Bank Branching Deregulation and the Syndicated Loan Market

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How do changes in banking regulation affect the syndicated loan market? Because branch networks and loan syndication both facilitate banks' ability to diversify geographical credit risk, we focus on the Riegle-Neal Interstate Branching and Banking Efficiency Act of 1994. We investigate its staggered state-wise implementation in a triple-difference identification strategy, exploiting the fact that the Act only changed the legal framework for out-of-state commercial banks. We find that branching deregulation decreased syndicated loan issuance but spurred bilateral lending to corporations. This shift is also reflected in interest rate spreads, pointing to a supply-driven substitution effect. Our results suggest that changes to banking regulation can affect not only the amount but also the type of credit in the economy.

JEL: G18 · G21 · G30

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The market for syndicated loans is the most important source of corporate financing in the United States.[1] As a result, syndicated loans have been at the center of a large and active body of research.[2] However, there is surprisingly little evidence on how the sweeping changes to banking regulations over the past decades have affected the market. In this paper, we make one step towards closing this gap and investigate the impact of the Riegle-Neal Interstate Banking and Branching Efficiency Act of 1994 (IBBEA) on the pricing and issuance of syndicated loans.

Syndicated loan contracts are originated, structured, and managed by lead banks, which usually screen and monitor borrowers. Apart from collecting fees, one of the primary motivations for lead banks to syndicate loans is to diversify credit risk (Simons, 1993; Preece & Mullineaux, 1996; Gadanecz, 2004). Syndication, however, comes at a cost: coordination among multiple creditors is fraught with asymmetric information and moral hazard problems (Pennacchi, 1988; Gorton & Pennacchi, 1995; Ivashina, 2009; Sufi, 2007) and becomes particularly complex during renegotiations (Gertner & Scharfstein, 1991; Preece & Mullineaux, 1996; Bris & Welch, 2005). As a result, lenders face a trade-off between the advantage of risk diversification and higher coordination costs vis-à-vis other syndicate members. An alternative for a lead bank to diversify its loan portfolio is to rely on direct lending via a branching network (Demsetz, 2000; Deng & Elyasiani, 2008a; Goetz, Laeven & Levine, 2013, 2016).

[1] With USD 2 trillion in annual issuance, it accounts for a larger volume than corporate bonds issued by domestic companies; half of all commercial and industrial (C&I) loans in the US are underwritten by a syndicate (see https://www.bloomberg.com/professional/blog/global-syndicated-loans-league-tables-fy-2016/, https://www.federalreserve.gov/releases/e2/current/default.htm, and Ivashina (2009); Lee, Liu & Stebunovs (2017)).

[2] For important contributions on syndicated loans see, among others, Sufi (2007); Ivashina (2009); Ivashina & Scharfstein (2010); Gopalan, Nanda & Yerramilli (2011); Lin, Ma, Malatesta & Xuan (2012); Ferreira & Matos (2012); Lim, Minton & Weisbach (2014); Chodorow-Reich (2014); Berg, Saunders & Steffen (2016); Amiram, Beaver, Landsman & Zhao (2017); and Cai, Saunders & Steffen (2017). Many other authors have used syndicated loan market data as the primary source in their analyses (e.g. De Haas & Van Horen, 2012; Axelson, Jenkinson, Strömberg & Weisbach, 2013; Dougal, Engelberg, Parsons & Van Wesep, 2015; Ivashina, Scharfstein & Stein, 2015; Kalemli-Ozcan, Kamil & Villegas-Sanchez, 2016; Falato & Liang, 2016; Saidi & Neuhann, 2017).

The IBBEA lifted restrictions on commercial banks to permit them to branch freely across state borders and thus facilitated geographical diversification by allowing more direct access to borrowers (Hughes, Lang, Mester & Moon, 1996; Demsetz, 2000; Calomiris, 2000; Akhigbe & Whyte, 2003). The effect on banking markets was transformative. Out-of-state banks formed extensive, consolidated branching networks, which increased their market share from only 2.5% in 1994 to 45.8% in 2011. Research on lender-borrower distance suggests that the resulting reduction of physical proximity enabled banks to screen and monitor borrowers more effectively (Petersen & Rajan, 2002; Degryse & Ongena, 2005; Hauswald & Marquez, 2006; Hollander & Verriest, 2016; D'Acunto, Liu, Pflueger & Weber, 2017), and without the coordination costs from syndication. We hypothesize that the IBBEA thus changed the incentives of commercial banks to originate syndicated compared to bilateral loans to diversify credit risk.

Consistent with this intuition, we find that the issuance of syndicated loans decreased at both the bank and state levels after branching deregulation, while bilateral lending increased. The overall lending impact of the IBBEA appears to have been positive, albeit small in percentage terms, consistent with Rice & Strahan (2010). Our results thus point to a shifting of debt contract types from syndicated to bilateral loans. We also show that the effects of the IBBEA on the state level crucially depend on banking sector concentration prior to the reform (also see Black & Strahan, 2002; Cetorelli & Strahan, 2006). States with little competition before deregulation – where branching networks expanded most after the restrictions were lifted – saw the largest effect for both direct and syndicated lending.

To refine our identification strategy, we exploit cross-sectional variation in loan characteristics. The resulting triple-difference set-up generates loan-level variation, which allows us to absorb unobserved time-varying borrower and bank characteristics using *borrower* × *year* and *bank* × *year* fixed effects. Since borrowers frequently issue multiple individual facilities in the same year (40% in our estimation sample), the data give us am-

ple variation. The benefit of this approach is that we can rule out alternative explanations based on unobserved borrower risk or credit demand.

We find that the IBBEA had a differential effect on debt pricing: interest rate spreads for bilateral loans decreased by about 7% but increased for syndicated loans by about 2%. While direct loans are more expensive throughout (also see Angbazo, Mei & Saunders, 1998; Dennis & Mullineaux, 2000; Ivashina, 2005), they are less so after branching deregulation. Interpreted jointly with the effect on lending volumes, these results suggest access to credit improved at the expense of syndicated lending following interstate branching deregulation. Exploring heterogeneity in borrower attributes, we also find that smaller, riskier, and more opaque firms experienced a decrease in interest rates. This is consistent with branches lowering monitoring costs by decreasing the distance between lenders and borrowers (D'Acunto et al., 2017).

Syndicated loans, however, also differ from bilateral contracts in other aspects: they tend to be considerably larger and carry longer maturities, even after controlling for borrower fundamentals. To rule out alternative explanations, we make use of the fact that only one group of lead arrangers was subject to the reform: out-of-state commercial banks. The regulatory change did not alter the legal framework for other lenders, such as pension funds, hedge funds, investment banks, or commercial banks with headquarters *within* a deregulating state (Johnson & Rice, 2008; Favara & Imbs, 2015).[3] Importantly, the loans issued by out-of-state commercial banks are not systematically different than issued by non-bank lenders or in-state banks. Consistent with our substitution hypothesis, we find that only affected lenders saw increased spreads, even within the same borrower-year; other loans carried lower interest rates post-deregulation, consistent with an increase in lending competition.

[3]Thrift and other deposit-taking and non-deposit-taking organizations were able to engage in interstate branching before the IBBEA (Rice & Strahan, 2010). See also http://www.pli.edu/product_files/Titles%2F4655%2F55199_sample02_20141011115534.pdf for a discussion of how branching regulations affected different financial institutions.

We go to great lengths to verify the validity of our empirical approach. We find that interest rate spreads and loan volumes exhibit a level-shift around the state-level implementation of the IBBEA without pre-existing trends or reversals. Further, we construct a placebo test and find that the sequence of the staggered deregulation was not correlated with changes in spreads before the reform. We also provide evidence that the reform timing was not driven by a state's syndicated loan market size or its market concentration. Our results are also robust to a host of validity checks that let us rule out sample selection concerns; the influence of single states; joint determination in loan terms; or the tranching of facilities for non-bank investors.

We build on the insight that banks are relevant even for large, publicly listed companies with direct access to financial markets (e.g. Dahiya, Saunders & Srinivasan, 2003; Bharath, Dahiya, Saunders & Srinivasan, 2007; Ross, 2010; Chodorow-Reich, 2014; Cai et al., 2017). For example, lending relationships in the syndicated loan market have been found to affect interest rates spreads (Schenone, 2010; Bharath, Dahiya, Saunders & Srinivasan, 2011), loan terms during financial distress (Li, Lu & Srinivasan, 2017), corporate governance (Dass & Massa, 2011), and firm outcomes (Gopalan, Udell & Yerramilli, 2011). More broadly, Sufi (2007) finds that proxies for information asymmetries shape monitoring incentives. In a recent paper, D'Acunto et al. (2017) show that the leverage of publicly listed firms increased with branching deregulation, which the authors interpret as a shock to monitoring costs. This literature suggests that the sweeping changes to the US banking system brought about by the IBBEA may also affect the syndicated loan market.

Our work further contributes to the literature on the influence of geographical diversification for bank risk (Diamond, 1984; Demsetz & Strahan, 1997; Acharya, Hasan & Saunders, 2006; Goetz et al., 2013, 2016); physical distance for loan terms and access to credit (Petersen & Rajan, 2002; Degryse & Ongena, 2005; Hauswald & Marquez, 2006; Hollander & Verriest, 2016); and the effects of US branching deregulation (e.g. Stiroh &

Strahan, 2003; Johnson & Rice, 2008; Rice & Strahan, 2010; Chava, Oettl, Subramanian & Subramanian, 2013; Jiang, Levine & Lin, 2017). More broadly, our work is embedded into the literature on financial deregulation in the US.[4] Banking deregulation has been associated with higher per capita growth rates (Jayaratne & Strahan, 1996); higher loan volumes (Amore, Schneider & Zaldokas, 2013); lower income inequality (Beck, Levine & Levkov, 2010); and many other economic outcomes. Our paper is also related to studies investigating the reallocation effects of financial reforms, such as Chava et al. (2013) and Hombert & Matray (2016), who show that *intrastate* branching deregulation decreased innovation by firms with fewer pledgeable assets.[5]

The remainder of the paper is organized as follows. In section 1, we discuss why lenders might use syndication to geographically diversify their loan portfolio and how this relates to branching restrictions. Section 2 presents the data and variable construction. Section 3 introduces the identification strategy and discusses the results. Section 4 concludes.

1 Hypothesis Development

The hypothesis we are testing in this paper is that allowing interstate branching decreased the benefits of loan syndication for the out-of-state commercial banks affected by the IBBEA. To explain the underlying rationale, we review institutional details of the IBBEA and discuss how it may have affected the profitability for lead banks to engage in loan syndication. A comprehensive discussion of loan syndication is beyond the scope of this paper; we focus instead on the mechanisms that were likely influenced by the existence of geographical restrictions and their deregulation.[6]

[4] See e.g. Kroszner & Strahan (2014) for an excellent review of the topic.

[5] See also Boot & Thakor (2000) for evidence on reallocation effects of increases in competition.

[6] Other important motives omitted in the discussion are that syndication allows lead arrangers to specialize in certain types of lending (Gorton & Pennacchi, 1995) and increase their fee-based income (Dennis & Mullineaux, 2000).

1.1 Riegle-Neal Interstate Banking and Branching Efficiency Act of 1994

Congress introduced the Riegle-Neal Interstate Banking and Branching Efficiency Act of 1994 with the intention to remove restrictions on interstate branching, i.e. to allow banks to open branches outside their home states and operate these directly within the central organization of the bank holding company (BHC). Interstate *branching* differs from interstate *banking*, where a BHC founds or acquires institutions in other states to operate them as separately chartered and capitalized subsidiaries. Most restrictions to this latter form of expansion as well as other geographical limitations within states were lifted several years before the IBBEA (Berger, Kashyap, Scalise, Gertler & Friedman, 1995; Jayaratne & Strahan, 1998; Johnson & Rice, 2008).[7] In contrast, interstate branching restrictions were almost uniformly in place in 1994, when only Utah allowed unrestricted and unconditional interstate branching.[8] While facilitating the transition towards a liberalized system, the IBBEA allowed individual states to effectively impose anti-competitive obstacles to interstate branching. First, states were allowed to set minimum age requirements (up to five years) with respect to how long a bank must have been in existence before being acquired in an interstate merger. This also forced banks to wait until newly established subsidiary offices could be consolidated to branches. Second, the IBBEA preserved the right of states to impose deposit caps of less than 30%. This served to prevent interstate mergers which would result in banks accounting for a greater share in insured state deposits than set by the cap (including deposits of affiliated institutions). In essence, this protected large state banks from takeovers. Third, an initial entry by a bank through de

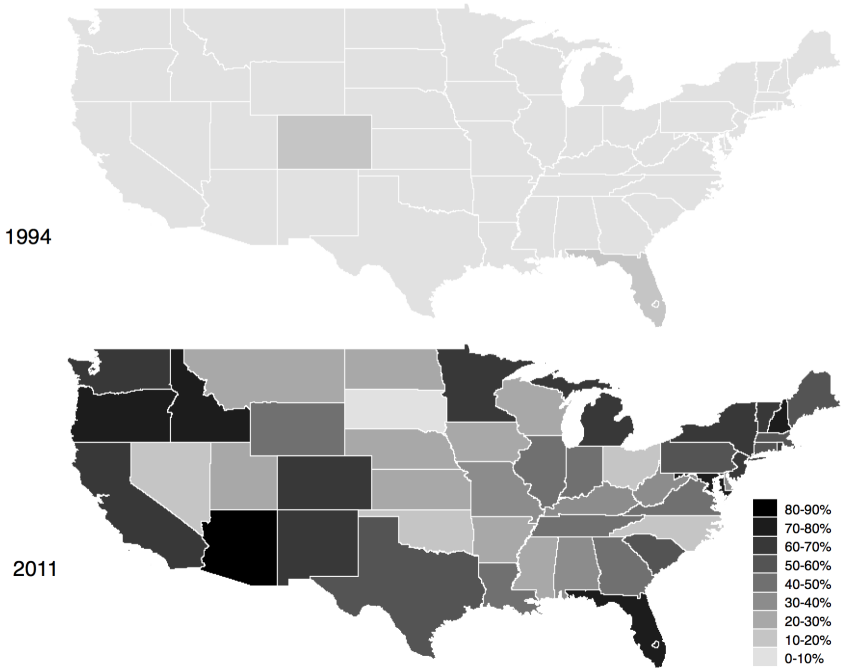
[7] All states except Hawaii had entered an interstate banking agreement by 1994, while failed large banks and thrifts or those at risk of failing could be acquired by out-of-state banks nationwide irrespective of state laws since 1982. Intrastate branching was deregulated in all states but one in 1994, while all states relaxed intrastate banking restrictions before 1992 (Jayaratne & Strahan, 1998; Kroszner & Strahan, 1999, 2014).

[8] Nevada allowed interstate branching for counties with a population of less than 100,000 while six other states allowed some form of interstate branching on a reciprocal basis. Prior to IBBEA, nationally chartered banks in locations close to a state border could also move their main office to a different state, leaving the former location as a branch (Johnson & Rice, 2008).

novo interstate branching was only permitted if states "opted in" to allow this explicitly. Fourth, states had to opt in to legalize the acquisition of a single branch or a number of branches (without acquiring the entire bank itself). Any of these four provisions could be offered by states with reciprocity.

All these possible restrictions limited the available paths of entry, slowed down the process, and added significant costs for banks to enter other states' markets. Further, individual states lifted these restrictions at different points of time and to varying degrees (Johnson & Rice, 2008), a fact we exploit in our identification strategy. The massive effect on local banking industries is illustrated in figure 1: the average deposit market share of out-of-state banks increased from only 2.5% in 1994 to 45.8% in 2011.

Figure 1: Market Share of Out-of-State Banks, 1994 vs. 2011



This figure plots the market share of out-of-state banks (those with an ultimate holding company located in another state) based on deposits in 1994 and 2011. The state-level market shares are the deposit-weighted averages based on county-level data from the FDIC.

1.2 Syndicated versus Bilateral Loans

Syndicated loans are partial substitutes for direct bilateral lending through bank branches (Demsetz, 2000). Such loans have several advantages but also involve additional coordination costs. In this section, we explain how the IBBEA may have affected this trade-off by enabling branching networks to expand across state borders, thus reducing some benefits of loan syndication. We hypothesize that the resulting branching expansion led to issuance volumes increasing for bilateral loans (while decreasing for syndicated loans) and a reduction of the interest rate discount at which syndicated loans usually sell (Angbazo et al., 1998; Dennis & Mullineaux, 2000; Ivashina, 2005).

Syndication incurs coordination costs because of adverse selection and moral hazard problems, arising from the incentive of lead banks to syndicate risky loans and cut down on monitoring effort (Pennacchi, 1988; Gorton & Pennacchi, 1995; Holmstrom & Tirole, 1997a,b; Sufi, 2007; Ivashina, 2009). The complexity and cost of renegotiations is also higher for syndicated contracts with multiple creditors and different claims or interests vis-à-vis a borrower than for bilateral loans (Gertner & Scharfstein, 1991; Preece & Mullineaux, 1996; Bris & Welch, 2005).[9] Accordingly, lenders must balance these syndicate-specific coordination costs with the benefits of syndication. Direct lending via a bank's own branching networks is not associated with creditor coordination costs (Demsetz, 2000) but involves screening and monitoring expenses. In a syndicate, these costs are shared with participants (which usually pay fees to the lead arranger) and are likely to be lower because syndicate leaders prefer to minimize their screening and monitoring efforts. Since the costs of screening and monitoring increase with physical lender-borrower distance (Petersen & Rajan, 2002; Degryse & Ongena, 2005; Hauswald & Marquez, 2006; Hollander & Verriest, 2016), the creation of branches in distant states can be interpreted

[9] A recent example for an analysis of creditor coordination costs in the context of covenant-light contracts is Becker & Ivashina (2016). Gertner & Scharfstein (1991) Hart & Moore (1995), Bolton & Scharfstein (1996), and Bris, Welch & Zhu (2006) focus on creditor coordination more broadly.

as a negative shock to these costs (D'Acunto et al., 2017). This likely increased the profitability of bilateral relative to syndicated loans.

Risk diversification is one of the main reasons commercial banks syndicate loans (Simons, 1993; Preece & Mullineaux, 1996; Sufi, 2007). Lead banks can share loan exposure with other lenders, which may reduce exposure to idiosyncratic local shocks and enhance stability (Diamond, 1984; Boyd & Prescott, 1986; Demsetz & Strahan, 1997; Berger, Demsetz & Strahan, 1999).[10] In a regulatory regime that inhibits (direct) geographical diversification and closely ties the fortunes of banks to their local economies (Calomiris, 2006), syndication helps overcome limitations and reduces exposure to geographical and local industry shocks (Simons, 1993; Dennis & Mullineaux, 2000; Gadanecz, 2004). Others have argued that direct lending via a broad branching network is an alternative way to diversify a loan portfolio (Demsetz, 2000; Deng & Elyasiani, 2008a; Goetz et al., 2013, 2016) and that banks took advantage of this newly available alternative following the IBBEA (Hughes et al., 1996; Calomiris, 2000; Akhigbe & Whyte, 2003).

Loan syndication is also a way for lead banks to reduce "concentration risk" and meet related regulatory rules (Berger & Udell, 1993; Simons, 1993; Dennis & Mullineaux, 2000; Gadanecz, 2004). For example, the FDIC requires banks to limit the loan size to a single borrower to 15% of its capital and reserves (25% if fully secured by readily marketable collateral).[11] To comply with regulatory requirements and reduce exposure to large individual borrowers, lead banks may prefer to share credit risk exposures with other lenders instead of holding them entirely on their own balance sheet (Pennacchi, 1988; Dennis & Mullineaux, 2000; Simons, 1993). Syndicated lending allows especially smaller banks to lend to large borrowers they could not otherwise serve without "overlining" regulatory limits. Large borrowers, however, have considerably lower default rates and more "pres-

[10] In line with this, Deng & Elyasiani (2008b) find a premium for US banks diversifying nationally and Goetz et al. (2013, 2016) find that geographic expansion reduces bank risk.

[11] See https://www.fdic.gov/regulations/pcis/rules/8000-7400.html. Syndication can also help meet minimum capital requirements (Simons, 1993).

tige" from which banks may benefit (Muermann, Rauter & Scheuch, 2017). The dramatic increase in bank size following interstate branching deregulation (median deposits were $74 million in 1994 and $137 million in 2011 in real terms) likely eroded this advantage of syndication by allowing the typical bank to underwrite considerably larger (bilateral) loans entirely on its own.[12]

Access to large or distant borrowers through syndication further enables lenders to cross-sell other services (Gadanecz, 2004). The revenue from these sales might have cross-subsidized spreads on syndicated loans prior to deregulation (Gaspar, Massimo & Matos, 2006; Lepetit, Nys, Rous & Tarazi, 2008), which may partially explain the discount compared to bilateral loans (Angbazo et al., 1998; Dennis & Mullineaux, 2000; Ivashina, 2005). The newly gained ability to directly access potential customers post-deregulation may reduce the willingness to continue offering the previously "subsidized" rates on syndicated loans.

Overall, it appears reasonable to expect that more extensive branching networks post-deregulation induced banks to partially substitute syndicated for bilateral loans. Empirically, we would expect less syndicated loan issuance, an larger supply of bilateral loans, and a lower interest rate discount for syndicated loans. In particular, one would expect a decrease in the spreads of bilateral contracts, consistent with an increase in credit supply (and previous evidence in Rice & Strahan (2010) and D'Acunto et al. (2017)). The impact of IBBEA implementation should also be entirely driven by the lenders that were legally affected by the IBBEA – namely out-of-state commercial banks (Johnson & Rice, 2008; Favara & Imbs, 2015). We put these empirical predictions to the test in section 3.

[12] In an interview with the *Frankfurter Allgemeine Zeitung* in 2008, the CEO of Commerzbank explained the rationale as follows: "If a multinational corporation requested a €10 billion loan for an acquisition in the past, you would have to form a syndicate for that. Today, a single large bank can lend such an amount and keep the entire profit for itself."

2 Data

2.1 Data Sources

The main data sources are detailed loan information from the Thomson Reuters LPC Dealscan database matched with firm-level data from S&P's Compustat North American Annual Fundamentals file. We use the Compustat Ratings package and Mergent's Fixed Income Securities Database (FISD) to obtain information on firm ratings. As is standard in the literature, we exclude financial firms (SIC 6000-6999), regulated utilities (SIC 4900-4999), and public administration (SIC >9000). We also drop all non-US firms and firm-years with negative assets.

We matched the Dealscan files to the firm-level data using the Dealscan-Compustat link file from Chava & Roberts (2008). We start our estimation sample in 1987 to establish a reasonable pre-reform control group before the IBBEA passing in 1994. In our main estimations, we restrict the sample to the period up to and including 2007 to abstract from the impact of the financial crisis, which may have had a differential impact across states depending on their liberalization policies. We show in section 3.6 that our results are robust to extending the sample period until 2012 (the end of the Chava-Roberts link file).

We capture the intensity of branching deregulation using the time-varying index of Rice & Strahan (2010). They set the index to 0 for states with no restrictions and add 1 for each type of restriction imposed, so that the index ranges from 0 to 4.[13] The index is matched to our Dealscan-Compustat dataset using Compustat's location data (variable *state*) and the exact date branch restrictions were lifted.[14] We use this variation to create

[13] Following Rice & Strahan (2010) and Favara & Imbs (2015), we set the index to "fully restricted" (0) before 1994 where we have no further information, assuming that states were fully restricted before the passage of the IBBEA. This is a reasonable approximation given the evidence on limited de facto cross-border branching activity presented above. Note that while the original index ends in 2005, we let it run until 2007 as in D'Acunto et al. (2017) because no state reversed its liberalization decision.

[14] For each facility, we know the exact issuance date and use this to merge loans with deregulation dates. All results presented here are robust to assigning the index based on the year of implementation. These

a dummy variable if a state lifted at least one restriction, as in D'Acunto et al. (2017), Chava et al. (2013), and Koetter, Kolari & Spierdijk (2012). The resulting "treatment" dummy is 1 for all state-years in which the index is equal to 1 or larger, and 0 otherwise, indicating that out-of-state state banks are allowed to erect or acquire local branches. We later show that our results are robust to using the (reversed) continuous index from Rice & Strahan (2010).

We use data from the FDIC's Summary of Deposits to measure a state's existing market concentration prior to the IBBEA passing in 1994.[15] More specifically, we calculate a county's Herfindahl-Hirschman Index (HHI) based on the deposits held by each bank and then aggregate this to the state-level by weighting by a county's total deposits. We also draw on FDIC data to identify whether a bank had a branch presence in a state prior to deregulation. These data are only available from 1994.

2.2 Variable Construction

The unit of observation in the first part of our study is a loan contract facility, where multiple facilities may be included in a deal package. The main dependent variable is a loan's all-drawn interest rate spread (usually over LIBOR).

To isolate the effect of branching deregulation on spreads, we hold the contract terms loan size and maturity (in natural logarithms) constant in the baseline regressions and include a dummy for whether a loan uses collateral. All regressions include dummy variables for whether a firm has ever received a loan from the lead arranger bank to control for an existing lender-borrower relationship; as well as for whether a facility is a term loan. We also control for differences across loan purposes by including a full set of 22 dummy variables.[16]

results are available upon request.

[15] Alaska is the only state that lifted a branching restriction in the year of the reform in 1994.

[16] Since we are interested in loans used for normal business transactions, we exclude all loans whose primary purpose is related to mergers and acquisition activities. Such loans are identified as "Acquis. line", "LBO", "MBO", "Merger", "SBO", or "Takeover". This exclusion, however, does not drive our results.

In our triple-difference estimations, we use cross-sectional exposure on the facility-level. First, we look at syndicated loans, which we define as facilities with a distribution method other than "sole lender". Second, we use a dummy equal to 1 when the lender is a commercial bank (identified as lenders with a SIC 3-digit code of 602). For both exposure variables, an average of approximately 79% of loans in the estimated sample are considered "treated".[17] We also condition the reform impact on whether a bank had a branch presence in the borrower state prior to IBBEA implementation ("out-of-state bank"). The focus on branches is motivated by the IBBEA institutional detail, as banks were allowed to operate local non-consolidated subsidiaries, which in turn may have had branches, even before IBBEA implementation. Most lead arrangers in the sample, around 62%, did not have a local presence prior to the reform.

We also control for borrower fundamentals from Compustat, including book leverage, Tobin's Q, total assets (in natural logarithm), sales growth, ROA, an indicator for rated firms, and quartiles for borrowers with negative or very high debt-to-EBITDA ratios (Roberts & Sufi, 2009; Nini, Smith & Sufi, 2009). The exact variable definitions can be found in the online appendix and the summary statistics in table 1. To minimize the impact of outliers, we exclude firm-years with total asset growth exceeding 200% and winsorize balance sheet variables at the 1st and 99th percentiles; Tobin's Q is further winsorized at 10, as in, e.g., Gompers, Lerner & Scharfstein (2005).[18] Note that our sample varies drastically from the Survey of Small Business Finance (SSBF) previously analyzed in the context of the IBBEA Rice & Strahan (2010); in the online appendix, we discuss these differences in more detail.

For the state-level analysis, we aggregate data from Dealscan for which we can identify borrower states in either Compustat or the Dealscan company file. In a few state-years,

[17] Note that the relatively high fraction of loans extended by commercial banks is not inconsistent with the increasing presence of non-bank intermediaries that has been documented in the leveraged loan market (see e.g. Nandy & Shao, 2010). The reason the share of commercial banks syndicates is higher in our sample is because it also includes investment grade loans.

[18] The results are not driven by these winsorization choices.

Table 1: Descriptive Statistics

	Mean	p10	p25	Median	p75	p90	SD	Obs.
Interstate Branching Deregulation Index								
Deregulated Dummy	0.639	0	0	1	1	1	0.480	14,102
Loan Characteristics								
Interest Rate Spread (log)	5.152	3.912	4.723	5.298	5.704	5.999	0.799	14,102
Maturity in Months (log)	3.560	2.398	3.178	3.664	4.094	4.277	0.739	14,102
Size in $ Million (log)	4.349	1.873	3.091	4.527	5.683	6.543	1.818	14,102
Secured Dummy	0.763	0	1	1	1	1	0.425	14,102
Pre-existing Bank Relation	0.364	0	0	0	1	1	0.481	14,102
Term Loan Dummy	0.278	0	0	0	1	1	0.448	14,102
Syndicated Loan Dummy	0.795	0	1	1	1	1	0.403	14,102
Commercial Bank Dummy	0.801	0	1	1	1	1	0.399	12,537
Out-of-State Bank Dummy	0.624	0	0	1	1	1	0.484	5,174
Lagged Firm Characteristics								
Leverage	0.343	0.040	0.170	0.313	0.468	0.635	0.257	14,102
Market/Book Ratio	1.425	0.585	0.779	1.073	1.596	2.480	2.210	14,102
Total Assets (log)	5.947	3.471	4.545	5.927	7.289	8.460	1.921	14,102
ROA	0.113	0.009	0.072	0.118	0.170	0.231	0.135	14,102
Negative Debt/Cash Flow	0.056	0	0	0	0	0	0.230	14,102
High Debt/Cash Flow	0.435	0	0	0	1	1	0.496	14,102
Sales Growth	1.023	-0.124	-0.004	0.100	0.278	0.623	34.911	14,102
Rating Dummy	0.398	0	0	0	1	1	0.490	14,102
Asset Tangibility	0.321	0.067	0.138	0.266	0.459	0.678	0.229	14,102
Pre-reform Borrower Characteristics								
Credit Rating	9.953	6	7	10	12	14	3.398	1,865
Asset Tangibility	0.343	0.079	0.156	0.296	0.494	0.684	0.230	4,404
Total Assets (log)	6.332	4.082	5.080	6.197	7.508	8.761	1.816	4,408
Bank-State-level Variables								
Log(1+Syndicated Issuance)	7.391	0	0	10.243	11.657	12.777	5.503	15,982
Log(1+Bilateral Issuance)	0.796	0	0	0	0	0	2.654	15,982
Out-of-State Bank Dummy	0.841	0	1	1	1	1	0.366	15,982
State-level Variables								
Syndicated Issuance/GDP	0.048	0.000	0.005	0.027	0.068	0.119	0.067	1,479
Bilateral Issuance/GSP	0.005	0.000	0.000	0.002	0.006	0.015	0.010	1,479
HHI (1994)	0.199	0.112	0.157	0.201	0.224	0.287	0.064	1,479
House Price Growth	0.036	-0.024	0.005	0.037	0.058	0.098	0.056	1,479
Real GSP p.c. Growth	0.025	-0.006	0.009	0.023	0.041	0.059	0.028	1,428

we cannot identify a single syndicated or bilateral loan and thus set the aggregate loan issuance variables to zero. We scale total state-level issuance volumes over Gross State Product (GSP), which we retrieve from the Bureau of Economic Analysis (BEA).[19] We further obtain data on all-transactions house prices from the Federal Housing Finance Agency (FHFA) and generate annual growth rates as the percentage change in end-of-year values. Real GSP growth comes from the BEA.

We also conduct an analysis on the bank-state-year level. Similar to the state-level aggregates, we calculate the total syndicated and bilateral loan issuance of a lender in a given state as the total of loan issuance volume.[20] We have more variation in syndicated loans because they are much more common in our sample than bilateral contracts. In the regressions, we use the natural logarithm of 1 + issuance volume and adjust the volumes for inflation.

3 Identification Strategy and Results

In this section, we outline the empirical strategy we use to assess the effect of the IBBEA on the syndicated loan market and present our results. We describe the loan-level specification and the results for spreads in section 3.1. In section 3.2, we investigate loan volumes at the bank-level. We then turn, in section 3.3, to state-level aggregate effects by exploiting a differences in pre-reform banking sector concentration. Finally, we discuss threats to identification in section 3.4, impact heterogeneity in section 3.5, and further robustness checks in section 3.6.

[19] We adjust for the time series break caused by the SIC-NAICS transition in 1997 by chain-linking the overlapping series. We also winsorize these ratios at the 0.25 and 99.75 level to account for a few outliers early in the syndicated loan time series, when the coverage is somewhat spottier. These adjustments do not drive our results.

[20] More precisely, we assign loan volumes to lenders based on the Dealscan variable "bankallocation".

3.1 Loan-Level Evidence

To test the effect of the IBBEA on loan contracts, our starting point is the following difference-in-differences set-up:

$$\begin{aligned} Interest\ Rate_f =& \beta Deregulated_{st} + \gamma Borrower\ Controls_{i,t-1} \\ &+ \delta Contract\ Controls_f + \alpha_i + \alpha_t + \varepsilon_f, \end{aligned} \tag{1}$$

where f, s, i, and t denotes loan contract facilities, states, borrowers, and years, respectively. $Deregulated_{st}$ is the 1/0 dummy for IBBEA implementation described above, which varies by state and date; $Borrower\ Controls_{i,t-1}$ is the vector of lagged borrower control variables; and $Contract\ Controls_f$ is the vector of contract-level controls. To absorb plausible within-correlation caused by the treatment effect (Petersen, 2009), we cluster standard errors by state.

The coefficient estimate $\hat{\beta}$ above is supposed to capture the deregulation effect. The setup in equation 1, however, has the disadvantage that the IBBEA had a plethora of effects on the financial sector and local economies. This makes it difficult to pin down a specific channel. To narrow down the substitution effect of the IBBEA, we thus use a triple-difference strategy. In its most saturated form, this is similar in spirit to Jiménez, Ongena, Peydró & Saurina (2014) and Khwaja & Mian (2008). In particular, we allow for differential treatment impact by interacting the dummy $Deregulated_{st}$ with contract details $Exposure_f$, which allows us to fully absorb time-varying *borrower*×*year* and *bank*×*year* factors with fixed effects, yielding:

$$\begin{aligned} Interest\ Rate_f =& \beta_1 Deregulated_{st} + \beta_2 Deregulated_{st} \times Exposure_f + \beta_3 Exposure_f \\ &+ \gamma Borrower\ Controls_{i,t-1} + \delta Contract\ Controls_f + \alpha_{it} + \alpha_{bt} + \varepsilon_f, \end{aligned} \tag{2}$$

where b stands for banks, which we identify as a loan's lead arranger as is standard

in the literature. Using the interacted fixed effects α_{it} and α_{bt} intuitively means that we compare loans issued by the *same borrower* in the *same year* while also taking out unobserved time-varying bank factors. This is important in our setting, because it rules out that borrower or bank fundamentals unrelated to the loan-level variation drive our results.

We use different variables for $Exposure_f$. First, we use a dummy for syndicated loans to directly test our substitution hypothesis. Second, we use a dummy variable for loans where the lead arranger is a commercial bank, and thus legally affected by the reform, within the group of syndicated loans. Third, we use a dummy for out-of-state banks within the group of syndicated loans issued by commercial banks (also see Favara & Imbs, 2015). We discuss these exposure variables in more detail momentarily.

The identifying assumption underlying our empirical specification is *not* that "exposed" loans are similar in observable or unobservable characteristics to those that are "not exposed". Rather, the assumption is that their interest rate spreads would have trended similarly in the absence of state-level branching deregulation. This assumption is supported by two pieces of evidence. First, while syndicated and bilateral loans differ in their typical loan terms, there are *no* consistent differences between syndicated loans issued by commercial banks vs. other lenders, or out-of-state vs. in-state commercial banks (see figure 5). Because we also exploit variation in lender types *within* the group of syndicated loans, we are unlikely to capture a deregulation effect that works through other loan characteristics. Second, and perhaps most important, spreads showed a similar trajectory for "exposed" and "not exposed" loans in the years prior to deregulation, as we show below. That is, they showed no *change* in spreads until after implementation of the IBBEA. We discuss these and other threats to identification in more detail below and in section 3.4.

Equipped with our empirical strategy, we begin by running the baseline regression 1, meaning we regress a loan facility's interest rate spread on the *Deregulated* dummy and

borrower and year fixed effects as well as control variables. Column (1) in table 2 plots the results. Recall that the clear majority of contracts in our sample are syndicated. The estimated coefficient is *positive* and highly statistically significant, with a point estimate of 0.054. Since the dependent variable is measured in natural logarithm, we use the Taylor series approximation throughout the paper to translate log-points into percentage changes to help the interpretation of our findings. An increase in 5.4 log points thus represents an increase of approximately 5.4% in interest rate spreads, which is equivalent to a small increase of around 11 basis points from the median spread of 200 basis points (see the summary statistics in table 1).

Table 2: The Effect of Deregulation on Syndicated and Bilateral Loan Spreads

		Interaction Variable:					
	Borrower FE (Baseline)	Syndicated Loan		Affected Lender		Out-of-State Bank (Pre-Reform)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Deregulated	0.054**	-0.073***	–	-0.049	–	-0.107	–
	(0.025)	(0.026)		(0.052)		(0.068)	
Deregulated × Interaction Variable		0.095***	0.122*	0.120**	0.248**	0.204***	0.609***
		(0.302)	(0.065)	(0.051)	(0.109)	(0.079)	(0.180)
Interaction Variable		-0.139***	-0.087*	-0.172***	–	-0.126	-0.231
		(0.025)	(0.052)	(0.052)		(0.073)	(0.229)
Loan Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	–	Yes	–	Yes	–
Borrower FE	Yes	Yes	–	Yes	–	Yes	–
Year FE	Yes	Yes	–	Yes	–	Yes	–
Borrower × Year FE			Yes		Yes		Yes
Bank × Year FE			Yes		Yes		Yes
Only Syndicated Loans				✓	✓	✓	✓
Only Commercial Banks						✓	✓
Observations	14,102	14,102	9,703	9,689	6,992	3,148	2,212
Adjusted R^2	0.819	0.820	0.951	0.842	0.958	0.886	0.967

This table reports the results of regressing the interest rates spreads of syndicated loans (in natural logarithm) on *Deregulated*, a dummy that equals 1 if a state has lifted one or more branching restrictions, and interaction variables. "Syndicated loan" is a dummy for loans with more than one creditor. "Affected lenders" is a dummy equal to 1 for commercial banks, i.e. lenders with a SIC code starting with 602; and 0 for other lenders where industry classification is available. "Out-of-State Bank (Pre-Reform)" are banks without branches in the borrower's state prior to IBBEA implementation. The sample in columns (4) and (5) is restricted to syndicated loans only, and in (6) and (7) to syndicated loans issued by commercial banks. "–" indicates absorbed estimates. See the text for included control variables. Robust standard errors (in parentheses) are clustered on the state-level, with ***, **, and * indicating statistical significance at the 1%, 5%, and 10% levels.

We next attempt to strengthen these baseline results by exploiting differential exposure of borrowers to the reform (as in equation 2). If our substitution hypothesis is correct, we would expect that interstate branching had markedly different effects on syndicated and bilateral loan pricing. We run a direct test in column (2) of table 2, where we introduce a dummy for whether a loan is syndicated and interact it with the *Deregulated* treatment dummy. The interaction term $Deregulated \times Syndicated\,Loan$ enters positively with a coefficient of 0.095; it is −0.073 for the deregulation measure itself. Both estimates are highly statistically significant. This implies that interest rates increased almost exclusively on loans with more than a single creditor: the effect on "bilateral loans" was a sizeable 7.3% decrease, while syndicated loan spreads *increased* by $0.095 - 0.073 = 2.2\%$. The loan-level variation of the treatment effect also allows for the inclusion of a full set of interacted fixed effects in column (3), where the triple-difference effect increases to 0.122. This suggests that unobserved time-varying factors on the bank or borrower levels do not drive our results.

Could it be that other loan characteristics that happen to correlate with syndication drive these results? To get around this issue, we exploit the nature of interstate branching laws to conduct a simple test based on bank types. The IBBEA only altered restrictions on cross-state branching for commercial banks and did not apply to other lenders (Johnson & Rice, 2008; Favara & Imbs, 2015). We thus treat commercial banks as "affected" and other lenders as "unaffected" within the group of syndicated loans. Importantly, commercial bank loans in our sample show considerably fewer differences from non-bank loans compared to the syndicated-bilateral split.[21] If we are indeed correct that spreads increased due to a substitution effect between local branches and syndication, only institutions affected by the changes in regulation should react. Indeed, previous work by Favara & Imbs (2015) and Rice & Strahan (2010) suggests that the impact on unaffected lenders should be a reduction in rates due to the increase in competition.

[21] We discuss these differences in more detail in section 3.4.

We test this by again running equation 2, this time interacting *Deregulated* with a dummy for whether a lender is classified as a commercial bank. The result is reported in column (4) of table 2. The interaction term now has an estimated coefficient of 0.120, which is significant at the 5% level. Again, the *Deregulated* dummy itself now turns negative with a value of -0.049. While it is imprecisely estimated, this suggests a *decrease* in interest rates for loans issued by lenders other than commercial banks. It is instructive to compare these estimates with the baseline estimate in column (1). Our estimates suggest that deregulation *increased* spreads for affected lenders by $0.120 - 0.049 \approx 7.1$ log points, which is close to the baseline estimate in column (1). The deregulation-induced increase in spreads thus appears to be largely driven by directly affected institutions. Importantly, the triple-difference specification also lets us absorb demand or risk-based explanations by including a full vector of $borrower \times year$ and $bank \times year$ dummies in column (5). The interaction term $Deregulated \times Exposure$ is again significant at the 5% level and approximately doubles in size.

Next, we turn to variation *within* the group of syndicated loans issued by the affected commercial banks by comparing out-of-state versus in-state banks. Intuitively, we would expect that banks with a local presence before the reform were subject to greater competition from out-of-state banks entering the market, which should drive down interest rates. The increase in spreads we uncover, in turn, should be driven by out-of-state banks, since these were able to acquire or erect branching networks following IBBEA implementation, decreasing the need to use loan syndication to geographically diversify their loan portfolios. This builds on the insight of Favara & Imbs (2015) that out-of-state, but not in-state banks increased their branch presence following the IBBEA.

We test these predictions in columns (6) and (7) by interacting our treatment dummy *Deregulated* with a dummy for out-of-state banks, which we define as commercial banks *without* a branch in the borrower state in the pre-reform year. The approach has the important advantage that all lenders in the sample are, at some point, out-of-state banks.

To illustrate, a bank that is classified as "out-of-state" in New York will be an "in-state" bank in Utah if it had a local presence there before IBBEA. As a result, loans issued by the two types of commercial banks are observationally equivalent (see figure 5).

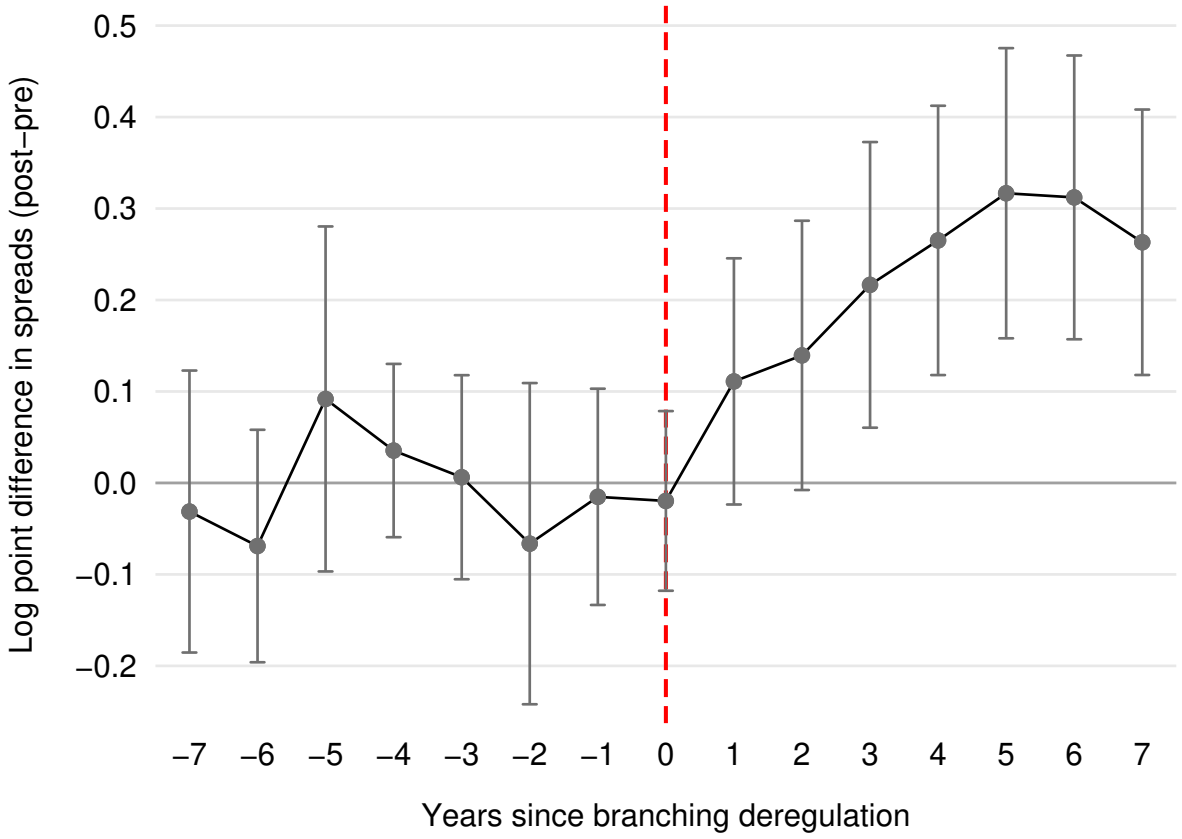
The interacted coefficients for both estimations are positive and highly statistically significant. Again, the *Deregulated* dummy itself turns negative, suggesting that, if anything, spreads decreased for lenders with local branch presences prior to IBBEA implementation. The effect for out-of-state banks, however, was a positive increase in loan rates of $-0.107 + 0.204 = 0.097$, or 9.7%. This effect becomes much larger if we allow for $borrower \times year$ and $bank \times year$ fixed effects in column (7), which absorbs all time-varying demand or risk factors. This result is consistent with a substitution effect of syndicated for bilateral loans by out-of-state lenders.

A potential concern at this point may be that state-level trends in spreads may have already been different prior to IBBEA implementation. In technical terms, this would constitute a violation of the parallel trends assumption required for causal inference from difference-in-differences estimates. Such pre-existing trends could be the result, for example, of the process of *intrastate* branching and interstate *banking* deregulation that had been unfolding starting in the 1970s (see e.g. Kroszner & Strahan, 2014).

Figure 2 investigates this possibility for syndicated vs. bilateral loans. We plot the estimates for the interaction of the syndicated loan dummy with year dummies around the deregulation date. In particular, we consider seven years before and after a state lifted its first branching restriction. The results suggest that there was no discernible trend in interest rates *before* interstate deregulation and that the positive effect we uncovered in table 2 developed relatively quickly over a few years after implementation, in line with previous evidence on branching deregulation (e.g. Beck et al., 2010; Chava et al., 2013).[22]

[22] A small lag in the effect should be expected because market entry takes time. Neither identifying possible new locations for new branches nor acquiring and integrating existing banks can be organized quickly. In anti-merger antitrust lawsuits, the US Department of Justice and the Federal Trade Commission usually mention two years as the minimum time it takes to enter markets in most industries.

Figure 2: IBBEA Implementation and the Spreads of Syndicated vs. Bilateral Loans



This figure plots the coefficient estimates of $\hat{\beta}_2^h$ of the regression $Interest\,Rate_f = \sum_{h=-7}^{7} \beta_1^h Deregulated_{st} + \sum_{h=-7}^{7} \beta_2^h Deregulated_{st} \times Exposure_f + \beta_3 Exposure_f + \gamma\, Borrower\,Controls_{i,t-1} + \delta Contract\,Controls_f + \alpha_i + \alpha_b + \alpha_t + \varepsilon_f$, where $\sum_{h=-7}^{7} D_{st}$ is a set of dummy variables for the 7 years before to 7 years after the date of IBBEA implementation. Confidence intervals are based on the 90% significance level with standard errors clustered on the state-level. We exclude facilities that are most likely structured for institutional investors, as in column (9) of table 12 in the online appendix.

We also use a regression framework to test for preexisting trends. Here, we differentiate between the time periods immediately before and after the reform as well as long-term effects. Table 3 plots the estimates from this exercise. In column (1), we see that spreads in the period immediately before state deregulation (t-4 to t-1) did *not* change compared to before: the coefficient of 0.029 is small and clearly statistically insignificant. Spread did change, however, immediately after IBBEA implementation. The coefficients of 0.095 and 0.114 for the post periods are considerably larger and statistically significant at the 5% and 10% level, respectively.

In columns (2) through (4), we find a similar pattern for the interaction terms of *Deregulated* with syndicated loans, affected lenders, and out-of-state commercial banks. These results are easily summarized by F-tests for the equality of coefficients between the time periods, reported at the bottom of the table.[23] In all regressions, the tests imply that the coefficients for the period of more than 5 years before IBBEA implementation ($Deregulated(<= t-5)$) are *not* statistically different from those for the period immediately before the reform ($Deregulated(t-4;t-1)$). This is consistent with the absence of preexisting trends, irrespective of potential differences in the *level* of interest rates between loan types. When we test for the equality of coefficients for the pre vs. post-implementation periods, the null hypothesis of no difference is strongly rejected, with values for the F-statistic between 4.5 and 27. Taken together, this suggests the discount on the spreads of syndicated loans disappeared after branching deregulation, but not before.

What do our results imply about the lending conditions of corporations post-IBBEA? At first glance, it might appear puzzling that the interest rate spreads of large borrowers with access to capital markets should increase because of changes to local banking regulation. However, our sample statistics suggest that even after branching deregulation, syndicated loans still remained considerably cheaper than bilateral contracts. Consider,

[23] Note that, in column (1), this test would be equivalent to a t-test of statistical significance. We thus only report the F-statistic for the interaction terms of interest in columns (2) through (4) to aid readability.

Table 3: Parallel Trends in Spreads Before Branching Deregulation

		Interaction Variable:		
	Baseline	Syndicated Loan	Affected Lender	Out-of-State Bank
	(1)	(2)	(3)	(4)
Deregulated (<= $t-5$)	–	–	–	–
Deregulated ($t-4;t-1$)	0.029	0.017	0.085	0.047
	(0.023)	(0.038)	(0.108)	(0.083)
Deregulated ($t;t+3$)	0.095**	-0.005	-0.045	0.004
	(0.040)	(0.048)	(0.111)	(0.123)
Deregulated (>= $t+4$)	0.114*	-0.105	-0.031	-0.039
	(0.061)	(0.064)	(0.117)	(0.128)
Deregulated (<= $t-5$) × Interaction Variable		-0.163***	–	-0.098
		(0.038)		(0.088)
Deregulated ($t-4;t-1$) × Interaction Variable		-0.159***	-0.048	-0.141
		(0.031)	(0.103)	(0.094)
Deregulated ($t;t+3$) × Interaction Variable		-0.055*	0.165*	0.028
		(0.032)	(0.100)	(0.040)
Deregulated (>= $t+4$) × Interaction Variable		0.069***	0.164†	0.112***
		(0.026)	(0.100)	(0.042)
Loan controls	Yes	Yes	Yes	Yes
Borrower controls	Yes	Yes	Yes	Yes
Borrower FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Only syndicated loans			✓	✓
Only commercial banks				✓
Observations	13,437	13,437	9,596	3,134
Adjusted R^2	0.848	0.849	0.861	0.895
H_0 : D (<= $t-5$) × Interaction Variable = D ($t-4;t-1$) × Interaction Variable		0.01	0.21	0.28
H_0 : D ($\sum$ Pre) × Interaction Variable = D ($\sum$ Post) × Interaction Variable		27.14***	7.94***	4.56**

This table reports the results from regressions of interest rates spreads (in natural logarithm) on *Deregulated*, a dummy that equals 1 if a state has lifted one or more branching restrictions. The excluded category is *Deregulated (<=t-5)*; the interaction with "Affected Lender" in column (4) is further absorbed by the bank fixed effects. See the text for details. The bottom two columns report the F-statistics for testing for the equality of the indicated coefficients; insignificant values indicate no difference. Robust standard errors (in parentheses) are clustered by states, with ***, **, and * indicating statistical significance at the 1%, 5%, and 10% levels. † indicates a single coefficient with a p-value equal to 0.11.

by way of example, a typical loan facility in our sample: a secured revolving loan with a five year maturity. The data indicates that the unconditional average spread on such a contract increased from approximately 180 to 200 basis points but decreased from 310 to 300 basis points for bilateral loans. This suggests a decreased discount for syndicated loans, rather than an increase in borrowing costs per se. The *average* interest rate increase also reflects the sample composition: 80% of the loan contracts we observe are syndicated, compared to around 50% of US corporate loans.

Another illustration of the estimated magnitudes are the differences in *basis points* after controlling for our baseline controls (as in equation 1). This exercise suggests that spreads increased almost exclusively for syndicated loans issued by out-of-state commercial banks. Even after controlling for fundamentals, this increase is sizeable: around 11 basis points for the same borrower. The spreads for bilateral loans went down throughout, consistent with more competition, as did those for syndicated loans issued by lenders other than commercial banks. The only other interest rate increase in the data is for syndicated loans issued by in-state commercial banks, on the order of 3.5 basis points. This is likely explained by the fact that all banks are an in-state bank somewhere, and are thus subject to spillover effects; competition from out-of-state lenders may have further heightened in-state banks' willingness to underwrite loans by themselves, leading to a decline in syndicated loan supply. Most importantly, the effect on in-state banks is much smaller than that on out-of-state banks. The increased rates we observe in the data should thus be interpreted as a reallocation from syndicated to sole lender loans, rather than an increase in the cost of credit.

Taken together, we find that the effect of branching deregulation increases with exposure to the reforms. Importantly, our results are fully driven by affected lenders and syndicated loans, in line with our predictions. If anything, our estimates imply a *decrease* in spreads for bilateral loans. These findings are consistent with the idea that syndication can be a substitute for direct lending through local branches to achieve geographical

diversification.

3.2 Bank-Level Regressions

Up to this point, we have focused on the effect of interstate branching on prices, documenting that spreads increased for syndicated loans but decreased for bilateral contracts. In this section, we provide some evidence that banks also reshuffled their loan portfolios and replaced at least some syndicated with bilateral loans.

More specifically, we run an analysis on the bank-state-year level, which allows us to absorb a full set of *bank* × *state*, *bank* × *year*, and *state* × *year* dummies. By way of example, this means comparing borrowers in Alabama in 1999 with other Bank of America customers in 1999, depending on whether the state was deregulated and the bank exposed to the reform changes. In particular, our substitution hypothesis implies that out-of-state banks without local branch presence prior to the state-level IBBEA reforms used loan syndication to diversify geographical credit risk. After interstate branching was allowed, these institutions could enter other state markets and open or acquire branches, which allowed for the issuance of bilateral loans.

More formally, we regress loan issuance by a given bank in a given state and year on the *Deregulated* dummy, interacted with a dummy for commercial banks with a local branch presence prior to the reform ("out-of-state bank"):

$$\begin{aligned} LoanIssuance_{bst} =& \beta_1 Deregulated_{st} + \beta_2 Deregulated_{st} \times Out-of-State\ Bank_{bs} \\ &+ \beta_3 Out-of-State\ Bank_{bs} + \alpha_{bt} + \alpha_{st} + \varepsilon_{bst}, \end{aligned} \tag{3}$$

where b indexes banks, s states, and t years. *LoanIssuance* is the natural logarithm of loan issuance volumes, referring either to syndicated or bilateral loans.[24] $Out-of-$

[24] We assign zero issuance to all bank-state-year observations between the first and last loan a bank has in a state for which we do not have any loan data.

Table 4: The Effect of Deregulation on Banks' Loan Issuance, by State and Year

	Syndicated Loan Volume			Bilateral Loan Volume		
Interaction Variables:		Out-of-State Bank (Pre-Reform)			Out-of-State Bank (Pre-Reform)	
	(1)	(2)	(3)	(4)	(5)	(6)
Deregulated	-0.299***	0.690***		-0.051	-1.159***	
	(0.116)	(0.277)		(0.055)	(0.255)	
Deregulated × Interaction Variable		-1.181***	-0.598**		0.994***	0.526**
		(0.238)	(0.259)		(0.244)	(0.245)
Interaction Variable		-1.156***	-1.543***		-1.570***	-1.296***
		(0.195)	(0.206)		(0.268)	(0.276)
Bank FE	Yes	Yes	–	Yes	Yes	–
State FE	Yes	Yes	–	Yes	Yes	–
Year FE	Yes	Yes	–	Yes	Yes	–
Bank × Year FE			Yes			Yes
State × Year FE			Yes			Yes
Observations	50,727	13,751	13,462	50,727	13,751	13,462
R^2	0.338	0.307	0.528	0.224	0.221	0.357

This table reports the impact of interstate bank branch deregulation on the volume of loans issued by banks in a particular state and year. *Deregulated* is a dummy that equals 1 if a state has lifted one or more branching restrictions. *Out-of-State Bank* is a dummy equal to 1 if a bank has no branch presence in a state prior to IBBEA implementation. Robust standard errors (in parentheses) are clustered on the State × Bank level, with ***, **, and * indicating statistical significance at the 1%, 5%, and 10% levels.

$State\ Bank_{bs}$ is a dummy for "out-of-state banks" without local branch presence before the reform, which should drive the substitution effect we outlined above. In the most stringent specification, our fixed effects absorb time-varying shocks to banks and individual states (e.g. to local demand or bank health). Standard errors are clustered on the bank-state level.

The results in table 4 illustrate that, consistent with our hypothesis, syndicated loan issuance contracted and bilateral volume expanded for banks that were not present before the deregulation but were able to enter ex-post. In column (1), we show that interstate branching reduced average syndicated loan issuance on the bank-level. The estimate of −0.299 implies an average decrease in issuance by about 30 log points. Next, we analyze the impact on commercial banks depending on whether they had a local branch presence prior to IBBEA implementation, implying that they could lend directly to local customers

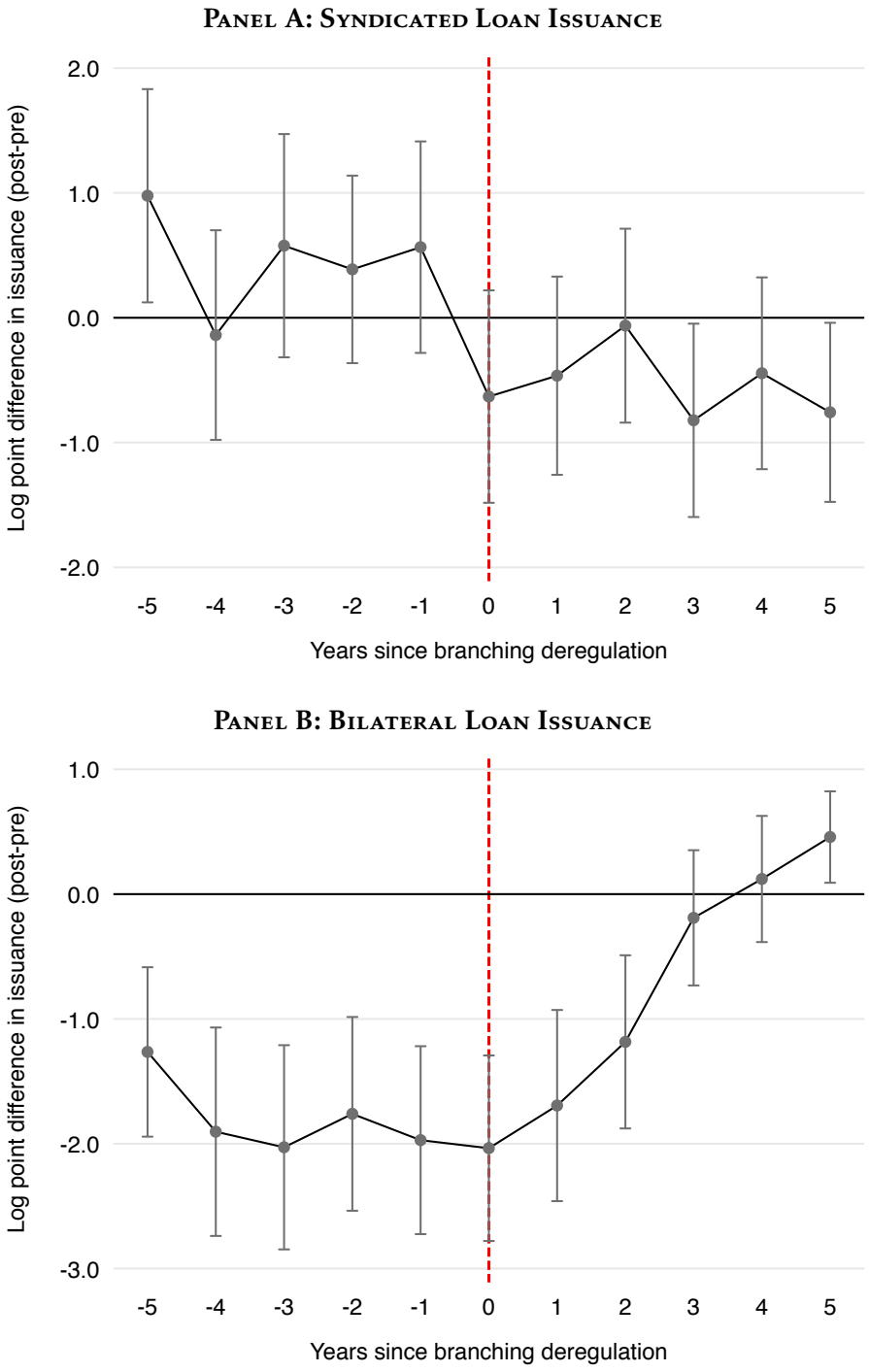
through their branches. The coefficients in column (2) indicate a large effect on out-of-state banks: branching deregulation increased syndicated lending by 69 log points for in-state banks under competitive pressure but *decreased* it by 49.1 log points for out-of-state banks ($-1.181 + 0.690 = 0.491$). The interaction term is still statistically significant when we include interacted fixed effects in column (3), implying that it is not driven by changes in demand or aggregate bank-level shocks. This aligns with previous evidence that lifting restrictions increased credit supply. For syndicated loans, however, it suggests a contraction for lending by out-of-state banks, which could lend to local markets via their expanded branching networks post-IBBEA.

To get a more complete picture, we re-run the analysis using *bilateral* loan issuance as a dependent variable in columns (4) through (6). On average, we do not find a statistically significant effect in column (4). However, whether banks had a branching presence prior to a state implementing the IBBEA clearly matteres, as shown by the highly significant coefficients in column (5). In particular, the coefficient on $Deregulated$ itself is negative (-1.159), but it is positive and of similar magnitude for the interaction with the out-of-state bank dummy (0.994). This shows that banks reallocated their bilateral loan issuance from states where they were already present before the IBBEA to newly opened states, where the overall impact was likely small. In column (6), we replicate this result using a much more stringent set of fixed effects.

These results are also confirmed by the graphical evidence in figure 3, where we allow for a dynamic effect of deregulation around the implementation year. More precisely, we rerun equation 3 but replace $Deregulated_{st}$ with a set of dummy variables for the years around a state's IBBEA implementation. On impact, but not before, out-of-state banks decreased their syndicated loan issuance in deregulated states and sharply increased their bilateral lending.

Overall, the results presented here increase our confidence in the idea of a "substitution channel" triggered by syndication becoming a less profitable way to diversify geo-

Figure 3: The Effect of Deregulation on Banks' Loan Issuance, by State and Year



This figures provides graphical evidence for changes in syndicated and bilateral loan issuance around a state's implementation of the IBBEA. We plot the $\hat{\beta}_2^h$ coefficients of regressions given by $Loan\,Issuance_{bst} = \sum_{h=-5}^{5} \beta_1^h Deregulated_{st} + \sum_{h=-5}^{5} \beta_2^h Deregulated_{st} \times Out-of-State\,Bank_{bs} + \beta_3 Out-of-State\,Bank_{bs} + \alpha_s + \alpha_b + \alpha_t + \varepsilon_{bst}$, where h indexes the year relative to deregulation. Panel A plots the results for syndicated loan issuance, Panel B those for bilateral loan issuance (both measured as the natural logarithm of 1 + Loan Volume).

graphical credit risk, leading banks to a reshuffle their loan portfolios.

3.3 State-Level Regressions

The results in the previous sections reveal that interstate branching deregulation was followed by shift from syndicated to bilateral lending. But were these effects large enough to matter at the state level as well? In this section, we provide some evidence based on cross-sectional exposure to the IBBEA induced by the degree of local market concentration prior to the reform. While we cannot rule out that unobserved time-varying state- or bank-level factors partially drive these results, they are useful for thinking about the aggregate effects of banking deregulation.

The IBBEA is widely considered to be a watershed event in American banking history due to its impact on bank expansion across state borders. Favara & Imbs (2015) show that interstate branching deregulation worked as intended: out-of-state banks drastically increased their local branching presences and market shares, with little impact on the branch networks of existing in-state players. Intuitively, one would expect that the IBBEA had a larger impact on states with lower initial competition among banks, an insight that Black & Strahan (2002) and Cetorelli & Strahan (2006) find to be true for earlier deregulatory episodes in the US.

Panel A of figure 4 shows that the same pattern holds for interstate branching deregulation, where we plot a state's Herfindahl index (HHI) of deposits in 1994 (before the IBBEA) against the log-change in the number of branches after its implementation.[25] The data suggest that the branching effect uncovered by Favara & Imbs (2015) strongly interacts with the pre-reform HHI: after deregulation, banks opened considerably more branches in states that previously had more concentrated markets. In figure 8 in the online appendix, we show that a similar pattern holds for changes to competition post-

[25]These graphs omit Hawaii and Rhode Island, which are clear outliers. The figures look very similar when these are included. Since the number of branches is a highly persistent variable, the results are also not sensitive to the exact time horizon.

IBBEA. These correlations also persist in simple OLS regressions (table 11).

Panel B next plots the evolution of syndicated loan issuance around IBBEA implementation at the state level. We scale issuance volumes by state GDP and show median values plus 90% confidence intervals. This reveals a striking pattern. Before deregulation, there was a clear upward trend in total syndicated loan issuance. After IBBEA implementation, however, syndicated lending volume stalled and then decreased as a percentage of GDP. This is consistent with previous findings in Carey & Nini (2007) and Gadanecz (2004), who show that the US syndicated loan market contracted after 1997, by which point most states had lifted at least some branching restrictions.

We use these insights to sharpen the identification strategy first used in Rice & Strahan (2010) by conditioning the deregulation effect on a state's market concentration in 1994, i.e. before states implemented the reform.[26] As we discuss in section 3.4 below, deregulation timing appears to be largely orthogonal to a state's pre-reform HHI. This gives us plausibly exogenous variation across states in the impact of interstate branching deregulation on the syndicated loan market.

More precisely, we run state-level panel regressions with a full set of state and year fixed effects of the following form:

$$\begin{aligned} Loans\,Issuance_{st} =& \beta_1 Deregulated_{st} + \beta_2 Deregulated_{st} \times HHI\ (1994)_s \\ &+ \gamma\, State\,Controls_{s,t-1} + \alpha_s + \alpha_t + \varepsilon_{st}, \end{aligned} \tag{4}$$

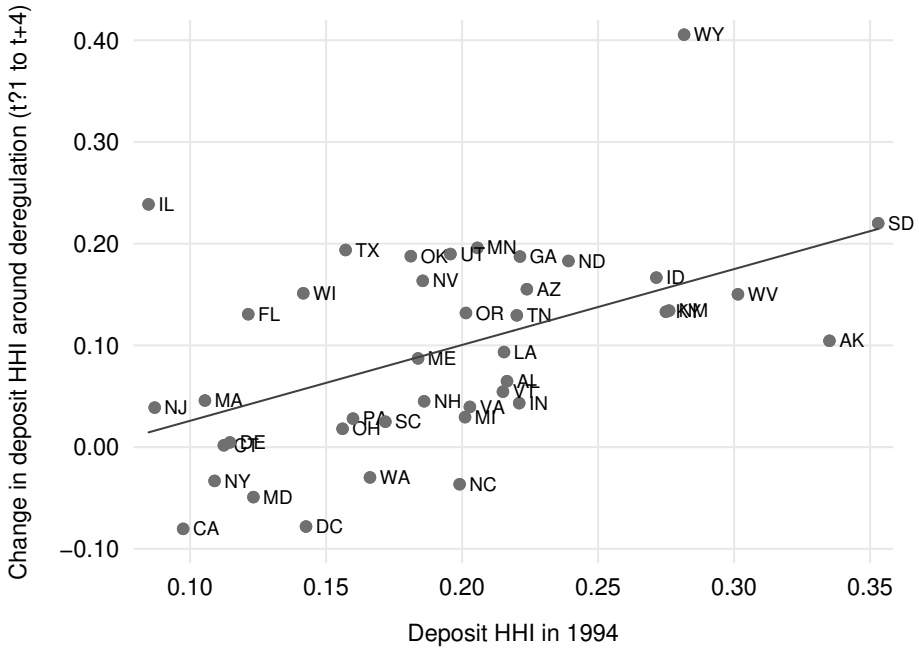
where α_s and α_t are state and year dummies, respectively. Note that $HHI\ (1994)_s$ by itself is absorbed by the state fixed effects. In some regressions, we also include the state-level controls real GSP per capita and house price growth. $Loan\,Issuance_{st}$ is either the ratio of aggregate syndicated loan volume or bilateral loan volume, scaled by state GDP.[27]

[26] Alaska is the only state that lifted any branching restriction contemporaneously with the passing of the IBBEA in 1994.

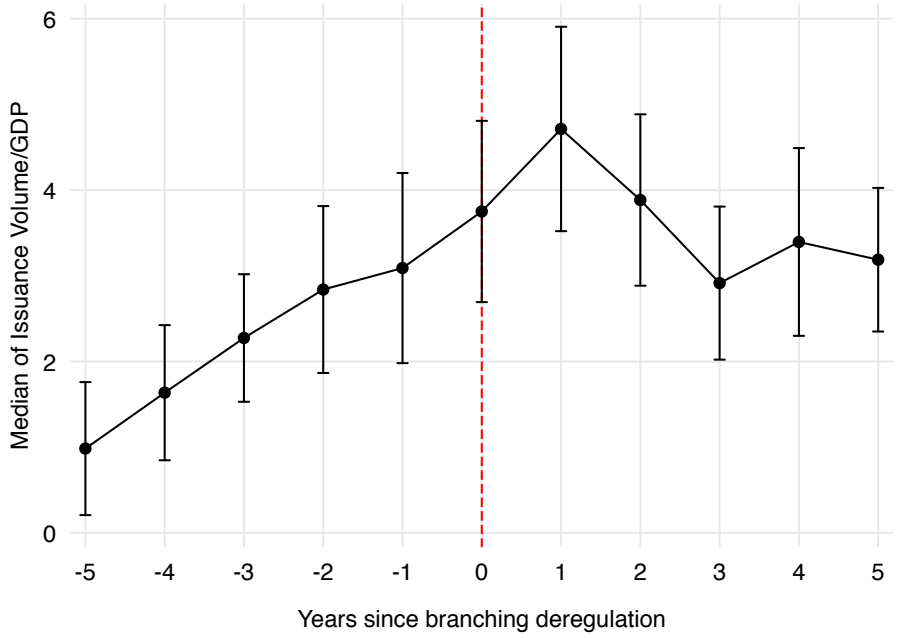
[27] Note that our results are not driven by an endogenous response of state GDP to deregulation; our results are qualitatively similar if we scale syndicated loan issuance over total C&I loans from the FDIC.

Figure 4: Pre-Reform HHI, IBBEA Implementation, and Syndicated Loan Issuance

Panel A: Change in log(1+branches), t-1 to t+4



Panel B: Syndicated Loan Issuance Volumes/GDP



Panel A plots a state's deposit HHI in 1994 (deposit-weighted county average) against the post-deregulation change in the log number of branches (we omit the outliers Hawaii and Rhode Island). Panel B plots the median and 90% confidence intervals of total state-level syndicated loan issuance in the Dealscan database around the first date a state lifted any of the branching restrictions identified in Rice & Strahan (2010), scaled by state GDP.

Standard errors are clustered on the state-level.

Table 5 plots the results of this exercise. In columns (1) to (3), we start by testing the impact of interstate branching on syndicated loan issuance, scaled over GSP. Consistent with the time series pattern, the interaction term $Deregulated \times HHI$ (1994) in column (2) has a *negative* estimated coefficient of −12.948, which is highly statistically significant. This suggests that total yearly syndicated loan issuance *decreased* in states that were particularly affected by the IBBEA; this result is also robust to including the growth in house prices and real GDP per capita as control variables (column (3)). To illustrate the implied magnitudes, consider the states with the highest and lowest pre-reform concentration: South Dakota and Illinois, with a deposit HHI of 0.353 and 0.085, respectively. Our estimates in column (2) imply that the more competitive Illinois saw an *increase* in the ratio of syndicated lending to GDP by $1.988 - 12.948 \times 0.085 \approx 0.891$. The syndicated loan ratio of South Dakota, in contrast, is estimated to drop sharply, by approximately 2.58. Comparing the least and most concentrated states around deregulation thus implies a quantitatively large contraction in syndicated loan issuance/GDP of 1.48, around 53% of the pre-reform average ratio of 2.85.

In columns (4) through (6), we find the opposite effect for bilateral loans. States that had little competition before the IBBEA saw disproportionate increases in direct lending post-deregulation. How does this lending increase compare to the drop in syndication? Consider again Illinois and South Dakota as examples at the opposite ends of market concentration prior to the reform. The estimates in column (5) imply that the ratio of bilateral loan issuance to GDP increased by $-0.492 + 2.400 \times 0.268 \approx 0.152$ in highly concentrated South Dakota, compared to Illinois. Because the pre-reform bilateral lending ratio stood much lower at 0.253, this suggests an increase in credit supply of around 60%.

These results are consistent with the result in Rice & Strahan (2010) that interstate branching eased credit constraints for small borrowers taking out direct loans. At the same time, our findings suggest a relatively small *overall* effect on credit volumes in per-

Table 5: Branching Deregulation and State-Level Loan Issuance

	Syndicated Loan Volume			Bilateral Loan Volume		
	(1)	(2)	(3)	(4)	(5)	(6)
Deregulated	-0.524	1.988**	1.735*	-0.027	-0.492**	-0.479**
	(0.652)	(0.979)	(0.954)	(0.072)	(0.201)	(0.217)
Deregulated × HHI (1994)		-12.948***	-11.008***		2.400***	2.366**
		(3.464)	(3.263)		(0.857)	(0.924)
State Controls			Yes			Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,071	1,071	1,020	1,071	1,071	1,020
Adjusted R^2	0.597	0.607	0.626	0.350	0.378	0.391

This table reports results from state-level regressions of syndicated and bilateral loan issuance volumes (from Dealscan) on a branching deregulation dummy, interacted with a state's market concentration in 1994. Loan volumes are scaled by state GDP. State controls are real GSP growth and house price growth. All regressions include state and year fixed effects. Robust standard errors (in parentheses) are clustered by state. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level.

centage terms: the approximate 53% reduction in syndicated volumes was only marginally offset by the 60% increase in bilateral loans. However, these outcomes are subject to much stronger identifying assumptions than our loan- and bank-level results and should thus be interpreted with caution. They do, however, suggest that even on the aggregate state level, allowing banks to build branching networks led to a reshuffling of loan portfolios from syndicated to bilateral contracts.

Again, there may be some concern that we are not picking up a change in bank incentives but rather a decrease in borrower demand for syndicated loans. This is counterfactual to the results on credit pricing. The results we present in section 3 show that IBBEA implementation was accompanied by *increases* in spreads, which suggests either *higher* credit demand, inconsistent with the trend in issuance, or *lower* credit supply, consistent with a substitution effect for bilateral lending. Taken together, the evidence presented here suggests that allowing interstate branching led to a substitution of syndicated for bilateral loan contracts, resulting in lower syndicated loan supply.

3.4 Threats to Identification

The results in the previous sections are consistent with the interpretation that branching deregulation led to a substitution effect from syndicated loans towards sole lender contracts. There are, however, a few threats to our identification strategy that open the door to alternative explanations. We discuss these below.

Loan-Level Correlates

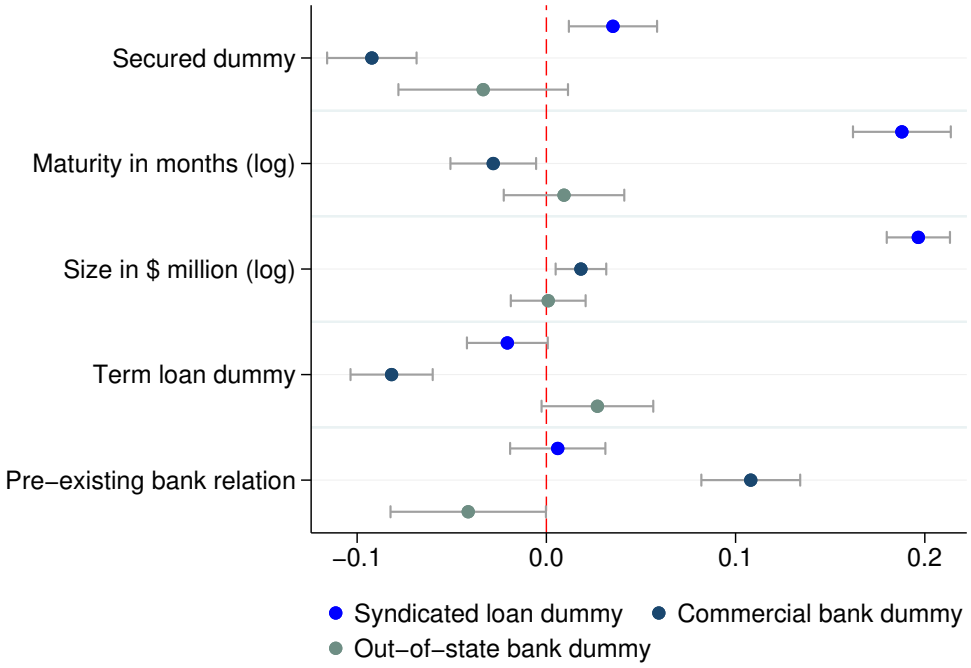
One concern with our loan-level results may be that our *Exposure* variables are not randomly assigned and thus capture deregulation effects unrelated to syndication. For example, banking sector changes induced by the IBBEA could have a differential effect across different types of loans, independent of the substitution hypothesis we are testing. Because our results hold with a full set of $borrower \times year$ and $bank \times year$ fixed effects, any such effect would have to work through an interaction of state-level deregulation with correlates of the *Exposure* measure.

Figure 5 investigates contract-level correlates of our exposure variables. More precisely, we plot standardized coefficients from regressing our loan-level control variables on the dummies for syndicated loans, commercial bank lenders, and out-of-state commercial banks. To make loans comparable, we control for borrower fundamentals and year dummies.[28]

As expected, the out-of-state bank dummy is not significantly correlated with any characteristic. The most obvious pattern that emerges is that syndicated loans are considerably larger than bilateral contracts and carry longer maturities, with a slightly higher likelihood of collateralization. Consistent with the existence of benefits to banking relationships, commercial banks do more repeat-lending with lower collateralization; their contracts are also less likely to be term loans (i.e. more likely to be lines of credit). What matters is that, across the different exposure measures, we find *no* systematic correlation

[28] The pattern is very similar without these control variables (available upon request).

Figure 5: Cross-Sectional Correlates of Exposure Variables



This figure plots the standardized estimated coefficients from regressing the loan characteristics on the left-hand side on the exposure variables (syndicated loans, commercial bank dummy, out-of-state bank dummy), after controlling for the vector of borrower fundamentals and year dummies. We also plot the 95% confidence intervals based on robust standard errors clustered by borrower.

with these other loan terms. Taken together with our results on preexisting trends in section 3.1, it thus seems unlikely that *unobservable* loan characteristics are behind our findings.

In a further test, we access Dealscan amendment files that record renegotiations resulting in interest spread changes *for the same loan*. This allows us to run regressions of *changes* in spreads for the same loan in a sample of amendment events with loan facility fixed effects, holding other loan terms constant.[29] Table 18 in the online appendix reports the results. Despite the small number of spread amendments, we find positive coefficients for all interacted coefficients as before, though these somtimes lack statistical precision. Nevertheless, these results are further evidence that unobserved loan-level differences are unlikely to drive our findings.

Endogenous Borrower Composition

The most obvious explanation for observing higher spreads after deregulation is an increase in borrower risk. Because our loan-level estimations include a full set of $borrower \times year$ and $bank \times year$ fixed effects, differences in ex-ante or realized firm-level risk cannot explain the increase. A change in risk may, however, play a role by interacting with our $Exposure$ variables.

We begin to address this by either including additional controls or altering the estimation sample. In column (3) of table 12 in the online appendix, we show that our results also hold when including a control for numerical credit ratings. An increase in risk may also result from small firms expanding their market share due to better financing terms following deregulation (Rice & Strahan, 2010). Yet, controlling for a borrower's market share in industry sales does not alter our estimates (column 4). In column (9) of

[29] Of course, this approach comes with another challenge: while we "buy" the advantage of taking out differences between facilities *at issuance*, amendments in spreads may coincide with other contract changes. However, such within-contract amendments are of smaller magnitude than between-contract differences and we can control for these using a vector of the available variables.

table 12, we also accommodate the concern that our findings may have something to do with institutional investor activity in syndicated loans by dropping all "Term Loan" and "Term Loan B" facilities. This yields a very similar point estimate of 0.052 (still highly significant).

It is also possible that safer borrowers opted out of the syndicated loan market after branching deregulation, which could affect the risk profile of *Exposed* loans. The intuition behind this concern is developed in models such as Bris & Welch (2005), where the "best firms" self-select into more concentrated credit contracts; bilateral loans would be the most extreme example. Figure 7 in the online appendix shows this is not a valid concern in the aggregate: the share of loans issued by investment-grade borrowers was approximately stable around the IBBEA and the share of rated borrowers slightly increased. This implies that – if anything – the quality of borrowers taking out syndicated loans *increased* with deregulation. More importantly, we find no differential selection of firms based on our *Exposure* measures: borrowers taking out "treated" loans were not riskier post-deregulation compared to before (see table 14). Taken together, these results allow us to conclude that changes in borrower risk are unlikely to drive our results.

Changes in Credit Demand

An alternative explanation for an increased syndicated loan rates would be higher credit demand. Work by Jayaratne & Strahan (1996) and Huang (2008), among others, suggests that other episodes of US banking deregulation have been associated with higher economic growth rates, which may increase the demand for credit. This concern is accommodated by our loan-level regressions with $borrower \times year$ fixed effects, which hold borrower fundamentals (including demand) constant. Further, in section 3.3, we clearly show that *syndicated* loan issuance contracted after branching deregulation. At the same time, we find that *bilateral* loan issuance grew post-IBBEA. The substitution between syndicated and bilateral loans thus cannot be explained by higher overall demand for credit.

Endogenous Deregulation Timing

Another potential threat to our identification strategy is that interstate branching deregulation may be endogenous to the size of the local syndicated loan market. Large lenders engaged in syndicated lending might have lobbied state legislators to delay deregulation in order to reap rents serving their client base. A decrease in syndicated loan activity could thus also reflect large banks lobbying less in these states due to lower future growth opportunities. While it is not possible to test whether the deregulation timing is endogenous, we believe this is not a concern in our setting for two reasons.

First, existing evidence in Kroszner & Strahan (1999) and Rice & Strahan (2010) suggests it was *smaller*, not larger banks that lobbied for later interstate branching deregulation. Jayaratne & Strahan (1996, 1998) further show that states did not deregulate in anticipation of future growth prospects (which may have affected syndicated and bilateral loans differently).

Second, we find no correlation between deregulation timing and the size of the syndicated loan market or the degree of local market concentration before IBBEA implementation. The results from these tests can be found in tables 9 and 10 in the online appendix. While only suggestive, they provide some evidence that our results are not driven by expectations correlated with deregulation.

3.5 Exploring Borrower Heterogeneity

Observing higher spreads post-deregulation may initially be surprising, given the evidence in Rice & Strahan (2010) that IBBEA implementation *decreased* the cost of credit for small firms. In this section, we show that differences in sample composition are key to understanding why we find differential effects on syndicated and bilateral loans. We show that even in our sample, smaller, riskier, and more opaque borrowers saw a *decrease* in loan pricing, a result that accords with Rice & Strahan (2010). An intuitive interpretation is that lifting interstate branching restrictions considerably leveled the playing field

in the US credit market by decreasing the costs of screening and monitoring these types of borrowers (D'Acunto et al., 2017). This is also consistent with the common finding in the literature on US banking deregulation that small and opaque firms were the main beneficiaries (see, among others, Black & Strahan, 2002; Kerr & Nanda, 2009; Beck et al., 2010; Chava et al., 2013).

To test how differences across borrowers matter for squaring our results with previous findings, we run regressions with an interaction term as given by equation 2, where we condition the effect of the *Deregulated* dummy on borrower characteristics. Importantly, we define these characteristics in the year *before* a state implemented the IBBEA because borrower fundamentals may themselves become endogenous afterwards. Specifically, we use borrower firm size (as measured by the log of total assets), asset tangibility, and numerical credit ratings as proxies for firms' ex-ante opaqueness. The interaction with firm-level characteristics conveniently also enables us to include $state \times year$ fixed effects to absorb other potential state-level trends.

The results on borrower heterogeneity can be found in table 6. In column (1), we introduce the interaction term with pre-reform firm size, as measured by the log of total assets. The interaction term $Deregulated \times Firm\ Size$ is positive, but the *Deregulated* dummy by itself now attracts a *negative* sign. The estimates suggest an important heterogeneity for the impact of IBBEA implementation within our sample: a firm in the 10th percentile of the size distribution saw a 12% *decrease* in spreads, while a firm in the 90th percentile saw a 17% *increase*.[30] Smaller firms thus benefited from interstate branching deregulation even within our sample, consistent with Rice & Strahan (2010), Acharya, Imbs & Sturgess (2011), and Chava et al. (2013), among others. While large firms still pay lower interest rates than small ones after deregulation, the differential between them narrows. Including a full set of $state \times year$ dummies in column (2) makes almost no

[30] We arrive at this estimate by calculating the total effect for a small firm (log assets of ≈ 4.082) as $-0.380 + 0.063 \times 4.082 \approx -0.122$. Equivalently, for a large firm with total (log) assets of ≈ 8.760, we calculate the total effect as $-0.380 + 0.063 \times 8.760 \approx 0.172$.

Table 6: Differential Effects Across Borrowers

	Interaction with pre-reform borrower characteristics:					
	Firm Size		Asset Tangibility		Credit Risk	
	(1)	(2)	(3)	(4)	(5)	(6)
Deregulated	-0.380***	–	-0.054	–	0.218	–
	(0.078)		(0.043)		(0.138)	
Dereg × Interaction Variable	0.063***	0.065***	0.148*	0.145*	-0.023**	-0.037***
	(0.012)	(0.013)	(0.078)	(0.084)	(0.012)	(0.013)
Loan Controls	Yes	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	Yes	Yes	Yes	Yes
Borrower FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	–	Yes	–	Yes	–
State × Year FE		Yes		Yes		Yes
Observations	4,407	4,323	4,403	4,319	1,861	1,754
Adjusted R^2	0.823	0.862	0.821	0.861	0.858	0.907

This table reports the impact of a interstate bank branch deregulation on interest rates of loans in the syndicated market in the US. The dependent variable is the lending rate spread from contracts included in the DealScan database. The explanatory variable is a dummy that equals 1 if a state lifts one or more branching restrictions for the first time. See the text for included control variables. Robust standard errors (in parentheses) are clustered on the State × Year level, with ***, **, and * indicating statistical significance at the 1%, 5%, and 10% levels.

difference to these estimates.

Other differences in borrower characteristics are also important mediators. Interest rate increases are concentrated in firms with higher pre-reform asset tangibility (as measured by fixed over total assets), with a negative (but imprecisely estimated) effect on firms with lower tangibility. Again, moving from the 10th to the 90th percentile, this implies a range from a 4.2% decrease ($-0.054 + 0.148 \times 0.079 \approx -0.042$)to a 4.7% increase ($-0.054 + 0.148 \times 0.684 \approx 0.047$). Interest rates thus became relatively less responsive to fixed asset holdings after IBBEA implementation. To the extent that collateral is a tool to overcome asymmetric information between lenders and borrowers, this indicates reduced financial frictions. Another important margin for banking deregulation is firm risk (Neuhann & Saidi, 2017). The estimates in column (5) imply that firms with inferior credit ratings saw lower interest rates following branching deregulation. Going from a AAA rating (equal to 1 in our rating scale) to BB+ (equal to 11 in our rating scale) implies

a sizeable difference of 20.7 log-points in the effect of branching deregulation. For safe borrowers, spreads clearly increased; risky borrowers saw a decline in rates.

Overall, our results suggest that lifting interstate branching restrictions benefited opaque borrowers, which may be more expensive to screen and monitor. While the average net effect on the spreads of syndicated loans in our sample was positive (albeit small), borrowers who are particularly subject to ex-ante difficulties in tapping credit markets did indeed see lower rates. We find that the firms in our sample most similar to those examined in Rice & Strahan (2010) were the unconditional winners.

3.6 Additional Robustness Checks

We conduct a battery of further validity checks to assess the robustness of our main results. One worry may be that – despite the evidence of no preexisting trends in section 3 – our treatment variable may pick up secular state-level trends in spreads of particular loans unrelated to deregulation. As an alternative test, we therefore conduct a placebo test by assuming that a state's IBBEA implementation occurred seven years prior to the actual deregulation year, similar to Rice & Strahan (2010). The choice of seven years is driven by the sample length: our observations start in 1987 and the first state to allow interstate branching, Alaska, implemented the IBBEA in 1994. In table 15 in the online appendix, we show the results of running our baseline regressions over the time period 1987 through 1994. All of the estimates for the placebo dummy are indistinguishable from zero, providing yet more evidence that we are capturing a causal effect of branching deregulation.

Another concern may be that including borrower fixed effects leaves us with a "biased" sample of repeat borrowers. Because the syndicated loan market is characterized by repeated interactions between borrowers and lenders, however, we lose less than 5% of observations by requiring at least two loan contracts in the sample. We also show that our results hold between (instead of within) borrowers by exchanging borrower dummies

with industry and state dummies in column (2) of 12.

The loan-level regressions we employ allow us to control for other contract determinants of spreads, such as the use of collateral. In order to alleviate concerns that these are jointly determined with interest rate spreads, we omit all loan controls in column (5) of table 12. This makes no material difference to our point estimate. Additionally, there may be a concern that our panel regressions understate standard errors, despite the conservative clustering. We address this concern by collapsing the panel into two observations per firm as suggested by Bertrand, Duflo & Mullainathan (2004). We implement this by calculating the average interest rate before and after deregulation for each firm in the full sample (weighted by loan size) and regressing it on the post-deregulation dummy. The estimates in column (6) in table 12 show that this simple regression yields a point estimate of ≈ 0.085 with a t-statistic of 6.32, suggesting that our results are unlikely to be driven by suppressed standard errors.

As described above, we establish our main results using the sample period 1987 through 2007, excluding the Great Recession. In table 16 in the online appendix, we rerun all regressions in the full sample period available until 2012. The results in almost all specifications are now even more precisely estimated with largely unchanged coefficients, suggesting that the increases in interest rates on large loans we have documented throughout the paper were not washed away by the financial crisis of 2007-08. We also repeat our main estimation with the original Rice & Strahan (2010) index; column (7) in table 12 shows this has no substantial bearing on our result.

As a last exercise, we address the concern that large states are overrepresented in our sample and thus may drive the results. We run the baseline regression in equation 1 and exclude all individual states in turn. Figure 9 plots the coefficient estimates of all of these regressions and compares them to the baseline result presented in table 2. With the exception of a single outlier, excluding individual states makes little difference to the point estimate. Excluding Texas leads to a substantially higher coefficient (by almost

50%). If anything, this suggests our sample composition actually *understates* the IBBEA's impact on syndicated loan spreads.

4 Conclusion

In this paper, we show that the wave of state-level deregulation following the passage of the Riegle-Neal Interstate Banking and Branching Efficiency Act of 1994 (IBBEA) had substantial effects on the syndicated loan market. We show that branching deregulation was associated with a substitution of syndicated for bilateral lending, accompanied by a narrower interest rate differential between (cheaper) syndicated and (more expensive) bilateral loan contracts. These findings are consistent with the interpretation that branching networks and syndication are partial substitutes for diversifying geographical credit risk.

The unique institutional setting of the IBBEA allows us to exploit cross-sectional loan-level exposure to identify the causal effect of interstate branching. In particular, we show that the substitution effect was entirely driven by out-of-state commercial banks, the only group legally affected by the reforms. Importantly, the higher lending competition expanded credit supply by other lenders, a result that aligns with previous evidence in Rice & Strahan (2010). We further find that these effects vary across borrowers: small, risky firms with few tangible assets in particular stood to benefit from branching deregulation, even in our sample of relatively large companies. We add the nuance that this likely contributed to a more level playing field between large and small borrowers in the US credit market.

Our findings suggest that abolishing geographic branching restrictions may have at least partially contributed to the declining share of the US in the global syndicated loan market starting in the mid-1990s, as documented by Carey & Nini (2007) and Gadanecz (2004). This implies that the effects of financial reforms are far from uniform and may have considerable differential impact on the pricing and allocation of credit. A promising

area of future work is to study the consequences of other types of financial reforms and how other regulatory changes affected to syndicated loan market.

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Appendix

Table 7: Variable Definitions

Variable	Definition
Loan characteristics (Dealscan)	
Size	Facility amount in million USD.
Interest rate spread	Interest rate spread, usually over LIBOR, in basis points.
Maturity	Loan maturity in months.
Secured dummy	Equal to 1 if loan is backed by collateral.
Pre-existing bank relation	Equal to 1 if a firm received a bank from the same lead bank before.
Term loan dummy	Equal to 1 if loan is a term loan.
Loan purpose	Vector of dummy variables for the different loan purposes.
Syndicated loan dummy	Equal to 1 if the loan's distribution method is not "sole lender", i.e. the loan is syndicated.
Bank characteristics (Dealscan/FDIC)	
Commercial bank dummy	Equal to 1 if the creditor's SIC code starts with 602, 0 otherwise.
Out-of-state bank dummy	Equal to 1 if a bank does not have branches in a borrower's state prior to deregulation; defined only for commercial banks.
Firm characteristics (Compustat)	
Leverage	[Long-term debt ($dltt$) + debt in current liabilities (dlc)] / Total assets (at).
Market/book ratio	[Common shares outstanding ($csho$) Price close - Annual - Calendar ($prccc$) + debt in current liabilities (dlc) + long-term debt ($dltt$)] / Total assets (at).
Total assets	Total assets (at).
ROA	Operating income before depreciation ($oibdp$) / Total assets (at).
Negative debt/CF	[Long-term debt ($dltt$) + debt in current liabilities (dlc)] / [Operating income before depreciation ($oibdp$) + Depreciation and amortization (dp). Equal to 1 for negative values.
High debt/CF	[Long-term debt ($dltt$) + debt in current liabilities (dlc)] / [Operating income before depreciation ($oibdp$) + Depreciation and amortization (dp). Equal to 1 for the fourth quartile.
Sales growth	Growth in sales/turnover (net) [($sale$ - $sale$ (t - 1)) / $sale$ (t - 1)].
Rating dummy	Equal to 1 if a firm has any rating from Standard & Poors, Fitch, Moody's, or Duffs & Phelps.
Rating	Numerical credit rating, ranging from AAA to D.
Asset tangibility	[Property, plant and equipment ($ppent$) / Total assets (at).
State characteristics	
Deregulation index	Reversed deregulation index from Rice & Strahan (2010) where 0 is most restrictive and 4 most liberalized.
Deregulated dummy	Set to 1 for the first year the reversed deregulation index is not 0.
Syndicated loan issuance/GSP	Aggregate total volume of syndicated loans in Dealscan, scaled over state GDP (GSP).
Bilateral loan issuance/GSP	Aggregate total volume of bilateral loans in Dealscan, scaled over state GDP (GSP).
C&I loan volume/GSP	Outstanding total volume of C&I loans from the FDIC, scaled over state GDP (GSP).
Pre-reform deposit HHI	The county-level Herfindahl index of deposits in the pre-deregulation year, aggregated to the state level using county deposits as weights.
Manufacturing employment share	Share of employees in the manufacturing sector (source: BEA).
House price growth	The year-on-year growth in house prices (source: Federal Housing Finance Agency).
Real GSP per capita growth	The year-on-year growth in real GSP per capita (source: BEA).

Online Appendix

Sample Comparison with Rice & Strahan (2010)

The existing evidence on the effects of IBBEA implementation on interest rates in Rice & Strahan (2010) is based on data from the Survey of Small Business Finance (SSBF). In order to interpret the results presented in this paper, we believe it is instructive to compare this data source with the matched Dealscan-Compustat sample we are using here. For illustrative purposes, we compare the 2003 edition of the SSBF with the data for 2003 in our sample.[31] As may be expected, the differences are staggering. Figure 6 visualizes these differences for firm size (as measured by total assets). The overlap of the two samples is minor. Table 8 in the appendix further summarizes the mean, median, and standard deviation of a host of other relevant borrower, loan, and lender characteristics in both samples. While the median SSBF borrower has total assets equal to 850,000 USD, the median Dealscan-Compustat borrower has 978,300,000 USD, a difference by a factor of 1,000. All Compustat firms are incorporated, and with a few exceptions publicly listed, where we define non-publicly listed as firms with no credit rating and no ticker information as in Sufi (2007). This contrasts strongly with the SSBF, where only a negligible part of borrowers has issued public equity, and about 25% of firms are not incorporated businesses.

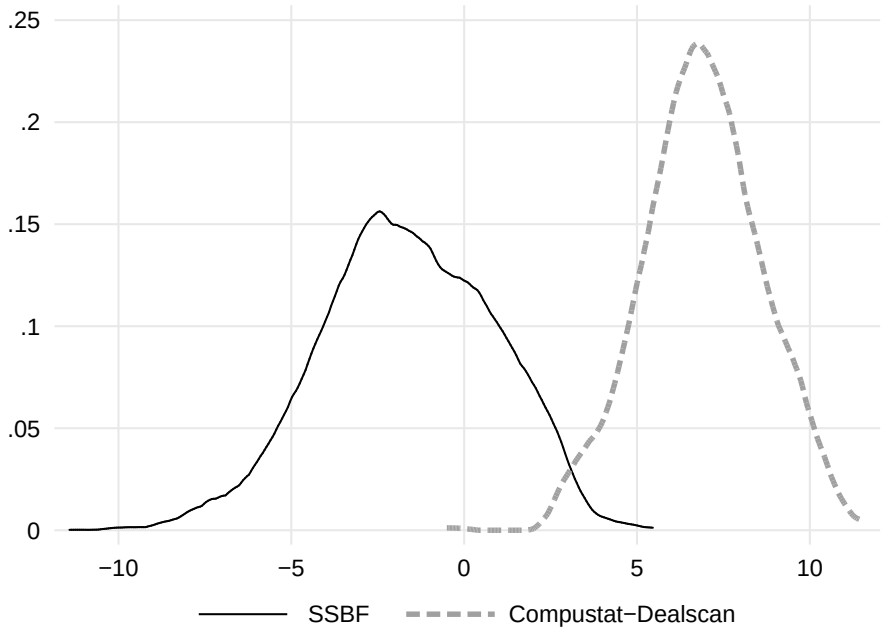
The differences, however, do not stop at borrower characteristics. The median SSBF loan is only 0.15% of that in our sample in terms of size and comes with a 50% higher interest rate, but half the maturity.[32] The main distinguishing feature of the Dealscan data is that most loans are syndicated, i.e. include multiple lenders. This is the case for about 80% of the contracts in our sample. Strikingly, *none* of the firms in the SSBF report

[31] The comparison yields the equivalent insights if we instead use the average characteristics over our whole sample or include the years 2002 and 2004 to enlarge sample size.

[32] Dealscan reports interest rates as spreads over a reference rate, usually LIBOR. We approximate actual interest rates here by adding the daily LIBOR to interest rate spreads.

loans including multiple lenders, likely because of the smaller loan size. We conclude that the sample we are using in this paper differs in many dimensions from the data used in previous efforts. As we will show below, these differences matter a great deal for the impact of the IBBEA on loan contracts.

Figure 6: Distribution of Firm Size in 2003, SSBF versus Dealscan-Compustat



This figure plots the distribution of firm size (as measured by the log of total assets) for the Survey of Small Business Finance (SSBF) and the Dealscan-Compustat estimation sample in 2003. To make the data comparable with Rice & Strahan (2010), we only include borrowers who indicate having at least one banking connection in the SSBF.

To alleviate concerns regarding endogenous deregulation timing, we regress a dummy for states that allowed interstate branching early (between 1994 and 1996) or the deregulation year on the average state-level syndicated loan market size prior to the lifting of branching restrictions. Syndicated loan market size is simply the average total volume of syndicated loans issued in a given state in the pre-reform years, which we scale over the Gross State Product (GSP).[33] The results are shown in table 9. For both the pre-reform

[33]The results are qualitatively similar when we scale over alternative variables such as total outstanding C&I loans or the number of firms or define pre-reform syndicated loan volume using only the year directly before the reform.

Table 8: Sample Comparison: DealScan-Compustat and SSBF in 2003

	SSBF 2003			DealScan-Compustat		
	Mean	Median	SD	Mean	Median	SD
Borrower characteristics						
ROA	0.714	0.116	4.850	0.133	0.125	0.101
Employees	54.157	23	74.59	17,701	4,487	40,377
Total assets (million USD)	4.43	0.85	13.89	3,764.5	978.3	8,853.7
Land/total assets	0.053	0	0.139	0.149	0.093	0.189
Depreciable/total assets	0.375	0.301	0.319	0.503	0.501	0.237
Debt/asset ratio	0.322	0.262	0.28	0.314	0.262	0.262
Borrower age (years)	18.3	16	13.1	79.5	103	36.8
Share publicly listed	0.013	0	0.113	0.943	1	0.227
Share incorporated	0.754	1	0.431	1	1	0
Loan characteristics						
Amount (million USD)	1.091	0.15	3.891	191.7	100	295.4
Interest rate	0.058	0.055	0.027	0.038	0.036	0.015
Maturity (months)	45.3	18	59.7	40.5	36	21.9
Share floating	0.553	1	0.497	1	1	0
Share credit lines	0.63	1	0.483	0.678	1	0.467
Lender characteristics						
Relationship length (years)	10.31	7	10.51	7.05	6.01	6.07
Share single lender	1	1	0	0.209	0	0.407
Share commercial banks	0.787	1	0.41	0.636	1	0.482

This table compares descriptive statistics for the sample we use in our baseline regression (model (3) from table 2) with the data from the 2003 Survey of Small Business Finance. To enable comparability, only observations from 2003 are used from our merged DealScan-Compustat file. For Compustat-DealScan, ROA is defined as oibdp/at; the number of employees excludes zero employee firms; depreciable /total assets is (ppent+intan)/at; debt/asset ratio is (dltt+dlc)/at; interest is allindrawn + LIBOR; "single lender" includes all loans that are signed by only one lead bank; "credit lines" include revolving loans; age is computed from the date of incorporation from Jay Ritter's website; for the share of publicly listed companies, private is defined as firms with missing ticker information and no credit rating as in Sufi (2007). As in Rice & Strahan (2010), statistics for SSBF only include firms which had a recent loan application and only loans and lenders which are associated with the most recent loan approved; a firm is defined as publicly listed if raised equity publicly; "credit lines" include renewables and new lines; debt includes total loans; "single lender" includes all loans that are not syndicated or signed by multiple institutions. For both files, leverage is only calculated for firms with a positive net worth.

syndicated loan volume and the HHI in 1994, the estimated coefficients are small and indistinguishable from zero; the regression models also yield tiny (pseudo) R^2 values. In the online appendix table 10, we further show that deregulation timing is uncorrelated with the market share of commercial banks or the share of risky borrowers in the syndicated loan market. Taken together, we reject the hypothesis that the timing of IBBEA implementation was influenced by a state's characteristics related to the syndicated loan market.

Table 9: Do Syndicated Loan Market Features Predict Deregulation Timing?

Dependent Variable:	Early Deregulation		Deregulation Year	
	(1)	(2)	(3)	(4)
Syndicated Loan Vol./GSP	1.658		0.624	
	(9.393)		(10.394)	
Deposit HHI (1994)		0.989		-1.442
		(1.132)		(1.287)
Estimation Method	Probit	Probit	OLS	OLS
Observations	43	43	43	43
(Pseudo) R^2	0.002	0.012	0.000	0.030

This table reports cross-sectional regressions of deregulation timing variables on measures of state-level aggregate syndicated loan volume in the year prior to IBBEA implementation and market concentration in 1994. The sample spans the 43 states included in the baseline regression (see equation (3) in table 2). *Early deregulation dummy* is 1 if a state started allowing interstate branching in the years 1994, 1995, or 1996, and 0 otherwise. *Deregulation year* is the year a state deregulated. *Syndicated loan vol./GSP* is the average syndicated loan volume in a state in the years prior to deregulation, where GSP is the Gross State Product from the BEA. The *Deposit HHI (1994)* is the deposit-weighted average county-level Herfindahl index of deposits in 1994. Robust standard errors are reported in parentheses.

Additional Tables and Figures

Table 10: Do Syndicated Loan Market Features Predict Deregulation Timing?

Dependent Variable:	Early Deregulation			Deregulation Year		
	(1)	(2)	(3)	(4)	(5)	(6)
Commercial Bank Share	-0.311			-2.218		
	(2.521)			(2.133)		
Junk Grade Borrower Share		1.311			-0.479	
		(0.944)			(1.027)	
Rated Borrower Share			-0.517			-0.031
			(1.182)			(1.296)
Estimation method	Probit	Probit	Probit	OLS	OLS	OLS
Observations	41	38	41	41	38	41
(Pseudo) R^2	0.001	0.049	0.001	0.005	0.010	0.001

This table reports cross-sectional regressions of deregulation timing variables on measures of state-level syndicated loan activity in the years prior to IBBEA implementation. The sample spans the 43 states included in the baseline regression (see equation (3) in table 2) but shrinks with data availability. *Early deregulation dummy* is 1 if a state started allowing interstate branching in the years 1994, 1995, or 1996, and 0 otherwise. *Deregulation year* is the year a state deregulated. *Commercial bank share* is the average share of commercial banks in issued syndicated loans. *Junk grade borrower share* is the share of rated borrowers with ratings of BAA+ or worse. *Rated borrower share* is the share of borrowers with a credit rating. Robust standard errors are reported in parentheses.

Table 11: First Stage Results – Pre-Reform HHI and Reform Impact

	$\Delta \ln(1 + \text{branches})$	$\Delta \text{HHI}_{t-1,t+4}$
	(1)	(2)
HHI (1994)	0.746***	-0.394***
	(0.239)	(0.116)
Observations	41	41
R^2	0.242	0.280

This table reports simple OLS regressions of the change in the log number of branches or the change in the county deposit HHI (aggregated to the state-level as deposit-weighted average) on a state's deposit HHI in 1994. We omit Hawaii and Rhode Islands, which are clear outliers; results are still highly significant when we include them. Robust standard errors are reported in parentheses.

Table 12: Loan-Level Results: Robustness

	Baseline Bank FE (1)	Industry FE (2)	Rating Control (3)	Market share Control (4)	Drop Loan Controls (5)	Collapsed Panel (6)	Continuous Index (7)	Deregulated Sample (8)	Drop Term Loans (9)
Panel A: Deregulated dummy only									
Deregulated	0.067***	0.138***	0.071***	0.064**	0.045**	0.085***	0.017**	0.048**	0.052**
	(0.025)	(0.037)	(0.024)	(0.025)	(0.022)	(0.014)	(0.008)	(0.023)	(0.022)
Panel B: Interaction with syndicated loans									
Deregulated × Exposure	0.148***	0.137***	0.153***	0.143***	0.152***	–	0.108***	0.136***	0.141***
	(0.031)	(0.032)	(0.030)	(0.033)	(0.031)		(0.027)	(0.032)	(0.035)
Panel C: Interaction with commercial bank dummy									
Deregulated × Exposure	0.184***	0.143***	0.188***	0.198***	0.182**	–	0.131***	0.212***	0.205***
	(0.071)	(0.039)	(0.073)	(0.067)	(0.072)		(0.043)	(0.069)	(0.066)
Panel D: Interaction with out-of-state bank dummy									
Deregulated × Exposure	0.215**	0.108*	0.204**	0.211**	0.232**	–	0.133*	0.215**	0.214**
	(0.099)	(0.059)	(0.088)	(0.102)	(0.102)		(0.072)	(0.099)	(0.100)
Loan controls	Yes	Yes	Yes	Yes		–	Yes	Yes	Yes
Borrower controls	Yes	Yes	Yes	Yes	Yes	–	Yes	Yes	Yes
Borrower FE	Yes		Yes	Yes	Yes	–	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	–	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	–	Yes	Yes	Yes

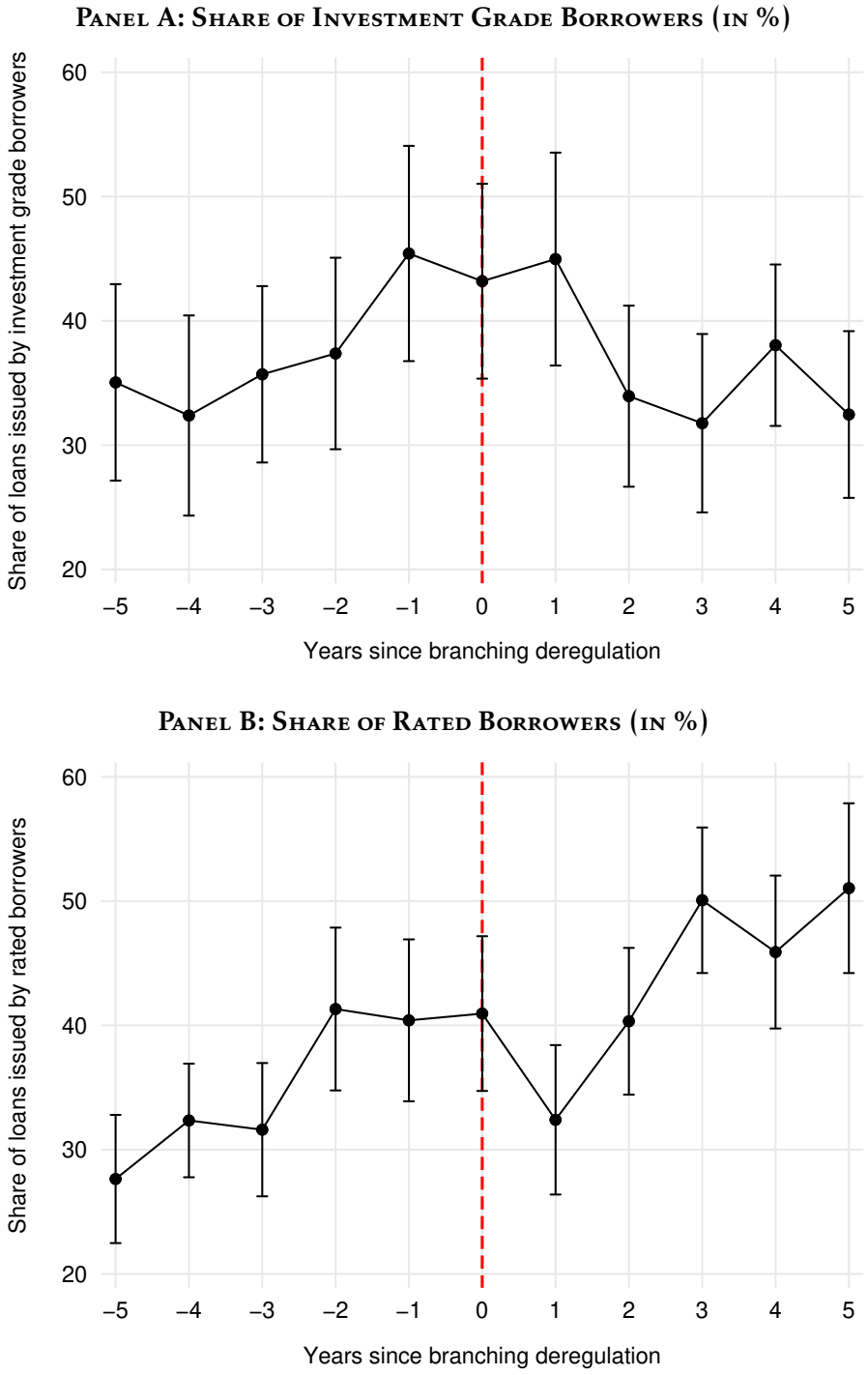
This table reports the results of regressing the interest rates spreads of syndicated loans (in natural logarithm) on *Deregulated*, a dummy that equals 1 if a state has lifted one or more branching restrictions, and interaction variables. *Exposure* refers to the variable in the panel header. Column (2) adds SIC 4-digit and state fixed effects. In column (3), we add numerical credit ratings (as dummies) as additional control variable. Column (4) adds a control variable for a firm's market share, based on Compustat data. Column (5) is estimated without loan controls. In column (6), we calculate a borrower's spreads as the weighted average spread before and after deregulation, and re-estimate the main effect on the firm-level. Column (7) replaces the *Deregulated* dummy with the Rice & Strahan (2010) deregulation index. Column (8) limits the sample to states that eventually deregulated until 2007. Column (9) excludes loans of the type "Term Loan" or "Term Loan B", which may be structured for institutional investors. The sample is restricted to syndicated loans for the regressions in panel C and to syndicated loans by commercial banks for the regressions in panel D. Robust standard errors (in parentheses) are clustered on the state-level, with ***, **, and * indicating statistical significance at the 1%, 5%, and 10% levels.

Table 13: Loan-Level Results: Robustness with Interacted Fixed Effects

	Baseline Two-Way FE (1)	Drop Loan Controls (2)	Continuous Index (3)	Deregulated Sample (4)	Drop Term Loans (5)
Panel A: Interaction with syndicated loans					
Deregulated × Exposure	0.130**	0.126**	0.073	0.120**	0.137*
	(0.051)	(0.059)	(0.047)	(0.059)	(0.074)
Panel B: Interaction with commercial bank dummy					
Deregulated × Exposure	0.248***	0.253***	0.106*	0.131	0.257*
	(0.081)	(0.086)	(0.059)	(0.149)	(0.147)
Panel C: Interaction with out-of-state bank dummy					
Deregulated × Exposure	0.609***	0.623***	0.531***	0.609***	0.691***
	(0.072)	(0.070)	(0.138)	(0.072)	(0.119)
Loan controls	Yes		Yes	Yes	Yes
Borrower × Year FE	Yes	Yes	Yes	Yes	Yes
Bank × Year FE	Yes	Yes	Yes	Yes	Yes

This table reports the results of regressing the interest rates spreads of syndicated loans (in natural logarithm) on *Deregulated*, a dummy that equals 1 if a state has lifted one or more branching restrictions, and interaction variables. *Exposure* refers to the variable in the panel header. Column (2) is estimated without loan controls. Column (3) replaces the *Deregulated* dummy with the Rice & Strahan (2010) deregulation index. Column (4) limits the sample to states that eventually deregulated until 2007. Column (5) excludes loans of the type "Term Loan" or "Term Loan B", which may be structured for institutional investors. The sample is restricted to syndicated loans for the regressions in panel B and to syndicated loans by commercial banks for the regressions in panel C. Robust standard errors (in parentheses) are clustered on the state-level, with ***, **, and * indicating statistical significance at the 1%, 5%, and 10% levels.

Figure 7: Borrower Risk Composition around Deregulation



Panel A plots the share of loans issued by firms with an investment grade rating (as a fraction of all loans with rating data) around the deregulation date. Panel B plots the share of loans issued by borrowers which have a long-term issuer rating from S&P.

Table 14: Were "Treated" Loans Riskier Post-Deregulation?

		Interaction Variable:		
	Baseline	Syndicated loan	Affected lender	Out-of-state bank
Deregulated	0.003	0.003	-0.005	-0.048
	(0.035)	(0.035)	(0.056)	(0.057)
Deregulated × Interaction Variable		-0.013	0.005	-0.020
		(0.035)	(0.045)	(0.046)
Interaction Variable		0.010		-0.049
		(0.036)		(0.045)
Loan controls	Yes	Yes	Yes	Yes
Borrower controls	Yes	Yes	Yes	Yes
Borrower FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Only syndicated loans			✓	✓
Only commercial banks				✓
Observations	5,841	5,841	5,175	1,766
Adjusted R^2	0.871	0.871	0.875	0.896

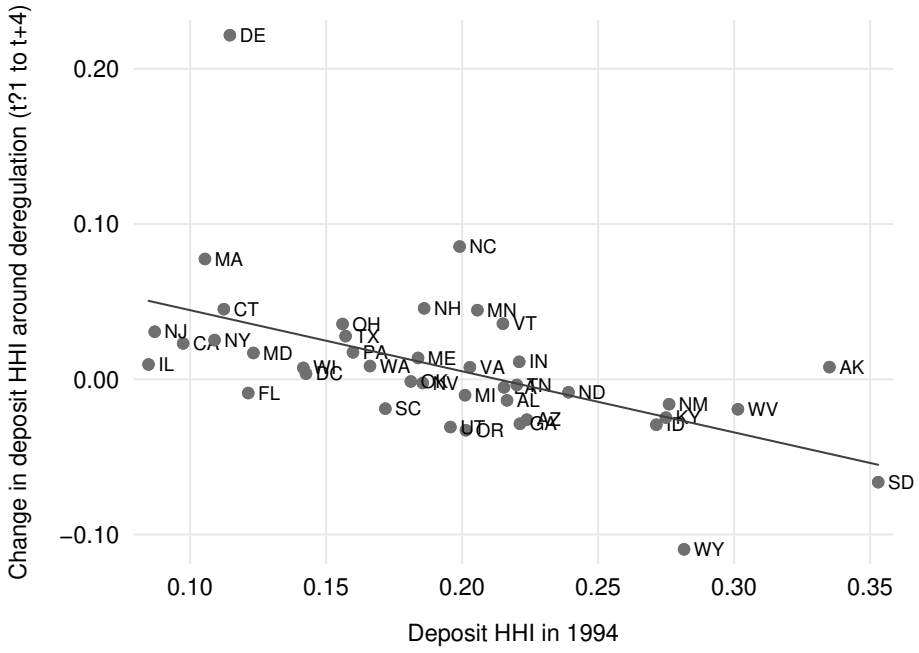
This table reports the results of regressing a dummy for loans issued by investment grade borrowers on *Deregulated*, a dummy that equals 1 if a state has lifted one or more branching restrictions, and interaction variables. 'Syndicated loan" is a dummy for loans with more than a single creditor. "Affected lenders" is a dummy equal to 1 for commercial banks, i.e. lenders with a SIC code starting with 602; and 0 for other lenders where industry classification is available. "Out-of-State Bank (Pre-Reform)" are banks without branches in the borrower's state prior to IBBEA implementation. The sample in columns (5) and (6) is restricted to syndicated loans only, and in (7) and (8) to syndicated loans issued by commercial banks. Robust standard errors (in parentheses) are clustered on the state-level, with ***, **, and * indicating statistical significance at the 1%, 5%, and 10% levels.

Table 15: Placebo Test: Assume IBBEA Happened Seven Years Earlier

		Interaction Variable:		
	Baseline	Syndicated loan	Affected lender	Out-of-state bank
Deregulated	-0.020	–	–	–
	(0.078)			
Deregulated × Interaction Variable		0.130	-0.079	0.021
		(0.099)	(0.191)	(0.465)
Interaction Variable		-0.219**	–	1.153
		(0.110)		(0.896)
Loan controls	Yes	Yes	Yes	Yes
Borrower controls	Yes	Yes	Yes	Yes
Borrower FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Year FE	Yes	–	–	–
State × Year FE		Yes	Yes	Yes
Only syndicated loans			✓	✓
Only commercial banks				✓
Observations	2,236	2,187	872	542
Adjusted R^2	0.858	0.894	0.944	0.955

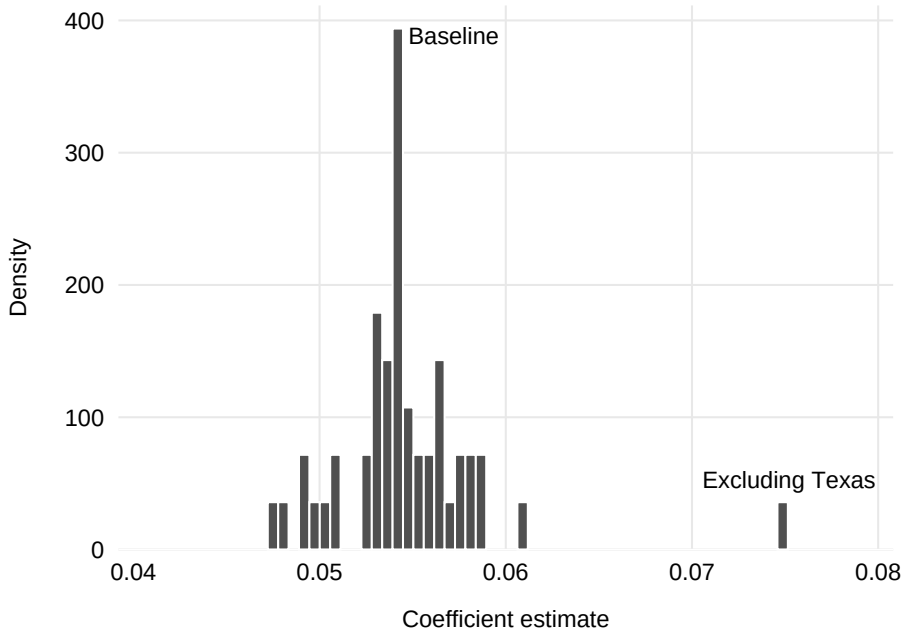
This table reports the results of regressing the interest rates spreads of syndicated loans (in natural logarithm) on placebo values for *Deregulated*, a dummy that equals 1 if a state has lifted one or more branching restrictions. In particular, we move the dummy values forward so that it equals one if a state lifts at least one branching restriction seven years later. For example, it is 1 for Alabama after 1990, which in fact deregulated in 1997. The estimation sample is 1987 through 1994, the year when the IBBEA was in fact first implemented. See the text for included control variables. Robust standard errors (in parentheses) are clustered on the state-level, with ***, **, and * indicating statistical significance at the 1%, 5%, and 10% levels.

Figure 8: Pre-Reform HHI and Change in HHI (t-1 to t+4)



This figure plots a state's deposit HHI in 1994 (deposit-weighted county average) against the post-deregulation change in the HHI. We omit the outliers Hawaii and Rhode Island.

Figure 9: Checking for Outliers by Excluding States in Turn



This figure shows the distribution of estimated coefficients $\hat{\beta}$ from the baseline regression $Interest\,Rate_f = \beta Deregulated_{st} + \gamma Borrower\,Controls_{i,t-1} + \delta Contract\,Controls_f + \alpha_i + \alpha_t + \varepsilon_f$, where all states in the sample are excluded in turn. The point estimate of the baseline regression is just under 0.055 (dark grey bar). The outlier on the upper tail is the regression excluding Texas ($p = 0.016$), which refers to more than 12% of the total number of facilities in the sample. This suggests that the overall effect we estimate is conservative, and likely much larger outside of Texas.

Table 16: The Effect of Deregulation on Syndicated Loan Rates (1987 to 2012)

		Interaction Variable:					
	Borrower FE (Baseline)	Syndicated Loan		Affected Lender		Out-of-State Bank (Pre-Reform)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Deregulated	0.062***	-0.079***	–	-0.049	–	-0.069	–
	(0.024)	(0.025)		(0.057)		(0.084)	
Deregulated × Interaction Variable		0.093***	0.104**	0.120**	0.248***	0.165*	0.613***
		(0.023)	(0.049)	(0.060)	(0.082)	(0.084)	(0.072)
Interaction Variable		-0.163***	-0.080*	-0.177***	–	-0.118	-0.232*
		(0.021)	(0.046)	(0.058)		(0.088)	(0.135)
Loan Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Borrower Controls	Yes	Yes	–	Yes	–	Yes	–
Borrower FE	Yes	Yes	–	Yes	–	Yes	–
Year FE	Yes	Yes	–	Yes	–	Yes	–
Borrower × Year FE			Yes		Yes		Yes
Bank × Year FE			Yes		Yes		Yes
Only Syndicated Loans				✓	✓	✓	✓
Only Commercial Banks						✓	✓
Observations	16,438	16,438	10,846	11,644	8,038	3,664	2,392
Adjusted R^2	0.799	0.800	0.951	0.824	0.958	0.874	0.967

This table reports the impact of interstate bank branch deregulation on interest rates spreads of US syndicated loans (in natural logarithm) in a sample from 1987 to 2012. *Deregulated* is a dummy that equals 1 if a state has lifted one or more branching restrictions. See the text for included control variables. "Syndicated loan" is a dummy for loans with more than a single creditor. "Affected lenders" is a dummy equal to 1 for commercial banks, i.e. lenders with a SIC code starting with 602; and 0 for other lenders where industry classification is available. "Out-of-State Bank (Pre-Reform)" are banks without branches in the borrower's state prior to IBBEA implementation. The sample in columns (4) and (5) is restricted to syndicated loans only, and in (6) and (7) to syndicated loans issued by commercial banks. "–" indicates absorbed estimates. Robust standard errors (in parentheses) are clustered on the state level, with ***, **, and * indicating statistical significance at the 1%, 5%, and 10% levels.

Table 17: Branching Deregulation and State-Level Loan Issuance (1987 to 2012)

	Syndicated Loan Volume			Bilateral Loan Volume		
	(1)	(2)	(3)	(4)	(5)	(6)
Deregulated	-0.535	1.363	1.033	-0.033	-0.510**	-0.510**
	(0.707)	(0.977)	(0.940)	(0.074)	(0.201)	(0.219)
Deregulated × HHI (1994)		-9.770***	-7.497**		2.454***	2.480**
		(3.266)	(2.817)		(0.855)	(0.930)
State Controls			Yes			Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,275	1,275	1,224	1,275	1,275	1,224
Adjusted R^2	0.599	0.605	0.623	0.337	0.368	0.379

This table reports results from state-level regressions of syndicated and bilateral loan issuance volumes (from Dealscan) on a branching deregulation dummy, interacted with a state's markte concentration in 1994. The sample period is 1987 to 2012. Loan volumes are scaled by state GDP. State controls are real GSP growth and house price growth. All regressions include state and year fixed effects. Robust standard errors (in parentheses) are clustered by state. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level.

Table 18: Robustness – Within Contract Changes in Spreads

		Interaction Variable:		
	Baseline	Affected Lender	Syndicated Loan	Out-of-State Bank
	(1)	(2)	(3)	(4)
Deregulated	1.450	0.924	-18.650**	-25.538
	(7.630)	(12.003)	(9.500)	(17.772)
Deregulated × Interaction Variable		0.674	21.046*	30.362*
		(12.056)	(11.678)	(18.507)
Loan Amendment Controls	Yes	Yes	Yes	Yes
Facility FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Observations	19,745	19,745	21,029	12,520
Adjusted R^2	0.304	0.304	0.301	0.303

This table reports the impact of a interstate bank branch deregulation on changes in interest rates of existing loans in the syndicated market in the US. The dependent variable is the basis point change in the lending rate spread from contracts included in the DealScan database. The explanatory variable is a dummy that equals 1 if a state lifts one or more branching restrictions for the first time. See the text for included control variables. Robust standard errors (in parentheses) are clustered on the borrower × year level, with ***, **, and * indicating statistical significance at the 1%, 5%, and 10% levels.