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FINANCIAL DEVELOPMENT AND CITY GROWTH: EVIDENCE FROM  
NORTHEASTERN AMERICAN CITIES, 1790-1870

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Cities and Innovation

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**FINANCIAL DEVELOPMENT AND CITY GROWTH: EVIDENCE  
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**ABSTRACT:** We find a positive and strong correlation between financial development and subsequent city growth in the Northeastern United States between 1790 and 1870. The correlation is robust to controls for geographical characteristics of the city, the percentage of population working in different sectors, and its initial population. Our estimates suggest that the presence of a bank at a given location increases its subsequent growth by one to two percentage points per year. Because urban growth was correlated with economic development in the nineteenth-century US, we believe our results provide further support for the finance-growth nexus.

JEL Codes: E41, E51

Keywords: Population growth, city growth, financial development, economic growth, bank liberalization, program evaluation.

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## 1. Introduction

In 1820 the place that was to become Lowell, Massachusetts was not even an incorporated village, so its population was not separately reported in that year's census. A decade later, the town already had nearly 6,500 residents and in 1860 Lowell's population exceeded 36,000. Lowell's experience was not unique; Worcester, Massachusetts, Nashua, New Hampshire and Cumberland, Rhode Island all experienced comparable patterns of growth. What happened to create bustling cities and towns in once backwater areas?

It wasn't the railroad, not at least in New England, nor was it the turnpike.<sup>1</sup> Taylor (1967) found that urbanization was well under way by the 1820s and 1830s, well before the age of rail transportation. Canals and river steamboating lowered transportation costs, but lower water transport costs were not the principal cause either. He attributed contemporary urbanization to the doubling of cotton textile output every few years, which increased the production of textile-related goods and ancillary services (David 1970). Taylor understood that he had barely scratched the surface of understanding the factors that contributed to city and town growth in the pre-Civil War years. What of flour milling, inspection and shipping, he asked? What of entrepreneurship? What of institutional and political factors? What of banks? Taylor equivocated on the last, but intimated at their importance. Contemporary observers certainly believed banks mattered, mostly because they fueled entrepreneurship (Ashmead 1914; Crothers 1999). Kroos (1967) acknowledged a link between banks and urbanization and observed that some "cities were more aggressive in expanding their financial institutions ... [but only] a daring generalizer would say that these slight differences had something to do with the way ... cities grew." He was not so daring a generalizer.<sup>2</sup> Kroos contended that finance was secondary to other factors, including geography and technological innovation.<sup>3</sup> We take up a study of the connection between finance and urban growth -- daring to become daring generalizers -- to sort out the relative weight of finance and offer some answers to Taylor and Kroos's long unanswered questions.

We begin by positing that the availability of external finance tends to mitigate financing constraints on entrepreneurial enterprises, which hastens economic growth.<sup>4</sup> Further, certain entrepreneurial firms, or emergent industries, are subject to agglomeration economies so that when the growth of one firm or one industry attracts related or complementary activities local industry expands, workers are drawn in, and urban growth follows (Glaeser and Gottlieb 2009). This is indeed what occurred in nineteenth-century Lowell, Massachusetts.

It is hard to imagine that, on average, cities and towns that experienced financial deepening would not subsequently grow, though a few that experienced some initial financial development failed to take off. Who, for example, now knows of Cherry Valley, New York, which was among the first of New York's interior towns to have a commercial bank? The issue is whether financial development accelerated subsequent urban growth. What we measure is the extent to which finance incrementally influenced urban growth.

An important empirical issue is whether financial development and its connection to economic (and therefore urban) growth may be due to what Bordo and Rousseau (2006) label "deep endogeneity." That is, the preconditions for both financial development and economic growth may be found in institutional factors that emerged long ago (Besley and Persson 2010; Acemoglu et al 2001; Sokoloff and Engerman 2000). Attempts to sort out the effects of policy from deep endogeneity are problematic and often rely on weak or questionable instruments (Roe and Siegel 2009). Moreover, Bordo and Rousseau (2006) and Rajan and Zingales (2003) find some aspects of the finance-growth nexus less than compelling when tested against long-run historical data. By restricting our analysis to the northeastern United States between 1790 and 1870, we hold constant much of that deep endogeneity. Having common English legal origins and having embraced the corporate form in nonprofit, commercial, manufacturing and financial activities, differential rates of financial development and urban growth should not be simultaneously driven by institutional

factors of deep historical origin. Instead, differential rates of growth were driven by state-specific idiosyncratic political factors that influenced incorporation policies (Bodenhorn 2003; 2008; 2009).

Instead of searching for instruments for institutions, we adopt an empirical strategy that should hold those historical institutional factors constant. We begin by investigating a series of cross sections, regressing urban growth on factors believed to influence it, including financial development. Our identification strategy centers on the considerable cross-sectional and time series variation in state-level banking development and urban population growth in the first half of the nineteenth century. This variation makes it possible -- and particularly interesting -- to explore the link between the process of finance and growth. Our strategy, therefore, exploits several advantages in the data. First, as noted above, all states shared common legal origins, namely English common law. Second, finance was likely to matter more early in the development process than after a place experienced substantial industrialization and urbanization (Rousseau, 2003). And, third, states differed in their policies toward bank incorporation, which had practical consequences for the rate of financial development. Because we cannot control for a host of potential contributing factors, we use fixed effects and general method of moment (GMM) approaches to control to the extent possible for unobservables. We also employ propensity score matching techniques and a Heckman-type selection model to take into account that the cities that receive the banking “treatment” may constitute a nonrepresentative sample.

Our results suggest a significant positive impact of banking activity on subsequent city growth. The presence of a bank and a ten percent increase in such activity are both associated with an increase in subsequent city growth of between one and two percentage points, depending on the estimates and the time periods considered. Compared to other measurable geographic and institutional features, banks mattered, often more than canals or the presence of manufacturing enterprises. We interpret this to mean that America’s nineteenth century financial revolution was as

important a factor in the country's growth as the much more studied transportation, commercial and industrial revolutions.

## **2. Related Literature**

The modern literature documenting the connection between financial development and economic growth is now so diverse that no simple taxonomy can capture the subtleties of each argument, but it can be usefully separated into four principal approaches (Pascali 2009). The first approach was that adopted by King and Levine (1993), Levine and Zervos (1998), and others. Using cross-country regressions, they found that a country's initial level of financial development – measured by four alternative metrics – in 1960 was positively associated with economic growth over the subsequent two decades. But inferring causality from cross-country regressions that adopt this approach is problematic because the estimates may suffer from omitted variable bias, reverse causality or deep endogeneity.

A second approach attempted to rule out potentially important omitted country-level factors by adopting an industry-level approach. Rajan and Zingales (1998) and Mitchener and Wheelock (2010) tested the finance-growth hypothesis by testing the related hypothesis that finance is more likely to lead to the expansion of manufacturing industries that are more dependent on external finance.<sup>5</sup> If finance incrementally increases growth in this sector of the economy, finance can be said to matter. One problem with this approach is that it assumes that countries share similar technologies and that similarly defined financial intermediaries perform similar tasks across economies, which is a dubious assumption, given the notable differences in the American market-based and European bank-based systems.

A third approach adopts a time-series approach and studies the consequences of financial liberalization on subsequent growth rates. Jayaratne and Strahan (1996), for example, found that economic growth increased in states that relaxed branching restrictions. One criticism of this

approach is that regulatory change may not be exogenous to other developments in financial markets or to other features of firm finance. Financial liberalization often occurs concurrently with other types of economic reform (Fry 1995).

The fourth approach attempts to deal with potential biases arising from reverse causality and endogeneity by adopting an instrumental variables approach. One formulation takes advantage of the panel nature of some data sets and uses generalized method of moments (GMM) methods where the instruments come from lagged values of the independent variables. Levine, Loayza and Beck (2000), for example, use a panel of 77 countries over a 35-year period that controls for country-level fixed effects, but this approach is not without its own interpretative difficulties (Levine 2005b): thus, the search for exogenous instruments that may explain financial development. Levine (1998, 1999), Levine, Loayza and Beck (2000) and La Porta et al (1998) use legal origins and Pascali (2009) religion as instruments, and find that the finance-growth link remains robust. Yet, this approach is not without its own shortcomings (Roe and Siegel 2009). It is not clear, for example, that legal origins or religions influence only finance and did not have lasting influences on a whole range of social, political, economic, legal and constitutional features that affect growth through channels wholly independent of finance (see essays in Haber 2007). Moreover, nearly all these studies' conclusions are based on small cross-country samples which implicitly assume deep structural commonalities between such diverse countries as Burkina Faso, India, New Zealand, the United States and Switzerland. Abramovitz (1986) considered these kinds of cross-country studies vacuous.

Still, the weight of evidence supports the hypothesis that finance affects growth, even if the mechanism remains poorly understood. In his two review essays Levine (1997, 2005a,b) argues that the existing research suggests three conclusions. First, countries with better functioning banks and financial markets grow faster, but the degree to which a country's financial sector is bank-based or market-based does not appear to matter. Second, simultaneity bias does not drive the



finance-growth result. And, third, better functioning financial sectors mitigate external financing constraints that often retard firm growth, which suggests that the easing of such constraints encourages innovation and entrepreneurship and, therefore, economic growth (Benfratello et al., 2008).

There is a long tradition of studying the finance-growth nexus in economic history (see, for example Cameron et al. 1967) and recent contributions by economic historians to this literature include Bodenhorn (2000) who found that a 10 percent increase in loans per capita increased the annual average rate of income growth in early nineteenth US by about 23 percent. Ramirez (2009) estimated that disintermediation due to bank failures during the panic of 1893 diminished subsequent state economic growth rates by 2 to 5 percent. Among economic historians, Rousseau and his coauthors have provided the most compelling evidence of the finance-growth nexus (Rousseau and Wachtel 2000; Rousseau 2003; Rousseau and Sylla 2005, 2006; Bordo and Rousseau 2006). These historical studies are, as Rousseau (2003) noted, “consistent with the view that financial factors matter most emphatically in the early stages of economic development by mobilizing and allocating resources.”

We build on these earlier historical studies by studying the connection between finance and economic growth, but use an alternative, more easily measured metric of growth, namely city or town population growth. Urbanization and economic modernization typically occurred simultaneously, so that the former may be a useful measure of growth when the more traditional measure is lacking. If we think about finance as a means through which external financing constraints are mitigated for particular firms or particular industries, it is reasonable to think that lifting at least some of those constraints will encourage firm or industry growth. Further, if agglomeration economies or if firm growth simply attracts related or complementary activities, local industry will expand, attract workers from the hinterlands, and therefore lead to urban growth.

Cities exist because they are places of high productivity (Glaeser and Gottlieb 2009). Finance may directly encourage high productivity through efficient allocation of capital with high rates of return. But the channel may be indirect in that financial intermediaries, especially in the earliest stages of the development process, act as information intermediaries between people with innovative ideas and people with capital. “Urban intellectual connections create agglomeration economies and ... remind us that many intellectual revolutions involve small numbers of connected inventors” (Glaeser and Gottlieb 2009, p.1016). Historical studies demonstrate the importance of finance in encouraging invention and industrial growth, sometimes in unlikely places (Bodenhorn 1999; Lamoreaux, Levenstein and Sokoloff 2004).

### *2.1. The emergence of banks and finance in the early United States*

In the late eighteenth and early nineteenth century the United States experienced the “Federalist financial revolution” (Sylla 1998). The Bank of the United States was chartered and its shares, along with the public debt, were traded in emerging secondary markets in Boston, Philadelphia and New York. By any standard of comparison, the speed at which the U.S financial system emerged was remarkable and probably unprecedented (Rousseau and Sylla 2005).

Impressive as the federal innovations were, the real action in financial development took place at the state level, and state-level variation affords us an identification strategy. Until 1837, every state required every commercial bank to obtain a legislative act of incorporation (charter) and states were not equally generous in granting charters. One useful comparison is Massachusetts, New York and Pennsylvania, each of which adopted some form of corporate chartering, but was differentially liberal in their granting of such charters (Bodenhorn 2008). Liberal Massachusetts had 116 operating banks prior to the panic of 1837. Illiberal Pennsylvania had 49 operating banks in 1837. New York, an intermediate case, had 98 operating banks in 1837. Comparably disparate chartering patterns are observed between neighboring Connecticut and Rhode Island, between New Hampshire and Vermont, as well as New Jersey and Delaware (Weber 2005).

The pattern of bank incorporations was unlikely to be endogenous to economic or urban growth, though we control for endogeneity to the extent possible in our empirical work. Due to the idiosyncratic nature of bank incorporation by state, it is difficult to succinctly summarize or categorize state to state differences. We might be concerned that banking was endogenous to urban growth if bank chartering followed from political power, which followed from the size of the legislator's home district. Any reasonable reading of the history is inconsistent with this interpretation (Knox 1900). Instead, committee structure, voting rules and partisan politics at a given moment determined incorporation policy and these were typically orthogonal to the growth of places. Bodenhorn (2009) details the torturous process of bank incorporation in New York and finds that financially underserved places (mostly growing towns) were not more likely than already well served places to be granted additional banking services. Ultimately, banking was endogenous to subsequent city growth only if legislators and legislative committees developed accurate predictions of population trends and allocated banks based on those predictions. Any causal reading of the 19<sup>th</sup> century legislative process suggests the improbability of such accurate and rational decision making.

## *2.2. City and town growth as a proxy for economic development*

Lacking evidence on traditional measures (such as state-level income), we use town and city growth as an indicator of wider economic growth. We are, of course, not the first to exploit the connection between urbanization and growth. Atack, Haines, and Margo (2008) connect urbanization and economic growth and De Long and Shleifer (1993) argued that urban populations are good measures of pre-industrial economic prosperity. It might be that urban centers arise because they are bureaucratic centers who extract tribute from their hinterlands, but cities in most western countries thrived because they were commercial and industrial centers (Ades and Glaeser 1995).<sup>6</sup> This also appears to be the case for the early nineteenth century United States. American towns and cities developed because they served as central places in the supply of goods and

services to their respective hinterlands (Crowther 1976). Their size and importance increased as the number of people in their hinterlands expanded. Rubin (1967), in fact, argued that one of the determinants of interior urban development was isolation. Transport barriers between places did not stifle growth, but rather encouraged domestic industry because transport costs acted like a protective tariff. It is also important to recognize that interior towns were not just collection points for outgoing primary output and incoming manufactures. Interior towns supported a wide array of commercial and manufacturing enterprises.

To be sure, many American interior towns and villages had no more than a few hundred to a few thousand residents and would not be considered urban centers today. But these early nineteenth century towns were well diversified for their time. As one contemporary exuberantly observed of his Ohio home town, “there is no manufacture in this country which is not found here” (Rubin 1967, p. 14). Further, a non-negligible fraction of westward bound Americans were not looking to put land under the plow. They sought a fresh start in a new town. As early as 1787, residents of Lexington, in then-western Virginia, petitioned the legislature for an act of incorporation, believing that corporate status would act as an “inducement to well-disposed persons, artisans and mechanics who from motives of convenience do prefer a Town life” (Rubin 1967, p. 13).

With just six urban places – those of at least 8,000 inhabitants -- in 1790, the original thirteen states had 139 such places by 1870 (U.S. Bureau of the Census 1909). In fact, more than one in four Americans resided in one of these places in 1870. These figures are all the more remarkable given two features of American population history. First, on the eve of the American Revolution, the population of the thirteen colonies amounted to just more than 2 million people, and most lived within 50 miles of the Atlantic coast. Less than a century later, more people lived in 68 eastern urban places than had lived in all the colonies. Second, population pushed westward, so that by 1870 there were as many people living in places unsettled by whites prior to the Revolution as there were in the original 13 states. If we include these westerners living in western places, the United

States had 226 urban places in 1870 with a total population of more than 8 million people, representing nearly 21 percent of the US population. During the first half of the nineteenth century, then, the United States urbanized as it built turnpikes, canals, railroads, steamboats, textile mills and applied steam power to a host of economic pursuits. Urbanization and economic growth, while not one and the same, were concurrent and changes in the former mirror movements in the latter.

### **3. The Data**

Historical data on the population of towns and cities – what the Census Bureau labeled “minor civil divisions” -- was collected every ten years beginning with the first federal census in 1790. Michael Haines has recently digitized these reports and made them available in separate files, one for each census. We merged these files so that we have city and town populations for each census year between 1790 and 1870.

Not every state is included because not every state consistently provided a time series of city and town population figures. Southern and western states, for example, often reported population aggregates only at the county level, especially for the earliest censuses. We exclude these states. Other states reported town and city aggregates intermittently (not every census) and irregularly (not every county in every year). These states were also dropped. After dropping states with unusable data, we were left with the northeastern and mid-Atlantic states (New Hampshire, Vermont, Massachusetts, Rhode Island, Connecticut, New York, New Jersey, Pennsylvania and Delaware) and Ohio.

Merging the files is not a trivial exercise because town names changed through time, because it was not uncommon for states to have more than one town by the same name, and because city and town borders changed through time. It is the third issue – changing borders – that presents the greatest challenge. In Connecticut, for example, new cities were carved out of old ones so that size

of a given city may appear to decline when, in fact, it may not have if we include the population living in areas formerly part of the original town (Taylor 1967). We can consider this a problem of measurement, one that introduces systematic error into the estimates; or, we might consider it as an indicator of some real, underlying urban dynamic in which a legally defined administrative unit outgrew its administrative capacity. The division of a single corporate entity into two separate entities, then, represents not measurement error to be corrected, but rather change to be accounted for. We accept the current definition of a city at each census as a meaningful economic and political unit and account for changes in that legally defined entity between censuses.

The correlate of primary interest is financial development. We use two measures of financial development taken from Fenstermaker (1965): the presence of a bank and *bank capitalization*, the modern corollary of the amount raised by the bank's organizers in an initial public offering.<sup>7</sup> *Bank capitalization* is measured in current dollars in the year of incorporation. The remarkable features of these two data series for modern observers are: (1) the small proportion of US towns and cities with a bank – just 0.14% in 1790, increasing to 4.38% in 1840; and (2) the relatively small size of 19<sup>th</sup> century banks – typically between \$100,000 and \$500,000 in current dollars (about \$1.5 - \$8.5 million in 2009). Despite their modest size, the assets controlled by the representative bank dwarfed those controlled by nearly every contemporary enterprise, with the exception of a handful of canals, railroads and insurance companies. We believe these two series provide useful indicators of a city's financial development at key benchmark dates.

To locate the cities and towns through which canals passed, we use both contemporary and modern maps, as well as contemporary legislative documents that listed the location of locks and shipping rates from a given place to the canal's terminus. Because states often had direct financial interests in the canals or taxed traffic revenues, legislative documents reproduced detailed accounts of revenues and expenses attributable to ports and locks along the canal. Similarly, contemporary

gazetteers reported mileages between entry points and locks and each terminus. All of these data were used to locate cities and towns, large and small, along the routes of major and minor canals.<sup>8</sup>

Finally, to capture the relative importance of large commercial seaports, we include a dummy variable for cities and towns with US customs houses.<sup>9</sup> Cities with customs houses may have experienced more or more rapid urbanization because they came to serve as central nodes of international trade and political power (Acemoglu, Johnson and Robinson 2005). The inclusion of a dummy variable is intended to capture any such effect.

Two censuses – 1820 and 1840 -- are of particular value to our study because they categorized and reported aggregate employment in several occupations at the city or town level. The 1820 census, for example, reported employment in agriculture, manufacturing and commerce. The 1840 census reported employment in agriculture, manufacturing, mining, commerce and professions, as well as inland and ocean-going navigation. We make use of these data to control for prior industrial development in our estimates of subsequent urban growth. Table 1 reports basic summary statistics of our data.

The summary statistics reveal the broad outlines of urban growth and economic development from the Federalist (1789-1800) through the Civil War (1861-1865) eras. The population of the average city or town separately reported in the federal censuses more than doubled over the 80-year period, from 1,400 to 2,900 inhabitants. Manufacturing employment increased from an average of 74 persons in 1820 to 122 persons in 1840, or from approximately 5% to 7% of the average urban population.<sup>10</sup> Agriculture, however, remained the principal employment. A constant 17% of the urban population received its income from agricultural pursuits between 1820 and 1840. Mining accounted for a constant fraction of 0.9% of urban population, while professional jobs and employment related to ocean navigation represented 0.5% of the urban population. Finally, the share of employment related to inland navigation was about 0.3%. As previously mentioned, the

percentage of cities with some bank activity (i.e. with at least one local bank operating there) increased by 4 percentage points between 1790 and 1840. Bank capitalization increased sevenfold between 1810 and 1840. Finally, canals passed through only 319 cities (about 5%) of our sample.

Figure 1 displays the number of cities that received their first bank by year. There is no easily characterized pattern up to 1812, other than in several years just one city witnessed the opening of its first bank. There is a sharp increase in charters in the years just after the Bank of the United States was closed (1811) until the panic of 1819 and subsequent recession. Between 1825 and the panic of 1837 there was a fairly constant increase of about 10 to 15 banks each year. Over the entire sample period (1782-1844), however, a first bank arrived in an average of about five cities or towns each year. Sylla's (1998) assertion of a financial revolution notwithstanding, the banking sector grew at a measured pace. By 1820, just 134 cities and towns had at least one bank; by 1843 that number had grown to 284.

Given our focus on finance and urban growth, Figure 2 displays the evolution of *bank capitalization* in the five cities with most banking activity in 1810 and further reveals the idiosyncratic nature of contemporary bank location practices. Philadelphia had the most banking activity in 1810, followed closely by New York. In 1820 and 1830 New York took the lead in banking, as it had in commercial activity, but it was surpassed by both Philadelphia and Boston in 1838. The Philadelphia figure is inflated by the Second Bank of the United States switching to a Pennsylvania charter after Jackson's veto of the federal recharter. New York's failure to add to its banking facilities is typically attributed to the legislative gridlock surrounding bank chartering that emerged in the state in the 1820s and reemerged after a brief chartering wave in the early 1830s (Knox 1900; Bodenhorn 2006).

#### **4. Empirical Strategy and Results**



We provide three set of estimates of the effect of bank establishment and bank size on city growth. The first are standard OLS cross-sectional regressions. We also discuss how to address the potential nonrandomness of our sample using the Heckman selection model. Next we report panel regressions controlling for fixed effects using both OLS and GMM methods. Finally, following the treatment effect literature, we provide four different propensity score matching estimates that compare similar cities – in terms of observables- that received (or not) the “treatment” of some local banking activity.

#### 4.1. Cross-sectional regressions

##### 4.1.1. OLS estimates-benchmark regressions, 1790-1870

Our regression strategy in this section follows the King and Levine (1993) strategy of regressing measures of initial financial development (either the presence of a bank or authorized capital in our case) on the subsequent rate of growth of city or town population. As additional controls we include the initial size of the town, whether a canal passed by the town, whether a customs house was located there, a state indicator variable and, when appropriate, the proportion of the labor force employed in each of several occupations. Several concerns have been raised about this procedure, including reverse causality, endogeneity and others, and we treat these as baseline regressions against which to compare more sophisticated attempts to deal with potential endogeneity. Specifically, we estimate the following cross-sectional regressions:

$$n_{T-t} = \alpha + \beta_1 D_{bank,t} + \beta_2 \ln N_t + \beta_3 D_{canal} + \beta_4 D_{custom} + \beta_5 D_{state} + \varepsilon \quad (1)$$

where  $n_{T-t}$  is the average population growth rate of a city between the initial year  $t$  and  $T=1870$ , with  $t = 1810, 1820, 1830, \text{ and } 1840$ .  $D_{bank,t}$  is a dummy variable that takes value of one if the value of the variable  $bankcap$  in  $t$  is positive (i.e. if the city had at least an operating bank in that year, and zero otherwise).  $N_t$  is the population of the city in  $t$ .  $D_{canal}$  and  $D_{custom}$  are dummy variables that take value of one if the city ever had a canal or a customs office. These dummies are meant to capture the importance of transport costs and trade, respectively.  $D_{state}$  is a state dummy variable for the ten states covered in the sample.<sup>11</sup>

In the 1820 regression we also include as regressors the percentage of population working in manufacturing, agriculture, and commerce reported in that year's census. Finally, in the 1840 regression we add as controls the employment percentages included in the 1820 regression in addition to the percentage of population working in mining, in professional jobs (attorneys, doctors, and so on), and in inland or ocean-going navigation.

The regressions reported in Table 2 perform reasonably well and, given the small number of explanatory variables, explain a surprising fraction (between 21% and 32%) of the cross-sectional variance in urban growth. Several notable features of urban growth are evident. First, the negative coefficient on the logarithm of the initial level of city size suggests population convergence. That is, smaller places tended to grow faster than larger places. Second, consistent with interpretations by Bruchey (1967), Crowther (1967), Taylor (1967) and Acemoglu et al (2005) trade, especially international trade as measured by the presence of a customs house, was an important driver of urban growth in the early nineteenth century. Cities with customs houses (sizeable seaports, mostly) grew at a 1 percentage point faster rate than non-custom house cities. The presence of a customs house may have encouraged traders to use these ports as points of entry because doing so lowered the costs of paying duties, which resulted in notably faster rates of urban growth. Third, location on water -- canal, river, bay or ocean -- increased the average growth by about 0.6 and 1

percentage point. The coefficient on the percentage of urban population employed in the manufacturing sector in 1820 (see specifications [2] and [3]) is 0.01. This implies that, evaluated at the mean, a one standard deviation increase in the share of the labor force employed in that sector would lead to a 0.2 percentage point increase in the rate of urban growth. In specification [4] the coefficient on manufacturing employment in 1820 is statistically insignificant but the one associated to 1840 is larger (0.02). Moreover, in this last specification, the percentage of urban population working in commerce in 1840 is quite large (0.05). In all cases none of the associated percentage in agriculture, mining, and professional jobs is statistically significant.

The result reported in Table 2 that we emphasize is the effect of financial development on city growth. The estimated coefficient is positive and statistically significant in all four specifications, which differ only in the initial date at which we observe a bank. Estimated coefficients are quantitatively very similar in the four specifications, implying that the year we choose to measure banking activity does not drive the result. Given that earlier generations of economic historians were, at best, agnostic and, at worst, skeptical of the influence of banks on urban growth, the magnitude of the estimated effect is notable. Generations of historians have highlighted the importance of the transportation (Taylor 1951; Majewski 2000), and commercial (Sellers 1991) and industrial (Atack and Passell 1994; Hughes 1990) revolutions.<sup>12</sup> Yet, it appears from our estimates that the Federalist financial revolution (Sylla 1998) was of comparable importance. Having at least one bank in a town in 1838 increased the rate of urban growth over the next three decades by a full percentage point. This is about 3.3 times the magnitude of having a canal pass through the town and 5 times the magnitude of having some employment in the navigation sector. Finance mattered; it mattered at least as much as other traditional explanations that center on changes in the real sectors of the economy.

In Table 3 we report the results of specifications comparable to those reported in Table 2, except that we replace the bank dummy variable with *banking capital* measured in nominal dollars

at the same benchmark dates. This measure estimates the extent to which incremental additions to the size of the financial sector influenced urban growth rates. The results on the control variables – initial population, percentage of manufacturing employment, water transport and the presence of a customs house – are all consistent- and indeed almost identical- with the coefficients reported in Table 2.

Again, the main result here is that the size, not just the presence of a bank influenced urban growth rates. Following the literature, the coefficient can be usefully interpreted in any of three ways. First, using the estimated coefficient in column (1) of Table 3, a 10 percent increase in bank capitalization is associated with an additional average population growth rate of 0.09 percentage points between 1810 and 1870. Alternatively, consider the city of Keene, NH. With just \$50,000 in bank capital, it had one of the smallest banks among those places with a bank in 1810. If Keene had increased its bank capitalization – via a larger number of local banks or an increase in the size of existing banks – to the median level of cities with a bank (\$250,000), its average growth rate for the period 1840-1870 would have increased by 0.14 percentage points. Finally, if a place like Haverhill, New Hampshire, at the 25<sup>th</sup> percentile of banking capital (\$75,000) had increased its capitalization to the 75<sup>th</sup> percentile (\$150,000), like Bedford, Massachusetts, annual average population growth would have increased by 0.06 percentage points.

As mentioned above, the previous regressions cannot be interpreted as reflecting a causal relationship from financial development to city growth since the relationship is potentially endogenous. Ideally, we would like to use an instrumental variable that affects bank location (or even bank size) but has no direct impact on subsequent city growth. We have been unable to identify an instrument that satisfies the usual statistical requirements. A second possibility would be to follow Duranton et al. (2009) and use regression discontinuity techniques to estimate the causal effect of banking activity on city growth. The idea would be to compare two cities located very close by but are located in different states. Such cities are arguably similar in terms of location and

geographic characteristics but they crucially differ in the fact that they are subject to different state laws and hence have – presumably- different access to local banking. We have too few city pairs that satisfy these requirements to implement this procedure.

In the alternative, in the Appendix we use a Heckman-type model of selection to control for selection of cities with banks. We first estimate a probit model to determine the probability that a bank locates in a given city and then control for this selection in our city growth-banking equation. Our exclusion restriction is that initial population (20 years prior to bank location) affects bank location but it does not have a direct impact on subsequent city growth. The two-step estimates suggest that banking activity in 1810, 1820, and 1838 affects subsequent city growth. The instruments seem to work well in 1810 and 1820, but less well in 1830 and 1838.

The next two sections present two robustness checks of our OLS estimates. We first explore whether the results are driven by the exclusion of Ohio's cities in our sample. For reasons explained below, Ohio differs from other states in significant ways. Second, we test whether the results hold when we exclude small towns (less than 2,500 inhabitants) from our sample.

#### 4.1.2. Including Ohio, 1790-1860

Because city-level population figures are unavailable for Ohio in 1870, it is not included in the previous estimates. However, the Ohio data is complete between 1810 and 1860, so we reestimate the same regressions reported earlier for a shorter sample period, but one that increases the number of usable observations by about 29 percent. Tables 4 and 5 provide OLS estimates comparable to those reported in Tables 2 and 3, except that the sample includes Ohio and considers growth over the shorter interval.

The estimated coefficients on the bank dummy in Table 4 are twice as large as those reported in Table 2. The presence of a bank in a city increases its average growth rate between 1840 and 1860

by 2 percentage points. The coefficient on log population in 1840 also increases, suggesting a more rapid rate of convergence in city size. Interestingly, the positive coefficient on the percentage of population working in the manufacturing sector in 1820 is now no longer significant but the one associated with 1840 (specification (4)) is larger than before. The impact of employment in both mining and commerce are now notably larger than before, as are the canal and customs house dummies. The coefficient on inland navigation is the same as before. Regressions using *bank capitalization* as a regressor in Table 5 are also comparable to those reported in Table 3. Although the inclusion of a single western state does not show that the effect of banking on urban growth generalizes outside the Northeast, it does not appear that a different process was driving western urban development.

#### 4.1.3. Excluding small cities

In our second robustness check, we drop all towns with fewer than 2,500 residents. Standard practice in economic history adopts the 2,500 inhabitant cutoff as the definition of an urban place, and we follow that practice here. Moreover, towns with fewer than 2,500 residents were unlikely candidates for a bank, so in excluding the smallest towns and villages from our sample we are estimating the effect of a bank on places that may have reasonably expected to have gotten one. Tables 6 and 7 report OLS coefficient estimates comparable to those reported earlier. Excluding small towns reduces the number of observations from about 2,000 to about 500, yet the  $R^2$ 's do not change much, indicating that the explanatory power of small towns is rather small.

The important result, of course, is that the exclusion of the smallest places does not change the estimated coefficients on the bank dummy or bank capitalization by much. The estimated bank and bank capitalization coefficients in the 1810-1870 regressions are no longer significant, but coefficient estimates in the remaining regressions are precisely estimated and of the same order of magnitude as those estimated from the full sample. This result increases our confidence that we are

identifying a substantive result that is not dependent on the sample. The result appears to be quite robust to meaningful subsamples of the data.

#### 4.2. Panel Data Estimates

In this section we recognize that there are a host of unobservable influences that may have influenced city growth that we cannot account for. We therefore estimate a model that exploits the panel dimension of the data and includes city fixed effects. Our panel has 25,656 observations (corresponding to 6414 cities). Of these 598 observations (268 cities) have a positive level of banking activity.<sup>13</sup> We estimate the following regression:

$$n_{it} = \xi_i + \beta_1 \text{bankcap}_{it} + \beta_2 \ln N_{it} + \varepsilon_{it} \quad (2)$$

where  $N_{it}$  denotes the population of city  $i$  in period  $t$  and  $n_{it} \equiv \frac{1}{10} * \ln \frac{N_{it+1}}{N_{it}}$  is the yearly growth rate of population between the years  $t$  and  $t+1$  (which are ten years apart given the nature of our data).  $\text{bankcap}_{it}$  denotes the level of banking activity (in dollars) of city  $i$  at period  $t$ . Finally,  $\xi_i$  is a city fixed effect (that includes a common constant term) and  $\varepsilon_{it}$  denotes a standard error term.<sup>14</sup>

We first estimate (2) using a simple OLS estimate. The results are shown in the Table 8. As in the cross-section case, increases in the degree of bank capitalization are clearly associated with increases in subsequent city growth. The impact now is of similar magnitude to the one reported in Table 3.

It is well-known that, by construction, the presence of the lagged dependent variable as a regressor and the use of fixed effects renders the OLS estimates inconsistent (Wooldridge, 2001). We therefore follow Beck and Levine (2004) and estimate the same regression using GMM techniques to alleviate endogeneity problems. In particular, we use lags of the city growth rate and of the lagged banking variable as instruments.<sup>15</sup> Specifications [1] and [2] of Table 9 present the

system GMM estimates, i.e we use the equation in levels in our set of instruments.<sup>16</sup> Instrumenting our equation with lags of the dependent variable does not change the estimates much. It is not unreasonable to conclude from these results that reverse causality issues are not driving the positive correlation between financial development and city growth.

#### 4.3. Estimation of Average Treatment Effects

Coefficients reported in previous sections estimated the effect of the establishment of a bank had on the subsequent growth of a city's population. The advantage of cross-sectional OLS estimates is that they allow us to control for meaningful covariates, such as employment mix or the presence of a canal or a customs office. The potential endogeneity of bank location may, however, bias our estimates. Panel data techniques, on the other hand, have the advantage of allowing for the use of lags of the dependent variables as instruments and hence mitigate this endogeneity bias. One drawback of the panel approach is that it is impossible to account for effects of some important covariates.

In this section we adopt a third approach, one that has the advantage of using all the variables used in the cross-section analysis and that is able to isolate the treatment effect of establishing a bank in a particular place at a particular time. Following the seminal work of Rosenbaum and Rubin (1983), we use propensity score matching techniques to reduce the potential endogeneity bias present in our earlier estimates.

As stated in Dehejia and Wahba (2002), a typical problem in the evaluation literature is to estimate treatment effects in observational studies in which a group of units (cities) is exposed to a well-defined treatment (establishment of a bank in a given city), but no systematic methods of experimental design are used to maintain a control group. In other words, the variable of interest (city growth) is observed under either the treatment (establishment of a local bank) or control (no establishment of a local bank), but never both.



The idea behind the different matching estimates that we describe below is to study the variable of interest in treatment and comparison units that are similar in terms of their characteristics. In our application we will seek to compare city growth after year  $t$  in cities that have had their first banking activity at period  $t$  with cities that never had a bank but are otherwise similar in terms of our controls.<sup>17</sup>

Our strategy to construct suitable control groups is the following. We first consider the subsample of cities that ever had a bank (266 out of the 6414 cities) and group them by the decade at which they experienced the treatment of a bank establishment.<sup>18</sup> We therefore use decade aggregates, which yields the decadal treatment groups in Table 10.

As previously noted, the four year period between 1840 and 1843 is not very informative because it represents less than a full decade and was characterized more by bank closings during the recession than bank openings. Thus, we omit this cluster as a treatment group. We also drop the initial group (1782-1789) because there are very few variables that we can use to establish an adequate comparison group and our population data begins only in 1790.

The propensity score is the conditional probability of receiving a treatment given pre-treatment characteristics:<sup>19</sup>

$$p(X) \equiv \Pr\{D = 1 \mid X\} = E\{D \mid X\} \quad (3)$$

where  $p(X)$  is known as the propensity score.  $D=\{0,1\}$  is the indicator of the treatment effect and  $X$  is a vector of pre-treatment characteristics. Finally,  $E(.)$  is the expectations operator. Denoting  $Y_{1i}$  and  $Y_{0i}$  the variable of interest in city  $i$  (population growth in our case) with and without treatment

respectively, it can be shown that, as long as the propensity score  $p(X)$  is known, the *Average Effect of Treatment on the Treated (ATT)* can be estimated as:

$$\tau = E\{E\{Y_{1i} | D_i = 1, p(X_i)\} - E\{Y_{0i} | D_i = 0, p(X_i)\} | D_i = 1\} \quad (4)$$

The two conditions that need to be satisfied to derive (2) from (1) are the so-called *balancing* and *unconfoundness hypothesis*. The former states that observations with the same propensity score must have the same distribution of observable and unobservable characteristics and this should be independent of whether they receive the treatment. Formally, this condition is stated as:

$$D \perp X | p(X)$$

The second hypothesis, which cannot be tested (see Becker and Ichino, 2002), states that the assignment to treatment is unconfounded given the propensity score, i.e.

$$Y_1, Y_0 \perp D | p(X)$$

#### 4.3.1. A Raw Treated-Untreated Comparison

Before proceeding to the four matching estimators often used in the literature we perform a simple exercise that compares the growth rate of any city that ever had a bank (in the years after having received the “treatment” of a bank) with the average growth rate of all (untreated) cities that never had a bank. This is not a matching estimation because we are not matching observations on the basis of any variable. The objective here is to construct a raw measure of average growth of treated cities and untreated cities for purely comparative purposes.<sup>20</sup>

There are 266 cities that ever had a bank. The average difference in city growth between treated and untreated cities is positive in all decades. On average over the entire 1780-1870 period, cities that ever had a bank grew 11% faster than those that did not. This difference is statistically significant at the 1% level and is especially large in the 1780s, although only two cities had a bank on that decade.<sup>21</sup> If one excludes the first and last decades – those that have a significantly lower number of treated cities, the average difference is 9%.<sup>22</sup>

### 4.3.2. Propensity Score Matching

We now apply propensity score matching to our problem. The first step is to estimate the propensity score of our model. This can be done by estimating a probit or logit model of the probability that a given location receives the treatment of a bank in a given year. The model we specify is:

$$p_i = \alpha + \beta' X + \varepsilon_i$$

where  $p_i$  is the probability that a new bank locates in city  $i$  in a given decade. We run five logit regressions, one for each decade: the 1790s, the 1800s, the 1810s, the 1820s, and the 1830s. The vector  $X$  includes the explanatory variables as they appear in Table 11.

The inclusion of the past population growth as a control variable is important because it controls for the fact that bank location may potentially be driven by the growth of a given city in the recent past. As we conjectured in the introduction, in most cases past population growth does not perfectly predict current bank location. Interestingly, population growth between 1790 and 1830 predicts bank location in 1830. This may be the explanation for the lack of a significant effect of banking on city growth in that year: most of the correlation seems driven by reverse causation.<sup>13</sup>

The estimated propensity score is then used in a second stage to estimate the average treatment on the treated (ATT). We use each of the four widely used methods. The first is the Nearest Neighbor method, which consists of taking each treated city and searching for the untreated city with the closest propensity score.<sup>24</sup> Once a match is identified, differences in growth rates between the treated and untreated units are calculated. The reported ATT is the average of these differences. One problem with this method is that, since all treated cities are matched, the match is sometimes poor. The *Radius Matching* and *Kernel Matching* solve this by matching only the units that are within a given distance (radius matching) and by weighting the matches based on the distance

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between the treated and control units (kernel matching). Finally, the *stratification* method consists of dividing the range of variation of the propensity score in different intervals such that the treated and control cities have the same propensity score within each of these intervals. The ATT is calculated as an average of a weighted average of the ATTs of each block, with weights given by the distribution of treated units across blocks.<sup>25</sup>

Tables 12 through 16 display our ATT estimates. The average treatment effect of having some banking activity in the 1790s is positive but statistically insignificant (Table 12). However, the impact is positive and significant in most of the estimates for subsequent decades. The estimated effects range between 0.006 and 0.02 (or between 0.6 and 2 percentage points) and average about 1.1 percentage points. The estimated range is consistent with the OLS estimates reported earlier and indicates that nonrandomness in bank assignment to cities is not driving our results.

## **5. Conclusions**

While there is little doubt about the positive correlation between finance and growth, the question of causation remains unresolved. The literature uses several methods to establish a causal link: correlations between initial financial development and subsequent economic growth, exogenous regulatory change, horse races between competing explanations, firm-level data, instrumental variables and historical cases studies that now include even ancient Rome (Malmendier 2009). Herculean efforts to control for endogeneity and reverse causality notwithstanding, lingering skepticism over what appears to be an obvious “practical need for advanced contracting and financial development to realize growth opportunities” means that additional evidence remains valuable (Malmendier 2009, p. 1095).

Our paper contributes to this already large literature by investigating a previously unexplored finance-growth nexus, namely the connection between prior financial development (proxied by the

presence of modern commercial banking) and urban growth. Urbanization is incidental to broader economic development and can be used as a measure of economic modernization. Although our OLS estimates can be criticized as simple *post hoc ergo propter hoc* results, our panel, GMM and propensity score matching results are consistent with our hypothesis that a critical cause of urban growth is the availability of financial services.

In considering the northeastern United States in the nineteenth century, our study avoids some of the problems inherent in the current literature. First, unlike cross-country regressions, which are subject to small sample sizes and are difficult to interpret unless we accept that the finance-growth nexus is similar in advanced and less-developed countries, our focus on a single country holds constant the underlying legal structure. It does not hold constant the political factors emphasized by Haber et al (2008), and in fact political differences across states provide us with an exogenous source of identification, namely that the politics of bank incorporation differed markedly across states and across time. Second, we explore the finance-growth nexus close to the origin of modern economic development in North America. If banking mattered, and we believe it did, it was likely to matter most before alternative financial markets were fully formed and emergent business found foreign finance difficult to access.

## Appendix

### Two-step Heckman regressions

The first stage regression is:

$$\text{prob}(\text{bank}_{it}) = \alpha + \beta N_{it-s} + \gamma' X_{it} + \varepsilon_{it} \quad (5)$$

where  $N_{it-s}$  is the initial population of city  $i$ . In particular, we use the population of that city 20 years prior to the beginning of some local banking activity there.  $X$  includes a canal and customs dummy, state dummies, and the percentage of employment in different sectors. The second regression stage is:

$$n_{it} = \delta + \gamma \text{bankcap}_{it} + \lambda \phi + \varphi' X_{it} + u_{it} \quad (6)$$

$n_{it}$  is our variable of interest i.e. population growth in city  $i$  after the beginning of some banking activity. *Bankcap* is our measure of bank capitalization. The term  $\lambda \phi$  controls for the sample selection i.e. the fact that the sample of cities that receive the “treatment” of a local bank is not random. ( $\phi$  is the Mill’s ratio from stage 1). For this identification strategy to work we need that the initial population  $N_{it-s}$  is a valid instrument i.e. it affects the probability of a bank locating in a given city, but it does not affect directly subsequent city growth. This is shown in columns (1), (3), (5), and (7) of Table A1, which present the estimates of (5). Initial population is clearly associated with current bank location. In results not shown here we also show that, once one controls for bank location, initial population does not affect future population growth for the years 1810 and 1820, indicating that our instrument is valid in these two years. Unfortunately, the correlation remains positive in 1830 and 1838, invalidating the instrument in these two years. Columns (2), (4), (6), and (8) show the estimates of regression (6). The degree of bank capitalization enters with a significant positive sign in all specifications and significantly so in all of them except column (6) - the one that corresponds to the effect of banking in 1830 (however, as mentioned above the estimates of columns (6) and (8) must be taken with caution since the instruments are not valid there).

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## Footnotes

1. Atack et al (2009) document the contribution of the railroad to urbanization in the antebellum Midwest.
2. Bruchey (1967, p. 139) also argued that the hypothesis that financial institutions had negligible effects on urban growth was "valid in the negative sense that it is impossible to establish a direct connection between financial institutions and urban growth, whether relative or absolute." He offered neither theory nor empirical estimates to support his contention. He drew his conclusion after considering the experiences of only major seaport cities, all of which had banks in the period he considered.
3. Bleakley and Lin (2009) study how early colonial portage sites emerged as towns that persist to the present as major cities.
4. The growth accounting literature suggests that physical capital accumulation alone accounts for only a small fraction of long-term growth. If finance encourages growth, it does so by influencing resource allocation decisions that lead to productivity growth (Levine, 2005a; p. 6).
5. Mitchener and Wheelock (2010) do not find evidence that differing reliance on external finance matters in the US in the early 20<sup>th</sup> century. They find that banking market concentration positively influenced industry growth.
6. Hughes (1990), in fact, argued that industrialization *without* urbanization was rare; it was possible, but rare. Iron works sometimes emerged in hinterlands near critical ore deposits, but these were unusual and often short-lived manufactories.
7. Acts of incorporation specified minimum and maximum capital and, generally, restricted loans and note issues to some multiple of capital, so capital imposed a binding constraint in some instances. Increases in capital were legal only if the bank received an amendment to its original charter.

8. Data on canals comes from French (1860), [www.canalsocietynj.org](http://www.canalsocietynj.org) (accessed September 2009), [www.americancanal.org](http://www.americancanal.org) (accessed September 2009).
9. See [www.archives.gov/research/guide-fed-records/groups/036.html](http://www.archives.gov/research/guide-fed-records/groups/036.html) for a listing of customs house ports and their dates of operation.
10. Our proxy of urban population is the sum of the population of the 6414 cities included in the sample.
11. For the sake of brevity we omit the estimates of the state dummies.
12. Lindstrom and Sharpless (1978) argued that a city's industrial composition only partly determined its growth rate. Manufacturing mattered, but it was not solely responsible.
13. In order to make our estimates comparable to the cross-sectional ones we focus on city growth in the period 1810-1840 and so omit the years prior and posterior to this time interval.
14. We have also attempted to include the percentage of population in manufacturing, agriculture, and mining, for which we have some information for the years 1820 and 1840. Unfortunately, the number of observations is always too low for stata to perform the estimation. The more favorable specification in terms of available data is the one that only includes the percentage of population in manufacturing (or agriculture). This yields to 4,068 observations, which is still not enough to perform the estimation.
15. We use one lag of each variable as instruments. The results are similar using more than one lag for all or some of the variables.
16. See Arellano and Bover (1995).
17. One important difference between our exercise and most of the ones studied in the evaluation literature is that we have multiple treatment effects instead of just one. For instance, in Lalonde (1986) and Deheija and Wahba (1999), the goal is to estimate the average treatment effect of participation in the National Supported Work (NSW), a U.S. federally and privately funded program that aimed to provide work experience for individuals who faced economic and social problems prior to enrolment in the program. The program was implemented during the mid-1970s in ten sites across the United States and, for those assigned to the treatment group, the program guaranteed a job for 9 to 18 months. In their studies, the same NSW program was implemented to a given group of workers, whereas in our case the treatment was intrinsically different for each city (because no two banks were identical in size and characteristics) and, perhaps more importantly, only one city received a given treatment at in a given year.
18. We cluster cities by decade because groupings at a single year would yield treatment groups that contain too few observations. With 27 new banks, 1814 witnessed the largest number of new bank openings (see Figure 2), but the average number of new banks per year is about 5, which represents a very small group to estimate a treatment effect. In most cases there is only one treated city in a given year.
19. The following review of the propensity score is from Becker and Ichino (2002).
20. Consider a city in which a new bank was established at year  $t$ . We calculate its growth rate from year  $t$  to  $T$ , where  $T$  is the final year (here, it is 1870). We then compare this city's growth rate with the average (and median, to control for outliers) growth rates of all towns that never had a bank. Finally, we calculate the difference between all these pairs of growth rates and test whether it is statistically significant. Note that with this strategy we use the same city as

part of the control group in all treatments. For example, when we evaluate the effect of a bank treatment in 1790 we compare the growth of all the cities that had their first bank in 1790 with all those that never had a treatment in the 1790-1870 period. We then do the same for all cities that had a new bank in 1800. So in this case the control group is the same as before, although their average growth of population is now calculated for the period 1800-1870.

21. The results using the median as a summary statistic are very similar, indicating that they are not driven by outliers.
22. The number of control cities – those that never had a bank is 6147 in all decades.
23. We add as regressors the square of population in 1790 in the treatment groups 1, 3, and 4, the square of population in 1800 in groups 3 and 4, and the square of population in 1810 in the treatment group 3. We do so in order to satisfy the balancing hypothesis, i.e. to ensure that the observations in this treatment group that have a similar propensity score also have a similar distribution of observable and unobservable characteristics. See below and Becker and Ichino (2002) for more on this.
24. This method is normally implemented with replacement, i.e. a control unit can be used as a match for more than one treated unit.
25. See Becker and Ichino (2002) for a formal expression for each of these estimators.

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