The Bank Entry Channel of Monetary Policy Transmission*

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June 4, 2020

Abstract

We argue theoretically and show empirically that monetary policy can shape the structure of local deposit markets by affecting banks' entry decisions. Using network-based shocks to bank entry barriers, we find that entry becomes more responsive to monetary policy when entry barriers are lower. In turn, entry affects credit provision and monetary policy pass-through: local establishment and employment grow more in response to expansionary monetary policy when bank entry barriers are lower, and these real effects are stronger for small and medium-sized establishments. Our results suggest that expansionary monetary policy might be less effective when bank entry costs are high, as is currently the case.

JEL Codes: G21, G28, E51

Keywords: Market Structure, Monetary Policy Pass-Through, Commercial Banking Deregulation.

*We thank Andrew Bird, Mehmet Canayaz, Jess Cornaggia, Matt Gustafson, Peter Iliev, Holger Mueller, Deniz Okat, Wenlan Quian, seminar participants at Penn State and the Queensland University of Technology, and participants at the Asia-Pacific Corporate Finance Online Workshop for helpful comments.

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1 Introduction

From 2000 to 2009, regulators granted a total of 1,323 new commercial bank charters in the United States. However, from 2010 to 2019, only 40 new charters have been granted—a 97% reduction in bank entry.[1] How has this reduction in bank entry affected the transmission of monetary policy to the real economy? More generally, is there an interaction between monetary policy and bank entry, and how does this interaction affect monetary policy pass-through?

In this paper we argue theoretically and show empirically that monetary policy affects banks' decision to enter local deposit markets, which in turn affects monetary policy pass-through. Through the lens of a stylized model of bank deposit competition, we show that when entry is costly and when short-term assets are imperfect substitutes for bank deposits, a decrease in the Fed funds rate increases bank profitability and incentivizes entry by prospective bankers. In turn, entry reduces local banks' profits but increases overall credit provision in the economy, providing a new channel of monetary policy transmission. We refer to this channel as the *entry channel* of monetary policy.

We test our theory using novel network-based shocks to bank entry barriers arising from the removal of US interstate banking restrictions in the 1980s and 1990s (Bisetti, Karolyi, and Lewellen (2019)). We show that a reduction in entry barriers increases the negative sensitivity of bank entry to the Fed funds rate, such that expansionary monetary policy induces more entry into local markets when entry barriers are low. At the individual bank level, lower entry barriers decrease the sensitivity of bank deposits and loans to the Fed funds rate, as the positive profitability effect of a decrease in the Fed funds rate is offset by higher competition by other banks. At the aggregate (MSA and county) level, however, lower entry barriers *increase* the sensitivity of real outcomes to monetary policy: the sensitivity of local establishment growth and employment growth to the Fed funds rate increases when bank entry barriers are lower. Overall, our results provide evidence that monetary policy can shape the structure of local banking markets, with large effects on the real economy.

[1] Source: https://www.fdic.gov/regulations/laws/bankdecisions/depins/index.html. See also Kleiner, Puri, and Yom (2019), who examine the characteristics of the individuals who form banks. The recent decline in bank entry is often attributed to regulatory burden (Buchak, Matvos, Piskorski, and Seru (2018)), and it is of both theoretical and practical interest to policymakers (see, e.g., Pierce (2013) and Hoskins and Labonte (2015)). A former senior official (Jamie McAndrews) is currently suing the Federal Reserve for unfairly limiting bank competition (see, e.g., "Federal Reserve Is Sued, Accused of Limiting Competition," by Binyamin Appelbaum, *New York Times*, September 6, 2018).

We start by illustrating our mechanism through the lens of a stylized model of bank deposit competition subject to costly entry. In the model, both lending rates and deposit rates offered by banks are functions of the aggregate interest rate set by the monetary authority (i.e., the Fed funds rate), and bank profits are the difference between interest income from lending and interest expense from deposit borrowing. We allow for costly entry, such that prospective bankers find it optimal to enter the deposit market as long as their profits are larger than the entry cost.

We show that if the Fed funds are imperfect substitutes for deposits (e.g., because deposits provide liquidity services), expansionary monetary policy makes it relatively cheaper for banks to borrow, increases profitability, and stimulates lending.[2] On the other hand, however, increased profitability attracts new entrants thus producing an offsetting effect on banks' profitability and lending. At the aggregate level, the entry effect dominates and entry increases monetary policy pass-through, generating a new channel of monetary policy transmission.

Importantly, we show that entry barriers *decrease* the sensitivity of monetary policy to the entry channel. Intuitively, when entry barriers are high, banks cannot respond to a reduction in borrowing costs by entering new markets, which makes entry and aggregate lending less sensitive to monetary policy. For the same reason, entry barriers *increase* the sensitivity of individual banks' deposits and lending to monetary policy.

We test our theoretical predictions using novel network-based shocks to entry barriers arising from the US interstate banking deregulation of the 1980s and 1990s. Most of the interstate deregulation agreements of the 1980s and 1990s were passed as reciprocal agreements, whereby local banks were allowed to access another state only if banks from the other state themselves were allowed to access the local market. We exploit the reciprocal nature of this deregulation process to identify quasi-exogenous shocks to bank entry barriers, and use these shocks to compare entry, bank-level outcomes, and aggregate outcomes in areas with different entry barriers. In the language of our model, we compare states characterized by infinitely large entry costs—where a decrease in the Fed funds rate should not lead to changes in entry by out-of-state banks—and states in which these entry costs are exogenously reduced.

[2] In what follows, we refer to the Fed funds as any short-term asset whose return is equal to Fed funds rate and therefore is directly influenced by monetary policy.

We conceptualize the US state map as a directed network, whose nodes are states and whose edges are determined by the possibility of banks to enter other states. Our first measure of shocks to entry barriers, which we call *States In*, is the number of states that can access the local state at any given point in time.[3] We show that this measure provides more precise identification of the timing of deregulation shocks relative to previous workhorse measures of deregulation intensity (e.g., Kroszner and Strahan (1999)), and that the reciprocal nature of the interstate banking agreements—whereby one state is allowed to enter another state only subject to a reciprocity condition—makes the timing of this measure quasi-exogenous from the perspective of local banks.

We complement our main deregulation measure with a second measure, constructed as the difference between *States In* and the number of states that banks from the home state can access at any given point in time. We call this second measure *Net States In*, and we show that *Net States In* enjoys additional empirical properties that make this measure a good complement to *States In* as a proxy for shocks to in-state entry barriers. As an important example, we show that *Net States In* is by construction zero on average across states, reducing concerns of possible interactions between aggregate trends in interest rates, deregulation intensity, and real outcomes.[4]

In our empirical tests, we regress different outcome variables on time fixed effects (picking up variation in the Fed funds rate and other aggregate economic conditions), our *States In* and *Net States In* deregulation measures, and the interaction between the Fed funds rate and these deregulation measures. By focusing on the interaction term, we compare how a given outcome variable changes in response to a change in the Fed funds rate in states characterized by different entry barriers at a given point in time. Importantly, the low-order terms in our regressions control for the baseline correlation between the outcome variable and the Fed funds rate (as well as other aggregate economic conditions, captured by time fixed effects), and between the outcome variable and changes in the competitive landscape faced by local banks (captured by *States In* and *Net States In*).

Identifying the effect of entry barriers on the transmission of monetary policy— the interaction

[3] In network language, *States In* represents the in-degree centrality of our directed network of states.

[4] We argue that *Net States In* also captures two distinct aspects of the deregulation process for local banks, namely shocks to local competition and shocks to out-of-state investment opportunities. While this distinction is potentially relevant from the perspective of local banks, the entry channel mostly focuses on the competition effect. We verify this claim empirically by showing that our results do not change substantially when using *States In* and *Net States In* in our tests.

term between the Fed funds rate and our *States In* and *Net States In* variables—requires the assumption that changes in the structure of the deregulation network do not anticipate monetary policy. In the data, we confirm this identification assumption by showing that the correlation between changes in (*Net*) *States In* and changes in the Fed funds rate is zero. More in general, the interaction between bank regulation and monetary policy pass-through seems to be still unexplored today (BIS (2015), Yellen (2017)), which reduces concerns of such strategic interaction considerations being at play in the 1980s and 1990s.

We present the results of three main sets of tests, corresponding to three sets of outcome variables. In the first set of tests, we focus on bank entry into local markets, which we measure with state-quarter level bank acquisitions by out-of-state Bank Holding Companies (BHCs) and with state-quarter level establishments of new bank branches.[5]. In these tests, we first document a negative correlation between the Fed funds rate and bank entry. For example, we show that when the target Fed funds rate increases by 1% (e.g., going from 4% to 5%), bank acquisitions decrease by 25% relative to the average number of state-quarter acquisitions in our sample. More important, we document large interaction effects between the Fed funds rate and our shocks to barriers to entry. For example, we show that a 50-state increase in *States In* increases the sensitivity of bank acquisitions by around 70% of the baseline negative correlation between the Fed funds rate and bank acquisitions. Our results are robust to a series of robustness tests, including alternative measures for the Fed funds rate and the use of a number of different definitions for entry barriers shocks.[6] We also show that our results disappear when we use the Kroszner and Strahan (1999) workhorse interstate deregulation measure as a proxy for changes in bank entry barriers, suggesting that our network-based shocks capture important an timing dynamics not picked up by previous measures of interstate banking deregulation.

In our second set of tests, we study the effect of the entry channel on bank deposit-taking and lending. In these tests, the main dependent variables are bank deposit rates and quantities as well as lending rates and quantities. We first show that the sensitivity of bank interest expense rates (but

[5]The data comes from the FDIC Call Reports and Summary of Deposits (SOD)

[6]These measures include *One-Ways In* non-reciprocal agreements in which some states allow banks from other states to enter without reciprocity clauses, *National States In* stemming from US-wide agreements aimed at mitigating concerns of local coordination between neighboring states, and *Asset-Weighted States In* giving higher network-weights to states with a larger banking sector.

not that of interest income rates) to the aggregate Fed funds rate is higher when bank entry barriers are low, consistent with the argument that deregulation decreased banks' ability to extract rents in local deposit markets. Consistent with our theoretical predictions, we also show that the sensitivity of individual banks' deposit-taking and lending is *lower* when bank entry barriers are low, since the positive (negative) effect of a decrease (increase) in interest rates is compensated by higher (lower) entry.

In our third set of tests, we turn to the real effects of the entry channel. Using annual MSA-level data from the Business Dynamics Survey on local establishments and employment, we show that the entry channel increases the sensitivity of firm growth, establishment entry and exit rates, employment growth, and job creation and destruction rates to monetary policy. For example, we show that local firm growth increases more in response to a decrease in the Fed funds rate when bank entry barriers are low than when bank entry barriers are high, and that the rate of establishment exit decreases more in response to a decrease in the Fed funds rate when bank entry barriers are low. These results are robust across the same battery of robustness checks performed on the bank entry tests (e.g., weighting state-network nodes by the size of local banking markets). Interestingly, we show that the real effects of the entry channel stem from *negative* changes in the Fed funds rate, suggesting that the entry channel is more effective in amplifying monetary policy transmission in periods of expansionary monetary policy, and that entry into local banking markets is only a partially-reversible investment. One implication of this result is that reducing bank entry barriers could stimulate the real economy in the current environment of low interest rates.

In the last part of the paper, we investigate which dimension(s) of local market heterogeneity are most important for the transmission of monetary policy through the entry channel. Using annual county-level data from the County Business Patterns dataset, we show that our real effects are more concentrated in metropolitan areas and in areas where the local banking sector is ex-ante more levered. Moreover, we show that the entry channel disproportionately affects the growth of small and medium-sized establishments with less than 100 employees, rather than the growth of larger establishments. Collectively, these cross-sectional results not only confirm our main findings using completely different data, but also contain the important policy implication that barriers to bank entry

might negatively affect the pass-through of expansionary monetary policy to small, credit-rationed businesses.

Our paper contributes to the literatures on bank market power, monetary policy pass-through, and bank deregulation in numerous ways. First, our paper contributes to the literature on monetary policy pass-through. While this literature is too large to fully list, a significant empirical literature has examined various characteristics of local banking markets, and the banking sector that affect monetary policy transmission. Particularly relevant characteristics include the liquidity of bank balance sheets, bank size, bank capital, and the level of securitization activity (Kashyap and Stein (2000); Kishan and Opiela (2000); Gambacorta (2005); Altunbas, Gambacorta, and Marques-Ibanez (2009); Agarwal, Chomsisengphet, Mahoney, and Stroebel (2017); Beraja, Fuster, Hurst, and Vavra (2019); Williams (2019)). We complement this literature by arguing theoretically and showing empirically that monetary policy has an impact on the structure of local deposit markets, thus affecting the overall provision of credit and the real economy.

Our paper also contributes to a large literature documenting evidence that bank market power affects deposit-taking and lending, especially through a monetary policy channel, using cross-sectional variation in banks' exposure to concentrated markets.[7] This literature generally uses the Herfindahl-Hirschman Index (HHI) of either deposits or branches as a measure of concentration and, different from our paper, finds that when the Fed funds rate increases, local growth is relatively high in areas with increased competition and it is relatively low in high-concentration banking markets (see, e.g., Drechsler, Savov, and Schnabl (2017)). However, bank concentration (as measured, for example, by HHI) is itself an outcome of different primitive parameters such as depositors' "sticky" preferences for deposits and bank barriers to entry, which might have different implications for the transmission of monetary policy. In this sense, our main contribution to this literature is to provide a clean identification of shocks to one specific determinant of concentration—local entry barriers—, and to provide new evidence that both complements and contrasts with this literature.

[7]See Hannan and Berger (1989, 1991); Hannan (1991, 1994, 1997, 1998); Neumark and Sharpe (1992); Jacklin (1987); Driscoll and Judson (2013); Brissimis, Delis, and Iosifidi (2014); Scharfstein and Sunderam (2016); Drechsler, Savov, and Schnabl (2017); Brunnermeier and Koby (2019); Li, Loutskina, and Strahan (2019); Wang (2019); and Wang, Whited, Wu, and Xiao (2019). Other papers such as Argyle, Nadauld, and Palmer (2017), Liebersohn (2018), Bachas and Liu (2019), and Nelson (2019) also examine market power and related frictions in financial institutions.

Finally, our paper contributes to the empirical literature on deregulation and bank outcomes. A large literature has related deregulation-driven changes in local banking markets to the real economy, including entrepreneurship (Black and Strahan (2002)), innovation (Amore, Schneider, and Žaldokas (2013); Cornaggia, Mao, Tian, and Wolfe (2015); Chava, Oettl, Subramanian, and Subramanian (2013)), and corporate bankruptcies (Cornaggia, Kaviani, and Maleki (2019)). In contrast, we link deregulation to the real economy via its effect on monetary policy transmission, using novel network-based measures of deregulation intensity from Bisetti et al. (2019). The closest papers to ours in this literature are Lakdawala, Minetti, and Schaffer (2018) and Segev and Schaffer (2019), which provide evidence that the deregulation of interstate branching enhanced the bank lending channel of monetary policy transmission. We use a different identification strategy, examine different outcome variables, and pursue a different economic channel than these papers.

Our results also have significant policy implications. Since the dawn of the interstate deregulation period, the U.S. banking sector has experienced significant consolidation, dropping from over 14,000 individual bank charters in 1985 to less than 5,000 in 2017.[8] As noted above, entry rates and branch establishments have also declined, particularly after the financial crisis (Ensign, Rexrode, and Jones (2018)). Policymakers have taken note of these trends and have raised questions about the effects of consolidation on monetary policy transmission (Yellen, 2017). To the extent that such consolidation and lack of entry are in part reflective of an increase in (regulatory) entry barriers since the crisis, our results imply that increased entry barriers might have reduced the effectiveness of expansionary monetary policy over the past decade.

2 Theoretical Framework

2.1 The Bank Entry Channel

The model is static. The economy is home to an infinite finite number of prospective bankers. At the beginning of the period, each prospective banker can pay a cost κ to set up a bank and compete for local deposits. Local competition and free entry are such that only a finite number of bankers finds

[8] https://www.americanbanker.com/opinion/survival-strategy-cut-the-number-of-banks-in-half.

it profitable to pay the startup cost and open a bank. We denote by n the number of banks operating in the market, and we index banks by $i = 1, 2, \ldots, n$. We denote by D_i the deposits issued by bank i. Banks earn profits by investing in assets that yield a common risk-adjusted rate of return s. This rate of return is influenced by the aggregate interest rate exogenously set by the monetary authority f (i.e., the Fed funds rate), and hence we let $s(f)$, with $s : \mathbb{R}_+ \rightarrow \mathbb{R}_+$, $s_f \geq 0$, where s_f denotes the derivative of the return on assets with respect to the Fed funds rate.

Banks compete for local deposits in a Nash fashion, and the equilibrium rate that banks offer to depositors is a function of both the total supply of deposits and the Fed funds rate f. We let aggregate deposits and the Fed funds rate map into the deposit rate according to the function $r(f, D_1, D_2, \ldots, D_n, n)$, where $r : \mathbb{R}_+^{n+1} \times \mathbb{N} \rightarrow \mathbb{R}_+$. We assume that $r_f \geq 0$, $r_n > 0$, and that, for all i, $r_{D_i} > 0$, and $r_{D_i f} > 0$, where $r_{D_i f}$ denotes the cross-partial derivative of r with respect to bank i's deposits and f.

The assumption that $r_{D_i f} > 0$ is critical for our results. This assumption implies that the Fed funds and bank deposit are substitutes in consumers' utility function, such that bank i has to provide a higher interest rate on its deposits when the level of the Fed funds rate is high. In the Appendix, we show that $r_{D_i f} > 0$ arises naturally when consumers have non-separable preferences between liquidity services from deposits and wealth (as in Drechsler et al. (2017)).

In a Nash equilibrium, each bank i chooses the quantity of deposits that is best response to the strategies of the other banks. This deposit size is chosen to maximize bank profits, denoted by π_i:

$$[s(f) - r(f, D_1, D_2, \ldots, D_n, n)] D_i - \kappa. \tag{1}$$

In a symmetric equilibrium among banks, an individual bank's first-order condition with respect to deposits gives

$$s(f) - r(f, D^*, n^*) - r_{D_i}(f, D^*, n^*) D^* \quad = \quad 0, \tag{2}$$

where D^* and n^* respectively represent the equilibrium levels of deposits and the equilibrium number of banks, and where $r(f, D^*, n^*)$ is short-hand notation for $r(f, D_1^*, D_2^*, \ldots, D_{n^*}^*, n^*)$. Together

with the free entry condition

$$[s(f) - r(f, D^*, n^*)] D^* \quad = \quad \kappa, \tag{3}$$

the first-order condition (2) uniquely pins down the equilibrium level of deposits and the equilibrium number of banks. Substituting $r(f, D^*, n^*)$ into (3), and eliminating functional dependencies in the interest of notation, we get

$$r_{D_i} D^{*2} \quad = \quad \kappa. \tag{4}$$

Proposition 1. *The equilibrium level of deposits D^* is decreasing in the Fed funds rate.*

Proof. Totally differentiating (4) with respect to the Fed funds rate and re-arranging, we get

$$\frac{dD^*}{df} \quad = \quad -\frac{1}{2}\frac{r_{D_i f}}{r_{D_i}} D^* < 0. \tag{5}$$

□

Proposition 1 shows that, under the maintained assumption that deposits and the Fed funds rate are non-separable in the inverse supply function r, then individual banks' equilibrium deposits are a function of the aggregate interest rate f. If instead deposits and the Fed funds were separable, then individual banks' deposits would be independent of f. Intuitively, an increase in the Fed funds rate would increase banks' profits and decrease banks' incentives to enter the market, and the two effects would perfectly offset each other. Next, we obtain conditions on the sensitivity of entry (the equilibrium number of banks n^*) and aggregate lending ($n^* D^*$) to the Fed funds rate.

Proposition 2. *Suppose that $r_f \geq s_f$. Then, the equilibrium number of banks n^* and aggregate lending $n^* D^*$ are strictly decreasing in the Fed funds rate.*

Proof. Take the total derivative of (3) with respect to the fed funds rate. Then,

$$(s - r)\frac{dD^*}{df} + \left[s_f - \left(r_f + n^* r_{D_i}\frac{dD^*}{df} + r_n \frac{dn^*}{df}\right)\right] D^* \quad = \quad 0, \tag{6}$$

where r, r_f, r_{D_i}, and r_n are evaluated at (f, D^*, n^*). Solving for dn^*/df, and using (2) and (4), we get

$$\frac{dn^*}{df} = \frac{1}{r_n D^*}\left[\left(s - r - n^* r_{D_i} D^*\right)\frac{dD^*}{df} - \left(r_f - s_f\right) D^*\right] \tag{7}$$

$$= -\frac{1}{r_n}\left[\left(r_f - s_f\right) + \left(n^* - 1\right) r_{D_i}\frac{dD^*}{df}\right] \tag{8}$$

$$= -\frac{1}{r_n}\left[\left(r_f - s_f\right) + \frac{n^* - 1}{2} r_{D_i f} D^*\right] < 0. \tag{9}$$

Moreover, the sensitivity of total deposits to the Fed funds rate is

$$\frac{dn^* D^*}{df} = D^*\frac{dn^*}{df} + n^*\frac{dD^*}{df} < 0. \tag{10}$$

□

Proposition 2 shows that if the sufficient condition that sensitivity of the deposit rate to the Fed funds rate is weakly higher than the sensitivity of the lending rate, then an increase in the Fed funds rate leads to higher entry and an increase in total deposits. Intuitively, if $r_f \geq s_f$, a decrease in the Fed funds rate decreases banks' costs by more than it decreases their profits, thus encouraging entry.[9]

2.2 Entry Barriers and the Entry Channel

In this section we study how changes in entry barriers affect the bank entry channel. To obtain closed-form solutions, we adopt a linear specification for lending and deposits interest rates (see, e.g., Boyd and De Nicolo (2005)). We assume that $s(f) = \phi^s f + c$, and that $r(f, D_1, D_2, \ldots, D_n, n) = \phi^r f + \xi(f)\sum_{i=1}^{n} D_i$, where $\phi^s, \phi^r \in (0,1)$ are parameters governing the baseline sensitivity of lending and deposit rates to the Fed funds rate, $c > 0$ is a constant credit spread such that $c > (\phi^r - \phi^s) f$, and $\xi(f) > 0$ is a function governing the substitutability between deposits and the Fed funds rate. We assume that $\xi'(f) > 0$ to capture in closed-form the substitutability between Fed funds and deposits, such that banks need to provide a higher compensation per unit of deposits when the Fed funds rate is higher.

[9]In Table 4, we show that $r_f > s_f$ in our sample. Drechsler et al. (2018) argue that $r_f = s_f$ over a longer period of time.

Bank i's profits are

$$\left[c - \Delta^{\phi} f - \xi\left(f\right) \sum_{i=1}^{n} D_i\right] D_i - \kappa, \tag{11}$$

with $\Delta^{\phi} \equiv \phi^{r} - \phi^{s} \geq 0$. In a symmetric equilibrium, the first-order conditions for this problem are

$$c + \Delta^{\phi} - \xi\left(f\right) n^{*} D^{*} \quad = \quad \xi\left(f\right) D^{*}, \tag{12}$$

which solving for the optimal amount of deposits gives

$$D^{*} \quad = \quad \frac{c - \Delta^{\phi} f}{\xi\left(f\right)\left(n^{*} + 1\right)}. \tag{13}$$

Plugging (13) into (11) and ignoring integer constraints, free entry determines the number of banks operating in the economy as

$$n^{*} \quad = \quad \frac{c - \Delta^{\phi} f}{\sqrt{\kappa \xi\left(f\right)}} - 1. \tag{14}$$

Plugging (14) back into (13) we get that the equilibrium level of deposits is

$$D^{*} \quad = \quad \sqrt{\kappa / \xi\left(f\right)}, \tag{15}$$

and aggregate lending in the economy is

$$n^{*} D^{*} \quad = \quad \frac{c - \Delta^{\phi} f}{\xi\left(f\right)} - \sqrt{\frac{\kappa}{\xi\left(f\right)}}. \tag{16}$$

The next proposition illustrates the role of entry barriers for the bank entry channel of monetary policy transmission.

Proposition 3. *Low entry barriers i) increase the sensitivity of entry to MP; ii) decrease the sensitivity of individual banks' deposits to MP; and iii) increase the sensitivity of aggregate outcomes to MP.*

Proof. Starting from (14), we have that

$$\frac{\partial n^*}{\partial f} = -\frac{1}{\sqrt{\kappa \xi(f)}} \left[\Delta^{\phi} + \left(c - \Delta^{\phi} f\right) \frac{\xi'(f)}{\xi(f)} \right] < 0, \tag{17}$$

which shows that as κ decreases, the negative sensitivity of n^* to the aggregate rate increases in magnitude. Similarly,

$$\frac{\partial D^*}{\partial f} = -\frac{1}{2}\sqrt{\kappa}\frac{\xi'(f)}{\xi(f)^{3/2}} < 0, \tag{18}$$

which shows that as κ decreases, the negative sensitivity of individual banks' deposits to the Fed funds rate decreases in magnitude. Finally,

$$\frac{\partial n^* D^*}{\partial f} = \frac{\partial n^*}{\partial f} D^* + \frac{\partial D^*}{\partial f} n^*, \tag{19}$$

$$= -\left[\frac{1}{\xi(f)} \left[\Delta^{\phi} + \left(c - \Delta^{\phi} f\right) \frac{\xi'(f)}{\xi(f)} \right] - \frac{1}{2}\sqrt{\kappa}\frac{\xi'(f)}{\xi(f)^{3/2}} \right] < 0, \tag{20}$$

which shows that as κ decreases, the negative sensitivity of aggregate lending to the Fed funds rate increases in magnitude. □

We conclude this section by highlighting the importance of substitutability between Fed funds rate and deposits for our results.

Corollary 1. *Suppose that $r_{D_i f} = \xi'(f) = 0$. Then, individual banks' deposits and aggregate outcomes are not affected by changes in entry barriers.*

Proof. The result immediately follows from (18) and (20). □

3 Institutional Setting and Bank Deregulation Measures

We use the 1980s and 1990s interstate banking deregulation activity as a laboratory to test the bank entry channel proposed in the previous section. We start by describing the institutional setting around the deregulation process in Section 3.1. In Section 3.2 we describe the Bisetti et al. (2019) measures of quasi-exogenous variation in local bank entry barriers that we use to test the entry channel.

3.1 Institutional Setting

Banks in the United States historically faced restrictions on branching across state lines. Starting in the early 1970s, individual states started passing laws lifting these restrictions and allowing interstate banking (though either *de novo* branching or acquisitions). The interstate banking deregulation process evolved gradually over a span of around fifteen years, and culminated in 1994 with the passage of the Riegle-Neal Interstate Banking and Branching Efficiency Act, which took effect in 1995. Following the passage of the Riegle-Neal Act, nearly all remaining interstate banking restrictions were removed.

A characteristic feature of the deregulation process was the reciprocal nature of the interstate banking agreements. In practice, any given state had two options to allow interstate banking. The first option was for the state to pass a non-reciprocal agreement allowing banks from other states to acquire local banks. The second option was to sign national or regional reciprocal agreements allowing banks from other states to acquire local banks if and only if such states also allowed local banks to acquire banks in these other states.

In practice, the vast majority of the interstate banking deregulation happened through reciprocal agreements. For example, on December 29, 1982, the State of Massachusetts passed "An Act Relative to Branch Offices and Acquisitions of Financial Institutions" stating that "Any out-of-state banking association or corporation [...] may establish and maintain branch offices or deposits in the commonwealth, or merge with or purchase the assets of any Massachusetts bank [...]; provided that the laws of the state in which such banking association or corporation has its principal place of business expressly authorized [...] Massachusetts banks to establish and maintain branches and deposits in such state or to merge with or purchase the assets of a banking institution in such state," further specifying that "the term out-of-state banking association or corporation shall mean an association or corporation with its principal place of business in one of the states of Connecticut, Maine, New Hampshire, Rhode Island, or Vermont [...]." In other words, the Act allowed banks from Connecticut, Maine, New Hampshire, Rhode Island, and Vermont to enter Massachusetts subject to banks from Massachusetts being allowed to enter these other states.

A key feature of these agreements is that even when a state chose to deregulate, banks from other

states could not enter that state until the other states *also* passed laws allowing entry into their states. Since individual states had to wait for other states to sign reciprocal laws, the date of passage of a given state's reciprocal interstate banking law infrequently coincided with the date on which banks from that state could acquire or be acquired by banks from other states. For example, even though the State of New York passed a national reciprocal law in 1982, only five other states had signed reciprocal agreements with New York by 1987, and only twenty-one states signed similar agreements by 1992. As such, New York banks experienced a number of deregulation events (and not just one) between 1982 and the implementation of Riegle-Neal in 1995.

3.2 Bank Deregulation Measures

Following Bisetti et al. (2019), we use the passage of reciprocal and non-reciprocal interstate banking laws to construct shocks to local entry barriers and test the entry channel. In particular, we use the interstate banking deregulation dates from Amel (1993) to construct state-year level reciprocity matrices mapping US states to each other based on whether they allow interstate banking between each other. In a given year, element (m, m') of the reciprocity matrix is equal to one if banks based in state m can be acquired by banks based in state m', and it is equal to zero otherwise.

We use our reciprocity matrices to compute a measure capturing annual changes in local entry barriers. For a given year t and a given state m, our first deregulation measure is the number of states whose banks can acquire state-m banks—the column-sum of the reciprocity matrix in year t. We call this measure *States In*. In network language, *States In* is the "in-degree centrality" of the time-varying directed network arising from interstate banking agreements between US states, and captures the extent to which a state is exposed to changes in local entry barriers and competition over time.

In our tests, we complement *States In* with a second deregulation measure, which we call *Net States In*. We construct this measure as the difference between *States In* and the number of states that state-m banks can enter at any given point in time—*States Out*, the row-sum of the reciprocity matrix in year t. As described in Bisetti et al. (2019), *Net States In* captures the extent to which a state's banks face greater internal competition *net* of the investment opportunities given by increased access to out-of-state markets.

Figure 6 provides a visual illustration of how we use interstate deregulation to identify (net) shocks to in-state entry barriers, taking Colorado as focal state. In the figure, arrows indicate whether banks from a given state are allowed to enter another state through interstate banking. Prior to 1988, Colorado, Nebraska, and Massachusetts share no interstate banking connection (Panel (a)). In 1988, Colorado signs a regional reciprocal agreement with Nebraska (along with six other states), which Nebraska does not reciprocate immediately.[10] In 1991, Nebraska reciprocates the agreement, allowing Colorado banks to enter Nebraska and Nebraska banks to enter Colorado (Panel (b)). We argue that from the perspective of Colorado banks, this corresponds to a change in both in-state and out-of-state entry barriers, which does not change our *Net States In* measure.[11]

In 1991, Colorado also signs a national non-reciprocal agreement allowing banks from all other states to enter Colorado. In Panel (c) of Figure 6, we take the example of Massachusetts and show that following this non-reciprocal agreement Massachusetts banks can now enter Colorado even if Colorado banks cannot enter Massachusetts—a one-state increase in *Net States In*, corresponding to an increase in competitive pressure (due to lower in-state entry barriers) not matched by a change in Colorado banks' out-of-state investment opportunities (due to lower out-of-state entry barriers). Finally, the Riegle-Neal Act of 1994 allows Colorado banks to expand in all other US states, including Massachusetts, in 1995 (Panel (d)). From the perspective of Colorado, the Riegle-Neal Act in this example represents a one-state decrease in *Net States In*, corresponding to an increase in investment opportunities not matched by a change in local competitive pressure.

The existing literature largely measures deregulation intensity using the approach developed by Kroszner and Strahan (1999), which labels a state as deregulated based on the first date that a state implemented a deregulation agreement of any kind. We argue that our network-based (*Net*) *States In* measures offer a number of advantages over the Kroszner and Strahan (1999) approach in identifying the effects of reduced entry barriers on bank outcomes. First, our measures capture changes in dereg-

[10]Note that while the earlier deregulation literature usually codes 1988 as Colorado's deregulation year, more recent studies such as Goetz, Laeven, and Levine (2016) and Jiang, Levine, and Lin (2016) take into account the reciprocal nature of the interstate banking deregulation process.

[11]One might argue that Colorado and Nebraska might be characterized by different banking sectors at the time of deregulation, such that increased competition and increased investment opportunities might not have perfectly-offsetting effects on Colorado banks' profitability. In the appendix, we show that our main results are robust when we weight our main competition measures by the size of each state's banking sector before the start of the deregulation process.

ulation that affect the same state at different points in time and with different intensity, as the result of multiple law changes within that state. Second, given the reciprocal nature of many deregulation agreements, our measures are often functions of *other* states' deregulation decisions and legislative timing. As such, many of the changes in these deregulation measures are arguably quasi-exogenous from the perspective of local banks, particularly in early-deregulating states.

We also argue that *Net States In* enjoys three further advantages relative to *States In*. First, from an economic standpoint, *Net States In* is a measure of changes in in-state entry barriers that are not matched by changes in out-of-state entry barriers. As shown in Bisetti et al. (2019), changes in in-state and out-of-state entry barriers can have opposite effects on incumbent banks' profitability and therefore on potential entrants' incentives. *Net States In* allows us to control for both effects simultaneously.

Second, from a measurement perspective any given change in *States In* has to be matched by an identical change in *States Out* in some other state, which makes *Net States In* equal to zero on average at any given point in time. This feature of *Net States In* is particularly helpful because it allows us to avoid possible contamination from aggregate trends (such as trends in the Fed funds rate) and state-level trends when deregulation is staggered over a long period of time (Roberts and Whited (2013)). In other words, our tests rely on the different competitive pressure faced by different states at any given point in time. Third, *Net States In* is constructed as the difference between two potentially-colinear variables (*States In* and *States Out*), which reduces colinearity concerns when separately adding *States In* and *States Out* in our regressions.[12] In Figures 2 and 3 we provide a visual illustration of our arguments and of the empirical properties of *Net States In*. Bisetti et al. (2019) provide additional details regarding the construction of the *States In* and *Net States In* measures and their empirical properties.

[12] In the Appendix, we show that our results are robust to alternative specifications of *States In* and *Net States In* where we weigh the nodes of our network for the size of state-level banking market.

4 Data and Empirical Methodology

4.1 Data Sources

Interest Rates We obtain data on the target Federal Funds interest rate from the website of the Federal Reserve Bank of St. Louis.

Bank Structure and Performance Quarterly income statement and balance sheet data on U.S. regulated commercial banking entities are sourced from FFIEC Forms 031/041 (the "Call Reports") from 1984–2000. Annual data on bank branch locations comes from Christa Bouwman's website for the years 1984-1993 and from the FDIC Summary of Deposits dataset for all subsequent years.[13]

Four main variables from the Call Reports are central to our analysis. First, we respectively define a bank's *interest income rate* and *interest expense rate* as the ratios of total interest income (RIAD4107) to total loans (RCFD1400) and total interest expense (RIAD4073) to total deposits (RCFD2200). We also use the total value of a bank's total loans (RCFD1400) and small time deposits (RCON6648) as measures of bank-level quantities. The sample period is 1984-1996.

County and MSA-level Firm Structure and Employment We obtain annual MSA-level data on establishment growth, entry rates, exit rates, employment growth, job creation, and job destruction from the U.S. Census Bureau's Business Dynamics Survey, and annual county-level data on firms' employment growth and establishment growth from the Census Bureau's County Business Patterns data set. These datasets include firms of all sizes and are not limited to firms listed in Compustat. The sample period for the Business Dynamics Survey is 1977-1996 and the sample period for the County Business Patterns data set is 1986-1996.

4.2 Summary Statistics

Table 1 presents summary statistics for our main variables of interest. During our sample period of 1984-1996, the mean (median) target Fed funds rate was 6.40% (6%). Average interest income rates and interest expense rates (both in percentage annualized terms) are 10.75% and 5.3%, respectively.

[13]https://sites.google.com/a/tamu.edu/bouwman/data.

In the average MSA, the number of firms grows by approximately 1.92% per year, while the number of employees grows by around 2.6% per year. Mean and median values are very similar for nearly all of our key variables, indicating that these variables are not heavily skewed.

By construction, our primary measure of local banking market competition, *Net States In*, has an average value of zero since every increase in *States In* must be associated with a corresponding increase in *States Out*. However, Figure 2 shows that there is still a significant amount of cross-sectional variation in *Net States In*. The left side of Figure 2 shows that the range of *Net States In* values at a given point in time can be quite large, even though all states mechanically start with *Net States In* equal to zero (since no states are connected) and end with *Net States In* equal to zero (since everyone is connected). The right side of Figure 2 shows that there is significant cross-sectional dispersion in *Net States In*. This dispersion forms the basis for our empirical tests, which are described below.

In Figure 3, we display the distribution of changes in *Net States In* in the cross-section of US states before the Riegle-Neal Act. The goal of this figure is to study which US states provide the most *Net States In* variation in our sample. In Panel A, we show the number of times that Net States In changes for any given state. In this panel, lighter colors identify states that experienced relatively few changes in *Net States In* before the Riegle-Neal Act. Panel A shows that states that implemented early national non-reciprocal agreements such as Colorado, Idaho, and Texas, experienced a large number of *Net States In* changes during our sample period—mainly driven by *States Out* changes.

In Panel B of Figure 3, we show the number of *Net States In* that are on average added to a given state, conditional on a *Net States In* change. In this panel, darker colors identify states with positive changes in *Net States In* on average—states that on average experience more intense net reductions in entry barriers. Panel B shows that states with different changes in entry barriers according to our *Net States In* definition are geographically dispersed around the country, suggesting that our main results are unlikely to be driven by geographic clustering of states with different economic environments. Additional evidence supporting this assertion can be found in Bisetti, Karolyi, and Lewellen (2019).

4.3 Naïve Tests

We start our analysis by documenting a negative correlation between state-level bank entry and the target Fed funds rate during the period 1982-2017. In Table 2, we run naïve regressions of quarterly state-level establishments of new branches (from the SOD dataset) on the Fed funds rate. To account for potential time-varying trends in both branch establishments and the Fed funds rate, we run these regressions in quarterly changes, and we include a "Post-2007" indicator to account for post-crisis quantitative easing. In Column (1) we run a simple OLS regression of changes in branch establishments on changes in the Fed funds rate. In Columns (2) and (3), we progressively include time and state fixed effects to account for common variation in branch establishment growth in different quarters and different states.

Table 2 documents a negative correlation between branch establishment and the Fed funds rate. When the Fed funds rate increases by one percentage point (e.g., going from 4% to 5%) in our preferred specification (3), branch establishments growth *decreases* by around 0.62 branches per state-quarter, corresponding to around 8.9% of the unconditional standard deviation of branch establishment growth in our panel. Interestingly, this effect completely reverses after the financial crisis. Our estimates indicate that since 2008, a one percentage point increase in the Fed funds rate is associated with a net 1.1 branches *increase* per state-quarter, suggesting different implications for our proposed bank entry channel in the post-crisis deregulation period.

Of course, the results presented in Table 2 are poised by problems of endogeneity and possible reverse causality between high interest rates and local bank entry. In the following section, we detail how we exploit interstate deregulation and the measures described in Section 3.2 to test the entry channel of monetary policy transmission.

4.4 Empirical Methodology

We use our *States In* and *Net States In* measures to identify the bank entry channel of monetary policy and its real effects. To do so, we pair state-level changes in *Net States In* with data on the effective Fed funds rate and a number of bank-level and county- or MSA-level outcome variables. This allows us to compare, at each point in time, how monetary policy affects bank entry, incumbent banks' responses,

and real outcomes in states characterized by different entry barriers.

We start the analysis by validating our deregulation measures as proxies for bank entry. Using the example of *States In* as an independent variable, we start by running state-quarter tests of the form

$$Entry_{st} = \beta_0 + \beta_1 States\ In_{st} + \beta_2 States\ In_{st} \times Fed\ Funds\ Rate_t + \gamma_s + \gamma_t + \varepsilon_{st}\ , \tag{21}$$

where s denotes states and t denotes a year-quarter. The main coefficient of interest is the interaction term β_2. For a specific increase or decrease in the Fed funds rate, this coefficient measures differences in entry in states whose local entry barriers are decreasing *relative* to states whose entry barriers remain unchanged. The Fed Funds rate does not appear as an independent variable above because it is invariant across banks at a given point in time and is hence absorbed by the inclusion of time fixed effects. However, we report the baseline results from a regression of the relevant dependent variable on the Fed funds rate in all our tests.

Our main bank-level tests take the form:

$$Bank\ Outcome_{ist} = \beta_0 + \beta_1 States\ In_{st} + \beta_2 States\ In_{st} \times Fed\ Funds\ Rate_t + \gamma_i + \gamma_s + \gamma_t + \varepsilon_{ist}\ , \tag{22}$$

where i denotes banks, and s and t again denote state and year-quarter.

Finally, for tests examining real effects, our regression specifications take the form:

$$Real\ Outcome_{gst} = \beta_0 + \beta_1 States\ In_{st} + \beta_2 States\ In_{st} \times Fed\ Funds\ Rate_t + \gamma_g + \gamma_s + \gamma_t + \varepsilon_{gst}\ , \tag{23}$$

where g denotes a geographic area such as a county or MSA and all other variable definitions remain unchanged. The coefficient of interest is again β_2, which here captures the change in the outcome variable at a given point in time, given a specific shock to the Funds rate, in areas with lower entry barriers versus changes in the same outcome at the same point in time given the same Funds rate shock for areas with no changes in entry barriers. Again, the main effect of the $Fed\ Funds\ rate_t$ variable is absorbed by the inclusion of time fixed effects.

4.5 Identification

Our main goal is to identify the β_2 coefficients in Equations (21)-(23), which pin down the marginal effect of a change in the Fed funds rate in areas that are subject to different entry barriers. The baseline assumption that we require for the correct identification of these coefficients is that the deregulation process does not anticipate changes in monetary policy and their pass-through effects. In other words, we need to exclude the hypothesis that individual states selectively deregulate by anticipating the effect of monetary policy on bank-level and real outcomes in other states. This baseline identification assumption is anecdotally supported by the fact that the debate about the effects of bank regulation on monetary policy transmission has only emerged after the post-crisis regulatory wave, and that, as we write, the debate is still ongoing in regulatory circles worldwide (see, e.g., BIS (2015), Yellen (2017)).

In our paper, violation of the identification assumption for β_2 requires even stronger conditions than the deregulation process and monetary policy being uncorrelated. Our identification assumption is violated if the *structure* of the interstate banking deregulation network is correlated with monetary policy. In Figure 6, we test this assumption by providing a scatter plot of year-on-year changes in the Fed funds rate on year-on-year changes in (*Net*) *States In*. The data cannot reject the null hypothesis of no correlation between changes in the deregulation network and changes in the Fed funds rate, providing support for our identifying assumption.

A secondary goal of the paper is to identify the coefficients β_1 in Equations (21)-(23), which capture the baseline effect of a reduction in entry barriers on the dependent variable. One potential concern with this strategy is that an omitted variable may exist that drives both a state's decision to deregulate and the outcome variables of interest, in which case our results would not cleanly capture shocks to the competitive environment of banks. In Bisetti et al. (2019), we present a number of placebo tests, parallel trends tests, and other tests suggesting that the *Net States In* variable does not simply capture a correlated omitted variable. In addition, our decision to only include reciprocal agreements in our *States In* and *States Out* measures after *both* parties have signed such agreements help to mitigate concerns about state-level political or strategic motives for deregulation (Kroszner and Strahan, 1999). We refer interested readers to Bisetti et al. (2019) for further evidence regarding

the suitability of the *Net States In* variable as a source of plausibly exogenous variation in local entry barriers and banking market competition.

5 Results

5.1 Bank Entry

In Table 3 we confirm that our *States In* and *Net States In* deregulation variables are good proxies for state-level changes in barriers to entry by showing a positive correlation between these variables and bank entry. More important, we show that the baseline negative correlation between the Fed funds rate and bank entry is stronger (i.e., more negative) for larger values of *States In* and *Net States In*. In other words, bank entry is more sensitive to changes in the Fed funds rate when entry barriers are low. This result is consistent with the predictions of our theoretical model.

We use two empirical measures of bank entry. First, motivated by the nature of the interstate banking deregulation (where banks could initially enter other states only by acquiring banks in these other states and turning them into branches), we use local bank acquisitions by out-of-state Bank Holding Companies (BHCs) as measures of entry into local banking markets. Second, as in Table 2, we use new branch establishments as a proxy for entry.

In Column (1) of Table 3, we show that when the target Fed funds rate increases by 1% (e.g., going from 4% to 5%), bank acquisitions decrease by 0.01 per state-quarter. This decrease is quantitatively large, accounting for around 25% of the average number of 0.4 acquisitions per state-quarter in our sample, and confirms the negative correlation between monetary policy and bank entry documented in the previous section. In Columns (2) and (3), we document two important results. First, for a zero level of the Fed funds rate, a 50-state increase in *States In* and *Net States In* are respectively associated with increases of 0.19/2 = 0.095 and 0.065 in the number of acquisitions at the state-quarter level—around 25% and 33% of the sample mean.[14] For an average level of the Fed funds rate of 6.4%, the total effect of a 50-state increase in (*Net*) *States In* is equal to 0.05 (0.07) acquisitions a year, around 12.5% (17.5%) of the sample mean.

[14]For exposition purposes, *States In* and *Net States In* are normalized by 100 in all our tables.

The second important result in Columns (2) and (3) is that bank entry becomes more sensitive to monetary policy when entry barriers are lower (i.e., when *States In* and *Net State In* are higher). For example, the coefficient β_2 in Column (2) reveals that a one-state increase in *States In* increases the negative sensitivity of bank acquisitions to a 1% change in the Fed funds rate by 0.00013, around 0.15% of the baseline effect documented in Column (1). Alternatively, a 50-state increase in *States In* increases the sensitivity of bank acquisitions by 0.065, around 72% of the baseline effect from Column (1). Similarly, the last row of Column (3) reveals that a 50-state increase in *Net States In* leads to an incremental bank acquisition sensitivity to the Fed funds rate that is more than twice as large as the baseline sensitivity from Column (1).

In Columns (4)–(6), we repeat the same exercise using new branch establishments as a dependent variable. Column (4) reveals that when the Fed funds rate increases by 1%, new branch establishments decrease by 0.187 per state-quarter, or 2% of the average number of branch establishments at the state-quarter level. Columns (4) and (6) reveal that the total effect of a 50-state increase in (*Net*) *States In* is equal to 0.82 (0.90) acquisitions per state-quarter, around 8.2% (9%) of the sample mean of 10 acquisitions per state-quarter in our sample. As in the case of bank acquisitions, the last two columns of the table also confirm that the negative relationship between the Fed funds rate and bank entry becomes stronger when banks face lower entry barriers. For example, the interaction term β_2 in the last row of Column (6) reveals that a 50-state increase in *Net States In* leads to an incremental branch establishment sensitivity to the Fed funds rate six times as large as the baseline sensitivity from Column (4).

In the appendix, we provide extensive robustness tests on our entry results. First, we show that our results hold when we replace the end-of-quarter target Fed funds rate with the quarterly average target Fed funds rate. Second, we show that our results disappear when we use the Kroszner and Strahan (1999) interstate deregulation measure to identify changes to local entry barriers. In turn, this suggests that the empirical properties of *States In* and *Net States In* described in Section 3.2 allow us to pick up variation in entry barriers that is not picked up by traditional measures. Third, we show that our results are robust if we isolate the asymmetric effects of deregulation (i.e. "one-way streets" whereby banks in a state gain access to another state's market in a non-reciprocal fashion), and if we

only focus on states that sign national agreements (reducing the likelihood that economic conditions in specific state-pairs drove deregulation decisions). Finally, we show that our results also hold if we account for possible asymmetries in the structure of the US interstate banking network by weighting our deregulation measures by the total banking assets in a given state.

Overall, the results of Table 3 use the quasi-exogenous variation in barriers embodied in *States In* and *Net States In* to confirm our theoretical model's prediction of a *negative* relationship between monetary policy and bank entry.

5.2 Bank Lending Behavior

In this section, we examine the effect of the entry channel on bank prices and quantities. In Table 4, we start by providing evidence supporting the assumption underlying the entry channel—that interest expense rates are more sensitive to monetary policy than interest income rates—, and go ahead to show that the entry channel increases banks' interest expense sensitivity to monetary policy. In Table 5, we investigate the impact of the entry channel on bank lending and deposit-taking.

In the first row of Table 4, we provide baseline estimates of the conditional correlation between the Fed funds rate and bank-level interest income rates (interest income divided by total loans), interest expense rates (interest expense divided by total deposits), and the difference between interest income rates and interest expense rates. Consistent with the predictions of our theoretical model, we show that a 1% increase in the Fed funds rate leads to a 0.45% increase in the interest income rate and a 0.54% increase in the interest expense rate, and that this difference is statistically significant.

In the remaining columns of the table, we show the effect of our baseline deregulation measures and the entry channel on bank prices. Columns (2) and (3) show that the data provides no evidence of a (*Net*) *States In* effect on bank interest income rates, nor evidence of an interaction effect between interest income rates, shocks to entry barriers, and monetary policy. In Columns (5) and (6), we document two sets of results. First, we document an overall *positive* relationship between our deregulation measures and the interest expense rate. At an average level of the Fed funds rate of 6.4%, a 50-state increase in *States In* is associated with an increase in interest expense rates of 0.1%, or 2% of the sample mean. The results for *Net States In* are quantitatively similar. Finally, our results reveal that lower

barriers to entry *increase* the sensitivity of the interest expense rate to monetary policy. For example, the interaction term β_2 in the third row of Column (5) reveals that a 50-state increase in *Net States In* leads to an incremental branch establishment sensitivity to the Fed funds rate which is around 10% of the baseline sensitivity from Column (4).

Overall, the results from Table 4 confirm our theoretical assumptions that the interest income rate sensitivity to the Fed funds rate is lower than the interest expense rate sensitivity to the Fed funds rate, and confirm the results in Bisetti et al. (2019) that the deregulation process of the 1980s and 1990s decreased banks' ability to extract rents in local deposit markets. Moreover, our results suggest that lower entry barriers and increased competition increase the sensitivity of bank deposit rates to the Fed funds rate.[15]

Next, in Table 5 we study the impact of the bank entry channel on bank lending and deposit-taking. In Column (1), we start by documenting a negative baseline correlation between bank-level lending and the Fed funds rate, while in Column (4) we document a similar negative correlation between deposit-taking and the Fed funds rate. Consistent with the prediction that an increase in the Fed funds rate increases banks' funding costs and reduces banks' lending capacity, we show that a 1% increase in the Fed funds rate leads to a 7.5% reduction in bank total loans and a 5.9% reduction in time deposits in our sample.

In the remaining columns of the table, we show that the baseline negative correlation between bank quantities and monetary policy is mitigated when entry barriers to local banking markets are low. In Columns (2)–(3) and (5)–(6), we show that the interaction coefficient β_2 between the Fed funds rate and our deregulation measure is positive, implying that changes in the Fed funds rate have a *lower* negative impact on bank lending and deposit-taking in states that are more exposed to entry. For example, the last row of Column (3) show that 50-state increase in *Net States In* leads to a 3.4% increase in lending for a 1% increase in the Fed funds rate, or 45% of the baseline effect from Column (1). Similarly, the last row of Column (6) shows that a 50-state increase in *Net States In* leads to a 2.4% increase in time-deposits for a 1% increase in the Fed funds rate, or 40% of the baseline

[15]Note that our theoretical model by construction does not deliver results on the sign of the interaction term β_2 in this table. However, our empirical results suggest that interest expense rates are more sensitive to monetary policy when there is more entry—or, in the language of our model, that $r_{f,n} > 0$.

effect in Column (1).[16]

Our lending and deposit-taking results are again consistent with our theoretical predictions. On the one hand, a decrease in the Fed funds rate decreases banks' funding costs, therefore increasing banks' deposit-taking and lending capacity. On the other hand, a decrease in the Fed funds rate also stimulates bank entry, which mitigates individual banks' ability to expand their balance sheets. Taken together, the results presented in this section document a strong relationship between the bank entry channel and bank-level outcomes. In the next section, we turn to the real effects of our the entry channel.

5.3 Real Effects of the Bank Entry Channel

5.3.1 Main Effects

How do the bank-level effects documented in the previous sections translate into real outcomes? To answer this question, we combine data on monetary policy shocks and data on bank deregulation dates with annual MSA-level data from the U.S. Census Bureau's Business Dynamics Survey and annual county-level data from the Census Bureau's County Business Patterns data set.

Tables 6–10 present the results of these tests. In Table 6, we examine the effects of the entry channel on six MSA-level outcome variables: the growth rate of the number of firms in a given area, establishment entry and exit rates, total employment growth rates in a given area, and job creation and destruction rates. In the interest of space, we only use *Net States In* as a measure of deregulation-induced changes in entry barriers, and leave *States In* specifications to the appendix. All specifications include year and state fixed effects, and standard errors in all specifications are clustered at the state level.[17]

Table 6 presents strong evidence that bank entry has significant effects on the transmission of monetary policy to the real economy. The MSA-level results in Panel A show that the total growth rate in the number of firms and the rate of establishment entry are more strongly inversely related to

[16] We obtain similar but statistically weaker results for total deposits, suggesting that time deposits are more affected by our proposed entry channel than other deposit types.

[17] Since our level of treatment is at the state-year level, we do not include state $\times$ year fixed effects, as this would identify outcomes based on variation in within-state exposure to deregulation intensity rather than capturing the average treatment effect of changing entry restrictions.

changes in the Fed funds rate when local banking markets are subject to lower entry barriers. The economic magnitudes of these effects are significant: for example, our estimates suggest that a 50-state increase in *Net States In* is associated with a *reduction* in establishment entry rate of 0.236 per MSA-year for a 1% increase in the Fed funds rate, a magnitude three times as large as the baseline effect from Column (3). Finally, the last two columns of Panel A show that establishment exit is more strongly positively correlated to changes in the Fed funds rate when local banking markets become more competitive.

Similar patterns emerge for MSA-level employment. The results in Panel B of Table 6 suggest that employment growth and job creation rates are more strongly negatively correlated with the Fed funds rate in MSAs where banks face lower entry barriers (relative to unaffected areas), and that job destruction rates are more strongly positively correlated with the Fed funds rate when they in MSAs where banks face lower entry barriers. Economic magnitudes are again significant; for example, a 50-state increase in *Net States In* is associated with a reduction in establishment entry rate of 0.364 per MSA-year for a 1% increase in the Fed funds rate, a magnitude twice as large as the baseline effect from Column (3).

The results of Table 6 suggest a stronger correlation between monetary policy and real outcomes when local banking markets are faced with lower entry barriers. In Table 7, we further investigate whether this correlation comes from periods of expansionary monetary policy (which we define as negative year-on-year changes in the Fed funds rate) or from contractionary monetary policy (which we define as positive year-on-year changes in the Fed funds rate). More specifically, we interact *Net States In* separately with positive changes in the Fed funds rate and the absolute value of negative changes in the Fed funds rate. Using the absolute value of decreases in the Fed funds rate allows for a more intuitive interpretation of our results: a positive coefficient estimate on $Net\ States\ In \times Abs(\%\Delta^{-}FF)$ means that the y variable is larger in areas with low entry barriers when the Fed lowers rates.

Table 7 shows that the baseline effects documented in Table 6 are mainly concentrated in periods of expansionary monetary policy. For example, the first two columns of Panels A and B suggest that when the Fed lowers rates, firm growth and employment growth are stronger in MSAs with lower

entry barriers. On the other hand, the results for contractionary monetary policy are economically smaller and not statistically different from zero, suggesting that an increase in the Fed funds rate does not deter bank entry in the same way as a decrease in the Fed funds rate encourages bank entry.

In the appendix, we complement the analysis of this section with a battery of robustness test. As in the case of Table 3, we show that our results hold when we replace the end-of-quarter target Fed funds rate with the quarterly average target Fed funds rate, when we isolate the asymmetric effects of deregulation from "one-way" connections between states, when we only focus on states that sign national agreements, and when we weigh our deregulation measures by the total banking assets in a given state. Finally, we also show that our real effects' results disappear when we use the Kroszner and Strahan (1999) interstate deregulation measure to identify changes to local entry barriers.

Collectively, the results of Tables 6 and Table 7 suggest that the entry channel can have large real effects, and that the interaction between monetary policy and entry is particularly strong in instances when the Fed decreases interest rates—thus making bank borrowing cheaper. In turn, these results have important policy implications for the effectiveness of monetary policy in the post-crisis expansionary environment. As noted in the introduction, bank entry in the United States has been dropped since the crisis, despite bank funding costs touching historical lows. The results of this section suggest that the current monetary policy stance could provide even more effective stimulus to local economies if accompanied by a reduction in the barriers to entry currently facing banks.

5.3.2 Cross-Sectional Evidence

In this section, we exploit cross-sectional geographic variation in exposure to the entry channel to ask which types of geographic areas and which types of establishments are more exposed to the entry channel.

To run our cross-sectional tests, we turn to the County Business Patterns (CBP) dataset. This dataset covers a shorter period of time than the BDS data (i.e., 1986-1996) and does not have information on firm and job creation and destruction. However, this panel provides information on the number of establishments (by employee size) and on the number of employees operating in a given sector in a given county. Therefore, our county-level analysis allows us not only to confirm the re-

sults from the previous section using a completely different dataset, but also to provide additional evidence in support of our mechanism using a richer cross-section of data.

In Table 8, we start by examining the impact of the bank entry channel on *county-level* establishment and employment growth rates. Because counties can be classified by the Census Bureau as being part of a metropolitan area or not, this test allows us to examine real effects on all areas (not just MSAs) and to separately identify any differences between urban ("'metro") areas and rural areas. Table 8 shows that while the average effects documented in the previous section hold across the entire country (and not just MSAs), our results are largely concentrated in metro areas. In particular, for a given increase in the Fed funds rate, the effects of increased local banking market competition on the transmission of monetary policy are amplified more in urban areas (i.e., employment growth and establishment growth are more negative given the same change in our state-level deregulation measure), thus supporting the results from the previous section.

Table 9 further exploits county-level data to examine heterogeneity in the growth rate of establishments across different establishment sizes (in terms of the number of employees per establishment). We separate establishments into four categories based on their number of employees: establishments with 1-19 employees, establishments with 20-99 employees, establishments with 100-999 employees, and establishments with more than 1,000 employees (in the county). Given the results in the previous table, this test is limited to establishments located in metropolitan areas.

Table 9 shows that the entry channel is stronger for small and medium-sized establishments within a given metro area. A one-state increase in *Net States In*, given an increase in the Fed funds rate, has stronger effects on establishment growth rates among the set of establishments with fewer than 100 employees. Establishments with more than 100 employees do not experience any significant effects due to increased banking market competition, suggesting that these larger establishments may be less bank-dependent and/or may be less dependent on local banks for their funding needs. Overall, the results of Table 9 suggest that small businesses benefit the most from expansionary monetary policy through the bank entry channel.

Finally, Table 10 examines how the transmission of monetary policy through the entry channel depends on cross-sectional differences in local banking sector characteristics during the deregulation

period. We interact our main effect ((*Net*) *States In* $\times$ *Fed funds rate*) with an indicator equal to one if county-level average bank leverage is above the cross-sectional county-level median in a given year, and equal to zero otherwise, and study the effect of the entry channel on new branch establishments, employment growth, and establishment growth at the county-level.

Table 10 shows that when a state's barriers to entry decrease (i.e. when (*Net*) *States In* increases), entry, employment growth, and establishment growth are more concentrated in counties where local banks are more levered, suggesting financial constraints as a reason why incumbent banks might find it difficult to expand even when their financing costs decrease.[18]

6 Conclusions

While a large and growing literature argues that market power in commercial banking affects the transmission of monetary policy to the real economy, the existing empirical literature generally takes market power as given and does not differentiate between the primitive economic forces that are responsible for the existence of market power. In this paper, we try to ascertain what types of underlying economic frictions are actually responsible for the monetary policy results commonly ascribed to banks' market power.

We focus exclusively on one such friction: namely, bank entry restrictions. We argue that increased competition due to reduced bank entry restrictions should asymmetrically affect monetary policy pass-through: lending should be higher in more competitive areas in both expansionary and contractionary monetary policy regimes. Using U.S. interstate banking deregulation as a laboratory, we develop novel measures of deregulation intensity that allow us to identify the effects of reduced entry restrictions on the pass-through of monetary policy to the real economy. Consistent with our arguments, we find that reduced entry restrictions are associated with asymmetric real effects: establishment growth and employment growth decrease by less when the Fed raises rates, and increase by more when the Fed lowers rates, in areas with reduced entry restrictions.

Our results have significant policy implications. Since the interstate deregulation period, the U.S.

[18] Note that our model does not need financial constraints to generate the entry channel predictions that we test in the data. Here, we are just suggesting financial constraints as a possible interaction with the entry channel.

banking sector has experienced significant consolidation. Entry rates have also declined, particularly after the recent financial crisis. Policymakers have taken note of these trends and have recently raised questions about the effects of consolidation on monetary policy transmission (Yellen, 2017). The monetary policy channel we document in this paper implies that reduced competition may be hindering monetary policy pass-through, and that pass-through can increase with more policies that stimulate bank entry.

Finally, while our focus has been on the relationship between bank entry and monetary policy transmission, we also suspect that other primitive drivers of bank market power may produce different predictions about monetary policy pass-through than the channel documented in this paper. We look forward to learning more about these predictions in future research.

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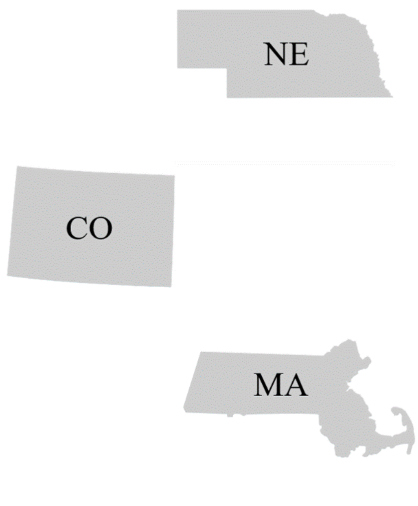
Figure 1

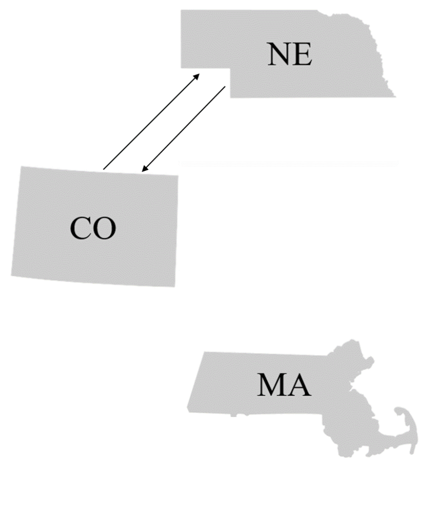
Identification Example: Colorado, Nebraska, and Massachusetts

This figure provides a visual illustration of the deregulation process that we use to identify net shocks to entry barriers (*Net States In*), taking Colorado as focal state. Arrows indicate whether banks from a given state are allowed to enter another state through interstate banking.



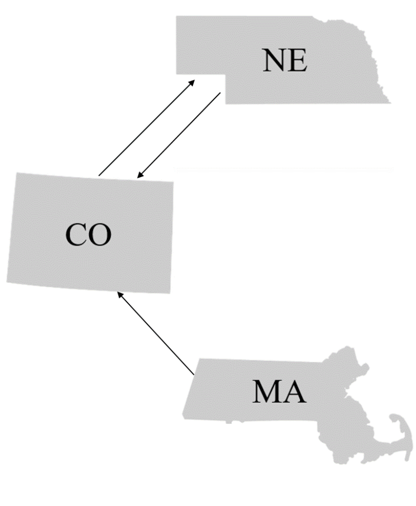
(a)

CO isolated until 1991



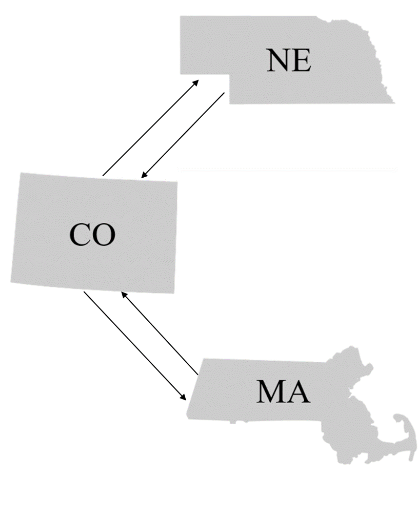
(b)

CO-NE reciprocal agreement (1991)



(c)

CO non-reciprocal agreement (1991)



(d)

Riegle-Neal Act (1995)

Figure 2

Net States In: Time Series and Cross-Section

The left panel of this figure shows the time series evolution of the two states with the highest (top *Net States In*) and lowest (bottom *Net States In*) values of *Net States In* every year, as well as average *Net States In* across states (the dashed flat line) during the period 1978-2000. The identity of the top and bottom *Net States In* can vary from year to year as different states sign interstate agreements. The right panel shows the distribution of *Net States In* from the start of our sample (1984) to the year after the passage of the Riegle-Neal Act (1995).

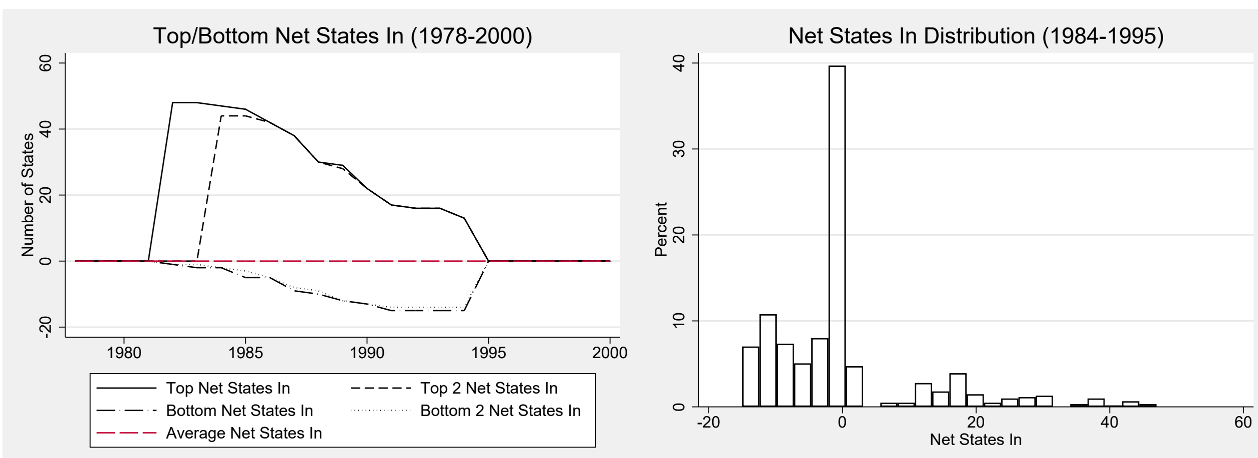
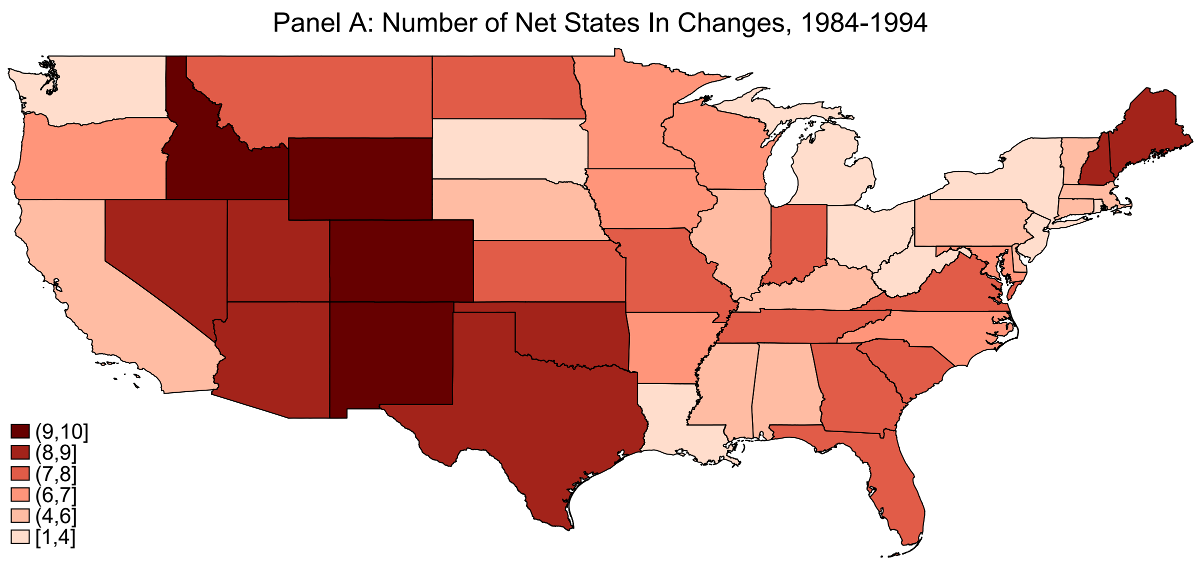


Figure 3

The Cross-Section of Net States In Changes

In this figure, we study the distribution of changes in *Net States In* in the cross-section of US states before the Riegle-Neal Act. In Panel A, we show the number of times that *Net States In* changes for any given state. In this panel, lighter colors identify states that experienced relatively few changes in *Net States In* before the Riegle-Neal Act. In Panel B, we show the number of *Net States In* that are on average added to a given state, conditional on a *Net States In* change. In this panel, darker colors identify states with positive changes in *Net States In* on average—states that on average experience more reductions in inward entry barriers.



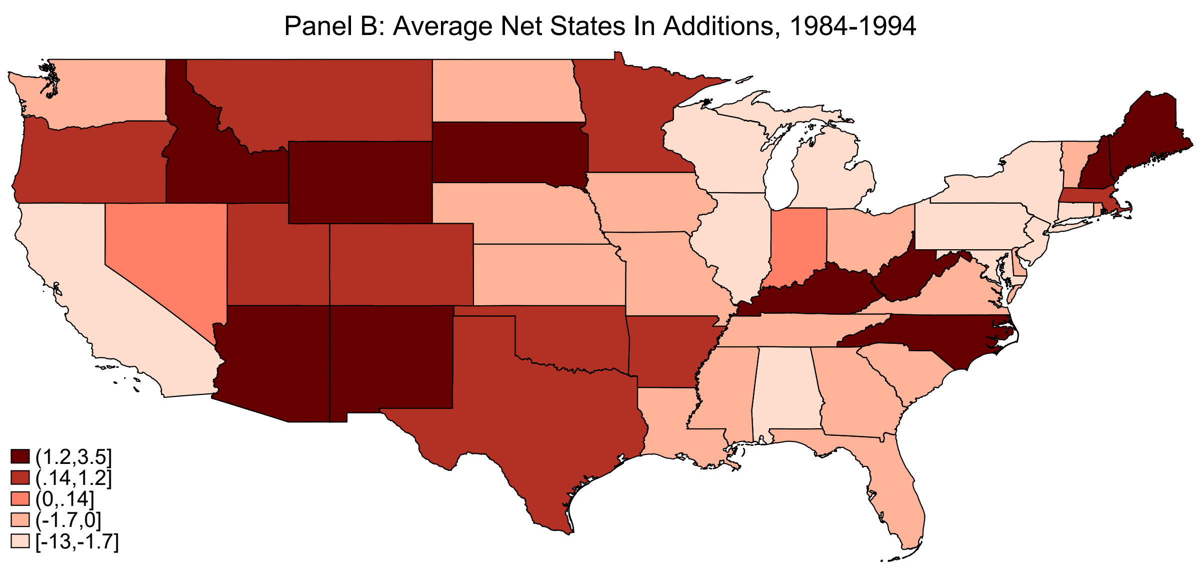


Figure 4

Identification

In this figure, we provide support for the identification assumption that the structure of our deregulation network (pinned down by changes in *States In* and *Net States In*) is not correlated with monetary policy (pinned down by changes in the Fed funds rate).

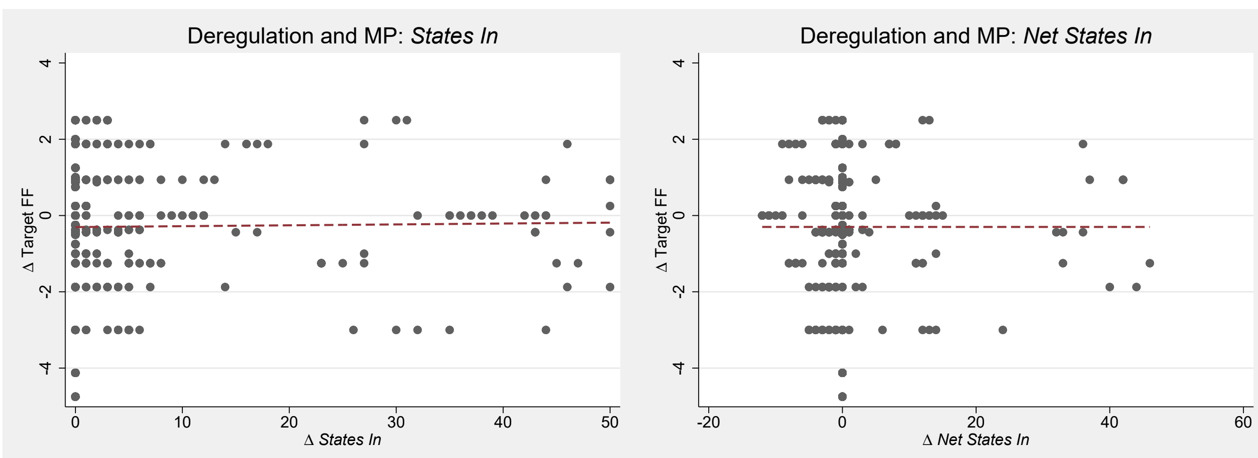


Table 1

Summary Statistics

This table describes the main variables in our paper. The Fed Funds Rate is the end-of-quarter target Fed Funds Rate, available on the Federal Reserve of St. Louis' website. *States In* and *Net States In*, our main measures of shocks to entry barriers, are constructed as described in Section 3.2. Quarterly bank-level data on interest income rates (interest on loans divided by total loans), interest expense rates (interest expense divided by deposits), lending, and time deposits for the period 1984-1996 come from the call reports. Annual MSA-level data on firm growth (log growth in the total number of firms within an MSA), establishment entry rates (the number of new establishments between year t and year $t-1$, divided by the total number of establishments in year $t-1$), establishment exit rates (the number of disappearing establishments between years t and $t-1$, divided by the total number of establishments in year $t-1$), employment growth (log growth in the total number of employees within an MSA), job creation rates (employment gains from expanding establishments, including startups, between year $t-1$ and year t, divided by total employment in year $t-1$), job destruction rates (employment losses from contracting establishments between year $t-1$ and year t, divided by total employment in year $t-1$) for the 1977-1996 period come from the Business Dynamics Survey dataset. Annual county-level data on employment growth and establishment growth for the years 1986-1996 come from the County Business Patterns dataset. All the data is publicly-available.

	Mean	SD	p25	p50	p75	Observations
Target Fed Funds Rate	6.40	2.15	5.25	6.00	8.06	52
States In	24.06	20.81	3.00	16.00	49.00	663
Net States In	0.00	10.96	-7.00	-1.00	0.00	663
Interest Income Rate	10.75	1.75	9.53	10.70	11.82	657,737
Interest Expense Rate	5.30	1.65	4.00	5.41	6.37	661,728
log(Loans)	10.28	1.41	9.35	10.13	11.02	694,870
log(Time Deposits)	9.67	1.22	8.89	9.60	10.34	653,880
MSA Firm Growth	1.92	2.92	0.46	1.82	3.24	6,954
MSA Establishment Entry Rate	13.62	3.08	11.40	13.10	15.20	7,320
MSA Establishment Exit Rate	11.12	2.18	9.60	10.80	12.30	7,320
MSA Employment Growth	2.59	4.99	-0.17	2.64	5.36	6,954
MSA Job Creation Rate	18.02	4.49	14.90	17.30	20.50	7,320
MSA Job Destruction Rate	5.51	1.86	4.20	5.20	6.50	7,318
County Employment Growth	2.59	8.19	-1.14	2.64	6.31	31,263
County Establishment Growth	1.39	4.53	-0.76	1.34	3.53	31,388

Table 2

Naïve Branch Entry Regressions

In this table, we document a negative correlation between branch establishments and monetary policy by regressing quarterly changes in the total number of new branch establishments at the state-level on quarterly changes in end-of-quarter target Fed Funds Rate. To account for post-crisis quantitative easing, our regressions include a "Post-2007" indicator equal to one for all years after 2007. The sample period is 1982q1-2017q4.

	Δ Branch Establishment		
	(1)	(2)	(3)
Δ FF	-0.110	-0.623**	-0.623**
	(0.11)	(0.28)	(0.28)
Post-2007	-0.575***		
	(0.09)		
Post-2007 × Δ FF	0.924*	1.696*	1.696*
	(0.47)	(0.90)	(0.90)
State FE	No	No	Yes
Year FE	No	Yes	Yes
R-Squared	0.003	0.008	0.008
Observations	6,936	6,936	6,936

Note: Standard errors (in parentheses) are clustered at the state level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table 3

Deregulation, Monetary Policy, and Bank Entry

In this table, we validate our deregulation variables as instruments for entry, and we show that bank entry is more sensitive to the Fed funds rate when entry barriers are low (i.e., when *States In* and *Net States In* are high). In the first three columns of the table, we regress total bank acquisitions by out-of-state BHCs on the target Fed Funds Rate at the end of the quarter, our *States In* and *Net States In* deregulation measures, and an interaction term capturing the incremental effect of changes in monetary policy when states are characterized by different barriers to entry. In Columns (4)-(6), we repeat the same exercise using new branch establishments as dependent variable. *States In* and *Net States In* are normalized by 100. The sample period is 1984q1-1996q4.

	Bank Acquisitions			New Branches		
	(1)	(2)	(3)	(4)	(5)	(6)
FF	-0.009***			-0.187**		
	(0.00)			(0.08)		
States In		0.190**			8.115	
		(0.08)			(4.93)	
States In × FF		-0.013*			-1.012	
		(0.01)			(0.61)	
Net States In			0.269**			17.375**
			(0.12)			(7.21)
Net States In × FF			-0.020*			-2.433***
			(0.01)			(0.89)
Year-Quarter FE	No	Yes	Yes	No	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.052	0.164	0.163	0.649	0.735	0.736
Observations	2,652	2,652	2,652	2,652	2,652	2,652

Note: Standard errors (in parentheses) are clustered at the state level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table 4

Lending Rates and Deposit Rates

In this table we validate our theoretical assumption that banks' interest expense rates are more sensitive to monetary policy than interest income rates, and we study the interaction between monetary policy, bank entry, and bank pricing of loans and deposits. In the first three columns of the table, we regress bank-level interest income rate (interest on loans to total loans) on the end-of-quarter Fed funds Rate, our *States In* and *Net States In* deregulation measures, and on the interaction between the Fed funds rate and our deregulation measures. In Columns (3)-(6), we repeat the same exercise using bank interest expense rate (interest on deposits to total deposits) as the dependent variable. In the last three columns, we use the difference between interest income rate and interest expense rate as the dependent variable. *States In* and *Net States In* are normalized by 100. The sample period is 1984q1-1996q4.

	Interest Income Rate (a)			Interest Expense Rate (b)			(a) – (b)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
FF	0.453***			0.539***			-0.083***		
	(0.01)			(0.01)			(0.01)		
States In		0.108			-0.525*			0.553*	
		(0.25)			(0.30)			(0.31)	
States In × FF		-0.004			0.115*			-0.108**	
		(0.04)			(0.06)			(0.05)	
Net States In			-0.245			-0.758**			0.421
			(0.35)			(0.32)			(0.34)
Net States In × FF			0.036			0.156**			-0.107*
			(0.05)			(0.08)			(0.06)
Year-Quarter FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.572	0.713	0.713	0.720	0.883	0.883	0.592	0.603	0.603
Observations	657,549	657,549	657,549	661,543	661,543	661,543	655,171	655,171	655,171

Note: Standard errors (in parentheses) are clustered at the state level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table 5

Bank Lending and Time Deposits

In this table, we study the interaction between monetary policy, entry barriers, and bank deposit-taking and lending activities. Specifically, we show that the bank entry channel (pinned down by the interaction term between (*Net*) *States In* and the Fed funds rate) reduces the baseline negative correlation between the Fed funds rate and bank-level deposit-taking and lending, as predicted by our theoretical model. The specifications are identical to those in Table 4, but we replace the left-hand side variables with total loans and time deposits (as defined in Table 1). *States In* and *Net States In* are normalized by 100. The sample period is 1984q1-1996q4.

	log(Loans)			log(Time Deposits)		
	(1)	(2)	(3)	(4)	(5)	(6)
FF	-0.075***			-0.059***		
	(0.01)			(0.00)		
States In		-0.330*			-0.060	
		(0.18)			(0.15)	
States In × FF		0.032			0.031*	
		(0.02)			(0.02)	
Net States In			-0.635***			-0.086
			(0.20)			(0.23)
Net States In × FF			0.068***			0.049**
			(0.02)			(0.02)
Year-Quarter FE	No	Yes	Yes	No	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.915	0.934	0.934	0.899	0.922	0.922
Observations	694,654	694,654	694,654	653,680	653,680	653,680

Note: Standard errors (in parentheses) are clustered at the state level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table 6

Real Effects

In this table, we use the Business Dynamics Survey dataset to study the real effects of the entry channel. In Panel A, we regress MSA-level firm growth, establishment entry rates, and establishment exit rates on the interaction between *Net States In* and the target Fed Funds Rate at the end of the year. In Panel B, we repeat the same exercise using MSA-level employment growth and job destruction rates as dependent variables. All the dependent variables (expressed in percentage points) are defined as in Table 1. As in Table 5, Specifications (1) and (4) of each panel include the lagged value of the target Fed Funds Rate, which we omit in the interest of space. *States In* and *Net States In* are normalized by 100. The sample frequency is annual, and the sample period is 1977-1996.

Panel A: Firms and Establishments

	Firm Growth		Estab. Entry Rate		Estab. Exit Rate	
	(1)	(2)	(3)	(4)	(5)	(6)
FF	-0.140**		0.089***		0.250***	
	(0.06)		(0.03)		(0.03)	
Net States In		8.325***		2.202		-5.584***
		(2.81)		(1.82)		(1.45)
Net States In × FF		-1.534***		-0.472*		0.994***
		(0.43)		(0.28)		(0.22)
Year FE	No	Yes	No	Yes	No	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.222	0.331	0.671	0.789	0.522	0.633
Observations	4,494	4,815	4,494	4,815	4,494	4,815

Panel B: Employment and Job Destruction

	Employment Growth		Job Creation Rate		Job Destruction Rate	
	(1)	(2)	(3)	(4)	(5)	(6)
FF	0.012		0.197***		0.139***	
	(0.10)		(0.04)		(0.02)	
Net States In		10.711***		3.920		-3.784***
		(3.64)		(2.41)		(1.28)
Net States In × FF		-1.976***		-0.728*		0.789***
		(0.56)		(0.42)		(0.20)
Year FE	No	Yes	No	Yes	No	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.126	0.291	0.588	0.636	0.415	0.498
Observations	4,494	4,815	4,494	4,815	4,492	4,813

Note: Standard errors (in parentheses) are clustered at the state level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table 7

Real Effects of Positive/Negative Changes in Monetary Policy

In this table, we show that the real effects of the entry channel documented in Table 6 are stronger for expansionary monetary policy (which we define as a negative annual change in the Fed funds rate). We identify these separate effects by interacting our *Net States In* competition measure with positive and (absolute) negative changes in the annual Fed Funds rate, respectively denoted by Δ^{+} FF and Abs(Δ^{-} FF). *Net States In* is normalized by 100. The sample frequency is annual, and the sample period is 1977-1996.

Panel A: Firms and Establishments

	Firm Growth	Estab. Entry Rate	Estab. Exit Rate
	(1)	(2)	(3)
Net States In	-3.525*	-1.371	2.173***
	(1.97)	(1.54)	(0.74)
Net States In × Δ^{+} FF	0.338	-0.000	-0.441
	(0.67)	(0.45)	(0.46)
Net States In × Abs(Δ^{-} FF)	2.729***	0.820**	-1.689***
	(0.80)	(0.38)	(0.41)
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes
R-Squared	0.328	0.789	0.629
Observations	4,815	4,815	4,815

Panel B: Employment and Job Destruction

	Employment Growth	Job Creation Rate	Job Destruction Rate
	(1)	(2)	(3)
Net States In	-5.604*	-1.929	2.347***
	(3.09)	(2.43)	(0.60)
Net States In × Δ^{+} FF	0.203	-0.253	-0.342
	(1.39)	(0.91)	(0.30)
Net States In × Abs(Δ^{-} FF)	5.371***	2.024**	-1.308***
	(1.85)	(1.00)	(0.23)
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes
R-Squared	0.295	0.637	0.495
Observations	4,815	4,815	4,813

Note: Standard errors (in parentheses) are clustered at the state level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table 8

County-Level Employment and Establishment Growth

In this table, we use the County Business Patterns dataset to study how the entry channel affects county-level employment and establishment growth. In columns (3) and (6), we study the incremental effects on employment and establishment growth for counties located in metropolitan areas (following the 2010 Census definition). The sample frequency is annual, and the sample period is 1986-1996.

	Employment Growth			Establishment Growth		
	(1)	(2)	(3)	(4)	(5)	(6)
FF	0.096			-0.250***		
	(0.09)			(0.05)		
Net States In		4.378	1.734		4.109*	2.465
		(4.37)	(4.76)		(2.31)	(2.30)
Net States In × FF		-1.384**	-1.070		-1.021***	-0.754**
		(0.56)	(0.65)		(0.31)	(0.30)
Net States In × FF × Metro Area			-0.912			-0.847**
			(0.64)			(0.32)
Low-Order Terms	No	No	Yes	No	No	Yes
Year FE	No	Yes	Yes	No	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.090	0.116	0.116	0.166	0.188	0.188
Observations	31,263	31,263	31,263	31,388	31,388	31,388

Note: Standard errors (in parentheses) are clustered at the state level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table 9

County-Level Employment and Establishment Growth by Establishment Size

In this table, we use the County Business Patterns dataset to study how the entry channel affects the growth of establishments of different size (as measured by the number of employees in each establishment). In light of the results from Table 8, we restrict the sample to counties belonging to metropolitan areas. The sample frequency is annual, and the sample period is 1986-1996.

	1-19 Emp.		20-99 Emp.		100-1000 Emp.		1000+ Emp.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
States In × FF	-1.053***		-1.530**		-0.859		-0.787	
	(0.35)		(0.58)		(0.64)		(0.92)	
Net States In × FF		-1.525***		-1.869**		-1.194		-0.836
		(0.39)		(0.74)		(0.75)		(1.24)
Low-Order Terms	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.240	0.241	0.070	0.070	0.069	0.070	0.061	0.061
Observations	10,680	10,680	10,633	10,633	10,188	10,188	5,554	5,554

Note: Standard errors (in parentheses) are clustered at the state level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table 10

Local Bank Characteristics and Real Effects

In this table, we exploit the cross-sectional differences in local banking sector characteristics to show that the entry effect documented in Table 3 and the real effects documented in Table 8 are stronger when local banks are more leverage-constrained. We interact our main *States In* and *Net States In* × FF effects with an indicator equal to one if county-level average leverage (total liabilities to total assets) is above its cross-sectional median in a given year, and equal to zero otherwise. As in the previous tables, we omit the lower-order terms for brevity. The sample frequency is annual, and the sample period is 1986-1996.

	New Branches		Emp. Growth		Estab. Growth	
	(1)	(2)	(3)	(4)	(5)	(6)
States In (SI) × FF	0.043		0.005		-0.092	
	(0.05)		(0.55)		(0.34)	
SI × FF × High Levg.	-0.180**		-2.699***		-1.235***	
	(0.07)		(0.51)		(0.34)	
Net States In (NSI) × FF		0.042		-0.478		-0.540
		(0.09)		(0.53)		(0.35)
NSI × FF × High Levg.		-0.145*		-2.024***		-0.727***
		(0.07)		(0.41)		(0.23)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.693	0.693	0.126	0.127	0.218	0.219
Observations	30,324	30,324	27,497	27,497	27,524	27,524

Note: Standard errors (in parentheses) are clustered at the state level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

A Appendix Tables

A.1 Entry Robustness

Table A1

Robustness: Average Quarterly Target Fed Funds Rate and Entry

In this table, we repeat our main bank entry exercise from Table 3 replacing the end-of-quarter target Fed funds rate with the average Fed funds rate during the quarter. The main objective of the table is to show that our entry results are not sensitive to how we define the Fed funds rate.

	Bank Acquisitions			BHC Acquisitions		
	(1)	(2)	(3)	(4)	(5)	(6)
Avg. FF	-0.009***			-0.187*		
	(0.00)			(0.10)		
States In		0.193**			8.062	
		(0.08)			(4.89)	
States In × Avg. FF		-0.013*			-1.003	
		(0.01)			(0.61)	
Net States In			0.274**			17.336**
			(0.12)			(7.10)
Net States In × Avg. FF			-0.021*			-2.431***
			(0.01)			(0.88)
Year-Quarter FE	No	Yes	Yes	No	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.052	0.164	0.164	0.649	0.735	0.736
Observations	2,652	2,652	2,652	2,652	2,652	2,652

Note: Standard errors (in parentheses) are clustered at the state level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A2

Robustness: Kroszner and Strahan (1999) Indicators and Entry

In this table, we validate our deregulation variables by showing that the entry channel does not manifest itself in bank entry when using workhorse deregulation measures as proxies for changes in barriers to entry. The table is identical to Table 3, but here we replace our own deregulation measures with the Kroszner and Strahan (1999) deregulation measure—a state-year level indicator equal to one after the year in which a state passes its first interstate deregulation law, and zero otherwise.

	Bank Acquisitions	New Branches
	(1)	(2)
KS (1999) Indicator	0.034	-0.427
	(0.04)	(1.73)
KS (1999) Indicator $\times$ FF	-0.003	0.266
	(0.00)	(0.21)
Year-Quarter FE	Yes	Yes
State FE	Yes	Yes
R-Squared	0.161	0.735
Observations	2,652	2,652

Note: Standard errors (in parentheses) are clustered at the state level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A3

Robustness: One-Ways In, National States In, and Entry

In this table, we conduct additional robustness on the entry results from Table 3 with additional network-based deregulation measures as detailed in Bisetti et al. (2019). In Panel A, we focus on one-way, non-reciprocal agreements that allow banks from other states to enter state *i*, without allowing banks from state *i* to enter those other states. We call such arrangements *One-Ways In*, and we call *Net One-Ways In* the difference between *One-Ways In* and non-reciprocal agreements that allow state *i* banks to enter other states without allowing banks from these other states to enter state *i* (*One-Ways Out*). In Panel B, we define *National States In* as *States In* that arise from national agreements, and *Net National States In* are *National States In* minus *States Out* that arise from national agreements.

Panel A: One-Ways In

	Bank Acquisitions		New Branches	
	(1)	(2)	(3)	(4)
One-Ways In × FF	-0.019		-3.311***	
	(0.02)		(1.04)	
Net One-Ways In × FF		-0.021*		-2.433***
		(0.01)		(0.89)
Year-Quarter FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
R-Squared	0.162	0.164	0.735	0.736
Observations	2,652	2,652	2,652	2,652

Panel B: National States In

	Bank Acquisitions		New Branches	
	(1)	(2)	(3)	(4)
National States In × FF	-0.009		-1.232**	
	(0.01)		(0.59)	
National Net States In × FF		-0.018*		-1.959**
		(0.01)		(0.75)
Year-Quarter FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
R-Squared	0.164	0.164	0.735	0.736
Observations	2,652	2,652	2,652	2,652

Note: Standard errors (in parentheses) are clustered at the state level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A4

Robustness: Asset-Weighted State Network Nodes and Entry

In this table, we conduct additional robustness on the entry results from Table 3, weighing our interstate deregulation network nodes by the size of the banking sector in each state (see Bisetti et al. (2019)). The regression specifications are otherwise identical to the specifications in Tables 3 and A3.

	Bank Acquisitions		New Branches	
	(1)	(2)	(3)	(4)
Asset-Weighted States In × FF	-0.005		-0.551*	
	(0.00)		(0.32)	
Asset-Weighted Net States In × FF		-0.009*		-0.943**
		(0.01)		(0.44)
Year-Quarter FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
R-Squared	0.164	0.163	0.735	0.735
Observations	2,652	2,652	2,652	2,652

Note: Standard errors (in parentheses) are clustered at the state level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

A.2 Real Effects Robustness

Table A5

Robustness: States In and Real Effects

In this table, we conduct robustness tests on the results of Table 6, using *States In* (and its interactions) instead of *Net States In* as a regressor. The specifications are otherwise identical to those in Table 6.

Panel A: Firms and Establishments

	Firm Growth	Estab. Entry Rate	Estab. Exit Rate
	(1)	(2)	(3)
States In	3.590	0.337	-3.096***
	(2.26)	(1.37)	(1.10)
States In × FF	-0.696	-0.164	0.515***
	(0.42)	(0.30)	(0.18)
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes
R-Squared	0.324	0.788	0.628
Observations	4,815	4,815	4,815

Panel B: Employment and Job Destruction

	Employment Growth	Job Creation Rate	Job Destruction Rate
	(1)	(2)	(3)
States In	6.254**	1.963	-2.402***
	(3.02)	(1.97)	(0.78)
States In × FF	-1.155**	-0.357	0.490***
	(0.51)	(0.42)	(0.13)
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes
R-Squared	0.288	0.636	0.495
Observations	4,815	4,815	4,813

Note: Standard errors (in parentheses) are clustered at the state level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A6

Robustness: Average Target Fed Funds Rate and Real Effects

In this table, we repeat our main exercise from Table 6 replacing the end-of-quarter target Fed funds rate with the average Fed funds rate during the quarter. The main objective of the table is to show that our results on the real effects of the entry channel are not sensitive to how we define the Fed funds rate.

Panel A: Firms and Establishments

	Firm Growth		Estab. Entry Rate		Estab. Exit Rate	
	(1)	(2)	(3)	(4)	(5)	(6)
States In × Avg. FF	-0.566*		-0.170		0.356**	
	(0.34)		(0.29)		(0.16)	
Net States In × Avg. FF		-1.239***		-0.437		0.769***
		(0.37)		(0.29)		(0.18)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.349	0.354	0.798	0.799	0.640	0.643
Observations	4,494	4,494	4,815	4,815	4,815	4,815

Panel B: Employment and Job Destruction

	Employment Growth		Job Creation Rate		Job Destruction Rate	
	(1)	(2)	(3)	(4)	(5)	(6)
States In × Avg. FF	-0.717**		-0.162		0.369**	
	(0.35)		(0.37)		(0.14)	
Net States In × Avg. FF		-1.143**		-0.430		0.661***
		(0.43)		(0.39)		(0.21)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.327	0.328	0.647	0.647	0.500	0.503
Observations	4,494	4,494	4,815	4,815	4,813	4,813

Note: Standard errors (in parentheses) are clustered at the state level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A7

Robustness: Kroszner and Strahan (1999) Indicators and Real Effects

In this table, we validate our deregulation variables by showing that the entry channel does not manifest itself in real variables when using workhorse deregulation measures as proxies for changes in barriers to entry. This table is identical to table 6 with the only difference that we replace our own deregulation measures with the Kroszner and Strahan (1999) interstate deregulation indicator (as in Table A2).

Panel A: Firms and Establishments

	Firm Growth	Estab. Entry Rate	Estab. Exit Rate
	(1)	(2)	(3)
KS (1999) Indicator	1.428	1.359*	-0.369
	(1.47)	(0.77)	(0.74)
KS (1999) Indicator × FF	-0.029	-0.134	-0.042
	(0.19)	(0.11)	(0.09)
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
MSA FE	Yes	No	No
R-Squared	0.358	0.623	0.570
Observations	4,494	4,815	4,815

Panel B: Employment and Job Destruction

	Employment Growth	Job Creation Rate	Job Destruction Rate
	(1)	(2)	(3)
KS (1999) Indicator	0.669	0.436	-0.035
	(2.54)	(1.15)	(0.56)
KS (1999) Indicator × FF	0.064	-0.019	-0.039
	(0.34)	(0.16)	(0.08)
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
MSA FE	Yes	No	Yes
R-Squared	0.329	0.483	0.499
Observations	4,494	4,815	4,813

Note: Standard errors (in parentheses) are clustered at the state level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A8

Robustness: County-Level Effects and States In

In this table, we conduct robustness tests on the results of Table 8 , using *States In* (and its interactions) instead of *Net States In* as a regressor. The specifications are otherwise identical to those in Table 8.

	Employment Growth			Establishment Growth		
	(1)	(2)	(3)	(4)	(5)	(6)
FF	0.096			-0.250***		
	(0.09)			(0.05)		
States In		3.835	2.258		2.523	1.321
		(2.73)	(3.10)		(1.55)	(1.49)
States In × FF		-0.863	-0.639		-0.601**	-0.399
		(0.54)	(0.60)		(0.29)	(0.28)
States In × FF × Metro Area			-0.749			-0.699***
			(0.45)			(0.25)
Year FE	No	Yes	Yes	No	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Low-Order Terms	No	No	Yes	No	No	Yes
R-Squared	0.090	0.114	0.115	0.166	0.186	0.187
Observations	31,263	31,263	31,263	31,388	31,388	31,388

Note: Standard errors (in parentheses) are clustered at the state level. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

B Non-Separable Utility and Deposit Supply

In this appendix, we show that an inverse deposit supply function such that $\partial^2 r/(\partial D_i \partial f) > 0$ arises naturally in an environment where consumers have non-separable preferences between their final wealth and liquidity services provided by deposits. A similar utility specification is used, for example, in Drechsler et al. (2017).

Suppose that consumers can invest in either bank i's deposits, thus earning the return r_i, or in the Fed funds, thus earning f. While the Fed funds are relatively illiquid and therefore do not enter consumers' utility functions directly, deposits provide consumers with liquidity services. In particular, the representative consumer with initial wealth W_0 derives her utility from a bundle of her final wealth and the liquidity services provided by deposits,

$$u(W_0) = W^{1-\alpha} D^{\alpha}, \tag{B.1}$$

where W is the representative consumer's final wealth, D is itself a utility bundle from the deposits provided by individual banks

$$D = \left(\sum_{i=1}^{n} D_i^{\rho} \right)^{\frac{1}{\rho}}, \tag{B.2}$$

the parameter $\alpha \in (0,1)$ governs the substitutability between wealth and deposits, and the parameter $\rho \in (0,1)$ governs the substitutability between the deposits offered by different banks. The representative consumer chooses W and D_i for $i = 1, 2, \ldots, n$ to maximize (B.1) subject to the budget constraint

$$W \leq \left(W_0 - \sum_{i=1}^{n} D_i \right) f + \sum_{i=1}^{n} r_i D_i \tag{B.3}$$

$$= W_0 f - \sum_{i=1}^{n} (f - r_i) D_i. \tag{B.4}$$

Proposition 4. *The inverse supply function r_i for bank i's deposits arising from the consumer problem (B.1)-(B.4) is such that $\partial^2 r/(\partial D_i \partial f) > 0$.*

Proof. Denote by μ the Lagrange multiplier on (B.4). The Lagrangean for this problem is

$$\mathcal{L} = W^{1-\alpha}\left(\sum_{i=1}^{n} D_i^{\rho}\right)^{\frac{\alpha}{\rho}} - \mu\left[W + \sum_{i=1}^{n}(f - r_i)\, D_i - W_0 f\right], \tag{B.5}$$

with FOCs with respect to W and a generic D_i respectively given by

$$(1-\alpha)\, W^{-\alpha} D^{\alpha} = \mu, \tag{B.6}$$

$$\alpha W^{1-\alpha} D^{\alpha-1} D^{1-\rho} D_i^{\rho-1} = \mu\,(f - r_i)\,. \tag{B.7}$$

Substituting the first FOC into the second, and re-arranging, we get

$$f - r_i = \frac{\alpha}{1-\alpha} W \frac{D_i^{\rho-1}}{\sum_{i=1}^{n} D_i^{\rho}}. \tag{B.8}$$

Denote $p_i \equiv f - r_i$. Then, substituting for W from (B.4), normalizing $W_0 = 1$, and re-arranging we get

$$p_i = \frac{\sum_{j\neq i} p_j - f}{\sum_{j\neq i} D_j^{\rho} - \frac{1}{\alpha}} \frac{D_i^{\rho-1}}{\sum_{i=1}^{n} D_i^{\rho}}, \tag{B.9}$$

where the term $\left(\sum_{j\neq i} p_j - f\right) \Big/ \left(\sum_{j\neq i} D_j^{\rho} - \frac{1}{\alpha}\right)$ is necessarily positive. Hence,

$$r_i = f - \frac{\sum_{j\neq i} p_j - f}{\sum_{j\neq i} D_j^{\rho} - \frac{1}{\alpha}} \frac{D_i^{\rho-1}}{\sum_{i=1}^{n} D_i^{\rho}}, \tag{B.10}$$

and

$$\frac{\partial^2 r_i}{\partial D_i \partial f} = -\frac{1}{\sum_{j\neq i} D_j^{\rho} - \frac{1}{\alpha}} \frac{(\rho-1)\, D_i^{\rho-2} \sum_{i=1}^{n} D_i^{\rho} - \rho D_i^{2(\rho-1)}}{\left(\sum_{i=1}^{n} D_i^{\rho}\right)^2} > 0. \tag{B.11}$$

□