.6	.432
Δassets during previous decade	23.3	41.5	98	29.8	50.4	67	6.5	.910
Per capita bank capital	39.2	27.5	196	38.6	30.0	120	−.5	−.153
Per capita bank loans	64.4	48.6	196	63.2	45.2	120	−1.2	−.227
Per capita bank assets	120.2	78.7	196	117.6	74.9	120	−2.6	−.292
No. of manufacturing establishments	394.0	538.9	194	486.6	845.5	120	92.7	1.186
Per capita manufacturing capital	82.9	81.2	194	92.8	98.3	120	9.9	.963
Per capita farm value	343.7	193.4	194	306.6	206.0	120	−37.2	−1.614
No. of farms	3,047.2	1,447.2	194	2,631.1	1,470.2	120	−416.1	−2.461
Years of railroad access	24.3	9.0	196	24.0	9.8	120	−.4	−.357
Railroad access (%)	97.4	15.8	196	97.5	15.7	120	.1	.028
No. of railroad connections	6.0	4.3	196	6.3	4.2	119	.3	.524
Distance to New York City (km)	854.9	701.7	196	885.3	710.8	120	30.4	.372
Distance to next city with more than 50k inhabitants (km)	93.1	76.8	196	102.8	100.4	120	9.7	.972
Distance to next populated location (km)	9.7	9.4	196	11.7	11.3	120	2.0	1.676

Note.—City-level data for towns that have more than 5,000 and fewer than 7,000 inhabitants as of the 1870, 1880, and 1890 censuses. The sample is restricted to cities with at least one national bank at the respective census publication and fewer 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. Railroad data come from Atack (2013). 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.

population of ±1,000 inhabitants around the cutoff. By construction, there are clear differences in population levels—the running variable—and treated towns have, on average, around 1,000 more inhabitants than nontreated towns. However, reassuringly for our purposes, treated and nontreated towns are similar in most other important observable characteristics, such as the number of national and state banks, railroad access, credit growth since the last census publication, and per capita levels of manufacturing capital, establishments, and output. Further, note that panels *A* and *B* of figure B.1 show 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 towns left of the cutoff and those to the right is that smaller towns tended to be located in counties that tended to have more farms, in line with smaller towns being likely to be located in rural areas.[21]

Given the differences in city size between treated and nontreated towns, banks in treated towns tend to be larger and thus have more outstanding loans (see table 2). However, panels *B* and *C* in figure B.1 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 markets 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 towns, which alleviates concerns of preemptive entry in anticipation of the census.

In our main analysis, we formally exploit the discontinuity at the 6,000-inhabitants cutoff and make use of the toolkit developed for the analysis of RD designs (Imbens and Lemieux 2008; Lee and Lemieux 2010; Cattaneo, Idrobo, and Titiunik 2019). Throughout our analysis, we estimate local polynomial regressions (Hahn, Todd, and Van der Klaauw 2001) with tight bandwidths right around the cutoff, applying a variety of different MSE (mean squared error)–optimal bandwidth selection methods (Imbens and Kalyanaraman 2011; Calonico, Cattaneo, and Titiunik 2014). Further, we conduct a series of validation and falsification tests to study whether the effect can be detected only at the true cutoff (Ganong and Jäger 2018) and show that there is no evidence of manipulation around the cutoff (McCrary 2008; Cattaneo, Jansson, and Ma 2018).

[21] Also note that the average distance of towns in our sample to the next city with a bank is around 10 km for both treated and nontreated towns. 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 between 6 and 13 km (Petersen and Rajan 2002; Brevoort, Wolken, and Holmes 2011). Further note that evidence from OCC examiner reports suggest that lending was largely local and within a town/city, and note that our results are robust to excluding towns that are within 5 km of another populated location.

TABLE 2

Descriptive Statistics II: Bank-Level Data for Incumbent National Banks in Towns with More than 5,000 and Fewer than 7,000 Inhabitants as of the Publication of the 1870, 1880, and 1890 Censuses

	Population ≤ 6,000			Population > 6,000			Difference	
	Mean	Standard Deviation	*N*	Mean	Standard Deviation	*N*	Difference	*t*-Statistic
Total assets (000s)	414.7	214.8	307	461.2	239.4	197	46.5	2.264
Capital paid in	108.3	58.4	307	120.1	78.8	197	11.8	1.925
Surplus fund	26.8	25.8	307	31.9	31.2	197	5.1	1.980
Deposits	191.9	132.7	307	209.8	140.7	197	17.9	1.443
National bank notes	63.0	57.2	307	71.1	68.8	197	8.1	1.438
Cash (specie and legal tender)	24.3	19.0	307	26.1	17.5	197	1.8	1.076
Liquid assets	71.7	57.1	307	79.3	63.2	197	7.6	1.390
Loans and discounts	223.0	127.8	307	248.1	150.8	197	25.1	2.006
Debt/assets	66.1	10.2	307	66.7	10.7	197	.6	.680
Equity/assets	33.9	10.2	307	33.3	10.7	197	−.6	−.680
Capital/assets	28.0	9.9	307	26.9	10.0	197	−1.1	−1.170
Loans/assets	54.0	14.1	307	53.7	14.5	197	−.3	−.260
Deposits/assets	45.1	17.0	307	45.5	18.6	197	.4	.224
Cash/assets	6.1	3.6	307	5.9	3.6	197	−.2	−.480
Liquid assets/assets	17.1	8.9	307	16.9	9.1	197	−.2	−.281
Reserves/(required reserves)	250.3	234.3	307	229.2	138.6	197	−21.1	−1.144
Bank president turnover (%)	8.3	15.3	288	7.2	13.2	191	−1.2	−.868
Bank cashier turnover (%)	8.2	16.2	288	8.2	18.5	191	.0	.002
Officers are related (%)	8.3	25.6	307	5.9	21.2	197	−2.4	−1.087
Age	12.2	8.4	307	12.5	8.1	197	.3	.393

Note.—The sample is 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 fewer than 6,000 inhabitants as of the respective previous census. President and cashier turnovers represent the percentage of banks that changed president or cashier in the decade preceding each census release, annualized. As a result of data limitations, for the 1870 census release we use only data from 1867 onward.

The second important 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—because of 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 affect 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-chartered commercial 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 state 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 entries. 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% of banking assets were held by national banks.[22] Third, we directly test for state bank entry and show that there is no evidence of state bank entrants substituting for national bank entrants. Finally, we also provide more background on why state banks and national banks were not perfect substitutes and conduct several other robustness checks in section C of the appendix. For instance, 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. We then confirm our main results for the subset of states in which state bank entry barriers

[22] State banks tended to be smaller than national banks, and the market share of state banks is larger when based on the number of banks. For instance, in 1881, more than 25% of the total number of banks were state banks (White 1983). While we cannot calculate the market share by assets for our sample, we find that in the sample of the around 2,100 city-year observations for which we have information on state bank existence, cities have, on average, 1.32 national banks and 0.51 state banks. The market share of state banks based on the number of state banks per market is thus around 28% for our sample and similar to the national average.

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 were, on average, organized with a larger capital base and may hence by construction be larger; see figure E.1. It may therefore be the case that incumbents react not only 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 by 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.

V. The Effects of Entry Barriers on Competition

In this section, we analyze the effect of increased entry barriers on bank entry and the contestability of a local market. 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-inhabitants cutoff in the years following a census publication.

We begin by providing visual evidence of the effect of higher entry barriers on the degree of local competition. Panels *A* and *B* of figure 2 depict binned scatterplots of the number of new national bank entrants by city throughout the decade following a census publication. We use binned scatterplots because the outcome variable is a discrete number, and a standard scatterplot would thus be uninformative. Bins are equal sized, contain around 20 observations, and are grouped by city population as of the respective census. In figure 2*A*, we include linear fits left and right of the 6,000-inhabitants cutoff, and in figure 2*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 entries over the following decade. However, there is also a discontinuity in the linear fits right around the 6,000-inhabitants cutoff. In the decade following a census publication, about 0.35 national banks entered in towns just left of the cutoff, while only 0.1 national banks entered in cities just right of the cutoff. 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 3 shows an RD 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.'s (2017) optimal data-driven methods

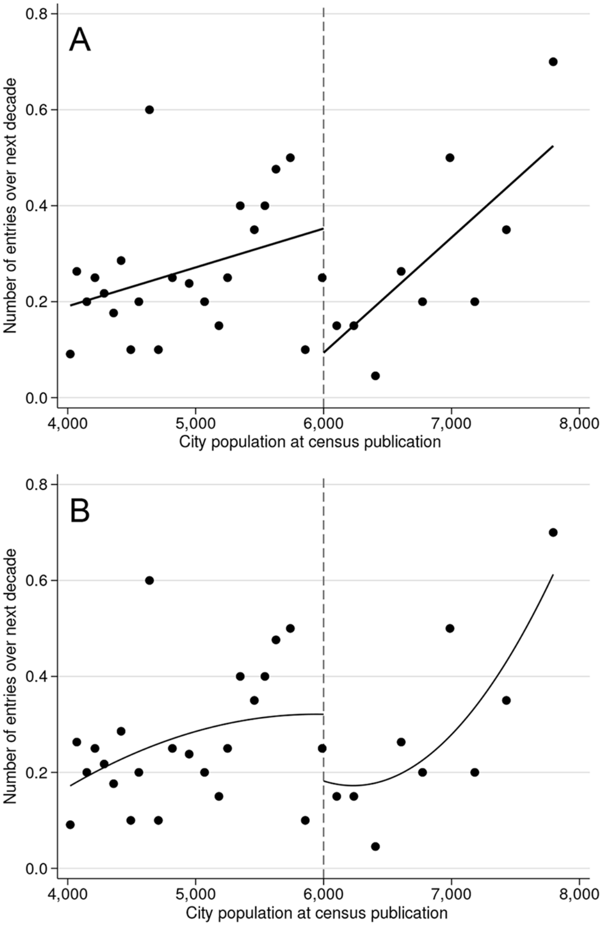

FIG. 2.—National bank entry after census publications. Binned scatterplot of the number of national bank entrants over the decade following a census publication by city population at a census publication. The figure pools data from the publication of the 1870, 1880, and 1890 censuses. This figure is created with the Binscatter package (Stepner 2013), using equal-sized bins with around 20 observations per bin, plus linear (*A*) and quadratic (*B*) fit lines left and right of the 6,000-inhabitants cutoff.

for automatically selecting the number and spacing of bins. As discussed by Cattaneo, Idrobo, and Titiunik (2019), doing so avoids the need for potentially subjective and ad hoc tuning parameters. Panels *A* and *B* show quantile-spaced bins with linear and quadratic fits, respectively. Using quantile-spaced bins has the advantage of taking into account the increasing sparsity of the data as the population size increases. Panels *C* and *D* plot the same outcome variable and polynomial fits, using evenly spaced bins.

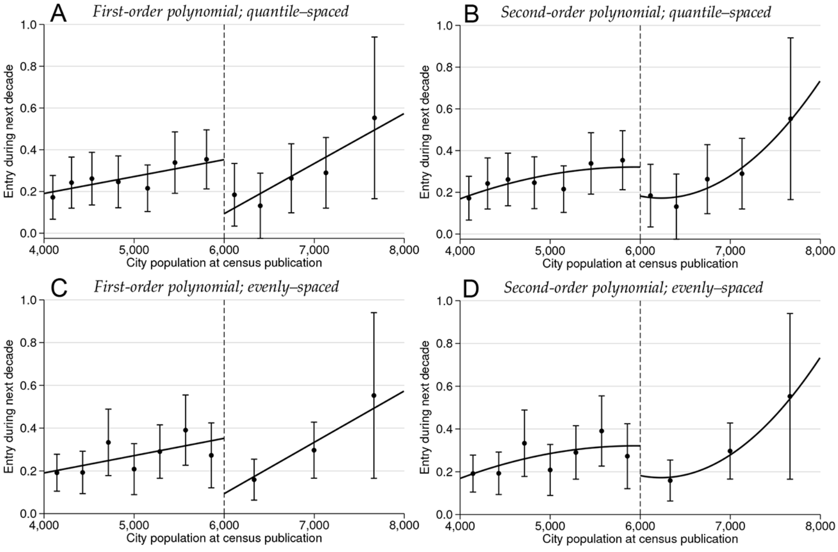

FIG. 3.—Bank entry after census publications: varying choice of polynomial and bin selection method. This figure shows RD plots constructed with the optimal data-driven methods of Calonico, Cattaneo, and Titiunik (2015), created with the rdplot package (Calonico et al. 2017). These methods automatically 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-inhabitants cutoff, with fewer banks opening to the right of the cutoff. *A*, *B*, Integrated MSE (IMSE)–optimal, quantile-spaced bins with local linear (*A*) and quadratic (*B*) polynomials. Using quantile-spaced bins has the advantage of taking into account the increasing sparsity of the data as the population size increases. *C*, *D* Equivalent results with IMSE-optimal, evenly spaced bins. The figure pools data from the publication of the 1870, 1880, and 1890 censuses. 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 are at the 95% level.

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 to the right of the cutoff are outside the confidence intervals just left of the cutoff, reinforcing the evidence on a discontinuity in entry at the 6,000-inhabitants cutoff that affected entry barriers.

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

To formally test the effect of capital regulation on entry in a local market, we estimate local polynomial regressions (Hahn, Todd, and Van der Klaauw 2001; Calonico, Cattaneo, and Titiunik 2014) that allow the functional form of the running variable to vary across treated and nontreated cities. Moreover, we use various bandwidth selection methods and different kernel functions to construct the local estimators.[23] Specifically, in the linear baseline specification, we estimate

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

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 is given by city c's population in census year t, and $\mathbf{1}_{ct}^{\text{pop}>6{,}000}$ is as defined above and is given by a dummy for whether a city's population is larger than 6,000. 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 MSE-optimal bandwidths around the cutoff in two ways, as suggested by Calonico et al. (2017). One case allows the optimal bandwidths to differ left and right of the cutoff, while the other uses a single symmetrical bandwidth. Moreover, for robustness we also consider nonlinear models and allow the running variable to be quadratic or cubic in some specifications.

The results from estimating equation (1) are reported in table 3. In line with the visual pattern of figures 2 and 3, there is a strong negative relationship between being to the right of the 6,000-inhabitants cutoff at a census publication and national bank entry in the subsequent decade. For instance, as reported in column 1, when allowing the bandwidths to be different left and right of the cutoff and using a uniform kernel, it is optimal to estimate the effect from using towns with around 4,000–8,000 inhabitants. Thus, in this specification, we are comparing towns with a rough average of 5,000 inhabitants to towns with an average of 7,000 inhabitants, and we are effectively using 441 observations left of the cutoff and 191 observations right of the cutoff to construct the estimator. Choosing different bandwidths on each side of the cutoff is attractive in our setting, as there are more data points left of the cutoff. In this specification, the estimated

[23] Note that all our results are robust to using parametric estimation techniques; see, e.g., table F.1.

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						Dependent Variable: State Bank Entrants	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conventional	−.26***	−.22***	−.21***	−.23***	−.22**	−.23***	−.18	−.20
	[.09]	[.08]	[.08]	[.09]	[.09]	[.09]	[.21]	[.22]
Bias corrected	−.27***	−.24***	−.23***	−.25***	−.22**	−.24***	−.26	−.28
	[.09]	[.08]	[.08]	[.09]	[.09]	[.09]	[.21]	[.22]
Robust	−.27***	−.24***	−.23***	−.25***	−.22**	−.24**	−.26	−.28
	[.10]	[.09]	[.09]	[.10]	[.10]	[.10]	[.24]	[.25]
Bandwidth type	2 MSE	2 MSE	2 MSE	MSE common	2 MSE	2 MSE	2 MSE	MSE common
Kernel type	Uniform	Epanechnikov	Triangular	Epanechnikov	Uniform	Epanechnikov	Epanechnikov	Epanechnikov
Order local polynomial (p)	1	1	1	1	2	2	1	1
Order bias (q)	2	2	2	2	3	3	2	2
Mean dependent variable	.21	.21	.21	.21	.21	.21	.21	.21
No. of counties	1,029	1,029	1,029	1,029	1,029	1,029	621	621
No. of cities	1,686	1,686	1,686	1,686	1,686	1,686	1,041	1,041
Observations	2,844	2,844	2,844	2,844	2,844	2,844	2,090	2,090
Left of cutoff	2,548	2,548	2,548	2,548	2,548	2,548	1,866	1,866
Right of cutoff	296	296	296	296	296	296	224	224
Left main bandwidth (h)	1,975	2,542	2,688	2,132	2,809	3,156	1,270	1,142
Right main bandwidth (h)	2,010	2,628	2,822	2,132	8,274	9,766	1,435	1,142
Effective observations (left)	441	648	707	493	773	991	237	218
Effective observations (right)	191	218	225	200	287	288	158	134

NOTE.—This table shows results from estimating local linear regressions of the form $y_{ct} = \alpha + \beta_1 \cdot \mathbf{1}_{ct}^{\text{pop}>6{,}000} + \beta_2 \cdot (\text{pop}_{ct} - 6{,}000) + \beta_3 \cdot \mathbf{1}_{ct}^{\text{pop}>6{,}000} \cdot (\text{pop}_{ct} - 6{,}000) + \varepsilon_{ct}$ (eq. [1]), where y_{ct} is the number of national bank entries (cols. 1–6) or state bank entries (cols. 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, uniform, and triangular) across specifications. In particular, in cols. 1–3 and 5–7, we use two different MSE-optimal bandwidth selectors (below and above the cutoff) for the RD treatment effect estimator. In cols. 4 and 8, we use a common MSE-optimal bandwidth selector for the RD treatment effect estimator. In cols. 5 and 6, the running variable is quadratic, as opposed to linear. Optimal bandwidth is calculated with 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 are from the publication of the 1870, 1880, and 1890 censuses. The sample is restricted to cities with at least one national bank at the respective census publication and fewer than 6,000 inhabitants in the respective previous census. Standard errors clustered at the city level are in brackets.

** Significant at the 5% level.

*** Significant at the 1% level.

effect is consistently between −0.26 and −0.27, independent of whether one considers a conventional RD estimate, a bias-corrected estimate, or a robust estimate. Further, note that columns 2 and 3 show that the effect is robust to the kernel 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 ±2,132. Under this bandwidth selection, to construct the estimator we effectively use only towns with around 3,900–8,100 inhabitants, 493 observations to the left of the cutoff and 200 to the right. Again, we find that towns right of the cutoff see around 0.23–0.25 fewer entrants following a census publication. Finally, columns 5 and 6 also show that the results are robust in magnitude and precision when allowing the running variable to be quadratic or cubic, as opposed to linear, although the effect becomes slightly weaker.[24] Finally, in section A of the appendix, we also show that the coefficient is stable for even narrower symmetric bandwidths and that we can detect an effect on entry using towns only within 1,000 inhabitants around the cutoff.

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

A. 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 4*A*, we plot coefficients for $\mathbf{1}_{ct}^{\text{pop}>X}$ from estimating local linear regressions of the form

$$\begin{aligned} y_{ct} = {} & \alpha + \beta_1 \cdot \mathbf{1}_{ct}^{\text{pop}>X} + \beta_2 \cdot (\text{pop}_{ct} - X) \\ & + \beta_3 \cdot \mathbf{1}_{ct}^{\text{pop}>X} \cdot (\text{pop} - X) + \varepsilon_c, \end{aligned} \tag{2}$$

where y_{ct} is the number of bank entries in city c in the decade following the publication of the year t census and X defines a cutoff that we vary between 4,000 and 8,000 inhabitants in incremental steps of 10. We estimate equation (2) for a fixed bandwidth of ±2,000 inhabitants around the cutoff X. We choose the manually selected symmetric bandwidths in order to ensure comparability of coefficients across regressions. Finally, instead of excluding towns with more than 6,000 inhabitants as of the previous census, we exclude all towns with more than X inhabitants as of the previous census.

[24] Following Gelman and Imbens (2019), we focus mostly on linear and quadratic polynomials.

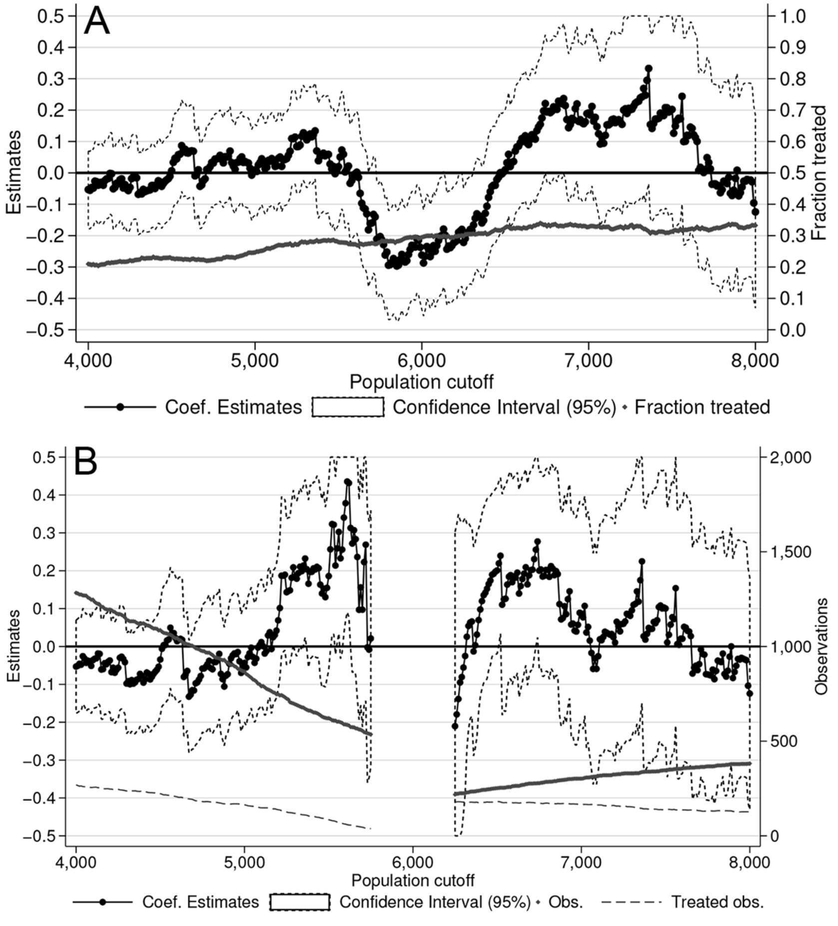

FIG. 4.—Validation and falsifications test: sensitivity of the effect on bank entry to varying cutoffs. In both panels, the dependent variable is the number of national bank entrants in the decade following a census publication. We use fixed bandwidths of ±2,000 to ensure comparability across estimations 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 confidence bands are at the 95% level. Coef. = coefficient; Obs. = observations.

Figure 4*A* reveals that the effect on bank entry is estimated to be the strongest near the true cutoff of 6,000. Once the cutoff is moved to a higher or lower population level, the estimates cease to be statistically significant. Being able to show that the results hold only 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 nonlinear effects of town growth, we conduct additional permutation tests by estimating equation (2) for a sample of either only nontreated towns or only treated towns, again varying population cutoffs. For instance, we exclude all treated towns with more than 6,000 inhabitants and compare only towns that have the same, low entry barriers and then vary the cutoff between 3,000 and 5,750. Alternatively, we exclude all nontreated towns and vary the cutoff between 6,250 and 9,000. If more general trends in town growth were 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 fewer entrants. The results are presented in figure 4*B* and indicate that there are no negative effects at any cutoff on bank entry in these restricted samples. This emphasizes that it is unlikely that our results are driven by town growth patterns rather than the change entry barriers.

An additional important concern is that agents could engage in manipulation or find ways to avoid the regulatory requirements. In the context of our study, there are two plausible worries. Either agents could manipulate the reported population around the cutoff, leading to bunching of towns on either side of the cutoff, or prospective bankers could avoid higher capital requirements by opening banks in neighboring towns that are subject to lower entry barriers. To this end, note that in section B of the appendix, we show that there is no evidence of either manipulation around the cutoff or circumvention of the requirement by relocating to neighboring towns.

B. State Bank Entry

Next, we address the concern that prospective bankers unwilling or unable to raise the $100,000 required to open a national bank in a town above the 6,000-inhabitants cutoff might instead have opted to open state-chartered banks. State banks were not subject to the same regulatory requirements as national banks. While state bank capital requirements varied widely across states, as documented by White (1983), most states required banks to have a minimum capital paid in based on the local population—conceptually equivalent to those of national banks. However, the amount required would typically be lower. Thus, the concern arises that 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 state bank entrants as the dependent variable. Columns 7 and

8 of table 3 show these results.[25] 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 negative but relatively small, compared to the standard errors, 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 several other reasons why state banks might not have been easily able to substitute for national banks. For instance, state banks had a comparative disadvantage in issuing bank notes and were less integrated into the interbank network. Given the relatively lax regulation at the state level, state banks were generally perceived as less safe institutions and were not as well reputed as national banks. State banks also tended to settle in locations where national banks were not viable (Barnett 1911; White 1983), and to the extent that they coexisted in the same location, some types of state banks, such as savings banks, tended to be complementary to national banks (Jaremski and Plastaras 2016).

In section C of the appendix, we provide additional background on state banking. For instance, we show that state banks in our sample are, to a large extent, mutual savings banks in the New England area that were not seen as competitors to national banks. Further, we also show that our results are robust to excluding regions in which state banks were more likely to be competitors to national banks, such as regions dominated by agriculture. Finally, we exploit that state bank entry requirements were relatively stricter in some states, which allows us to conduct an additional robustness check. In particular, we identify a subset of states where competition from state bank entry was unlikely in any scenario and confirm that our main results hold in these states as well.

Altogether, in line with national and state banks being imperfect substitutes, our evidence suggests that towns subject to higher entry barriers for national banks had a lower actual frequency of national bank entry but not a higher frequency of state bank entry. Hence, our evidence suggests that an increase in capital requirements for national banks is a good predictor for the ease with which a local market could have been contested.

VI. 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

[25] Note that we have state bank data for only a subset of the towns in our main sample. Our findings on national bank entry are robust to using the subsample of towns for which we have state bank data. Our results are also robust to using the total number of bank entrants as the dependent variable.

banks. We contrast how incumbents behaved in markets with low and high entry barriers in the decade following a census publication, in the following dimensions. First, we ask whether higher entry barriers affected their credit provision, deposit issuance, or other balance sheet components. Second, we provide more analysis on the mechanism through which competition affected outcomes, and we study whether differences in credit provision were the result of incumbents attempting to deter entry. We also consider whether relationships between lenders and borrowers may have attenuated the effects of competition. Finally, we study the connection between competition and financial stability, whether banks were more likely to fail if they were located in more contestable markets, and whether indicators of banks' risk taking differed on the basis of local entry barriers.

A. *Loans, Deposits, and Total Assets*

We start our study of the effect of entry barriers on credit provision by plotting incumbent loan growth in the decade following a census publication by town population; see figure 5*A*. The figure reveals that loan growth is larger for banks in larger towns but drops discontinuously around the 6,000-inhabitants cutoff, which indicates that incumbents provided less credit when their markets became more difficult to contest.

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

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

where y_{bt} is a bank-level outcome variable such as the growth rate of loans, deposits, or assets in the 10 years following the publication of the census of year *t*, and pop_{ct} and $\mathbf{1}_{ct}^{\text{pop}>6{,}000}$ are defined as above. Note that, also as above, we estimate the model using various bandwidth selection methods and 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 and have larger loan portfolios (see table 2), we also control for bank size and bank loans (in logs), using the covariate-adjustment approach developed by Calonico et al. (2019).

Table 4 reports the 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 4,000 and 9,000 to estimate the effect of higher entry barriers on loan growth. The estimates indicate that loan growth is around 12–14 percentage points lower in the 10 years

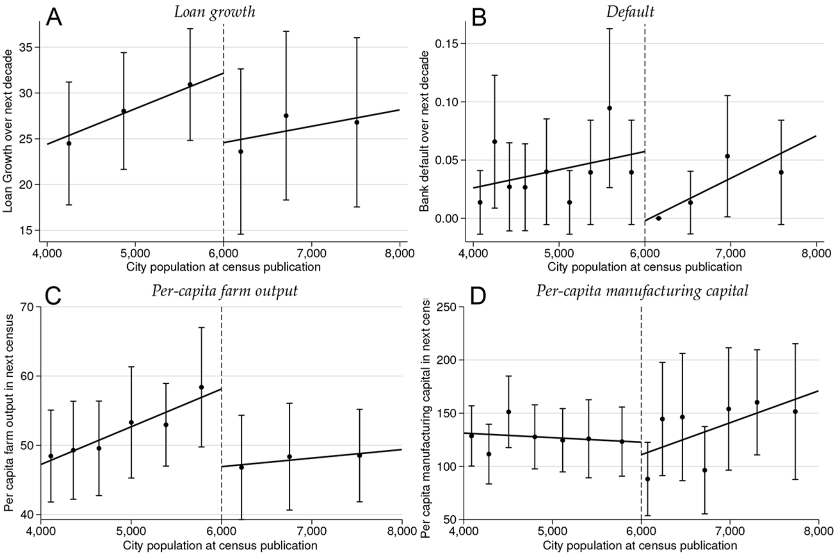

FIG. 5.—RD plots for loan growth, bank default, and real economic outcomes. This figure shows RD plots constructed with the optimal data-driven methods of Calonico, Cattaneo, and Titiunik (2015), created with the rdplot package (Calonico et al. 2017). These methods automatically select the number of bins as well as the spacing between them, thus avoiding the need for potentially subjective and ad hoc tuning parameters. All panels show integrated MSE (IMSE)–optimal, quantile-spaced bins with local linear polynomials. Using quantile-spaced bins has the advantage of taking into account the increasing sparsity of the data as the population size increases. The figure pools data from the publication of the 1870, 1880, and 1890 censuses in *A*–*C* and only data of the 1880 and 1890 censuses in *D*. The running variable is the population of each town at each respective census publication, and the dependent variable is loan growth in the decade following a census publication (*A*), an indicator variable for bank default in the decade following a census publication (*B*), per capita county-level farming output (*C*), or per capita county-level manufacturing capital as of the next census (*D*). Confidence bands are at the 95% level.

that followed the census publication if a bank operated in a market with high entry barriers. The difference is substantial, given the unconditional 10-year growth rate of 27 percentage points. Further, columns 2–5 show that this 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 that across specifications, most bias-corrected estimates are significant at the 5% level. Finally, in section A of the appendix, we also show that the effect of entry barriers can be detected for bandwidths that are tighter than the MSE-optimal bandwidth and that the effect is statistically significant when only towns with 500 inhabitants around the cutoff are used.

Next, we investigate whether the additional loan growth in markets with lower entry barriers was financed by an expansion of the banks' balance

TABLE 4
Credit: Bank-Level Evidence on Incumbent Bank Loan Growth in the Decade Following a Census Publication

	Dependent Variable: ΔLoans						
	All Cities					No New Entrants	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Conventional	−11.81*	−12.25**	−11.70*	−13.76**	−13.93**	−16.52**	−13.66**
	[6.13]	[6.13]	[6.45]	[6.44]	[6.63]	[7.59]	[6.95]
Bias corrected	−14.25**	−14.62**	−13.66**	−15.52**	−14.07**	−18.33**	−14.84**
	[6.13]	[6.13]	[6.45]	[6.44]	[6.63]	[7.59]	[6.95]
Robust	−14.25**	−14.62**	−13.66*	−15.52**	−14.07*	−18.33**	−14.84*
	[6.81]	[6.82]	[7.23]	[7.09]	[7.22]	[8.86]	[7.92]
Bandwidth type	2 MSE	2 MSE	MSE common	2 MSE	MSE common	2 MSE	MSE common
Kernel type	Epanechnikov	Triangular	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov
Order local polynomial (p)	1	1	1	2	2	1	1
Order bias (q)	2	2	2	3	3	2	2
Mean dependent variable	26.87	26.87	26.87	26.87	26.87	23.63	23.63
No. of counties	1,029	1,029	1,029	1,029	1,029	791	791

No. of cities	1,673	1,673	1,673	1,673	1,673	1,288	1,288
No. of banks	2,358	2,358	2,358	2,358	2,358	1,725	1,725
Observations	3,077	3,077	3,077	3,077	3,077	2,473	2,473
Left of cutoff	2,641	2,641	2,641	2,641	2,641	2,182	2,182
Right of cutoff	436	436	436	436	436	291	291
Left main bandwidth (h)	2,052	2,171	2,352	2,733	5,337	1,792	2,269
Right main bandwidth (h)	2,972	3,151	2,352	10,109	5,337	1,607	2,269
Effective observations (left)	593	651	729	893	2,562	381	539
Effective observations (right)	320	327	295	423	392	191	214

NOTE.—This table presents results from estimating $y_{bt} = \alpha + \beta_1 \cdot \mathbf{1}_{ct}^{\text{pop}>6{,}000} + \beta_2 \cdot (\text{pop}_{ct} - 6{,}000) + \beta_3 \cdot \mathbf{1}_{ct}^{\text{pop}>6{,}000} \cdot (\text{pop}_{ct} - 6{,}000) + \varepsilon_{bt}$ (eq. [3]), where y_{bt} is bank b's loan growth over the 10 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 cols. 1, 2, 4, and 6, we use two different MSE-optimal bandwidth selectors (below and above the cutoff) for the RD treatment effect estimator. In col. 3, we use a common MSE-optimal bandwidth selector for the RD treatment effect estimator. Optimal bandwidth is calculated with the rdrobust package (Calonico et al. 2017). In cols. 4 and 5, the running variable is quadratic, as opposed to linear. In cols. 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 are from incumbent national banks at the publication of the 1870, 1880, and 1890 censuses. The sample is restricted to banks in cities with a fewer than 6,000 inhabitants in the respective previous census. Standard errors clustered at the city level are in brackets.

* Significant at the 10% level.

** Significant at the 5% level.

sheets 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 capital paid in, total equity, deposits, cash, reserves, and total assets as outcome variables.[26]

In line with the lower credit provision in markets with higher entry barriers, table 5 shows that these banks also have an 11–13 percentage point lower growth in deposits and a 9–10 percentage point lower growth in equity and overall assets, although the latter is relatively imprecisely estimated. These findings are in line with a fall in both loan supply and deposit demand by incumbents when entry barriers increase, inducing a drop in overall intermediation activity.

Further, there is also no statistically significant difference in the growth of reserves and cash. The lower credit provision of banks in towns with higher entry barriers coincides with a contraction of the banks' deposit base, equity finance, and overall assets rather than an increase in liquid assets.

1. Entry Deterrence

Naturally, it is of interest to learn more about the mechanism giving rise to the differential behavior documented above. 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, 1982b; Klemperer 1987).

To shed light on this question, we first estimate equation (3) with a sample consisting only of towns in which the number of national banks is unchanged 10 years after a census publication. Restricting the sample in this way leaves us with 1,288 towns in which the number of competing banks is unchanged in at least one decade. 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. Observing differential behavior across markets with different entry barriers but with no changes in the number of competitors could be taken as an indication that entry barriers alone can determine bank behavior.

[26] Total 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 funds due from reserve agents. Cash is the sum of specie, fractional currency and coins, and legal-tender notes.

TABLE 5

CAPITAL, EQUITY, DEPOSITS, CASH, RESERVES, AND ASSETS: BANK-LEVEL EVIDENCE ON GROWTH RATES OF VARIOUS BALANCE SHEET ITEMS IN THE DECADE FOLLOWING A CENSUS PUBLICATION

	DEPENDENT VARIABLE					
	ΔCapital Paid In (1)	ΔEquity (2)	ΔDeposits (3)	ΔCash (4)	ΔReserves (5)	ΔAssets (6)
Conventional	−5.20	−5.67	−10.74*	−1.22	−5.12	−8.90
	[4.01]	[4.27]	[6.45]	[9.56]	[9.75]	[5.66]
Bias corrected	−7.25*	−7.57*	−12.61*	−2.20	−9.14	−10.38*
	[4.01]	[4.27]	[6.45]	[9.56]	[9.75]	[5.66]
Robust	−7.25	−7.57	−12.61*	−2.20	−9.14	−10.38
	[4.50]	[4.78]	[7.36]	[11.13]	[11.11]	[6.48]
Bandwidth type	2 MSE	2 MSE	2 MSE	2 MSE	2 MSE	2 MSE
Kernel type	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov
Order local polynomial (p)	1	1	1	1	1	1
Order bias (q)	2	2	2	2	2	2
Mean dependent variable	−1.32	4.70	50.87	20.57	38.73	27.74
No. of counties	1,029	1,029	1,029	1,029	1,029	1,029
No. of cities	1,673	1,673	1,673	1,673	1,673	1,673
No. of banks	2,358	2,358	2,358	2,358	2,358	2,358
Observations	3,077	3,077	3,075	3,077	3,077	3,077
Left of cutoff	2,641	2,641	2,639	2,641	2,641	2,641
Right of cutoff	436	436	436	436	436	436
Left main bandwidth (h)	1,408	1,300	2,547	2,046	1,875	1,456
Right main bandwidth (h)	2,800	3,156	3,044	3,301	3,276	2,863
Effective observations (left)	352	313	805	592	526	362
Effective observations (right)	313	327	324	333	333	318

NOTE.—The table presents results from estimating $y_{bt} = \alpha + \beta_1 \cdot \mathbf{1}_{ct}^{\text{pop}>6{,}000} + \beta_2 \cdot (\text{pop}_{ct} - 6{,}000) + \beta_3 \cdot \mathbf{1}_{ct}^{\text{pop}>6{,}000} \cdot (\text{pop}_{ct} - 6{,}000) + \varepsilon_{bt}$ (eq. [3]), where y_{bt} is bank b's growth capital, deposits, assets, and so on over the 10 years following the publication of the census from year $t \in \{1870, 1880, 1890\}$ census. In all specifications, we use two different MSE-optimal bandwidths selectors (below and above the cutoff) to construct the RD treatment effect estimator. Optimal bandwidth is calculated with 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 are from incumbent national banks at the publication of the 1870, 1880, and 1890 censuses. The sample is restricted to banks in cities with fewer than 6,000 inhabitants in the respective previous census. Standard errors clustered at the city level are in brackets.

* Significant at the 10% level.

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 hold up in this specific subset of markets. If anything, the effects are even stronger in magnitude. This shows that a change in entry barriers can have effects on credit provision even if no measurable change in market concentration has taken place and indicates that incumbent decisions are governed importantly by consideration of potential entrants.[27]

This interpretation is further supported by evidence on the dynamics of loans and deposits of incumbent banks around census publications. In particular, we estimate the following model with data in a 10-year window around census publication (up to 4 years before and 5 years after):[28]

$$y_{bs} = \eta_s + \gamma_b + \beta_\tau \cdot \mathbf{1}_{ct}^{\text{pop}>6,000} + \delta_\tau \cdot X_{bs} + \varepsilon_{bs}, \qquad (4)$$

where y_{bs} is the natural logarithm of bank b's loans or deposits in calendar year s; $\mathbf{1}_{ct}^{\text{pop}>6,000}$ is an indicator of whether city c crossed the 6,000-inhabitants cutoff in census year t; τ is defined as the difference between calendar year s and its closest census year t, so $\tau \in \{-4, \dots, 5\}$. The set of coefficients $\{\beta_{-4}, \dots, \beta_5\}$ thus provide information about the dynamics of loans or deposits around census years, comparing markets that end up with higher entry barriers to those that do not. In addition, η_s are calendar year fixed effects, γ_b are bank fixed effects, and X_{bs} are a set of control variables such as the lagged bank size measured in log assets and the lagged equity ratio. Further, the coefficients δ_τ can vary across time, allowing the relationship between control variables and outcomes to change before and after the increase in entry barriers.

For this analysis, we manually restrict our sample to towns with ±2,000 inhabitants around the 6,000-inhabitants cutoff to make sure that we are comparing sufficiently similar banks. Further, we restrict the sample to data points around the censuses of 1880 and 1890 and exclude 1870, so that

[27] The finding that our results are robust to considering only markets in which no entry occurs also partially addresses the concern that results may be driven not by competition per se but by whether the entrant is more or less likely to be a large bank. (Entrants above the cutoff are, on average, larger because of a higher required minimum capital, so incumbents above 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 below the cutoff can also be large entrants with a high regulatory amount of capital paid in (as evidenced in fig. E.1). Ultimately, however, we cannot empirically distinguish whether incumbent behavior above the cutoff is a response to a lower probability of getting an additional competitor or a reaction to the prospect of a relatively larger competitor.

[28] We have an asymmetric event window (−4 to +5) because otherwise it would possible to have the same city-year observation twice in our regression but at different times relative to a census publication event.

we use data for the period from 1876 to 1896. We do so for two reasons. First, the OCC Annual Reports for 1863–66 did not include bank-level information, so we have data only from 1867 onward. Second, the data for 1867–68 were collected with a different procedure, which limits comparisons with subsequent data (they were collected at predetermined call dates instead of unannounced call dates; see the 1869 OCC Annual Report, p. VIII, under the heading "Reports"). Thus, we do not have a consistent preevent sample for the 1870 census publication.

The results are presented in figure 6. Figure 6*A* shows the coefficients relating higher entry requirements to incumbent credit provision over time. While there is no differential growth in loans before a census publication across markets with different entry requirements before a census publication, a differential appears immediately after the publication of the census. Figure 6*B* shows the coefficients resulting from estimating equation (4) on deposits. As with credit, deposit growth falls quickly after census publication.

The abrupt drop in both deposits and loans is striking, as actual entry is infrequent, so that variation in actual concentration for markets in our sample evolves only gradually. Recall from table 3 that the unconditional probability of entry is around 0.2 over a period of 10 years in our sample. Thus, the abrupt and direct response of incumbents is in line with the idea that incumbents react to changes in their market power that are not directly measured by market concentration and that faster loan growth occurs as incumbents attempt to deter entry by increasing their overall intermediation activity. As the pressure to deter entry drops when entry barriers increase, incumbents are quickly able to make use of the additional market power.

We also investigate how incumbent market shares and overall market size react to the entry of a new bank (this analysis is detailed in sec. D of the appendix). We find that while overall market size increases, incumbents experience a persistent drop in their market share of both deposits and loans after a new entrant contests a market. These findings support and reinforce the idea that the actual arrival of an additional competitor comes, in part, at the expense of incumbents, making entry deterrence attractive to begin with.

Altogether, our evidence suggests that higher entry barriers lead to a lower degree of credit provision. Moreover, the incumbent bank behavior seems to have been governed largely by changes in the threat of entry, rather than simply driven by actual entry of competitors. Our findings on entry deterrence are consistent with results in other industries and time periods, such as those of Goolsbee and Syverson (2008), who find that pricing in the airline industry is partly driven by attempting to deter entrants. They are also consistent with findings from Tomy (2019), who shows that decreasing entry barriers results in incumbents reporting lower profits to deter entrants.

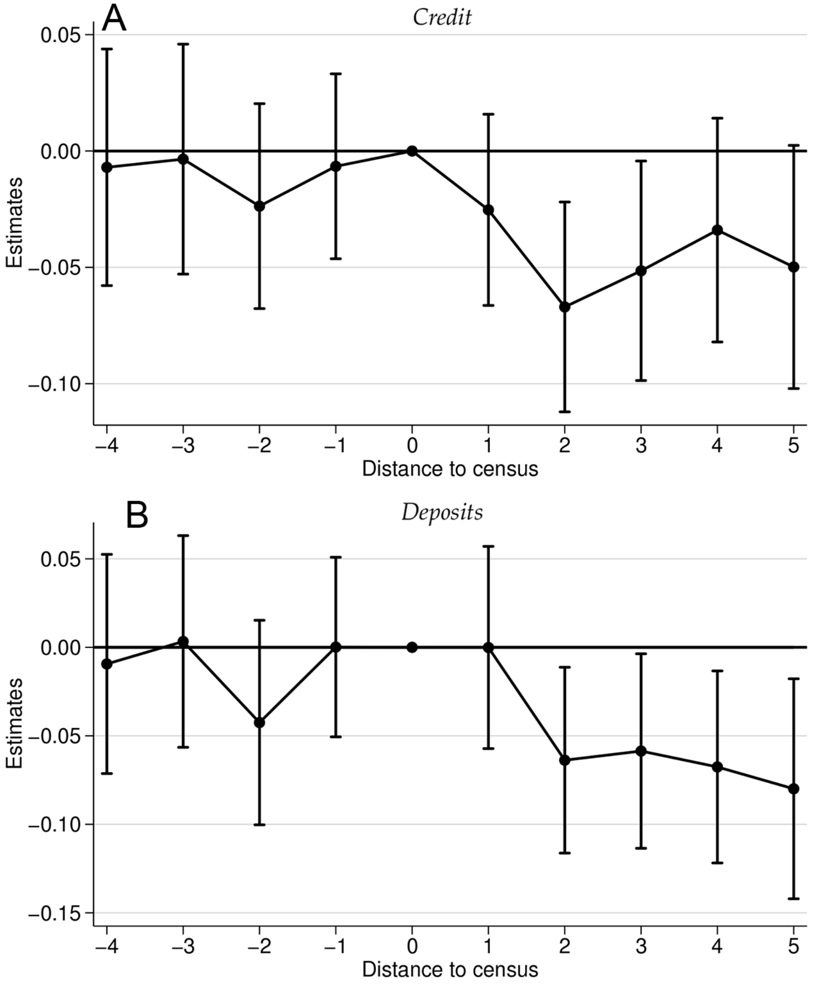

FIG. 6.—Effect on entry barriers on loan and deposit growth. The figure shows the set of coefficients $\{\beta_{-4}, \ldots, \beta_5\}$ obtained from estimating $y_{bs} = \eta_s + \gamma_b + \beta_\tau \times \mathbf{1}_{ct}^{\mathrm{pop}>6,000} + \delta_\tau \times X_{bs} + \varepsilon_{bs}$ (eq. [4]), where y_{bs} is the natural logarithm of bank b's loans (A) or deposits (B) in calendar year s; $\mathbf{1}_{ct}^{\mathrm{pop}>6,000}$ is an indicator of whether city c crossed the 6,000-inhabitants cutoff in census year t; τ is defined as the difference between calendar year s and its closest census year t, so $\tau \in \{-4, \ldots, 5\}$. We normalize coefficients to 0 in the year of the census, $t = s$. The sample is restricted to using data from 1876–96. $N = 5{,}420$, number of banks = 694, $R^2 = 0.92$ in A and 0.93 in B. Standard errors are clustered at the city level, and confidence bands are at the 95% level.

Thus, it appears that entry deterrence can also be a driver in bank credit provision and that changes in the market structure can have substantial effects on banking outcomes even in absence of any changes in measurable market concentration.

2. Relationship Banking

While we find that credit increased as a response to increased competition, recall that a priori the effects of an increase in competition on credit are unclear. Increased competition can plausibly even induce a contraction in credit when relationships between lenders and borrowers are especially important (see, e.g., Petersen and Rajan 1995).[29] Thus, we next ask, Is there evidence that relationships attenuated the positive effects of banking competition?

To measure the importance of relationships, we exploit variation in the tenure of a bank's president and whether a change in the bank's most important management position had taken place recently. During the National Banking Era, the bank president was a major stock owner of the bank and typically by far the most influential person within the organization. Further, presidents—who tended to be very wealthy individuals (Koudijs, Salisbury, and Sran 2021)—also tended to be well connected in the local industry and thus were key for the relationships with the bank's most important and largest customers. Finally, presidents tended to have relatively long tenures and, on average, spent around 9–10 years at the top of the bank.

Given these facts, we argue that president turnover is a good way to measure the depth of existing relationships of a bank.[30] The longer a president had been around, the more likely the bank had strong relationships with local businesses. We thus reestimate our main model for loan growth for a set of banks that had a new president in the past three years and for a set of banks that had a president with a tenure of more than 9 years.[31]

Results can be found in table F.2. Columns 1 and 2 show that the effect of a higher barrier to entry on loan growth is stronger in the sample of banks that had a new president in the three years before the census. For this sample of banks, loan growth was 27–30 percentage points slower at banks in

[29] On the one hand, reduced market power should lead to a reduction in the offered loan rates and an increase in offered deposit rates, increasing the overall amount of credit and deposits. On the other hand, creditors are more likely to finance credit-constrained firms when credit markets are concentrated, because it is easier for these creditors to internalize the benefits of assisting the firms (Petersen and Rajan 1995).

[30] Note that using president turnover as a measure raises the concern that turnover may be endogenous. Markets in which a president is more likely to get replaced may be fundamentally different from those in which tenure is long. However, this concern is alleviated by work showing that the most frequent reason by far for turnover of the president is the death of the incumbent president. See, for instance, Calomiris and Carlson (2016), who find that the death or severe illness of the president was the stated reason for turnover in about two-thirds of cases for which information on the reason for turnover was available. This evidence thus suggests that president turnover is likely to be orthogonal to local lending conditions.

[31] Splitting the sample this way roughly amounts to splitting the sample into the upper and lower terciles of the distribution of president tenure for a given census year.

towns with higher entry barriers than at similar banks in towns with lower entry barriers. That is more than twice as high as the average effect we estimate in our main specification in table 4. In contrast, although loan growth is slower, on average, in less competitive markets for banks with longer-tenured presidents, the effect is weaker, only 7–17 percentage points—close to the average effect in table 4 but less precisely estimated. Thus, the sensitivity of a bank's lending decision to changes in entry barriers is arguably higher when a bank has recently changed its president. This higher sensitivity could plausibly be driven by the fact that new presidents have not yet been able to establish relationships with the bank's active and potential customers. In contrast, once a bank is "established" and has already developed relationships with its customer base, other sources of market power, such as entry barriers, matter less.

Thus, while we find that incumbents in more competitive markets provided more credit—in part to deter entry—we also find that the effect is attenuated when relationships are likely to have been more developed. This result is also in line with established relationships being themselves a source of market power (Sharpe 1990; Rajan 1992).

B. Bank Failures

We next analyze how competition affected financial stability. We start by asking whether banks in more competitive markets were more or less likely to fail. Our sample 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 former has been attributed to the end of a railroad boom and the latter to concerns about the commitment of the United States 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 and widespread bank failures (Romer 1986; Davis 2004).

To study the relation of entry barriers and bank failures, we construct an indicator variable for whether a receiver was appointed and a bank defaulted sometime in the decade after a census publication.[32] As with bank entry and loan growth, we can plot the default probabilities in the decade following a census publication by town population; see figure 5*B*. The figure reveals that the unconditional default probability is around 4%. However, considering the towns just left and right of the cutoff, we see that the default probability drops from between 5% and 10% just left of the cutoff

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

to zero in towns just right of the cutoff. Thus, the visual evidence suggests that incumbents become less likely to fail when their markets become more difficult to contest.

We also study this effect more formally, estimating equation (3) with the indicator variable of whether a bank failed as the dependent variable, again applying various bandwidth selection methods. Columns 1 and 2 of table 6 report these results. The coefficients in columns 1 and 2 confirm the visual evidence and show 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 an 8 percentage point lower failure probability, which is considerable, given an unconditional default probability of 4 percentage points.

The above finding concerns only the extensive margin of bank default and tests only whether a bank defaults or not. The OCC, however, also reported the losses incurred by depositors upon final liquidation of the bank. This variable allows us to deepen our understanding of the effect of entry barriers on default: upon default, how big are the losses? This is important, as in principle the intensive margin could offset the effect of the extensive margin.

We thus reestimate our original regression on bank default but use the loss ratio as the dependent variable. Doing so allows us to estimate the combined effect of the extensive and intensive margins. Results are shown in columns 3 and 4 of table 6. While the above findings indicate that a bank default is about 8 percentage points less likely for banks in markets with higher entry barriers, our findings here suggest that the expected loss ratio is around 4 percentage points lower for banks in these less competitive markets. This additional finding is again in line with the marginal borrower being riskier in more competitive markets.

We interpret our results as indicating that banks in more competitive towns provide more credit but are also more likely to fail. A potential alternative explanation for our results is that they might reflect particular developments in the American economy from 1870 through 1900. During this period, the US population doubled. Many towns grew more or less steadily. However, there were also places where population growth boomed amid excessive land expansion in the early post–Civil War years. The excessive population growth was in part driven by mistaken beliefs about the fertility of Western lands that led to overinvestment in some areas and was followed by stagnation or declines in population as these booms collapsed into busts (see, e.g., Bogue 1955). Such stagnation following a period of “excessive” land expansion may be associated with a higher chance of financial distress.[33] Calomiris and Gorton (1991) discuss that

[33] Moreover, distress may have been exacerbated by deflation that occurred in this era as well as by the repeal of the Sherman Silver Purchase Act in 1893.

TABLE 6
RISK TAKING I: BANK-LEVEL EVIDENCE ON BANK DEFAULT IN THE DECADE FOLLOWING A CENSUS PUBLICATION

	Dependent Variable: Default		Dependent Variable: Loss Ratio	
	(1)	(2)	(3)	(4)
Conventional	−.07***	−.08***	−.03***	−.04***
	[.02]	[.02]	[.01]	[.01]
Bias corrected	−.08***	−.09***	−.04***	−.04***
	[.02]	[.02]	[.01]	[.01]
Robust	−.08***	−.09***	−.04***	−.04***
	[.02]	[.03]	[.01]	[.01]
Bandwidth type	2 MSE	MSE common	2 MSE	MSE common
Kernel type	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov
Order local polynomial (p)	1	1	1	1
Order bias (q)	2	2	2	2
Mean dependent variable	.04	.04	.02	.02
No. of counties	1,029	1,029	1,029	1,029
No. of cities	1,673	1,673	1,673	1,673
No. of banks	2,358	2,358	2,358	2,358
Observations	3,650	3,650	3,645	3,645
Left of cutoff	3,125	3,125	3,123	3,123
Right of cutoff	525	525	522	522
Left main bandwidth (h)	2,406	1,818	2,399	1,840
Right main bandwidth (h)	1,550	1,818	1,615	1,840
Effective observations (left)	858	590	854	597
Effective observations (right)	267	286	275	286

NOTE.—This table presents results from estimating $y_{bt} = \alpha + \beta_1 \cdot \mathbf{1}_{ct}^{\text{pop}>6,000} + \beta_2 \cdot (\text{pop}_{ct} - 6,000) + \beta_3 \cdot \mathbf{1}_{ct}^{\text{pop}>6,000} \cdot (\text{pop}_{ct} - 6,000) + \varepsilon_{bt}$ (eq. [3]), where y_{bt} can be one of three possible outcome variables. In cols. 1 and 2, the outcome is a dummy for whether a bank defaults (receiver appointed) over the course of the decade following a census publication. In cols. 3 and 4, the outcome variable is the loss ratio, defined as the ratio of assets deemed invaluable in receivership relative to the amount of total assets available in receivership. The ratio is, by construction, 0 for all banks that do not default. We vary bandwidth selection methods. In particular, in cols. 1 and 3, we use two different MSE-optimal bandwidths selectors (below and above the cutoff) for the RD treatment effect estimator. In cols. 2 and 4, we use a common MSE-optimal bandwidth selector for the RD treatment effect estimator. Optimal bandwidth is calculated with 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 are from incumbent national banks at the publication of the 1870, 1880, and 1890 censuses. The sample is restricted to banks in cities with fewer than 6,000 inhabitants in the respective previous census. Standard errors clustered at the city level are in brackets.

*** Significant at the 1% level.

region-specific asset shocks in western lands were important in explaining the peculiar regional patterns of bank failures in the 1890s. In our framework, stagnating towns would also be more likely to be found to the left of the cutoff. Thus, this alternative narrative raises the concern that our results on bank failures are driven by forces other than banking competition.

To test this alternative hypothesis and to rule out that our results are driven by factors other than banking competition, we restrict our sample in ways that exclude time periods or locations that are particularly aligned with this alternative. For instance, we reestimate equation (3) separately for the times before and after 1890. The 1890s were a time of particularly high bank failure rates, and the failures during this time best fit the above narrative (Calomiris and Gorton 1991). Thus, finding an effect of entry barriers on bank failures during the period before 1890 supports the idea that competition is driving the results. This is indeed exactly what we find, as shown in column 1 of table F.3.

In addition, we reestimate equation (3) using a sample that excludes towns whose population peaked before 1900. By excluding these locations, we specifically exclude the towns where there was a pronounced boom that proved unsustainable and turned into a bust such that population started to decline. Such towns would be the ones where the alternative narrative suggests that bank failures would be most likely. Even when we exclude these locations, we continue to find an effect of entry barriers on bank default, as shown in column 5 of table F.3. Indeed, the results are little changed from our main results; if towns experiencing population busts were particularly important for our results, we would have expected larger changes in the coefficients.

Additionally, since the alternative narrative focuses on agriculture and overestimating land fertility, we reestimate the model using a sample that excludes towns with relatively strong agricultural activity and a sample that excludes towns with relatively low manufacturing activity, here defined as being in the upper and lower quartiles of agriculture and manufacturing output per capita in 1870, respectively. This allows us to test whether our findings on bank default are driven by locations in which agricultural developments are the most important driver of economic outcomes. Again, we find that our results hold for each of these subsets of our main sample, as seen in columns 3 and 4 of table F.3. Altogether, these findings indicate that these broader trends in the American economy were not driving the results in our paper even if they were important for the larger narrative of the period.

C. *Additional Measures of Risk Taking*

Next, we provide additional corroborating evidence on risk-taking behavior by studying alternative measures of risk taking. One measure, from the asset side of the balance sheet, is an ex post indicator of asset quality based on banks' holdings of real estate that they obtained when loans went bad, referred to as "other real estate and mortgages owned" (OREO). Assuming that banks have similar collateral requirements across markets, higher OREO holdings relative to assets are indicative of a bank that had

TABLE 7
Risk Taking II: Bank-Level Evidence on Bank Balance Sheet Characteristics 10 Years after a Census Publication

	Dependent Variable						
	OREO (1)	Rediscounts (2)	Equity/Assets (3)	Equity/Loans (4)	Deposits/Assets (5)	Cash/Loans (6)	Reserves/Required Reserves (7)
Conventional	−.08**	.00	2.71	9.60*	−4.63	−.24	17.51
	[.04]	[.03]	[1.80]	[5.50]	[2.96]	[1.20]	[14.52]
Bias corrected	−.08**	−.01	3.42*	11.60**	−5.83**	−.53	21.72
	[.04]	[.03]	[1.80]	[5.50]	[2.96]	[1.20]	[14.52]
Robust	−.08*	−.01	3.42*	11.60*	−5.83*	−.53	21.72
	[.05]	[.03]	[2.05]	[6.10]	[3.42]	[1.38]	[16.18]
Bandwidth type	2 MSE	2 MSE	2 MSE	2 MSE	2 MSE	2 MSE	2 MSE
Kernel type	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov
Order local polynomial (p)	1	1	1	1	1	1	1
Order bias (q)	2	2	2	2	2	2	2
Mean dependent variable	.07	.06	28.95	58.52	52.61	10.62	215.96
No. of counties	992	1,029	1,029	1,029	1,029	1,029	1,029
No. of cities	1,589	1,673	1,673	1,673	1,673	1,673	1,673
No. of banks	2,200	2,358	2,358	2,358	2,358	2,358	2,358

Observations	2,476	3,077	3,077	3,077	3,077	3,077	3,072
Left of cutoff	2,122	2,641	2,641	2,641	2,641	2,641	2,636
Right of cutoff	354	436	436	436	436	436	436
Left main bandwidth (h)	1,808	2,310	2,164	2,271	1,837	2,305	2,416
Right main bandwidth (h)	3,145	3,375	3,752	3,440	3,616	3,985	4,138
Effective observations (left)	385	711	649	691	512	707	754
Effective observations (right)	269	335	344	335	343	348	357

NOTE.—This table presents results from estimating $y_{bt} = \alpha + \beta_1 \cdot \mathbf{1}_{ct}^{\text{pop}>6{,}000} + \beta_2 \cdot (\text{pop}_{ct} - 6{,}000) + \beta_3 \cdot \mathbf{1}_{ct}^{\text{pop}>6{,}000} \cdot (\text{pop}_{ct} - 6{,}000) + \varepsilon_{bt}$ (eq. [3]), where y_{bt} represents various bank-level outcomes. In col. 1, 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) 10 years after the respective census has been published. The item is collected only from 1890 onward, and thus we restrict the analysis to the effect of the 1880 and the 1890 censuses. In col. 2, the outcome variable rediscounts is a dummy variable that takes the value 1 if a bank reports extensive use of emergency funding. In cols. 3–8, the dependent variable is a ratio of balance sheet items of bank b' 10 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 is calculated with 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 are from incumbent national banks at the publication of the 1870, 1880, and 1890 censuses. The sample is restricted to banks in cities with a population of fewer than 6,000 inhabitants in the respective previous census. Standard errors clustered at the city level are in brackets.

* Significant at the 10% level.

** Significant at the 5% level.

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.[34]

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

The results are reported in table 7. With respect to OREO, column 1 shows that banks operating in a less competitive market had an 8 percentage point lower probability of holding collateral, compared to nontreated banks. 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 column 2 shows, we find some evidence that banks are less likely to make use of costly short-term funding, though the effect is not statistically significant.

As ex ante measures of risk taking, we also estimate equation (3) using various balance sheet ratios as dependent variables. The results are also reported in table 7. We find that incumbent national banks in markets with higher entry barriers had a 2–3 percentage point higher equity-to-asset ratio and a 10–12 percentage point higher ratio of equity to loans; see columns 3 and 4. Assuming equally risky loan portfolios across banks, larger equity buffers relative to assets or loans indicate that a bank was pursuing a more conservative investment strategy. Thus, the above findings are indicative of incumbent banks in markets with higher entry barriers having more conservative business models.

In addition, we find that incumbent banks with more market power after a census publication also had a lower deposits-to-assets ratio (col. 5) but no difference in the cash-to-loans ratio (col. 6), which together indicate a relatively more conservative funding structure and less liquidity mismatch. Further, banks in less contested markets also maintained a 20 percentage point higher reserve-to-required-reserves ratio (see col. 7), another indication that these banks were taking less risk, although the t-statistics are close to but never above 1.66. Overall, the results on balance sheet ratios provide

[34] 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 a higher cost, by banks that were experiencing difficulties (White 1983; Calomiris and Mason 1997; Calomiris and Carlson 2018).

suggestive evidence that institutions in areas with lower entry barriers behaved in a riskier manner than institutions in areas with higher barriers.

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 by making safer loans and being more cautious when making credit available (Keeley 1990). Banks with more market power made fewer loans and took fewer risks, in line with protecting their relatively more profitable charters. In turn, this reduced the probability of failure and reduced the likelihood of financial distress.

VII. Evidence on Real Economic Outcomes

After studying how competition affected credit availability and risk taking, in this section we provide evidence on how it affected real economic output. In particular, we study whether towns that had been subjected to higher entry barriers exhibited 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).

To study the real effects, we use data on farm outcomes and manufacturing outcomes provided by the census. In particular, for farming outcomes we study the effect of entry barriers on the value of farms, on the output produced by farms, and on the number of farms. For manufacturing outcomes we study the effect on capital invested, on the value of manufacturing output, and on the number of manufacturing establishments.

Note that these data are available only at the county level and are thus only a proxy for the activity in the respective towns. To avoid possible complications arising from changing county size and county borders, we measure real economic activity per capita outcomes at the county level.[35]

[35] Using county-level instead of city-level data (when the treatment is at the city level) can lead to measurement errors. First, our treatment is at the city level, but county-level outcomes may only partially reflect the economic fortunes of a single city in the county—especially if the county is large and contains multiple towns. However, this concern is alleviated by the fact that the median county in our sample has two towns, implying that each town may contribute to county-level economic activity in a substantial way. Also, note that our results are robust to dropping counties with larger cities that are not in our sample. Second, county borders changed every time new counties were founded or existing county boundaries were redefined, implying that measures of manufacturing and agriculture were not necessarily comparable across time. However, in additional robustness checks we found that our results are robust—albeit slightly less precise—to various types of county-border adjustments, including the method suggested by (Hornbeck 2010), or to dropping counties that change borders. Further, we also tested whether entry barriers themselves can explain county border changes (which would possibly induce bias) but found this not to be the case.

To test whether entry barriers affect real economic output, we start by providing visual evidence. Figure 5*C* shows that towns with higher entry barriers exhibit a lower per capita farming output 10 years after a census publication, thus indicating that the additional credit supply in more competitive markets translated into higher real economic output. Figure 5*D* also shows the same plots for manufacturing capital, but the evidence is less clear, and manufacturing capital is not necessarily lower to the right of the cutoff.[36]

To formally establish our results, we again estimate local linear regressions of the form of 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. Further, to account for the fact that some areas are generally more exposed to agricultural activity and others more to manufacturing activity, we also control for a county's original exposure to either agriculture or manufacturing outcomes in 1870.

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 10 years later. Considering farming outcomes, results in table 8 indicate that towns with higher entry barriers for national banks after a census publication are located in counties that exhibit a lower per capita farm output and farm value 10 years later. For instance, the per capita farm output is about 9–11 dollars lower when a town is subject to higher entry barriers, relative to a similar town with lower entry barriers—a considerable effect, given the average per capita value of 60 dollars. In addition, the number of farms per capita is also estimated to be relatively lower in areas with higher entry barriers. This indicates that a decrease in competition in the banking sector might also decrease competition in the nonfinancial sector, 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 using only outcomes after the 1880 and 1890 censuses (i.e., when excluding the 1870s). Here we find that towns that had been subjected to higher entry barriers for national banks had a lower per capita manufacturing capital 10 years later. This finding is largely in line with the evidence provided by Jaremski (2014), which 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.

[36] Figure 5*D* uses only data around the 1880 and the 1890 censuses. As discussed further below, the effect on manufacturing outcomes is relatively weak and not robust to including the 1870s.

TABLE 8

REAL EFFECTS I: FARMING OUTCOMES IN DOLLARS (PER CAPITA) 10 YEARS AFTER THE 1870, 1880, AND 1890 CENSUSES

	DEPENDENT VARIABLE					
	Farm Output		Farm Value		Number of Farms	
	(1)	(2)	(3)	(4)	(5)	(6)
Conventional	−8.72**	−9.94**	−39.14	−39.46	−10.64**	−11.02**
	[4.40]	[4.12]	[28.11]	[26.49]	[4.78]	[4.85]
Bias corrected	−9.99**	−11.53***	−48.83*	−46.51*	−12.46***	−12.30**
	[4.40]	[4.12]	[28.11]	[26.49]	[4.78]	[4.85]
Robust	−9.99**	−11.53***	−48.83	−46.51	−12.46**	−12.30**
	[4.99]	[4.47]	[31.76]	[30.06]	[5.25]	[5.31]
Bandwidth type	2 MSE	MSE common	2 MSE	MSE common	2 MSE	MSE common
Kernel type	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov
Order local polynomial (p)	1	1	1	1	1	1
Order bias (q)	2	2	2	2	2	2
Mean dependent variable	62.77	62.77	399.29	399.29	86.39	86.39
No. of counties	1,029	1,029	1,029	1,029	1,029	1,029
No. of cities	1,686	1,686	1,686	1,686	1,686	1,686
Observations	2,801	2,801	2,801	2,801	2,801	2,801
Left of cutoff	2,507	2,507	2,507	2,507	2,507	2,507
Right of cutoff	294	294	294	294	294	294
Left main bandwidth (h)	1,409	1,770	1,564	2,125	1,544	1,856
Right main bandwidth (h)	3,036	1,770	3,209	2,125	2,693	1,856
Effective observations (left)	274	374	314	488	312	401
Effective observations (right)	231	178	234	200	221	181

NOTE.—This table shows results from estimating local linear regressions of the form $y_{ct} = \alpha + \beta_1 \cdot \mathbf{1}_{ct}^{\text{pop}>6{,}000} + \beta_2 \cdot (\text{pop}_{ct} - 6{,}000) + \beta_3 \cdot \mathbf{1}_{ct}^{\text{pop}>6{,}000} \cdot (\text{pop}_{ct} - 6{,}000) + \varepsilon_{ct}$ (eq. [3]), where y_{ct} is the city c's county-level per capita farm value in dollars (cols. 1, 2), per capita farm output (cols. 3, 4), or per capita (in thousands) number of farms (cols. 5, 6) as of the census 10 years after census of year $t \in \{1870, 1880, 1890\}$. We vary the bandwidth selection methods across specifications. In particular, in cols. 1, 3, and 5, we use two different MSE-optimal bandwidths selectors (below and above the cutoff) for the RD treatment effect estimator. In cols. 2, 4, and 6, we use a common MSE-optimal bandwidth selector for the RD treatment effect estimator. Optimal bandwidth is calculated with 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 and the per capita farm value in 1870, using the covariate-adjustment approach developed by Calonico et al. (2019). County-level and city-level data are from the 1870, 1880, and 1890 censuses. The sample is restricted to cities with at least one national bank at the respective census publication and fewer than 6,000 inhabitants in the respective previous census. Standard errors clustered at the county level are in brackets.

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

TABLE 9

REAL EFFECTS II: MANUFACTURING OUTCOMES (PER CAPITA) 10 YEARS AFTER THE 1880 AND 1890 CENSUSES

	DEPENDENT VARIABLE					
	Manufacturing Value		Manufacturing Capital		Manufacturing Establishments	
	(1)	(2)	(3)	(4)	(5)	(6)
Conventional	4.82	−8.27	−23.07*	−26.20**	.61	.15
	[15.34]	[16.69]	[12.14]	[12.10]	[.44]	[.43]
Bias corrected	7.17	−6.49	−24.02**	−28.90**	.76*	.16
	[15.34]	[16.69]	[12.14]	[12.10]	[.44]	[.43]
Robust	7.17	−6.49	−24.02*	−28.90**	.76	.16
	[17.24]	[19.15]	[13.76]	[13.65]	[.49]	[.49]
Bandwidth type	2 MSE	MSE common	2 MSE	MSE common	2 MSE	MSE common
Kernel type	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov	Epanechnikov
Order local polynomial (p)	1	1	1	1	1	1
Order bias (q)	2	2	2	2	2	2
Mean dependent variable	123.28	123.28	99.45	99.45	6.60	6.60
No. of counties	992	992	992	992	992	992
No. of cities	1,598	1,598	1,598	1,598	1,598	1,598
Observations	2,260	2,260	2,260	2,260	2,261	2,261
Left of cutoff	2,023	2,023	2,023	2,023	2,024	2,024
Right of cutoff	237	237	237	237	237	237
Left main bandwidth (h)	1,324	2,312	1,761	2,116	1,137	2,173
Right main bandwidth (h)	3,903	2,312	3,015	2,116	3,764	2,173
Effective observations (left)	186	437	283	373	160	392
Effective observations (right)	198	166	185	159	196	161

NOTE.—This table shows results from estimating local linear regressions of the form $y_{ct} = \alpha + \beta_1 \cdot \mathbf{1}_{ct}^{\text{pop}>6{,}000} + \beta_2 \cdot (\text{pop}_{ct} - 6{,}000) + \beta_3 \cdot \mathbf{1}_{ct}^{\text{pop}>6{,}000} \cdot (\text{pop}_{ct} - 6{,}000) + \varepsilon_{ct}$ (eq. [3]), using county-level per capita manufacturing capital in dollars (cols. 1, 2), per capita manufacturing output (cols. 3, 4), or per capita (in thousands) number of manufacturing establishments (cols. 5, 6) as of the next census as the outcome variables and varying bandwidth selection methods. In particular, in cols. 1, 3, and 5, we use two different MSE-optimal bandwidths selectors (below and above the cutoff) for the RD treatment effect estimator. In cols. 2, 4, and 6, we use a common MSE-optimal bandwidth selector for the RD treatment effect estimator. Optimal bandwidth is calculated with 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 and the per capita manufacturing capital in 1870, using the covariate-adjustment approach developed by Calonico et al. (2019). County-level and city-level data are from the 1880 and 1890 census. The sample is restricted to cities with at least one national bank at the respective census publication and fewer than 6,000 inhabitants in the respective previous census. Standard errors clustered at the county level are in brackets.

* Significant at the 10% level.

** Significant at the 5% level.

Further, note that the results for manufacturing are not robust to including the 1870s, as is evidenced in table F.4. The weaker effect on manufacturing during the 1870s can be rationalized by the disruptions of the Civil War lasting well into the 1870s as well as the fact that during the nineteenth century, the majority of US gross domestic product still stemmed from agricultural output, with manufacturing becoming more important only toward the end of the nineteenth century. Further, manufacturing industries were more likely to be found in larger towns—especially in the earlier parts of the nineteenth century—and less so in the small towns contained in our sample.[37]

Altogether, our findings show that credit growth induced from lower entry barriers matters for real economic outcomes, in line with existing evidence on the importance of financial conditions for economic growth (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 VI, we observed that banking competition leads to an increase in credit growth, risk taking, and ultimately bank failures. Here we find that 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.

VIII. Conclusion

Understanding the role of bank market power is important for both academics and policy makers and thus is a key objective of the empirical banking literature. This paper makes valuable progress on identifying the causal effects of banking competition, by providing evidence from the National Banking Era. This period featured well-defined local banking markets and exogenous variation in entry barriers across markets. Thus, it is better suited to identify the effects of banking competition than existing empirical settings that either equate concentration with competition or study events, such as the lifting of branching restrictions, that typically coincide with other changes in the banking industry.

In this setup, we find that competition affects the incentives of financial institutions to extend credit, which in turn may affect economic growth. Further, we show that competition also leads to an increase in risk taking, which has implications for financial stability.

[37] Finally, yet another explanation is that farmers were the marginal and risky borrowers; in other words, farmers might have been more financially constrained than manufacturers. For example, using antebellum evidence, Mao and Wang (2021) find that entrants lent to a new set of borrowers in competing with existing bankers, as the latter had already formed lending relationships with merchants with good collateral.

However, it is important to keep in mind that one should be cautious when generalizing from historical experience. While the institutional and regulatory details of the National Banking Era allow us to identify the effect of banking competition more precisely than previous work, they also complicate the comparison to modern banking systems. We highlight two important differences with modern financial systems and how they may affect the interpretation of our results.[38]

First, the National Banking Era featured a banking system consisting of local unit banks. Contemporary banking systems involve nationwide, branch-based banks and a considerably wider range of financial institutions. Importantly, the dynamics of competition may play out differently in branch banking systems, as, for instance, the historical comparison between Canada and the United States suggests (Calomiris and Haber 2014).[39]

A second key difference is the role of government in credit markets. The National Banking Era featured a banking system with relatively few direct government interventions, and there were no government backstops such as a lender of last resort or deposit insurance. In contrast, regulatory requirements and government interventions are ubiquitous in contemporary credit markets and banking systems. Such interventions may, in turn, affect how the competitive environment shapes bank risk taking. Indeed, existing evidence suggests important interactions between regulatory interventions, banking competition, and bank risk taking (Agarwal et al. 2012; Calomiris and Haber 2014).

Nonetheless, we believe that our study has two important general lessons. First, the richness of our empirical setup allows us to test the empirical relevance of bank entry barriers. We identify the threat of entry as a key driver of bank behavior. Our evidence thus complements empirical evidence on the importance of entry deterrence for economic outcomes (Goolsbee and Syverson 2008) with evidence from a different industry at a different time. Establishing the empirical relevance of entry barriers in banking is especially valuable in the light of the fact that the empirical banking literature treats concentration often not as a market outcome but as a market primitive. Our findings, however, show that changes in

[38] There are, of course, other differences, such as those involving governance practices. First, bank shareholders faced double liability in case of losses: even after valuing the equity shares at zero, receivers of failed banks could still assess shareholders at an amount equal to the face value of those shares. Second, managers were asked to post security bonds to protect against malfeasance. Third, managers often owned a higher portion of their banks' equity than is typical today. These factors created incentives for bank managers to take a relatively more cautious approach (Koudijs, Salisbury, and Sran 2021).

[39] Canada has a highly concentrated banking system that is nonetheless often seen as highly competitive (see, e.g., Shaffer 1993). As discussed by Calomiris and Haber (2014), Canada's financial system has historically also been more stable than the US banking system. This is suggestive evidence that the effects of competition on financial stability can be very different if banks are not constrained in their ability to diversify geographically.

the ability of firms to enter a market can have substantial effects on banking and real outcomes even in the absence of any changes in measurable market concentration.

Second, our paper provides empirical support for an explicit trade-off between stability and credit availability, often considered by policy makers (Corbae and Levine 2018). In particular, we find evidence that more competitive environments may be both areas of greater credit availability that support economic growth and areas of greater risk taking associated with financial instability. This is important, as previous work documents that credit booms precede busts (Rancière, Tornell, and Westermann 2008; Reinhart and Rogoff 2009; Schularick and Taylor 2012; Rajan and Ramcharan 2015; Mian, Sufi, and Verner 2020) but offers no explicit empirical support for a causal relationship. Our results provide evidence that this relationship can indeed be causal, with credit expansions inducing both growth and financial instability.

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