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Banking Networks and Economic Growth: From Idiosyncratic Shocks to Aggregate Fluctuations[†]

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This paper investigates the role of banking networks in the transmission of shocks across borders. Combining banking deregulation in the United States with state-level idiosyncratic demand shocks, we show that geographically diversified banks reallocate funds from economies experiencing negative shocks to unaffected regions. Our findings indicate that in the presence of idiosyncratic shocks, financial integration reduces business cycle comovement and synchronizes consumption patterns. Our findings contribute to explaining the Great Moderation and provide empirical support for theories that predict that banking integration facilitates the insurance of region-specific risk and the efficient allocation of resources as markets become more complete. (*JEL* E32, F65, G21, G28, J24, L14, R11)

Understanding how economic shocks spread across borders and influence global economic activity is a fundamental question in economics. Cross-border linkages, particularly banking networks, are widely recognized as key channels for transmitting these shocks across space. While international real business cycle (IRBC) models, such as those by Backus, Kehoe, and Kydland (1992), suggest that banking linkages can reduce business cycle comovement by reallocating capital efficiently, existing empirical research presents conflicting evidence. For instance, in a cross-country study using European data, Kalemli-Özcan, Papaioannou, and Peydró (2013) find that banking integration has a strong negative effect on output synchronization. Conversely, Morgan, Rime, and Strahan (2004) document an

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increase in business cycle convergence across US states following banking integration. This paper seeks to reconcile these conflicting results by proposing that the role of banking linkages in business cycle comovement depends crucially on the type of economic shocks. Specifically, we provide empirical evidence supporting the framework proposed by Perri and Quadrini (2018), which suggests that endogenous banking sector shocks can lead to synchronized business cycles, while exogenous, country-specific shocks originating outside the banking sector may cause desynchronization.

The cleanest natural experiment to test the transmission of shocks through banking networks requires an exogenous shock to the banking network and the measurement of idiosyncratic shocks. We exploit the dissolution of regulatory barriers to geographic bank expansion in the United States between the 1980s and 1990s as an exogenous shock to banking networks across state boundaries. We combine this natural experiment with state-level idiosyncratic demand shocks to examine the role of banking linkages in shock transmission and economic growth comovement. We test how exogenous shocks originating outside the banking system (henceforth, "idiosyncratic shocks") are transmitted through banking linkages. Our findings indicate that in the presence of idiosyncratic shocks, financial integration reduces business cycle comovement across states. We argue that while shocks originating within the banking system directly affect the *aggregate* amount of loanable funds, idiosyncratic shocks affect the *relative* lending share across geographies, keeping the total stock of funds fixed. Specifically, if a banking network spans two economies, domestic and foreign, negative idiosyncratic shocks in the foreign economy may lead to an increase in the domestic loan supply, thereby stimulating subsequent domestic economic growth and reducing the covariance of business cycle fluctuations across geographies.

These findings have significant implications for understanding the effects of banking integration and financial globalization. First, our results highlight the importance of financial integration in facilitating the movement of loanable funds toward productive investment. This is especially relevant in light of the recent backlash against financial globalization. In particular, our results inform the European Union's financial integration initiatives, such as the European Banking Union and the European Capital Markets Union. Our results suggest that a robust banking union may lead to economic divergence among member states in response to idiosyncratic shocks. This consideration is also pertinent to emerging markets transitioning to private banking systems. Second, our research contributes to understanding the Great Moderation—a period of reduced macroeconomic volatility starting in the mid-1980s. We propose that banking integration helped temper aggregate fluctuations through geographic diversification, reducing business cycle comovement while synchronizing consumption patterns across states. As consumption becomes more closely tied to aggregate shocks following integration, our findings provide empirical support for the canonical IRBC model presented in Backus, Kehoe, and Kydland (1992).

We construct state-level idiosyncratic shocks using labor productivity shocks to large firms headquartered in that state after partialing out industry-wide labor productivity shocks as in Gabaix (2011). We focus on these shocks for three key

reasons. First, these shocks are geographically isolated, lack temporal dynamics, and reflect firm-specific events, making them ideal for our analysis. Second, we show that large firms are less reliant on banks for external financing and that these shocks are unlikely to affect bank capital contemporaneously. Third, these idiosyncratic shocks predict future economic growth, influencing banks' expectations of future state-level economic growth. These three characteristics make idiosyncratic shocks well-suited for measuring economic shocks that originate outside the banking system.

Our analysis reveals that idiosyncratic shocks in state *j* were positively correlated with economic growth in state *i* during the late 1970s and early 1980s. This implies that good (bad) news for state *j* was also good (bad) news for state *i*, suggesting that states behaved as complements during that period. However, the relation monotonically reversed post-1984; i.e., good (bad) news for state *j* became bad (good) news for state *i*, suggesting that states behaved as substitutes after this period.

We attribute the changing relation between idiosyncratic shocks in state *j* and economic growth in state *i* to banking integration between the two states. In a difference-in-differences (DID) framework, combining the state-pairwise banking integration natural experiment with the measurement of idiosyncratic shocks, we show that a one standard deviation negative idiosyncratic shock, $\Gamma_{j,t-1}$, in state *j* increases economic growth in state *i* by 0.05–0.19 percentage points after the state pair (i,j) is integrated via banking linkages.

The effect of idiosyncratic shocks in state *i* on economic growth in state *j* operates via changes in bank loan supply. We employ an instrumental variables (IV) strategy similar to the granular IV methodology presented in Gabaix and Koijen (2020) and employed in the banking context by Kundu, Park, and Vats (2021). Using the idiosyncratic shock, $\Gamma_{j,t-1}$, in state *j* combined with banking integration as an instrument for bank lending in state *i*, we estimate that a 1 percent increase in a bank's loan supply in state *i* increases economic growth by 0.06–0.25 percentage points in state *i*.

The relevance of the instrument stems from the assumption that different states, when integrated, compete for bank lending and that geographically diversified banks allocate funds away from geographies experiencing negative idiosyncratic shocks, increasing loan supply in unaffected states. This assumption is verified in the first-stage regression. The exclusion restriction is satisfied under the assumption that the covariance of loan demand between states (i,j) does not change around the time of banking deregulation. Alternatively, the exclusion assumption holds if the covariance in loan demand is sticky relative to changes in the covariance in loan supply around banking integration. We note that even if this assumption is violated, it will bias our test toward a null result if the ex ante covariance in loan demand is positive. The ex ante covariance in loan demand is likely positive, as states behaved as complements before banking deregulation. We verify this assumption using a dynamic stochastic general equilibrium (DSGE) model.

Building on this framework, we test a key prediction of Backus, Kehoe, and Kydland (1992) regarding the impact of banking integration on the comovement of economic growth and consumption across states. Specifically, we investigate how banking integration affects the correlation of GDP growth and food consumption across states. Our analysis reveals two striking findings: First, the correlation of

GDP growth across states decreases after banking integration, and, second, the correlation of food consumption increases over the same period. The coexistence of these two results is consistent with the shock transmission mechanism hypothesized in the standard international real business cycle model (Backus, Kehoe, and Kydland 1992), which predicts that banking integration facilitates the insurance of region-specific risk and the efficient allocation of resources as markets become more complete.

We provide further evidence that the effect indeed operates through the banking channel. Our analysis confirms that banks expanded across state lines following banking integration, and the heterogeneity in the baseline estimate across states can be attributed to the extent of out-of-state bank expansion in each state post-deregulation. Moreover, the effect develops gradually over time, consistent with the notion that while banking deregulation can be enacted swiftly, the development of actual banking infrastructure, acquisition of private information by banks, and formation of banking relationships takes time to mature.

Exploring the underlying mechanism, we dissect the anatomy of idiosyncratic shocks and find that the effect propagates through geographically isolated idiosyncratic shocks. Our analysis reveals two key insights. First, consistent with the argument that geographic expansion provides diversification benefits to banks when shocks are uncorrelated across geographies, we show that the effect is driven by shocks with low spatial correlation. Second, banking integration increases banking competition, which alters the sensitivity of banks to different types of shocks. Preintegration, persistent shocks matter more in monopolistic environments (Petersen and Rajan 1994), whereas, postintegration, banks become more sensitive to temporally isolated shocks in competitive environments (Diamond 1984). Consistent with this view, we find that the effect is larger in magnitude for shocks with little temporal dynamics.

We further elucidate the underlying mechanism by investigating the role of bank constraints as a key friction driving our results. We argue that, unlike unconstrained banks that operate at the optimal investment level across regions, constrained banks face limited funding and cannot exhaust all investment opportunities. As a result, constrained banks may redirect funding to other regions when hit by an idiosyncratic shock in a particular region. Our empirical analysis confirms this theoretical argument, showing that the transmission of idiosyncratic shocks across geographies is indeed more pronounced when banks face tighter capital constraints.

To further understand the transmission of idiosyncratic shocks across geographies, we examine the micro-level mechanisms underlying this phenomenon. Using a bank-firm matched dataset, we investigate how banks' reallocation of funds across firms contributes to the aggregate response in economic growth across states. Our analysis reveals that younger firms, which are more dependent on external finance, are more responsive to foreign idiosyncratic shocks after banking integration. Specifically, we find that younger firms exhibit greater sensitivity in debt growth, sales growth, market-to-book ratio, and work-in-progress inventory growth to foreign idiosyncratic shocks compared to older firms. We also find that idiosyncratic shocks to large, less bank-dependent firms in state j transmit to small, bank-dependent firms in state i after banking integration. This suggests that banks form expectations about

future economic growth through shocks to firms that are less reliant on banks and transmit these shocks to bank-dependent firms across regions. Thus, our findings corroborate the hypothesis that firms that are more bank dependent drive the aggregate response in economic growth.

We argue that this phenomenon can help explain the decline in aggregate volatility during the period of relative quiescence in macroeconomic volatility, starting from 1984, referred to as the "Great Moderation." When the correlation between shocks and economic growth across states becomes negative, aggregate fluctuations are tempered. Theoretically, the combined effect of banking integration and idiosyncratic shocks on aggregate volatility is ambiguous. The geographic diversification of banks in the presence of idiosyncratic shocks can reduce the covariance of business cycle fluctuations across regions, but it may also increase the variance of these fluctuations. This latter effect arises because banking integration makes domestic growth more sensitive to foreign shocks.

To verify our mechanism and quantitatively analyze these competing effects, we employ a DSGE model developed by Kalemli-Özcan, Papaioannou, and Perri (2013a). The model shows that as banking integration increases, the relationship between domestic economic growth and foreign shocks shifts from positive to negative when the foreign shocks are idiosyncratic. However, when the foreign shocks are related to bank capital, the relationship between domestic economic growth and foreign shocks becomes more positive with greater banking integration.

Regarding the Great Moderation, the calibrated model produces two key results. First, banking integration reduces the covariance of business cycle fluctuations across regions. Second, the reduction in covariance outweighs the increase in individual variances, leading to a decline in aggregate volatility. Thus, our paper proposes an alternative theory explaining the Great Moderation. We argue that simultaneous changes in the banking system during the 1980s and 1990s increased banks' role in intermediating shocks between states. Specifically, the presence of new cross-state intermediaries altered the transmission of shocks, allowing for greater diversification and reducing aggregate volatility.

We conduct a battery of robustness tests to ensure the validity of our results. First, we conduct a parallel trend analysis to show that the results are not driven by pre-trends before deregulation. Second, we conduct a placebo test in which we randomize the timing of banking integration and show that the results disappear when using randomly created deregulation dates. This indicates that the precise timing of banking deregulation is important. Additionally, we argue that the results are unlikely to be driven by geography-based measurement error in the idiosyncratic shock, nor are they sensitive to the methodology adopted to construct idiosyncratic shocks. Last, we show that the results are not driven by other confounding variables such as industry similarity, covariance of personal income growth, covariance of GDP growth, exports, imports, and migration across state pairs.

The key contribution of this work is identifying a novel mechanism through which idiosyncratic shocks propagate across borders and impact economic activity. Specifically, our results provide empirical support for standard IRBC models, which suggest that banking linkages can reduce the comovement of business cycles (Backus, Kehoe, and Kydland 1992; Kalemli-Özcan, Papaioannou, and

Perri 2013b), and highlight the role of idiosyncratic shocks in driving this outcome (Holmstrom and Tirole 1997; Perri and Quadrini 2018). This paper thus offers one of the first well-identified pieces of evidence documenting the benefits of financial integration through banks, where loanable funds are directed toward productive investment opportunities. This finding directly speaks to the first-order diversification function of banks in an economy, as proposed by Diamond (1984). Additionally, we show that, following banking integration, the correlation of economic growth across states decreases, while the correlation of consumption increases. Thus, our results show that banking integration facilitates the insurance of region-specific risk and the efficient allocation of resources as markets become more complete. These results present an alternative to the trade-based explanations of the BKK puzzle presented in Rogoff and Obstfeld (2000).

Our work is most closely related to Kalemli-Özcan, Papaioannou, and Peydró (2013). In their cross-country study, Kalemli-Özcan, Papaioannou, and Peydró (2013) find a strong negative effect of banking integration on output synchronization, conditional on global shocks and country-pair heterogeneity. To the best of our knowledge, Kalemli-Özcan, Papaioannou, and Peydró (2013) is the only other paper that also documents a negative effect of financial integration on business cycle comovement. We add to the findings of this paper in several ways. First, by focusing on within-country variation across states and exploiting the exogenous shock of banking deregulation, we offer a more precise identification strategy. Second, we empirically identify the underlying mechanism through which banking integration leads to negative output synchronization. Finally, our distinction between different types of shocks helps reconcile our findings with those of Morgan, Rime, and Strahan (2004), who show that the volatility of state-level economic growth declines as banks in a state become more integrated with those in other states. This decline is attributed to banking system-originated shocks. We complement Morgan, Rime, and Strahan (2004) by distinguishing between idiosyncratic shocks and bank shocks. Our empirical design captures the effect of shocks that arise not from contemporaneous shocks to collateral or capital but, rather, from banks' future expectations of local economic growth.

Last, our paper provides a critical link in the discussion on the Great Moderation by proposing an alternative explanation.[1] We show that the decline in the covariance of economic growth across states following banking deregulation can help explain the decline in aggregate volatility during the Great Moderation.

This paper is organized as follows. Section I discusses the institutional details of banking deregulation. Section II describes the data, construction and properties of idiosyncratic shocks. Section III presents key results. Section IV outlines and presents evidence in support of the underlying mechanism. Section V presents robustness results. Section VI presents a discussion on the linkage between our results and the Great Moderation, and Section VII concludes.

[1] See Davis and Kahn (2008) for a survey of previous studies that offer explanations for the Great Moderation.

I. Institutional Details

This section examines the natural experiment of state-pairwise banking deregulation, which removed regulatory barriers and enabled cross-border banking expansion from the 1980s to the 1990s. This experiment has been utilized in previous studies (Morgan, Rime, and Strahan 2004; Michalski and Ors 2012b; Landier, Sraer, and Thesmar 2017) to analyze the effects of deregulation.

The McFadden Act of 1927 prohibited interstate branching, restricting national banks to operate within their home state. However, the banking sector underwent significant changes in the 1980s, leading to deregulation in a staggered manner across states until 1994. During this period, three main types of reforms occurred based on reciprocity: national nonreciprocal, national reciprocal, and bilateral reciprocal. National nonreciprocal reforms allowed banks from all other states to enter a state's banking market, accounting for 33.8 percent of state pairs. National reciprocal reforms permitted interstate banking deregulation between states with similar reforms, involving 21.6 percent of state pairs. Bilateral reciprocal agreements between individual state pairs resulted in deregulation for 8.8 percent of state pairs. (For further details on banking deregulation, see Amel 1993; Morgan, Rime, and Strahan 2004; Michalski and Ors 2012b.) The era of banking deregulation concluded with the enactment of the Riegle-Neal Interstate Banking and Branching Efficiency Act (IBBEA) in 1994, allowing banks to branch across all state lines.

State-pairwise banking deregulation provides an exogenous source of variation in banking linkages across states. Our identifying assumption is that these state-pairwise banking deregulation agreements are not correlated with unobserved heterogeneity in economic growth comovement, implying that states did not cherry-pick the states with which they deregulate based on preexisting linkages in economic growth. This is likely to be true as only 8.8 percent of all state pairs deregulated via bilateral agreements, whereas all other states deregulated nationally either voluntarily or forcibly in 1994. Michalski and Ors (2012b) argue that interstate trade share and flows were not a driver for banking deregulation, ruling out preexisting comovement in economic growth as a result of trade linkages between states. Additionally, we control for any geographic patterns of deregulation through fixed effects. Further, on the political economy of these reforms, Kroszner and Strahan (1999) document that deregulation was influenced by lobbying activity from small banks. However, there is limited evidence whether these agents are responsible for the expansion in credit supply post-deregulation.[2]

Another key assumption is that the removal of regulatory barriers following deregulation resulted in actual geographic expansion of banks across state lines. A survey of existing literature suggests that this is a reasonable assumption. Berger, Kashyap, and Scalise (1995b) document that interstate branching increased the percentage of deposits held by out-of-state bank holding companies (BHCs) in a typical state from 2 percent to 28 percent between 1979 and 1994. Morgan, Rime, and Strahan (2004) document a 14 to 17 percentage point increase in interstate banking activity

[2] Mian, Sufi, and Verner (2020) document that the credit supply expansion following banking deregulation primarily affected real economic activity through the household demand channel. Moreover, large banks were responsible for the credit supply expansion post–banking deregulation (Demsetz and Strahan 1997; Stiroh and Strahan 2003).

post-deregulation. In an identical setting, Landier, Sraer, and Thesmar (2017) show that the average adjusted lending co-Herfindahl of banking assets across state pairs increases post–banking integration. We independently replicate this result using an alternate dataset on gross banking assets held by out-of-state banks used in Berger, Kashyap, and Scalise (1995a) in Supplemental Appendix B. We document that the share of gross domestic banking assets owned by out-of-state banks grew from $\sim$7 percent in 1979 to $\sim$35 percent in 1994 and that this growth is explained by banking integration.

II. Data

The dataset used in this analysis is a balanced panel of all US state pairs (i,j) from 1978 to 2000. It contains data on real GDP growth rate for state i, a measure of idiosyncratic shock for state j, a binary variable that takes a value of 1 for periods after which state i permitted entry from banks in state j, total loan supply and total commercial loan supply issued in state i, 1977 state-pairwise commodity flow data, and food consumption in state i. We use five key sources of data: annual state-level real GDP growth rate from the Bureau of Economic Analysis (BEA) (US Bureau of Economic Analysis 1978–2000), state-level annual bank lending data from the Call Reports (Federal Financial Institutions Examination Council 1978–2000), data on dates of state-pairwise deregulation dates, data on total and directional commodity flows from the 1977 Commodity Flow Survey (CFS) dataset compiled by Michalski and Ors (2012a), idiosyncratic shocks constructed using Compustat data (Standard & Poor's 1978–2000), and food consumption data from the Survey of Buying Power published by *Sales & Marketing Management* magazine from 1978 through 1995 (*Sales & Marketing Management* 1978–1995).

A. *Bank Lending Data*

We measure both the total amount of commercial lending and all lending for each state and year, using the annual Consolidated Report of Condition and Income (Federal Financial Institutions Examination Council 1978–2000). We compute the total loan supply by aggregating all new loans and commercial and industrial loans at the BHC-state level. This aggregation methodology assumes that commercial banks do not operate outside the border of the state in which they are located. Morgan, Rime, and Strahan (2004) and Landier, Sraer, and Thesmar (2017) argue that this is a reasonable approximation before the enactment of IBBEA in 1994. We supplement our analysis using call report data from Drechsler, Savov, and Schnabl (2017).

B. *Food Consumption Data*

We compile novel state-level food consumption data by hand-collecting and digitizing food sales figures from the Survey of Buying Power reports published by the *Sales & Marketing Management* magazine from 1978 through 1995 (*Sales & Marketing Management* 1978–1995). Note that we were unable to retrieve data for 1989 and 1990 because the reports for these years are not available in the archives. These data are used to test whether consumption patterns are more synchronized

after states following banking integration, shedding light on the Backus, Kehoe, and Kydland (1992) puzzle. We focus on food sales as they align more closely to consumption in classical macroeconomic models than measures of total spending.

C. *Idiosyncratic Shocks*

Idiosyncratic shocks measure shocks originating in a specific geography and are orthogonal to bank capital shocks and other fundamental shocks. Construction of state-level idiosyncratic shocks requires annual sales and employment numbers along with information on headquarter location and industry. This information is sourced from Compustat (Standard & Poor's 1978–2000). We narrow our focus to US companies, headquartered in one of the 50 states or Washington, DC. We eliminate firms operating in heavily regulated industries such as oil and gas extraction, finance, and utilities. Our analysis is limited to firms that have data on both employment and sales. The firm-level data are used to construct our measure of state-level idiosyncratic productivity shocks.

Construction of Idiosyncratic Shocks.—In this section, we describe the process for constructing idiosyncratic shocks. We follow a methodology similar to Gabaix (2011) to construct state-level idiosyncratic shocks. Labor productivity (z^{s}_{kt}) of firm k headquartered in state s at time t is measured as the natural logarithm of the ratio of sales and employees. It is assumed that the sales and employees of firm k originate in the state in which they are headquartered.[3] We define labor productivity shock to firm k in state i as $g^{(i)}_{kt}$, where $g^{(i)}_{kt} = z^{(i)}_{kt} - z^{(i)}_{k,t-1}$.

We construct state-level idiosyncratic shock using a two-step process. First, we regress firm-level productivity shocks on industry-year fixed effects (θ_{mt}) based on the four-digit SIC industry code to which the firm belongs. We then compute firm-level residuals from this regression. These residuals (ε^{i}_{kt}) are devoid of any industry-wide systematic shocks and provide a better approximation to the ideal firm-level idiosyncratic shocks (Gabaix 2011). In the next step, we aggregate firm-level idiosyncratic shocks for the K largest firms. We sort firms based on sales for each state and narrow our focus to the top K firms in each state. For aggregation, each firm-level idiosyncratic shock is Domar-weighted by its sales to total nominal GDP. We denote these state-level idiosyncratic shocks as Γ_{it}, computed as follows:

$$g^{(i)}_{kt} = \theta_{mt} + \varepsilon^{(i)}_{kt}, \tag{1}$$

$$\Gamma_{it} \equiv \sum_{\substack{k=1 \\ k\in i}}^{K} \frac{S^{(i)}_{k,t-1}}{Y_{t-1}}\, \varepsilon^{(i)}_{kt}. \tag{2}$$

Γ_{it} is used as our main measure of the idiosyncratic shock at the state level, referred to as Γ^{ind}_{it}. We construct state-level shocks, Γ, using the top ten firms in each state.[4]

[3] This assumption may result in a geography-based measurement error problem. We refer the readers to Section VE for a detailed discussion on this issue.

[4] In Section VC we discuss the sensitivity of our results to alternative construction methodology such as altering the value of K, allowing Γ_{it} to have a factor structure with heterogeneous exposures, etc.

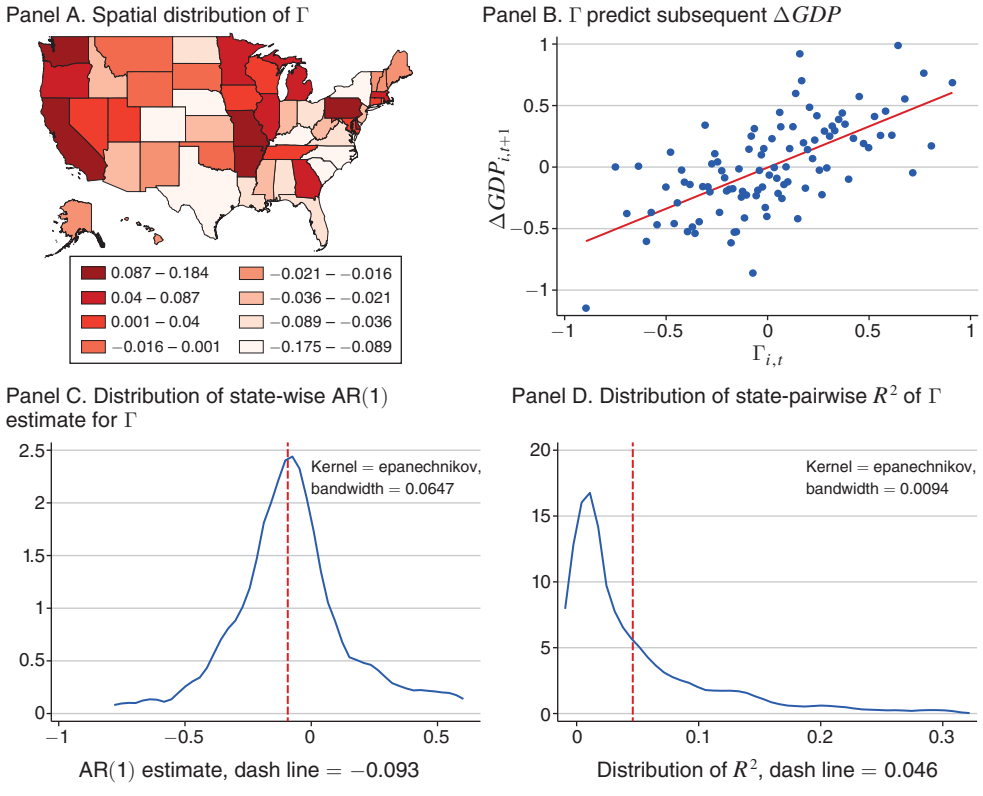

FIGURE 1. PROPERTIES OF IDIOSYNCRATIC SHOCKS, Γ

Notes: The figure describes the properties of idiosyncratic shocks, documenting their spatial distribution, geographic isolation, temporal nonpersistence, and ability to predict future economic growth. Figure 1, panel A plots the cross-sectional distribution of Γ over US states between 1978 and 1995. We take a time series average of $\Gamma^{ind}_{j,t-1}$ for each state and use these average values to plot the heat map of the cross-sectional distribution of idiosyncratic shocks. We winsorize the idiosyncratic shocks at the 5 percent level. Figure 1, panel B plots the binscatter plot of Γ and subsequent annual economic growth in the same state. State-level idiosyncratic shocks and subsequent annual economic growth are standardized to mean zero and unit variance. Figure 1, panel C plots the estimated coefficients of the AR(1) term from a state-wise regression. We run time series AR(1) regression for each state and estimate the AR(1) coefficient. The blue line reports the kernel density of AR(1) coefficients obtained from the time series regression. The dashed red line plots the AR(1) estimate obtained from a pooled regression of all states. Figure 1, panel D plots the kernel density of R^2 of Γ for each state pair. The red dashed line plots the mean value of R^2. Our data span the period from 1978 to 2000.

Properties of Idiosyncratic Shocks.—We begin by examining the cross-sectional distribution of idiosyncratic shocks, denoted as Γ. Figure 1, panel A illustrates the distribution of Γ from 1978 to 2000 across states, revealing significant heterogeneity in both the magnitude and sign of these shocks. Notably, certain states, such as Texas, North Carolina, South Carolina, Florida, and New York, experienced predominantly negative shocks during the sample period. In contrast, states like California, Washington, Illinois, and Michigan encountered mostly positive shocks.

Gabaix (2011) argues that in modern economies dominated by large firms, idiosyncratic shocks to these firms can lead to nontrivial aggregate shocks. First, we show that idiosyncratic shocks are indeed granular in the sense of Gabaix (2011).

We verify the dominance of large firms in each state in Supplemental Appendix C.1, showing that the top ten firms by sales in a state account for at least 50 percent of sales by all firms in that state.[5] Second, we verify that these shocks predict future economic growth. Figure 1, panel B presents the pooled binscatter plot of idiosyncratic shocks and subsequent annual economic growth in a given state. The line is upward sloping, with a β of 0.67 from the pooled regression—significant at the 1 percent level—and a model R^2 of 7 percent. We redo this regression at the state level and estimate an average (median) β of 0.71 (0.83) with a model R^2 of 13 percent (11 percent).[6] Hence, these shocks exhibit predictability of future economic growth at the state level.

Next, we analyze the idiosyncratic shocks further by examining their persistence over time and their spatial correlation across states. Figure 1, panel C reports the kernel density of the coefficients of a state-wise AR(1) process for Γ. While the AR(1) estimate exhibits heterogeneity, the majority of the mass is bunched around zero. The average AR(1) estimate for a pooled regression has a value of -0.092. This indicates on average a low degree of persistence among these shocks. Furthermore, the impulse response functions from an AR(1) and AR(3) model report that idiosyncratic shocks exhibit short-lived temporal dynamics (see Supplemental Appendix C.3). Figure 1, panel D plots the kernel density of the state-pairwise R^2 computed by running a simple OLS regression of idiosyncratic shocks in state i on state j. Despite some heterogeneity, the mass of the model R^2 is concentrated around zero with an average value of 0.046 (dashed red line). This suggests that the state-level idiosyncratic shocks are local and cannot explain idiosyncratic shocks in other states.

Last, we investigate the impact of idiosyncratic shocks on banks' capital constraints. The underlying intuition is that if idiosyncratic shocks influence bank capital, a significant negative correlation should exist between bank constraints and idiosyncratic shocks. Supplemental Appendix Table C.2, panel A examines the effect of state-level idiosyncratic shocks on state-level bank constraints. The state-level bank constraint measure is constructed by weighting each bank's liabilities-to-assets ratio by its lending share in that state. The point estimate reported in Supplemental Appendix Table C.2 is economically small, precisely estimated with a small standard error, and statistically insignificant. Panel B of Supplemental Appendix Table C.2 yields similar results using bank deposits as the dependent variable. This suggests a weak economic relationship between idiosyncratic shocks and bank constraints and deposits, consistent with the assumption that idiosyncratic shocks derived from large firms are unlikely to affect bank health.

[5] A related concern is that a firm classified as large in one state may be small relative to a large firm in another state. This does not threaten our construction of state-level shocks, provided that the firms used to construct these shocks are large relative to the state economies in which they are headquartered. However, it does raise concerns about whether large firms in some states, being smaller relative to firms in other states, may be less dependent on banks for external financing. To address this issue, we compare the ratio of bank debt to total debt across states and find no meaningful differences; see Supplemental Appendix Figure C.6.

[6] We supplement this descriptive analysis by showing the comovement in the series of idiosyncratic shocks and subsequent annual economic growth for selected states in Supplemental Appendix C.2.

Why Use These Shocks?—The idiosyncratic shocks constructed in Section IIC enable us to identify the impact of geographically isolated shocks that originate outside of the banking system. We focus on these shocks for three key reasons. First, they are geographically isolated and do not exhibit long-run temporal dependence, making them ideal for analyzing localized economic effects. Second, the shocks predict future economic growth, which may influence banks' expectations of future economic growth in a state. Third, these shocks are constructed using large firms, which do not primarily rely on bank credit for external funding (Gertler and Gilchrist 1994; Kashyap, Lamont, and Stein 1994). We verify this assumption by comparing the ratio of bank debt to total debt for the sample of firms used in constructing the state-level idiosyncratic shocks (shock firms) to all other firms in the S&P Capital IQ database. The median (mean) bank debt to total debt ratio for shock firms is 23.63 percent (30.35 percent), compared to a value of 44.63 percent (48.03 percent) for other firms (see Supplemental Appendix C.4). Hence, state-level idiosyncratic shocks constructed from labor productivity shocks to large firms are prime candidates for measuring geographically isolated shocks with limited long-run temporal dynamics that do not affect bank capital contemporaneously.

Narrative Analysis of Idiosyncratic Shocks.—In this section, we employ a narrative-driven approach to investigate how firm-level labor productivity shocks, used to construct state-level shocks, can be attributed to firm-specific events. We identify the top three firms per state-year with the largest magnitude of temporally adjusted labor productivity as *significant* observations. For each significant firm-year observation, we conduct a thorough event study, gathering historical events from the website fundinguniverse.com, supplemented by additional sources, including *Businessweek* archives, the ABI/INFORM Collection, and historical archives of annual reports sourced from ProQuest.[7]

The hand-collected information reveals that the majority of firm-specific events are related to restructuring activity within a firm, hostile takeover attempts, leveraged buyouts, litigation, scandals, mergers and acquisitions, other corporate governance issues, and discovery and release of new products. Table 1 presents a selected sample of the most economically and methodologically interesting firm-level productivity shocks. A key insight from the narrative analysis of firm-level events that contribute to state-level idiosyncratic shocks is that observation of these events does not require access to private information.

D. *Data Description*

Table 2 reports the summary statistics for the variables of interest in this study from 1978 to 2000. The median annual change in GDP is 3.3 percent (mean is 3.25 percent). The twenty-fifth and seventy-fifth percentiles for GDP growth are 1.4 percent and 5.3 percent, respectively. The granular residual has a median of 0.000. The twenty-fifth and seventy-fifth percentiles are −0.053 and 0.059, respectively.

[7] Fundinguniverse.com sources its information on company history and significant events from various volumes of the *International Directory of Company Histories*.

Table 1—Narrative Analysis of Firm-Level Productivity Shocks

Year	Firm name	HQ state	Γ_{it}	$\Gamma_{it} - \mathbb{E}_t[\Gamma_{it}]$	$\Gamma_{it} - \mathbb{E}_{jt}[\Gamma_{it}]$	News
1977	Whirlpool Corp.	Michigan	14.8%	0.6919%	0.5434%	Introduced the first automatic clothes washer and microwave ovens.
1978	The Kroger Co.	Ohio	−23.9%	−8.4179%	−7.6038%	FTC crackdown for violation of 1973 trade law. Price patrol cheating scandal.
1979	Paramount Communications, Inc.	New York	23.2%	2.1942%	1.9918%	Deal with Teleprompter Corp (largest cable systems operator in United States).
1981	Chevron Corp.	California	−0.7%	−3.0755%	−2.9696%	Aramco is nationalized by the Saudi government.
1982	Savannah Foods & Industries, Inc.	Georgia	−7.5%	−0.1893%	−0.0020%	Big clients switched to high-fructose corn syrup.
1983	Storage Technology Corp.	Colorado	−19.0%	−0.6869%	−0.3325%	Loss in market share to IBM due to delay in the product release.
1984	Skyline Corp.	Indiana	−0.6%	−0.0827%	−0.0417%	Internal managerial decision to cut costs to remain debt free.
1985	Montgomery Ward & Co.	Illinois	−9.0%	−2.1164%	−2.0918%	Massive restructuring of the firm after three years of experimentation under former CEO. The firm closed its catalog business after 113 years.
1986	Reynolds Metal Co.	Virginia	6.7%	0.2358%	2.7674%	Discovered gold in a bauxite ore.
1987	Eli Lilly & Company	Indiana	10.3%	0.3694%	0.4763%	FDA approves the use of Prozac for treating depression.
1988	Johnson & Johnson	New Jersey	7.7%	0.0920%	0.4588%	Acuvue disposable contact lenses are introduced.
1989	Boeing Co.	Washington	−7.6%	−3.1492%	−1.3783%	Boeing jets involved in accidents. Delivery delayed.
1990	Intel Corp.	California	13.0%	0.4189%	0.7062%	Intel launches i486.
1991	Eastman Kodak Co.	New York	−1.8%	−0.8057%	−1.1724%	Polaroid's suit against Kodak is settled. Made payment of $925 million.
1993	Circuit City Stores, Inc.	Virginia	7.2%	0.2308%	0.2269%	Circuit City launches its new CarMax chain, a retailer of used cars.
1994	Xerox Corp.	Connecticut	14.4%	2.1486%	3.2586%	Brand makeover.
1995	The Black & Decker Corp.	Maryland	10.4%	0.2561%	0.4333%	Introduces the VersaPak interchangeable battery system and the SnakeLight flexible flashlight.
1996	Dell Inc.	Texas	17.3%	0.8928%	0.5149%	The company begins selling over the internet.

Notes: The table reports the events for a selected sample of firm-year observations between 1977 and 1996. The firm-year observations that we believe to be economically and methodologically most interesting are included in the table. HQ state refers to the name of the state of headquarters of the firm in that year. Γ_{it} refers to the firm-level labor productivity shock, $\Gamma_{it} - \mathbb{E}_t[\Gamma_{it}]$ refers to the firm-level labor productivity shock adjusted for aggregate labor productivity shocks during the period, and $\Gamma_{it} - \mathbb{E}_{jt}[\Gamma_{it}]$ refers to the firm-level labor productivity shock adjusted for aggregate industry labor productivity shocks during the period. $\Gamma_{it} - \mathbb{E}_t[\Gamma_{it}]$ and $\Gamma_{it} - \mathbb{E}_{jt}[\Gamma_{it}]$ have been multiplied by 100 before reporting.

The distribution of these shocks appears symmetric around zero. In addition, the table reports the log of annual commercial and industrial lending and of total lending. The average values for these are 16.65 and 18.13, respectively. The standard deviation is 1.33 and 1.26, respectively. Last, the table reports the log of food sales. The average value for this is 15.024, with a standard deviation of 1.054.

III. Results

This section examines the aggregate trend in the comovement of economic growth in state *i* and idiosyncratic shocks in state *j*, revealing that banking integration between state pairs drives this comovement. Through an IV strategy, we demonstrate that the effect is mediated by shocks to loan supply, providing evidence

TABLE 2—SUMMARY STATISTICS

	N	p25	Median	p75	Mean	SD
ΔGDP	1,173	1.400	3.300	5.300	3.247	3.254
Γ^{ind}	1,157	−0.053	0.000	0.059	0.005	0.331
log(C&I Loans)	1,173	15.668	16.526	17.388	16.651	1.334
log(Total Loans)	1,173	17.295	18.036	18.923	18.132	1.262
log(Food Sales)	805	14.228	15.092	15.754	15.024	1.054

Notes: The table reports the number of observations, first quartile, median, third quartile, mean, and standard deviation of observations for the key variable used in our analysis. Our data span the period from 1978 to 2000.

of a causal link between banking integration and the comovement of economic growth and idiosyncratic shocks.

A. *Comovement in Economic Growth and Idiosyncratic Shocks*

We first document the relation between economic growth in state *i* and idiosyncratic shocks in state *j*. Figure 2, panel A displays the evolution of the relation between GDP growth in state *i* and idiosyncratic shocks in state *j* over time. We plot the estimated βs from five-year forward rolling regressions of $\Delta GDP_{i,t}$ on $\Gamma^{Avg}_{j,t-1}$ (i.e., $\Delta GDP_{i,t} = \alpha + \beta\Gamma^{Avg}_{j,t-1} + \varepsilon_{i,t}$) from 1978 through 1995, where $\Gamma^{Avg}_{j,t-1}$ is the average of $\Gamma^{ind}_{j,t-1}$ for all other states. The magnitude of β exhibits a monotonically declining trend from 1978 until 1991. The estimated value decreases from a value of $\sim+1$ in 1978 to a value of ~-1 in 1991.[8] The average β coefficient exhibits a notable shift between the two subperiods. Specifically, it is positive, 0.28, for the 1978–1986 period but turns negative, −0.39, for the 1986–1994 period.[9] This implies that states behaved as complements before 1986 and as substitutes thereafter.

The secular decline in the nature of cross-border spillovers from 1978 to 1994 motivates further examination into the underlying factors driving the change. The time period in which the relation between economic growth in state *i* and idiosyncratic shocks in state *j* exhibits a monotonic change coincides with the period in which the US banking industry underwent structural reforms. We study this in a rigorous manner, providing prima facie evidence that the change in the relation between economic growth in state *i* and idiosyncratic shocks in state *j* is attributable to geographic banking integration. Figure 2, panel B plots the point estimate obtained from the state-pairwise regression between GDP growth in state *i* and idiosyncratic shocks in state *j* from two subsets. "*Pre*" refers to a sample of all state pairs before banking integration. "*Post*" refers to a sample of all state pairs after banking integration. Point estimates are plotted with the 90 percent confidence interval (CI) obtained by two-way clustering of the standard errors at the state *i* and state *j* level. The estimate

[8] The only exception to this trend is the year 1979, which exhibits a large positive deviation from the trend. This can potentially be explained by the fact that 1979 was the year of oil crisis due to decreased oil output in the wake of the Iranian Revolution. Since oil was crucial to production and households at that time, a large systematic oil shock can explain the extremely large positive correlation estimated for 1979.

[9] The year 1979 is not included in the calculation of averages. The *t*-statistic associated with the difference in the average beta for the two periods is 4.50.

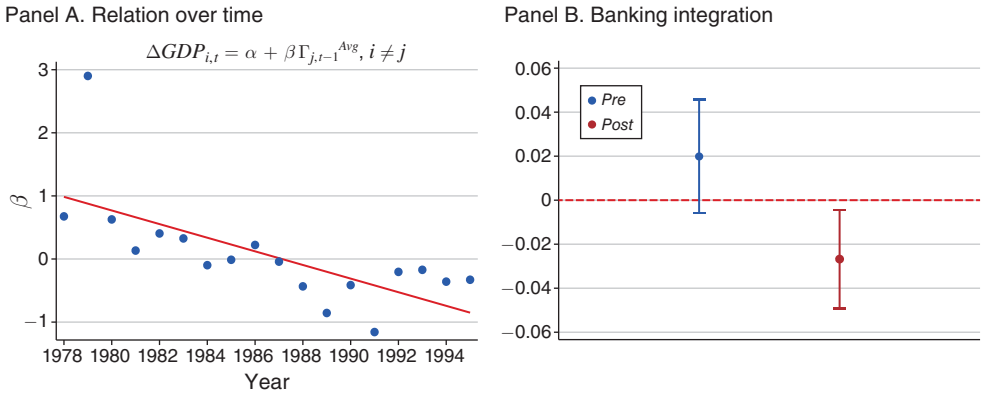

FIGURE 2. RELATION BETWEEN GDP GROWTH AND IDIOSYNCRATIC SHOCKS

Notes: The figure documents the relation between GDP growth in state i and idiosyncratic shocks in state j, where $i \neq j$: its evolution over time and its relation to banking integration. Figure 2, panel A plots the relation between GDP growth in state i and idiosyncratic shock in state j. We run five-year forward rolling regressions of $\Delta GDP_{i,t}$ on $\Gamma_{j,t-1}^{Avg}$ from 1978 to 1995 and estimate the point estimate β. We plot the point estimates of β for each year between 1978 and 1995. Figure 2, panel B plots the point estimate obtained from the regression between GDP growth in state i and idiosyncratic shocks in state j from two subsets. "*Pre*" refers to a sample of all state pairs before banking integration. "*Post*" refers to a sample of all state pairs after banking integration. Ninety percent CIs are plotted with point estimates. The CIs are obtained by two-way clustering the standard errors at the state i and state j level. All variables used in regressions were standardized to mean zero and unit variance.

for the pre-period is positive in magnitude but statistically insignificant, whereas the estimate for the post-period is negative and statistically significant. The difference in magnitude between the two estimates is -0.046, statistically significant at the 5 percent level. Moreover, this difference is stable across different quantiles of ΔGDP (see Supplemental Appendix E.1). Next, we move to a more robust specification to formally attribute the shift in the relationship between economic growth in state i and idiosyncratic shocks in state j to geographic banking integration.

B. *Baseline Result*

Motivated by the observed aggregate trend in the comovement of economic growth in state i and idiosyncratic shocks in state j, and its coincidental timing with banking deregulation in the United States, we investigate whether this trend can be attributed to increased banking integration across state pairs. To this end, we develop a statistical framework in Supplemental Appendix A.1 to motivate the DID specification as in equation (3). Our baseline specification estimates a regression at the (i,j,t) level where each observation corresponds to a state pair (i,j) at time t.

$$\Delta GDP_{it} = \beta_0 Post_{i,j,t} \times \Gamma_{j,t-1}^{ind} + \beta_1 Post_{i,j,t} + \alpha_{i,j} + \theta_i \times t + \theta_{jt} + \varepsilon_{i,j,t}, i \neq j, \tag{3}$$

where ΔGDP_{it} denotes real GDP growth for state i, $\Gamma^{ind}_{j,t-1}$ denotes state-level idiosyncratic shock for state j, and $Post_{i,j,t}$ is a binary variable taking a value of 1; if banks in state j are allowed to expand operations in state i. $\alpha_{i,j}$ denotes state-pairwise fixed effects controlling for all time-invariant state-pair-specific heterogeneity such as distance. θ_{jt} captures time-varying heterogeneity for state j. We do not include the level term for $\Gamma^{ind}_{j,t-1}$ as it is absorbed by θ_{jt}. We also control for $\theta_i \times t$, denoting linear trend specific to state i. $\varepsilon_{i,j,t}$ denotes the idiosyncratic term in the baseline specification.

The baseline specification, (3), is estimated at the state-pair level, building on the conceptual framework outlined in Supplemental Appendix A.1. Importantly, each state (state i) appears in the regression sample $N-1$ times per year, with the residual of each other state (state j) serving as a regressor in turn ($i \neq j$), where N is the total number of states. This system of equations can be equivalently represented by the collapsed specification in equation (4). The econometric equivalence between these two specifications is provided in Supplemental Appendix A.3.

$$\Delta GDP_{it} = \beta_0^* \sum_{j,j\neq i} \left(Post_{i,j,t} \times \Gamma^{ind}_{j,t-1} \right) + \beta_1^* \sum_{j,j\neq i} Post_{i,j,t} + \beta_2 * \sum_{j,j\neq i} \Gamma^{ind}_{j,t-1} + \alpha_i^* \times t + \theta_t^* + \varepsilon_{i,t}^*. \tag{4}$$

However, estimating specification (3) offers several advantages over the collapsed specification (4). First, it allows us to exploit heterogeneity in deregulation at the state-pair level. Second, by including state-pair fixed effects, we effectively control for all time-invariant state-pair heterogeneity. Third, time-varying fixed effects for state j encompass a broad array of observed and unobserved factors that contribute to idiosyncratic shocks specific to state j. This includes state-specific economic conditions, policy changes, and other unique circumstances that may impact state j over time. Importantly, specification (3) implicitly controls for idiosyncratic shocks in other states and their interactions with the timing of deregulation. Nonetheless, to assess robustness, we estimate the collapsed specification in Supplemental Appendix Table G.1 and find consistent results. Given the potential for correlation in the error term at the state-pair level, standard errors for specification (3) are two-way clustered by state i and state j.

Table 3 reports the estimates of the state-level impact of idiosyncratic shocks on GDP growth before and after banking integration of the state pair. Column 1 reports the baseline specification devoid of any fixed effects. The point estimate of interest is the interaction term of *Post* and Γ. The interaction term is negative and statistically significant at the 5 percent level. The negative point estimate indicates that a negative idiosyncratic shock in state j is related to an increase in economic growth in state i after banking integration of state i and j, relative to pre–banking deregulation. Column 2 adds year fixed effects to the specification in column 1. The estimate remains negative and statistically significant at 1 percent level. The addition of further fixed effects in columns 3 to 6 does not alter the significance of the negative interaction term, indicating a robust relationship. Column 6 estimates the specification in equation (3). Economically, the baseline estimate of column 6 indicates that

Table 3—Baseline Specification

$$\Delta GDP_{it} = \beta_0 Post_{i,j,t} \times \Gamma^{ind}_{j,t-1} + \beta_1 Post_{i,j,t} + \alpha_{i,j} + \theta_i \times t + \theta_{jt} + \varepsilon_{ijt}, i \neq j$$

ΔGDP_{it}	(1)	(2)	(3)	(4)	(5)	(6)
$Post_{i,j,t} \times \Gamma^{ind}_{j,t-1}$	−0.0428	−0.0026	−0.0058	−0.0079	−0.0177	−0.0164
	(0.0178)	(0.0005)	(0.0012)	(0.0001)	(0.0021)	(0.0007)
$\Gamma^{ind}_{j,t-1}$	0.0184	0.0010	0.0023	0.0031		
	(0.0155)	(0.0002)	(0.0005)	(0.0000)		
$Post_{i,j,t}$	0.2550	0.0085	0.0764	0.0769	0.0857	0.0783
	(0.0641)	(0.0789)	(0.0605)	(0.0470)	(0.0526)	(0.0491)
Year fixed effects		Yes				
Region$_i$-year fixed effects			Yes	Yes	Yes	Yes
Region$_j$-year fixed effects			Yes	Yes		
State$_i$-state$_j$ fixed effects				Yes	Yes	Yes
State$_j$-year fixed effects					Yes	Yes
State$_i$–linear trend						Yes
Observations	57,700	57,700	57,700	57,700	57,700	57,700
R^2	0.0163	0.3094	0.5168	0.6113	0.6114	0.6583

Notes: This table reports the results from the estimation of baseline specification. The dependent variable is the change in the real GDP growth rate in percentage. The main independent variable is $\Gamma^{ind}_{j,t-1}$, which denotes the idiosyncratic shocks in state j constructed by aggregating the Domar-weighted labor productivity shocks of top-ten firms, by sales in state j after accounting for industry-year fixed effects. The unit of observation in each regression is a state$_i$-state$_j$-year pair. All nonbinary variables used in the regression are standardized to mean zero and unit variance. Standard errors reported in parentheses are two-way clustered by state$_i$ and state$_j$.

a one standard deviation (0.3) negative $\Gamma^{ind}_{j,t-1}$ increases economic growth in state i by 0.05 percentage points post–banking integration.[10]

To assess the robustness of our results to domestic idiosyncratic shocks, we augment the baseline specification by incorporating state-level idiosyncratic shocks for state i, denoted as $\Gamma^{ind}_{i,t-1}$, and its interaction with $Post_{i,j,t}$. This interaction term allows us to control for domestic idiosyncratic shocks that could influence bank capital reallocation across states after banking integration. Results from the augmented specification, presented in Table 4, show that the coefficient on the interaction between *Post* and foreign idiosyncratic shocks remains negative, statistically significant, and comparable to the baseline estimate in Table 3. This suggests our findings are robust to domestic idiosyncratic shocks.

Another concern with our baseline model may be that the estimation repeats the outcome variable for each state 49 times. This could lead to a misspecification error, possibly biasing our estimates. To address this, we conduct a robustness test using a state$_i$-year level of observation rather than a state$_i$-state$_j$-year level. We create a time-varying deregulation index for each state$_i$ based on the fraction of other states with which state$_i$ has deregulated, ranging from 0 to 1. Idiosyncratic shocks are computed as the average of all other state-level idiosyncratic shocks. Supplemental Appendix Table E.1 presents the relationship between economic growth in state$_i$ on the interaction term of the deregulation index and average foreign idiosyncratic

[10] All nonbinary variables in Table 3 are standardized to mean zero and unit variance. The effect is estimated by multiplying the point estimate of β_0 in column 6 with the standard deviation of GDP growth rate. Effect of 1 SD $\Gamma = \beta_o \times \sigma_{\Delta GDP} = 0.0164 \times 3.254 = 0.0534$.

TABLE 4—AUGMENTED SPECIFICATION

$$\Delta GDP_{it} = \beta_0 Post_{i,j,t} \times \Gamma^{ind}_{j,t-1} + \beta_1 Post_{i,j,t} + \beta_3 Post_{i,j,t} \times \Gamma^{ind}_{i,t-1} + \beta_4 \Gamma^{ind}_{i,t-1} + \alpha_{i,j} + \theta_i \times t + \theta_{jt} + \varepsilon_{ijt},\ i \neq j$$

ΔGDP_{it}	(1)	(2)	(3)	(4)	(5)	(6)
$Post_{i,j,t} \times \Gamma^{ind}_{j,t-1}$	−0.0436	−0.0028	−0.0051	−0.0071	−0.0159	−0.0141
	(0.0178)	(0.0006)	(0.0014)	(0.0008)	(0.0019)	(0.0015)
$Post_{i,j,t} \times \Gamma^{ind}_{i,t-1}$	−0.0547	0.0017	0.0130	0.0057	0.0057	0.0033
	(0.0351)	(0.0260)	(0.0242)	(0.0213)	(0.0213)	(0.0225)
$\Gamma^{ind}_{j,t-1}$	0.0217	0.0011	0.0021	0.0029		
	(0.0154)	(0.0003)	(0.0005)	(0.0003)		
$\Gamma^{ind}_{i,t-1}$	0.0294	0.0013	−0.0002	0.0011	0.0012	−0.0010
	(0.0294)	(0.0212)	(0.0167)	(0.0169)	(0.0169)	(0.0166)
$Post_{i,j,t}$	0.2519	−0.0081	0.0665	0.0693	0.0776	0.0665
	(0.0669)	(0.0776)	(0.0601)	(0.0467)	(0.0524)	(0.0502)
Year fixed effects		Yes				
Region$_i$-year fixed effects			Yes	Yes	Yes	Yes
Region$_j$-year fixed effects			Yes	Yes		
State$_i$-state$_j$ fixed effects				Yes	Yes	Yes
State$_j$-year fixed effects					Yes	Yes
State$_i$–linear trend						Yes
Observations	56,758	56,758	56,758	56,758	56,758	56,758
R^2	0.0177	0.3191	0.5419	0.6391	0.6393	0.6897

Notes: This table reports the results from the estimation of baseline specification. The dependent variable is the change in the real GDP growth rate in percentage. The main independent variable is $\Gamma^{ind}_{j,t-1}$, which denotes the idiosyncratic shocks in state j constructed by aggregating the Domar-weighted labor productivity shocks of top-ten firms by sales in state j after accounting for industry-year fixed effects. $\Gamma^{ind}_{i,t-1}$ denotes the idiosyncratic shocks in state i constructed by aggregating the Domar-weighted labor productivity shocks of top-ten firms by sales in state i after accounting for industry-year fixed effects. The unit of observation in each regression is a state$_i$-state$_j$-year pair. All nonbinary variables used in the regression are standardized to mean zero and unit variance. Standard errors reported in parentheses are two-way clustered by state$_i$ and state$_j$.

shocks. The estimate of the interaction term is negative, statistically significant, and similar to the baseline estimate presented in Table 3.

Effect on State-Pairwise GDP Growth Correlation.—As noted earlier, there are several advantages of preserving the bilateral structure of the data. Alternatively, we can calculate the rolling GDP growth correlation for all state pairs and regress this correlation on $Post_{i,j,t}$. This specification has the key advantage of being insensitive to the methodology used to construct foreign idiosyncratic shocks and can capture time-varying heterogeneity for both states in the pair by including state$_i$ × year and state$_j$ × year fixed effects. Supplemental Appendix Table E.2 presents the results, showing a negative and statistically significant coefficient on $Post_{i,j,t}$, supporting our hypothesis. Although this alternative specification provides a useful robustness check, we retain our original baseline specification as the primary one due to its greater transparency in revealing the underlying mechanisms driving our results. Overall, this finding suggests that banking integration led to reduced comovement of GDP across states.

Effect on State-Pairwise Consumption Correlation.—Classical models predict that increased banking integration should facilitate the pooling of state-specific

Table 5—Rolling Consumption Correlation and Banking Deregulation

$Corr(cons_{it}, cons_{jt})$	(1)	(2)	(3)	(4)	(5)	(6)
$Post_{i,j,t}$	0.0441	0.0376	0.0384	0.0595	0.0392	0.0234
	(0.0212)	(0.0207)	(0.0199)	(0.0259)	(0.0203)	(0.0097)
State$_i$-state$_j$ fixed effects			Yes	Yes	Yes	Yes
State$_j$-year fixed effects						Yes
Year fixed effects				Yes		
Region$_i$-year fixed effects	Yes	Yes	Yes			
Region$_j$-year fixed effects		Yes	Yes		Yes	
State$_i$-year fixed effects					Yes	Yes
Observations	36,346	36,346	36,346	36,346	36,346	36,346
R^2	0.2605	0.3302	0.4309	0.3088	0.6038	0.7884

Notes: This table reports the results from the estimation of the following regression specification:

$$Corr(cons_{it}, cons_{jt}) = \beta_0 Post_{i,j,t} + \alpha_{i,j} + \theta_i \times t + \theta_{jt} + \varepsilon_{ijt}, i \neq j.$$

The dependent variable is the rolling consumption correlation between states i and j, computed over a five-year window from $t-4$ until t. State-level consumption is measured as the natural logarigm of the total value of food sales in state i during year t. The unit of observation in each regression is at the state$_i$-state$_j$-year level. Standard errors reported in parentheses are two-way clustered by state$_i$ and state$_j$.

output risks, thereby decoupling domestic consumption growth from state-specific income shocks (Backus, Kehoe, and Kydland 1992). Consequently, consumption patterns across states should become more synchronized following banking integration, as they respond primarily to aggregate shocks rather than state-specific factors. This leads to the prediction that consumption correlations across states should increase significantly post-integration.

To test this conjecture, we compile novel state-level food consumption data by hand-collecting and digitizing food sales figures from the Survey of Buying Power reports published by *Sales & Marketing Management* magazine from 1978 through 1995 (*Sales & Marketing Management* 1978–1995).[11] Table 5 reports the results from regressing the rolling consumption correlation across all state pairs on $Post_{i,j,t}$. Our preferred specification includes state-pair fixed effects as well as state$_i$ $\times$ year and state$_j$ $\times$ year fixed effects to control for time-varying heterogeneity. The coefficient of interest is consistently positive and statistically significant across all specifications, indicating a significant increase in consumption correlation across states following banking integration.

Overall, our findings shed light on the Backus, Kehoe, and Kydland (1992) puzzle. Specifically, we demonstrate that consumption correlation across state pairs increases following banking integration, aligning with predictions from canonical macroeconomic models.

Weighted Estimation.—The estimates produced from our baseline analysis are predicated on the assumption that the strength of banking linkages are equal across state pairs. Given that banking linkages are likely to differ across state pairs, we

[11] Note that we focus on food sales as they align more closely to consumption in classical macroeconomic models than measures of total spending.

estimate a weighted specification of our baseline regression. In this specification, we assume that the strength of banking linkages is proportional to the strength of nonbanking real linkages. Michalski and Ors (2012b) argue that this is a reasonable assumption since banks that are present in two regions charge the appropriate risk premiums for trade-related projects between these markets, whereas higher rates are charged for projects involving shipments to markets where banking linkages are absent. Hence, we hypothesize that accounting for nonbanking linkages will produce point estimates of larger magnitude, relative to the equal-weighted assumption. Supplemental Appendix Table E.3 reports the results from the weighted estimation. Results show that a one standard deviation $\Gamma_{j,t-1}$ shock increases economic growth in state *i* by 0.13–0.19 percentage points post–banking integration. Hence, by accounting for the strength of banking linkages using nonbanking linkages, we find a larger effect of idiosyncratic shocks in state *i* on economic growth than in state *j* post–banking integration.

C. *Staggered Nature of Treatment*

Recent advances in the DID literature have highlighted that the standard DID estimator may not yield a valid estimand in staggered treatment designs with heterogeneous treatment effects. Specifically, Sun and Abraham (2021) point out that the conventional staggered DID estimator can be biased due to the "bad comparisons" problem, which arises when different treated cohorts experience distinct treatment effect trajectories. To address this issue, we adopt the "stacked regression" approach of Gormley and Matsa (2011), as implemented by Vats (2020).

Table 6 presents the results from the stacked regression. There are several treatment cohorts (state pairs) in the stacked regression that underwent treatment until 1993. We use state pairs that underwent treatment in 1994 as the set of controls. We restrict the sample until 1993 to ensure that our control group of state pairs is never treated in the data. This method of creating a control group is similar to the strategies adopted in the new DID estimator literature when all units get treated eventually. The data are structured at the cohort-state-pair level. Specifically, all state pairs that deregulated in year *t* are compared to the state pairs that deregulated in 1994. Together, these groups form the treatment and control groups for cohort *t*. Column 1 presents the results with the set of fixed effects identical to the baseline specification. Column 3 presents the results from the estimation of our preferred specification which interacts all the fixed effects with the cohort indicator variable. This inclusion ensures that our estimates represent weighted averages of differences between treatment and control groups within each cohort, mitigating the potential issue of "bad comparisons." The results indicate that our baseline finding is robust to heterogeneous treatment effects across cohorts.

Heterogeneous Treatment Effects.—Thus far, we have presented the average effect for the sample across all states. De Chaisemartin and d'Haultfoeuille (2020) argue that linear regressions estimate weighted sums of the average treatment effects (ATEs) in each group and period, with weights that could be negative. This may produce a negative estimate, though all the ATEs are positive. This section documents

Table 6—Stacked Regression

ΔGDP_{it}	(1)	(2)	(3)
$\text{Treatment}_{i,j} \times \text{Post}_{i,j,t} \times \Gamma^{ind}_{j,t-1}$	−0.0204	−0.0203	−0.0185
	(0.0098)	(0.0095)	(0.0091)
$\text{Treatment}_{i,j} \times \text{Post}_{i,j,t}$	0.0335	0.0299	0.0318
	(0.0516)	(0.0500)	(0.0473)
$\text{Treatment}_{i,j} \times \Gamma^{ind}_{j,t-1}$	0.0033	0.0028	0.0032
	(0.0089)	(0.0087)	(0.0079)
$\text{Post}_{i,j,t} \times \Gamma^{ind}_{j,t-1}$	−0.0007	0.0005	
	(0.0011)	(0.0011)	
$\text{Post}_{i,j,t}$	0.0015		
	(0.0030)		
Region_i-year fixed effects	Yes	Yes	
State_i-state_j fixed effects	Yes	Yes	
State_j-year fixed effects	Yes	Yes	
State_i–linear trend	Yes	Yes	
Cohort-year fixed effects		Yes	
Cohort-treatment fixed effects		Yes	Yes
Region_i-year-cohort fixed effects			Yes
State_j-year-cohort fixed effects			Yes
State_i-state_j-cohort fixed effects			Yes
State_i–linear trend–cohort			Yes
Observations	218,128	218,128	218,128
R^2	0.6704	0.6707	0.6743

Notes: This table reports the results from the estimation of baseline specification using the stacked regression framework. The dependent variable is the change in the real GDP growth rate in percentage. The main independent variable is $\Gamma^{ind}_{j,t-1}$, which denotes the idiosyncratic shocks in state j constructed by aggregating the Domar-weighted labor productivity shocks of top-ten firms by sales in state j after accounting for industry-year fixed effects. There are several treatment cohorts (state pairs) in the stacked regression that underwent treatment until 1993. We use state pairs that underwent treatment in 1994 as the set of controls. We restrict the sample until 1993 to ensure that our control group of state pairs is never treated in the data. The data are structured at the cohort-state-pair level. Specifically, all state pairs that deregulated in year c are compared to the state pairs that deregulated in 1994. Together, these groups form the treatment and control groups for cohort c. The results indicate that our baseline finding is robust to issues arising due to treatment effect heterogeneity across treated cohorts. All nonbinary variables used in the regression are standardized to mean zero and unit variance. Standard errors reported in parentheses are two-way clustered by state_i and state_j.

the heterogeneous effects of banking integration across states and argues that the majority of the ATEs are negative. However, the estimates exhibit a great degree of heterogeneity, indicating that states are affected differently by banking integration. We show that a significant portion of this heterogeneity can be explained by the extent of new entry by out-of-state banks following banking integration.

Figure 3 reports the results from the state-wise estimation of the baseline specification. The estimated coefficients from the state-level regressions exhibit a great degree of heterogeneity across states. The majority of state-specific estimates (75 percent) are negative. Forty-five percent of these negative estimates are statistically significant at the 90 percent level of confidence. Less than 18 percent of these estimates have a positive magnitude. The mean value of the estimates is −0.0444, and the standard error of the average estimate is 0.0086. Furthermore, the mean value is negative and lower than the baseline estimate of −0.0164. To characterize the distribution of

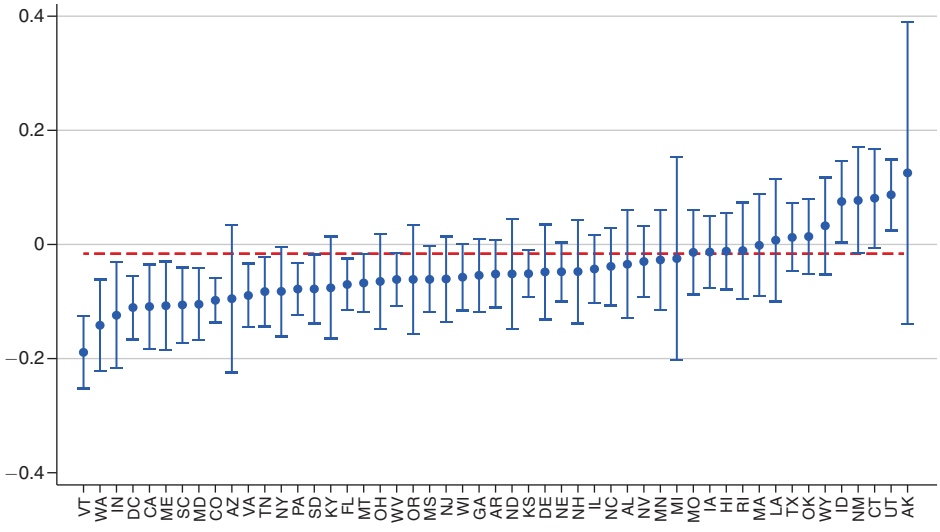

FIGURE 3. HETEROGENEOUS TREATMENT EFFECTS

Notes: The figure plots the point estimates of the interaction term of *Post* and Γ in the baseline specification for each state; i.e., we run the baseline specification as in Table 3 for each state i and estimate the coefficient of the interaction term of *Post* and Γ. The graph also reports the 90 percent CI associated with each estimate. The 90 percent error bands are estimated using standard errors clustered at state$_j$ level. The red dashed line reports the baseline estimate from column 6 in Table 3.

the state estimates, the tenth percentile of the estimates is -0.1073 and the ninetieth percentile is 0.0327. For illustration, California, Maine, Maryland, and South Carolina exhibit β values in the tenth percentile range, while Indiana, Washington and Vermont exhibit β values below the tenth percentile value. The estimates for all of these states are statistically significant at the 90 percent level. Conversely, Wyoming, Idaho, New Mexico, Connecticut, Utah, and Alaska exhibit estimates above the ninetieth percentile value. Apart from Idaho and Utah, the majority of these estimates are statistically insignificant at the 90 percent level. Massachusetts and Louisiana exhibit estimates numerically very close to zero.[12] In Supplemental Appendix E.4 we discuss reasons for heterogeneity in the state-level estimates. We attempt to explain this heterogeneity using two key variables: (i) the median timing of deregulation—i.e., early- versus late-deregulation states—and (ii) the degree of penetration by out-of-state banks.

Assessment of Pre-trends.—A key identifying assumption is that in the absence of treatment, treated and control groups would have followed similar trends. To assess this parallel trends assumption, we examine whether the response of GDP

[12] We direct readers to Supplemental Appendix E.3 for alternative methodologies to compute the effect for each state. In these exercises, a single state pair is compared before and after treatment. We also document that the estimates produced using alternative methodologies are highly correlated with the estimates presented here; see Supplemental Appendix Figure E.2b.

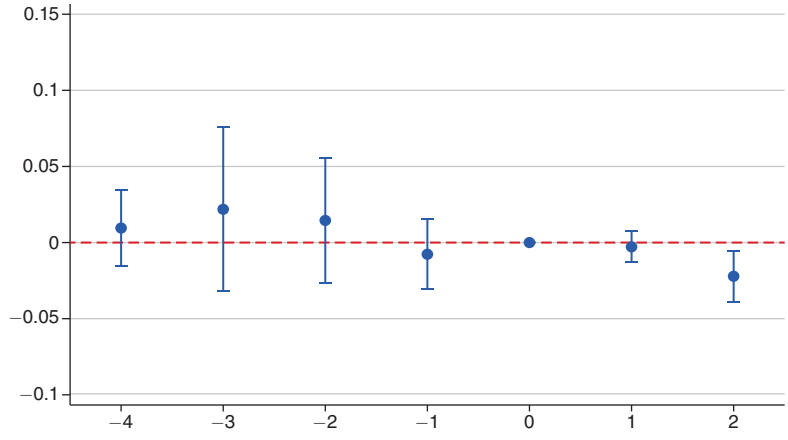

FIGURE 4. PARALLEL TRENDS ASSUMPTION: ASSESSMENT OF PRE-TRENDS

Notes: The figure plots the estimated coefficients β_k and the 90 percent CI from the following equation:

$$\Delta GDP_{i.c,t} = \sum_{k=-4,k\neq-1}^{k=+2} \beta_k Treatment_{i,j,c} \times Time_{i,j,c,t}(t = k) \times \Gamma_{j,t-1}^{ind} + \sum_{k=-4,k\neq-1}^{k=+2} \lambda_k^1 Treatment_{i,j,c} \times Time_{i,j,c,t}(= k) + \sum_{k=-4,k\neq-1}^{k=+2} \lambda_k^2 Treatment_{i,j,c} \times \Gamma_{j,t-1}^{ind} + \alpha_{i,j,c} + \theta_{i,c} \times t + \theta_{j,c,t} + \varepsilon_{i,j,c,t}, i \neq j,$$

which includes a set of leads and lags of the deregulation between states i and j interacted with state-level idiosyncratic shocks in state j. The excluded category is one year before the deregulation. There are several treatment cohorts (state pairs) in the stacked regression that underwent treatment until 1993. We use state pairs that underwent treatment in 1994 as the set of controls. We restrict the sample until 1993 to ensure that our control group of state pairs is never treated in the data. The data are structured at the cohort-state-pair level. Specifically, all state pairs that deregulated in year c are compared to the state pairs that deregulated in 1994. Together, these groups form the treatment and control groups for cohort c. All variables are standardized to mean zero and standard deviation of one. The 90 percent error bands are estimated using standard errors two-way clustered at the state_i and state_j level.

growth in state i to idiosyncratic shocks in state j exhibits common trends for treated and control state pairs before banking integration. We employ a dynamic version of the stacked regression discussed in Section IIIC. Our control group comprises state pairs that were treated in 1994. We restrict the sample to pre-1994 data to ensure that the control group remains untreated throughout the sample period. Figure 4 presents these trends. We find no substantial differences in the response of domestic GDP growth to foreign idiosyncratic shocks between treated and control state pairs in the pre-deregulation period.

D. *Instrumental Variables Strategy*

Thus far, we have established that banking integration alters the relationship between economic growth in state i and idiosyncratic shocks in state j. To provide stronger evidence that this effect operates through changes in loan supply, we employ an IV strategy similar in spirit to the "granular" IV of Gabaix and Koijen (2020) and applied to banking in Kundu, Park and Vats (2021). This section presents the results and discusses the validity of the exclusion restrictions. A detailed framework for our IV design is provided in Supplemental Appendix D.

Identifying Assumptions.—Our identification relies on two key assumptions: relevance and exclusion. The relevance of the instrument stems from the assumption that geographically diversified banks allocate funds away from states experiencing negative idiosyncratic shocks, increasing loan supply in other states. This assumption is verified in the first stage, which shows substitution of lending away from affected states and toward unaffected states.

The exclusion restriction requires that the instruments do not affect economic growth via any channel other than the loan supply channel. While assuming that shocks in state j do not effect loan demand in state i would ensure exclusion, this assumption may be implausible, as the state pair is likely to have nonzero covariance in loan demand via nonbanking channels such as trade, input-output linkages, etc. To address this, we rely on two alternative identification assumptions. First, the *weak identification assumption* posits that the covariance in loan demand between states is stable in magnitude around the timing of banking integration. This allows loan demand in state i to respond to idiosyncratic shocks in state j while ensuring identification of the pure loan supply effect in the DID setup of the first stage. Additionally, if the covariance in loan demand is assumed to be time invariant, state-pair fixed effects control for fluctuations in loan demand. Second, a relatively *weaker identification assumption* posits that the covariance in loan demand between two states is sticky relative to loan supply around the deregulation event.[13] This allows the covariance in loan demand to change post-deregulation but assumes that changes in loan supply covariance between states are more immediate than changes in loan demand covariance.[14] These assumptions enable us to control for fluctuations in loan demand and isolate the loan supply effect. We discuss potential violations of the exclusion restriction in Supplemental Appendix D.1 and argue that such violations would likely bias estimates toward a null effect, as states behave as complements on aggregate in the absence of banking linkages.

2SLS Estimation Results.—Table 7 presents the first- and second-stage IV estimates. The results indicate that, following banking integration, a negative idiosyncratic shock in state j is associated with increased bank lending in state i, which in turn increases economic growth in state i.

The first-stage estimation regresses loan supply in state i on idiosyncratic shocks in state j and an indicator for a banking linkage between state i and j. The coefficient of interest, β_2, represents the interaction term of $\Gamma_{j,t-1} \times Post_{i,j,t}$. The negative and statistically significant estimate reported in column 1 indicates that a negative idiosyncratic shock in state j increases loan supply in state i after banking integration. In column 3, we augment the specification by controlling for time-varying regional demands and state-pair-level time-invariant heterogeneity through state-pair fixed effects. This estimator aligns with our weak identification assumption, allowing

[13] We refer to this assumption as the weaker identification assumption and the previous assumption as the weak identification assumption.

[14] The extant literature is consistent with this assumption. The quantity correlation increases by 1.4 percent, as implied by Michalski and Ors (2012b), while price correlation increases by 3.2 percent, as implied by Landier, Sraer, and Thesmar (2017), following pairwise banking integration indicating that demand covariance responds slowly relative to the loan supply channel.

TABLE 7—INSTRUMENTAL VARIABLES REGRESSION

FIRST STAGE: $\log(l_{i,t}) = \alpha_2 + \beta_2 \Gamma^{ind}_{j,t-1} \times Post_{i,j,t} + \beta_3 Post_{i,j,t} + \beta_4 \Gamma^{ind}_{j,t-1} + \varepsilon_{i,j,t}$

SECOND STAGE: $\Delta GDP_{i,t} = \alpha_1 + \beta_1 \widehat{\log}(l_{i,j,t}) + \mu_{i,j,t}$

	First stage (1)	Second stage (2)	First stage (3)	Second stage (4)	First stage (5)	Second stage (6)	First stage (7)	Second stage (8)
$\log(C\&I - Loan_{i,t})$		1.8751		7.5966		5.3601		4.1422
		(0.5844)		(2.8664)		(0.5940)		(1.5187)
$Post_{i,j,t}$	0.4169	−0.5600	−0.0300	0.2729	−0.0272	0.2023	−0.0240	0.1596
	(0.0877)	(0.2641)	(0.0553)	(0.4208)	(0.0545)	(0.2668)	(0.0539)	(0.2070)
$Post_{i,j,t} \times \Gamma^{ind}_{j,t-1}$	−0.0492		−0.0077		−0.0092		−0.0067	
	(0.0211)		(0.0040)		(0.0039)		(0.0031)	
$\Gamma^{ind}_{j,t-1}$	0.0200							
	(0.0129)							
$Post_{i,j,t} \times \Gamma^{ind}_{j,t-2}$					−0.0091		−0.0082	
					(0.0019)		(0.0020)	
$\Gamma^{ind}_{i,t-1}$							−0.0110	0.0469
							(0.0102)	(0.0432)
$\Gamma^{ind}_{i,t-2}$							−0.0206	0.0783
							(0.0085)	(0.0445)
Constant	16.4706	−30.9787						
	(0.1786)	(9.5737)						
$Region_i$-year fixed effects			Yes	Yes	Yes	Yes	Yes	Yes
$State_i$-$state_j$ fixed effects			Yes	Yes	Yes	Yes	Yes	Yes
$State_j$-year fixed effects			Yes	Yes	Yes	Yes	Yes	Yes
$State_i$–linear trend			Yes	Yes	Yes	Yes	Yes	Yes
Observations	50,838	50,838	50,838	50,838	50,561	50,561	50,180	50,180
Hansen χ^2 p-value	0.5569		1.0000		0.4076		0.9206	
Anderson-Rubin χ^2	10.27		5.84		20.80		12.09	
Anderson-Rubin χ^2 p-value	0.0059		0.0157		0.0000		0.0024	
KP LM statistic	3.908		2.201		4.762		7.596	
KP χ^2 p-value	0.1417		0.1380		0.0925		0.0224	
KP F-statistic	2.997		3.750		12.421		10.868	
Sanderson-Windmeijer F-statistic	3.00		3.75		12.42		10.87	
Sanderson-Windmeijer F-statistic p-value	0.0592		0.0586		0.0000		0.0001	

Notes: This table presents the estimates of our IV strategy. The first-stage regressions reported in columns 1, 3, 5, and 7 establish a causal relation between bank lending in state i and idiosyncratic production shocks to the top ten firms in state j after banking integration with varying fixed effects and lags. Columns 2, 4, 6, and 8 report the second-stage regression of real GDP growth rate in percentage points on bank lending, using the instrumented measures from the first stage. The unit of observation in each regression is a $state_i$-$state_j$-year pair. Observations are weighted by the share of exports from state i to state j, using the 1977 CFS data. All nonbinary variables used in the regression are standardized to mean zero and unit variance except $\log(C\&I - Loan_{i,t})$. Standard errors reported in parentheses are two-way clustered by $state_i$ and $state_j$.

us to isolate the pure effect of the loan supply channel. By controlling for all time-varying heterogeneity at the $state_j$ level, we obtain a more precise estimate of the interaction term. Notably, the point estimate in column 3 is smaller than the estimate in column 1, which captures the combined effects of both loan demand and loan supply channels. In column 5, we control for the interaction of $Post_{i,j,t}$ and the lag of idiosyncratic shock in state j. The point estimate of β_2 remains negative and increases in magnitude relative to the estimate in column 3. In column 7 we control for idiosyncratic shocks in state i as well as the lagged idiosyncratic shocks in both

state *i* and *j* to better identify the pure effect of the loan supply channel. The point estimate of the interaction term of deregulation and idiosyncratic shocks in state *j* is negative and statistically significant at the 1 percent level. The estimates from columns 1, 3, 5, and 7 indicate that after banking integration, a negative idiosyncratic shock in state *j* leads to an increase in bank lending in state *i* through the loan supply channel.

Despite strong second-stage results, the first-stage *F*-statistic, as indicated by the Kleibergen-Paap (KP) rk Wald *F*-statistic, particularly in columns 1 and 3, raises concerns about weak instruments. While columns 5 and 6 exhibit *F*-statistics above the conventional threshold of 10, we employ the robust inference approach suggested by Andrews, Stock, and Sun (2019), reporting identification-robust Anderson-Rubin CIs, which remain statistically significant across all specifications.[15]

In the second stage, we regress the projected loan supply from the first stage on economic growth. The point estimate of interest is β_1, the coefficient associated with the predicted $\log(C\&I - Loan_{it})$ denoted by $\widehat{\log(C\&I - Loan_{i,j,t})}$. The point estimate is positive and statistically significant at the 5 percent level across columns 2, 4, 6, and 8. This coefficient estimate represents the loan supply effect on economic growth, revealing a positive relationship between economic growth and lending. A notable observation is the increase in magnitude of the second-stage estimates from columns 4, 6, and 8 compared to column 2. While this might initially suggest a weakening instrument due to additional controls, the substantial increase in the first-stage *F*-statistic from 3.9 in column 1 to values exceeding the conventional threshold of 10 in columns 5 and 7 suggests that this is unlikely the primary explanation. Instead, the higher second-stage estimates from the 2SLS specifications may be attributed to the fact that 2SLS estimators help alleviate classical measurement error issues, which can downward bias the estimate in a simple OLS regression. Pancost and Schaller (2022) argue that classical measurement error can explain why IV estimates are generally larger than OLS estimates, even when omitted variable bias is expected to lead to the opposite result. This phenomenon, where 2SLS estimators tend to be higher in magnitude than OLS estimators, has been observed in the returns to education literature (Card 2001) and the finance literature (Jiang 2017). For comparison, we report the OLS coefficients associated with the 2SLS specification in Supplemental Appendix Table D.1.

Discussion on the Magnitude of Estimate.—Economically, the results indicate that a 1 percent increase in bank lending through the loan supply channel increases economic growth by 0.06–0.25 percentage points.[16] The existing literature presents point estimates of similar or higher magnitudes. Most recently, Herreño (2020) estimates that a 1 percent decline in aggregate bank lending supply reduces aggregate output by 0.2 percent. Herreño (2020) estimates the aggregate effect using a

[15] Andrews, Stock, and Sun (2019) suggest that when researchers are confident in the validity of their instruments but suspect they may be weak, relying solely on *F*-statistic screening may be undesirable, as it may lead to the exclusion of economically meaningful specifications.

[16] We direct readers to Supplemental Appendix D.2 for a detailed discussion on calculating the economic magnitude of the effect.

general equilibrium model that incorporates multibank firms, relationship banking, endogenous credit dependence, and bank market power.[17]

Our estimate of the impact of loan supply on economic growth is notably lower than previous literature. This discrepancy may be attributed to several factors. First, our sample period (1978–2000) encompasses multiple business cycles, potentially diluting the estimated effect. Second, unlike prior studies that primarily rely on negative bank shocks, our analysis includes both positive and negative shocks. This suggests a possible asymmetry in bank responses, with more-pronounced reactions to negative shocks.

IV. Mechanism

In this section, we explore how idiosyncratic shocks are transmitted through banks. We find that the effect of deregulation develops slowly over time, consistent with the notion that banking linkages and relationships develop over time. Regarding the typology of shocks, we show that the effect is pronounced for shocks that are more likely to be geographically isolated and exhibit less temporal persistence, as well as shocks that are less likely to affect bank capital. We supplement the empirical analysis with the theoretical model of Kalemli-Özcan, Papaioannou, and Perri (2013b) to show that the underlying mechanism of the baseline result is driven by geographic diversification of idiosyncratic shocks following banking integration.

A. *Long-Run Effect*

We consider the dynamic effect of the impact over time. For each state, we estimate the effect of the impact over time by constructing time windows of varying length around the event. Figure 5 reports the plot of these estimates for different time horizons. A time horizon or window of x in this plot indicates that for each state pair, we include observations for x years before and after the year of banking integration. The size of the windows is reported on the x-axis. For each time horizon, the point estimate for the interaction term is estimated as in baseline specification (3), and the estimated coefficients are plotted on the y-axis along with the 95 percent CI. The figure shows that the point estimate develops slowly over time and stabilizes after five years of banking integration. This finding aligns with the hypothesis that the effect is established through banking linkages. While a law can be passed in a day, the implementation of banking linkages across borders and the establishment of relations can take time (D'Acunto et al. 2018). Diverging trends between states before deregulation cannot drive these results, as we find a lack of pre-trends, discussed in Section IIIC.

[17] The model is calibrated using estimates reported in Huber (2018). While the Huber (2018) employment-elasticity-to-bank-lending estimate applies to Germany, its magnitude is quantitatively similar to the estimate presented by Chodorow-Reich (2014) for the United States and by Bentolila, Jansen, and Jiménez (2018) for Spain. Cramer et al. (2024) present estimates for the relationship between Fintech credit and local economic activity in India.

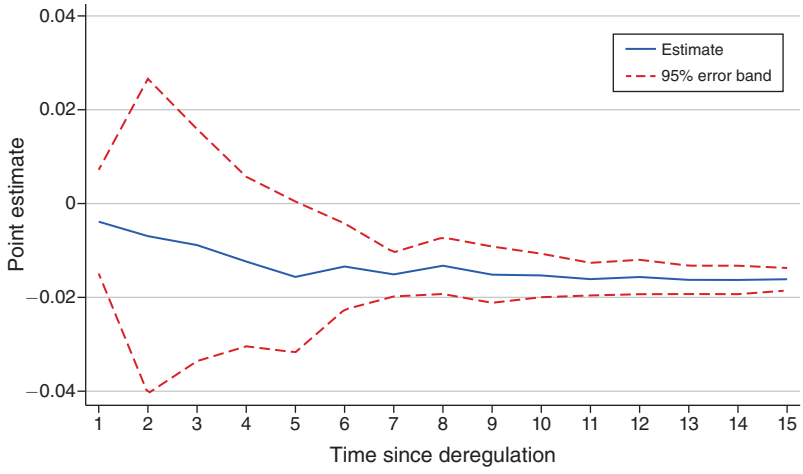

FIGURE 5. LONG-RUN EFFECT

Notes: The figure plots the effect of impact of deregulation over time. We define impact as the year in which state i allows banks of state j to enter its territory. For each state, we estimate the effect of this impact over time by trimming the data for each state pair before and after the passage of the law at different time horizons. We consider horizons from 1 through 15 years before and after the law. These different horizons are reported on the x-axis. For each horizon we run our baseline specification and estimate the coefficient for the interaction term of *Post* and Γ. We plot the point estimate for the interaction term of *Post* and Γ on the y-axis for each time horizon. The 90 percent error bands are estimated using standard errors two-way clustered at the state_i and state_j level.

B. *Effect by Properties of Shocks*

In this section, we show that the effect is pronounced for shocks that are more geographically isolated, exhibit low temporal persistence, and are less likely to effect bank capital. This exercise lends credence to our conjecture that the effect develops through the transmission of idiosyncratic shocks via banking integration.

How do idiosyncratic shocks transmit to the economy through banking integration? First, the geographic expansion of banks provides diversification benefits as long as shocks are not correlated across geographies. Second, banking integration increases banking competition. Prior to deregulation, banking markets were concentrated, and banks could forgo rents in one period with the expectation of recouping and profiting in future periods, as in Petersen and Rajan (1994). In this period, persistent shocks mattered more for credit supply, whereas temporally isolated shocks had little effect. Post-integration, however, lending markets became more competitive. Therefore, in the absence of any commitment between the lender and the borrower, lending contracts were designed such that banks could at least break even each period, as in Diamond (1984). Hence, shocks with low temporal dynamics matter more post-integration. Table 8 reports results based on cross-state spatial correlation (column 1) and temporal persistence (column 2) of the shock. *Low* R^2 takes a value of 1 if the R^2 of the shock between states i and j, where $i \neq j$, is below the median value. *Low* $- AR(1)$ takes a value of 1 if the AR(1) coefficient for the state i is between the first and the third quartile values. The results in column 1 indicate that, post-integration, economic growth in state i increases (decreases) more

TABLE 8—ASYMMETRIC EFFECT BY PROPERTIES OF SHOCK

ΔGDP_{it}	(1)	(2)	(3)
$Low - R^2 \times Post_{i,j,t} \times \Gamma^{ind}_{j,t-1}$	−0.0111		
	(0.0006)		
$Low - R^2 \times Post_{i,j,t}$	−0.0023		
	(0.0012)		
$Low - AR(1) \times Post_{i,j,t} \times \Gamma^{ind}_{j,t-1}$		−0.0073	
		(0.0033)	
$Low - AR(1) \times Post_{i,j,t}$		−0.0057	
		(0.0019)	
$(Neg = 1) \times Post_{i,j,t} \times \Gamma^{ind}_{j,t-1}$			−0.0101
			(0.0100)
$(Neg = 1) \times Post_{i,j,t}$			−0.0387
			(0.0151)
$Post_{i,j,t} \times \Gamma^{ind}_{j,t-1}$	−0.0074	−0.0121	−0.0205
	(0.0011)	(0.0033)	(0.0045)
$Post_{i,j,t}$	0.0815	0.0812	0.0992
	(0.0501)	(0.0484)	(0.0483)
$Region_i$-year fixed effects	Yes	Yes	Yes
$State_i$-$state_j$ fixed effects	Yes	Yes	Yes
$State_j$-year fixed effects	Yes	Yes	Yes
$State_i$–linear trend	Yes	Yes	Yes
Observations	54,250	57,700	57,700
R^2	0.6584	0.6583	0.6583

Notes: This table presents baseline specification where we dissect the effect by the properties of idiosyncratic shocks in state *j*. The dependent variable is the change in the real GDP growth rate in percentage. The main independent variable is $\Gamma^{ind}_{j,t-1}$, which denotes the idiosyncratic shocks in state *j* constructed by aggregating the Domar-weighted labor productivity shocks of top-ten firms by sales in state *j* after accounting for industry-year fixed effects. The unit of observation in each regression is a $state_i$-$state_j$-year pair. $Low - AR(1)$ takes a value of 1 if the shocks for a $state_j$ have an AR(1) estimate between the first and third quartile values. $LowR^2$ takes a value of 1 if the squared correlation of shock in $state_i$ with $state_j$ with $i \neq j$ is below the median value of R^2. R^2 between $state_i$ and $state_j$ are calculated by squaring the correlation coefficient of Γ between each pair and averaging the values over all $state_i$. **Neg = 1** takes a value of 1 if the shock in $state_j$ is a negative shock. All nonbinary variables used in the regression are standardized to mean zero and unit variance. Standard errors reported in parentheses are two-way clustered by $state_i$ and $state_j$.

when negative (positive) shocks in state *j* are geographically isolated. Results in column 2 show that, post-integration, economic growth in state *i* increase (decreases) more when negative (positive) shocks in state *j* exhibit low temporal correlation. The results seem to be dominant for shocks that lack temporal dynamics and spatial structure, strengthening our conjectures regarding the mechanism behind the baseline results.

While we attempt to construct shocks that have a low likelihood of being correlated with bank capital shocks, we cannot completely rule out this correlation. Hence, we study how the transmission varies with the sign of the shock. We posit that negative shocks are likely to affect banks' total amount of loanable funds by pushing banks closer to their constraint and, hence, are unlikely to be transmitted across state boundaries in the hypothesized fashion. Consistent with this hypothesis, we find that our effect is smaller in magnitude when shocks are negative; see

column 3 of Table 8. Further, we replicate our baseline table, constructing state-level idiosyncratic shocks using only positive firm-level shocks, shown in Supplemental Appendix Table G.5.

C. *Firms and Growth*

Thus far, the results indicate that banks allocate funds away from economies experiencing negative shocks and toward unaffected economies. In this section, we further examine the reallocation of funds by banks across firms. We hypothesize that firms that are more dependent on banks as a source of external financing drive the aggregate response in economic growth across states. We use age as a proxy for external finance dependence. Prior work has shown that firm age is a key determinant of external financing needs and bank dependence (Hadlock and Pierce 2010).

We show that younger firms are more responsive to foreign idiosyncratic shocks after banking integration. We segment firms into "young" and "old" based on median firm age across all firms. The differential response of "young" and "old" firms is presented in Table 9. Consistent with our hypothesis, we find that younger firms are more responsive to idiosyncratic foreign shocks after deregulation. We study debt growth in column 1, sales growth in column 2, market-to-book ratio in column 3, and work-in-progress inventory growth in column 4. After accounting for firm and industry-year fixed effects, we find that a one standard deviation idiosyncratic shock in state j is associated with a 0.60 standard deviation increase in debt growth, a 0.47 standard deviation increase in sales growth, a 0.46 standard deviation increase in market-to-book ratio, and a 0.77 standard deviation increase in work-in-progress inventory for young firms relative to old firms after banking integration. Hence, these findings corroborate our hypothesis that firms that are more dependent on banks as a source of external financing drive the aggregate response in economic growth.

D. *Domestic Small Firms*, *Banking Integration*, *and Shocks to Large Foreign Firms*

This section complements the analysis in Section IVC by documenting that idiosyncratic shocks to large firms in foreign states affect the idiosyncratic shocks to small firms in the home state. Specifically, we document that negative shocks to large firms in state j result in positive shocks to small firms in state i after the two states are financially integrated. We construct shocks to small firms in a state by aggregating the Domar-weighted labor productivity shocks experienced by firms below the top ten firms by sales after adjusting for industry $\times$ year fixed effects. Column 1 of Table 10 reports the results of regressing idiosyncratic shocks to small firms in state i on the interaction term of idiosyncratic shocks to large firms in state j and an indicator for the post-deregulation period. The estimate of interest is negative and statistically significant. The estimate indicates that negative shocks to large, less bank-dependent firms in the foreign state are transmitted as positive shocks to small bank-dependent firms in the home state following banking integration. As a falsification test, we present the regression of idiosyncratic shocks to large firms in state i on the interaction term of idiosyncratic shocks to large firms in state j and an indicator for the post-deregulation period. The underlying intuition of this test is

Table 9—Reallocation of Funds to Bank-Dependent Firms

	$\Delta\ln(Debt)_{f,t}$ (1)	$\Delta\ln(Sales)_{f,t}$ (2)	$\frac{M_{f,t}}{B_{f,t}}$ (3)	$\Delta\ln(Inventory)_{f,t}$ (4)
$Young_f \times Post_{i,t} \times \Gamma^{avg}_{j,t-1}$	−0.5992	−0.4722	−0.4601	−0.7717
	(0.3564)	(0.2515)	(0.1206)	(0.3424)
$Post_{i,t} \times \Gamma^{avg}_{j,t-1}$	0.3540	0.0857	−0.0265	0.3077
	(0.1534)	(0.1753)	(0.1038)	(0.2174)
$Young_f \times \Gamma^{avg}_{j,t-1}$	0.7788	0.1695	0.0531	0.6765
	(0.2003)	(0.1651)	(0.0683)	(0.2667)
$Young_f \times Post_{i,t}$	0.0173	0.2986	0.4332	−0.0815
	(0.0957)	(0.0974)	(0.1725)	(0.1477)
$Post_{i,t}$	0.1594	−0.0572	0.0170	0.1489
	(0.0857)	(0.0702)	(0.0657)	(0.0825)
$\Gamma^{avg}_{j,t-1}$	−0.3316	0.0341	0.3585	−0.3447
	(0.4277)	(0.2809)	(0.3468)	(0.3466)
Firm fixed effects	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes
Number of observations	19,369	19,324	20,305	11,862
R^2	0.3098	0.4641	0.5786	0.3482
Mean	0.0120	0.0717	2.3625	0.0106
Standard deviation	0.5185	0.3022	4.8366	0.2111

Notes: This table presents the results from a firm-level regression of characteristics of firm f, headquartered in state i at time t, on the triple interaction term $Young_f \times Post_{i,t} \times \Gamma^{avg}_{j,t-1}$. The triple interaction term measures the response of young firms relative to old firms following a shock in another state after banking integration of the two states. $Young_f$ is a firm-level variable that takes a value of 1 if the firm age is less than the median age of all firms and 0 otherwise. $Post_{i,t}$ is a continuous variable between 0 and 1 that denotes the fraction of other states that are integrated with state i via banking networks at time t. $\Gamma^{avg}_{j,t-1}$ denotes the average value of idiosyncratic shocks in all other states, $j \neq i$. Columns 1, 2, 3, and 4 use change in the natural logarithm of total debt, change in the natural logarithm of total sales, market-value-to-book-value ratio, and change in the natural logarithm of the work-in-progress inventory, respectively, as the dependent variables. Total debt is defined as the sum of debt in current liabilities and long-term debt. Total sales are defined as the net annual turnover. Market-to-book ratio is defined as the ratio of the sum of the market value of equity and assets to the book value of assets. Work-in-progress inventory is defined as total inventories—work in process. All regressions include firm and industry-year fixed effects. Industry refers to the four-digit SIC codes. The table includes data on all nonfinancial and non-utilities firms in Compustat from 1975 through 2000. The last two rows of the table indicate the mean and the standard deviation of the dependent variables. All variables are winsorized at the 1 percent level on both tails and standardized to a mean of 0 and standard deviation of 1. Standard errors reported in parentheses are clustered by state of the firm headquarters.

that larger firms are less reliant on bank financing and therefore are unlikely to be directly impacted by the reallocation of bank funds across borders. Consistent with this expectation, the estimate of interest is statistically insignificant, suggesting no discernible effect on large firms. Overall, the results presented in Table 10 provide valuable insights into the specific channel through which banks intermediate shocks via their networks.

E. *Bank Constraint and Transmission of Idiosyncratic Shocks*

Bank constraints are an important friction that plays a crucial role in the transmission of idiosyncratic shocks through banking networks. Geographically diversified, unconstrained banks are likely to operate at the first-best investment level across regions and, hence, have little scope to divert funding to other geographies when a

Table 10—Domestic Small Firms, Banking Integration, and Shocks to Large Foreign Firms

$\Gamma^{ind}_{i,t}$	Small firms (1)	Large firms (2)
$Post_{i,j,t} \times \Gamma^{ind}_{j,t-1}$	−0.0165	−0.0254
	(0.0075)	(0.0306)
$Post_{i,j,t}$	0.0286	−0.1175
	(0.0553)	(0.1832)
Region$_i$-year fixed effects	Yes	Yes
State$_i$-state$_j$ fixed effects	Yes	Yes
State$_j$-year fixed effects	Yes	Yes
State$_i$–linear trend	Yes	Yes
Observations	40,474	40,474
R^2	0.2745	0.2827

Notes: This table reports the results from regressing the idiosyncratic shock experienced by small and large firms in the home state on lagged foreign idiosyncratic shocks. The dependent variable is the idiosyncratic shock experienced in the home state by small (large) firms in column 1 (2). The main independent variable is $\Gamma^{ind}_{j,t-1}$, which denotes the idiosyncratic shocks in state j constructed by aggregating the Domar-weighted labor productivity shocks of top-ten firms by sales in state j. The unit of observation in each regression is a state$_i$-state$_j$-year pair. All nonbinary variables used in the regression are standardized to mean zero and unit variance. Standard errors reported in parentheses are two-way clustered by state$_i$ and state$_j$.

particular geography is hit by an idiosyncratic shock. However, constrained banks cannot exhaust the set of available investment opportunities due to their limited supply of funds. Hence, constrained banks are more likely to transmit idiosyncratic shocks across geographies. This section documents the role of the constrained banking sector in the transmission of idiosyncratic shocks across geographies.

Table 11 reports the results. We measure the average constraint of banks in a state by weighting a bank's constraint in each state by the bank's share of lending in that state. Bank constraint is measured as the ratio of liabilities to assets. Our coefficient of interest—the triple interaction of idiosyncratic shocks to large firms in state j, average constraint of banks in state j in the pre-deregulation period, and the indicator for the post-deregulation period—is negative, economically large, and statistically significant. Moreover, our estimate of the interaction term of idiosyncratic shocks to large firms in state j and the indicator for the post-deregulation period decreases in magnitude. Together, these results indicate that bank capital constraint plays a crucial role in driving the transmission of idiosyncratic shocks across geographies.

F. *Model*

In this section, we provide an overview of the theoretical model presented in Kalemli-Özcan, Papaioannou, and Perri (2013b) and leverage this framework to demonstrate that financial integration is the primary mechanism linking shocks to economic growth. The model enables us to conduct a counterfactual analysis, which allows us to examine how the relationship between economic growth in state

Table 11—Bank Constraint and Transmission of Idiosyncratic Shocks

ΔGDP_{it}	(1)	(2)	(3)
$Post_{i,j,t} \times \Gamma^{ind}_{j,t-1}$	−0.0164	−0.0148	0.0630
	(0.0007)	(0.0011)	(0.0339)
$Post_{i,j,t}$	0.0783	0.5134	0.5066
	(0.0491)	(0.1768)	(0.1755)
$Post_{i,j,t} \times Constrained_j$		−0.8572	−0.8431
		(0.3360)	(0.3325)
$Post_{i,j,t} \times \Gamma^{ind}_{j,t-1} \times Constrained_j$			−0.1481
			(0.0657)
Region$_i$-year fixed effects	Yes	Yes	Yes
State$_i$-state$_j$ fixed effects	Yes	Yes	Yes
State$_j$-year fixed effects	Yes	Yes	Yes
State$_i$–linear trend	Yes	Yes	Yes
Observations	57,700	57,700	57,700
R^2	0.6583	0.6583	0.6583

Notes: This table reports the results from the estimation of baseline specification. The dependent variable is the change in the real GDP growth rate in percentage. The main independent variable is $\Gamma^{ind}_{j,t-1}$, which denotes the idiosyncratic shocks in state j constructed by aggregating the Domar-weighted labor productivity shocks of top-ten firms by sales in state j. The unit of observation in each regression is a state$_i$-state$_j$-year pair. We account for the degree of bank constraint for each state by weighting a bank's level of constraint in each state by the bank's share of lending in that particular state. Bank constraint is measured as $\frac{Liabilities}{Assets}$ in each year. We sum across these values at the state-year level to produce a measure of bank constraint at the state level for each year. We use the mean value of the state-bank constraint in the years prior to deregulation as our measure of *Constrained*. All nonbinary variables used in the regression are standardized to mean zero and unit variance. Standard errors reported in parentheses are two-way clustered by state$_i$ and state$_j$.

i and idiosyncratic shocks in state j changes based on the ex ante correlation of idiosyncratic shocks. This counterfactual exercise helps us assess the validity of the exclusion restriction discussed in Section IIID.

Overview.—In the model, there are two countries—e.g., *home* and *foreign*—each with two segments with size λ and $1 - \lambda$, respectively. The λ segments (segment 2) of each country are financially integrated, while the $1 - \lambda$ segments are financially separate (segment 1); i.e., a $1 - \lambda$ share of the domestic and foreign economies operate in autarky so that banks intermediate only between households and firms in that $1 - \lambda$ segment, respectively. In each segment of each country, there are households that supply labor to firms, and borrow and save with banks. Firms pay dividends and wages to the households and make investment decisions. It is assumed that firms need to pay workers before they realize sales; hence, firms must fund their working capital needs via external funding provided by banks. Banks in segment 2 of each country are *global banks*. For illustration of the schema of the economy in the model, we reproduce Figure 1 from Kalemli-Özcan, Papaioannou, and Perri (2013a) in Supplemental Appendix Figure F.1. The model focuses on two types of stochastic shocks that drive economic fluctuations—(i) standard productivity shocks and (ii) banking shocks that affect the value of risky assets held by banks. In particular, we use this DSGE model to study how exogenous changes to financial

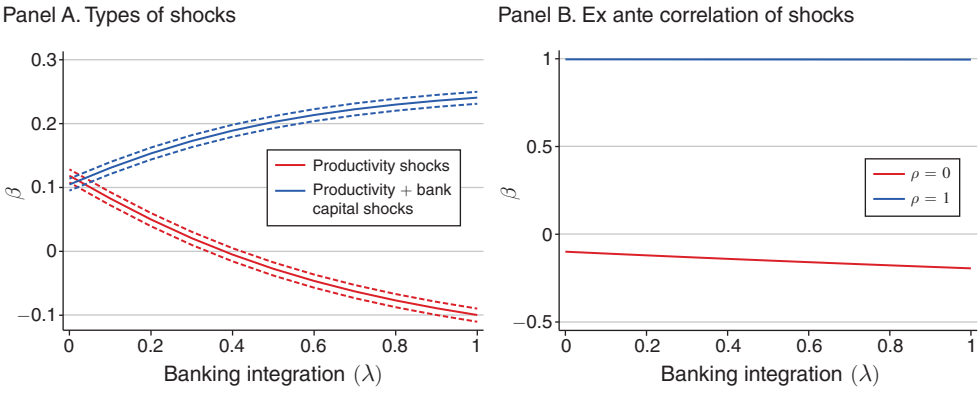

FIGURE 6. DOMESTIC GROWTH, FOREIGN SHOCKS AND BANKING INTEGRATION

Notes: The figure plots the relationship between domestic growth and foreign shocks for different levels of banking integration. We run the regression $\Delta GDP_{i,t} = \alpha + \beta\Gamma_{j,t} + \varepsilon_{i,t}$ and estimate β for different values of banking integration, λ, between i and j. Figure 6, panel A plots the relationship for different types of shocks—productivity shocks or idiosyncratic shocks and productivity shocks along with bank capital shocks. Figure 6, panel B plots the value of β for different values of $\lambda \in [0, 1]$ based on ex ante correlation, ρ, of nonbank capital shocks between the domestic and the foreign economy. The shocks used in Figure 6, panel B are productivity shocks.

integration affect the cross-border transmission of shocks. We interpret standard productivity shocks as idiosyncratic shocks and banking shocks as shocks that affect bank capital. In this stylized model, bank lending to firms is risk-free; hence, productivity shocks do not affect bank capital—productivity shocks alter the demand for loans of firms experiencing these shocks. We refer the readers to Supplemental Appendix F for in-depth discussion on model details such as setup, solution, calibration, etc.

Results.—We generate synthetic data from the model to study the relation between economic growth in state i (home) and shocks in state j (foreign) as we increase the level of banking integration between the two. We focus on two distinct scenarios: productivity shocks only, and productivity and bank capital shocks. We run the regression of economic growth in state i on the two sets of shocks in state j for each value of λ and estimate the regression β. Figure 6, panel A presents this result. The key result is that the relation between economic growth in state i and idiosyncratic shocks in state j changes with the degree of banking integration, λ. For foreign idiosyncratic shocks (blue line), β decreases in the degree of banking integration. For foreign bank capital shocks (red line), β increases in the degree of banking integration. As noted earlier, the distinction between the two shocks is that bank capital shocks alter the total supply of capital available for lending, whereas idiosyncratic shocks change the relative share of lending by affecting demand.

The diversification benefits of the geographic expansion of banks may only be realized if shocks being faced by banks are geographically isolated. To test this, we consider two counterfactual scenarios—one where the productivity shocks have zero spatial correlation, $\rho = 0$, and another where productivity shocks are perfectly positively correlated, $\rho = +1$, across geographies. The correlation in productivity shock reflects the strength of the relation between the two states via

nonbanking linkages such as trade, input/output, etc. Positive correlation in productivity shocks reflects positive correlation in loan demand. Hence, a negative shock in state *j* reduces loan demand in both states *i* and *j*, dampening the loan supply effect. Figure 6, panel B plots this result. The blue and the red lines plot the β for the regression of economic growth in state *i* and productivity shocks in state *j* with $\rho = 0$ and $\rho = +1$, respectively. We use this result to make two points. First, the change in β is more pronounced when shocks are geographically diversifiable, $\rho = 0$, as in our mechanism. Second, this result aligns with our identification strategy, which posits that when states exhibit complementary behavior with positive demand correlation, our estimation strategy is biased toward finding a null effect due to the presence of nonbanking channels.

V. Robustness

We conduct a battery of robustness tests to ensure that our results are invariant to alternative measurements of idiosyncratic shocks, geography-based measurement error in idiosyncratic shocks, and endogeneity of banking integration.

A. *How Well Do Our Shocks Capture Idiosyncratic Shocks?*

Our interpretation of the findings relies on the assumption that idiosyncratic shocks represent shocks originating outside the banking system. However, various factors may cause these shocks to not exclusively reflect idiosyncratic elements. Large local economic shocks may simultaneously impact both large and small firms, while shocks to large firms may have spillover effects on smaller firms. In both cases, our idiosyncratic shocks may inadvertently capture shocks that contemporaneously affect bank capital, potentially confounding our results. In this section, we argue that these concerns are unlikely to threaten our core hypothesis for five reasons.

First, contamination of idiosyncratic shocks by capital shocks will underbias our results, as the latter will result in amplification rather than the substitution effect that we argue. Second, we present a narrative analysis of our constructed shocks in Section IIC to show that we are indeed capturing idiosyncratic shocks specific to large firms.

Third, our shock construction methodology orthogonalizes industry $\times$ year fixed effects in an attempt to measure shocks that are devoid of industry-specific macroeconomic cycles that can affect both large and small firms and, hence, bank capital. However, our shocks may still capture some degree of state-specific macroeconomic cycles. We address this concern by modifying our shock construction methodology to orthogonalize state $\times$ year fixed effects in addition to the industry $\times$ year fixed effects. Supplemental Appendix Table G.2 reports the results from our baseline estimation, using shocks that are orthogonalized to state $\times$ year fixed effects in addition to the baseline industry $\times$ year fixed effects. Qualitatively, the results reported in Supplemental Appendix Table G.2 are similar to the results reported in our baseline Table 3. Quantitatively, the magnitude of the estimate presented using our modified shock construction approach is larger than the magnitude of the estimate in Table 3. This not only indicates that our results are robust to partialing out all state-specific

shocks but strengthens our first argument that the estimate under our hypothesis is likely to be understated if our shocks are contaminated by bank capital shocks.

Fourth, we examine whether idiosyncratic shocks to small firms predict those of large firms.[18] This could be a concern if a state-level banking shock helps small firms, which in turn benefits their large firm customers. Supplemental Appendix Table G.3 presents the results, showing that shocks to small firms have limited predictive power for shocks to large firms. Quantitatively, shocks to small firms explain only 1.22 percent of the variation in shocks to large firms.

We extend our baseline specification by incorporating interaction terms between the post-deregulation indicator and aggregated shocks to small firms in both the home state and foreign state. The results, presented in Table 12, show that our key finding remains unchanged. The interaction between idiosyncratic shocks to large firms and the post-deregulation indicator is negative, statistically significant, and consistent with our baseline estimate. This suggests that our baseline estimate is robust to controlling for shocks to small firms in both the home state and foreign state. Notably, the interaction term between idiosyncratic shocks to small firms and the post-deregulation indicator is positive. This supports the idea that shocks to small firms affecting bank capital are transmitted across states after banking integration, similar to a bank capital shock.

Fifth, we rerun our baseline specification after explicitly controlling for bank capital shocks measured through loan loss provisions of banks in the foreign state. Specifically, we control for the interaction term of aggregate loan loss provisions of banks in state j and the indicator for the post-deregulation period. We calculate aggregate loan loss provisions at the state level by aggregating the loan loss provisions of individual banks, weighted by their lending share within the state. Specifically, for each state, we sum the product of each bank's lending share within that state and its loan loss provision, across all banks. Table 13 presents the results from this exercise. Our coefficient of interest—the interaction of idiosyncratic shocks and the indicator for the post-deregulation period—is negative, statistically significant, and qualitatively similar to our baseline estimate. This evidence strengthens our confidence that the shocks are not contaminated by other factors affecting bank capital.

B. *Alternative Transmission Mechanisms*

Another concern of our identification strategy is that preexisting nonbanking relationships between two states may drive our findings. This may be due to the endogeneity of banking deregulation, the transmission of shocks through nonbanking channels, or the interaction of the newly formed banking channel with these preexisting nonbanking channels that can result in the transmission of shocks between two states following banking deregulation.[19]

[18] *Small* firms are those that are not among the top ten firms in terms of sales in the state in which they are headquartered. *Large* firms are defined as those that *are* among the top ten firms in terms of sales in the state in which they are headquartered.

[19] The analysis in this section controls for covariates based on their pre-deregulation values. However, for completeness we also present our results with time-varying characteristics. Supplemental Appendix Table G.4 reports the results with time-varying covariates and finds similar results to ones reported in Table 14.

Table 12—Baseline specification Accounting for Shocks to Small and Large Firms

ΔGDP_{it}	(1)	(2)	(3)	(4)	(5)
$Post_{i,j,t} \times \Gamma^{ind}_{j,t-1}$	−0.0164	−0.0141	−0.0171	−0.0055	−0.0105
	(0.0007)	(0.0015)	(0.0031)	(0.0055)	(0.0052)
$Post_{i,j,t} \times \Gamma^{ind}_{i,t-1}$		0.0033	0.0036	−0.0109	−0.0118
		(0.0225)	(0.0228)	(0.0208)	(0.0211)
$\Gamma^{ind}_{i,t-1}$		−0.0010	−0.0013	0.0211	0.0215
		(0.0166)	(0.0168)	(0.0137)	(0.0137)
$Post_{i,j,t}$	0.0783	0.0665	0.0720	0.1199	0.1212
	(0.0491)	(0.0502)	(0.0512)	(0.0591)	(0.0615)
$Post_{i,j,t} \times \Gamma^{ind,small}_{j,t-1}$			0.0069		0.0130
			(0.0045)		(0.0066)
$\Gamma^{ind,small}_{i,t-1}$				−0.0124	−0.0139
				(0.0174)	(0.0171)
$Post_{i,j,t} \times \Gamma^{ind,small}_{i,t-1}$				0.0232	0.0253
				(0.0246)	(0.0243)
Region$_i$-year fixed effects	Yes	Yes	Yes	Yes	Yes
State$_i$-state$_j$ fixed effects	Yes	Yes	Yes	Yes	Yes
State$_j$-year fixed effects	Yes	Yes	Yes	Yes	Yes
State$_i$–linear trend	Yes	Yes	Yes	Yes	Yes
Observations	57,700	56,758	39,741	39,741	27,644
R^2	0.6583	0.6897	0.6908	0.7467	0.7480

Notes: This table reports the results from the estimation of baseline specification. The dependent variable is the change in the real GDP growth rate in percentage. The main independent variable is $\Gamma^{ind}_{j,t-1}$, which denotes the idiosyncratic shocks in state j constructed by aggregating the Domar-weighted labor productivity shocks of top-ten firms by sales in state j. $\Gamma^{ind,small}_{j,t-1}$ ($\Gamma^{small}_{i,t-1}$) denotes the idiosyncratic shocks in state j (i) constructed by aggregating the Domar-weighted labor productivity shocks of small firms—firms that are in not in the top ten firms by sales—in state j (i). $\Gamma^{ind}_{j,t-1}$ ($\Gamma^{ind}_{i,t-1}$) denotes the idiosyncratic shocks in state j (i) constructed by aggregating the Domar-weighted labor productivity shocks of large firms—firms that are in the top ten firms by sales—in state j (i). The unit of observation in each regression is a state$_i$-state$_j$-year pair. All nonbinary variables used in the regression are standardized to mean zero and unit variance. Standard errors reported in parentheses are two-way clustered by state$_i$ and state$_j$.

We account for these concerns by controlling for the trade relationship, personal income comovement, GDP comovement, and state-pair proximity in industry composition. First, we measure the trade relationship between two states, using the share of goods exported from state i to j and imports into state i from state j. The data on bilateral trade flows are from 1977 and come from Michalski and Ors (2012a). Second, we construct the covariance in personal income growth and GDP growth between two states, using data from the pre-deregulation period to account for the comovement in personal income growth or business cycles between two states. Third, we construct the state-pair proximity by industry composition, measured by the Euclidean distance of the share of employment in 77 industries between the two states. This number is large when the two states have very different industrial specializations. Additionally, we also control for the pre-period average of income per capita in the foreign state.

Table 14 presents the baseline specification augmented with interaction terms between foreign idiosyncratic shocks and several control variables: share of exports and imports, foreign state income per capita, income covariance, industry similarity, and GDP growth covariance. These interactions account for potential

TABLE 13—ESTIMATION ACCOUNTING FOR LOAN LOSS PROVISION SHOCKS

ΔGDP_{it}	(1)	(2)	(3)
$Post_{i,j,t} \times \Gamma^{ind}_{j,t-1}$	−0.0164	−0.0161	−0.0162
	(0.0007)	(0.0019)	(0.0022)
$Post_{i,j,t}$	0.0783	0.0773	0.0772
	(0.0491)	(0.0491)	(0.0492)
$Post_{i,j,t} \times LLP_{j,t-1}$		−0.0118	−0.0120
		(0.0012)	(0.0014)
$Post_{i,j,t} \times \Gamma^{ind}_{j,t-1} \times LLP_{j,t-1}$			0.0008
			(0.0039)
$Region_i$-year fixed effects	Yes	Yes	Yes
$State_i$-$state_j$ fixed effects	Yes	Yes	Yes
$State_j$-year fixed effects	Yes	Yes	Yes
$State_i$–linear trend	Yes	Yes	Yes
Observations	57,700	57,700	57,700
R^2	0.6583	0.6583	0.6583

Notes: This table reports the results from the estimation of baseline specification. The dependent variable is the change in the real GDP growth rate in percentage. The main independent variable is $\Gamma^{ind}_{j,t-1}$, which denotes the idiosyncratic shocks in state j constructed by aggregating the Domar-weighted labor productivity shocks of top-ten firms by sales in state j. The unit of observation in each regression is a $state_i$-$state_j$-year pair. We account for annual loan loss provision for each state. We weight a bank's loan loss provision in each state by banks' share of lending in that particular state. We then sum across these values at the state-year level to produce a measure of loan loss provision at the state level. All nonbinary variables used in the regression are standardized to mean zero and unit variance. Standard errors reported in parentheses are two-way clustered by $state_i$ and $state_j$.

preexisting nonbanking channels of shock transmission. We further include triple interaction terms involving these covariates, foreign idiosyncratic shocks, and the post-deregulation indicator to address the possibility that our findings are driven by interactions between the new banking channel and existing nonbanking channels. Finally, we control for interactions between these covariates and the post-deregulation indicator. Our core estimate, the interaction of idiosyncratic shocks and the post-deregulation indicator, remains negative, statistically significant, and qualitatively similar to the baseline, suggesting that our results are unlikely to be driven by preexisting nonbanking channels.

C. *Alternative Measures of Idiosyncratic Shocks*

We conduct several robustness checks to verify the sensitivity of our results to the methodology used in constructing idiosyncratic shocks. First, we alter the construction methodology by using the top 20 and top 30 firms instead of the top 10 firms. Second, we employ a time-invariant measure of idiosyncratic shocks using a time series average of shocks in a state. Third, we adjust idiosyncratic shocks for aggregate temporal shocks instead of industry-level temporal shocks. Our results remain robust across these alternative measures (see Supplemental Appendix Table G.6). Fourth, we test the sensitivity of our results to states where top-ten firms' share of sales is high. We repeat our baseline analysis with alternative samples, excluding

Table 14—Baseline Specification with Controls

ΔGDP_{it}	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$Post_{i,j,t} \times \Gamma^{ind}_{j,t-1}$	−0.0194	−0.0161	−0.0187	−0.0190	−0.0219	−0.0183	−0.0121
	(0.0005)	(0.0033)	(0.0084)	(0.0047)	(0.0030)	(0.0022)	(0.0068)
$Post_{i,j,t} \times \Gamma^{ind}_{j,t-1} \times \text{Exports}_{i,j,pre}$		−0.0057					−0.0054
		(0.0020)					(0.0044)
$Post_{i,j,t} \times \text{Exports}_{i,j,pre}$		−0.0071					−0.0099
		(0.0054)					(0.0052)
$\Gamma^{ind}_{j,t-1} \times \text{Exports}_{i,j,pre}$		−0.0005					−0.0011
		(0.0006)					(0.0030)
$Post_{i,j,t} \times \Gamma^{ind}_{j,t-1} \times \text{Imports}_{i,j,pre}$		−0.0019					−0.0000
		(0.0050)					(0.0074)
$Post_{i,j,t} \times \text{Imports}_{i,j,pre}$		−0.0051					−0.0071
		(0.0095)					(0.0097)
$\Gamma^{ind}_{j,t-1} \times \text{Imports}_{i,j,pre}$		0.0060					0.0014
		(0.0034)					(0.0061)
$Post_{i,j,t} \times \Gamma^{ind}_{j,t-1} \times \text{Income}_{j,pre}$			0.0131				0.0102
			(0.0102)				(0.0083)
$Post_{i,j,t} \times \text{Income}_{j,pre}$			0.0345				0.0436
			(0.0232)				(0.0326)
$\Gamma^{ind}_{j,t-1} \times \text{Income}_{j,pre}$			−0.0047				−0.0015
			(0.0083)				(0.0074)
$Post_{i,j,t} \times \Gamma^{ind}_{j,t-1} \times \text{Income Covariance}_{i,j,pre}$				−0.0067			−0.0084
				(0.0114)			(0.0145)
$Post_{i,j,t} \times \text{Income Covariance}_{i,j,pre}$				0.0020			0.0143
				(0.0635)			(0.0831)
$\Gamma^{ind}_{j,t-1} \times \text{Income Covariance}_{i,j,pre}$				0.0054			0.0017
				(0.0056)			(0.0063)
$Post_{i,j,t} \times \Gamma^{ind}_{j,t-1} \times \text{GDP Covariance}_{i,j,pre}$					−0.0039		0.0007
					(0.0090)		(0.0113)
$Post_{i,j,t} \times \text{GDP Covariance}_{i,j,pre}$					0.0036		0.0087
					(0.0168)		(0.0177)
$\Gamma^{ind}_{j,t-1} \times \text{GDP Covariance}_{i,j,pre}$					0.0072		0.0055
					(0.0084)		(0.0097)
$Post_{i,j,t} \times \Gamma^{ind}_{j,t-1} \times \text{Industry Similarity}_{i,j,pre}$						0.0022	−0.0020
						(0.0062)	(0.0088)
$Post_{i,j,t} \times \text{Industry Similarity}_{i,j,pre}$						−0.0072	−0.0065
						(0.0235)	(0.0320)
$\Gamma^{ind}_{j,t-1} \times \text{Industry Similarity}_{i,j,pre}$						−0.0072	−0.0052
						(0.0037)	(0.0054)
$Post_{i,j,t}$	0.0874	0.0879	0.0955	0.0871	0.0869	0.0875	0.0963
	(0.0518)	(0.0519)	(0.0552)	(0.0523)	(0.0523)	(0.0517)	(0.0551)
Region_i-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State_i-state_j fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State_j-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State_i–linear trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	53,445	53,445	53,445	53,445	53,445	53,445	53,445
R^2	0.6661	0.6661	0.6662	0.6661	0.6661	0.6661	0.6663

Notes: This table reports the results from the estimation of baseline specification. The dependent variable is the change in the real GDP growth rate in percentage. The main independent variable is $\Gamma^{ind}_{j,t-1}$, which denotes the idiosyncratic shocks in state j constructed by aggregating the Domar-weighted labor productivity shocks of top-ten firms by sales in state j after accounting for industry-year fixed effects. The unit of observation in each regression is a state_i-state_j-year pair. We include additional control variables, including exports from state i to state j, imports from state i to state j, personal income per capita in state j, the similarity in industry composition between states i and j, and covariance in personal income growth and GDP between states i and j measured before deregulation, which are trimmed at 1 percent. All nonbinary variables used in the regression are standardized to mean zero and unit variance. Standard errors reported in parentheses are two-way clustered by state_i and state_j.

states where the top 10 firms account for more than 95 percent, 90 percent, 80 percent, and 70 percent of all sales. The point estimate remains insensitive to these alternative samples (see Supplemental Appendix Table G.7). Fifth, we reconstruct state-level idiosyncratic shocks by partialing out the idiosyncratic shocks to small firms from those to large firms using three methods: (i) directly controlling for idiosyncratic shocks to small firms in state j, (ii) subtracting idiosyncratic shocks to small firms from those to large firms, and (iii) regressing idiosyncratic shocks to large firms on those to small firms and using the residuals as the measure of idiosyncratic shocks. Our baseline results remain robust across these methods (see Supplemental Appendix Table G.8). Furthermore, we reconstruct idiosyncratic shocks assuming heterogeneous but time-invariant exposure to aggregate macroeconomic shocks. Under this factor structure assumption, we find that the point estimate for shocks constructed using this framework is quantitatively similar to our baseline estimates (see Supplemental Appendix Table G.11). For further discussion on methodology, properties, and baseline results using shocks constructed under the factor structure methodology, we refer readers to Supplemental Appendix G.2.

D. *Placebo Test*

We conduct a placebo test wherein we randomize the timing of banking integration. This test addresses two concerns. First, it addresses whether the timing of banking integration is meaningful by checking whether the results disappear if the timing is randomly selected. Second, it verifies that results are not driven by omitted variable bias, as long as the structure of omitted variables is identical across state pairs. A placebo deregulation year is generated for each state pair (i,j) from a uniform distribution between 1982 and 1994. The baseline specification is estimated using the generated placebo year. We estimate this process 3,500 times. Supplemental Appendix Figure G.3 plots the kernel density of the point estimates of $Placebo - Post_{i,j,t} \times \Gamma_{j,t-1}$ obtained from 3,500 Monte Carlo simulations where we randomize the timing of state-pairwise banking integration. The distribution of the coefficient of the interaction term is centered around zero, with a mean and standard deviation of 0.0001 and 0.0076, respectively. The dashed red line indicates the estimated point estimate from our baseline regression in Table 3, with 1.74 percent of the estimated coefficients of the $Placebo - Post_{i,j,t} \times \Gamma_{j,t-1}$ lying to the left of the dashed line. Hence, we can argue that the timing of banking integration is special and that results are unlikely to be driven by omitted variables as long as the structure of such variables is identical across state pairs.[20]

[20] In an alternative placebo test we randomize the idiosyncratic shocks in state j and estimate the coefficient of the interaction term of $Post_{i,j,t} \times Placebo - \Gamma_{j,t-1}$. The results disappear with randomization of Γ, ruling out the claim that the results are spurious in nature (see Supplemental Appendix G.3.2).

E. *Addressing Geography-Based Measurement Error*

In the construction of Γ in Section IIC, we assume that a firm's sales and employment are located in the same state as its headquarters. This assumption may introduce measurement error, but we argue that its impact is likely minimal for two reasons. First, measurement error is expected to be small, as headquarters and production facilities tend to be clustered in the same state (Chaney, Sraer, and Thesmar 2012). Moreover, headquarters represent a significant fraction of corporate real estate assets, and on average, firms have a substantial proportion of their employees at their headquarters (Barrot and Sauvagnat 2016). Specifically, the average (median) Compustat firm in their sample has 60 percent (67 percent) of its employees at its headquarters. Second, even if measurement error is significant, it is likely to bias our estimates against finding the proposed effect. Therefore, our results are likely to be conservative, and the true effect may be even more pronounced than what we estimate.[21]

Nevertheless, we compute two alternative measures of state-level shocks to circumvent the measurement issue and find qualitatively similar results (see Supplemental Appendix G.4). The first measure is constructed by aggregating annual growth in GDP contribution from each industry within a state, adjusted for the annual aggregate growth in GDP contribution from each industry. This measure is constructed from the BEA data and is immune to geography-based measurement error. The second measure is constructed based on discovery of new oil reserves. These oil discoveries at the state level are likely to result in positive local idiosyncratic shocks. While both measures alleviate concerns associated with geography-based measurement error, they are limited by other issues. The value-added shocks are likely to be endogenous to the banking sector, as they include shocks from both the large and the small firms. The oil discovery shocks can only be created for a smaller sample of states, resulting in a test with low power. Additionally, the oil discovery shocks are predictable toward the later part of the sample, lessening the predictive power of these shocks even further.

F. *Addressing Concerns Related to Migration*

This section addresses concerns of whether the results presented in the paper are driven by interstate migration, contemporaneous with the state-pairwise banking integration. We address this concern in two ways. First, we assume that the tendency to move between state i and state j is likely to be similar or smooth across other states in the same economic regions as state i and state j, respectively. Under this assumption, we augment the baseline specification by including $\text{region}_i \times \text{region}_j \times$ year fixed effects, as well as $\text{region}_i \times \text{state}_j \times$ year, where region refers to the BEA economic region of the state. In an alternative test, we randomly form groups of

[21] Assume that a firm, headquartered in state j has majority of its employees and sales in state i, then Γ constructed using our methodology will wrongly attribute the idiosyncratic shock in state i to state j. As shown earlier idiosyncratic shock in state i have a positive correlation with future economic growth. Hence, under such a geography-based measurement error the estimate of the interaction term of Γ and *Post* will be either positive or zero, biasing our strategy against finding the proposed effect.

states of different sizes and control for the random-region$_i$ $\times$ random-region$_j$ $\times$ year fixed effects, and random-region$_i$ $\times$ state$_j$ $\times$ year in the baseline specification. We repeat this process of randomization of states into groups 3,500 times and estimate the distribution of the interaction term of the $\text{Post}_{i,j,t}$ and $\Gamma^{ind}_{j,t-1}$. The second test, in contrast to the first test, assumes that the choice set of within-US migration is coarsely distributed across space. Supplemental Appendix G.5 discusses these results and finds that the coefficient of interest is qualitatively and statistically similar to the baseline results. Hence, our findings are unlikely to be driven by contemporaneous migration.

VI. Great Moderation and Banking Integration

The Great Moderation refers to a period of stable macroeconomic activity starting from the mid-1980s. While several explanations have been proposed to explain the Great Moderation (see Davis and Kahn 2008), the three most common hypotheses explaining the Great Moderation are good luck (Stock and Watson 2002), improvements in monetary policy (Bernanke 2004), and broad-based structural change (Summers 2005). In this paper, we posit a new hypothesis to explain the relative quiescence in aggregate volatility.

We propose an alternative mechanism that explains the persistence of lower macroeconomic volatility during the Great Moderation. We argue that banking reforms—namely, banking deregulation that took effect during the 1980s and 1990s—increased the overall role of banks in intermediating shocks between states. We have shown that during the later 1970s and early 1980s, idiosyncratic shocks in one state were positively correlated with economic growth in another state, suggesting that in the absence of banking linkages, states behaved as complements. However, this monotonically reversed post-1984, during which states began behaving like substitutes. We have attributed this change in the cross-border transmission of productivity shocks to banking integration. As banks could cross state lines and operate, their investment choice set expanded, allowing them to geographically diversify their portfolio. In other words, prior to banking integration, when shocks in one state were correlated with growth in another, aggregate fluctuations for the overall US economy could be quite large. After banking integration, the negative cross-state correlation allowed banks to ultimately "hedge" their portfolio and reduce risk, lowering the level of aggregate fluctuations. Hence, banking integration provides a mechanism that explains "good luck" and why even large idiosyncratic shocks did not snowball into large aggregate fluctuations. Banking reforms altered the cross-border transmission of shocks; thus, the overall US economy did not react to exogenous shocks during the period of the Great Moderation as strongly as in previous periods.

We exploit the two-country model presented in Section IVF to demonstrate how banking integration can lead to a decrease in aggregate volatility. Banking integration influences the variance and covariance of economic growth between two geographies.[22] The data simulated from the model show that banking integration increases

[22] An extension of the simple framework in Supplemental Appendix A.1, presented in Supplemental Appendix A.2, yields similar conclusions about the effects of banking integration on variance and covariance.

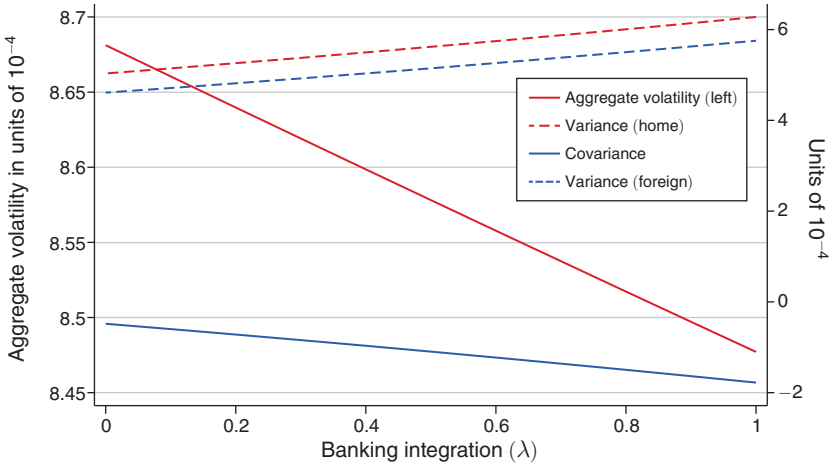

FIGURE 7. BANKING INTEGRATION, VARIANCE, COVARIANCE, AND AGGREGATE VOLATILITY

Notes: The figure plots the variance in economic growth for domestic and home county, the covariance in the economic growth of two countries, and the aggregate volatility of the system for different values of banking integration, λ. For each value of λ, we simulate the path of each economy with only productivity shocks such that these shocks have zero spatial correlation and zero persistence, and compute the value of variance and covariance of economic growth. Aggregate volatility is computed by adding the variance of economic growth of the two countries and twice the covariance.

and the covariance in economic growth between the two geographies decreases, while the variance in economic growth in each geography increases. The decrease in covariance is sufficiently large to compensate for the increase in variance, resulting in a net decrease in aggregate economic volatility for the entire system as banking integration increases. Figure 7 provides a visual depiction of this result. Specifically, when λ increases from 0 to 1, the variance in each geography increases by 25 percent, while the covariance decreases by 269 percent. Consequently, aggregate volatility decreases by 2 percent, with a 28 percent contribution from increased variance and a -30 percent contribution from decreased covariance. Notably, the decline in aggregate volatility is likely to be more pronounced in a multicountry setup, as shocks are distributed across a larger geographic area, amplifying the decrease in covariance while dampening the increase in individual geographic variance.

VII. Conclusion

In this paper, we identify the effect of banking networks on the cross-border transmission of idiosyncratic shocks. We introduce new empirical findings on how idiosyncratic shocks transmit through the economy via banks. Specifically, we provide evidence that geographically diversified banks divert funds away from states that experience negative shocks and toward unaffected state economies. While the extant empirical literature focuses on the transmission of bank capital shocks, the focus of this paper is on the transmission of idiosyncratic shocks through banking networks. Our results suggest that the transmission of idiosyncratic shocks result in negative comovement of business cycles.

We introduce several new stylized facts in this paper. First, we find that in the late 1970s and early 1980s, idiosyncratic shocks in state *j* were positively correlated with economic growth in state *i*, suggesting that two states operated as complements during this period. This relation monotonically changed after 1984 through 1994. Idiosyncratic shocks in state *j* are *negatively* correlated with economic growth in state *i*. Second, we attribute this change in relationship to contemporaneous changes in banking linkages across states. In the presence of banking linkages, shocks do not directly transmit cross-border—they are intermediated by banks, providing a mechanism for how idiosyncratic shocks in state *j* can affect economic growth in state *i* by changes in the share of bank loan supply across states. Third, we use this empirical setup to causally estimate the relation between changes in bank loan supply and economic growth. Concretely, we find that a 1 percent increase in bank loan supply is associated with a 0.06–0.25 percentage point increase in economic growth. Fourth, this mechanism has the potential to explain why the overall economy did not react to exogenous shocks during the Great Moderation as strongly as in previous periods.

Our findings have implications for policymakers in advanced and emerging economies. In recent years, the European Union has proposed and implemented steps toward the creation of a European Banking Union and European Capital Markets Union, part and parcel of a broader Economic and Monetary Union (EMU). These policies are intended to converge the economies of EU states and improve the resiliency of the EMU through a centralized "shock absorption" system. Our results suggest that a stronger banking union could lead to divergence of economic growth between member states in the presence of idiosyncratic shocks. Our results are also informative to policymakers in emerging market economies where the banking industry is gradually moving from state ownership to private ownership of banks. In the presence of idiosyncratic shocks and financially integrated banks, there may still be convergence across microeconomics of a country in the presence of welfare-maximizing or monopolistic banks, such as state-owned banks. With a high level of financial integration, moving from welfare-maximizing state-owned banks to profit-maximizing private banks may potentially result in the divergence of microeconomies of a country. We do not claim to settle these debates, but provide another dimension for deliberations while formulating such policies.

Finally, our work highlights how banks can aggregate idiosyncratic shocks in an economy. This aids our understanding of the origins of aggregate fluctuations. Study of the interaction of bank and idiosyncratic shocks and their effects on aggregate fluctuations provides an important avenue of future empirical research that can further the discussion on the nature of cross-border transmission of shocks.

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