Bank Networks, Credit Frictions, and Economic Growth: Evidence from U.S. Cross-State Banking Deregulation*

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Abstract

In the 1980s and 1990s, U.S. states deregulated restrictions on cross-state banking operations. These policies were oftentimes implemented on a pairwise basis: A state would allow another state to expand banking operations within its borders only if its banks had the option to expand in a reciprocal manner. In this paper, we leverage the timing and pairwise structure of these policy innovations to assess how bank deregulation affected geographic financial integration, banking operations, and economic growth. We have three main findings. First, bank holding companies (BHCs) quickly responded to effective deregulation events by increasing the number of interstate relationships by a large and significant amount. Second, the balance sheet of acquired banks expanded on average by ten percent. Third, we present state-level evidence that the real economic effects of deregulation occurred only to the extent that BHCs actually established interstate banking networks. Once connected to an interstate banking network, relative state-level real personal income growth rose by an annual rate of 1.2 percentage points, and relative auto sales growth rose by an annual rate of 1.9 percentage points.

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I Introduction

Financial deepening plays an important role in economic growth (c.f., Levine, 2005). A strong financial sector can more efficiently channel savings into productive investment, lower the cost of external financing, and limit the effect of volatile external shocks. Over the last half-century, the United States has experienced two interrelated phenomena: the financial sector has grown in importance as a share of GDP while at the same time the commercial banking sector experienced substantial consolidation—a small number financial institutions capturing increasingly larger shares of the market. Naturally, there is a question as to whether this form of financial deepening has promoted economic growth.

In the wake of the 2008 financial crisis, policymakers are reconsidering the costs and benefits of a large and loosely regulated financial sector dominated by a small number of major financial institutions.[1,2] While a large and concentrated financial sector may promote the efficient allocation of savings towards productive investment opportunities, and hence growth, large individual institutions may also create inefficiencies in the optimal provision of credit, which could negatively affect economic growth. For example, a reliance on standardized lending procedures may lead large financial institutions to fail to incorporate local market expertise in the pricing and provision of loans to households and firms. Or, banks with monopoly power may limit the supply of credit relative to its efficient level in order to increase profits.[3]

In this paper, we estimate the effects of geographic expansion in the commercial banking sector in the United States since the late 1970s. Our contributions are twofold. First, we bring in new data on bank linkages that differentiate between mergers (which dissolves the bank charter of one of the banks) and relationships (where both banks keep their charters, but one

[1] See Coure (2014) for a discussion of the policy response for Europe.

[2] Although after the time period studied in this paper, the resilience of larger banks during the economic downturn associated with the COVID-19 pandemic also has been noted.

[3] A classic study of competitive pressures related to unit-banking is Schwartz (1947); See Drechsler et al. (2017) for evidence of differential effects of monetary policy pass-thru depending upon degree of local monopolistic competition in commercial banking sector.

bank takes at least partial ownership of another). Second, we merge these bank linkages with call report data and, leveraging differential timing of interstate bank deregulation through the twentieth century, identify the effect that geographic expansion and financial integration had on bank balance sheets and the local economy.

In order to estimate the causal effect of bank geographic expansion and financial integration on economic growth, one must not only accurately measure a bank's geographic structure but also identify a plausibly exogenous shock to that geographic structure. In this paper we overcome both of these problems.

To remedy the first problem, measuring the geographic structure of a financial institution, we use novel data on all bank linkages in order to accurately measure the geographic expansion of Bank Holding Companies (BHCs) across space. This stands in contrast to the existing literature that has not fully taken into account the institutional details of how BHCs were able to operate across state borders following deregulation. While it was the case that state legislation allowed for the formation of interstate BHCs, oftentimes states continued to outlaw *interstate* branching. In other words, mergers and acquisitions of local banks by out-of-state banks, which entailed the *dissolution* of the local bank charter and the formation of an interstate branching system, were frequently not allowed. Instead, out-of-state BHCs were, as a result of deregulation, typically only able to purchase controlling interests in local BHCs as a way to establish ownership over local banks.

Addressing the second concern, our shock to bank structure comes from leveraging the period 1976-199. During this time, the United States banking system experienced a wave of deregulation that coincided with substantial bank consolidation and geographic expansion. While at the beginning of the century, banking was largely local, with bank branching almost strictly prohibited, by the end of the century there were essentially no restrictions on the ability of banks to operate across state lines. We use the time variation in when states chose to permit interstate bank expansion to study the effect of cross-state banking deregulation on linkages between BHCs. Previous work has argued these state deregulation events were

plausibly exogenous with respect to local economic conditions(see Jayaratne and Philip E. Strahan (1996)), allowing us to further study the effect of geographic financial integration on economic growth.

Our first result is that interstate deregulation had a sizable effect on the propensity of BHCs to expand across borders via banking relationships. To identify the causal effect of interstate banking deregulation, we use an event study design and show that the probability of expanding via BHC to BHC relationships increased by around four standard deviations following deregulation. On average, expanding BHcs increased the size of their balance sheets by an average of 10% of their assets; expanding BHCs typically increase the number of commercial banks controlled by around three. The lack of a pre-trend and the near instantaneous cross-state expansion of BHCs suggests that there were salient barriers to profitable interstate expansion prior to deregulation.[4]

Our second result is that the balance sheet of acquired commercial banks, as measured by total assets, expanded by approximately 10% following acquisition. To show this, we again use an event study specification, where the bank-specific event is the deregulation event interacted with being acquired as a result of an interstate banking expansion that occurred within one year of deregulation. Focusing on the year following deregulation allows us to restrict our attention to deregulation-induced acquisitions. We find no evidence that the balance sheets of acquired banks were growing differently relative to non-acquired banks around the time of acquisition. We find that this expansion was principally financed through new liabilities. Liabilities rise by 10%. We find that the ratio of equity to assets falls slightly, although this effect is neither economically or statistically significant. Finally, we revisit results from the literature on the aggregate growth effects of banking deregulation and find that incorporating the timing of when bank linkages occurred increases the estimated magnitude and significance of the effect of interstate deregulation on real economic activity. Relative to either de

[4]An early literature on financial integration in the U.S., notably Davis (1965), has emphasized the existence of national investment markets by the early twentieth century. It is therefore not obvious that relaxing restrictions on interstate bank operations mattered much for the allocation of credit across the U.S., since banks could easily borrow and lend in a national interbank market.

jure or de facto deregulation measures, our deregulation-induced, bank-network-expansion measure predicts a much larger rise in real personal income per capita growth, both at the quarterly and annual frequency, as well as auto sales growth at the annual frequency. These results are robust to controlling for intrastate branching deregulation.

To argue that such growth effects are related to the expansion of credit, we analyze the within-state growth effects of deregulation by industry. Corroborating our interpretation of the growth effects, we find that the effect of interstate bank linkages was more pronounced in sectors that relied more heavily on external financing. Using the measure of external financing as in Rajan and Zingales (1998), we find that log employment growth was nearly half a percentage point higher for sectors that relied more heavily on external financing following the establishment of a new cross-state bank linkage. Importantly, this specification allows us to include state-time fixed effects to control for unobserved measures of growth at the state level.

In a horse race between our measure of deregulation and other conventional measures of deregulation used in the previous literature, we find that our measure captures most of the variation in real personal income growth across states. In other words, the real effects on economic growth of bank deregulation occurred only to the extent that BHCs took advantage of the newly established opportunities to operate across state lines.

We contribute to the literature by directly incorporating novel data on banking relationships to study the effects of interstate banking deregulation. While we are not the first paper to investigate the effect of interstate banking deregulation in the U.S. on the real economy, to the best of our knowledge this paper is the first to emphasize that its effects operated principally through the formation of interstate bank networks. Moreover, to the best of our knowledge we are the first to identify the changes in bank balance sheets brought about through the formation of interstate banking networks. This emphasis is not only interesting in its own right since we also show that the estimated effect of interstate banking deregulation on the real economy is significantly larger when accounting for actual bank linkages

formed between states.

Literature Review

Our paper relates to three important sets of literature. First, our paper is related to how finance affects growth. The seminal contribution in this area includes King and Levine (1993). For a review see Levine (2005). In recent years, there has been a renewed interest in this question given the increasing size of the financial sector. For example, in a cross-country study Arcand et al. (2015) find limits to the effect of finance on growth. The IMF itself has cautioned about engaging in financial deepening at too quick a pace. (Sahay et al., 2015)

Second, our paper builds on the literature connecting credit supply shocks to real outcomes in financial markets. On the empirical side, credit supply shocks have been linked to real outcomes at least as far back as Peek and Rosengren (2000), who study the effect of the Japanese banking crisis on state-level economic activity. Similarly, Chodorow-Reich (2014) finds significant effects of credit supply shocks on firm-level employment by exploiting firms' connections to banks with higher exposure to the mortgage-backed security (MBS) market. Benmelech et al. (2011), using a series of tests including a version of the natural experiment we consider, find significant effects of credit supply shocks on labor markets. Calomiris et al. (2019) study interbank networks in the Great Depression, and find that banks were more likely to fail when their correspondent banks failed. The creation of the Fed helped reduce this network risks Carlson et al. (2018) study the National Banking Era; they find that higher bank competition in the form of lower barriers to entry is associated with a higher amount of credit extended, but such lending is more risky.

Finally, our paper relates to previous papers studying the impact of state financial deregulation in the United States. Jayaratne and Philip E. Strahan (1996), focusing on intrastate bank deregulation, find significant increases in per capita growth following reform as well as improvements in the quality of bank portfolios. Similarly, Beck et al. (2010) link intrastate and interstate reform to a tightening of income distributions by raising incomes below the

median, although they find that once controlling for intrastate reform, interstate reform is no longer significant. Analyzing both intrastate and interstate deregulation, Benmelech et al. (2011) find significant effects of reform on unemployment, lowering the the unemployment rate by between 0.45 and 1.14%. Leblebicioglu and Weinberger (2017) find significant declines in the labor share following interstate bank branching, with weaker results with respect to intrastate bank branching. In contrast to these studies, we explicitly utilize the pairwise structure of deregulation in our analysis.Kundu and Vats (2020) similarly rely upon the bilateral nature of deregulation in their analysis; they find that geographically diversified banks reallocate funds away from regions that experience negative, idiosyncratic shocks, with resulting positive effects on the destination's economy.

The outline of the paper is as follows. Section II provides institutional details regarding interstate banking deregulation. Section III describes the data. Section IV explores the effect of deregulation on banking linkages. Section VI explores the effect of deregulation on commercial bank balance sheets, and Section V explores the effect of deregulation on the real economy. Section VII concludes.

II Interstate Bank Deregulation - A Natural Experiment

For the majority of the 20th century, federal and state banking regulation in the United States constrained the extent to which banks were able to operate across state borders.[5] These constraints took the form of restrictions on interstate *branching*, that is the establishing of a new bank branch office in a state other than the state in which the head office is located, as well as restrictions on interstate *banking*, that is the acquisition or establishment of a separately chartered subsidiary bank in different state than the head bank's location.[6]

[5]The following summary of regulatory changes to cross-state banking state and federal policy draws heavily from Kane (1996) and Johnson and Rice (2008).

[6]These definitions are the ones provided by Johnson and Rice (2008).

As a result of the Banking Act of 1933, national banks were restricted in their branching activities by state-level regulation. And, since no states allowed cross-state branching, this entailed that interstate branching was prohibited (see Johnson and Rice, 2008). The multi-bank holding company (BHC) corporate structure was developed in the early 20th century in an attempt to circumvent state and federal prohibitions on interstate banking and branching. As Kane (1996) writes:

> Applicable state and federal statutes typically left room for a bank holding company to locate subsidiary banks in jurisdictions where the law would not permit its lead bank to operate a branch office. Although each bank owned by the holding company must be separately capitalized and must have its own board of directors, a holding company's management could integrate production and decision structures across its subsidiaries to mimic the hub-and-spokes flow of control that is characteristic of a branch network. (p. 144)

The Bank Holding Company Act of 1956 halted this practice. In particular, it "prohibited acquisition by a banking company of an out-of-state bank or banking company unless statutorily authorized by the state in which the target resides" (Johnson and Rice (2008), p 83). It was not until 1978 that the first state, Maine, allowed bank holding companies to acquire states within its borders.

Although Maine passed its law in 1978, it did not become effective until the fourth quarter of 1981 when Delaware similarly passed its own law allowing out of state BHCs to establish de novo banks subject to some restrictions. This delay in actual deregulation was because Maine's law required reciprocity before becoming effective. That is, only states that allowed Maine's BHCs to expand into their borders were allowed to expand into Maine's.[7]

In the years following 1978, one by one states progressively reduced restrictions on multi-state BHC geographic expansion and interstate branching. In the majority of instances,

[7]This characterization of when Maine's law becomes effective differs slightly from what is reported in Kozak and Sosyura (2015). They report that Maine's law became effective when New York passed its law in the first quarter of 1982.

allowances of interstate BHC expansion and interstate branching was implemented on a reciprocal basis.

Our research design, outlined below, exploits the temporal and spatial variation induced by the *effective* deregulation events as well the pairwise structure in which these events occurred — that is, the specific pair of states involved in each effective deregulation. At the micro level, we document that deregulation led to rapid interstate financial consolidation in the banking sector and an expansion of the balance sheet of newly acquired banks; at the macro level, we present evidence of faster economic growth in income and durable consumption, consistent with an expansion of both commercial and consumer credit. This interpretation is corroborated by a within-state analysis of the effects on growth by sector—sectors more reliant on external financing grew relatively more rapidly following deregulation.

III Data

III.1 Deregulation Indicators

We construct our measure of deregulation using the summary of regulatory changes in Amel (1993). We construct *effective* deregulation events between state i and state j by first constructing one-way deregulation indicators. Let $z_{j \leftarrow it}$ be an indicator variable for whether state i is allowed to expand banking operations into state j in quarter t (in our case, through BHC acquisitions). There are two ways that states deregulated.

First, some states deregulated unconditionally. That is, regardless of whether or not some state i allows BHCs in state j to enter, state j may allow banks in state i to enter. We denote such events with an asterisk $z^*_{j \leftarrow it}$. For example, Alaska in 1982 allowed BHCs to enter on a nonreciprocal basis. Thus, for BHCs in California, we write $z^*_{AK \leftarrow CA, 1982Q3} = 1$

Second, some states only allowed reciprocal entry. That is, while state j may have passed a law allowing state i to enter, the law will only became effective when state i allowed state j to enter. We denote these deregulation events by $\tilde{z}_{j \leftarrow it}$. Following the example from the

previous section, since Maine passed a law in 1978 on a reciprocal basis with Delaware, we have $\tilde{z}_{MA \leftarrow DE, 1978Q1} = 1$

Our definition of deregulation explicitly accounts for the reciprocal nature of interstate bank deregulation that occurred during the 1980s and 1990s. We define our effective deregulation measure $z_{j \leftarrow it} = \max\left\{z^*_{j \leftarrow it}, \tilde{z}_{jit}\tilde{z}_{ijt}\right\}$. In the case of Alaska and California, $z_{AK \leftarrow CA, 1982Q3} = 1$. In the case of Maine and Delaware, $z_{MA \leftarrow DE, t}$ will be equal to zero until the first quarter of 1981, when Delaware deregulated to allow Maine BHCs to enter.

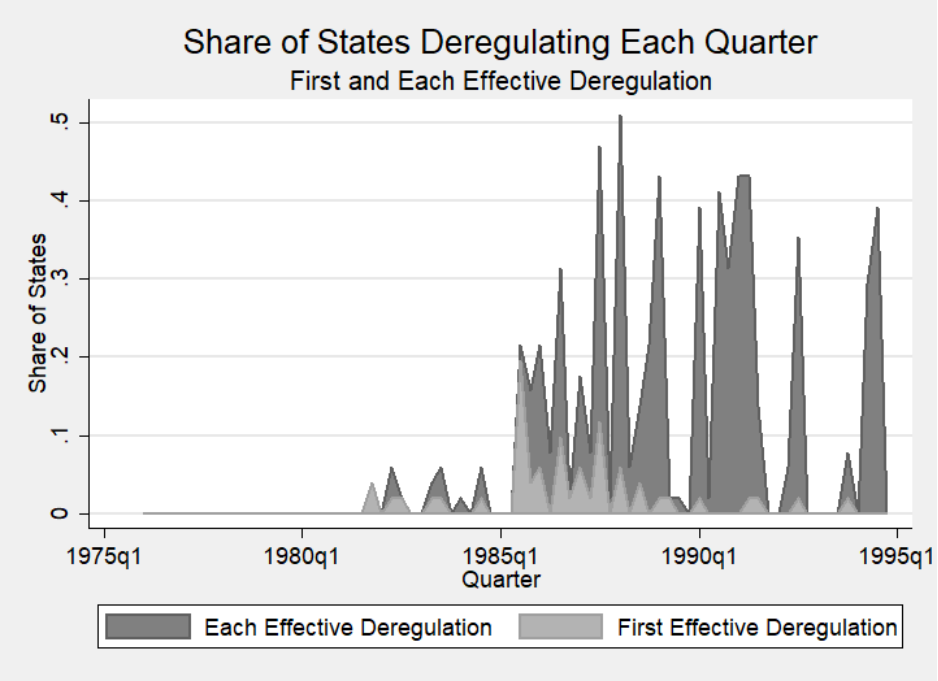
Our benchmark sample runs from 1976Q1-1994Q4. Consistent with the literature on banking deregulation, we drop South Dakota and Delaware from all regressions. We include the District of Columbia, giving us 49 observational units in total, although we refer to them as states for the remainder of the paper. There are 314 deregulation events in this sample. Figure 1 displays a histogram of the deregulation events over time. Most of the deregulation events are clustered in the mid to late 1980s, although some initial (though not effective) deregulation events occurred in the early 1980s.

Table 1 plots summary statistics for our deregulation events at the state-quarter level. Out of the 3,724 state-quarter observations (49 states over 68 quarters), roughly 8.4% of them include at least one effective deregulation. For each state in our sample, the average number of events is 7.51, although some states have as few as 1 and others have as many as 18. The average amount of time between events is a little less than a year, although it can be over four years in our sample. The average number of states added in each event is around 4.5, although in some cases states opened up to the entire country all at once.

There are two important observations to make about Table 1 and Figure 1. First, there is substantial heterogeneity across states in terms of how deregulation was implemented. While some states opened up to the rest of the country in one swift fashion, others took more time and opened up only gradually. Second, there is variation in when states chose to deregulate, even for the first time. The majority of states experienced their first effective deregulation in the mid 1980s; subsequent effective deregulation events occur at a relatively constant rate

from the mid-1980s until the passage of the Riegle-Neal Interstate Banking and Branching Efficiency Act of 1994, which removed all remaining barriers to interstate banking.

Figure 1: Share States with Effective Deregulation Events: Initial and Subsequent



This figure shows the share of states within each quarter that deregulated. The light gray color denotes the share of states that effectively deregulated for the first time, where an effective deregulation is defined as two states opening to each other. The dark gray color denotes for any subsequent effective deregulation. Source: Amel (1993).

Table 1: Deregulation Summary Statistics

	count	mean	sd	min	max
Deregulation Event	3724	0.08	0.278	0.00	1.00
Num Z per State	49	7.51	5.193	1.00	18.00
Years Since Last Z	266	0.91	0.841	0.25	4.25
Additional States Added	314	4.50	9.702	1.00	50.00

Summary statistics for deregulation events. Source: Amel (1993).

III.2 Bank-Level Data

We acquire bank-level data from two sources. First, we use data on institutional linkages from the National Information Center (NIC), an online repository of financial data collected by the Federal Reserve System for financial institutions. A financial institution here refers to an entity with a unique Replication Server System Database identification number (RSSDID). Financial institutions in the system include commercial banks, bank holding companies, and many other types of financial institutions. In this paper, we focus on bank holding companies (defined as having charter type 500) and commercial banks (defined as having charter type 200).[8]

There are three tables maintained by the NIC. The first table is a *relationships* table, which provides a history of ownership changes between financial institutions. Second, there is a *transformations* table, which provides a history of mergers, acquisitions, and failures. The underlying data for these reports comes from the report of changes in organizational structure, or Form FR Y-10. While Form FR Y-10 has only technically been issued since 2001, the report of changes in organizational structure has been filled out in some capacity since the passage of the Bank Holding Company Act of 1956. Finally, the NIC maintains an *attributes* table, which documents institutional characteristics such as institution type or location. This last table only provides information for closed or failed institutions at the last observable date, and the most recent information for open and active institutions.

The difference between transformations and relationships is important for understanding the interstate banking deregulation we study in this paper. Transformations refer to cases in which at least one banking charter is discontinued. This may occur because of an acquisition or merger by a parent bank, or because a bank has failed and its assets are acquired

[8]BHCs and commercial banks comprise approximately three-quarters of the parents in our sample and approximately 15% of the offspring in our sample. We classify banks based on the charter type rather than the entity type. This is because the charter type determines the legal status of a financial institution, whereas the entity type is derived by the NIC from other observable characteristics. Note that charter type 720, which represents "other non-depository institutions", accounts for 23% of the parents in our sample and 70% of the offspring in our sample. We choose not to include them in our benchmark analysis as it is not clear exactly what institutions they represent.

by another institution (with or without government assistance). However, the structure of banking organizations frequently change without the dissolution of a banking charter. For example, banks must report ownership changes of more than 5% of a class of voting securities or if there is a change in the type of ownership of a banking institution. We trim the data of relationships and transformations that are not expected to be affected by deregulation, such as the acquisition of failing financial institutions, using information provided in the NIC tables.

We construct our sample of banking relationships by merging the relationships database with the attributes database. We keep only relationship changes that result in an actual change in control, are regulated by banking statues, and are a direct relationship. We exclude relationships that include a foreign office. Finally, we only consider the initial relationship record. As before, we include the District of Columbia and drop Delaware and South Dakota.

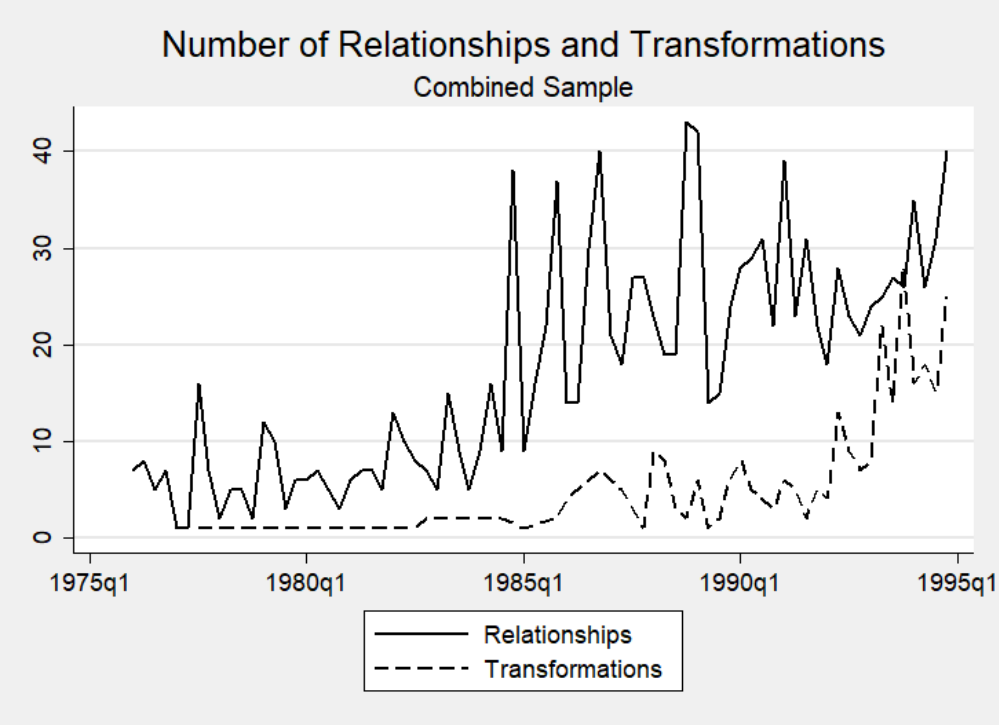
Figure 2 plots a time series of the number of interstate transformations and relationships over time.[9] The number of relationships is consistently higher than the number of transformations. The co-movement between the two series is moderately high, at around 68%, but there are clear differences. While both series rise significantly when deregulation first begins, the relationship series starts increasing around 1984, while the transformation series only begins rising substantially in the early 1990s.

We use the state in which financial institutions are physically located to define each bank's location. We then take the count of the number of one-way interstate relationships. One-way in this context refers to the *parent* being located in state i and the *offspring* located in state j. Thus, it is not true that relationships from i to j are equal to relationships from j to i.

There are three instances where there are 10 or more relationships formed in a given pair-quarter. All of these involve Ohio banks forming relationships with New York banks.

[9]Interstate relationships are those involving entities headquartered in two distinct states, as described below. We use historical location information for BHCs that we received through a FOIA request for historical NIS attribute data for BHCs.

Figure 2: Number of Relationships and Transformations



This figure shows the number of changes in structure of financial institutions, where a financial institution is defined as having a unique charter. Relationships denote a change in the ownership of one financial institution without a dissolution of a charter. Transformations include the dissolution of one of the charters. Source: FFIEC

This is overwhelmingly the result of JP Morgan, which is located in Ohio. All our results are robust to dropping JP Morgan from the sample.

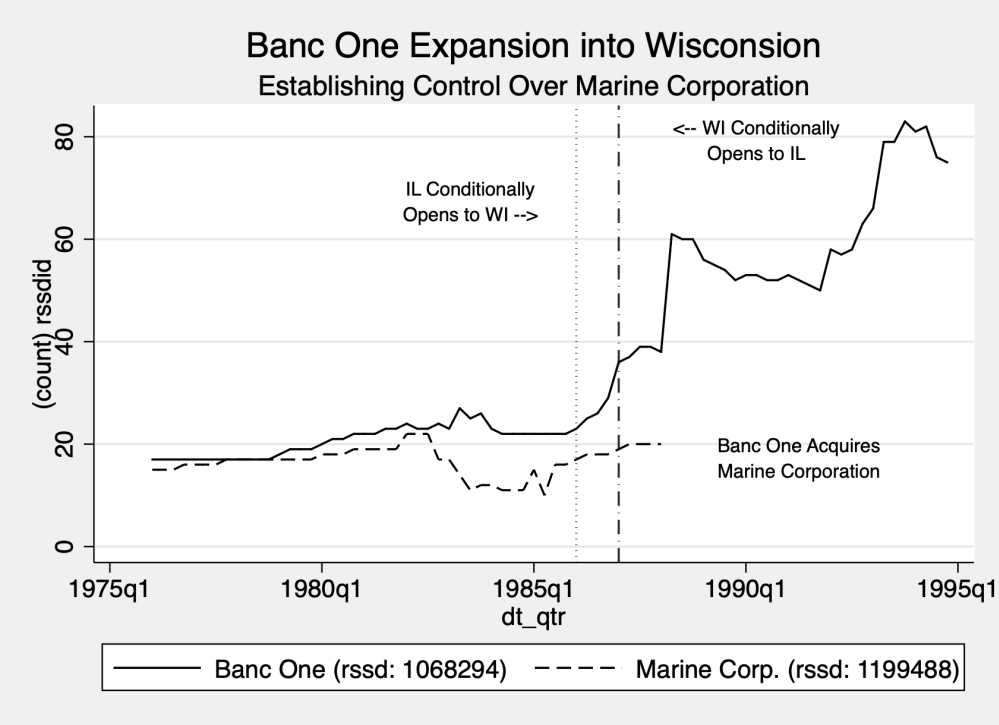
Our financial variables on balance sheet data at the bank level come from the bank Call Reports.[10] The call report data is at the Commercial Bank RSSD ID level. Each commercial bank reports a top-level BHC RSSD ID as well, which also varies at the quarterly frequency. The top-level BHC RSSD ID refers to the BHC for whom there is a majority ownership share, as evidenced by the following example.

As an example of what we mean by "relationships" and "transformations," and the importance of distinguishing between them, consider the relationship established by the Illinois bank Banc One and the Wisconsin bank Marine Corporation, as shown in Figure 3. This relationship was established in April 1988, with a 100% equity ownership of Marine Corporation by Banc One, at which point banks in the Call Report previously owned by Marine Corporation are recorded as being owned by Banc One. It would not be until 1996 that Marine Corporation (by then known as Banc One Wisconsin Corporation) was officially acquired by Banc One and its charter discontinued. By only looking at official mergers and acquisitions, this link would be erroneously delayed by almost eight years. To the extent that eventual acquisitions of banking charters are mainly due to reasons uncorrelated with interstate deregulation, one would not find any effect of deregulation on mergers and acquisitions.

This figure also illustrates the distinction between initial deregulation and effective deregulation. In 1986 Illinois passed legislation allowing BHCs in Wisconsin to expand within its borders, but again only on a reciprocal basis. It would not be for another year before Wisconsin passed similar legislation, deregulating relative to Illinois on a reciprocal basis. Within a year of this effective deregulation the Banc One forms the interstate relationship.

[10]We standardize the Call Report data using the methodology used in Drechsler et al. (2017). These authors generously provide the SAS code for querying these data through WRDS: `http://pages.stern.nyu.edu/~pschnabl/data/data_callreport.htm`.

Figure 3: Banc One Example



This figure shows an example from Banc One expansion for the difference between relationships and transformations. Source: Amel (1993), FFIEC, Authors' calculations.

III.3 State-Level Data

We obtain annual quarterly data on nominal personal income from the Bureau of Economic Analysis (BEA)'s regional economic accounts. We also use annual population data from the BEA. We deflate nominal personal income using the annual CPI from the Bureau of Labor Statistics (BLS). We also obtain auto sales at the annual frequency from the US Department of Transportation.

IV The Effects of Deregulation on Interstate Bank Linkages

In this section, we evaluate whether and how interstate banking deregulation, which relaxed restrictions on interstate BHC linkages, affected the financial network of banking relationships across the United States. In what follows, we present evidence that BHCs responded quickly to deregulation by expanding across state borders and acquiring out-of-state BHCs. This suggests that, from the perspective of expanding banks, legal restrictions in place prior to deregulation constrained banks from operating at optimal scale and scope.

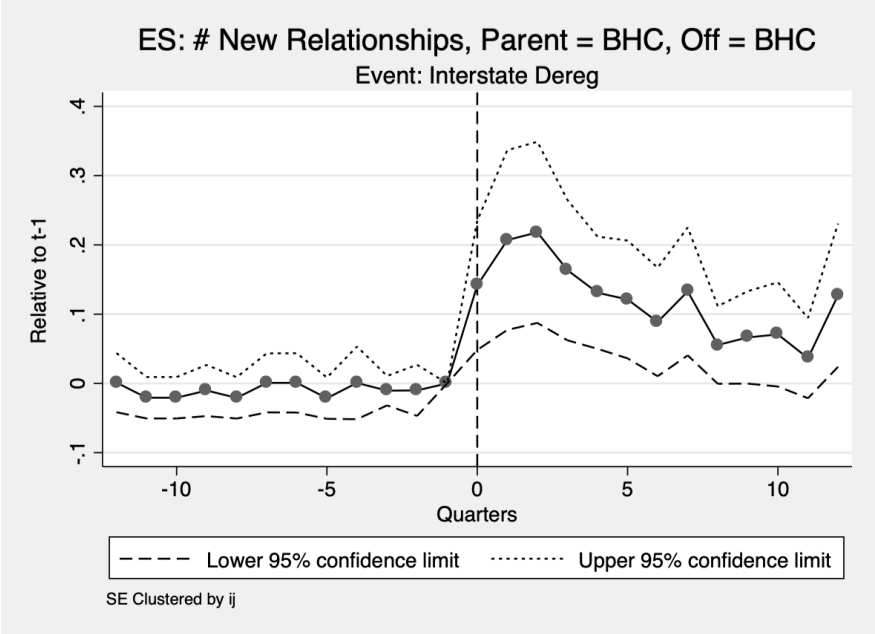
We use an event study approach in order to estimate the effect of state deregulation on banking relationships. Our main specification takes the following form.

$$Num\_Rel_{ijt} = \alpha_{ij} + \alpha_t + \sum_{c=-\underline{C}}^{\overline{C}} \beta_c \left(\Delta z_{ijt+c}\right) + \underline{D}_{ijt} + \overline{D}_{ijt} + \varepsilon_{ijt} \tag{1}$$

The outcome of interest, Num_Rel_{ijt}, represents the number of *relationships* between financial institutions in state i and financial institutions in state j in quarter t. Our main independent variable of interest is indicated by the change in z_{ijt}, which is equal to 1 in the quarter of effective deregulation.

We are interested in the dynamic response of relationships around each deregulation date. In what follows, we therefore plot the sequence of β_c, which together represent this

Figure 4: Number of Relationships



This figure shows event studies from estimating Equation (1). Standard errors are clustered by state pair. Source: Amel (1993), FFIEC

dynamic effect before and after deregulation. Because the model is saturated with event time dummies, we normalize our results to one quarter prior to the acquisition event. To do so, we re-scale the results by taking the ratio of the β_c's relative to β_{-1}.[11]. We include pair and time fixed effects α_{ij} and α_t respectively. In all specifications, we cluster our standard errors at the state-pair level.

Figure 4 plots the estimated β_c's when estimating the sample using the number of new relationships between BHCs as the outcome of interest. We highlight three important results. First, note that there is no significant pre-trend prior to deregulation. This lack of pre-trend suggests that the parallel trends assumption, critical to all event study specifications, holds. One may be surprised to see that this pre-trend is not identically zero. After all, didn't regulation explicitly prohibit *all* interstate BHC relationships? In practice, this is

[11] Although our data is strongly balanced over the sample, we are not balanced in event time. To partially remedy this problem, we include dummy variables that bin up the endpoints $\underline{D}_{ijt} = \mathbf{1}\left[t < \Delta z_{ijt+\overline{C}}\right]$ and $\overline{D}_{ijt} = \mathbf{1}\left[t < \Delta z_{ijt+\overline{C}}\right]$. These variables in effect control for "long-run" effects of interstate deregulation.

not the case. Various legal loopholes existed which allowed for cross-state expansion and acquisition (e.g. an out-of-state BHC was permitted, in some instances, to acquire in-state failing BHCs) means that the pre-trend is not identically zero. The flat pre-trend suggests that these loopholes were not systematically exploited by BHCs prior to an effective bilateral deregulation.

The second result is that the number of new relationships rises on impact by around 0.15 relationships, with a peak of around 0.2 relationships after a couple of quarters. This is a large change. The unconditional mean in the number of relationships per quarter during this period is only around 0.002, with a standard deviation of 0.06. This significant rise in BHC to BHC relationships is evidence that the policy change had real effects on the banking network.

The third result is that the increase in the propensity of forming interstate banking networks is not permanent. The effect lasts for around two years before declining to be insignificantly different from zero. The bulk of the direct effects of interstate bank relationship formation takes place in the couple of years following deregulation.

In the appendix, we show two additional results. First, our focus on relationships is to highlight that the way in which BHCs expanded was through ownership changes, not by dissolving charters. When we re-run our event study using transformations only, we find no significant effect of deregulation. Second, our focus on BHC to BHC relationships is because the law specifically targeted such interactions. In the data, we confirm that the number of relationships involving a BHC parent and commercial bank offspring does not respond to the effective deregulation event.

Next, we investigate whether or not these new linkages resulted in significant changes to the total size of BHC balance sheets. To do so, we link call report data to the NIC relationship data using each commercial bank's top-level BHC identification number.[12] In

[12]Our call report data only reports the top-level BHC. Therefore, we cannot directly observe the assets of offspring BHCs that are acquired, as they essentially disappear due to all the commercial banks changing top-level BHCs. We could construct a synthetic offspring by tracking the commercial bank assets of offspring BHCs over time. In lieu of this approach, we instead look at commercial bank balance sheets directly in the

theory, we should see significant changes around the time of deregulation, assuming the acquired banks are large relative to consolidated balance sheet of the acquiring BHC.

We use the same event study specification in Equation (1), with two changes. First, we replace the left-hand side with balance sheet indicators. Second, in order to isolate acquisitions that resulted from an effective deregulation event, we focus our attention on interstate BHC acquisitions that occur between the quarter of the deregulation and 4 quarters following. Formally, the relationship event for BHC B at time t, $relationship_{Bt}$:

$$relationship_{Bt} = \max\left\{\Delta z_{ijt-k}\right\}_{k=0}^{4} \times \mathbf{1}\left(num\_rel_{Bijt} > 0\right)$$

Thus, $relationship_{Bt}$ captures only those relationships that occur following deregulation. Our results in Figure (4) suggest that these relationships can be thought of as being caused by deregulation.

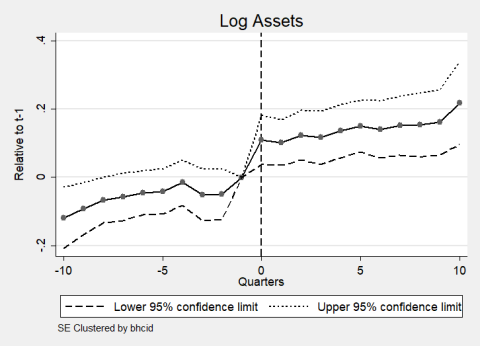
Figure (5a) plots the results using log total assets as the outcome variable. Although there is a slight pre-trend over a year prior to the event, we see assets jump by 10% and then slowly grow over time.[13] While the initial jump cannot be completely due to the new relationship due to the possibility of a pre-trend, it is effectively an upper bound on the size of the average new commercial bank relative to existing assets. That assets continue to grow in the long-run suggests that either the existing commercial banks or the newly acquired commercial banks grew faster relative to banks not involved in such cross-state BHC relationships. We investigate this further in the next section and find that newly acquired banks expanded their balance sheets considerably following acquisition.

Figure (5b) plots the event study for the number of commercial banks. We emphasize two things. First, there does not appear to be an indication of a pre-trend like in the balance

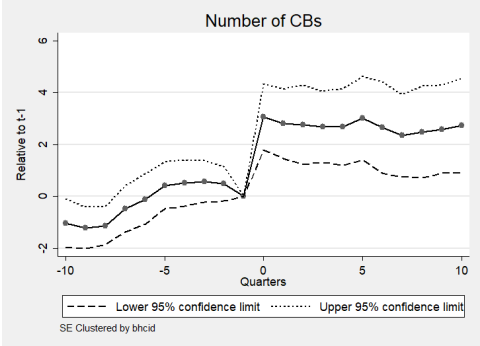
next section.

[13]In the appendix, we break down total assets into residential loans and commercial and industrial loans, respectively. In each of these cases, a similar pattern appears, whereby the relationship (mechanically) induces a sharp rise in assets at the time of the event before rising slowly over time. It is noteworthy that both types of assets rise by the same amount of 10%. This suggests that BHCs were not systemically acquiring commercial banks that may have specialized in one type of lending.

Figure 5: Effect of BHC to BHC Relationships on BHC Balance Sheets



(a) Total Assets



(b) Number of Banks

This figure shows event studies from estimating Equation (1), using log assets (left panel) and number of commercial banks owned by the BHC (right panel). Standard errors are clustered by BHC. Source: Amel (1993), FFIEC

sheet variables. This result suggests that BHCs were not already differentially expanding along the extensive margin by acquiring banks within their state or in other states that they already had access to at the time of deregulation. Second, the rise in the number of CBs is around 3, so that BHCs were on average forming relationships with BHCs that controlled multiple CBs.

It is important to emphasize the point of these figures. We use these figures to see how important the typical interstate relationship was relative to the dynamics of the parent BHCs that chose to deregulate. While these figures cannot show whether or not BHCs that deregulated were "bigger" on average (due to the inclusion of BHC fixed effects), they can show whether or not expanding BHCs were growing differentially prior to deregulation The pre-trends in the event studies suggest that in the year immediately leading up to expansion, there was little evidence of such differential growth.[14] At longer leads, there is suggestive evidence that BHCs that eventually established cross-state banking relationships as a result of deregulation were growing somewhat faster than their peer BHCs. This differential growth, however, cannot explain the instantaneous, discrete, and large change that occurred around

[14]Even if observable characteristics, such as assets, are not growing disproportionately at BHCs that choose to expand, unobservable characteristics such as political connections or "entrepreneurial spirit" may make BHCs that expand fundamentally different. We cannot test the presence of such differences.

the time of deregulation.

V The Effects of Deregulation on Acquired Banks' Balance Sheets

Having established that interstate banking deregulation resulted in a significant rise in cross-state banking linkages, we now investigate how these new linkages altered the balance sheets of acquired commercial banks by using data from the call reports. The purpose here is to illustrate that the establishment of a multi-state banking network altered the local provision of credit, as reflected in the balance sheet of acquired banks. We find that the balance sheet of acquired banks expanded substantially and permanently following acquisition by an out-of-state BHC.[15]

To isolate acquisitions that resulted from an effective deregulation event, we again focus our attention on interstate BHC acquisitions that occur between the quarter of the deregulation and 4 quarters following. Formally, the acquisition event $acquisition_{bt}$ for commercial bank b at time t can be defined as

$$acquisition_{bt} = \max\left\{\Delta z_{ijt-k}\right\}_{k=0}^{4} \times \mathbf{1}\left(num\_rel_{B(b)ijt} > 0\right)$$

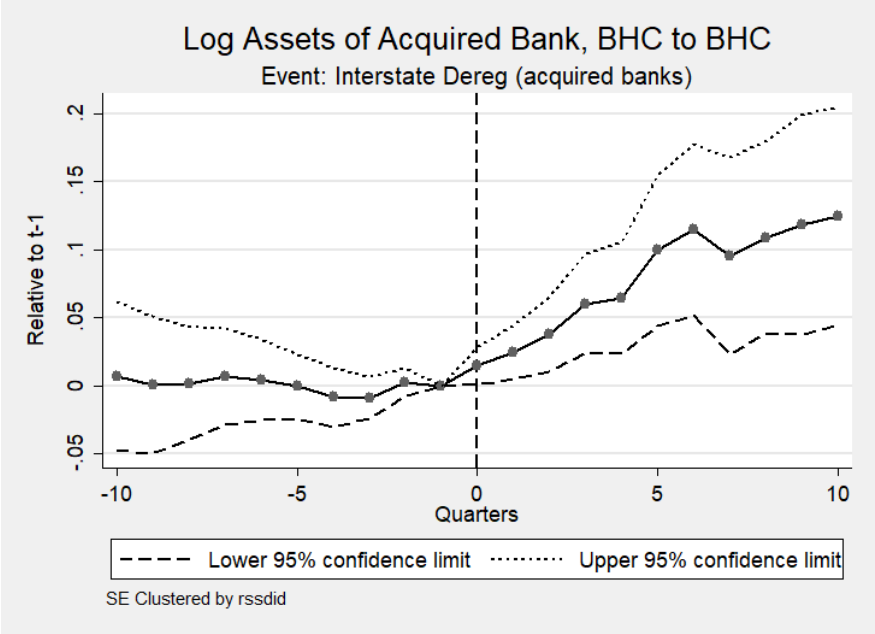
where $\Delta z_{i,j,t}$ is defined as before. When a deregulation event occurred in the last four quarters, the first term is equal to one. $num\_rel_{B(b)ijt}$ is equal to 1 if and only if BHC B, which manages commercial bank b, is involved in a relationship between states i and j at time t.[16]

We then estimate event studies of bank balance sheet variables around the time of the

[15] Due to the nature of the call report data, we use the term acquisition here to refer specifically to commercial banks whose top-level BHC changed.

[16] We impose an additional restriction which is to only look at acquired banks that does not acquire another commercial bank in the 10 quarters prior to or following acquisition. In preliminary and unreported results, it appears that some BHCs established a consolidated branching network within the states to which they expanded soon after expansion. Including these banks in the regression would spuriously inflate the balance sheet response even if, on a consolidated basis, there is no change in the balance sheet.

Figure 6: Total Assets of Acquired Banks



This figure shows event studies from estimating Equation (2), using log assets of the acquired bank as the outcome variable. Standard errors clustered by bank. Source: Amel (1993), FFIEC

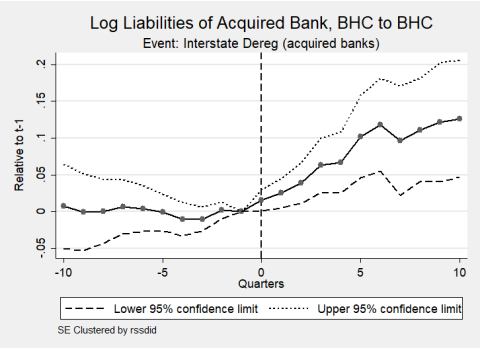
acquisition. Specifically, the estimating equation for some balance sheet outcome $a_{b,t}$ is

$$a_{b,t} = \alpha_b + \alpha_t + \sum_{c=-\underline{C}}^{\overline{C}} \beta_c \times acquisition_{b,t+c} + \Gamma' \mathbf{X}_{bt} + \varepsilon_{bt} \tag{2}$$

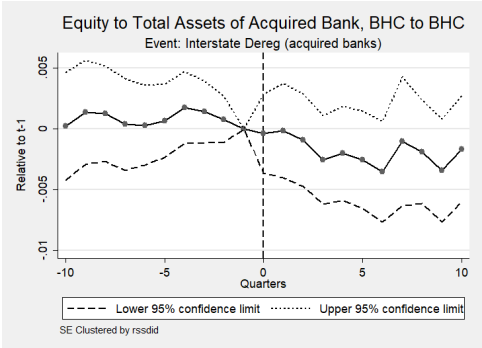
Our first outcome of interest is the log of total assets of the banks acquired by an interstate BHC acquisition. To the extent that acquired banks continued operating as usual (with only a change in ownership), then we should not see any change in the overall size of the balance sheet. Figure 6 plots the normalized coefficients ten quarters prior to and ten quarters following the time of acquisition. Again, the vertical dashed line indicates the time of acquisition.

We find that total assets rise by around 10% for acquired banks, relative to unacquired banks, in the eight quarters following acquisition. We see no evidence of a pre-trend, which implies that the balance sheets of acquired banks were not differentially changing prior to acquisition.

Figure 7: Effect of BHC to BHC Relationships on BHC Balance Sheets



(a) Liabilities



(b) Equity Ratio

This figure shows event studies from estimating Equation (2), using log liabilities of the acquired bank (left panel) and the equity to asset ratio (right-panel) as the outcome variables. Standard errors clustered by bank. Source: Amel (1993), FFIEC

Did banks fund this balance sheet expansion primarily via an expansion in liabilities or through an equity-injection, as might occur from acquiring banks? We answer this question by investigating both total liabilities and the equity to asset ratio. Figure 7a shows the event study results for total log liabilities. We find that acquired banks' liabilities rose by similar amounts as total assets, on the order of around 10%. In Figure 7b we see that the ratio of equity to assets declines slightly for acquired banks, although the effect is neither economically nor statistically significant. Taken together, these results imply that acquired banks funded the increase in their assets by increasing liabilities and equity proportionally.

In the appendix, we investigate the sub-components of both the asset and liability sides of the balance sheets. Although the results are a bit noisier, the evidence suggests that acquired banks were actively re-balancing their balance sheets prior to acquisition, tilting away from short-term assets towards longer-term assets such as commercial and industrial loans. Since the size of the balance sheet of acquired banks was flat prior to acquisition, this suggests that these banks—despite having profitable investment opportunities—were nevertheless size-constrained. On the liabilities side of the balance sheet, much of the new funding for acquired banks appears to come from new deposits in the long-run. Unfortunately, however, these results are in some cases quite noisy, and data limitations prevent us from

breaking down demand deposits into finer detail (e.g. interbank deposits vs. retail deposits) to better understand the underlying mechanism. We leave this investigation for future research.

VI The Effects of Deregulation on the Real Economy

Our analysis thus far has highlighted that interstate banking deregulation led to the rapid formation of multi-state banking networks. In this section, we revisit results from the literature on interstate deregulation to investigate whether or not such interstate BHC linkages resulted in changes to the real economy. We first replicate results from the literature that, on the face of it, deregulation resulted in no statistically discernible effect on real output growth. We show that, in these specifications, including our measure of deregulation—which explicitly accounts for banking linkages between states—results in statistically significant and economically large positive effects on output growth. We then investigate whether industries with a relatively higher reliance on external financing benefited disproportionately when a state was brought into a multi-state banking network. This specification permits the inclusion of state-time fixed effects to control for unobserved determinants of state-level growth, thereby relying upon weaker assumptions than in the main analysis.

To facilitate comparison with previous research, we begin by following the same difference in difference approach to estimating the effects of banking deregulation on real variables as in the literature. We estimate variants of the following regression

$$Y_{jt} = \alpha_j + \alpha_t + \beta z_{jt} + \varepsilon_{jt} \tag{3}$$

Y_{jt} represents a real economic outcome variable. We focus on the state level growth rate in real per capita income and auto sales. z_{jt}, broadly, is an indicator variable equal to one when a state is deregulated and zero prior to deregulation. We include state and time fixed effects α_i and α_t. The inclusion of time fixed effects controls for aggregate changes in the real economy, such as national business cycles. To the extent that states are differentially

sensitive to national business cycles, and such sensitivity drives the decision to deregulate when output is low (high), then our estimates would be biased against (towards) finding an effect.

We focus on three different measures of z_{jt}. First, we construct a variable that represents the first time a state opens up to another state, regardless of whether or not it is an effective deregulation. We denote this measure as "traditional deregulation" or z_{jt}^{tra}. Using our notation, this can be written as $z_{jt}^{tra} = \max\{z_{j\leftarrow it}^{*}, \tilde{z}_{j\leftarrow it}\}_i$. This is the deregulation event used in, for example, Beck et al. (2010). In some cases, such as Maine, the deregulation year used is prior to an effective deregulation event.[17]

Second, we use the effective deregulation date whereby states that open up conditionally are reciprocated. We denote this measure as "effective deregulation." Using our notation above, this can be written as $z_{jt}^{eff} = \max\{z_{j\leftarrow it}\}_i$. This is the deregulation event used in, for example, Michalski and Ors (2014). In the case of Maine, this would set the deregulation date to be later, in 1982.

Finally, we construct a new deregulation event indicator. We denote this variable z^{link} for "linked deregulation." We use the date of the first cross-state banking relationship that occurs at any point after an effective deregulation at the state-level.

An example will make the differences between these three regulation measures clear. On July 1, 1987 Oklahoma deregulated on a non-reciprocal basis. If this deregulation event led BHCs to expand into Oklahoma across state borders, than those states would have been effectively treated by the policy change despite not explicitly passing legislation allowing interstate BHC expansion. Continuing the example, on July 2, 1987 First Dodge City Bancshares inc., a BHC headquartered in Kansas, acquired Metro Bancshares inc., a BHC located in Oklahoma. As a result of the Oklahoma's deregulation, the banking system in Kansas was affected even though it would not explicitly deregulate until 1992, five years later.[18] This

[17] Beck et al. (2010) list the date of Maine's deregulation as 1978. This was when they passed the reciprocal law. It was not until 1982 that another state reciprocated.

[18] In this example, the Metro Bancshares BHC in Oklahoma was created a few months prior to the deregulation, in May 1987, presumably in anticipation of being acquired by First Dodge City Bancshares in Kansas.

Table 2: State-Level Summary Statistics - Annual

Variable	Obs	Mean	Std. Dev.	Min	Max
Traditional	1029	.456	.498	0	1
Effective	1029	.45	.498	0	1
Linked	1029	.431	.495	0	1
Auto Sales Growth	1029	.892	6.52	-47.136	37.873
Real Personal Income per Capita Growth	1029	1.17	2.25	-10.073	7.336

This table reports summary statistics for the state-time analysis. "Traditional" denotes deregulation dates for which a state opens up to another state regardless of whether the other state reciprocates. "Effective" denotes deregulation dates for when a state opens up to another state and that state reciprocates (or unconditional deregulation where reciprocity is not required). "Linked" denotes dates of the first pairwise BHC acquisition following an effective deregulation. Source: Amel (1993), BEA, DOT

second type of deregulation event is also captured by our variable.

Table 2 shows summary statistics for the annual sample. On average, states are traditionally and effectively connected around 45% of the sample. For the linked measure, which is lower, connections only occur around 40% of the sample. Auto sales growth experiences large variation around a mean of around 0.9 percentage points. Real personal income per capita growth experiences a slighly lower level of volatility around a mean of around 1.2 percentage points.

Table 3 reports results of estimating Equation 3.[19] Column (1) directly replicates the specification in Philip E Strahan (2003). Our point estimates are very close to what he obtains, and in particular the measure of interstate deregulation has an insignificant (albeit positive) effect. In column (2), we use the effective measure of deregulation that accounts for the pairwise structure of the policy changes. Again, we find a noisy positive estimate. Finally, in column (3), we show that our measure of linked deregulation is large and highly significant. Deregulation which allowed the formation of interstate banking networks appears to have had an effect on real economic growth to the extent that banks actually formed cross-state networks.

As we saw above in the bank balance sheet event studies, there is some evidence of anticipatory responses by acquired banks around the time of actual acquisition.

[19]We also extend the sample to 1976-1996 and drop the periods of deregulation, in line with Philip E Strahan (2003). The differences in the number of observations therefore comes from which years we drop based on the different measures of deregulation.

Previous work, such as Jayaratne and Philip E. Strahan (1996), has argued that the timing of interstate bank deregulation was orthogonal to other determinants of economic growth. While this may be true, in order for the point estimate in column (3) to have a causal interpretation, we need an additional assumption, since the decision of whether and when to acquire out-of-state BHCs (once allowable) is endogenous. That assumption is that the timing of when a link is established following deregulation is as good as random.

However, when factoring in the bank's decision of whether to actually expand across borders, there is an obvious concern that BHCs only chose to subsequently acquire out-of-state BHCs when expectations of future economic growth were high. Alternatively, BHCs may have only expanded when economic growth in their state was or was expected to be relatively low, since expansion broadens the set of investment opportunities.

To account for this concern, we estimate an instrumental variables version of Equation 3, instrumenting our linked measure of deregulation with the effective measure. Under the assumption that this effective measure is orthogonal to other determinants of economic growth (as argued by Jayaratne and Philip E. Strahan (1996)), then this should provide a causal estimate of the effect on growth of being brought into an interstate banking network. The coefficient estimate in column (4) is of a similar magnitude as column (3), providing additional support for our causal interpretation. We caution that while the IV estimator is consistent, we lack the precision to conclusively reject the null of no effect.

Columns (5)-(8) look at household durable consumption. We use the annual growth rate of private auto registrations as our measure of durable consumption due to its availability.[20] We see a similar pattern as with the log growth of real personal income. The traditional and effective deregulation measures, while positive, are insignificant. In contrast, our linked measure has a larger and more precisely estimated effect. Moreover, to the best of our knowledge, this is the first evidence of the effects of cross-state bank deregulation on durables

[20]Hausman (2016) uses auto sales in his study of the effects of the 1936 Veterans Bonus. Hausman argues that auto sales are a good indicator of consumption because they are well-measured (due to state laws mandating registration) and because they are likely to be recorded within the state they are actually purchased

Table 3: State-Level Event Study - Annual

	$\Delta \ln Y$	$\Delta \ln Y$	$\Delta \ln Y$	$\Delta \ln Y$	$\Delta \ln Auto$	$\Delta \ln Auto$	$\Delta \ln Auto$	$\Delta \ln Auto$
Traditional	0.60				0.91			
	(0.480)				(0.928)			
Effective		0.71				1.42		
		(0.521)				(1.018)		
Linked			1.21***	1.26			1.92***	2.55
			(0.352)	(0.946)			(0.692)	(1.805)
Branching	0.56**	0.56**	0.58**	0.58**	0.68	0.67	0.72	0.71
	(0.271)	(0.273)	(0.278)	(0.274)	(0.508)	(0.498)	(0.491)	(0.492)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs	945	945	945	945	945	945	945	945
$\mathcal{R}^2$	0.44	0.44	0.44	0.01	0.24	0.24	0.24	-0.02
KP F Stat				51.58				51.58

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

This table reports results from Estimating (3). Source: Amel (1993), BEA, DOT

consumption.

We next look at the effect of each measures of deregulation at the quarterly frequency. We again use the annualized growth of real personal income per capita. Table 4 displays summary statistics for our three measures of deregulation as well as real personal income per capita growth over the course of our sample. Recall that we have 49 states between 1976Q1 and 1994Q4, giving a total of 3,724 observations. Real personal income per capita annual growth averages around 1.15%, with a minimum of -10.8% (Alaska in 1977Q3) and a maximum of 8.8% (West Virginia in 1979Q1).[21]

Table 5 provides results from estimating Equation 3 via OLS. Column (1) estimates the quarterly effect of interstate deregulation using the traditional measure. The estimate is again small and insignificant, at a similar magnitude of around 0.56 percentage points.

[21] As in the annual regressions, we drop the quarter of deregulation as well as two quarters before and after deregulation.

Table 4: State-Level Summary Statistics - Quarterly

Variable	Obs	Mean	Std. Dev.	Min	Max
Traditional	3724	.431	.495	0	1
Effective	3724	.424	.494	0	1
Linked	3724	.406	.491	0	1
Real Personal Income per Capita Growth	3724	1.148	2.373	-10.773	8.853

This table reports summary statistics for the state-time analysis. "Traditional" denotes deregulation dates for which a state opens up to another state regardless of whether the other state reciprocates. "Effective" denotes deregulation dates for when a state opens up to another state and that state reciprocates. "Linked" denotes dates following the first pairwise BHC acquisition following an effective deregulation. Source: Amel (1993), BEA

Column (2) uses the effective measure and finds a similar result as the annual regressions of around 0.66 percentage points. In the final column, we find a positive and significant increase of around 1.14 percentage points.[22]

The additional power from the quarterly regressions permits us to run a horse race between our variable and the two other measures of deregulation. Table 6 shows the results. Note that due to the high correlation across indices, collinearity of the independent variables results in wide standard errors. Neverhteless, our measure captures retains its large (and stable) point estimate. The point estimates for the traditional and effective measures are much smaller in magnitude. We view this as further evidence supporting the claim that formation of interstate bank networks was the principle mechanism by which deregulation had real economic effects.

One may be surprised to see such differences in the effects of the different deregulation, and wonder where the differences come from. Table 7 shows the correlation coefficient of the three different measures. The correlation between the traditional measure and the effective measure is unsurprisingly very high at 99%. However, the correlation of these measures with the linked measure is considerably lower, in the upper 80% range.

[22]In the appendix, we report results using weighted least squares, and show results are robust to this specification.

Table 5: State-Level Event Study - Quarterly Frequency

	$\Delta \ln Y$	$\Delta \ln Y$	$\Delta \ln Y$	$\Delta \ln Y$
Traditional	0.56			
	(0.360)			
Effective		0.66		
		(0.402)		
Linked			1.14**	1.18
			(0.508)	(0.740)
Branching	0.72***	0.73***	0.74***	0.74***
	(0.267)	(0.268)	(0.269)	(0.269)
State FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
No. Obs	3318	3318	3318	3318
$\mathcal{R}^2$	0.53	0.53	0.54	0.01
KP F Stat				31.10

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

This table reports results from Estimating (3) at the quarterly frequency. Source: Amel (1993), BEA

Table 6: Horserace Among z's

	$\Delta \ln Y$	$\Delta \ln Y$	$\Delta \ln Y$
Linked	1.15**	1.12**	1.12**
	(0.459)	(0.469)	(0.470)
Traditional	-0.01		-0.14
	(0.298)		(0.197)
Effective		0.03	0.16
		(0.349)	(0.343)
State FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
No. Observations	3318	3318	3318
$\mathcal{R}^2$	0.54	0.54	0.54

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

This table reports summary statistics from estimating Equation (3) using Log Real GDP growth as the outcome variable. Source: Amel (1993), BEA

Table 7: Deregulation Measure Correlations

Variables	Effective	Traditional	Linked
Effective	1.00		
Traditional	0.99	1.00	
Linked	0.89	0.88	1.00

VI.1 Corroborating Industry Evidence

Our argument thus far has been that interstate deregulation is associated with higher economic growth. We now provide corroborating evidence using industry-level data to highlight one mechanism by which interstate deregulation can support economic growth. We show that industries more reliant on external financing had higher rates of employment growth.

We begin by defining reliance on external financing. We follow Rajan and Zingales (1998) and estimate external financing using firm-level data from Compustat. This measure uses the median ratio of capital expenditures financed by external funds relative to total capital expenditures within an industry. We refer the reader to the appendix for more details on its construction, but note that the main difference between our measure and the measure in Rajan and Zingales (1998) is that, prior to taking the median across firms within an industry, we sum across variables between the period 1980-1985. We focus only on the manufacturing sector, as this sector is most likely to rely on financing for investment and production.

We estimate the effect of deregulation across industries by augmenting equation 3 using our measure of external financing. We interact the measure of external financing with each measure of deregulation, z

$$Y_{jst} = \alpha_{jt} + \alpha_s + \beta \left(z_{jt} + ExtFin_s \right) + \varepsilon_{jt} \tag{4}$$

$ExtFin_s$ represents the reliance on external financing for industry s. We interact this variable

with the three different measures of deregulation, traditional, effective, and linked, that have already defined. The coefficient of interest, β, now measures the relative effect of deregulation on industries more exposed to deregulation.

We note one other important change from equation 3. Because our measure of treatment is now at the state-time-sector level, we can now include *state-time* fixed effects. These state-time fixed effects allow us to control for all other determinants of economic growth at the state level. Our specification relies on differential exposure to banking deregulation via the external financing measure within a state during a specific quarter. This includes the direct effect of both interstate and intrastate deregulation, and so we do not include controls for interstate deregulation z or branching. We also include sector-specific fixed effects α_s

Table 8 reports the results of estimating equation 4. In column one, we regress the annual change in employment on the interaction between the traditional measure and the reliance on external financing. We find an imprecisely estimated value of 0.23. Replacing the traditional measure with the effective measure does little for either the magnitude of the coefficient or its precision.

However, using our linked measure of deregulation interacted with the measure of external financing, we find a positive and marginally significant value of 0.45. Given our inclusion of state-time fixed effects, this estimate implies that following deregulation, industries more reliant on external financing saw higher growth rates than those without such reliance within the same state. We take this as corroborating evidence that interstate deregulation had real economic effects.

VII Conclusion

In this paper, we revisit the effects of interstate banking deregulation in the United States by focusing on when and how bank holding companies (BHCs) formed relationships across state borders. While previous work in this literature typically finds little role for interstate deregu-

Table 8: Industry Heterogeneity

	$\Delta \ln Emp$	$\Delta \ln Emp$	$\Delta \ln Emp$
Trad x RZ	0.23		
	(0.229)		
Effective x RZ		0.27	
		(0.232)	
Linked x RZ			0.45*
			(0.230)
State-Time FE	Y	Y	Y
Ind FE	Y	Y	Y
No. Obs	310260	310260	310260
$\mathcal{R}^2$	0.05	0.05	0.05

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

This table reports estimates of the coefficient β from the regression $y_{jst} = \alpha_{jt} + \alpha_s + \beta\,(z_{jt} + ExtFin_s) + \varepsilon_{jst}$. z_{jt} represents one of three measures of deregulation. Standard errors are clustered at the state level.

lation in explaining real economic growth (relative to intrastate branching deregulation), we find that deregulation-induced bank expansion led to faster real economic growth and faster durable goods consumption among the states brought into an interstate bank network. In contrast to the pre-existing literature studying the effects of deregulation on real economic outcomes, our approach employs a novel measure of deregulation that explicitly accounts for whether Bank Holding Companies acquired commercial banks across state lines. This measure has improved predictive power over previous measures of interstate deregulation, even after controlling for intrastate branching.

Turning to bank-level outcomes, we find that BHCs responded quickly to interstate deregulation by establishing controlling ownership of other BHCs across state borders. This rapid expansion suggests that the de jure restrictions on cross-state banking effectively constrained some BHCs from operating at (from their perspective) optimal scale. The newly formed relationships resulted in significant increases in the size of consolidated BHC balance sheets by as much as 10% and increased the number of commercial banks managed by around 3.

We then investigate the effect such relationships had on acquired commercial banks. We

find large and positive effects on both assets and liabilities.Acquired commercial banks increase their level of assets by around 10% in the long-run relative to non-acquired commercial banks. Similarly, liabilities also rise by around 10% in the long-run relative to that of non-acquired commercial banks. Unfortunately, the data to disentangle the sources of the change on both the asset and liability side are either very noisy or nonexistant, preventing us from diving deeper using our data. We leave this open as an avenue for future research.

Our paper provides empirical support for the hypothesis that bank regulation can stifle real economic growth, in line with evidence summarized by Levine (2005). At face value, our paper supports the idea that the growth of the financial sector results in positive welfare gains for households.

Notably, our paper does not investigate how bank expansion and consolidation may contribute to the likelihood of propagation and amplification of various financial shocks throughout the bank network. Such costs may be nontrivial, as shown in the historical context by Anderson et al. (2019), and more recently during the Global Financial Crisis in work such as Ivashina et al. (2015). However, these papers do not investigate the propagation of shocks *within* individual bank networks, a task we leave for future work.

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A Data Construction

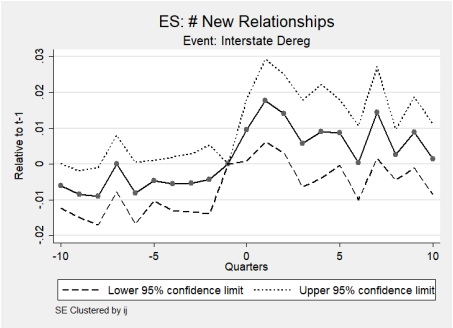
Our data on BHC linkages comes from the FFIEC.

- ctrl_ind = 1
- reg_ind = 1
- reln_lvl = 1
- fc_ind = 0
- reason_row_crtd = 1

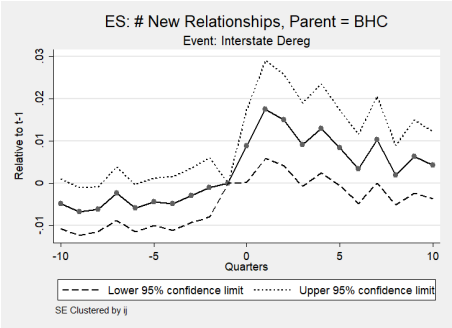
B Additional Figures and Tables

B.1 Linkages

In this section, we report additional event studies using different measures of interstate links. In the main text, we focus on new BHC to BHC relationships as other pairs of charter types and mergers do not show significant results. We report that evidence here.



(a) Number of Relationships



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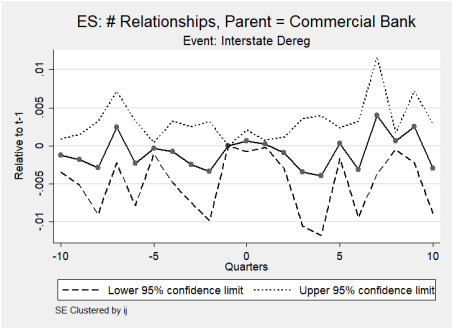

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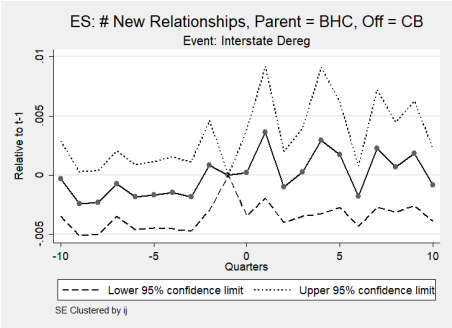

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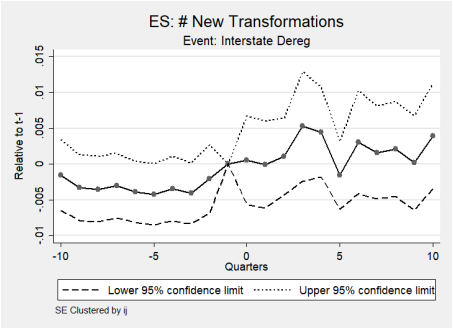

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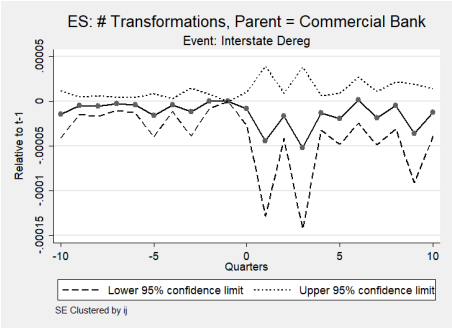

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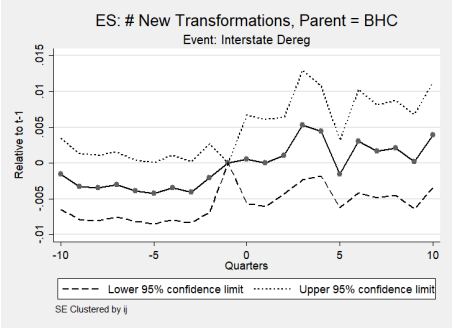

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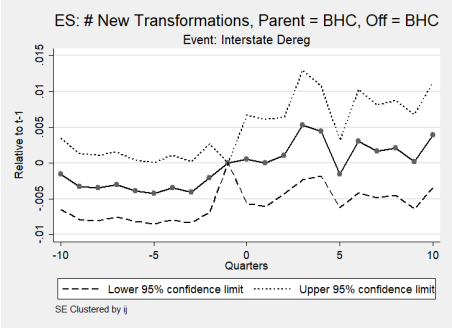
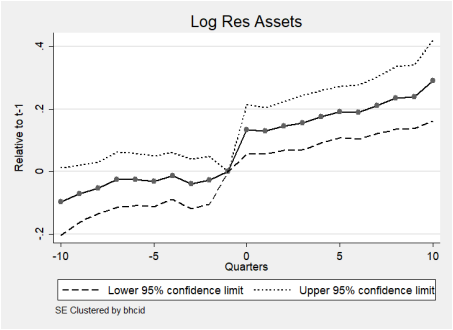
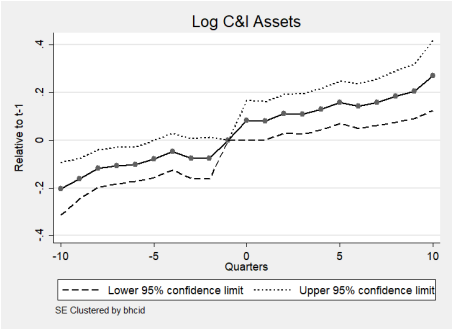


(h) Number of Transformations

B.2 Additional BHC Results



(a) Log Residential Loans



(b) Log Commercial & Industrial Loans

B.3 Effects on the Real Economy

Table 9: Annual Regressions - Weighted

	$\Delta \ln Y$	$\Delta \ln Y$	$\Delta \ln Y$	$\Delta \ln Y$	$\Delta \ln Auto$	$\Delta \ln Auto$	$\Delta \ln Auto$	$\Delta \ln Auto$
Traditional	1.20**				1.15*			
	(0.485)				(0.667)			
Effective		1.29**				1.54**		
		(0.496)				(0.686)		
Linked			1.38***	2.02**			1.92***	2.41**
			(0.399)	(0.834)			(0.569)	(1.102)
Branching	0.83***	0.82***	0.85***	0.82***	0.81	0.80	0.82	0.79
	(0.221)	(0.221)	(0.226)	(0.226)	(0.516)	(0.519)	(0.514)	(0.520)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs	945	945	945	945	945	945	945	945
$\mathcal{R}^2$	0.55	0.55	0.55	0.02	0.25	0.25	0.25	-0.02
KP F Stat				92.18				92.18
Weight	Pop	Pop	Pop	Pop	Pop	Pop	Pop	Pop

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 10: Quarterly Regressions - Weighted

	$\Delta \ln Y$	$\Delta \ln Y$	$\Delta \ln Y$	$\Delta \ln Y$
Traditional	1.15***			
	(0.323)			
Effective		1.26***		
		(0.316)		
Linked			1.46***	1.74***
			(0.347)	(0.420)
Branching	0.94***	0.94***	0.94***	0.93***
	(0.238)	(0.237)	(0.238)	(0.236)
State FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
No. Obs	3318	3318	3318	3318
$\mathcal{R}^2$	0.66	0.66	0.66	0.04
KP F Stat				93.69
Weight	Pop	Pop	Pop	Pop

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

C Balance Sheet Components

In the main text, we show that the assets and liabilities of acquired commercial banks each rise by approximately 10% relative to non-acquired commercial banks. Here, we decompose both assets and liabilities into their observable components. We stress that these results are to be interpreted with caution given the (in some cases) wide significant bands and (in other

cases) stark pre-trends.

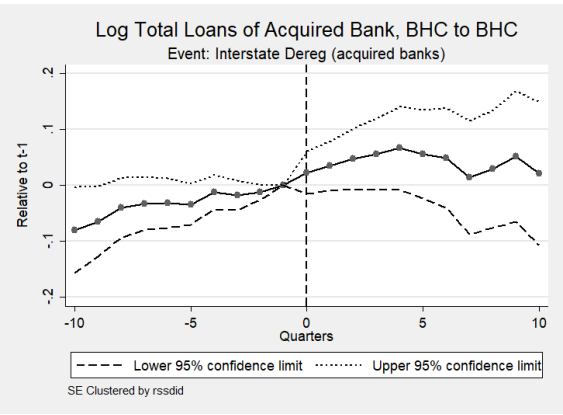
We begin on the asset side of the balance sheet. We break down assets into three main categories: loans, liquid assets and fed fund assets.[23] We choose these categories because of their representation of investment in local real activity (loans), investment in safe assets (liquid assets), investment in out of state real activity (fed fund assets), respectively. This interpretation assumes that all loans are "local" in the sense that only firms and households that are geographically close to the acquired banks are recipients. Fed fund assets, on the other hand, we view as a proxy for non-local lending. For example, banks within the BHC network may borrow from the newly acquired banks if the lending opportunities nearby the acquired banks are less profitable than investment opportunities elsewhere within the network. Finally, liquid assets such as government securities represent the opportunity cost of risky lending, and so proxy for the outside option of the acquired bank.

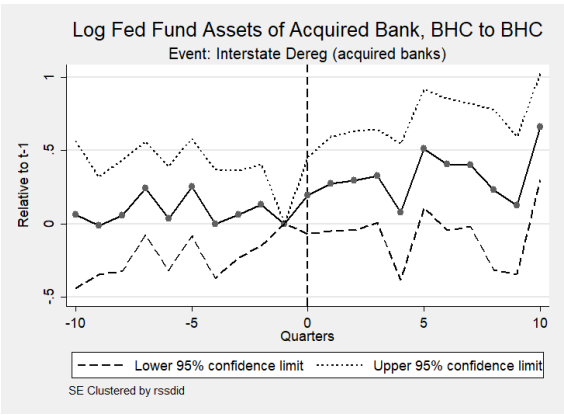
Figure 10 reports the results. The top-left panel shows results for total loans. Total loans rise by approximately 5-7%, which is significant at the 95% level. There is a slight pretrend, which we return to shortly. The top-right panel reports results for fed fund assets. Here, we see an immediate yet very noisy, rise in the amount of fed fund assets on the order of around 25%. This series starts from a very low level, which explains the large magnitude of the rise. In the bottom panel, we find a substantial negative pre-trend in liquid assets prior to acquisition. Interestingly, the trend somewhat flattens beginning at the time of acquisition.

Figure 11 breaks down loans by type: commercial and industrial (C&I) loans, personal loans and real estate loans. We find suggestive evidence as for why total loans rises following acquisition. First, consider C&I loans in the lop-left figure. Loans are essentially flat until around two quarters following acquisitions, when there is a temporary rise in log C&I loans relative to the control group. The top-right panel shows total personal loans. Here, we do not see any substantial change either before or following acquisition. Finally, in the bottom panel, we show real estate loans. If anything, real estate loans decline following acquisition.

[23]Other components, such as property and salaries, do not move and encompass around 8% of assets on average. Results are available upon request.

Figure 10: Components of Assets





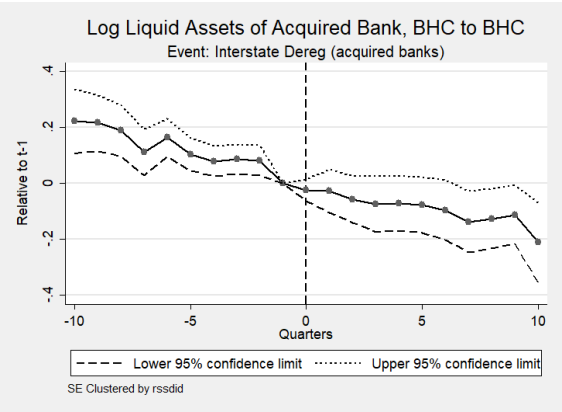
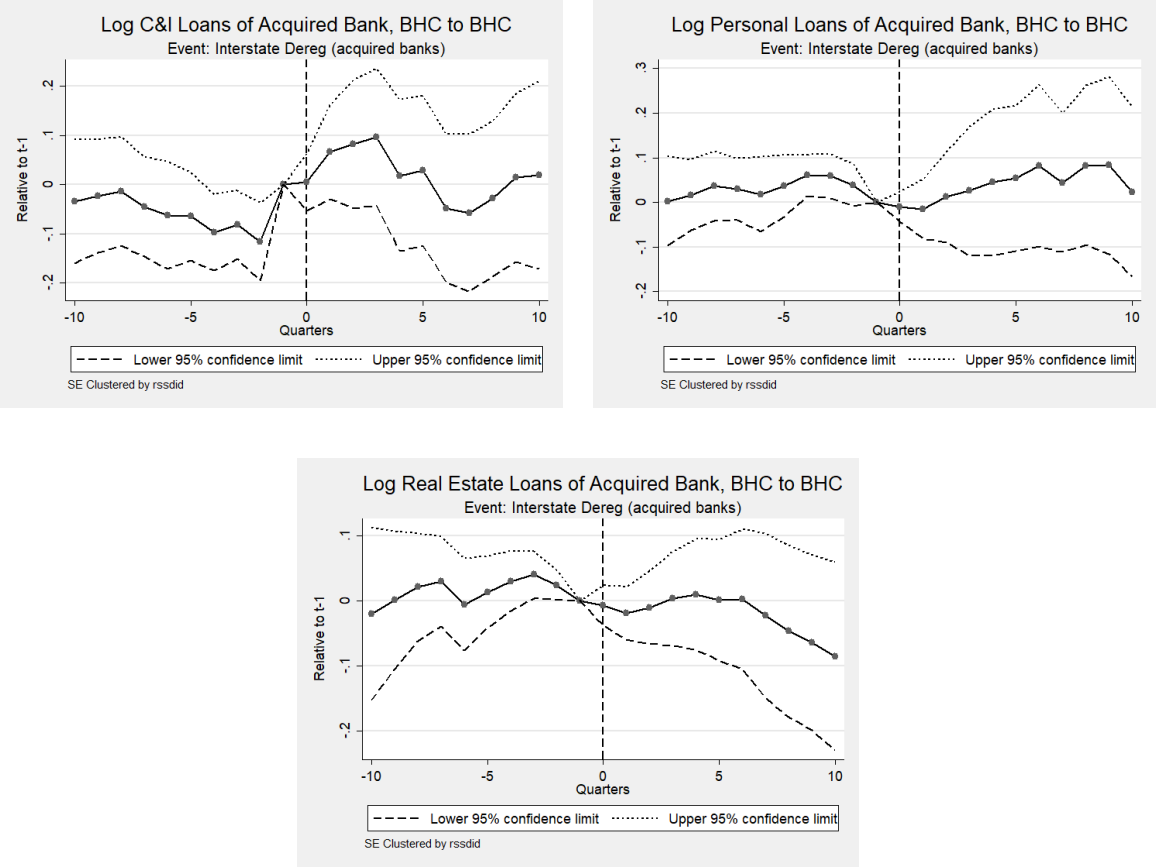


Figure 11: Components of Loans

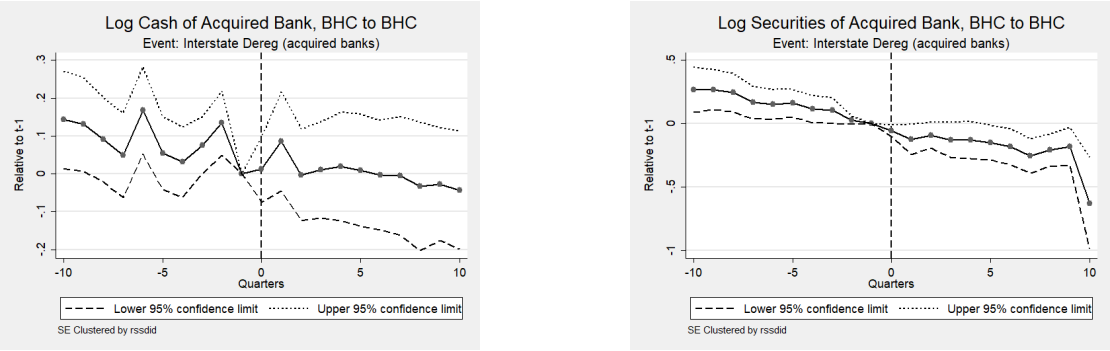


We caution that all of these estimates of the components are rather noisy. Our interpretation is that there is substantial heterogeneity in terms of local lending opportunities of acquired banks, an issue we leave to future research.

We next decompose liquid assets. The trend in liquid assets following is driven exclusively by trading securities. Figure 12 plots the evolution of cash and securities, the two main components of liquid assets. In the top panel, we see that cash has a slight decline prior to acquisition on the order of 10% before staying remarkably stable following acquisition. In the bottom panel of 12, we see that securities have a similar pattern, with a significant decline following acquisition before staying stable over the next year and a half.

Taken together, we view the evidence on the asset side of the balance sheet as supporting the hypothesis that there were real frictions in bank balance sheet expansion. Prior to acquisition, banks were in the process of substituting liquid assets, such as cash and secu-

Figure 12: Components of Liquid Assets



rities, for loans, as shown by the pre-trends in both series—even as total assets remained unchanged. After acquisition, however, total assets began to rise along with total loans and Fed Fund assets even as liquid assets of acquired banks flattened. Unfortunately, we do not have the power to disentangle the sources of the change in loans. Suggestive evidence points to increasing C&I loans as driving the post-acquisition change.

We next turn to the liability side of the balance sheet. We look only at total deposits, shown in Figure 13. We see a significant, albeit delayed, rise in total deposits on the order of 10%, suggesting that nearly all of the rise in liabilities comes from changes in total deposits. Unfortunately, the historical data do not permit a breakdown into types of deposits (by households or other banks, for example), so we cannot say for certain what sector increased their deposits or how each individual component moved.

Figure 13: Deposits