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Finance and urbanization in early nineteenth-century New York

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ABSTRACT

Using information on the location of commercial banks in early nineteenth-century New York State we show that the opening of a bank had a significant positive effect on town population growth after the bank opened. Our results are robust to different samples of cities, control groups, time spans, as well as omitting New York City or small towns. To identify this relationship we exploit the legislative granting of corporate bank charters and argue that lobbying introduced enough exogenous variation in bank location choices that the location of banks in this period was different from what it would have been under a system of free entry based on entrepreneurial expectations of a towns' future population. Using a generalized difference-in-difference approach, we show that towns that petition for and received a bank between 1821 and 1835 saw their population increase more rapidly than those that petitioned for and did not receive a bank. Because urbanization and modernization were correlated in the nineteenth century, these results are broadly consistent with the finance-growth nexus identified elsewhere in the economics and economic history literatures.

1. Introduction

In his study of early modern capitalism, Braudel (1967, 396–97) wondered why some towns were more like steam engines than clocks. That is, why some towns were dynamic, noisy, turbulent, and ever-changing, while others ran in a "long, straight and unbroken line across time?" Steam-engine towns were integral to Braudel's (1967, 373) account of early capitalism because they were the places that "increase tension, accelerate the rhythm of exchange and ceaselessly stir up men's lives." This study addresses two issues: whether and which of New York's nineteenth-century towns experienced the dynamism Braudel attributed to steam-engine towns; and, the factors, particularly financial development, that enhanced the dynamism of New York's urban spaces.

Contemporary observers and historians alike believe the Erie Canal transformed towns and cities as surrounding lands were cleared, improved and populated, and regional markets expanded and integrated into the larger Atlantic system (Albion 1939; Sokoloff 1988; Bernstein 2005). Canals and, later, railroads (or, more generally, good transportation networks) were undoubtedly sources of urban dynamism in nineteenth-century New York. And there were myriad other sources of growth from indigenous and imported entrepreneurship, quality educational facilities, an engaged electorate, wide and deep rivers, and natural harbors (Albion 1939). We show there was another important and fundamental source of urban growth and development, namely financial modernization embodied in the nineteenth century in the local commercial bank. Our estimates imply that the presence of a bank, all else constant, significantly increased the annual average rate of growth of population for those places fortunate enough to get a bank.

Our identification strategy uses a generalized difference-in-differences approach similar to that used elsewhere in the literature (Greenstone et al., 2010; Kline 2012; Currie et al., 2015; Nguyen 2014) and makes use of a novel feature of nineteenth-century bank chartering that introduces a plausible degree of exogeneity in the location of banks. Prospective bankers could not open a bank in a potentially profitable location unless and until they received a special legislative grant of incorporation. In the 1820s and 1830s New York's legislature was not liberal in granting charters and, when they did incorporate new banks, they extended charters to political allies (Bodenhorn 2006, 2017; Murphy 2015). A critical step in the chartering process was petitioning the legislature for a charter, but few petitions were successful. Between 1821 and 1835 the New York assembly received 563 petitions from 111 cities and towns. Only 45 petitioners from 40 towns ultimately obtained a charter. The towns in which new banks opened between 1821 and 1835 were initially larger than towns without a new bank, but, as we show below, these two groups of towns follow parallel trends prior to 1820. After the new banks opened, towns with banks grew at significantly higher rates. Our difference-in-differences estimates, which control for additional factors likely to influence urban

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growth, imply that by 1870 towns that witnessed the opening of a new bank in the 1820s and 1830s were, four decades later, about twice the size of towns that failed to obtain a bank. We interpret this result to imply that finance was a powerful, independent source of urban growth in the early nineteenth century.

Our paper contributes to a sizeable empirical literature that investigates the consequences of bank deregulation in the United States and elsewhere (Kroszner and Strahan, 2014 review this literature). These studies identify causal effects from deregulatory reforms that were motivated by political rather than economic reasons. Kroszner and Strahan (1999) and Rice and Strahan (2010), for instance, contend that state-level bank deregulation, particularly the elimination of branching restrictions, was driven by the relative lobbying power of small and large banks, but not by contemporaneous economic activity, which makes it possible to trace their short- or long-term effects using traditional econometric techniques. Studies typically posit that deregulation creates an exogenous credit supply shock that impacts local economic growth, employment, mortgage originations, housing prices, small business lending, among other outcomes and the empirical results generally confirm the hypotheses (Kroszner and Strahan 2014; Favara and Imbs 2015; Nguyen 2014; Greenstone et al., 2015).

Our study follows this literature in positing that successful petitioning-lobbying campaigns that resulted in a town receiving a bank represented a marked deregulation, from complete prohibition to a grant allowing incorporated commercial banking in a specific location. We also follow the literature, as well, in arguing that successful petitioning-lobbying bank charter efforts were determined less by contemporaneous economic than contemporaneous political conditions. Charters were granted to political supporters and allies of the party then in control of the legislature. And, following the literature, the extreme deregulation implies that a charter generated a loan supply shock because New York's 1804 and 1818 restraining acts imposed substantial penalties on unincorporated banking so that there was little or no impersonal bank-supplied credit available in small and medium-sized towns without a bank. Lacking quality, high-frequency data on local economic outcomes, such as employment or asset prices, we consider how the opening of a bank influenced long-run growth in local population, which was surely influenced by changes in local economic opportunities.

In finding that banks were integral to urban growth, our paper fits into a long tradition in economic history that posits a link between finance and urbanization. Ashmead (1914), Taylor (1967), Kroos (1967), Crowther (1976), Weiman (1988), Crothers (1999), Jaremski and Rousseau (2013), and Atack et al. (2014) all point to the important role banks played in nineteenth-century urban development, mostly through the banks' encouragement of entrepreneurship or investments in transport and trade. With the exception of the last two studies, however, relatively little direct evidence has been brought to bear on the subject and these two studies generate equivocal results. We show that finance had profound effects on the pace of urban development in the nineteenth century. Moreover, banks made a difference not just for emerging metropolises; getting a bank was in fact a fortuitous, even celebrated, event for small and medium-sized towns. Finally, our paper also contributes to the finance-growth literature. Despite the many difficulties involved in identifying causal effects, the weight of the evidence supports the hypothesis that finance positively affects growth (Pascali 2016). Demirguc-Kunt and Levine (2001) and Levine (1997; 2005) argue that existing research suggests that countries with better functioning banks and financial markets experience higher rates of economic growth. In particular, the evidence implies that more efficient financial sectors mitigate external financing costs and constraints that retard growth. Easing financial constraints by increasing the local availability of loanable funds encourages growth, most likely by encouraging innovation and entrepreneurship (Benfratello et al., 2008; Kendall 2012). Fulford (2015) shows that banking's effects also extend beyond industrial entrepreneurship; proximity to a bank in the National Banking Era (1863–1913) increased land under cultivation and improved agricultural yields. While population growth and increasing urban density proxy for economic growth, we are interested in urbanization per se. Urbanization is an integral feature of modernization and deserves to be better understood in its own right (deVries 1984; Hohenberg and Lees 1985). Our results are, therefore, of more than historical interest. While historical in nature, the results provide insights into connections between urbanization and modernization and reflect on the development process more generally.

2. Background: a brief history of banks and politics

In the twenty-first century United States banks and bank offices are ubiquitous – busy intersections, strip malls, near the entrance of Walmart – and automated teller machines fill in where brick-and-mortar banks are uneconomic. In short, financial services are easily accessed. In the early nineteenth century banks were not on every street corner. Legislators and other regulators limited entry, in part, to preserve rents from monopoly banking for distribution to favored groups and, in part, to protect the public from the consequences of bank failures. Before 1836, just 5% of all New York towns had a bank, and only 13% of towns with more than 2,000 people did. Potential borrowers in towns without a bank were not cut off from bank services, of course, but the state's reluctance to grant charters meant that financial services were scarce even in the proximity of a bank. Modern studies show that the median distance between a bank and its borrowers is about 5 miles in 2010 (Greenstone et al., 2015; Nguyen 2014), and it is plausible that this distance was shorter 150 years ago when it was even more difficult for banks to gather information on borrowers. Not being close to a bank surely made it more costly for aspiring entrepreneurs to borrow (Chinitz 1961; Diamond 1984), so that towns with banks were more likely to attract businesses, employment opportunities and, ultimately, people. Thus, banks are likely to have had a causal effect on population growth in the long run.

2.1. Political chartering provides an identification strategy

Identification of the finance effect is complicated by the fact that bank location may have been endogenous to a town's subsequent population growth. That is, aspiring bankers or bank regulators may have been particularly adept at identifying towns that were likely to grow relatively quickly and opened banks in those towns. If true, any estimated effect of bank on growth captures, at least in part, reverse causality; that is, anticipated growth may have caused banks. To deal with this possibility we exploit an important characteristic of early American corporations, namely the legislative granting of corporate charters of all kinds, including bank charters. Legislative chartering did not randomize bank location, but it introduced enough plausibly exogenous variation into bank locations with respect to subsequent town population growth that bank locations were not completely endogenous to expectations of future growth. Because the choice of where to locate was subject to political concerns and could be, and sometimes was, subject to corruption, the location of banks was not what it would have been under a system of free entry based on expectations of future population or profitability (Bodenhorn 2006, 2017).

For the purposes of understanding the connection between chartering and entry, antebellum New York's banking system can be divided into three regimes: a period of legislative chartering that devolved into a system of what Wallis (2006) labels *venal corruption* from 1784 to 1820; a period of legislative chartering he labels *systematic corruption* from 1820 to 1837; and a period of free entry, or open access, under a general incorporation law that lasted from 1838 to the passage of the National Banking Act of 1863. It is the second period—1820 to 1837—that is under study here, and is understood mostly in the context of what came before it.

Under New York's legislative chartering regime, aspiring bankers

petitioned the legislature for a charter. The process involved hiring an attorney to draft a petition consistent with contemporary legal conventions, delivering the petition to the aspirant's legislative representative who would then introduce the petition and place it on the legislature's docket. (Petitioners virtually always petitioned the lower house, known as the Assembly, and sometimes concurrently petitioned the upper house, or Senate.) Petitioning and chartering were high-stakes politics, in part, because of the money lobbyists had to spread around and, in part, because restrictions on bank entry created a stream of economic rents for petitioners fortunate enough to receive a charter. Legislators used various mechanisms to capture a share of the expected rents. The first chartering regime (1784–1820) was characterized by venal corruption, or situations in which the pursuit of private economic interests (i.e., securing a charter) corrupts the political process (Wallis, 2006). The implication of venal corruption in the present context is that political affiliations and the willingness to share anticipated economic rents accruing to a local banking monopoly with politicians of the same party determined who secured a charter and where banks were located. Banks were not necessarily located in towns with the brightest prospects; they were located based on the preferences of politically influential businessmen.

Competing parties needed a mechanism by which the rents could be distributed and, in the process, secure the loyalty of the beneficiaries. In New York and elsewhere, Republican-dominated legislators chartered Republican-dominated banks; Federalists chartered Federalist-dominated banks (Lu and Wallis, 2017). It was in the second period of chartering, between about 1820 and 1837, that the system was transformed into one of *systematic corruption*, defined as a system in which politicians "deliberately create rents by limiting entry into valuable economic activities" (Wallis, 2006, 25), the purpose of which is to bind together a political coalition that dominates government. Where venal corruption is consistent with the traditional view that economics corrupts politics; systematic corruption is a system in which polities corrupt economies. The beginning of the second period is dated to 1820 because New York's revised 1821 constitution extended the franchise from about 30% of adult white males to nearly all adult white males and new anti-corruption political parties emerged to challenge Republican control (Kass, 1965). The second period is also dated to 1820 because it corresponds to the opening of the Erie Canal, which fundamentally transformed the economy of western New York (Bernstein, 2005; Sokoloff, 1988).

A third defining feature of the second regime was ascendance of Martin Van Buren's so-called Regency faction of the emergent Democratic Party. Benton (1856, 360) contends that 1825 marked the "last attempt to procure bank charters by bribery." Van Buren and his lieutenants channeled the rent seeking surrounding bank chartering to the direct benefit of the party. Individual politicians continued to reap the rewards from chartering, but once the Regency took control, rents were no longer allocated through outright vote selling. The party leadership determined which petitioners would receive charters based on whether a charter served the party's interests, not who was willing to pay the most for a charter (Wainwright, 1953). Once the leadership determined which petitioners would receive a charter, the leadership allocated rents in service of the party.

Van Buren understood that banks were a critical element of a growing economy, that monopoly banks had charter value, and – most importantly – that the newly expanded electorate was less tolerant of outright vote selling and unapologetic graft. The issue facing Van Buren was how the system could be harnessed to serve his and his party's interest. The result was as effective as it was simple. Only staunch party supporters who petitioned would receive charters, and party supporters received shares in newly chartered banks in proportion to their importance to the party. Again, there is virtually no evidence in the documentary or historical record to suggest that towns received banks because they had better growth prospects, whether due to geography, current industrial mix, or other factors. Among the petitioning towns, the evidence points to chartering based on political considerations, nearly exclusively (New York State Senate, 1837).

The gradual evolution of New York's banking law is consistent with the interest-group politics hypothesis advanced by Kroszner and Strahan (2014) to explain why a deregulatory movement emerged roughly 50 years after the New Deal reforms. More important for present purposes, however, is that the change from an open-bribery regime (1784–1820) to the partisan chartering regime (1821–1837) affords a reasonable identification strategy. It is virtually certain that petitions for bank charters came from towns that petitioners expected to grow at above average rates, which would create an endogeneity problem (assuming petitioners' expectations were, on average, correct) if the analysis included every city and town in New York. But we focus on the 103 cities and towns from which petitions originated between 1821 and 1835 because, among these cities and towns, the location of successful petitioners was driven more by political than economic considerations.

3. Data

Our analysis is conducted with an extensive, original data set that includes town-level populations between 1800 and 1870 reported in federal and state censuses. The federal government canvasses the population at decade intervals in years ending in zero. Beginning in 1825, New York State conducted statewide canvasses of its population at decade intervals in years ending in five; it also conducted a census in 1814. Population data on New York's cities and towns were taken from two sources. Population data from the federal censuses was taken from Haines' (2010–2015) files that provide town-level data for each decennial census between 1800 and 1870. We merged the separate census year files, amended the data to account for changes in town names over time, corrected a nontrivial number of coding errors, and filled in many missing observations from the census originals. To these we added town-level population data in intervening years from data reported in New York (1867). These two sources yielded usable information on incorporated cities and towns, which increased from 131 in 1790 to 573 in 1820 to 977 in 1870. In 1800, most of the incorporated towns were in the eastern half of the state, mostly along the Hudson and Mohawk River valleys. In 1870 they were spread across the state.

Town population data was supplemented with information on the chartering and location of banks between 1800 and 1860, on the number of petitions originating from each town between 1821 and 1835, and with other topographical, geographical, social and economic data. The topographical and geographical data include whether a town is proximate to a major waterway, its longitude and latitude, its elevation (in feet) above sea level, and whether it serves as a county seat (i.e., a regional governmental center, which typically included the county courthouse, the county jail, and other administrative offices). Social data include the number of churches, the number of schools, and the number of youth attending school in 1824. The economic data include information collected by the 1820 census on the number of people employed in agriculture, manufacturing, and commerce. Not all these variables are included in our regression analysis, but they reveal the similarities among petitioning towns, whether successful or not, and differences between petitioning and nonpetitioning towns.

Because the point of the study is to investigate whether banks influence town growth, the question naturally arises whether the town is the appropriate unit of observation. Modern studies tend to focus on counties, standard metropolitan statistical areas, or even states, even though some studies consider units as small as the census tract (Favara and Imbs 2015; Nguyen 2014). The city or town is the smallest unit of observation in early nineteenth-century censuses. Because the United States census bureau in the nineteenth century delineated urban places as those with 2,500 or more inhabitants, we adopt the traditional definition of a town. New York had 354 cities and towns that crossed the 2,500 threshold, including the 14 cities with ward-level data. The smallest town included in our set of petitioning towns had a population

of just 119 in 1800; the smallest ever-petitioning town in 1870 had just 888 inhabitants. New York City is the largest city in each census; between 1800 and 1860 New York City grew from 60,480 to 942,292, or by fifteen-fold. Between 1835 and 1860, the population of Brooklyn increased nearly ten-fold. In the empirical analysis, we estimate regressions with and without New York City and Brooklyn, as well as with and without small towns. Our results are not driven by either the large or the small towns.

3.1. Petitioning and the delineation of treated and untreated towns

Data on bank petitions was gathered from the *Journals* of the New York State Assembly between 1821 and 1835. Most petitions arrived in the early days of each legislative session, which ran from January to April, and we read the journals for each session for mentions of petitions received.[1] These data were augmented by a close reading of the Albany Evening Journal (1825–1837), which regularly reported on happenings in the Assembly. Because bank chartering was contentious, the *Evening Journal* noted most but not all bank petitions. Neither the Assembly Journals nor the *Evening Journal* provided the names of the individual petitioners, but the Assembly Journals typically identify the town and county in which the petitioners sought to establish a bank.[2] Relatively few petitions requested a charter for a bank in a specific county without specifying a town. Petitions that failed to mention a town are dropped, but this should have little influence on the results because none of these petitions resulted in a bank charter.

Information on the location of banks is taken from two sources. For the entire period 1790 to 1860 the location of new banks is taken from Weber's (2011) census of banks. For the period between 1821 and 1837 we rely on the chartering acts reported in the New York Assembly *Journals* and the annual session acts between 1820 and 1837, supplemented with dates provided by Root (1895). There are some discrepancies between Weber's list, Root's list, and ours in that the dates sometimes differ by a year or two. Differences in dating arise because we use the date of incorporation whereas Weber uses the first mention of a bank in a bank superintendent's report, banknote reporter, or newspaper, which would appear only with a lag after the bank was chartered and opened for business. Because population is observed at five-year intervals and we investigate long-term changes in population, determining the exact date of a bank's opening is less important than if we were using annual data and investigating short-term effects.

Fig. 1 provides a map of New York on which the red circles identify towns that petitioned for and received a bank and blue squares identify towns that petitioned for and did not receive a bank. It is apparent that petitions, both successful and unsuccessful, arrived from across the state, but the distribution of petitioners was not random. Between 1821 and 1835, 15 petitions originated in towns along the Hudson River Valley (along an approximately vertical line from New York City to Albany/Troy that passes through Newburgh, Kingston, and Catskill), nine of which (60%) were successful. Among these cities and towns New York, Newburgh, Catskill, Hudson, Albany, and Troy already had banks, however. Van Buren's strongest support was in the eastern half of the state (a line between Sackets Harbor and Binghamton approximately demarcates the eastern Democratic stronghold from the western Antimasonic and, later, Whig-leaning region), so the charters granted along the Hudson River Valley broke up the monopolies and undermined the charter values of banks previously chartered by Federalists and by Dewitt Clinton's Democratic faction, which battled Van Buren's Regency Democrats until the Regency prevailed circa 1825 (Kass, 1965).

A second group of 26 petitions originated in towns along the Erie Canal (along a line from Albany to Syracuse and then to Buffalo), only 10 of which (38.5%) were successful, mostly because a majority of voters and petitioners along western stretches of the canal were Antimasonic-leaning. The map also highlights that petitioners from relative Antimasonic strongholds, such as Chenango County (five petitions, zero charters), Oneida County (six petitions, one charter), Wyoming County (four petitions, zero charters), and Chautauqua County (five petitions, zero charters) were rarely successful in securing bank charters. Historians have in fact attributed the rise of the Antimasonic Party in western New York to middle-class merchants and manufacturers frustrated by the relative shortage of banks and transportation improvements in their region (Gunn 2001; Formisano 1993; Kutolowski 1984).

The statistics on petitioning in Table 1 are consistent with what is known about petitioning between 1821 and 1835. The second column reports the number of bank petitions read into the assembly journals; the third column reports the number of bills reported out of committee. It is notable that in many years the number of bills reported exceeded the number of petitions received and read into the record. In 1825, for example, 132 bank chartering bills were debated on the floor even though the Assembly Journal of that year recorded just 35 bank charter petitions being read into the record. The difference between 132 bills and 35 petitions is accounted for by the speaker invoking a privilege allowed by the assembly's rules under which an individual member could request leave to bring in a bill. Leave was granted by the assembly speaker, who then appointed a committee to prepare and bring in a bill for consideration. The member moving the bill and its seconder were appointed to the committee (which might be a committee of the whole), which offered an alternative to petitioning for aspiring bankers (Clark 1816, 103–4). To have a bill introduced into the assembly, non-petitioning aspirants needed to convince at least two members – a mover and a seconder – that they were capable bankers and that the community could support a bank. In tracing the bills from introduction to passage or rejection, we could not identify any cases in which a bill introduced by leave of the speaker passed through the three required readings.

Three features of the statistics reported in Table 1 support an identification strategy based on the petitioning/chartering procedure. First, a petition-driven process identifies the towns whose inhabitants believed that a bank would be profitable. Second, despite the ability of individual members to introduce bills without the proposed incorporators forwarding a petition, the failure of such bills means that chartering was a petition-driven process. It is not clear whether the decline in bills introduced by leave of the speaker after 1825 resulted from a change in the rules or whether assemblymen and aspiring bankers learned that such bills invariably failed. And, third, despite a marked increase in the number of petitions between 1821 (one petition) and 1832 (91 petitions), few petitioners were successful. Thus not every town whose residents considered it capable of supporting a bank got one. The fifth column reports the number of bills passed and the last column reports the relatively small number of charters finally granted after being supported by an assembly and senate committee, a majority of each house, and approved by the governor. With the exception of 1828 and 1830, the success rate, defined as the ratio of charters to bills passed, was low.

Table 2 reports statistics on the characteristics of towns, circa 1820, that received a bank (excluding New York City and Brooklyn), towns whose residents petitioned and did not receive a bank, and towns that did not petition. The statistics highlight two important features that underlay our identification strategy. Panel A reveals marked differences between petitioning towns that received a bank (column 1) and

[1] When we compiled the data, we were unable to obtain copies of the assembly's journals for 1836 and 1837. They have subsequently become available, but we have not gathered these data.

[2] Although we could not locate lists of petitioners (due to a fire at the state capitol in the early twentieth century), Bodenhorn (2017) analyzes shareholder lists of newly chartered banks, many of whom were petitioners. An analysis of these lists reveals a strong partisan bias in share ownership. State assemblymen, state senators, judges, and local politicians affiliated with the Bucktail faction of the Democratic Party held majorities of these banks' shares.

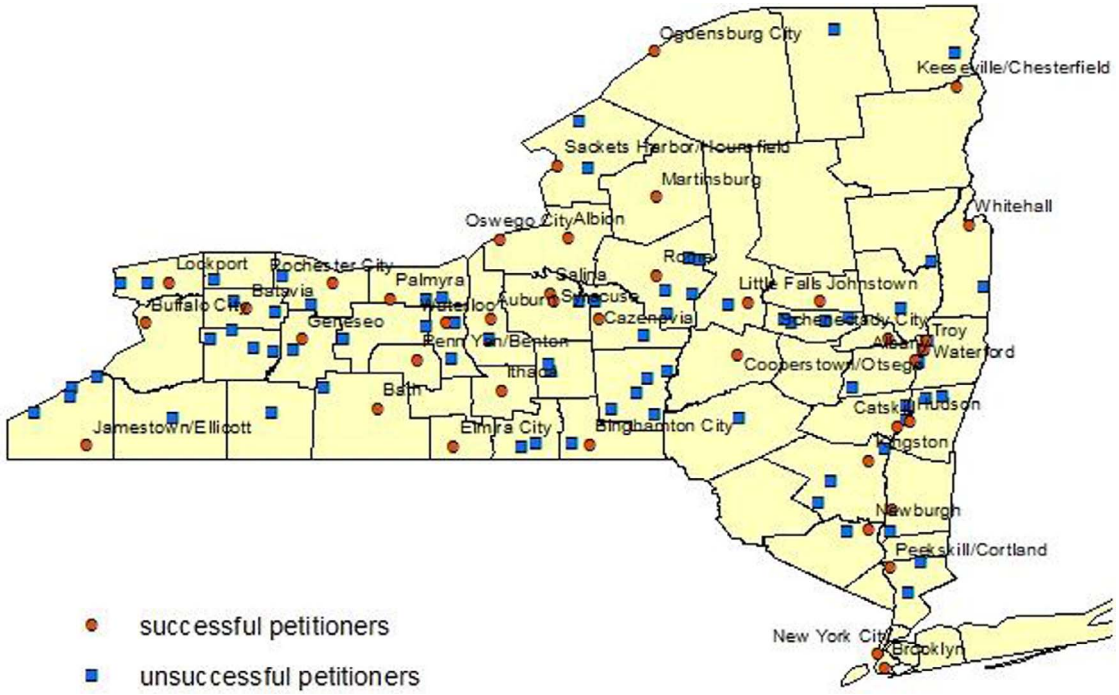

Fig. 1. Towns from which at least one bank petition originated, 1821–1835.

Table 1
Petitions, bills reported and passed, and charters in New York assembly, 1821–1837.
Sources: New York State Department of State (1867); Albany Evening Journal (1825–1837).

Year	Petitions	Bills reported	Bills introduced by speakers	Bills passed	Charters
1821	1	2	1	0	0
1822	5	6	1	1	0
1823	13	20	7	1	0
1824	12	31	19	8	1
1825	35	132	97	17	1
1826	17	5	0	0	0
1827	11	12	1	6	0
1828	44	55	11	3	2
1829	62	52	0	20	11
1830	23	22	0	12	9
1831	54	36	0	26	9
1832	91	49	0	19	7
1833	83	38	0	22	8
1834	92	31	0	21	8
1835	20	4	0	2	0

petitioning towns that did not receive a bank (column 2). Successful petitioning towns, on average, had about 850 more inhabitants than unsuccessful petitioning towns (p-values of tests for differences are reported in column 4). According to the employment data in the 1820 census, unsuccessful petitioning towns also had a smaller fraction of the workforce employed in manufacturing and commercial occupations; they had larger fractions employed in agriculture. Despite significant differences in employment shares, information reported in Spafford (1824), which provides detailed accounts of the number of different types of manufactories in most of New York's towns, there were not pronounced differences in the number of distilleries per 1,000 inhabitants. Moreover, there were no significant differences in the per capita number of grist (flour) mills, or fulling mills. Untabulated comparisons of pearl-ash, potash manufacturers, iron works, tanneries, and cotton and wool textile mills per 1,000 inhabitants also reveal no significant differences. The discrepancy between the significance of employment shares and the non-significance of manufactories points to differences in scale rather than industrial mix; that is, larger towns may have supported larger, rather than more, factories. Successful petitioners were more likely to be on a canal, but not more likely to be near rivers or great lakes. One notable difference, and one that points to a difference in politics and successful and unsuccessful petitioning is that unsuccessful towns had significantly fewer attorneys per 1,000 inhabitants. Moreover, successful towns were more likely to be county seats than unsuccessful ones.

Just as there were differences between towns that petitioned for and received a bank and towns that petitioned for and never received a bank, Column 5 of Table 2 also reveals differences between petitioning towns that received a bank and towns that never petitioned a bank. Successful petitioning towns were initially larger, had a larger fraction employed in manufacturing and commerce, and a smaller fraction employed in agriculture. They also had more distilleries, grist mills, and fulling mills and were more likely to be located on a canal, a major river, or a great lake. The fraction of successful towns that were county seats was also larger than that of non-petitioning towns.

Although petitioning for a bank charter did not generate a genuine random assignment of banks, it introduced enough politically-driven variation into the process that not every town whose residents considered it capable of supporting a bank received one. Our approach is analogous to the Kroszner and Strahan (2014) approach, which uses politically-driven differences in the dates at which states deregulate banks to estimate the economic consequences of deregulation. In the case at hand, the political clout of petitioners determined whether and when a prohibition on commercial banking – a significant deregulation – in a specific town was lifted. The analysis then estimates the consequences of the deregulation on long-run urbanization.

3.2. Banks and the transportation network: Canals, lakes and rivers

Proximity to good transportation facilities is known to influence urban growth (Fujita and Mori 1996; Beeson et al., 2001; Bosker et al. 2008; Da Mata et al., 2007; Redding and Turner, 2015), and a recent study proposes that railroads and finance were mutually reinforcing (Atack et al., 2014). In the period under consideration here, the Erie Canal network and other water transportation, including the

Table 2
Comparison of petitioning and non-petitioning towns.
Sources: authors' calculations from Haines (1790–2002), Spafford (1824). New York City and Brooklyn excluded.

Variable	(1) petition = 1 bank = 1	(2) petition = 1 bank = 0	(3) petition = 0 bank = 0	(4) p-value (1) vs (2)	(5) p-value (1) vs (3)
Panel A: population and economic structure					
Population 1820	3505.69	2631.77	1992.28		
	(430.92)	(167.17)	(56.15)	0.026	0.000
Population 1825	3879.78	2661.71	1953.52		
	(486.07)	(154.3)	(47.85)	0.004	0.000
Manufacture employ 1820	0.31	0.18	0.14		
	(0.04)	(0.01)	(0.005)	0.000	0.000
Commercial employ 1820	0.07	0.02	0.01		
	(0.01)	(0.005)	(0.001)	0.000	0.000
Agriculture employ 1820	0.62	0.79	0.84		
	(0.05)	(0.02)	(0.006)	0.000	0.000
Distilleries per 1000 pop	2.77	2.02	1.45		
	(0.66)	(0.24)	(0.09)	0.2	0.001
Grist mills per 1000 pop	4.64	4.14	3.16		
	(0.74)	(0.42)	(0.12)	0.52	0.004
Fulling mills per 1000 pop	2.68	2	1.48		
	(0.95)	(0.22)	(0.08)	0.37	0.002
Asheries per 1000 pop	1.45	2.39	1.86		
	(0.3)	(0.36)	(0.16)	0.086	0.53
Attorneys per 1000 pop	15.82	4.56	0.72		
	(2.78)	(0.77)	(0.06)	0.000	0.000
Panel B: geography					
County seat	0.71	0.22	0.02		
	(0.07)	(0.05)	(0.005)	0.000	0.000
Canal	0.6	0.34	0.15		
	(0.08)	(0.06)	(0.01)	0.007	0.000
Major river	0.24	0.13	0.07		
	(0.07)	(0.04)	(0.008)	0.173	0.000
Any river	0.55	0.51	0.3		
	(0.08)	(0.06)	(0.01)	0.711	0.000
Great Lake	0.16	0.07	0.05		
	(0.06)	(0.03)	(0.007)	0.175	0.003

Notes: the number of observations is different for each test because not all variables were reported for all cities. There are approximately 34 observations in column 1; 65 in col. 2; and 520 in col. 3.

introduction of steamboats on New York's principal rivers, were important in integrating formerly peripheral regions into the Atlantic economy (Taylor, 1966). Railroads grew in importance in the era, but water access represented an important initial condition to population growth. Most of the regressions include controls for the presence of different types of waterways to take this effect into account.[3]

[3] In addition to canals and waterways, we used railroad maps and other sources to identify New York towns with rail connections in 1860 to estimate their effects on growth. Because the rail network was not large and remained small compared to the traffic on the Erie Canal, and because our analysis ends in 1870s, we find that the railroad had a small impact on urban growth in New York. Wright (2005) discusses the impact of the Erie Canal on the rise of New York City. Cronon (1991) analyzes the importance of the Illinois and Michigan Canal on Chicago's rise to dominance among Midwest cities. In a classic controversial study, Fogel (1964) constructs a counterfactual analysis to estimate the role that canals would have had in the U.S economy in the absence of railroads.

A majority of towns in New York, as elsewhere in the eastern half of the United States, were located on some type of waterway – ocean, river, lake, or stream. A New York gazetteer (Spafford, 1824) provided a description of the state's cities and towns, including the type of waterway, if any, on which the community was located, and his descriptions were used to code the largest waterway type for each town. Spafford (1824, 417), for example, described Plattsburgh as "well watered, by the Saranac river, which runs eastward to L[ake] Champlain, and by salmon river, or creek, and several smaller streams, which supply abundance of hydraulic works [i.e., water-powered manufactories]." Plattsburgh is considered to be and was coded as being on a Large Lake, which included the Great Lakes and Lake Champlain. Other towns were coded similarly. Thus Buffalo, Oswego and Rochester are on a Great Lake, whereas Ogdensburgh, at the confluence of the Oswegatchie and St. Lawrence rivers is coded as being on a major river, which includes the Hudson, St. Lawrence, and Delaware rivers. Any town on both a river or lake and canal is considered to be on both and is coded for both.

Panel B of Table 2 reports the fraction of towns by type located on a waterway. The second row shows that, 55% of towns that petitioned *and* received a bank were located on a canal, compared to 34% of towns that petitioned and did not receive a bank and 15% of non-petitioning towns. The difference between petitioning and non-petitioning towns and that between successful and unsuccessful petitioners is significant at the 1% level. In addition, location on a river or a great lake (Ontario, Erie, or Champlain), increased the likelihood of petitioning, but conditional on petitioning, being on a river did not increase the likelihood of receiving a charter. Water access appears to have been one criterion in the bank chartering process, but it was not determinative. In any case we acknowledge that it is important to account for access to waterways in our regressions, and to do so we generate several alternative measures that we add as controls.

In addition to waterways, the first line of Panel B shows that 71% of successful petitioners were located at a county seat; only 22% of unsuccessful petitioners were, and only 2% of non-petitioning towns were county seats. Petitioners asking to erect a bank in a county seat were more successful for two principal reasons. First, county seats were judicial, and administrative centers, which made them political centers, as well. Courthouses, jails and public offices were located in county seats. Second, when a new county was formed in the 1810s and 1820s, the legislation authorizing the new county established the county seat. Because inhabitants needed to travel to the county seat to conduct legal business and pay taxes, legislators tended to choose a centrally-located town, rather than the largest town. The geographic centrality of county seats may also explain the legislators' preference toward petitioners from county seats. By locating a bank in a county seat, the legislators created relatively equal (geographic) access to financial services. All except two of the county seats were established before the town received its first chartered bank, so that county seats were not endogenous to bank location. We exploit these features of county seats in our instrumental variable (IV) approach below.

4. Empirical approach

The structural relationship of interest is the effect of a bank opening on local population growth over the long run:

$$\ln(pop)_{ijt} = \alpha + \eta_i + \phi_j + \varphi_t + \delta Bank_{ijt} + \beta X_i + \varepsilon_{ijt} \quad (1)$$

where pop_{ijt} is the population of town i in county j in year t; α is constant term common for all towns, η_i is a town fixed effect, ϕ_j is a county fixed effect, φ_t is a year fixed effect to model possible nonlinearities in the evolution of population not captured by linear time trends, X_i is a vector of town characteristics that include proximity to water, the proportion of population engaged in agriculture in 1820, whether the town was crossed by the Iroquois Trail, and its (log) elevation and ε_{ijt} is

a mean-zero error term.[4] The Iroquois Trail dummy is included because Bleakley and Lin (2012) show that North American cities tended to locate at native portage points and the Iroquois Trail was an ancient, Native-American trade route used by colonial military forces in troop deployments that crossed several rivers. A town's log elevation is included following Nunn and Puga (2012), who discuss the importance of terrain irregularities on local economic development. $Bank_{ijt}$ is an indicator variable equal to one if a town received a bank between 1821 and 1835, and zero otherwise. The OLS estimate of δ is unbiased if $Bank_{ijt}$ is orthogonal to ε_{ijt}, that is, if the opening of a bank is unrelated to local factors that would also affect the level of population in prior and subsequent years. In general this assumption is unlikely to hold because population shocks that result in an increase in the size of a town will probably influence the profitability of banking and the attractiveness of a community to aspiring bankers.

A solution to the probable endogeneity of bank location is to employ a generalized event-study, differences-in-differences (DD) framework to compare population changes in petitioning towns that received a bank (treated towns) and petitioning towns that did not receive a bank (control towns) in the period before, during and after the 1820–1835 window, allowing for time trends. Under the DD approach the identification assumption is one of parallel trends. Absent the chartering and opening of a bank, population would have evolved along similar trajectories. To facilitate an examination of pre-bank and post-bank trends in the data, we estimate Eq. (2), using a census year-by-year DD and present our results in tables and event study plots.

$$\ln(pop)_{ijt} = \alpha + \eta_i + \phi_j + \varphi_t + \sum_{t=1825}^{1870} \theta_t Bank{*}Post_t{*}\varphi_t + \varepsilon_{it} \quad (2)$$

The coefficients of principal interest are the θ_t's, which measure the differences, conditional on the controls, in the natural logarithm of population between treated and untreated towns in census years before and after 1820. The other variables and parameters are defined in the same way as in Eq. (1). Following Bertrand et al. (2004), standard errors are clustered at the town level to avoid understating the standard errors and drawing unwarranted inferences.

An alternative solution to the endogeneity of bank openings to future population is to generate plausibly exogenous variation in the incidence of bank openings. Based on our contention that the success of a petitioning effort was determined in large part by politics and geography, we use whether the petitioning town was a county seat as an instrument for bank opening. As discussed earlier, county seats served as governmental and political centers that legislators established near the geographic center of the county, when possible. If legislators were concerned in creating relatively equal access to bank customers (not bank owners), county seats may have been more likely to receive a bank charter.[5] We therefore estimate two first-stage regressions of the form:

$$Bank_{ijt} = \chi + \kappa_i + \gamma_j + \tau_t CountySeat + \upsilon_{it} \quad (3)$$

$$Bankpost_{ijt} = \mu + \pi_i + \xi_j + \rho_t CountySeatpost + e_{it} \quad (4)$$

County seat is an indicator variable that equals one if the town from which one or more petitions originated was the political center and County Seat Post is an indicator variable that equals one if County Seat equals one and the year corresponds to any period after the bank was established. The first-stage prediction of the Bank and Bank Post variables are then used to estimate the second-stage IV regression using the traditional DD equation given in Eq. (5). Instead of the generalized form, Eq. (4) follows the familiar DD approach, which estimates a common effect for the opening of a bank on population after 1835.

$$\ln(pop)_{ijt} = \alpha + \eta_i + \phi_j + \varphi_t + \beta \widehat{Bank}_{ijt} + \theta \widehat{BankPost}_{ijt}{*}\varphi_t + \varepsilon_{it} \quad (5)$$

where $\widehat{Bank}_{ijt}$ and $\widehat{BankPost}_{ijt}$ are the predicted values of the first-stage regressions (3) and (4).

As Nguyen (2014) notes, the internal validity of the DD approach turns on the assumption of parallel trends that are evident in the data. External validity of the IV estimate turns on whether the local average treatment effect (LATE) is identified from petition-induced bank openings and whether the resulting estimate is informative for understanding the effect of bank openings more generally. One approach is offered by Angrist and Pischke (2009) who divide a population into four groups: compliers, always-takers, never-takers, and defiers. *Compliers*, the subpopulation of interest, are those whose treatment status is affected by the instrument in the right direction. In this case, compliers are those towns that are more likely to be treated with a bank because they are a county seat, conditional on petitioning. A *defier* is a town whose treatment status is affected by the instrument in the wrong direction, which would imply that its inhabitants petition the legislature to enact a law prohibiting the opening of bank in their town. We found no such petition, so we assume there are no defiers in the population. The set of *always-takers* is the set of towns in which a bank is petitioned independently of being a county seat. The only obvious example of an always-taker in our sample is New York City (including Brooklyn) because it was such important commercial centers that it would have had multiple banks regardless of its status as the county seat. Our interpretation of LATE is more straightforward because we exclude New York City and Brooklyn from most regressions since it was an extreme population and bank outlier.[6] Finally, a *never taker* is a town that never petitions for a bank independent of its status as a county seat. Because we condition on petitioning there are no never-takers in the sample.

Table 3 shows that compliers are representative of the sample.[7] Column 1 reports the percentage of compliers that lie above the median value for the sample of petitioning towns. Less than 20% of compliers are above the sample median for any of the economic and social characteristics, including aggregate population, manufacturing, commercial or agricultural employment, schools, or churches. Column (2) compares mean values for compliers to the overall sample. The ratio was approximately one-third higher for population, manufacturing employment, and churches per capita. It was about 15% lower for agricultural employment share and schools per youth in 1824. Because the characteristics of compliers do not radically differ from the sample, the estimated treatment effects should capture the average impact of a bank on a town's long-term population growth. Still, the IV results will be unbiased only to the extent that the exclusion restriction and the monotonicity assumption are satisfied.

5. Empirical results

5.1. Generalized difference-in-difference estimates

This section presents evidence for the relationship between the opening of a bank and subsequent town growth. Figs. 2a and b provide templates for the event study results in that they plot the θ_t estimated

[4] We also included the percentage of population in manufacturing or commerce but these variables were dropped from the regressions due to multicollinearity.

[5] Elmira City had its first chartered bank in 1833 but the county seat was established in 1836. Johnstown has its first chartered bank in 1831 but its county seat was established in 1838. Whitehall does not have accurate information on when its county seat was established. We drop these two towns from our instrumental variable regressions to avoid endogeneity issues.

[6] It is likely that Albany, too, would be an always-taker because it was a state capitol into addition to a county seat and a large deep-water trading port. But Albany was designated a county seat in 1683 and got its first bank in 1801, or prior to the dates that we consider.

[7] Angrist and Pischke (2009) provide an approach to back out the characteristics of implied compliers by differentiating between compliers and always-takers. Because we assume that New York City and Brooklyn were the only always-taker and they are excluded from the analysis, we assume compliers to be the set of towns that were county seats and obtained a bank. The sample consists of all petitioning towns, which includes the compliers.

Table 3
Complier characteristics.
Sources: authors' calculation from data described in text. New York City and Brooklyn excluded.

Variable	(1) Proportion of compliers above sample median	(2) Ratio: complier to sample average
Population 1820	16.30%	1.33
Manufacturing employ 1820	14.8	1.39
Commerce employ 1820	17.2	1.98
Agriculture employ 1820	11.5	0.83
Schools per youth	21.7	0.85
Churches per capita	8.1	1.3
ln elevation	12.6	1
Canal	15.1	
Major river	5.6	
Any river	25.5	
Great lake	25.5	

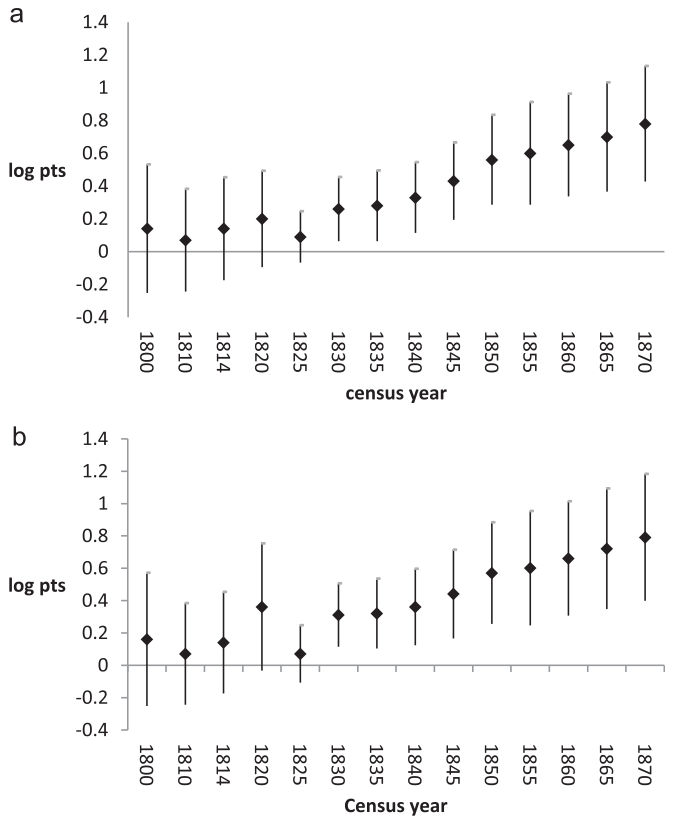

Fig. 2. Opening of bank and town population, 1800–1870. (a) No additional controls and (b) Additional controls.*Source and notes:* Authors' calculations. The figure plots theta estimates from Eq. (2) in text, when Post is turned on over the full sample period for towns in which a bank opens between 1820 and 1835, excluding New York City and Brooklyn, with town, county, and year fixed effects but no additional correlates. Diamonds represent point estimates and bars are 95% confidence intervals.

from Eq. (2), where the dependent variable is the natural logarithm of town population in year t. The bars show the 95% confidence intervals for an unbalanced panel at census dates between 1800 and 1870.[8] A $\theta_t > 0$ indicates a greater population in treated towns relative to untreated control towns before, during, and after the 1820 and 1835 bank chartering period under study.

Fig. 2a, which includes only town, county, and year fixed effects as additional regressors, and Fig. 2b, which includes economic and geographic controls in addition to the fixed effects, show that towns that experienced a bank opening between 1821 and 1835 were initially no larger than towns that did not. Up to 1830, banked and unbanked towns had approximately the same populations and. However, the crucial assumption for the difference-in-difference model is that both treated and untreated towns had a parallel trend in the evolution of population before 1821. The two figures imply that banked and unbanked towns followed parallel paths through 1825. The long-run effect of finance on city growth, however, becomes apparent after 1835; within a decade of getting one, towns with newly opened banks are significantly larger than petitioning towns without. Moreover, the estimates imply a large long-run effect. In 1870 the average town that gets a bank between 1821 and 1835 is 0.7 log points larger than an equivalent town that does not get a bank during that period. That is, over the subsequent four decades treated towns grow two times as large as untreated towns.

Table 4 reports DD coefficient estimates for a panel of petitioning towns formed prior to 1825 when we control for proximity to a canal and a complete set of major waterways, which includes canals, major rivers, large lakes and the Atlantic Ocean. Column (1) reports our theta estimates from Eq. (2), like those displayed in Fig. 2, for the post-1820 sample of towns. The coefficients on the economic and geographic controls in this and in subsequent regressions are consistent with prior expectations. Coefficients on the logarithm of initial population are positive, which implies that larger towns in 1820 were larger in subsequent years. Towns in which a larger proportion of the population was employed in agriculture in 1820 were smaller. Towns at higher elevations, especially those in the Adirondack Mountains, were smaller than towns at lower elevations. And towns along the so-called Iroquois Trail (historic trading routes among Native Americans) were smaller than petitioning towns not along the trail. The coefficients of principal interest – the thetas – imply that the average town to receive a bank has grown nearly 30% ($\exp(\theta_{1840}) = 1.297$) larger than an unbanked town by the end of the 1830 s. The average banked town is twice as large in 1870 as a comparable unbanked town.

In Column (2), we add an indicator variable to control for proximity to a canal and interact the canal and bank indicators to investigate whether a post-1820 bank and a post-1820 canal had synergistic effects. This specification is particularly relevant because the Erie Canal was opened in stages beginning in 1820 and was open across its entire 363-mile length in 1825. The coefficient on the canal variable (0.41) implies that a town proximate to a canal port or lock was, on average, about 50% larger than a comparable town not on a canal. The theta coefficients are unchanged from the specification in Column (1). The coefficient on the interaction term implies that the combination of a bank and a canal had a smaller effect than the sum of the individual effects. Still, the interaction effect is such that the total effect of a canal on population is positive. Towns with a canal and a bank were about 18% larger than those without a canal (i.e., exp(0.41–0.24) = 1.185). The inclusion of the canal and interaction terms diminish but do not eliminate the effect of banks on town populations, which is consistent with our contention that banks influenced city populations independent of transportation advances.

Column 3 of Table 4 provides coefficient estimates using a broader definition of water access that adds natural waterways, including rivers and the Great Lakes, to canals. The results are similar to those reported in Column 2, except the coefficient on the water variable is more than twice the coefficient on the canal-alone variable, and the coefficient on the bank-canal interaction is also larger. The net effect of a bank on population remains positive for 1850 and after, however. Towns with any waterway and a bank grew 71% more than those without a waterway (i.e., exp(0.92–0.38) = 1.71). Our findings accord with Sokoloff's (1988) contention that proximity to a canal connected communities to the wider Atlantic economy, increased market

[8] When we use a balanced panel of towns organized prior to 1820, the theta coefficients for each census year are slightly larger than those reported here and the p-values are <0.1 for 1840, and <0.05 for 1845, and <0.01 after. Because about 400 observations are lost using the balanced panel, we report the results from the larger unbalanced panel.

Table 4
Theta coefficients from generalized difference-in-differences with economic and geographic controls.
Sources: authors' calculations.

	No waterway controls	Canals	Canals + Rivers + big lakes + Atlantic
θ_{1825}	0.003	0.003	0.003
	(0.081)	(0.081)	(0.081)
θ_{1830}	0.236***	0.236***	0.236**
	(0.089)	(0.089)	(0.089)
θ_{1835}	0.23**	0.23**	0.23**
	(0.104)	(0.104)	(0.104)
θ_{1840}	0.264**	0.264***	0.264***
	(0.113)	(0.113)	(0.113)
θ_{1845}	0.346**	0.346***	0.346***
	(0.133)	(0.133)	(0.133)
θ_{1850}	0.472***	0.472***	0.472***
	(0.153)	(0.153)	(0.153)
θ_{1855}	0.508***	0.508***	0.508***
	(0.174)	(0.174)	(0.174)
θ_{1860}	0.564***	0.564***	0.564***
	(0.175)	(0.175)	(0.175)
θ_{1865}	0.624***	0.624***	0.624***
	(0.185)	(0.185)	(0.185)
θ_{1870}	0.694***	0.694***	0.694***
	(0.196)	(0.196)	(0.196)
Waterway	–	0.416***	0.92***
		(0.041)	(0.1)
Waterway*Bank	–	-0.245***	-0.384***
		(0.079)	(0.095)
Log population in 1820	0.526***	0.472***	0.421***
	(0.017)	(0.008)	(0.007)
Perc. labor in agriculture in 1820	-1.118***	-1.566***	-1.86***
	(0.159)	(0.029)	(0.018)
Iroquois trail	0.082	-0.193***	-0.542***
	(0.055)	(0.034)	(0.01)
Log elevation	-0.308***	-0.183***	-0.033***
	(0.027)	(0.013)	(0.006)
Constant	6.37***	6.263***	5.853***
	(0.158)	(0.097)	(0.124)
R^2	0.755	0.755	0.755
Observations	1,090	1,090	1,090

Notes: All the regressions include town, county and year fixed effects. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust clustered errors at the town level in parentheses. New York City and Brooklyn excluded.

participation, and inventive activity. Banks had similar effects because access to external finance lowered the costs of engaging in commercial and manufacturing activities, including commercialized agriculture (Fulford, 2015).

The results reported in Table 4 and those underlying Fig. 2 highlight an important feature of finance and growth, namely that finance manifests itself over the long run. In nineteenth-century New York towns that had yet to get a bank in 1820 and whose inhabitants petitioned for one had similar populations and looked similar in terms of industrial mix, employment, and social capital. For towns whose petitioners secured a charter, changes in population appeared quickly afterwards and towns with banks and banks without followed different population trajectories for the next 40 years. Our results are consistent with the finance-growth literature, which typically finds that initial differences in financial development generate meaningful differences in economic growth and productivity growth over the next several decades (Beck and Levine, 2004).

5.2. Instrumental variables estimates

As an alternative to the DD analysis this section provides instrumental variables (IV) estimates based on the first- and second-stage system defined by Eqs. (3) and (4). As in the traditional DD approach, POST is a dummy variable equal to 1 if year t occurs after a town receives a bank. The instrument for Bank is the variable County Seat, which is a dummy variable equal to 1 if a town was a county seat established prior to 1820 and the instrument for Bank*Post is the interaction of County Seat and Post. We provide our arguments for the appropriateness of county Seat as an instrument in Section 4. The Post variable in this section is a single indicator instead of the individual year indicator used in the earlier analysis. Table 5 reports the IV estimates along with the standard DD approach. The coefficients of interest are the coefficients on Bank*Post in the DD analysis and DD-IV rather than a series of theta coefficients like those reported in Section 5.1.[9] For the sake of space, we do not report the first-stage regressions but they satisfy the Stock and Yogo (2005) weak instrument criterion, namely a first-stage F-statistic in excess of 10.00.

Table 5
Difference-in-differences estimates Ordinary least squares and instrumental variables.
Sources: Authors' calculations.

Variable	OLS DD	IV DD
Bank	-0.639***	-0.270***
	(0.087)	(0.061)
Bank*Post	0.466***	0.537***
	(0.128)	(0.064)
Trend	0.076***	0.073***
	(0.008)	(0.004)
Log population 1820	0.668***	0.568***
	(0.008)	(0.062)
Percent labor in agriculture 1820	-0.947***	-1.189***
	(0.023)	(0.132)
Iroquois trail	-0.100***	0.000
	(0.004)	(0.079)
Log elevation	-0.045***	-0.069**
	(0.004)	(0.516)
Constant	3.882***	4.721***
	(0.147)	(0.516)
Observations	1,090	1,090
R-square	0.743	0.697
Bank first-stage F stat		1183.7
Bank*Post first-stage F stat		93.64

Notes: all regressions include town, county, and year fixed effects. * $p < 0.05$; ** $p < 0.01$; ***$p < 0.001$. Standard errors clustered on town. New York City and Brooklyn excluded.

The coefficient on Bank in the IV regression is one-third the magnitude of the OLS estimate but it retains the same sign. Our crucial coefficient, the interaction Bank*Post is similar in both specifications and it implies that, on average, a petitioning town that got a bank had more than 50% more inhabitants than a petitioning town that did not. But because a standard DD analysis may not completely control for endogeneity, we instrument for Bank*Post and the resulting IV estimate is larger than the DD and implies that the average successfully petitioning has more than 53% more inhabitants than unsuccessfully petitioning towns.

Most of the coefficients on the economic and demographic controls are similar in size and significance to the estimates of the OLS estimates of the standard difference-in-differences. The coefficient associated with population engaged in agriculture is larger in the IV regression and the one associated with the Iroquois Trail loses significance.

6. Robustness analysis

In this section we provide alternative estimates of the generalized difference-in-differences regressions using several subsamples to document that our main results are robust to the inclusion or exclusion of

[9] We could not include the waterway controls in these regressions because interacting them with banks generated a nonsingular matrix in the IV regressions.

Table 6
Theta coefficients from generalized difference-in-differences with economic and geographic controls.
Sources: authors' calculations.

	Drop towns with banks before 1821	Control for towns with banks after 1835	Control group: towns that never had a bank	Pre Civil War	Include year 1790	Drop small towns	Include NYC and Brooklyn
θ_{1825}	–	0.003	-0.021	0.045	0.153	0.001	-0.119
		(0.081)	(0.084)	(0.068)	(0.107)	(0.093)	(0.152)
θ_{1830}	0.323***	0.236***	0.206**	0.23***	0.292**	0.253**	0.212**
	(0.111)	(0.089)	(0.091)	(0.08)	(0.118)	(0.098)	(0.088)
θ_{1835}	0.275**	0.23**	0.256**	0.227**	0.294**	0.215*	0.285**
	(0.124)	(0.104)	(0.116)	(0.105)	(0.134)	(0.117)	(0.109)
θ_{1840}	0.32**	0.264**	0.332**	0.261**	0.328**	0.256**	0.331***
	(0.133)	(0.113)	(0.126)	(0.114)	(0.141)	(0.126)	(0.121)
θ_{1845}	0.41**	0.346**	0.392***	0.344**	0.411**	0.331**	0.431**
	(0.159)	(0.133)	(0.149)	(0.134)	(0.16)	(0.15)	(0.145)
θ_{1850}	0.54***	0.472***	0.503***	0.47***	0.537***	0.448**	0.574***
	(0.183)	(0.153)	(0.168)	(0.155)	(0.176)	(0.172)	(0.168)
θ_{1855}	0.59***	0.508***	0.553***	0.505***	0.572***	0.487**	0.626***
	(0.209)	(0.174)	(0.196)	(0.176)	(0.195)	(0.2)	(0.192)
θ_{1860}	0.624***	0.564***	0.611***	0.561***	0.628***	0.547***	0.694***
	(0.21)	(0.175)	(0.189)	(0.177)	(0.194)	(0.2)	(0.197)
θ_{1865}	0.681***	0.624***	0.674***	–	0.688***	0.604***	0.751***
	(0.225)	(0.185)	(0.2)		(0.202)	(0.21)	(0.205)
θ_{1870}	0.762***	0.694***	0.78***	–	0.759***	0.669***	0.832***
	(0.239)	(0.196)	(0.204)		(0.212)	(0.222)	(0.217)
Waterway	0.777***	0.92***	-0.182*	0.897***	0.978***	0.869***	1.019***
	(0.101)	(0.1)	(0.105)	(0.085)	(0.122)	(0.105)	(0.105)
Waterway*Bank	0.434***	-0.384***	-0.229**	-0.393***	-0.443***	-0.379***	-0.552***
	(0.147)	(0.095)	(0.108)	(0.08)	(0.117)	(0.109)	(0.099)
Log population in 1820	0.079**	0.421***	0.917***	0.461***	0.423***	0.28***	0.519***
	(0.036)	(0.007)	(0.012)	(0.008)	(0.007)	(0.031)	(0.008)
Perc. labor in agriculture in 1820	–	-1.86***	–	-1.724***	-1.869***	-1.697***	-2.165***
		(0.018)		(0.021)	(0.02)	(0.034)	(0.026)
Iroquois trail	-0.303***	-0.542***	0.332***	-0.58***	-0.532***	-0.46***	-0.844***
	(0.011)	(0.01)	(0.006)	(0.011)	(0.008)	(0.014)	(0.021)
Log elevation	0.254***	-0.033***	0.063***	0.024***	-0.036***	0.06**	-0.098***
	(0.009)	(0.006)	(0.004)	(0.007)	(0.006)	(0.025)	(0.002)
Constant	5.971***	5.854***	-0.278	5.155***	5.821***	6.545***	5.633***
	(0.173)	(0.124)	(0.19)	(0.113)	(0.141)	(0.101)	(0.17)
R^2	0.708	0.755	0.77	0.735	0.757	0.717	0.82
Observations	932	1,090	708	930	1,090	891	1,132

Notes: All the regressions include town, county and year fixed. All regressions exclude New York City and Brooklyn, except the last one. effects. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust clustered errors at the town level in parentheses.

certain towns and cities. Column (1) of Table 6 reports estimates for a sample that takes into account that 13 towns included in our earlier sample had a bank by 1820. Eight of these 13 towns were successful petitioners in that they petitioned for and received a second bank in our analysis window (1820–1835), the other five towns with an existing bank were unsuccessful petitioners. In the main results, the eight successful petitioners were considered to be "treated" in that they received a new bank between 1820 and 1835, but they may not be comparable to towns without an existing bank that petitioned. The theta coefficients in Column (1) are estimated after the 13 towns with pre-existing banks are dropped from the sample. The coefficients are positive and significant and so the independent effect of a bank, after accounting for the presence of a waterway, is large and statistically significant. Within the group of petitioning towns without an existing bank, the waterway-bank interaction is positive and significant. Within this subsample, banks and water were synergistic.

In our second robustness check, we take account of the fact that a bank opens in some (but not all) of the unsuccessful petitioning towns in the years after 1835. New York liberalized bank entry between 1838 and 1864, so that small towns were more likely to witness the opening of bank. In Column (2) were report theta estimates controlling for towns that get a bank after 1835. The theta coefficients are about the same as in our baseline sample. The theta coefficient for 1870, for example, implies that a successful petitioning town had twice the population of an unsuccessful petitioner.

Our third robustness check uses an alternative control group. In the baseline sample, the control group for the towns that receive a bank are the 35 towns that petition for and do not receive a bank. In Column (3) we report theta coefficients for the treated towns against a control group of towns that *never* got a bank. For this smaller sample, the theta coefficients are slightly larger than the baseline results. In 1870 successful petitioning towns would have more than twice the population of towns that never received a bank.

Our fourth robustness check limits the analysis to the pre-Civil War era, so that the last estimated theta is for 1860 instead of 1870. It may be reasonable to exclude 1865 and 1870 because passage of the federal banking and currency acts during the Civil War era fundamentally changes the nature of bank and bank entry. Yet the estimates imply that in 1860 a successful petitioning town was about 75% larger than unsuccessful petitioners.

The fifth robustness check in Column (5) of the same table makes use of population data for the year 1790. In our main analysis we dropped this year since the number of towns with data on population is much lower than in subsequent years.. The estimates show that the effect of including 1790 in the estimation is negligible.

In column 6 we omit any town with less than 2,500 inhabitants in 1835. This results in the loss of five treated towns and fifteen untreated ones. The results are not affected by this omission.

Finally, in the last column we include New York and Brooklyn in the regression to check whether these two large and fast-growing cities would alter our results. Our estimates shows that this is not the case. The inclusion of New York City and Brooklyn (they were separate

municipalities in this era) increases the absolute value of the coefficient on waterways and the water-bank interaction, but their inclusion does not alter any of conclusions drawn from the baseline sample. In the early and middle of the nineteenth century – an era that spans the first industrial revolution and the emergence of a capitalist America – banks exerted a powerful influence on town and city growth.

7. Conclusions

Evidence from nineteenth-century New York supports the hypothesis that financial development positively influenced urbanization and town growth. Jaremski and Rousseau (2013) use county-level data to show that the presence of a bank in 1850 or 1860 increased county-level urbanization -the fraction of a county population residing in a place of 2,500 or more residents- in the subsequent decade, though free banking's effect was modest. One contribution of our study, then, is that it shows that the effect they uncover at the county level applied to individual cities and towns and that it unfolds over relatively longer horizons than the two decades they study. In New York, at least, banks substantially increased the annual average rate of town population growth in the decades between 1820 and 1870 by about between one and two percentage points per year.[10] Urbanization and industrialization are often believed to progress together. Our results suggest one potential transmission mechanism. The availability of capital in the form of a bank may have attracted capitalists and entrepreneurs that in turn attracted migrants. Thus, cities and towns with bank grew larger than towns without one.

Contemporary observers noted that nineteenth-century Americans were a people on the make – bartering, trading, speculating and "smart dealing" – behavior that Europeans found curious and troubling (Toqueville 2002; Dickens, 1842/1970, 171). Given contemporary Americans' propensity to trade, it is not surprising that places that provided financial services attracted more people, especially those of an enterprising or speculating bent. The question of which cities attracted migrants was answered, in part, by the answer to a different question: where are the banks? There is any number of reasons why an individual might find a location appealing: it might have good schools, access to transportation networks, scenic surroundings, lucrative employment opportunities, and so on. Our study finds that places with banks were incrementally more attractive destinations than places without, all else constant. Economic historians have connected the idea of an early nineteenth-century financial revolution with the finance-growth literature to posit that finance encouraged development and modernization (Sylla, 1998; Sylla and Rousseau, 2005). Our study adds a new wrinkle to that conclusion; finance also mattered in the process of urbanization, population growth and, possibly, the agglomeration of economic activity.

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[10] A 1.6 growth factor of 1.6 (exp(0.466)) in the mean-shift model implies that, for treated towns, the predicted average population in 1870 is 1.6 times the average population in 1820 i.e. 1.6*7,500 = 12,000. This is a 60% population growth over a 50 year-period or a 1.2% growth per year. The dynamic model gives us a factor of 2 (exp(0.694)) which corresponds to a 2% growth per year.

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