Board of Governors of the Federal Reserve System

International Finance Discussion Papers

Number 1305

November 2020

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Please cite this paper as:
Goetz, Martin R. and Juan Carlos Gozzi (2020). "Financial Integration and the Co-Movement of Economic Activity: Evidence from U.S. States," International Finance Discussion Papers 1305. Washington: Board of Governors of the Federal Reserve System, https://doi.org/10.17016/IFDP.2020.1305.

Financial Integration and the Co-Movement of Economic Activity: Evidence from U.S. States

Martin R. Goetz[a] and Juan Carlos Gozzi[b]

Abstract: We analyze the effect of the geographic expansion of banks across U.S. states on the co-movement of economic activity between states. Exploiting the removal of interstate banking restrictions to construct time-varying instrumental variables at the state-pair level, we find that bilateral banking integration increases output co-movement between states. The effect of financial integration depends on the nature of the idiosyncratic shocks faced by states and is stronger for more financially dependent industries. Finally, we show that integration (1) increases the similarity of bank lending fluctuations between states and (2) contributes to the transmission of deposit shocks across states.

Keywords: Banking integration; synchronization; financial deregulation; business cycles

JEL classifications: E32; F36; F44; G21

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1 Introduction

This paper analyzes the effect of the geographic expansion of banks across U.S. states on the co-movement of economic activity between states. To identify the causal effect of financial integration through banks on the synchronization of economic activity, we exploit the removal of bilateral restrictions to interstate banking to construct time-varying instrumental variables at the state-pair level. We also provide novel insights on the underlying mechanisms by analyzing heterogeneity across states and industries and showing how integration affects state-level lending and the transmission of bank funding shocks across states.

The effect of financial integration (through banks) on output synchronization between regions is theoretically ambiguous and depends on the nature of the shocks that drive local economic fluctuations.[1] In the presence of idiosyncratic real (e.g., productivity) shocks, financial integration can decrease the co-movement of economic activity between regions. In a financially integrated world, if firms in a particular region face a negative productivity shock, multi-market banks may shift lending to non-affected regions, causing a further divergence in economic activity between regions and reducing output synchronization.[2] In contrast, in the presence of idiosyncratic financial shocks, integration can increase the co-movement of economic activity between regions. For instance, if multi-market banks face a negative funding shock in one market, they may cut lending in other markets, negatively affecting economic activity in regions that were not directly hit by the initial shock and increasing output synchronization.[3]

Identifying the causal effect of financial integration (through banks) on output synchronization empirically faces several challenges. First, unobservable time-varying

[1] Morgan et al. (2004) show this using a multi-state version of the banking model of Holmstrom and Tirole (1997). Kalemli-Ozcan et al. (2013a) draw similar conclusions using a DSGE model.

[2] See, among others, Backus et al. (1992), Obstfeld (1994), and Heathcote and Perri (2004).

[3] See, among others, Calvo and Mendoza (2000), Allen and Gale (2000), Devereux and Yetman (2010), Mendoza and Quadrini (2010), Dedola et al. (2012), and Devereux and Yu (2014).

factors may jointly determine financial integration and output co-movement between regions. Second, changes in real economic integration, such as increased trade, might affect output co-movement and could also be correlated with changes in banking integration. Finally, banks choose where and when to expand and this decision might be correlated with the level of or changes in output synchronization between regions.

Given these challenges to identification and the differing theoretical predictions, it is not surprising that the empirical evidence on the effect of financial integration on output co-movement is somewhat mixed. Cross-country analyses tend to find a positive relationship between financial integration and output synchronization (Kose et al., 2004; Baxter and Kouparitsas, 2005; Imbs, 2006; Rose, 2009). Consistent with this evidence, Morgan et al. (2004) find that banking integration between U.S. states is positively correlated with the co-movement of economic activity between states. In contrast, Kalemli-Ozcan et al. (2013b) argue that the positive link between financial integration and output synchronization at the national level reflects permanent differences between countries and find a negative relationship between integration and synchronization for a sample of industrialized countries when controlling for country-pair fixed effects. Duval et al. (2016) find similar results for a panel of advanced and emerging economies.[4]

In this paper, we identify the causal effect of banking integration across U.S. states on output synchronization by exploiting the removal of legal restrictions to interstate banking to construct time-varying instrumental variables (IV) at the state-pair level. Restrictions on interstate banking prohibited entry from out-of-state banks for much of the 20th century in the U.S. Starting in the late 1970s, states gradually removed these restrictions in different years and through different methods.[5] The removal of interstate

[4]Cesa-Bianchi et al. (2019) find that financial integration is positively correlated with synchronization when countries face idiosyncratic shocks.

[5]This process culminated with the Riegle-Neal Interstate Banking and Branching Efficiency Act of 1994, which eliminated all remaining barriers to entry at the federal level.

banking restrictions has a direct effect on financial integration between states, as once these barriers are removed, banks can expand across state borders (Michalski and Ors, 2012; Goetz et al., 2013; Landier et al., 2017). Earlier work suggests that there are good economic reasons for treating the process of interstate banking deregulation as exogenous to state economic conditions (Kroszner and Strahan, 1999). Consistent with this, we find no evidence that the level of and changes in output synchronization are correlated with the timing of deregulation.

Using panel data at the state-pair level over the 1976-1994 period and controlling for state-pair fixed effects, we first find a positive correlation between banking integration and output synchronization between states, consistent with Morgan et al. (2004).[6] However, this relationship is not robust to different empirical specifications and alternative measures of synchronization. Moreover, even when the effect is statistically significant, the estimated economic magnitudes are very small. As discussed above, OLS estimates are likely to be biased and do not have a causal interpretation.

Using our IV strategy, we find a consistent strong positive effect of banking integration on output synchronization between states, controlling for state-pair fixed effects and time-varying variables. These findings are robust to different specifications and alternative measures of synchronization. Our estimates show an economically significant effect of banking integration on output synchronization: an increase in the share of assets and deposits held by banks with operations in two states equal to the mean of this variable leads to an increase in our main measure of synchronization (the negative absolute difference in residual real GDP growth between two states) of 13 percent of its standard deviation. The finding of a positive effect of banking integration on output synchronization between states indicates that integration contributed to the transmis-

[6]We focus on the period 1976 to 1994 because data on bank assets and ownership structure from regulatory filings become available in 1976. After 1994 it is impossible to distinguish assets of the same bank holding company in different states because the Riegle-Neal Act allowed banks to consolidate bank charters across states.

sion of financial shocks across state borders, making state economic fluctuations more similar, and suggests that shocks to financial intermediaries were a significant source of local economic fluctuations in the U.S. over our sample period.

To better understand why financial integration increases output synchronization, we examine heterogeneity across states and industries. First, we analyze whether the effect of integration on synchronization depends on the nature of the idiosyncratic shocks faced by different states. To this end, we identify (1) states that face financial shocks, proxied by the extent of bank failures in a state and year, and (2) states that face real shocks, proxied by the monetary losses due to natural disasters in a state and year.[7] We find that the effect of banking integration on output synchronization between two states is larger when at least one of the states in the pair faces significant bank failures. We also find that the effect of integration on synchronization is smaller or statistically insignificant when at least one state in the pair experiences large losses due to natural disasters. This is consistent with theoretical arguments outlined above. Second, we examine differences across industries. If multi-market banks transmit shocks across states through changes in their lending, then we would expect integration to have a larger effect on output co-movement for industries that rely more on bank financing. Indeed, we find that banking integration has a strong positive effect on output synchronization for industries with a high dependence on external finance, while it does not have a statistically significant effect for industries that are less dependent on external financing. To our knowledge, we are the first (1) to show how the effects of financial integration on output synchronization differ depending on whether regions face financial or real shocks and (2) to document that this effect differs across industries.[8]

[7] A number of papers have analyzed the response of financial institutions to natural disasters, finding that they tend to ameliorate the negative impact of disasters on households (Morse, 2011; Chavaz, 2016; Cortes and Strahan, 2017).

[8] Kalemli-Ozcan et al. (2013a) study the role of financial shocks, showing that financial integration is associated with greater output synchronization between countries during financial crises, but do not analyze real shocks.

Our findings are consistent with the idea that multi-state banks transmit shocks across states through their internal capital markets, creating a commonality in lending among states which then increases output synchronization.[9] To examine this underlying channel further, we analyze whether banking integration increases the similarity of bank lending fluctuations between states. Indeed, we find robust evidence that integration increases the co-movement of business lending between two states.[10]

Finally, we analyze whether banking integration contributes to the transmission of funding shocks across states. If banks operating in different states transmit funding shocks through their internal capital markets, then we would expect aggregate bank lending in a state to respond to changes in aggregate deposits in other states with which it is financially integrated. Analyzing this question empirically raises some challenges, as states that are financially integrated might face common shocks that affect both deposits and loans. To overcome this challenge, we use a second identification strategy, following Goetz et al. (2013, 2016). Specifically, we first exploit the process of interstate banking deregulation to generate the predicted banking integration (i.e., share of jointly-owned assets and deposits) between each state pair. We then compute for each state and year the weighted average of the growth rate of real state-level bank deposits across all other states, using as weights the predicted banking integration between state i and each state. Finally, we use this predicted weighted average deposit growth rate as an instrument for the actual weighted average deposit growth rate across all other states. Using this approach, we find that lending in a state responds positively to deposit changes in other states with which it is integrated. This is consistent with the idea that banking integration facilitates capital mobility, fostering the propagation of

[9] See Houston et al. (1997), Houston and James (1998), Ashcraft (2006), and Holod and Peek (2010), among others, for evidence that U.S. bank holding companies operate internal capital markets.

[10] We focus on Commercial and Industrial (C&I) lending, that is, lending for commercial and industrial purposes to business enterprises, following most of the literature on the role of banks in the propagation of economic fluctuations in the U.S. (Kashyap and Stein, 2000; Driscoll, 2004).

funding shocks across states.

This paper contributes to a large literature, described above, that analyzes the effect of financial integration on the synchronization of economic activity between regions. We contribute to this literature by (1) estimating the causal effect of banking integration on output co-movement using an IV estimation strategy and (2) showing that, consistent with theoretical arguments, the effect of integration on synchronization varies across industries and depends on the idiosyncratic shocks faced by different regions. Furthermore, we present novel evidence on the underlying economic mechanisms that drive the effect of banking integration on output synchronization, showing that integration fosters the co-movement of bank lending between states and also contributes to the transmission of bank funding shocks across state borders.

This paper is also related to a large literature that studies the effects of banking deregulation in the U.S. Earlier research shows that intrastate branching and interstate banking deregulations are associated with higher economic growth, an acceleration in business formation, increased entry and exit by new firms, and improved financing for small firms (Jayaratne and Strahan, 1996; Black and Strahan, 2002; Cetorelli and Strahan, 2006; Kerr and Nanda, 2009; Rice and Strahan, 2010).[11] One mechanism that could account for some of these findings is increased capital mobility across states following deregulation. However, there is little direct evidence on the effect of deregulation on capital mobility. We show that increased integration following interstate banking deregulation contributed to capital flows through banks across states.

Finally, our paper is also related to a growing literature that analyzes how multi-market banks in the U.S. transmit local shocks to funding (Gilje et al., 2016) and to credit demand (Ben-David et al., 2017; Cortes and Strahan, 2017; Chakraborty et al.,

[11] Intrastate branching refers to the ability of banks to expand their branch networks within a state. Interstate banking refers to the ability of bank holding companies to own and operate banks in more than one state. Since we are interested in the effects of integration across states, we focus our analysis on interstate banking restrictions.

2018) across markets. Different from these papers, we do not focus on the transmission of particular shocks through banks' pre-existing geographic networks, but rather look at the aggregate effect of banking integration between states, while accounting for the endogeneity of geographic integration.

2 Data

2.1 Banking Integration across U.S. States

We measure interstate banking integration based on bank affiliations through bank holding companies (BHCs). We link each bank to its ultimate parent BHC and construct two measures of banking integration for each state pair i, j, following Morgan et al. (2004).[12] First, we define a dummy variable equal to one if bank assets or deposits in state i are held by a BHC that also holds assets or deposits in state j, and zero otherwise (*Dummy =1 if jointly-owned assets or deposits*). Second, we construct a continuous measure of integration by computing the share of jointly-owned assets and deposits, defined as the bank assets and deposits in a state pair held by BHCs with operations in both states divided by the sum of the total bank assets and deposits of both states (*Share of jointly-owned assets and deposits*).[13] We consider both assets and deposits for our measures of banking integration to capture different dimensions of integration. This also makes our measures comparable to those used in previous research on international financial integration, which usually considers both assets and liabilities.

[12]Banks report their unique parent company, and there can be several layers of subsidiaries and parent companies before the ultimate parent company is reached. We assign a bank to the parent BHC that owns at least 50 percent of the bank's equity.

[13]For each state pair i, j we calculate the jointly-owned assets and deposits as the sum of the assets and deposits in state i held by BHCs that also hold assets or deposits in state j plus the sum of the assets and deposits in state j held by BHCs that also hold assets or deposits in state i. We scale this variable by the sum of total bank assets and deposits of states i and j.

Data on bank assets and ownership structure are obtained from the Report of Condition and Income (*"Call Reports"*). All banking institutions in the United States regulated by the Federal Deposit Insurance Corporation (FDIC), the Federal Reserve, or the Office of the Comptroller of the Currency, must file these reports on a regular basis. These reports hold balance sheet, income, and ownership information. Data on deposits come from the FDIC's Summary of Deposits, which provides branch-level data on deposits, location, and ownership for all branches of insured banks.[14]

We focus on the 48 contiguous U.S. states. Moreover, we omit Delaware and South Dakota since changes to their usury laws were followed by a relocation of BHC headquarters, affecting the measurement of integration with these two states (Jayaratne and Strahan, 1996). Our sample consists of 1,035 (46 * 45 /2) unique state pairs over the period 1976-1994.

2.2 Synchronization of Economic Activity

We measure the synchronization of economic activity between two states using three different variables based on state GDP. First, following Morgan et al. (2004) and Kalemli-Ozcan et al. (2013b), we measure output synchronization between states i and j as the negative of the absolute difference of residual real GDP growth:

$$Synch_{i,j,t} = - \mid \varepsilon_{i,t} - \varepsilon_{j,t} \mid \tag{1}$$

where $\varepsilon_{i,t}$ is the residual from the following regression:

$$\mathrm{Y}_{i,t} = \alpha_i + \delta_t + \varepsilon_{i,t} \tag{2}$$

[14]Summary of Deposits data are reported as of June 30 of each year, so we also take the data on bank assets and ownership structure from the Call Reports as of June 30 of each year to construct our measures of integration.

where $Y_{i,t}$ is the real GDP growth of state i in period t; α_i and δ_t are state and time fixed effects, respectively. The residuals $\varepsilon_{i,t}$ capture the deviation of a state's real GDP growth in a given year from its sample mean and from the mean of all the states in our sample in that year.

This synchronization measure has some advantages relative to the Pearson correlation coefficient used by most of the earlier empirical cross-country work on financial integration and output synchronization. First, it can be calculated at every point in time, rather than over an interval of time. Second, it is invariant to the volatility of the underlying shock (Forbes and Rigobon, 2002; Corsetti et al., 2005). However, a potential limitation of this variable is that it conflates a measure of co-movement and a measure of dispersion (Cesa-Bianchi et al., 2019). For instance, even if two states respond in the same direction to a particular shock, this measure could fall if the magnitude of their responses is different.

Our second measure of output synchronization is the instantaneous quasi-correlation of real GDP growth rates between states i and j (Abiad et al., 2013; Duval et al., 2016), which is not subject to the above criticism, and is defined as:

$$QCorrel_{i,j,t} = \frac{(Y_{i,t} - \bar{Y_i}) - (Y_{j,t} - \bar{Y_j})}{\sigma_i \sigma_j} \tag{3}$$

where $\bar{Y_i}$ and σ_i are the average and the standard deviation of real GDP growth of state i over our sample period, respectively.

Finally, to make our results comparable to the earlier cross-country literature, we also measure the synchronization of economic activity between two states using the five-year correlation of real GDP growth. In particular, for each state pair we calculate the correlation of real GDP growth between the two states in year t in a forward-looking manner, using information for years t to $t + 4$. We calculate this measure for non-overlapping five-year periods to avoid artificially introducing autocorrelation.

We construct our measures of output synchronization using state real GDP growth. Data on nominal GDP for each state and year come from the Bureau of Economic Analysis. We deflate these data using the national U.S. consumer price index from the Bureau of Labor Statistics. We then calculate the annual growth rate of real GDP in each state and year as the change in the natural logarithm of this variable.

We control for several state-pair time-varying variables in our regressions.[15] First, we control for (lagged) differences in industrial structure between states, as this might affect their output synchronization (Obstfeld, 1994; Kalemli-Ozcan et al., 2001).[16] Second, to account for time-varying gravity factors, we control for the (lagged) product of the logarithm of the two states' real GDP.[17] Finally, we include a dummy variable equal to one after (at least) one of the states in a pair eliminates restrictions to intrastate branching, because many states lifted these restrictions during our sample period. We winsorize all variables at the 1st and 99th percentiles to limit the influence of outliers; we obtain similar results if we do not winsorize.

2.3 Descriptive Statistics

Table 1 shows summary statistics for our main variables. In terms of banking integration, we find that only 18 percent of the state-pair year observations in our sample have any jointly-owned assets or deposits, and these jointly-owned assets and deposits represent on average 2.5 percent of the total assets and deposits in a state pair. Regarding output synchronization, the negative absolute difference in residual real GDP growth between states averages three percent over our sample, while the mean of the

[15]We obtain similar results if we exclude these controls.

[16]For each state pair and year we first calculate the difference between states in the share of total employment accounted for by each one-digit SIC sector in each state. We then add the square of these differences across sectors and take the square root of this sum.

[17]Note that we account for cross-sectional differences across state pairs (including gravity factors such as distance) by controlling for state-pair fixed effects in all our regressions.

five-year correlation of real GDP growth between states is about 57 percent.

Banking integration between U.S. states increased significantly over our sample period. Figure 1 illustrates the evolution of integration from 1976 to 1994. The top panel shows the fraction of all state pairs in our sample that are financially integrated (i.e., have any jointly-owned bank assets or deposits) in each year. While only nine percent of all state pairs were financially integrated in 1976, more than a third of all state pairs were integrated by 1994.[18] The bottom panel of Figure 1 illustrates the evolution of banking integration at the state-pair level, using the example of California. It shows the evolution of the share of jointly-owned assets and deposits between California and three other states (Florida, Texas, and Washington). As this graph illustrates, the banking integration of a given state with other states can exhibit significant variation - both across states and over time. For instance, the integration between the banking systems of California and Washington increased significantly after 1984, whereas banking integration between California and Florida remained fairly low and changed little over the sample period. Moreover, bilateral integration can be quite volatile over time, as illustrated by the case of California and Texas.

Figure 2 illustrates the evolution of output co-movement between states over our sample period. The top panel shows the average across state pairs of our main synchronization measure (the negative absolute difference of residual real GDP growth) from 1976 to 1994. The average level of output synchronization between states showed some volatility during the 1980s, but was fairly stable after 1988. The bottom panel of Figure 2 illustrates the co-movement of economic activity at the state-pair level, showing the evolution of our main synchronization measure between California and three other states (Florida, Texas, and Washington). As these graphs illustrate, the

[18]Some state pairs were financially integrated before the process of interstate banking deregulation started in the early 1980s because some states allowed out-of-state bank entry before the Douglas Amendment to the 1956 Bank Holding Company Act effectively restricted interstate banking. Existing multi-state BHCs at the time were grandfathered by the Bank Holding Company Act.

output co-movement of a given state with other states can show significant variation, both across states and over time.

3 Banking Integration and Output Synchronization between States

3.1 OLS Estimates

As a preliminary assessment of the relationship between banking integration and output synchronization we estimate OLS regressions. The baseline regression model is specified as follows, following Morgan et al. (2004) and Kalemli-Ozcan et al. (2013b):

$$Synchronization_{i,j,t} = \alpha_{i,j} + \delta_t + \beta * Banking\ integration_{i,j,t} + \mathbf{X'}_{i,j,t}\gamma + \varepsilon_{i,j,t} \quad (4)$$

where $Synchronization_{i,j,t}$ is a measure of the synchronization of economic activity between states i and j in year t; $Banking\ integration_{i,j,t}$ measures the integration of state i and j's banking systems; and $\mathbf{X}_{i,j,t}$ are state-pair time-varying controls. We also include time fixed effects (δ_t) to capture common national time-varying factors and state-pair fixed effects ($\alpha_{i,j}$) to account for state-pair time-invariant characteristics. The coefficient β estimates the relationship between within-state pair changes in banking integration and output synchronization, although it does not have a clear causal interpretation. Standard errors are clustered at the state-pair level.

Table 2 presents OLS results from estimating equation (4). Columns (1) to (4) show regression results considering as dependent variable the negative absolute difference in residual real GDP growth. The dependent variable in columns (5) to (8) is the instantaneous quasi-correlation of real GDP growth and in columns (9) and (10) it is the five-year correlation of real GDP growth. The measure of banking integration in odd numbered columns is a dummy variable equal to one if the two states in a given pair have any common assets or deposits. In even numbered columns, we use a continuous measure of integration, namely, the share of jointly-owned assets and deposits. Columns (3), (4), (7) and (8) include state-pair linear time trends to control for time-varying, unobservable factors at the state-pair level.[19]

The results in columns (1) and (2) of Table 2 show that banking integration between U.S. states is positively correlated with output synchronization, consistent with the findings by Morgan et al. (2004). However, the coefficient on the banking integration variable becomes statistically insignificant once we control for state-pair linear time trends (columns (3) and (4)). Further, we find no significant relationship between banking integration and output co-movement when analyzing the instantaneous quasi-correlation of real GDP growth (columns (5) to (8)). We do find a positive and statistically significant relationship when using the five-year correlation of real GDP growth as dependent variable (columns (9) and (10)).

Overall, the results in Table 2 suggest that banking integration tends to be positively correlated with output synchronization, although this relationship is not robust to different specifications and to alternative measures of synchronization. Moreover, even in those specifications where we do find a statistically significant relationship, the estimated economic magnitudes are very small. For example, the estimated coefficient in column (2) of Table 2 (0.861) indicates that an increase in the share of jointly-owned assets and deposits between two states equal to its mean (0.025) is associated with an increase in output synchronization (measured by the negative absolute difference in residual real GDP growth) of 0.02, less than one percent of the standard deviation of this variable. Moreover, as discussed above, it is not possible to draw causal inferences from these OLS results, as they are likely to be affected by selection and omitted

[19]When analyzing the five-year correlation of real GDP growth we do not include state-pair linear time trends because we only have four observations for each state pair.

variables which could bias these estimates in any direction.

3.2 Instrumental Variables (IV) Estimates: Causal Effect of Banking Integration on Synchronization

To identify the causal effect of banking integration on the co-movement of economic activity, we use an IV approach based on the deregulation of interstate banking. We first briefly describe the process of interstate banking deregulation and then present our IV approach and results.

3.2.1 Interstate Banking Deregulation

For many decades, banks in the U.S. were not allowed to expand their geographical scope beyond certain areas. States imposed limits on the location of bank branches and offices in the 19th century, restricting the expansion of banks both within states through branches (intrastate branching restrictions) and across state lines (interstate banking restrictions). While state-chartered banks were always subject to state banking laws, the McFadden Act of 1927 extended the application of these laws to national-chartered banks. The ability of states to exclude out-of-state bank holding companies from entering was further strengthened in the Douglas Amendment to the 1956 Bank Holding Company Act.[20] These restrictions were supported by the argument that allowing banks to expand freely could lead to a monopolistic banking system. Furthermore, granting bank charters was a profitable income source for states.

Starting in the 1970s, technological and financial innovations eroded the value of entry restrictions for banks. In particular, improvements in data processing, telecom-

[20] The Douglas Amendment prohibited a bank holding company that had its principal place of business in one state from acquiring a bank located in another state, unless the acquisition was "specifically authorized by the statute laws of the State in which such bank is located, by language to that effect and not merely by implication." Since no state provided such authorization, BHCs were in practice prohibited from crossing state lines.

munications, and credit scoring weakened the advantages of local banks, reducing their willingness to fight for the maintenance of restrictions on entry by out-of-state banks and triggering deregulation (Kroszner and Strahan, 1999). Maine was the first state to allow entry by out-of-state bank holding companies in 1978. In particular, BHCs from another state were allowed to enter Maine if that other state reciprocated and allowed entry by BHCs headquartered in Maine. While Maine enacted this policy in 1978, no other state changed its entry restrictions until 1982, when New York put in place a similar legislation and Alaska completely removed entry restrictions on out-of-state BHCs. Over the following 12 years, states removed entry restrictions by unilaterally allowing out-of-state BHCs to enter or by signing reciprocal bilateral and multilateral agreements with other states to allow interstate banking. This deregulation process culminated with the Riegle-Neal Interstate Banking and Branching Efficiency Act of 1994, which removed all remaining entry barriers at the federal level.

To analyze the process of interstate banking deregulation, we use data from Amel (2000) and Goetz (2018) on the dates of changes to state laws that affect the ability of banks to expand across state borders. We define the effective date of deregulation for each state pair i, j as the date when state i allows entry by BHCs headquartered in state j, or vice versa. For instance, if state i opens up its banking system on a reciprocal manner to all states, the date of effective deregulation corresponds to the date when state j allows entry of state i's BHCs as well.

Figure 3 illustrates the evolution of the interstate banking deregulation process, showing the cumulative fraction of state pairs in our sample that had removed entry restrictions between each other by each year, differentiating between methods of deregulation. Although Maine opened up its banking system to all states on a reciprocal manner in 1978, the fraction of state pairs that removed restrictions remained at zero until 1982, when New York reciprocated and put in place similar legislation.[21] The pace of interstate deregulation accelerated significantly in the second half of the 1980s, and by 1994 76 percent of the state pairs in our sample had removed restrictions to bank entry between each other. Moreover, Figure 3 shows that the most common form of deregulation was unilaterally opening entry to BHCs from all states (accounting for 60 percent of interstate banking deregulations in our sample), followed by nationwide reciprocal agreements (18 percent of deregulations).

3.2.2 Empirical Strategy: Timing of Interstate Banking Deregulation

To identify the causal effect of banking integration on the synchronization of economic activity we use the timing of interstate banking deregulation between two states as an instrument for their bilateral banking integration. As described above, different state pairs eliminated entry restrictions between each other at different points in time and as a result we have an instrument for each state pair in our sample. We hypothesize that state pairs that deregulated earlier have a greater degree of banking integration. Our first stage regression is given by:

$$Banking\ integration_{i,j,t} = \alpha_{i,j} + \delta_t + \beta * Deregulation_{i,j,t} + \mathbf{X'}_{i,j,t}\gamma + \varepsilon_{i,j,t}, \qquad (5)$$

where $Banking\ integration_{i,j,t}$ is a measure of the integration of state i and j's banking systems in year t; $Deregulation_{i,j,t}$ is a variable based on the timing of interstate banking deregulation between states i and j; and $\mathbf{X}_{i,j,t}$ are a set of state-pair time-varying controls. We also include time fixed effects (δ_t) to capture common national time-varying factors and state-pair fixed effects ($\alpha_{i,j}$) to account for time-invariant characteristics at the state-pair level.

[21] Although Alaska eliminated all entry restrictions in 1982, it is not included in Figure 3 because our sample is restricted to the 48 contiguous states.

We construct two sets of time-varying state-pair instruments based on the process of interstate banking deregulation. First, we use the number of years since a state pair removed entry restrictions and its square, to allow for a non-linear relationship between the time since deregulation and integration. Second, we implement a non-parametric specification, including separate dummy variables for each year since two states removed entry restrictions, all the way through the first ten years after deregulation.

The underlying assumption of our econometric strategy is that the timing of deregulation is not associated with expected changes in output synchronization between states, or with unobserved variables that might drive these changes. Several arguments support this hypothesis. First, as described above, deregulation occurred in a somewhat chaotic manner over time and through different methods. The most common form of deregulation was unilaterally opening entry to BHCs from *all* states. Changes in bilateral output synchronization with a particular state are unlikely to have played a role in the decision to allow entry by BHCs from all states. Second, empirical evidence suggests that deregulation was driven by political economy considerations related to the private benefits of local banks, and not by changes in economic conditions (Kroszner and Strahan, 1999).

To provide additional evidence, we examine whether the timing of deregulation between two states is associated with their level of output synchronization or its change, prior to deregulation. Specifically, for each state pair we first compute the median (a) level of and (b) change in our main synchronization measure over the five years prior to deregulation. We then account for state-specific differences by computing the within-state difference in these variables and in the timing of deregulation.[22] Figure

[22]We compute the within-state differences by subtracting the state-level mean from each of the state-pair variables. For instance, to calculate the within-state difference in the timing of deregulation, for each state pair i, j we take the difference between the year of interstate deregulation between states i and j and the average year of state i's deregulation with all states. We also take the difference between the year deregulation between states i and j and the average year of state j's deregulation with all states. Thus, each state pair in our sample is included twice in this analysis.

4 illustrates the relationship between these variables, plotting the within-state timing of interstate deregulation against (a) the within-state level of output synchronization before deregulation (top panel) and (b) the within-state change in synchronization before deregulation (bottom panel). The graphs are centered at zero because we account for within-state differences. Figure 4 shows that there is no relationship between the timing of interstate banking deregulation between two states and their prior levels of and changes in bilateral synchronization.

Our instrumental variables approach assumes that state pairs that deregulated earlier have a greater degree of bilateral banking integration. To test whether this is the case, we estimate the following regression:

$$Banking\ integration_{i,j,t} = \alpha_{i,j} + \delta_t + \sum_{r=-10}^{+10} \beta_r Y_{i,j,r,t} + \varepsilon_{i,j,t} \qquad (6)$$

where $Banking\ integration_{i,j,t}$ is the share of jointly-owned assets and deposits for state pair i, j in year t; $Y_{i,j,r,t}$ are dummy variables equal to one if in year t, states i and j deregulated r years before; δ_t and $\alpha_{i,j}$ are year and state-pair fixed effects, respectively. The coefficient on integration for the year of interstate banking deregulation is excluded due to collinearity, so the coefficients β_r capture differences relative to the year of deregulation. Standard errors are clustered at the state-pair level.

Figure 5 shows that the removal of interstate banking restrictions has a first order effect on the integration of state banking systems. This figure plots the estimated β_r coefficients from equation (6), as well as their 99 percent confidence interval. Banking integration does not change significantly prior to deregulation but, once states remove bilateral entry barriers, integration increases significantly over time.

3.2.3 2SLS Estimates

Table 4 reports the second stage results from our 2SLS estimation of the effects of banking integration on output co-movement, following the same structure as Table 2. We include state-pair and time fixed effects and the full set of controls used in Table 2. We use two alternative measures of banking integration: a dummy variable equal to one if the two states in a given pair have any common assets or deposits, and the share of jointly-owned assets and deposits. We present results using two alternative instruments: the number of years since a state pair removed entry restrictions and its square (Panel A) and separate dummy variables for each year since two states liberalized entry restrictions (Panel B).

Table 3 reports the first stage regression results for the different specifications and measures of banking integration presented in Table 4. Consistent with Figure 5, the results in Table 3 show that the removal of interstate banking restrictions has a significant positive effect on bilateral banking integration. These results hold across the different measures of integration and for the different sets of instruments, conditioning on state-pair and time fixed effects and the full set of controls. F-test statistics of the instruments' joint significance are very high, even in the regressions using the five-year correlation of real GDP growth as a measure of output synchronization (columns (5) and (6) of Table 3) where we only have four observations for each state pair.

The second stage results presented in Table 4 show that banking integration increases output synchronization between states. Different from the OLS results in Table 2, the estimated coefficients on the banking integration measures from our 2SLS estimations are positive and statistically significant in all the regressions, indicating that these results are robust to different specifications and alternative measures of output synchronization. Moreover, the estimated magnitudes are economically relevant. Consider, for instance, the results in column (4) of Panel A in Table 4. The estimated coefficient (14.146) implies that an increase in the share of jointly-owned assets and deposits between two states equal to its sample mean (0.025) leads to an increase in output synchronization (as measured by the negative absolute difference in residual real GDP growth) of 0.35, which is about 13 percent of the standard deviation of this variable. The finding of a positive causal effect of banking integration on the co-movement of economic activity between states suggests that integration contributed to the transmission of idiosyncratic shocks that affect financial constraints across state borders, making state economic fluctuations more similar.

Comparing the results in Table 4 to those in Table 2 shows that OLS estimates are biased downwards. Even in those specifications for which the OLS estimates are statistically significant (i.e., columns (1), (2), (3), (9), and (10)), the 2SLS estimates are between 2 and 16 times larger. The downward bias of OLS estimates suggests that, after controlling for state-pair fixed effects, financial integration is negatively correlated with output synchronization. This negative conditional correlation could arise, for instance, because banks might choose to expand into regions with different economic fluctuations than their home area in search of diversification (controlling for state-pair time-invariant characteristics).[23]

3.2.4 Robustness Checks and Extensions

We conducted several additional tests to confirm the robustness of our results, which are described in detail in Appendix A. First, we re-estimated our regressions using other

[23]In our regressions we account for cross-sectional differences across state pairs by including state-pair fixed effects. If we do not control for these fixed effects, we find a positive unconditional correlation between output synchronization and banking integration. This is consistent with the evidence that commonalities and proximity are among the most significant predictors of synchronization and financial integration (Baxter and Kouparitsas, 2005). Our results suggest that while (unconditionally) banks might be more likely to expand into geographically proximate states (Goetz et al., 2013) which are subject to similar economic fluctuations as their home states, once we account for time-invariant state-pair characteristics, banks are actually more likely to expand into states with lower output co-movement with their home states.

measures of synchronization. In particular, we constructed all our synchronization variables using (1) employment or (2) real personal income, instead of real GDP. We found results similar to those reported throughout the paper (Appendix Table A.1).

Second, we re-estimated our regressions using other measures of banking integration. In particular, we constructed our continuous measure of integration using (1) deposits or (2) bank assets, instead of their sum as in our main variable. We also constructed alternative measures of integration by scaling jointly-owned assets and deposits by the sums of (1) the GDP or (2) the population of the two states in a pair, alternatively. These results, presented in Appendix Table A.2, confirm our findings.

Third, our sample covers the period 1976 to 1994 because after 1994 we cannot identify the assets of a bank holding company in different states. As a robustness, we extended our sample to the period 1976-2007 using only deposits to construct our banking integration measures, because data on the geographic distribution of deposits are available for a longer period. We found results similar to those reported throughout the paper (Appendix Table A.3).

Fourth, we re-estimated our regressions controlling for interstate trade, as a large literature suggest that trade may affect output synchronization (Frankel and Rose, 1998; Clark and Wincoop, 2001; Imbs, 2004) and trade might also be correlated with financial integration (Rose and Spiegel, 2004; Aviat and Coeurdacier, 2007). Our findings are robust to controlling for interstate trade (Appendix Table A.4).

Fifth, the underlying assumption of our IV approach is that the timing of deregulation is not associated with (expected) changes in output synchronization between states, or with unobserved variables that might drive these changes. There might be some concerns that for those state pairs that deregulated through bilateral reciprocal agreements, the decision to deregulate could be correlated with changes in other forms of bilateral integration, which could affect output synchronization. To address this concern, we re-estimated our regressions excluding state pairs that deregulated through bilateral agreements. We also re-estimated our regressions restricting the sample to states that deregulated by unilaterally opening entry to BHCs from all states, because changes in synchronization with a particular state are unlikely to have driven this form of deregulation. These results, reported in Appendix Table A.5, confirm our findings.

Sixth, to account for any unobserved state-pair time-varying shocks that may be correlated with both financial integration and output synchronization, and that were not accounted for through our instrumental variables approach and the inclusion of state-pair linear time trends, we focused our analysis on differences in banking integration and output synchronization between state pairs that share a metropolitan statistical area, adapting the approaches by Huang (2008) and Michalski and Ors (2012) to our setting. These results, presented in Appendix Table A.6, are similar to those reported throughout the paper, indicating that our findings are not driven by time-varying regional shocks.

Finally, to further address concerns that our results might be affected by time-varying state-specific shocks, we estimated our regressions controlling for state-year fixed effects. The state-year fixed effects absorb a significant part of the variation in our deregulation instruments, because the most common form of deregulation was unilaterally opening up entry to BHCs from all states, which varies at the state-year level. Nevertheless, we confirm our findings when including these fixed effects (Appendix Table A.7).

4 Effect of Banking Integration on Output Synchronization: Differences across States and Industries

The results in Table 4 show that banking integration increases output synchronization between states. In this section, we analyze whether this effect varies across state pairs and industries to better understand what drives our findings.

4.1 Differences across States

As discussed above, theoretical arguments predict that the effect of financial integration on the synchronization of economic activity depends on the nature of the idiosyncratic shocks faced by different regions. Testing this prediction requires identifying periods when states face different types of shocks.

To identify financial shocks at the state level, we rely on aggregate measure of bank failures.[24] In particular, we first determine the total assets and deposits held by all commercial banks that failed in a given state and year. To do this, we combine data from the FDIC's Historical Statistics on Banking, which report detailed information on bank failures starting in 1934, with balance sheet data from the Call Reports. During our sample period there were 1,448 commercial bank failures in the United States, with average total assets and deposits of 254 million U.S. dollars at 1994 prices per failure. We add up the assets and deposits held by all failing banks in a given state and year and then scale this total amount by the state's GDP in the previous year.[25] This ratio is relatively low since large bank failures are infrequent, but shows

[24]Bank failures in some cases might have been driven by shocks to the real economy. As stressed by Cesa-Bianchi et al. (2019), in a two-country real business cycle model augmented with credit or collateral constraints, any country-specific shock that makes these constraints binding will lead to a positive effect of integration on synchronization, irrespective of whether it is a supply or demand shock or a shock to financial intermediaries. Thus, we interpret our aggregate measure of bank failures as proxying for how binding these constraints are, irrespective of the nature of the shock that caused the failures in the first place.

[25]We use lagged GDP as a denominator to avoid capturing the potential effects of bank failures on

large variation, both across states and over time within states. We classify a state as facing a financial shock in a given year if the ratio of total assets and deposits held by failing banks to lagged GDP exceeds two percent.[26] We consider a relatively high threshold for our classification because we want to clearly identify periods when state banking systems face distress. Based on this definition, 21 states are classified as having experienced financial shocks for an average of two years each over our sample period (see Appendix Table A.8 for the states and years included in this classification). This classification identifies states and periods when local banking crises in the U.S. are commonly considered to have occurred, including the Southern states (particularly Texas, Louisiana, and Oklahoma) in the second half of the 1980s (Grant, 1998) and New England in the early 1990s (Jordan, 1998).

To identify real shocks at the state level, we focus on the monetary losses caused by natural disasters, as these can be considered as exogenous shocks that affect a state's real economy. In particular, for each state and year we first determine the monetary losses caused by all natural disasters. To do this, we use data from the Spatial Hazard Events and Losses Database for the United States (SHELDUS), which is a county-level dataset that reports the date and monetary losses (including property and crop losses) for different types of natural hazard events, such as thunderstorms, hurricanes, floods, wildfires, and tornados. We aggregate the county-level losses up to the state level and then scale this total amount by the state's GDP in the previous year. We classify states as experiencing a real shock due to natural disasters in a given year if the ratio of total losses to lagged GDP exceeds 0.75 percent.[27] We consider a relatively high threshold for our classification as we want to identify periods when a state's real economy faces a large shock.[28] Based on this definition, 23 states are classified as having experienced

GDP. We obtain similar results if we used contemporaneous GDP as the denominator instead.

[26]See Appendix Table A.8 for summary statistics for this variable.

[27]See Appendix Table A.9 for summary statistics for this variable.

[28]Several papers have analyzed the short-run impact of natural disasters on economic activity, with

real shocks due to natural disasters at least once during our sample period (Appendix Table A.9 shows the states and years included in this classification).

To analyze whether the effect of financial integration on the synchronization of economic activity depends on the nature of the idiosyncratic shocks faced by different states, we estimate 2SLS regressions similar to those reported in Table 4 including the interaction between our measures of integration and different dummy variables that capture whether one (or both) state in a given pair experienced financial or real shocks, following the definitions described above.[29] Based on our classification, 19 percent of the state-pair year observations in our sample are classified as experiencing a financial shock and 8 percent are classified as experiencing a real shock due to natural disasters.[30] Table 5 presents the 2SLS results, showing estimations similar to those in columns (3) and (4) of Table 4 including the interaction terms.[31]

The results in Table 5 show that the effects of financial integration on output synchronization depend on the nature of the idiosyncratic shocks experienced by different states, consistent with theoretical arguments. In particular, the results in columns (1) and (2) show that the interaction between our integration measures and a dummy variable that captures whether (at least) one state in a given pair and year experi-

some papers documenting a negative effect (Raddatz, 2007; Hochrainer, 2009; Noy, 2009), while others find no or even positive effects, as a result of the stimulus generated by reconstruction efforts (Albala-Bertrand, 1993; Belasen and Polachek, 2009; Cavallo et al., 2013). Loayza et al. (2012) find that small disasters have a positive short-run effect on national economic growth, while large disasters have negative effects. For our analysis, we focus on periods when states experience large direct monetary losses from natural disasters. We find that the states and years included in our classification are associated with a decrease in state-level real GDP growth of about one percentage point (see Appendix Table A.10).

[29]In these regressions we have more than one endogenous variable (i.e., banking integration and the interaction between integration and the different dummies for financial or real shocks). Therefore, we use as an additional set of instruments the interaction between our instruments based on the timing of deregulation and the dummies that capture the different shocks.

[30]See Appendix Table A.11 for the number of states and state-pairs classified as experiencing different shocks in each year over our sample period.

[31]To keep the size of the table manageable we focus on our main measure of output synchronization (the negative absolute difference in residual real GDP growth between states) and only report results for one set of instruments (the number of years since deregulation and its square). We obtain similar results for other synchronization measures and instruments.

enced banking system distress is positive and statistically significant, indicating that banking integration increases output synchronization relatively more when states experience financial shocks. Columns (3) and (4) show that the interaction between our integration measures and a dummy variable that captures whether (at least) one state in a given pair and year experienced large losses due to natural disasters is negative and statistically significant, indicating that the effect of banking integration on output synchronization is smaller when states experience real shocks. Indeed, we find that the overall effect of integration (i.e., the sum of the coefficients on the integration variable and the interaction term) is not statistically significant when states experience large losses due to natural disasters. Columns (5) and (6) confirm our results when including the dummies and interactions for both financial and real shocks in the same regression.

We conducted several additional tests to confirm the robustness of our results, described in detail in Appendix A. First, we re-estimated our regressions considering alternative cut-offs to define periods when states face financial or real shocks. In particular, we classified states as facing a financial shock in a given year if the ratio of total assets and deposits held by failing banks to lagged GDP exceeds, alternatively, 1.5 or 2.5 percent (Appendix Table A.12). We also tried alternative definitions of real shocks, classifying states as experiencing a real shock due to natural disasters in a given year if the ratio of monetary losses from natural disasters to lagged GDP exceeds, alternatively, 0.5 or 1 percent (Appendix Table A.13). These results confirm our findings.

Second, as an alternative to exploiting natural disasters to identify real shocks, we also analyzed changes in state-level military spending driven by national military buildups and draw-downs. Nakamura and Steinsson (2014) show that these changes, which can be treated as exogenous from the perspective of a particular state, have significant multiplier effects on state GDP growth. We find that the effect of banking integration on output co-movement is smaller or not statistically significant when one (or both) of the states in a pair experiences large exogenous changes in military spending, consistent with the theoretical arguments (Appendix Tables A.14 and A.15).

4.2 Differences across Industries

The theoretical arguments outlined above suggest that multi-market banks transmit shocks across states through changes in their lending. In this case, we would expect integration to have a larger effect on synchronization for those industries that rely more on bank financing. To test this hypothesis, we construct measures of synchronization between states for different industry groups based on their dependence on external financing.

We first calculate the dependence on external finance at the industry level following the methodology of Rajan and Zingales (1998).[32] Then, we define high (low) financial dependence industries as those that are above (below) the median level of external financial dependence across all industries. Based on this classification, we calculate the aggregate GDP of high and low financial dependence industries for each state and year, by summing up the GDP of all the industries in each category. We then calculate the real GDP growth of these two groups of industries for each state and year and use these data to construct our measures of output synchronization between states.[33] Thus, we have two measures of synchronization for each state pair and year, one for industries with high financial dependence and one for industries with low dependence.[34]

[32] Using data from Compustat for the period 1980-1990, we aggregate firm-level data on reliance on external funds (proxied by the fraction of investment not financed with funds from operations) up to the two-digit SIC sector, which gives us a sample of 72 industries.

[33] To calculate the residual real GDP growth, we estimate separate regressions of the real GDP growth of each industry category in a state and year, on state and year fixed effects.

[34] An alternative to constructing industry categories based on external financial dependence would be to conduct our analyses at the state-industry-year level. We aggregate the data into categories because many industries are very small in some states and therefore their annual growth rates are very volatile. Using these industry-level growth rates to construct bilateral synchronization measures would likely introduce measurement error.

To analyze whether the effect of financial integration on the synchronization of economic activity varies across industries, we estimate 2SLS regressions similar to those reported in Table 4 separately for industries with high and low dependence on external finance. These results are presented in Table 6, which shows regressions similar to those reported in columns (3) and (4) of Table 4 for the different industry categories.

The results in Table 6 show that, consistent with our hypothesis, the effect of banking integration varies across industries depending on their dependence on external finance. In particular, the results in columns (1) and (2) show that integration has a positive effect on output synchronization between states for those industries that rely relatively more on external finance. In contrast, the results in columns (3) and (4) show that integration does not have a significant effect on synchronization for industries with low dependence on external finance.[35] This pattern is consistent with the argument that multi-market banks transmit shocks across states through changes in their lending, and that this affects more those industries that rely more on bank financing.

5 Banking Integration and Output Synchronization: Evidence on Underlying Mechanisms

The results reported throughout the paper are consistent with the idea that multi-market bank holding companies operate internal capital markets and respond to shocks originating in one state by changing their lending in other states where they are active. This creates a commonality in aggregate lending among these states, which then increases their output synchronization (to the extent that bank lending affects economic

[35] We also estimated similar 2SLS regressions considering two observations for each state pair and year (one for high financial dependence industries and one for low dependence industries), instead of conducting separate regression for each industry category as in Table 6. Our instrumental variables only vary at the state-pair level. So for these 2SLS regressions we use a split-sample IV approach (Angrist and Krueger, 1994) where we first use our set of instruments to estimate the exogenous component of banking integration at the state-pair level and then use this predicted integration in an OLS regression at the industry-category state-pair year level. These results confirm our findings.

activity). We provide evidence on this underlying channel by analyzing whether banking integration (1) increases the similarity of bank lending fluctuations between states and (2) contributes to the transmission of bank funding shocks across state borders.

5.1 Banking Integration and Lending Synchronization between States

To analyze whether integration increases the similarity of state-level fluctuations in bank lending, we first compute the total Commercial and Industrial (C&I) loans by banks in a given state and year by aggregating bank-level data from the Call Reports. We focus on C&I loans following the literature on the bank lending channel in the U.S. (Kashyap and Stein, 2000; Driscoll, 2004). Over our sample period, C&I loans accounted for about 28 percent of total lending by commercial banks. We then calculate the growth rate of real C&I lending for each state and year and use these data to construct our main measure of synchronization. We estimate 2SLS regressions similar to those in Table 4, using C&I lending synchronization between states as the dependent variable. These results are presented in Table 7.[36]

We find that banking integration increases the synchronization of C&I lending between states. In particular, the results in columns (1) and (2) of Table 7 (which follow the same specifications as columns (3) and (4) of Table 4) show that the coefficients on the different integration measures are positive and statistically significant. One potential concern about these results is that our main findings show that banking integration increases output synchronization. Thus, the finding that integration leads to a higher co-movement of C&I lending between states may just reflect the higher co-movement of output between states. To try to address this concern, in columns (3) and (4) of Table 7 we report results including the lagged value of output synchronization between

[36]Given that bank balance sheet data from the Call Reports are available starting in 1976, we lose one annual observation when computing the growth rate of C&I lending. Therefore, the sample for this analysis covers the period 1977 to 1994.

states as an additional control variable. We find that banking integration has a significant positive effect on C&I lending synchronization between states, even controlling for lagged output synchronization.

5.2 Banking Integration and the Transmission of Bank Funding Shocks across States

This section analyzes whether banking integration contributes to the transmission of bank funding shocks across state borders. In particular, we analyze whether aggregate C&I bank lending in a state responds to changes in aggregate deposits in other states with which it is financially integrated. Note that this analysis has to be conducted at the state-year level, and not at the state-pair year level as the rest of the analyses in the paper. In particular, we estimate the following baseline regression model:

$$\begin{aligned} Bank\ loan\ growth_{i,t} = \beta * Deposit\ growth\ in\ financially\ integrated\ states_{i,t-1} \\ + \alpha_i + \delta_t + \varepsilon_{i,t} \quad (7) \end{aligned}$$

where $Bank\ loan\ growth_{i,t}$ is the growth rate of real C&I loans in state i in year t; $Deposit\ growth\ in\ financially\ integrated\ states_{i,t-1}$ is a lagged measure of the deposit growth in other states with which state i is financially integrated. In particular, for each state i and year t we take the weighted average of the growth rate of real state-level bank deposits across all other states, using as weights the share of jointly-owned assets and deposits between state i and each state. We also include time (δ_t) and state fixed effects (α_i) to account, respectively, for common national time-varying factors and state time-invariant characteristics. Standard errors are clustered at the state level.

A key empirical challenge for this analysis is how to distinguish the transmission of shocks through the internal capital markets of multi-state bank holding companies from common factors that affect states that are financially integrated. For instance, states are more likely to integrate with geographically close states which might be subject to similar macroeconomic shocks and, as a result, lending and deposits in financially integrated states may move together, even if BHCs are not transmitting shocks across state borders.[37]

To overcome this challenge, we use a second identification strategy that exploits the process of interstate banking deregulation, following Goetz et al. (2013, 2016). Specifically, we first estimate an OLS regression of the banking integration (i.e., share of jointly-owned assets and deposits) between two states on the number of years since the liberalization of bilateral interstate banking restrictions and its square, state-pair fixed effects, year fixed effects, state-pair linear time trends, and other state-pair controls.[38] Using the estimated coefficients from this regression, we then generate the predicted level of banking integration between two states and impose a zero for state pairs that do not allow interstate banking. Then, for each state i and year t we calculate the weighted average of the growth rate of real state-level bank deposits across all other states, using as weights the predicted share of jointly-owned assets and deposits between state i and each state. Finally, we use this predicted weighted average deposit growth rate as an instrument for the actual weighted average deposit growth rate.

Table 8 presents OLS and 2SLS results of estimating equation (7). Columns (1) and (2) show OLS regressions, while columns (3) to (6) show 2SLS regressions using the instrumental variable described above. Columns (2), (4), (5), and (6) include Census division-year fixed effects to control for regional time-varying shocks. Columns (5) and (6) also include state-level linear time trends to control for other time-varying factors

[37] In all the other analyses reported throughout the paper we account for common time-invariant factors between states such as distance by including state-pair fixed effects; we cannot do this for the analysis in this section, as it is conducted at the state-year level.

[38] We follow the specification in column (4) of Table 3.

at the state level.

The OLS regressions in Table 8 show that there is a positive correlation between aggregate lending in a state and aggregate deposit growth in other states with which it is financially integrated, but this correlation seems to reflect common regional shocks. In particular, the results in column (1) show that the coefficient on our measure of the deposit growth in other states with which state i is financially integrated (i.e., the weighted average of the growth rate of real state-level bank deposits across all other states) is positive and statistically significant. However, this coefficient loses its statistical significance once we control for regional time-varying shocks (column (2)).

The 2SLS results in Table 8 show that aggregate lending in a state responds to changes in deposits in other states with which it is financially integrated. The first-stage results in Panel B indicate that the instrumental variable constructed following the approach described above explains the actual deposit growth in other states with which state i is financially integrated. The second stage results in Panel A show that the coefficients on deposit growth in financially integrated states is positive and significant in all specifications. Different from the OLS estimates, these results are robust to controlling for Census division-year fixed effects (column (4)). In addition, in column (5) we also include state-linear time trends to account for unobservable time-varying factors at the state level and find that this does not affect our results. Furthermore, in column (6) we also control for the lagged growth rate of deposits in state i to capture state-level funding shocks. We find that this does not affect our results, suggesting that our findings reflect the transmission of shocks to deposits in other states, and not common shocks that affect deposits in both state i and other states with which it is financially integrated. Overall, these findings suggest that banking integration facilitates capital mobility, fostering the propagation of bank funding shocks across states.

6 Conclusion

This paper analyzes the effect of the geographic expansion of banks across U.S. states on the co-movement of economic activity between states. Estimating the causal effect of banking integration on output co-movement raises a number of empirical challenges, which we address by exploiting cross-state, cross-time variation in the removal of interstate banking restrictions to construct instrumental variables to identify exogenous changes in banking integration over time at the state-pair level. Using this approach, we find that banking integration increases output synchronization between states. These findings are consistent with the argument that integration contributed to the transmission of financial shocks across states, making state economic fluctuations more alike.

We also find that the effect of financial integration depends on the nature of the idiosyncratic shocks faced by different states and also varies across industries. In particular, our results show that, consistent with theoretical arguments, the effect of bilateral banking integration on output synchronization between two states is larger when at least one of the states faces financial shocks, whereas this effect is smaller or not statistically significant when one (or both) of the states faces real shocks. Our results also show that financial integration has a strong positive effect on output synchronization for industries with a high dependence on external finance, while it does not have a statistically significant effect for industries that are less dependent on external financing. These findings stress the role of shock transmission through financial intermediaries in accounting for the positive effect of integration on output co-movement.

Finally, we find that integration increases the similarity of fluctuations in bank lending between states and that aggregate lending in a state responds to deposit changes in other states with which it is financially integrated. These findings are consistent with the idea that banks operating in several states transmit shocks across states through their internal capital markets, creating a commonality in lending among these states, which then increases output synchronization to the extent that lending affects economic activity.

Our findings provide novel information on the effects of interstate banking deregulation and financial integration across U.S. states and also offer insights about current policy debates. In particular, our findings indicate that increased integration following interstate banking deregulation contributed to capital flows through banks across states and highlight the role of multi-market banks in the geographic transmission of shocks. Our results also show that increased financial integration can contribute to making economic fluctuations more alike, especially during periods of systemic bank distress. Future research may explore the extent to which our findings may reflect the particular types of shocks that drove local economic fluctuations in the U.S. over our sample period and the way in which banking integration across U.S. states occurred, to understand whether these findings may apply to other settings and, in particular, to international integration through cross-border banking.

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Table 1
Summary Statistics

This table shows descriptive statistics for the main variables used in our empirical analyses. Observations at the state-pair and year level. Sample covers the 48 contiguous U.S. states (excluding Delaware and South Dakota) over the period 1976-1994.

	N	Mean	Std. Deviation	1st Percentile	99th Percentile	Median
Banking integration measures						
Dummy =1 if jointly-owned assets or deposits between states	19,665	0.18	0.38	0.00	1.00	0.00
Share of jointly-owned assets and deposits (jointly-owned assets and deposits/sum of assets and deposits of both states)	19,665	0.025	0.075	0.000	0.444	0.000
Output co-movement measures						
Negative absolute difference in residual real GDP growth between states * 100	19,665	-2.97	2.76	-14.62	-0.04	-2.21
Instantaneous quasi-correlation of real GDP growth between states * 100	19,665	39.4	104.8	-242.8	508.8	15.8
Five-year correlation of real GDP growth between states * 100	4,140	57.3	44.4	-70.3	99.4	75.3
Additional state-pair controls						
Difference in industry employment shares between states	19,665	0.11	0.05	0.03	0.28	0.10
Dummy =1 if intrastate branching allowed in any of the two states	19,665	0.40	0.49	0.00	1.00	0.00
ln(GDP of state *i*)*ln(GDP of state *j*)	19,665	21.5	1.6	17.8	25.2	21.5

Table 2
Interstate Banking Integration and Output Co-movement between States
OLS Regressions

This table reports OLS regressions at the state-pair and year level. Columns (1) to (4) dependent variable: negative absolute difference in residual real GDP growth between two states. Columns (5) to (8) dependent variable: instantaneous quasi-correlation of real GDP growth between two states. Columns (9) and (10) dependent variable: forward-looking five-year correlation of real GDP growth between two states. Standard errors clustered at the state-pair level in parentheses. *, **, *** denote significance at ten, five, and one percent level, respectively. Sample covers the 48 contiguous states of the United States (excluding Delaware and South Dakota) over the period 1976-1994.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Negative absolute difference in residual real GDP growth between states				Instantaneous quasi-correlation of real GDP growth between states				Five-year correlation of real GDP growth between states	
Dummy =1 if jointly-owned assets or deposits between states	0.332***		0.186*		2.137		-3.947		6.724**	
	(0.082)		(0.098)		(2.621)		(3.725)		(2.629)	
Share of jointly-owned assets and deposits		0.861**		0.299		7.086		-23.445		27.288**
		(0.424)		(0.608)		(15.892)		(24.745)		(13.724)
Difference in industry employment shares between states	-0.016***	-0.016***	-0.051***	-0.051***	-0.595***	-0.595***	-1.756***	-1.758***	-0.105***	-0.104***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.053)	(0.054)	(0.152)	(0.152)	(0.039)	(0.039)
ln(GDP of state *i*)*ln(GDP of state *j*)	-1.826***	-1.838***	-2.232***	-2.222***	25.213***	25.090***	29.174**	29.360**	-4.013	-4.376
	(0.168)	(0.170)	(0.292)	(0.293)	(5.606)	(5.636)	(12.227)	(12.287)	(3.486)	(3.499)
Dummy =1 if intrastate branching allowed in any of the two states	0.491***	0.498***	0.377***	0.381***	17.758***	17.805***	20.234***	20.155***	11.802***	12.070***
	(0.065)	(0.066)	(0.068)	(0.068)	(2.165)	(2.161)	(2.505)	(2.505)	(1.918)	(1.914)
State-pair fixed effects	x	x	x	x	x	x	x	x	x	x
Year fixed effects	x	x	x	x	x	x	x	x	x	x
State-pair linear time trends			x	x			x	x		
Observations	19,665	19,665	19,665	19,665	19,665	19,665	19,665	19,665	4,140	4,140

Table 3
Interstate Banking Integration and Output Co-movement between States
2SLS Regressions - First Stage

This table reports first stage regression results from 2SLS analysis at the state-pair and year level. Columns (1), (3), and (5) endogenous variable: banking integration measure 'Dummy =1 if jointly-owned assets or deposits between states'. Columns (2), (4), and (6) endogenous variable: banking integration measur 'Share of jointly-owned assets and deposits'. Columns (5) and (6) consider observations every five years. Excluded instruments in Panel A: number of years since the liberalization of interstate restrictions between two states and its square. Excluded instruments in Panel B: separate dummy variables for each year since two states liberalized entry restrictions. State pair controls: product of the natural logarithm of real GDP of the two states, difference in industry employment shares between states, and dummy variable equal to one after at least one of the states in a given state pair eliminates restrictions to intrastate branching. Standard errors clustered at the state-pair level in parentheses. *, **, *** denote significance at ten, five, and one percent level, respectively. Sample covers the 48 contiguous states of the United States (excluding Delaware and South Dakota) over the period 1976-1994.

Panel A: Excluded instruments - Years since interstate deregulation						
	(1)	(2)	(3)	(4)	(5)	(6)
	Dummy =1 if jointly-owned assets or deposits between states	Share of jointly-owned assets and deposits	Dummy =1 if jointly-owned assets or deposits between states	Share of jointly-owned assets and deposits	Dummy =1 if jointly-owned assets or deposits between states	Share of jointly-owned assets and deposits
Years since interstate deregulation	0.056***	0.007***	0.067***	0.009***	0.048***	0.005***
	(0.006)	(0.001)	(0.007)	(0.001)	(0.011)	(0.002)
Years since interstate deregulation	-0.001*	0.001***	-0.003***	0.000	0.001	0.001***
squared	(0.001)	(0.000)	(0.001)	(0.000)	(0.002)	(0.000)
State-pair fixed effects	x	x	x	x	x	x
Year fixed effects	x	x	x	x	x	x
State-pair controls	x	x	x	x	x	x
State-pair linear time trends			x	x		
Observations	19,665	19,665	19,665	19,665	4,140	4,140
F-test of instruments' joint significance	71.41	67.21	58.32	76.10	53.23	50.49

Table 3 (continued)
Interstate Banking Integration and Output Co-movement between States
2SLS Regressions - First Stage

Panel B: Excluded instruments - Years since interstate deregulation [nonparametric]						
	(1)	(2)	(3)	(4)	(5)	(6)
	Dummy =1 if jointly-owned assets or deposits between states	Share of jointly-owned assets and deposits	Dummy =1 if jointly-owned assets or deposits between states	Share of jointly-owned assets and deposits	Dummy =1 if jointly-owned assets or deposits between states	Share of jointly-owned assets and deposits
Dummy =1 if year state pair deregulated	0.037***	0.003**	0.048***	0.004***	0.094***	0.021***
interstate banking	(0.010)	(0.001)	(0.010)	(0.001)	(0.030)	(0.005)
Dummy =1 if 1 year after state pair	0.119***	0.016***	0.128***	0.016***	0.090**	0.027***
deregulated interstate banking	(0.014)	(0.002)	(0.014)	(0.002)	(0.036)	(0.008)
Dummy =1 if 2 years after state pair	0.173***	0.026***	0.181***	0.025***	0.070**	0.016***
deregulated interstate banking	(0.017)	(0.003)	(0.018)	(0.003)	(0.028)	(0.006)
Dummy =1 if 3 years after state pair	0.202***	0.036***	0.211***	0.034***	0.246***	0.032***
deregulated interstate banking	(0.019)	(0.003)	(0.021)	(0.003)	(0.052)	(0.007)
Dummy =1 if 4 years after state pair	0.238***	0.042***	0.251***	0.041***	0.279***	0.041***
deregulated interstate banking	(0.022)	(0.004)	(0.024)	(0.004)	(0.035)	(0.005)
Dummy =1 if 5 years after state pair	0.271***	0.055***	0.274***	0.050***	0.427***	0.113***
deregulated interstate banking	(0.026)	(0.005)	(0.028)	(0.005)	(0.060)	(0.015)
Dummy =1 if 6 years after state pair	0.335***	0.074***	0.311***	0.065***	0.587***	0.199***
deregulated interstate banking	(0.030)	(0.006)	(0.033)	(0.005)	(0.097)	(0.032)
Dummy =1 if 7 years after state pair	0.412***	0.094***	0.375***	0.080***	0.268***	0.049***
deregulated interstate banking	(0.034)	(0.008)	(0.037)	(0.006)	(0.069)	(0.014)
Dummy =1 if 8 years after state pair	0.421***	0.123***	0.332***	0.082***	0.704***	0.211***
deregulated interstate banking	(0.043)	(0.012)	(0.042)	(0.008)	(0.028)	(0.040)
Dummy =1 if 9 years after state pair	0.421***	0.118***	0.326***	0.078***	0.727***	0.153***
deregulated interstate banking	(0.052)	(0.015)	(0.047)	(0.009)	(0.024)	(0.004)
Dummy =1 if 10 years or more after	0.424***	0.096***	0.345***	0.077***		
state pair deregulated interstate banking	(0.064)	(0.015)	(0.073)	(0.010)		
State-pair fixed effects	x	x	x	x	x	x
Year fixed effects	x	x	x	x	x	x
State-pair controls	x	x	x	x	x	x
State-pair linear time trends			x	x		
Observations	19,665	19,665	19,665	19,665	4,140	4,140
F-test of instruments' joint significance	15.49	15.07	14.30	15.55	159.6	301.9

Table 4

Interstate Banking Integration and Output Co-movement between States

2SLS Regressions - Second Stage

This table reports 2nd stage regression results from 2SLS analysis at the state-pair and year level. Columns (1) to (4) dependent variable: negative absolute difference in residual real GDP growth between two states. Columns (5) to (8) dependent variable: instantaneous quasi-correlation of real GDP growth between two states. Columns (9) and (10) dependent variable: forward-looking five-year correlation of real GDP growth between two states. Endogenous variables: banking integration measures 'Dummy =1 if jointly-owned assets or deposits between states' and 'Share of jointly-owned assets and deposits'. Excluded instruments in Panel A: number of years since the liberalization of interstate restrictions between two states and its square. Excluded instruments in Panel B: separate dummy variables for each year since two states liberalized entry restrictions. State pair controls: product of the natural logarithm of real GDP of the two states, difference in industry employment shares between states, and dummy variable equal to one after at least one of the states in a given state pair eliminates restrictions to intrastate branching. Standard errors clustered at the state-pair level in parentheses. *, **, *** denote significance at ten, five, and one percent level, respectively. Sample covers the 48 contiguous states of the United States (excluding Delaware and South Dakota) over the period 1976-1994.

Panel A: Excluded instruments - Years since interstate deregulation and its square

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Negative absolute difference in residual real GDP growth between states				Instantaneous quasi-correlation of real GDP growth between states				Five-year correlation of real GDP growth between states	
Dummy =1 if jointly-owned assets or deposits between states	2.132***		2.981***		34.205***		59.125***		33.600***	
	(0.352)		(0.611)		(9.669)		(22.221)		(8.434)	
Share of jointly-owned assets and deposits		8.695***		14.146***		136.879***		275.298**		153.676***
		(1.555)		(3.241)		(41.463)		(125.655)		(42.093)
State-pair fixed effects	x	x	x	x	x	x	x	x	x	x
Year fixed effects	x	x	x	x	x	x	x	x	x	x
State-pair controls	x	x	x	x	x	x	x	x	x	x
State-pair linear time trends			x	x			x	x		
Observations	19,665	19,665	19,665	19,665	19,665	19,665	19,665	19,665	4,140	4,140
F-test of instruments' joint significance	71.41	67.21	58.32	76.10	71.41	67.21	58.32	76.10	53.23	50.49

Panel B: Excluded instruments - Years since interstate deregulation [nonparametric]

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Negative absolute difference in residual real GDP growth between states				Instantaneous quasi-correlation of real GDP growth between states				Five-year correlation of real GDP growth between states	
Dummy =1 if jointly-owned assets or deposits between states	2.217***		2.863***		32.602***		46.595**		31.714***	
	(0.356)		(0.595)		(9.669)		(21.364)		(7.411)	
Share of jointly-owned assets and deposits		8.690***		11.597***		149.782***		293.487**		61.124**
		(1.498)		(2.968)		(42.546)		(116.951)		(25.751)
State-pair fixed effects	x	x	x	x	x	x	x	x	x	x
Year fixed effects	x	x	x	x	x	x	x	x	x	x
State-pair controls	x	x	x	x	x	x	x	x	x	x
State-pair linear time trends			x	x			x	x		
Observations	19,665	19,665	19,665	19,665	19,665	19,665	19,665	19,665	4,140	4,140
F-test of instruments' joint significance	15.49	15.07	14.30	15.55	15.49	15.07	14.30	15.55	159.6	301.9

Table 5
Interstate Banking Integration and Output Co-movement between States
2SLS Regressions - Second Stage
Differences between State-Pairs

This panel reports 2nd stage regression results from 2SLS regressions at the state-pair and year level. Dependent variable: negative absolute difference in residual real GDP growth between two states. Endogenous variables: banking integration measures 'Dummy =1 if jointly-owned assets or deposits between states' and 'Share of jointly-owned assets and deposits', and their interactions with dummy variables financial and/or real shocks in a state. Excluded instruments: number of years since the liberalization of interstate restrictions between two states and its square, and their interactions with dummy variables for financial and/or real shocks. State pair controls: product of the natural logarithm of real GDP of the two states, difference in employment shares between states, and dummy variable equal to one after at least one of the states in a given state pair eliminates restrictions to intrastate branching. 'Dummy =1 if real shocks associated with natural disasters' is a dummy variable equal to one if the ratio of total monetary losses due to natural disasters to lagged GDP exceeds 0.75 percent in (at least) one of the states in a given state pair and year. 'Dummy =1 if financial shocks associated with bank failures' is a dummy variable equal to one if the ratio of total assets and deposits held by failing banks to lagged GDP exceeds two percent in (at least) one of the states in a given state pair and year. Standard errors clustered at the state-pair level in parentheses. *, **, *** denote significance at ten, five, and one percent level, respectively. Sample covers the 48 contiguous states of the United States (excluding Delaware and South Dakota) over the period 1976-1994.

	(1)	(2)	(3)	(4)	(5)	(6)
	Negative absolute difference in residual real GDP growth between states					
	Financial shocks		Real shocks		Financial and real shocks	
Dummy =1 if jointly-owned assets or deposits between states (a)	2.529***		3.042***		2.542***	
	(0.598)		(0.609)		(0.596)	
Dummy =1 if jointly-owned assets or deposits between states * Dummy =1 if financial shocks associated with bank failures (b)	2.376***				2.550***	
	(0.619)				(0.624)	
Dummy =1 if jointly-owned assets or deposits between states * Dummy =1 if real shocks associated with natural disasters (c)			-1.936***		-2.278***	
			(0.713)		(0.741)	
Share of jointly-owned assets and deposits (a)		11.354***		15.126***		12.344***
		(3.112)		(3.289)		(3.151)
Share of jointly-owned assets and deposits * Dummy =1 if financial shocks associated with bank failures (b)		6.455**				7.556***
		(2.528)				(2.602)
Share of jointly-owned assets and deposits * Dummy =1 if real shocks associated with natural disasters (c)				-7.731**		-9.403***
				(3.181)		(3.313)
Dummy =1 if financial shocks associated with bank failures	-0.945***	-0.582***			-0.995***	-0.620***
	(0.153)	(0.092)			(0.153)	(0.093)
Dummy =1 if real shocks associated with natural disasters			0.071	-0.076	0.089	-0.068
			(0.127)	(0.093)	(0.129)	(0.094)
State-pair fixed effects	x	x	x	x	x	x
Year fixed effects	x	x	x	x	x	x
State-pair controls	x	x	x	x	x	x
State-pair linear time trends	x	x	x	x	x	x
Observations	19,665	19,665	19,665	19,665	19,665	19,665
Effect of banking integration if financial shocks associated with bank failures (a)+(b) [test (a)+(b)=0 p-value]	4.905***	17.81***			5.092***	19.90***
	[0.000]	[0.000]			[0.000]	[0.000]
Effect of banking integration if real shocks associated with natural disasters (a)+(c) [test (a)+(c)=0 p-value]			1.106	7.395*	0.264	2.941
			[0.214]	[0.0682]	[0.775]	[0.472]
Excluded instruments						
Years since interstate deregulation	x	x	x	x	x	x
Years since interstate deregulation squared	x	x	x	x	x	x
Interactions	x	x	x	x	x	x
F-test of instruments' joint significance	29.21	38.11	28.97	38.60	19.36	25.82

Table 6
Interstate Banking Integration and Output Co-movement between States
2SLS Regressions - Second Stage
Differences between Industries: External Finance Dependence

This table reports 2nd stage regression results from 2SLS regressions at the state-pair and year level. Industries are classified in two groups: High/low external finance dependence based on whether they are above/below the median across industries of the Rajan and Zingales (1998) external finance dependence measure. We aggregate the GDP of all industries in each of these groups in a given state and year and calculate the aggregate real growth rate of high/low financial dependence industries. These series are then used to construct our measure of output co-movement at the state-pair, year, and industry-type level. Endogenous variables: banking integration measures 'Dummy =1 if jointly-owned assets or deposits between states' and 'Share of jointly-owned assets and deposits'. Excluded instruments: number of years since the liberalization of interstate restrictions between two states and its square. State pair controls: product of the natural logarithm of real GDP of the two states, difference in employment shares between states, and dummy variable equal to one after at least one of the states in a given state pair eliminates restrictions to intrastate branching. Standard errors clustered at the state-pair level in parentheses. *, **, *** denote significance at ten, five, and one percent level, respectively. Sample covers the 48 contiguous states of the United States (excluding Delaware and South Dakota) over the period 1976-1994.

	(1)	(2)	(3)	(4)
	Negative absolute difference in residual real GDP growth between states			
	Industries with high external finance dependence		Industries with low external finance dependence	
Dummy =1 if jointly-owned assets or deposits between states	3.462**		0.233	
	(1.683)		(1.049)	
Share of jointly-owned assets and deposits		34.844***		-2.570
		(9.179)		(5.462)
State-pair fixed effects	x	x	x	x
Year fixed effects	x	x	x	x
State-pair controls	x	x	x	x
State-pair linear time trends	x	x	x	x
Observations	19,665	19,665	19,665	19,665
Excluded instruments				
Years since interstate deregulation	x	x	x	x
Years since interstate deregulation squared	x	x	x	x
F-test of instruments' joint significance	58.32	76.10	58.32	76.10

Table 7
Interstate Banking Integration and Bank Lending Co-Movement between States
2SLS Regressions - Second Stage

This table reports 2nd stage regression results from 2SLS regressions at the state-pair and year level. Dependent variable: negative absolute difference in residual real growth rate of Commercial and Industrial (C&I) loans between two states. Endogenous variables: banking integration measures 'Dummy =1 if jointly owned assets or deposits between states' and 'Share of jointly-owned assets and deposits'. Excluded instruments: number of years since the liberalization of interstate restrictions between two states and its square. State pair controls: product of the natural logarithm of real GDP of the two states, difference in industry employment shares between states, and dummy variable equal to one after at least one of the states in a given state pair eliminates restrictions to intrastate branching. Standard errors clustered at the state-pair level in parentheses. *, **, *** denote significance at ten, five, and one percent level, respectively. Sample covers the 48 contiguous states of the United States (excluding Delaware and South Dakota) over the period 1977-1994.

	(1)	(2)	(3)	(4)
	Negative absolute difference in residual real C&I loan growth rates between states			
Dummy =1 if jointly-owned assets or deposits between states	6.060*** (1.637)		5.264*** (1.589)	
Share of jointly-owned assets and deposits		45.140*** (9.313)		40.001*** (8.815)
Negative absolute difference in residual real GDP growth between states (lag)			0.307*** (0.024)	0.309*** (0.024)
State-pair fixed effects	x	x	x	x
Year fixed effects	x	x	x	x
State-pair controls	x	x	x	x
State-pair linear time trends	x	x	x	x
Observations	18,630	18,630	18,630	18,630
Excluded instruments				
Years since interstate deregulation	x	x	x	x
Years since interstate deregulation squared	x	x	x	x
F-test of instruments' joint significance	55.66	73.74	55.59	73.72

Table 8

State-level C&I Loan Growth and Deposit Growth in Financially Integrated States

This table reports regressions at the state and year level following equation (7) in the main text of the paper. Dependent variable: annual real growth of state-level Commercial and Industrial (C&I) loans. Columns (1) and (2) report OLS regressions. Columns (3) to (6) report 2SLS regressions. Endogenous variable: Average real deposit growth of other states, weighted by bilateral share of jointly-owned assets and deposits. Excluded instrument: Average real deposit growth rate of other states, weighted by predicted bilateral share of jointly-owned assets and deposits. To construct the predicted share, we first estimate a regression of the share of jointly-owned assets and deposits between two states on state-pair fixed effects, year fixed effects, state-pair linear time trends, the number of years since the liberalization of interstate banking restrictions between two states and its square, and other state-pair controls. Using the coefficients from this regression, we generate the predicted share of jointly-owned assets and deposits for each state pair, imposing a zero for state pairs that do not allow interstate banking. First-stage results are reported in Panel B. Standard errors clustered at the state-pair level in parentheses. *, **, *** denote significance at ten, five, and one percent level, respectively. Sample covers the 48 contiguous states of the United States (excluding Delaware and South Dakota) over the period 1976-1994.

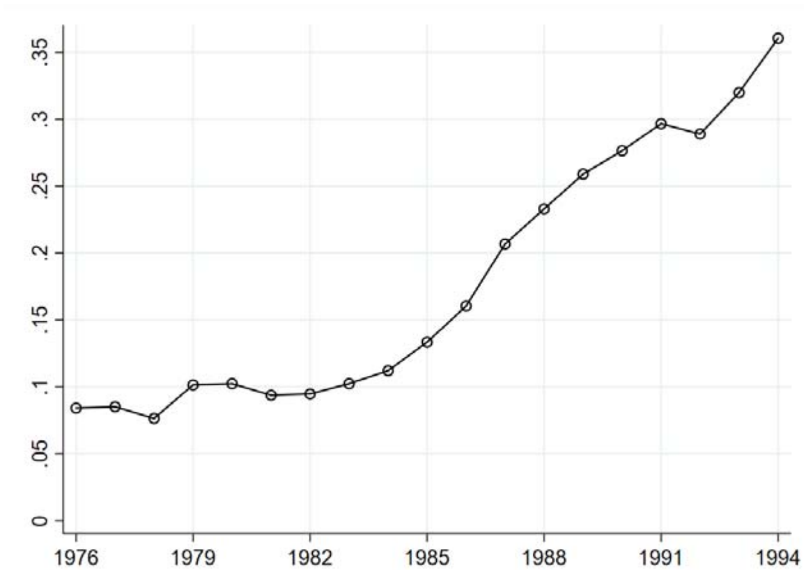
Panel A: OLS and 2nd stage results from 2SLS regressions						
	(1)	(2)	(3)	(4)	(5)	(6)
	State-level C&I loans real growth					
	OLS		2SLS			
Average real deposit growth of other states, weighted by share of jointly-owned assets and deposits (lag)	0.252***	0.128	0.490***	0.457**	0.442**	0.354**
	(0.076)	(0.135)	(0.153)	(0.204)	(0.198)	(0.160)
State-level real deposit growth (lag)						0.320*
						(0.161)
State fixed effects	x	x	x	x	x	x
Year fixed effects	x		x			
Census region-year fixed effects		x		x	x	x
State linear time trends					x	x
Observations	782	782	782	782	782	782
F-test of instruments' joint significance			1023	176.7	134.9	140.7

Panel B: First stage - Excluded instruments						
Average real deposit growth rate of other states, weighted by predicted share of jointly-owned assets and deposits (lag)			1.061***	1.012***	1.031***	1.028***
			(0.033)	(0.076)	(0.089)	(0.087)

Figure 1
Evolution of Banking Integration between States

This figure illustrates the evolution of banking integration between states. Panel A shows the fraction of all unique state pairs in our sample that were integrated (i.e., had any jointly-owned bank assets or deposits) in each year over the period 1976-1994. Sample covers the 48 contiguous states of the United States (excluding Delaware and South Dakota). Panel B shows the evolution of the share of jointly-owned assets and deposits between California and three other states (Florida, Texas, and Washington) over our sample period.

Panel A: Fraction of State Pairs with Jointly-Owned Assets or Deposits



Panel B: Share of Jointly-Owned Assets and Deposits between Selected State Pairs

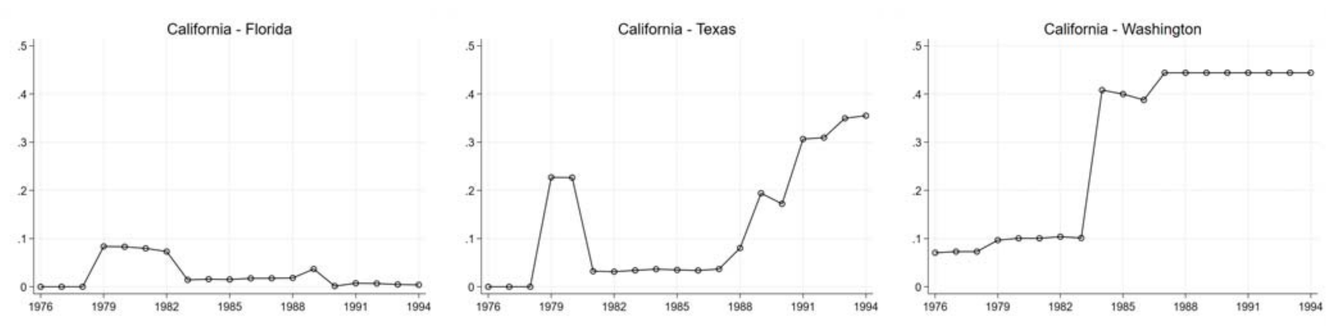
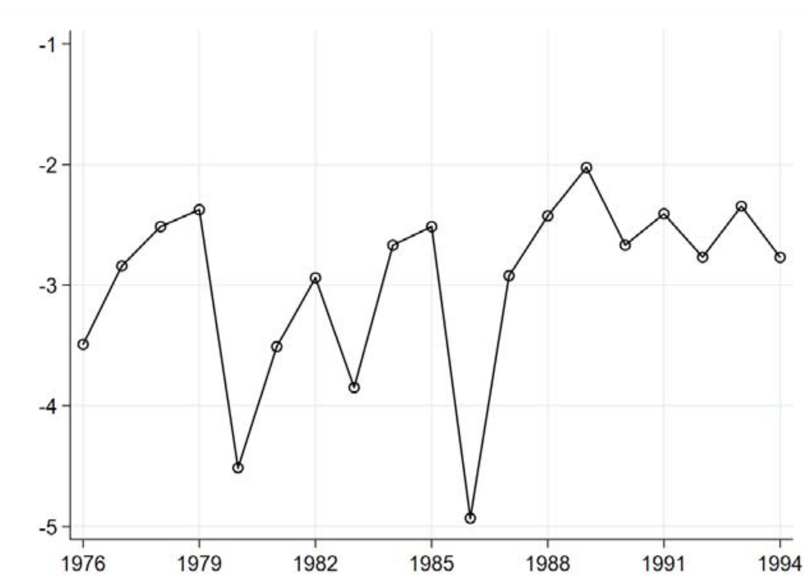


Figure 2

Evolution of Output Co-movement between States

This figure illustrates the evolution of output co-movement between states. Panel A shows the mean across all unique state pairs in our sample of the negative absolute difference in residual real GDP growth between two states (multiplied by 100) over the period 1976-1994. Sample covers the 48 contiguous states of the United States (excluding Delaware and South Dakota). Panel B shows the evolution of the negative absolute difference in residual real GDP growth between California and three other states (Florida, Texas, and Washington) over our sample period.

Panel A: Negative Absolute Difference in Residual Real GDP Growth between States - Average across all state pairs



Panel B: Negative Absolute Difference in Residual Real GDP Growth between States - Selected State Pairs

Figure 3
Evolution of Interstate Banking Deregulation

This figure shows the cumulative fraction of state pairs in our sample that had removed barriers to bank entry between each other by each year over the period 1976-1994, differentiating between methods of deregulation. Unilateral deregulation refers to cases in which (at least) one of the states in a given pair unilaterally allowed entry by bank holding companies (BHCs) from all other states. Reciprocal deregulation are cases in which states enacted nationwide reciprocal agreements with all other states. In these cases, the date of effective deregulation for a given state pair depends not only on the decision of the state that deregulated on a reciprocal manner, but also on the other state's decision to reciprocate. Bilateral deregulation refers to cases in which the two states in a given pair allowed BHC entry by signing a bilateral interstate banking agreement. Sample covers the 48 contiguous states of the United States (excluding Delaware and South Dakota).

Fraction of State Pairs that Removed Bilateral Restrictions to Interstate Banking

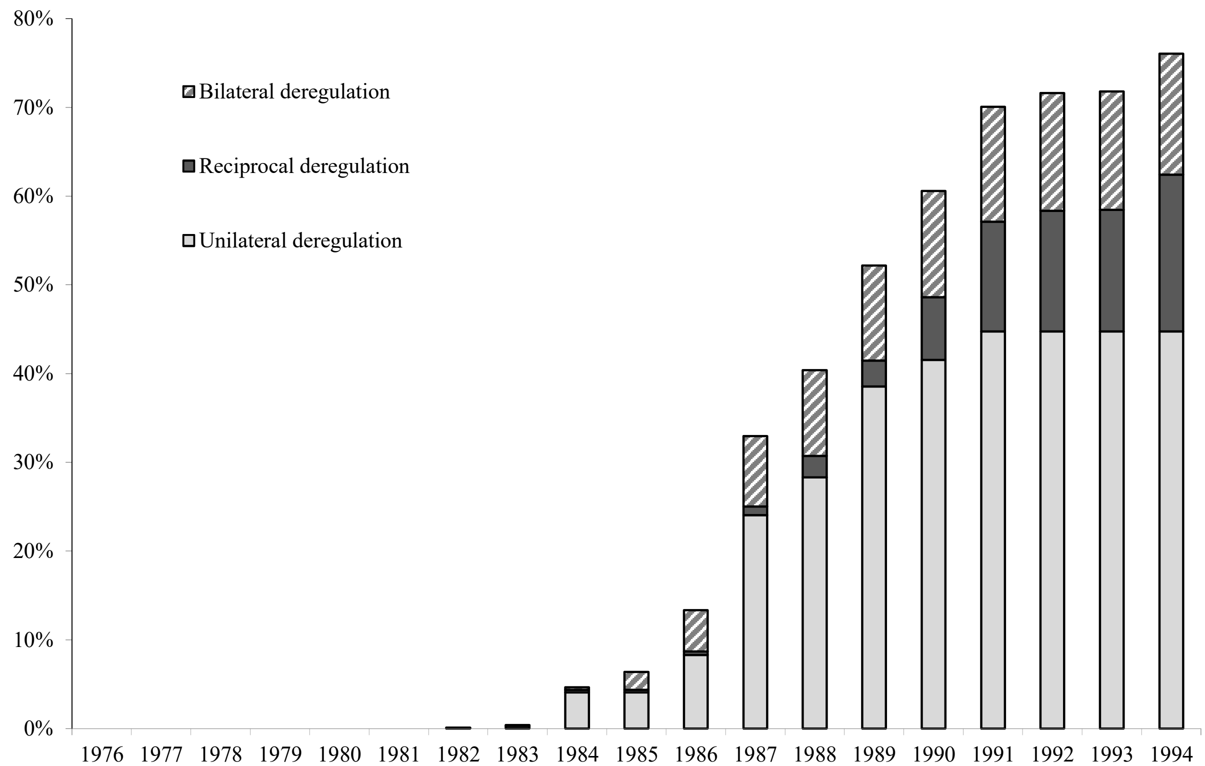
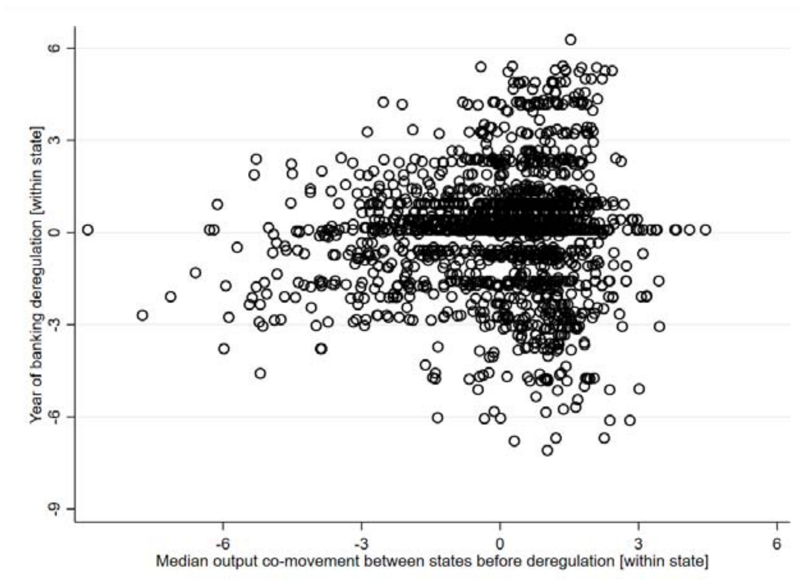


Figure 4

Interstate Banking Deregulation and Output Co-movement Before Deregulation -Within State Differences

This figure plots the relationship between the timing of bilateral interstate banking deregulation and the level of or change in output co-movement beween states prior to deregulation. For each state-pair *i,j* we first determine the year of interstate banking deregulation and then compute the median level of (Panel A) or median change in (Panel B) the negative absolute difference in residual real GDP growth between the two states over the five years prior to deregulation. To focus on within-state differences, we substract the state-level mean from all the variables.

Panel A: Timing of Interstate Banking Deregulation and Level of Output Co-movement Before Deregulation



Panel B: Timing of Interstate Banking Deregulation and Change in Output Co-movement Before Deregulation

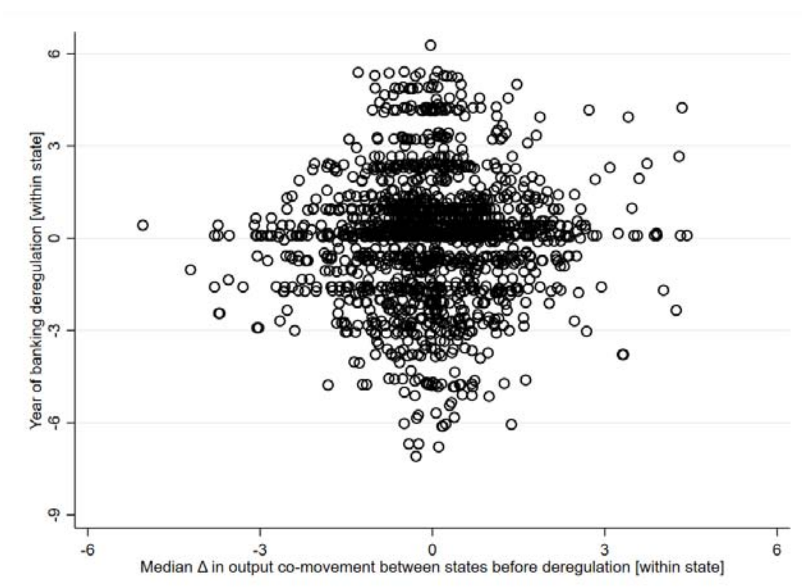


Figure 5

Dynamic Effect of Interstate Banking Deregulation on Banking Integration between States

This figure illustrates the impact of bilateral interstate banking deregulation on banking integration between states. In particular, the figure shows coefficients from the following regression:

$$Banking\ integration_{i,j,t} = \alpha_{i,j} + \delta_t + \sum_{r=-10}^{+10} \beta_r Y_{i,j,r,t} + \varepsilon_{i,j,t}$$

where *Banking integration* $_{i,j,t}$ is the share of jointly-owned assets and deposits for state pair *i, j* in year *t*; $Y_{i,j,r,t}$ are dummy variables equal to one if in year *t*, states *i* and *j* deregulated *r* years before; δ_t and $\alpha_{i,j}$ are year and state-pair fixed effects, respectively. The dots show the estimated br coefficients, while the dashed lines show the 99 percent cofidence interval. The coefficient on integration for the year of interstate banking deregulation is excluded due to collinearity, so the coefficients β_r capture differences relative to the year of deregulation. Standard errors are clustered at the state-pair level.