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Credit Supply and the Price of Housing[†]

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An exogenous expansion in mortgage credit has significant effects on house prices. This finding is established using US branching deregulations between 1994 and 2005 as instruments for credit. Credit increases for deregulated banks, but not in placebo samples. Such differential responses rule out demand-based explanations, and identify an exogenous credit supply shock. Because of geographic diversification, treated banks expand credit: housing demand increases, house prices rise, but to a lesser extent in areas with elastic housing supply, where the housing stock increases instead. In an instrumental variable sense, house prices are well explained by the credit expansion induced by deregulation. (*JEL* G21, G28, R21, R31)

Are asset prices affected by the supply of credit? The answer is key to the modeling choices that underpin virtually any asset pricing model.[1] It is also central to understanding the market response to changes in the regulation of credit markets and financial intermediaries, a question of immediate topical interest. Empirically, a definite answer is elusive because of well-known identification issues. The provision of credit is not an exogenous variable. There is every reason to expect that credit supply depends on the price of assets, which may be used as collateral. Credit also responds endogenously to current and expected economic conditions. Reverse causality and omitted variable biases are both rampant issues.

This paper identifies exogenous shifts in the supply of credit with regulatory changes to bank branching across US states, traces their effects on the size and standards of

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[1] An expansion in credit can relax financial frictions, increase market participation, with consequences on liquidity, asset prices, and potentially financial stability and the real economy. See Allen and Gale (2009) and Vayanos and Wang (2012) for recent reviews.

mortgage loans, and evaluates their impact on house prices. Crucially, the deregulation itself is *not* assumed exogenous. Rather, identification rests on two carefully chosen control groups: first, the set of lenders that are unaffected by the deregulation because of their legal status; second, the set of banks that are unaffected by the deregulation because of their location. In both cases, lenders in the control group should not respond to the deregulation if it constitutes a credit supply shock, but they will if it reflects changes in the demand for credit, contemporaneous or expected.

The event of interest records changes to the regulation of interstate branching in the United States after 1994. Even though cross-state branching was fully legal after the passage of the Interstate Banking and Branching Efficiency Act (IBBEA) of 1994, US states retained the right to erect roadblocks to hamper interstate branching. For instance, states were allowed to put limits on banks' size and deposits, or to forbid de novo branching. Rice and Strahan (2010) constructed a time-varying index capturing these differences in regulatory constraints between 1994 and 2005. This paper evaluates the consequences of these deregulations, with county-level measures of mortgage credit and house prices.

The framework is used to answer three questions: (i) Did the deregulation impact the mortgage market? (ii) Did it impact house prices? and (iii) Is the end effect on house prices channeled via a response of the mortgage market? The key finding is that deregulation affected the supply of mortgage loans, and via its effect on credit, the price of housing. Both effects are *causal*. Between 1994 and 2005, deregulation can explain between one-half and two-thirds of the observed increase in mortgage loans, and between one-third and one-half of the increase in house prices. Branching deregulations enabled banks to diversify deposit collection across locations, and to lower the cost of funds. Some of the saving was passed through to borrowers, with loans originated at better terms. With expanded credit, the demand for housing rose. This translated in large price effects in areas where construction is inelastic, but a response of the housing stock in areas where it is elastic.

A large empirical literature investigates the consequences of the successive waves of banking deregulation in the United States. Identification typically rests on the fact that state-level deregulation is motivated by political, rather than economic, reasons. Such lack of correlation is often viewed as sufficient to use the chronology of deregulation episodes as an exogenous event, whose consequences can be traced over time using simple regression techniques.[2] In this paper, causality is established without having to invoke such orthogonality, thanks to the introduction of two carefully designed control groups.

First, branching deregulation only covered depository institutions, not independent mortgage companies (IMCs) that do not collect deposits, but do originate mortgages. Within depository institutions themselves, only federal and state chartered commercial banks were concerned. Thrifts and credit unions (TCUs) were not, even though they also collect deposits and lend through local branches. The very purview of the legislation provides, therefore, a natural placebo sample, constituted

[2] Kroszner and Strahan (1999) or Rice and Strahan (2010) show that state-level banking deregulation in the United States is correlated with the lobbying power of small (insulated) banks relative to large (expansion-minded) banks, but not with contemporaneous economic conditions. See also Calomiris (2006). Kroszner and Strahan (2014) provide a thorough review of this extensive literature. Most of the papers consider the deregulation waves that precede IBBEA, i.e., that predate the one considered in this paper.

of IMCs and TCUs. Perhaps unsurprisingly, lending by commercial banks responds significantly to the deregulation: the number of originations, the total volume lent, and the loan to income ratio all increase significantly. But lending originated by placebo institutions remains unchanged. Such a differential response suggests that the observed credit expansion cannot be due to a boom in credit demand, expected or not. If it were, placebo lenders would also react on impact.

One could take issue with the claim that IMCs, TCUs, and commercial banks cater to similar borrowers. The paper documents that the observable differences in borrowers' characteristics across these lenders are minimal. But to further ascertain identification, a second control group is introduced that splits the sample of commercial banks themselves according to the location of their branches. The banks that stand to benefit from deregulation in a given state should be out-of-state banks, headquartered outside of it, since they become able to open new branches in the deregulating state.[3] The deregulation should not affect lending by in-state banks, headquartered in the deregulating state. In addition, a difference should also arise between out-of-state banks that open new branches in the treated state, versus out-of-state banks that never do: the former should find it easier than the latter to expand locally and reap large diversification gains.

The evidence suggests that origination, volume, and the loan to income ratio all increased for out-of-state banks that opened new branches in treated states. In contrast, no significant effect can be detected either among in-state banks, or out-of-state banks without local branches. By definition, each commercial bank can take either one of these three roles, depending on the identity of the deregulating state. On average, there can be no systematic difference between the three types. Yet, only out-of-state commercial banks with local branches expand credit in response to deregulation. It must be that they are the only ones able to do so, thanks to diversification gains and the decreased costs of funds they afford. These differential responses once again rule out demand-based explanations: if the demand for credit had increased, or had been expected to, all commercial banks would have increased lending, branch or no branch, in-state or out-of-state. Deregulation must have triggered a credit supply shock for banks in deregulated states.

The lenders that expanded credit did so with new originations and with increased loan to income ratios. At the same time, balance sheet data suggest that on average lenders in deregulated states had lower deposit costs, but also that they charged lower interest rates, with unchanged profitability. These findings suggest some of the cost savings were passed through into mortgage rates. As credit expanded, the demand for housing increased, pushing house prices up. Price responses were most pronounced in counties where housing supply is inelastic, and muted in elastic areas, where the stock of housing increased instead.

The paper finally shows that branching deregulation constitutes a legitimate instrument for the independent variable in a regression of house prices on mortgage credit. In an instrumental variable sense, branching deregulation can account for the expansion of credit supply, and can explain a significant share of the resulting increase in

[3] Banks could lend out of state all along, through subsidiaries. It is only with the passage of the IBBEA in 1994 that out of state branches became de jure possible. The index constructed by Rice and Strahan (2010) captures the de facto restrictions to out of state branching that survived after 1994.

house prices. At the time of the deregulation, a 1 percent change in (instrumented) credit increases the growth rate of house prices by 0.2 percent. The effect on growth peaks after two years, and reverts to zero after five to six years. Instrumented credit can explain all of the observed increase in house prices between 1994 and 2002, and between one-third and one-half of the subsequent increase between 2002 and 2005.

All of the paper's results prevail in a subsample formed by counties that abut a state border, and that belong to the same Metropolitan Statistical Area (MSA). Such robustness is remarkable, for three reasons. First, it is much harder to get any result with such a reduced sample size, and at most 35 states. Second, counties near a border are likely to be those where arbitrage happens. Borrowers living in regulated states have an incentive to cross the nearby border, and borrow there to purchase a house in their home county. Credit should then increase equally across the border, and so should house prices.[4] Third, MSAs regroup by definition adjacent territories with a high degree of social and economic integration. Unobserved differences between counties are presumably minimal there, which helps controlling for omitted variables. That is not to say counties are identical in a typical MSA, but they are presumably more similar than state-level averages. In addition, the location of the state border within an MSA is predetermined, and certainly exogenous to current local economic conditions. Both arguments strengthen the causal interpretation of the responses in mortgage credit and in house prices.

It is difficult to identify a shift in credit that is exogenous to the current (or expected) state of the economy. By extension, it is equally difficult to identify the causal effect of credit supply on asset prices, and to the best of our knowledge no paper has achieved this. Mian and Sufi (2009) argue the expansion in subprime mortgage credit, made possible by the securitization of the early 2000s, had significant consequences on house prices between 2002 and 2005. Adelino, Schoar, and Severino (2012) argue changes in the conforming loan limit (CLL) made credit cheaper, and had significant consequences on house prices especially around the limit. Di Maggio and Kermani (2014) argue the local proportion of national banks that were exonerated from anti-predatory laws in 2004 has significant consequences on local credit growth, employment, and house prices, both during the boom and the bust years after 2006. In all cases, a change in equilibrium credit affects house prices. But the changes in securitization or in the CLL can themselves be caused by the perspectives on the real estate market (or by the state of the economy).[5] And the local proportion of national banks can itself be caused by the entry of out-of-state banks in response to the branching deregulations of interest here. This paper's main contribution is to ascertain that the considered shift in credit is indeed exogenous.[6]

Such exogeneity is crucial, and necessary to draw meaningful inferences. For instance, Glaeser, Gottlieb, and Gyourko (2013) argue that the observed variation in

[4] Mortgages are aggregated at county level using the location of the purchased property, not the location of the lender. Interestingly, the results are weaker when the data are focused on the subset of counties within a close distance from the border: arbitrage seems to happen in the data, but for distances below 15–20 miles from the border.

[5] The response of credit to deregulation identified in this paper is not channeled via an increase in securitized loans. That is not to say securitization does not matter; rather, the mechanism in this paper works in addition to the securitization channel. Also, in this paper, changes in the conforming loan limit are soaked up by year effects.

[6] Rajan and Ramcharan (2012) document that in the 1920s the effect of foreign commodity shocks on US farmland prices increased with the number of local banks. This is consistent with the availability of credit affecting asset prices then. But it still assumes the supply of credit, i.e., the location of banks, is predetermined.

credit conditions over the past two decades was too small to account for the much larger movements in house prices. But they refrain from inferring that credit conditions could not have mattered: if banks anticipate large increases in the demand for housing, choosing not to change interest rates, rather than increasing them, could in fact amount to relaxing lending standards. Then, observed variations in credit are dwarfed by changes in house prices, but the fact is silent on the actual credit supply decisions made by banks, and their impact on house prices. The identification of an exogenous shift in credit supply is of the essence to estimate a meaningful elasticity.

The paper makes two additional contributions. First, it considers the consequences of deregulation of the banking sector on asset prices. Most papers in this extensive literature focus on the real economy, and typically consider earlier deregulation waves in the United States, in the 1980s and 1990s.[7] Second, the paper offers a narrative for the consequences of deregulation on mortgage credit, on banks' balance sheets, and ultimately on both the price and the stock of housing. As such, the paper also adds to the literature that studies the real consequences of bank liquidity shocks.[8]

The rest of the paper is structured as follows. Section I introduces the data. Section II discusses the effect of deregulation on the mortgage market, and Section III describes the effect on house prices. Both mechanisms are also examined jointly in the context of an instrumental variable estimation. Section IV concludes.

I. Data

This section introduces the data. It first describes the nature of the changes to bank branching regulations experienced in the United States since 1994. The mortgage and house price data are discussed next.

A. *Branching Deregulation*

The US banking sector has gone through decades of regulatory changes regarding banks' geographic expansion (Kroszner and Strahan 2014). The deregulation waves culminated in 1994 with the passage of the Interstate Banking and Branching Efficiency Act (IBBEA). Banks could then operate across state borders without any formal authorization from state authorities.

While the IBBEA authorized free interstate banking, it also granted individual states some latitude in deciding the rules governing entry by out-of-state branches. The IBBEA gave states the right to oppose out-of-state branching by imposing restrictions on: (i) de novo branching without explicit agreement by state authorities; (ii) the minimum age of the target institution in case of mergers; (iii) the acquisition of individual branches without acquiring the entire bank; (iv) the total amount of statewide deposits controlled by a single bank or bank holding company. As discussed in Johnson and Rice (2008), most states exercised their authority under the new law, hampering banking competition de facto across states. Rice and Strahan

[7] Jayaratne and Strahan (1996) look for growth effects. Morgan, Rime, and Strahan (2004), or Acharya, Imbs, and Sturgess (2011) emphasize volatility. Beck, Levine, and Levkov (2010) consider income inequality. Goetz, Laeven, and Levine (2013) discuss the market valuation of deregulated banks.

[8] Peek and Rosengren (2000), Ashcraft (2005), and Paravisini (2008) use natural experiments to generate exogenous liquidity supply shocks. See also, Khwaja and Mian (2008) and Jimenez et al. (2012).

(2010) compute a time varying index that records these restrictions on interstate branching. Their index runs from 1994 to 2005 and takes values between 0 and 4; the index is reversed so that high values refer to deregulated states.[9]

Appendix Figure B1 illustrates the geographic dispersion of the deregulation episodes over three-year intervals. Nine states had already moved to full deregulation by 1996. But the bulk of the change took place between 1996 and 2002.[10] By 2005, the end of the sample, 26 states had effectively stopped resorting to 3 or more of the restrictions considered. Eight midwestern states still had not deregulated at all. Appendix Figure B1 suggests deregulation was bunched over time and geographically. Given such a pattern, the paper seeks to explore the compounded effects of these policy steps taken in close succession, rather than each of their components taken in isolation.

Appendix Figure B1 raises the question of the timing of deregulation. It is the object of a large literature surveyed in Kroszner and Strahan (2014). A consensus view is that the timing of banking deregulation reflects the strength and political clout of large (expansion minded) banks relative to small (insulated) banks. The argument is consistent with the geography of deregulation in Appendix Figure B1, with relatively quick deregulation in coastal areas—where large banks tend to be located. This obviously implies a correlation with the growth in house prices, as these very same regions saw real estate prices soar over the sample period. The question is which way does the causality go. The placebo samples introduced in this paper help establish that deregulation was an exogenous trigger.

B. *Mortgage Credit and Banks*

Detailed information on mortgage loans is available from the Home Mortgage Disclosure Act (HMDA) database. HMDA reports information on mortgages originated by both depository institutions and independent mortgage companies (IMCs). Any depository institution must report to HMDA if it has received a loan application, and if its assets are above an annually adjusted threshold. In the paper, depository institutions are commercial banks, thrifts, and credit unions. Non-depository institutions, such as independent mortgage companies (IMCs), must also report to HMDA if their portfolio of loans for house purchase exceeds $10 million USD. IMCs are for-profit lenders that are neither affiliates nor subsidiaries of banks' holding companies.

Banks and IMCs differ in many respects. For this paper's purposes, the most important difference is that banks use branches to collect deposits and originate loans, while IMCs rely on wholesale funding and mortgage brokers (Rosen 2011). Only banks should respond to the branching deregulation discussed in this paper, as their customer base and sources of funding change when new branches can be

[9] This definition assumes the nature of the restriction that is lifted is irrelevant. In most cases, deregulating states choose to lift several restrictions at once, so that the four components of deregulation are highly correlated. It is in fact impossible to distinguish their individual effects.

[10] As in Rice and Strahan (2010), every state is assumed fully restricted in 1994. Prior to 1994 eight states permitted some limited interstate branching (i.e., Alaska, Massachusetts, New York, Oregon, Rhode Island, Nevada, North Carolina, and Utah). But the option to branch out of state lines was never exercised, except in a few cases (Rice and Strahan 2010, footnote 4). Johnson and Rice (2008) report that in 1994, just before the passage of the IBBEA, the average number of out-of-state branches per state was 1.22, and the proportion of out-of-state branches to total branches was just 0.07.

Table 1—Loans by Commercial Banks, Independent Mortgage Companies, Thrifts, and Credit Unions

	Loans			
	Full sample 1994–2005	1995	2000	2005
Number of applications received				
Commercial banks	1,777	1,027	1,830	2,468
Independent mortgage companies	1,324	749	1,086	2,137
Thrifts and credit unions	738	534	740	921
Number of loans originated				
Commercial banks	1,437	865	1,361	2,083
Independent mortgage companies	973	526	734	1,659
Thrifts and credit unions	610	456	632	727
Average loan originated (thousand of dollars)				
Commercial banks	111	85	103	144
Independent mortgage companies	95	71	94	121
Thrifts and credit unions	114	83	115	143
Average applicant's income (thousand of dollars)				
Commercial banks	65	56	64	75
Independent mortgage companies	58	47	58	69
Thrifts and credit unions	67	54	70	75

Notes: Mean values of county-year pooled data. Conventional loans are for purchase of single-family owner occupied houses. Lenders are commercial banks, independent mortgage companies, or thrifts and credit unions. The sample includes US counties in urban areas for which mortgage data are available for the period 1994–2005.

opened across state borders. In contrast, IMCs cannot directly make use of the deregulation to gain access to new borrowers, or to new funding. In addition, the deregulation pertained to commercial banks only, and not to thrifts and credit unions (TCUs), even though they finance most of their activity with deposits, and lend through branches. This is the sense in which IMCs and TCUs form a placebo sample.

Given the importance of legally unaffected lenders, Table 1 describes the main characteristics of mortgages originated by banks, TCUs, and IMCs.

Over the 1994–2005 period, commercial banks constitute approximately half of the mortgage market, both in terms of number of applications, and in terms of number of originations; IMCs constitute two-thirds of the remainder, while TCUs are the smallest category. All three expanded credit at roughly the same rate over the decade, though an acceleration over the first half of the 2000s exists for IMCs. Throughout the period, the amounts of the average loan originated by commercial banks or by TCUs were essentially identical, as were the average applicant's incomes. There is no observable reason to expect the customer base of commercial banks and of TCUs to display any systematic differences over the decade. In contrast, IMCs tend to lend amounts that are lower by 12 percent relative to commercial banks, and they lend to slightly poorer applicants, with average income smaller by 9 percent. It is hard to ascertain conclusively whether such small discrepancies constitute systematic and permanent differences in the customer bases of the two categories. Suffice it to say that the literature seems to conclude against such features of the mortgage market.[11]

[11] Rosen (2011) shows the markets shares of commercial banks and IMCs remain virtually unchanged through the mid-2000s, with averages around 70 percent and 30 percent, respectively. He also shows the trends in loan-to-income ratios, and the shares of subprime mortgages for both type of lenders tend to track each other closely well into the 2000s.

For any reporting institution, HMDA provides information on the loan characteristics (response, amount, but *not* the interest rate), and the applicant's income. In the paper, HMDA data are aggregated up to county level according to the location of the purchased property. The data are used to keep track of the number of applications, the number of originations, and the loan volume, given by the total dollar amount of loans originated in each county for purchase of single family owner occupied houses. The number of denials is simply the number of applications net of the number of originations. An object of special interest is the number of originations that are subsequently securitized. HMDA reports whether a loan was sold within a year after origination to another nonaffiliated financial institution or government-sponsored housing enterprise, which is assumed to mean they are securitized. Finally, the loan to income ratio is computed as the county-level total loan amount from HMDA, divided by total income taken from Internal Revenue Service data.[12] Originations, volume, denials, loans resold, and the loan to income ratio constitute the five variables that are computed between 1994 and 2005.

This paper contends the ability for banks to open branches across state borders has effects on the supply of credit, and ultimately on house prices. Information about the existence and the location of branches for commercial banks is obtained by merging HMDA with the Summary of Deposits, collected by the Federal Deposit Insurance Corporation. Then for every bank loan in HMDA it becomes possible to identify whether the lending institution owns a branch in the county where the property is purchased. Thus, an out-of-state bank, that is by definition headquartered outside of the state where the property is located, may or may not have a branch in it. A difference should therefore exist in the expansion of credit by out-of-state banks with at least one local branch, and by out-of-state banks without any. Similarly, a difference should also prevail in the supply of credit by in-state banks, or by out-of-state banks with local branches. The former are actually not affected by their home state deregulation, but the latter can readily respond to it. Of course, there is no reason to expect any systematic differences between in-state banks, out-of-state banks with local branches, or out-of-state banks without any: all banks in the sample take all roles in turn, depending on the identity of the deregulating state. This is the sense in which the geography of branches can provide a meaningful placebo sample.

The effect of deregulation on the supply of credit works via changes in banks profitability, presumably thanks to the geographic diversification gains afforded by branching. The mechanism should therefore be apparent in the balance sheets of commercial banks that operate in deregulated states. To investigate this possibility, the HMDA dataset is merged with the year-end Call Reports (Reports of Condition and Income for commercial banks), keeping track of banks' size and profitability, equity capital, the total interest and fee income earned on mortgages loans, the fraction of nonperforming mortgage loans, total deposits, and their cost. Balance sheet data are averaged at the county level, using the volume of loans originated by each commercial bank in that county as weights.[13]

[12] HMDA reports the applicant's income, but it is self-declared and notoriously problematic. Even though IRS data capture average county-level income rather than borrower's income, they assuage the issue of self-declaration. We thank an anonymous referee for this suggestion.

[13] Both mergers, between HMDA and the Summary of Deposits, and between HMDA and the Call Reports, are made possible thanks to the HMDA Lender File, compiled by the Board of Governors of the Federal Reserve

C. *House Prices and Other Controls*

County level house price indexes are collected by Moody's Economy.com, and refer to the median house price of existing single family properties. The series compounds data from a variety of sources including the US Census Bureau, regional and national associations of Realtors, and the house price index computed by the Federal Housing Finance Agency (FHFA). The data are used for urban counties, which implies the large cross section illustrated in Appendix Figure B2. The figure also maps the subsample formed by those counties that are part of a single MSA traversed by one state border (or more). The coverage is reduced but continues to include the main metropolitan areas in the continental United States.

A prominent alternative to Moody's Economy.com is the CoreLogic index, which measures changes in housing market prices holding quality constant. But its coverage is considerably reduced, so the main text is based on Moody's data, and results implied by CoreLogic are available upon request. All of the paper's conclusions hold with the alternative index.

Data on income per capita and population at the county level are collected by the Bureau of Economic Analysis. Information on the stock of (single-family occupied) housing units is obtained from Moody's Economy.com, using data from the US Census Bureau on housing permits, housing completions, and obsolescence rates. The elasticity of housing supply is taken from the topography-based index introduced by Saiz (2010). Finally, HMDA data on each lender's identifier and the location of the origination are used to compute a Herfindahl index of the concentration in loan origination at county level, a measure of local market power. Online Appendix Tables OA1 and OA2 report data sources, and key descriptive statistics for all variables.

II. Branching Deregulation and Mortgage Credit

This section establishes the effect of interstate branching deregulations on credit conditions. First, the main specification is introduced. Results are then presented for the two placebo samples, based on lenders' legal status, and on their location. Results are discussed for the full sample of counties, and then for the reduced sample focused on counties that abut a state border.

A. *Specification*

Identification is conventional and akin to a treatment effect, where deregulated states are treated. We estimate

$$\ln L_{c,t} - \ln L_{c,t-1} = \beta_1 D_{s,t-1} + \beta_2 \mathbf{X}_{c,t} + \alpha_c + \gamma_t + \varepsilon_{c,t}, \tag{1}$$

where c indexes counties and s indexes states. $L_{c,t}$ is one of the five observed measures of county-level activity in the mortgage market: the number of origina-

System. The file maps the lender identifier in HMDA with the corresponding bank identifier in the Summary of Deposits or the Call Reports.

tions, their total amount, the number of applications denied, the loan to income ratio, and the number of loans resold within the year. $\mathbf{X}_{c,t}$ summarizes time-varying county-specific controls, that include: a lagged dependent variable, the (current and lagged) log changes in income per capita, population, house prices, and the Herfindahl index of loan origination. The controls hold constant some of the conventional determinants of credit demand at the county level, and potential county-level heterogeneity in banking competition, before and after deregulation.

In equation (1) the fixed effects, α_c, ensure that all county-specific influences are accounted for, provided they are invariant over time. They also guarantee that other (time-invariant) state-specific laws, such as homestead and personal property exemptions, or foreclosure laws are taken into account. This minimizes the concern that other state regulations drive the paper's results. Year fixed effects, γ_t, are also included to reflect time-varying factors common to all counties. A prominent example are fluctuations in the US credit activity driven, for instance, by changes in the Federal Funds rate. Another one is the change in the conforming loan limit that Adelino, Schoar, and Severino (2012) use to identify credit supply shocks.

With county and time fixed effects, the approach is akin to a difference-in-difference model. Identification rests on the dispersion across states (and time) of deregulation, captured by $D_{s,t}$, which aggregates the four elements of deregulation to interstate branching compiled by Rice and Strahan (2010) discussed in Section IA.

The measures $L_{c,t}$ of the mortgage market and the controls $\mathbf{X}_{c,t}$ all display heterogeneous trends across counties. Following Paravisini (2008), the most parsimonious treatment of these trends is to take first-differences, as in equation (1). With variables in differences, the presence of county fixed effects guarantees that different county specific trends are controlled for in all variables. Estimates of β_1 therefore capture the impact effect of deregulation on the growth rate in credit conditions. Since the specification includes a lagged dependent variable, the growth response is allowed to be temporary.

Deregulation is state-specific but loans are observed at the county level. The error terms $\varepsilon_{c,t}$ in equation (1) can therefore display a potentially time-varying state component. Following the recommendations in Bertrand, Duflo, and Mullainathan (2004) and Angrist and Pischke (2009), the residuals are clustered by state. This allows for maximum flexibility in the variance-covariance matrix of residuals.[14]

B. *Control Group I: Legally Unaffected Lenders*

Table 2 presents the results for the full sample of counties. Panel A focuses on loans originated by commercial banks. The first two columns reveal that the number and volume of loans both increase significantly with deregulation, which suggests the actual size of the mortgage market expands. The point estimate for β_1 in the first column implies that, on impact, states where all branching restrictions are lifted experience an annual growth rate in loans 12 percent higher than states imposing

[14] It is also possible that residuals are clustered across states by year. Clustering standard errors by state and by year, following Petersen (2009), makes little difference, presumably because the specification in equation (1) includes year effects.

Table 2—Commercial Banks versus Placebo Samples

	Number of originations	Volume	Number of denials	Loan to income ratio	Number sold
Panel A. Commercial banks					
Deregulation	0.028**	0.029**	0.013	0.029**	0.008
	(0.010)	(0.012)	(0.018)	(0.011)	(0.012)
Observations	10,992	10,992	10,923	10,922	10,689
Number of counties	1,018	1,018	1,018	1,018	1,018
Number of states	50	50	50	50	50
R^2 within	0.122	0.132	0.389	0.124	0.112
Panel B. Independent mortgage companies, thrifts, and credit unions					
Deregulation	0.002	0.001	0.011	0.001	−0.015
	(0.008)	(0.008)	(0.015)	(0.008)	(0.011)
Test	[0.029]	[0.038]	[0.721]	[0.042]	[0.268]
Observations	10,580	10,580	10,566	10,579	9,859
Number of counties	1,017	1,017	1,016	1,017	1,006
Number of states	50	50	50	50	50
R^2 within	0.406	0.315	0.613	0.295	0.427

Notes: Dependent variables are the log change in the number or volume of mortgages originated, the number of applications denied, the loan to income ratio, and the number of mortgages originated and securitized. Regressors are: the Rice and Strahan (2010) Index of Interstate Branching Deregulation, a lagged dependent variable, current and lagged log change in county's income per capita, population, house price, and the Herfindahl index of loan concentration. All variables are defined in online Appendix Table OA1. The sample includes all US counties in urban areas for which mortgage data are available for the period 1994–2005. Panel A reports regression results for mortgage loans originated by commercial banks. Panel B reports regression results for the placebo sample of mortgage loans originated by independent mortgage companies, thrifts, or credit unions. The index of interstate branching deregulation ranges from 0 (most restricted) to 4 (least restricted). All regressions include county and year fixed effects. Standard errors are clustered by state. "Test" reports *p*-values associated with the null hypothesis that the coefficients in panel A are zero and equal to those in panel B.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

full restrictions. The magnitude is similar for the volume of loans. The number of denials is unchanged, but the loan to income ratio does rise with deregulation.

The last specification in Table 2 suggests β_1 is not different from zero for the number of loans that are resold within the year to other nonaffiliated financial institutions and government sponsored enterprises.[15] It is the non-securitized segment of the mortgage market that expands when geographic restrictions on branching are lifted. In other words, the expansion in credit supply identified in this paper does not depend on the possibility to securitize loans: it happens independently of financial innovation. Of course, that does not mean securitization does not matter for mortgage credit in general: it just does not matter for the shock identified by branching deregulation.

How does the response in credit growth change over time? Jorda (2005) introduces a method that estimates impulse response functions directly, without specifying or estimating the unknown true multivariate dynamic process, for instance through a vector autoregression (VAR). Jorda (2005) shows an impulse response

[15] The separate responses of loans sold to either government sponsored enterprise or to private institutions are similar to those in Table 2.

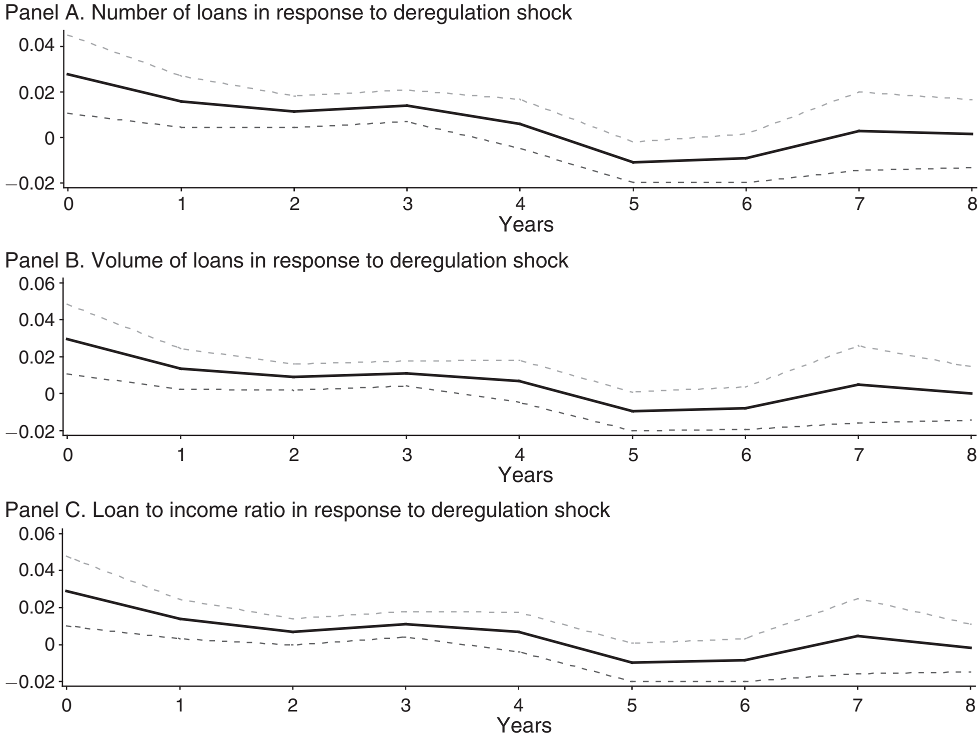

Figure 1. Mortgage Credit by Commercial Banks: Impulse Responses to Branching Deregulation Shock

Note: Dashed lines are 90 percent confidence bands.

function estimated from a local projection is robust to a mis-specification of the data generating process, can accommodate nonlinearities that would be intractable in VARs, and can be estimated in a simple univariate framework. Local projections are based on sequential regressions of the endogenous variable shifted forward. In the case of equation (1), it is given by a vector of estimates $\left\{\beta_1^{(i)}\right\}_{i=0,1,\ldots}$ collected from the estimations of

$$\ln L_{c,t+i} - \ln L_{c,t+i-1} = \beta_1^{(i)} D_{s,t-1} + \beta_2 \mathbf{X}_{c,t} + \alpha_c + \gamma_t + \varepsilon_{c,t},$$

where each estimate of $\beta_1^{(i)}$ captures the effect of deregulation at horizon i . Figure 1 presents the impulse response functions implied by the estimates in the panel A of Table 2, for originations, loan volume, and the loan to income ratio. In all three cases, the growth effects of deregulation on credit supply are temporary: they peak on impact, at the values reported in Table 2, and peter down until they become insignificant four years after the shock. Deregulation has temporary but long-lasting effects on credit growth, i.e., permanent effects on the level of credit that take up to four years to develop.

How do these estimates translate in the aggregate? Figure 2 illustrates how much of the actual volume of US mortgage loans originated by commercial banks between 1994 and 2005 can be explained by deregulation. This is done with two in-sample predictions. The first one assumes that, for each observed deregulation in each state,

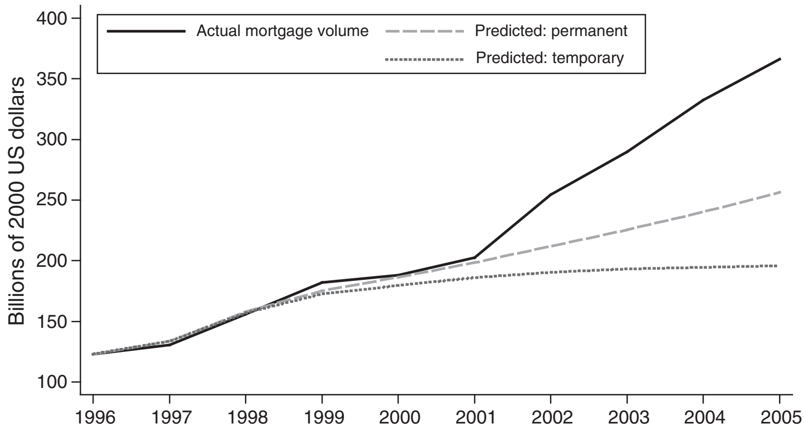

Figure 2. Actual and Predicted Aggregate Mortgage Volume by Commercial Banks

Note: This figure plots the aggregate real volume of mortgages for home purchase originated by commercial banks between 1994 and 2005, and the predicted volume (dashed and dotted lines) as described in Section IIB.

the growth rate of credit is increased by 2.8 percent, as per the results in Table 2. In other words, it assumes the growth effects of deregulation are permanent, which constitutes an upper bound. The second fitted line assumes the response of credit growth to each observed deregulation is still 2.8 percent on impact, but it tapers off over time: down to 2 percent one and two years after the deregulation, further down to 1 percent after three years, and zero afterwards, as per Figure 1. But there are no compounded effects: a given deregulation stops having any dynamic effects the year it is superseded by a second, consecutive one. The exercise constitutes, therefore, a lower bound. Figure 2 suggests deregulation can explain between one-half and two-thirds of actually observed US credit expansion between 1994 and 2005.

Panel B in Table 2 reports estimates of equation (1) for loans originated by placebo lenders. These institutions are unaffected by changes in branching regulations. And in fact, deregulation has no effect on their credit origination: estimates of β_1 are all insignificant in panel B. Compared with the significant estimates in panel A, the point estimates in panel B are one order of magnitude smaller. There is a significant difference in estimates, and it does not come from larger standard errors in panel B. This differential effect of branching regulations across categories of lenders sharpens the causal interpretation of our estimates. If deregulation were endogenous and simply responding to expected large increases in the demand for mortgage, β_1 should be significant across both panels in Table 2.[16]

How are IMCs responding to a change in market structure triggered by the deregulation? One view is that branching restrictions provided IMCs with a competitive advantage in controlling market shares in regulated states. Deregulation then triggered a reallocation of capital away from them and toward commercial banks, as the latter gained efficiency. While this view explains the positive response of banks, it

[16] Favara and Imbs (2010) focused on IMCs only, and established the same differential effect as in Table 2. Both sets of unaffected lenders, IMCs and TCUs taken separately, imply insignificant estimates of β_1 .

also implies negative coefficients in panel B of Table 2, rather than the insignificant estimates. There is no response of IMCs, thrifts, and credit unions on impact. It must therefore be that IMCs and TCUs take time to react to the change in competition, as commercial banks open new branches. But they do react, as they manage to keep loan growth unchanged after deregulation.

The absence of any significant consequence of deregulation in a placebo sample puts to rest the possibility that β_1 is significant because overall economic activity has improved with the deregulation. But the finding may seem surprising in light of the literature that argues deregulation affects the real economy directly, as this implies all lenders would expand credit. For instance, Jayaratne and Strahan (1996) established a systematic correlation between the deregulation of intrastate branching and the growth rates of output per capita at state level, between 1972 and 1992. Morgan, Rime, and Strahan (2004) show the relaxation of interstate banking regulations tends to be associated with low year-to-year growth fluctuations within a state between 1976 and 1994. Cetorelli and Strahan (2006), and Kerr and Nanda (2010) document a positive correlation between firm entry rates and the deregulations in intrastate branching or interstate banking that occurred between 1977 and 1994. All these papers suggest economic activity did respond directly to the deregulation episodes that preceded the IBBEA. This creates an apparent contradiction with the differential effects documented in this paper, that the branching deregulations post-1994 affected credit origination by certain categories of lenders only.

But the deregulation episodes considered in this paper have little connection with those that were documented to have real effects. The index of restrictions used here starts after 1994, once all impediments to intrastate branching and interstate banking were lifted with the passing of the IBBEA, i.e., once all the deregulations documented as having had direct real effects were complete. In fact Jayaratne and Strahan (1996) find that deregulation in their sample does *not* increase loan growth, exactly the opposite of what is documented here for mortgage credit. The data on real output or firm entry do not overlap either with the sample used here: there is no reason to expect that the real effects documented in earlier data, in response to earlier (and different) events, should be an indication of what happened after 1994, in response to conceptually different deregulation episodes.[17]

In Table 2, equation (1) is estimated on the full sample of urban counties with available data. Appendix Table A1 focuses instead on the sample formed by counties that are located in one of the 36 MSAs traversed by a state border. The main assumption in this reduced sample is that the control variables in equation (1)—observed or unobserved—vary continuously around the border. The assumption is maintained on the basis of the high degree of social and economic integration among adjacent counties in the same MSA.

The focus on this reduced sample is important because it helps alleviate concerns of an omitted variable bias, and the reverse causality that comes with it. In principle,

[17] In fact, a regression of county-level income per capita on this paper's deregulation index always gives an insignificant estimate. The growth effects of deregulation are not uncontroversial. Focusing on contiguous counties that abut state borders, Huang (2008) finds no significant growth effects before 1985, and only five instances of significant growth effects after 1985. In all five instances, growth effects are associated with the lifting of both intrastate branching and interstate banking regulations, both of which are complete by 1994. Huang's approach is an additional reason why this paper also focuses on a sample of counties that abut state borders.

the positive estimates of β_1 in Table 2 could reflect unobserved variables driving both the deregulation and the expansion in credit, both at state level. For instance, branching deregulation could be motivated by lobbying on the part of commercial banks who anticipate soaring credit demand at state level. In that case, causality would go from credit (demand) to deregulation. This argument already has trouble explaining why IMCs and TCUs do not seem to be taking advantage of such a hypothetical expected boom. It has more trouble still explaining a differential response between contiguous counties, separated by a state border, but part of the same MSA. The argument would have to be that the demand boom that motivates commercial banks to lobby for deregulation is extremely localized: the boom would have to prevail in counties on one side of the state border, but not in others across the border, even though they are contiguous and actually part of the same MSA. That seems unlikely.

Appendix Table A1 reports regression estimates of equation (1) in the restricted sample of 36 MSAs, for commercial banks, and for IMCs/TCUs. As before, the number and volume of mortgage loans originated by commercial banks increase significantly. The point estimates imply that the repeal of all four restrictions to interstate branching increases credit growth by as much as 16 percent. As in Table 2, the number of denials remains unchanged, and the loan to income ratio rises. There is an increase in the number of loans resold, significant at 5 percent confidence level. But its magnitude is about half of the response in loan numbers and volume, suggesting more than securitization is at play. All responses continue to be absent for loans originated by placebo lenders, with significant differences across the two panels. The differential effect documented in Table 2 survives in a sample of relatively homogeneous counties: the mortgage market expands in counties located in deregulating states, while their immediate untreated neighbors see no change in market size. What is more, only treated banks respond.[18]

C. *Control Group II: Geographically Unaffected Lenders*

By definition, deregulation affects out-of-state banks, that become able to open branches and collect deposits there. Thus, it is the lenders that are headquartered outside of a deregulating state that should benefit the most. If branching is the channel of credit expansion, it is those out-of-state banks with local branches that should respond to the deregulation. Credit originated by in-state banks, i.e., institutions headquartered in the state where the property is located, or by out-of-state banks without a branch in deregulating states, should remain unchanged.

Table 3 splits the sample according to this logic. Out-of-state banks are defined as institutions headquartered outside of the state where the property purchased with the mortgage is located. They may or may not have branches in the county of the property, i.e., local branches.

Panel A reports the estimation of equation (1) on the sample of out-of-state banks with branches in the deregulating state. As in Table 2 and Appendix Table A1,

[18] Standard errors are clustered at state level in Appendix Table A1. The results survive with double clustering at state and MSA level, following Cameron, Gelbach, and Miller (2011). With double clustering, residuals are allowed to correlate within states and across the counties that are in the same MSA, but not in the same state.

Table 3—The Importance of Bank Location and of Bank Branches

	Number of originations	Volume	Number of denials	Loan to income ratio	Number sold	Number local branches
Panel A. Out-of-state banks: local branches						
Deregulation	0.161**	0.167***	0.069	0.165***	0.055	0.077**
	(0.060)	(0.061)	(0.059)	(0.061)	(0.082)	(0.030)
Observations	4,514	4,514	4,108	4,513	3,356	4,783
Number of counties	767	767	738	767	700	790
Number of states	49	49	49	49	47	49
R^2 within	0.159	0.145	0.221	0.140	0.258	0.108
Panel B. Out-of-state banks: no branches						
Deregulation	0.016	0.015	0.012	0.014	0.010	—
	(0.011)	(0.011)	(0.024)	(0.012)	(0.014)	
Test	[0.012]	[0.017]	[0.429]	[0.018]	[0.673]	
Observations	9,988	9,988	9,926	9,987	9,771	—
Number of counties	1,018	1,018	1,018	1,018	1,017	—
Number of states	50	50	50	50	50	—
R^2 within	0.160	0.122	0.475	0.112	0.188	—
Panel C. In-state banks						
Deregulation	0.003	−0.003	0.005	−0.003	−0.025	0.001
	(0.010)	(0.011)	(0.015)	(0.011)	(0.019)	(0.007)
Test	[0.016]	[0.024]	[0.431]	[0.024]	[0.400]	[0.032]
Observations	9,841	9,841	9,339	9,840	9,175	9,019
Number of counties	1,017	1,017	1,005	1,017	1,006	978
Number of states	50	50	50	50	50	50
R^2 within	0.025	0.391	0.067	0.042	0.097	0.047

Notes: Dependent variables are the log change in the number or volume of mortgages originated, number of applications denied, loan to income ratio, number of mortgages originated and securitized, and the log change in the number of local branches (in panels A and C). Regressors are: the Rice and Strahan (2010) Index of Interstate Branching Deregulation, a lagged dependent variable, current and lagged log change in county's income per capita, population, house price, and the Herfindahl index of loan concentration. All variables are defined in online Appendix Table OA1. The sample includes all US counties in urban areas for which mortgage data are available for the period 1994–2005. Panel A reports results for out-of-state-banks with local branches, i.e., lending in a location outside of the state where they are headquartered, and where they have at least one branch. Panel B reports results for out-of-state-banks without local branches, i.e., lending in a location outside of the state where they are headquartered, and where they have no branch. Panel C reports regression results for in-state banks, i.e., lending for a property in the state they are headquartered. The index of interstate branching deregulation ranges from 0 (most restricted) to 4 (least restricted). All regressions include county and year fixed effects. Standard errors are clustered by state. "Test" reports p-values associated with the null hypothesis that the coefficients in panel A are zero and equal to those in panels B and C.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

growth in the number of originations and in the volume of loans both increase at the time of deregulation. The number of denials still remains unchanged, while the loan-to-income ratio increases. Interestingly, growth in the number of out-of-state bank *branches* also responds to the deregulation, in the last column of Table 3. It is because they open local branches in the deregulating state that out-of-state banks are able to expand credit.[19]

[19] The point estimates in Table 3 are larger than in Table 2. In terms of economic significance, however, there is little difference in the two coefficients, because credit originated by out-of-state banks has larger average growth and larger cross-sectional dispersion than overall credit lent by commercial banks. For instance, lifting all four restrictions implies an increase in the growth rate of credit from its mean to its eightieth percentile in both samples.

Panels B and C in Table 3 estimate equation (1) on the samples of out-of-state banks without a local branch, and of in-state banks. In both cases, no significant changes in the credit market can be observed. Originations, volumes, loan-to-income ratios all remain unchanged around the date of deregulation. Interestingly, in-state banks do not open new local branches as the deregulation happens: new branches are only opened by out-of-state banks, i.e., by those affected by the deregulation. The coefficients are significantly different from panel A. Once again, such differential responses could not obtain if the expansion in credit were motivated by an increase in demand, expected or contemporaneous, or indeed if the deregulation itself were motivated by demand. It has to characterize an exogenous shock to the supply of credit.

By definition, any bank in the sample is either in-state, in one instance, or out-of-state, in the others. And out-of-state banks themselves have local branches in some states, but none in others. In other words, any commercial bank in the sample can take any role, depending on the location of the property purchased and the location of its branches. So on average, there cannot be any other difference between the three types than the location of their home state, and the geography of their deposit collection. It must be that it is this very geography that warrants the entry of out-of-state banks. A natural interpretation is that the deregulation enabled out-of-state banks to reap geographical diversification gains. The diversification gains made it possible for out-of-state banks to lend at better conditions than the incumbents, and to expand the local mortgage market.

The lack of response by in-state banks may seem surprising. After all, from the standpoint of other US states, they are out-of-state banks. So they should be able to branch into other deregulating states, collect deposits there, and reap diversification gains. That would be happening if states deregulated in a synchronized manner, or if the deregulation involved some bilateral agreements. In fact, a (small) fraction of the pre-1994 deregulations had a bilateral dimension.[20] Out-of-state banks could then enter the deregulating state, just as in-state banks could enter the other party in the agreement. Asymmetric responses to deregulation would not prevail. But bilateral agreements disappeared after 1994, with the passage of the IBBEA. Some deregulations post-1994 still entailed some multilateral reciprocity, but no explicit agreements between pairs of states. In results available upon request, the differential responses in Table 3 are shown to be larger within the sample of deregulations that were not reciprocal (approximately 60 percent of the cases); they are insignificant when the deregulations were reciprocal.

Appendix Table A2 once again focuses the three-way sample splits in Table 3 on counties that abut a state border. All results stand: out-of-state banks expand in deregulating states, opening new branches to increase the number and volume of loans. Estimates of β_1 in panel A are virtually identical to those in Table 3. In contrast, panels B and C show that in-state banks, or out-of-state banks without local branches do not respond. Estimates of β_1 are significantly different from panel A. The differential responses documented in the full sample survive in the reduced sample of counties that are adjacent to a state border.

[20] See Dean (1983), Landier, Sraer, and Thesmar (2013), or Michalski and Ors (2012).

D. *Robustness*

The online Appendix reports three additional exercises. First, the growth rates in $L_{c,t}$ and $\mathbf{X}_{c,t}$ are computed over three-year averages, which leaves enough time for the reactions of both commercial banks and IMCs/TCUs to unfold. Second, the deregulation variable is interacted with year-specific binary variables, in order to investigate which period witnessed the largest effects on the mortgage market. Third, the expansion of credit between 1990 and 1994 is regressed on subsequent deregulation, between 1996 and 2000, to verify the expansion of credit is indeed specific to the post-1994 period, rather than an artifact of decisions made before the passage of IBBEA in 1994.

Panel A of online Appendix Table OA3 reports estimates of β_1 for three-year average values of the growth rate of $L_{c,t}$ and $\mathbf{X}_{c,t}$. The time effects, γ_t, now refer to three-year intervals, i.e., 1994–1996, 1997–1999, 2000–2002, 2003–2005. For banks, the response present in yearly growth rates holds as well for longer periods. Interestingly, the point estimates of β_1 for placebo lenders in panel B remain insignificant in all instances. Mortgage companies, thrifts, and credit unions do not seem to increase credit in response to changes in market structure induced by the regulatory environment, not even after three years.

Online Appendix Table OA4 investigates when deregulation has the most effect on credit. A variable is introduced reporting each year the regulation index changes, and equal to zero otherwise. The variable is interacted with the index itself, to isolate the years when deregulation happens. The table reports the interaction coefficients, for full and border samples. Deregulation has the most effect on the expansion in credit in the second half of the 1990s. In the reduced sample, the coefficients are largest in 1997 and 1998, and decrease until they become insignificant in 2002. The year 2003 is also significant in both samples, but with a coefficient that is small in magnitude.[21] It seems the credit expansion triggered by deregulation is most prevalent in the earlier part of the sample, when the majority of states deregulated.[22]

Online Appendix Table OA5 reports the results of the benchmark regression in equation (1) estimated over a placebo sample period of four years: credit measured between 1990 and 1994 is regressed on deregulation measured between 1996 and 2000. The objective is twofold: First, to verify credit did not respond in anticipation of future expected deregulation. Second, to verify the expansion of credit between 1996 and 2005 documented in the paper had not in fact begun earlier. The table shows clearly that credit between 1990 and 1994 was unaffected by subsequent deregulation, passed after the IBBEA. This is true of banks, and of placebo lenders.

[21] In 1996, deregulation only changed in Alaska, i.e., in three counties. In 2005, deregulation only changed in North Dakota, i.e., in four counties. The two years are omitted for simplicity. For the same reason, the significant coefficient in 2003 has little aggregate consequence, as only four states deregulated then.

[22] The paper's results seem to contradict the findings of Rice and Strahan (2010). These authors focus on bank loans contracted by small firms and identify a response of loans' terms, but not of their quantity, to the very same deregulation index. In this paper, mortgage lending is observed at bank level, not debtors' overall portfolios. It is difficult to ascertain the response of overall household debt to the deregulation, because the only publicly available data on changes in the stock of household debt (from the Consumer Credit Panel/Equifax at the Federal Reserve Bank of New York) are available from 2000, i.e., after the bulk of the deregulation waves studied in this paper occurred. Chakraborty, Goldstein, and MacKinlay (2013) show that banks active in housing markets between 1988 and 2006 increased mortgage lending at the expense of commercial lending. This could imply unchanged total household debt over the sample considered in this paper.

III. Credit Supply and the Price of Housing

The previous section established the existence of an exogenous shock to the supply of credit, motivated by deregulation in the ability of banks to branch across state borders. This section discusses whether the credit supply shock affects house prices in a causal sense: first directly, and second in an instrumental variable sense, via changes in mortgage credit.

A. *Reduced Form: House Prices and Deregulation*

It is well known that house prices display considerable geographic heterogeneity in the United States (Ferreira and Gyourko 2012). Such heterogeneity can arise from differences in housing supply elasticities, for instance because of local costs, land use regulation, or geographical restrictions (Gyourko and Saiz 2006; Gyourko, Saiz, and Summers 2008; Saiz 2010). It can also come from the demand side of the market, simply because income, demographic factors, and amenities are geographically heterogeneous (Lamont and Stein 1999; Gyourko, Mayer, and Sinai 2013; Glaeser and Gyourko 2006; Burnside, Eichenbaum, and Rebelo 2011; Favara and Song 2014). In this paper, the geographic dispersion in house prices is explained with differences in the availability of credit, which are, in turn, driven by heterogeneous branching regulations across states.

The empirics follow the treatment approach described in the previous section. The dependent variable is the growth rate in house prices, regressed on state branching deregulation. Based on the previous section, the deregulation episodes can be taken as exogenous to local demand conditions, and in particular to house prices. Consider the specification

$$(2)\quad \ln P_{c,t} - \ln P_{c,t-1} = \beta_1 D_{s,t-1} + \beta_2 D_{s,t-1} \times \eta_c^S + \beta_3 \mathbf{X}_{c,t} + \alpha_c + \gamma_t + \varepsilon_{c,t},$$

where c indexes counties, s states, and t years. The variable $D_{s,t}$ continues to denote the Rice-Strahan deregulation index. $P_{c,t}$ is the county house price index as given by Moody's Economy.com, and $\mathbf{X}_{c,t}$ summarizes additional determinants of house prices documented in the literature. Glaeser and Gyourko (2006) and Himmelberg, Mayer, and Sinai (2005) include rents, while Lamont and Stein (1999) include contemporaneous and lagged per capita income. No information is available on rents at the county level, so local influences on the real estate market are approximated with the contemporaneous and lagged growth rates in per capita income and population. Following Case and Shiller (1989), $\mathbf{X}_{c,t}$ also includes a lagged value of the dependent variable to allow for momentum in house prices. As in equation (1), the presence of a lagged dependent variable allows for the growth effects of deregulation to peter out over time.

Equation (2) is estimated in first differences because house prices in the United States display heterogeneous trends. More importantly, $P_{c,t}$ is effectively an index, whose level has no economic interpretation (Himmelberg, Mayer, and Sinai 2005). As in equation (1), γ_t captures country-wide cycles in the growth of real estate prices. And α_c captures county-specific, time invariant trends in house prices.

To account for the possible heterogeneity in the responses of house prices across counties, equation (2) is estimated using weighted least squares. There are many reasons why the responses of house prices to a given credit shock can be heterogeneous across counties, for instance local topographic differences, as discussed next. The weights are given by the (inverse of) the number of counties per state. In OLS, states with many counties are given prominence, even though this is precisely where the responses of house prices are most likely to be heterogeneous.

The coefficient of interest, β_1, traces the consequences on house prices of deregulation episodes, which is expected to be significantly positive. A channel that works via increased demand for housing implies a larger price response wherever construction is restricted. Equation (2) lets the effect of deregulation depend on the elasticity of housing supply, η_c^S, constructed by Saiz (2010). The index η_c^S captures geographic limits to constructible land, measured at MSA level. Equation (2) assumes therefore that land topography is the same across the counties that form each MSA with elasticity η_c^S. If the fundamental shock is to the demand for housing, the response of house prices should be muted in areas where construction is responsive, i.e., $\beta_2 < 0$. Analogously, a version of equation (2) where the dependent variable is the *stock* of housing should imply a muted response of the housing stock where the supply of housing is most *inelastic*.

Table 4 presents the estimates of equation (2) for different control sets. The unconditional estimate of β_1 is positive and significant in the total sample of counties with house price data (column 1). It remains positive with a control for the elasticity η_c^S (column 2). Interestingly, β_2 is significantly negative, so that the increase in house prices triggered by deregulation is in fact muted in counties where the supply of housing is elastic. The two coefficients continue to be significant with the full set of controls (column 3).

The last two columns of Table 4 present the results of estimating equation (2) with the stock of housing as a dependent variable, and where the inelasticity of housing supply is the inverse of the measure in Saiz (2010). Irrespective of the control set, the stock of housing grows significantly in response to deregulation. Interestingly, the effect is significantly smaller in inelastic counties. This is consistent with the interpretation of deregulation as a positive shock to the demand for housing, channeled into the economy via an increase in the supply of credit.

Suppose deregulation were in fact systematically correlated with η_c^S, as if restrictions were lifted fastest in states where construction is inelastic. Then the results in Table 4 would only mean house prices increase most where supply is inelastic, since $D_{s,t}$ and η_c^S would then effectively be multi-collinear. Of course, deregulation is time-varying and so perfect multi-collinearity is implausible. And it is unlikely deregulation constitutes in fact a response to state-specific economic developments, since that would imply all lenders would increase credit. But assuming these arguments away, the lifting of branching restrictions is the outcome of lobbying on the part of banks. If banks were indeed maneuvering to capture the rents associated with rising house prices, they would in fact argue *against* deregulation in counties with low η_c^S. That would imply a positive correlation between η_c^S and $D_{s,t}$, as regulation is kept tight wherever prices boom. This is the opposite from what the data say.

In Appendix Table A3, equation (2) is reestimated on the subsample of counties straddling a state border. All results are virtually unchanged, with significant

Table 4—House Prices, Housing Stock, and Deregulation

	House prices			Housing stock	
	(1)	(2)	(3)	(4)	(5)
Index of interstate branching deregulation	0.0103**	0.0242***	0.0122***	0.00176***	0.00028
	(0.004)	(0.005)	(0.002)	(0.0005)	(0.0002)
Index of interstate branching deregulation × house supply elasticity		−0.008***	−0.005***		
		(0.002)	(0.000)		
Index of interstate branching deregulation × house supply inelasticity				−0.00258***	−0.00083***
				(0.0006)	(0.0003)
Lagged house price			0.482***		
			(0.029)		
Lagged housing stock					0.6204***
					(0.062)
Income per capita			0.043		0.110***
			(0.044)		(0.026)
Lagged income per capita			0.070***		0.0279***
			(0.026)		(0.009)
Population			0.590***		0.777***
			(0.124)		(0.029)
Lagged population			0.243*		−0.4962***
			(0.129)		(0.069)
Herfindahl			0.001		0.0006*
			(0.002)		(0.0003)
Lagged Herfindahl			0.002		0.0004*
			(0.001)		(0.0002)
Observations	12,214	10,627	9,735	10,627	9,735
Number of counties	1,021	887	886	887	886
Number of states	50	48	48	48	48
R^2	0.199	0.202	0.428	0.048	0.737

Notes: County level linear regressions of the log change in house prices (columns 1 to 3) or the log change in the housing stock (columns 4 and 5) on the Rice and Strahan (2010) Index of Branching Deregulation and its interaction with the Saiz's Index of Housing Supply Elasticity (columns 1 to 3) or its inverse (columns 4 and 5). All variables are defined in online Appendix Table OA1. The sample includes all US urban counties for which mortgage data are available for the period 1994–2005. The index of interstate branching deregulation ranges from 0 (most restricted) to 4 (least restricted). Regressions include county and year fixed effects. Standard errors are clustered by state.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

effects of deregulation on house prices and housing stocks, even in this reduced sample where significance is harder to obtain. The results in this section suggest the relaxation of branching regulations has a causal and sizeable impact on house prices. A natural interpretation is that bank branching deregulation affects the supply of mortgage credit, which in turn stimulates the demand for houses. The next section investigates rigorously the empirical validity of this channel.

B. *The Credit Channel: An Instrumental Variable Approach*

This section investigates whether the expansion in credit triggered by deregulation *causes* the response of house prices. This is done by combining the intuitions from equations (1) and (2). Consider the instrumental variable (IV) estimation of

$$(3)\quad \ln P_{c,t} - \ln P_{c,t-1} = \delta_1\big(\widehat{\ln L_{c,t} - \ln L_{c,t-1}}\big) + \delta_2 \mathbf{X}_{c,t} + \alpha_c + \gamma_t + \varepsilon_{c,t},$$

where $\widehat{\ln L_{c,t} - \ln L_{c,t-1}}$ is the prediction associated with the first-stage regression

$$(4) \qquad \ln L_{c,t} - \ln L_{c,t-1} = \beta_1 D_{s,t-1} + \beta_2 \mathbf{X}_{c,t} + \alpha_c + \gamma_t + \varepsilon_{c,t}.$$

Equation (3) continues to include conventional controls for house price dynamics. The system formed by equations (3) and (4) investigates econometrically the relevance of branching deregulation to account for the cross section in the growth rate of mortgage variables $L_{c,t}$, and ultimately for the cross section of house prices, $P_{c,t}$.

Table 5 presents regression results for the three significant measures of $L_{c,t}$ in Section II: the number and volume of loans, and the loan to income ratio.

Appendix Table A4 presents the results for the reduced sample of counties adjacent to a state border. Both tables report F-tests for weak instruments, that evaluate the null hypothesis that the instruments $D_{s,t}$ are excludable from the first stage regression (4). Stock, Wright, and Yogo (2002) recommend the F-statistics should take values above 10, lest the end estimates become unreliable. Branching deregulations satisfy the recommendation in two specifications in Table 5, and in all three specifications in Appendix Table A4: the dispersion in credit conditions across counties is well explained by $D_{s,t}$.

The estimates of δ_1 are always significant in Table 5 and Appendix Table A4. The expansion of the mortgage market that was triggered by branching deregulation has explanatory power for house prices. Growing volume and number of loans, and growing loan to income ratios, once instrumented by $D_{s,t}$, result in rising house prices. The elasticity estimates are remarkably stable, at 0.12. Thus, a 1 percent *exogenous* increase in the growth rate of credit *results* in a 0.12 percent increase in the growth rate of house prices. This is the impact response. The dynamic responses implied by the estimates in Table 5 can once again be computed using the linear projection methodology introduced by Jorda (2005). They are plotted in Figure 3. The response of the growth in house prices to an exogenous shock to the supply of credit are long lived: they peak two years after the credit shock, with an elasticity equal to 0.2, and peter down almost linearly to become insignificant five to six years after the shock. The peak elasticity estimate of 0.2 means the blanket lifting of all four restrictions to interstate branching can imply close to a one percent increase in the growth rate of house prices.

How much of observed house prices in the United States can be explained by the expansion in credit triggered by deregulation? The question is once again answered with two in-sample predictions, based on the two fitted values of credit on deregulation represented in Figure 2. In the first case, the upper bound for the dynamics of credit in Figure 2 are used to fit house prices in equation (3), assuming the elasticity of house prices is always 0.12. This clearly constitutes an upper bound. In the second case, the lower bound for the dynamics of credit in Figure 2 is used to fit house prices, and in addition the elasticity of house prices is allowed to die out over time, as it does in Figure 3. This constitutes a lower bound. Figure 4 plots the resulting predicted house prices. The expansion in credit caused by deregulation can explain virtually all of the increase in house prices until 2002, and between a third and one-half of the increase between 2002 and 2005.

Table 5—Instrumental Variable Regressions for House Prices

	House prices		
	(1)	(2)	(3)
Instrumented number of loans	0.141**		
	(0.069)		
Instrumented loan volume		0.134**	
		(0.062)	
Instrumented loan to income ratio			0.120**
			(0.057)
Lagged house price	0.507***	0.457***	0.473***
	(0.029)	(0.039)	(0.037)
Income per capita	0.021	0.007	0.061***
	(0.031)	(0.029)	(0.020)
Lagged income per capita	0.076**	0.056	0.116***
	(0.037)	(0.037)	(0.042)
Population	−0.071	−0.067	0.277
	(0.334)	(0.310)	(0.243)
Lagged population	0.220*	0.195	0.094
	(0.131)	(0.142)	(0.299)
Herfindahl	0.021**	0.018**	0.015*
	(0.010)	(0.009)	(0.008)
Lagged Herfindahl	0.004	0.003	0.004
	(0.004)	(0.004)	(0.004)
First stage F-test of excluded instruments	10.13	10.31	8.59
Underidentification test (p-values)	0.000	0.000	0.000
Observations	11,107	11,107	11,107
Number of counties	1,018	1,018	1,018
Number of states	50	50	50

Notes: Second stage county level linear regressions of an IV specification of the log change in house prices on the number of loans, the loan volume, or the loan to income ratio of commercial banks. Number of loans, loan volume, and loan to income ratio are instrumented with the Rice and Strahan (2010) Index of Branching Deregulation. All variables are defined in online Appendix Table OA1. The sample includes all US urban counties for which mortgage and house price data is available for the period 1994–2005. Regressions include county and year fixed effects. Standard errors are robust to heteroskedasticity and autocorrelation.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

C. *The Credit Channel: Diversification and the Terms of Credit*

The evidence is that an exogenous deregulation triggered an expansion in the supply of mortgage credit in the United States between 1994 and 2005. More mortgage loans were originated, which shifted the demand for housing. In counties where construction is inelastic, the price of housing increased, while it is the stock of houses that increased where its supply is relatively elastic. It is tempting to conclude from this chain of causal events that branching deregulation helped banks lend at improved conditions, thus expanding the credit market.

If such is the mechanism at play, clues should be apparent in banks' balance sheets, in terms of deposit growth, deposit costs, mortgage rates, the fraction of nonperforming loans, and profitability. To explore this possibility, data from the

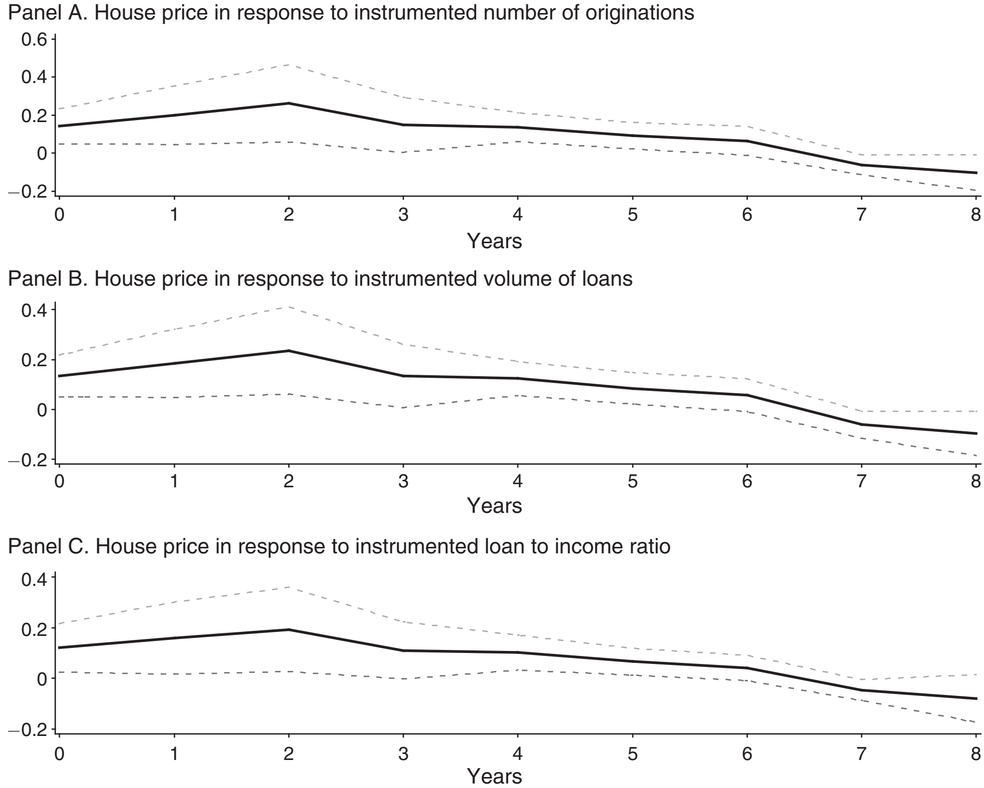

Figure 3. House Prices: Impulse Responses to Instrumented Credit Shock

Note: Dashed lines are 90 percent confidence bands.

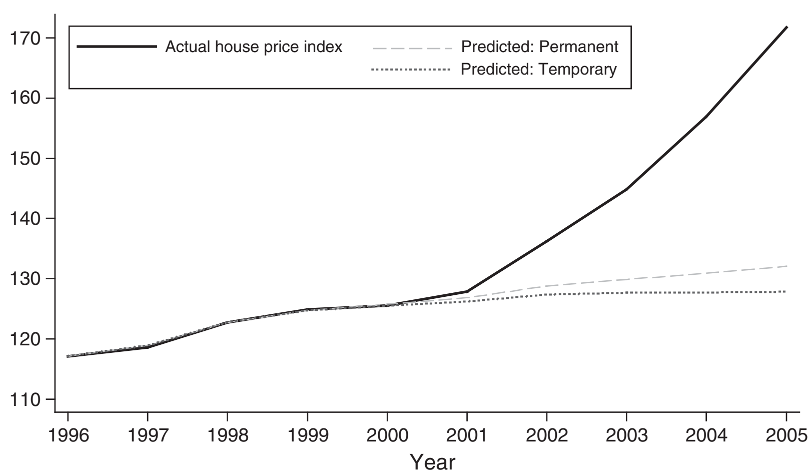

Figure 4. Actual and Predicted Real House Price Index

Note: This figure plots the aggregate real house price index between 1994 and 2005, and the two predicted indexes (dashed and dotted lines) as described in Section IIIB.

Call Reports were collected for each commercial bank in HMDA, and aggregated up to county level. A weighted average of each variable of interest was computed for each county, using each lender's loan volume as weight. Importantly, these are not loan-specific data: for instance, mortgage rates are given by a weighted average of interest and fee income for real estate loans collected for the banks that are active in each considered county, divided by total loans. Such an aggregation acts to dilute any effect, and works against finding any significant response to deregulation.

Table 6 presents the results of an estimation of equation (1) where the dependent variable is in turn each of the five measures collected from the Call Reports. All regressions include the same county-level controls as in the rest of the paper, as well as an additional set of bank-specific variables, aggregated to county level like the dependent variables.[23]

The results are informative. First, as expected, deposit growth increased, and deposit costs fell for deregulated banks. This is consistent with the existence of diversification gains as interstate branching is permitted (see, for instance Berger, Demsetz, and Strahan 1999). Second, interest rates on mortgages fell with deregulation, while profitability remained unchanged. Some of the cost savings were thus passed through into mortgage rates, at the expense of profitability. Lower mortgage rates can mean more lending to existing customers, or lending to new, presumably riskier borrowers. Column 4 in Table 6 suggests the proportion of nonperforming loans remained unchanged, presumably because lower interest rates sustained riskier borrowers at unchanged performance. Balance sheet data are therefore consistent with increased access to property as a result of branching deregulation, with sizeable effects on house prices, but not at the expense of loan performance.

D. *Robustness*

The online Appendix reports the results of three additional exercises. Online Appendix Table OA6 verifies that the impact of deregulation on house prices continues to be significant over three-year intervals. The table reports estimates of equation (2) for three-year average values of the growth rates in $P_{c,t}$ and $\mathbf{X}_{c,t}$. The estimated values of β_1 are comparable with those in Table 4, and the consequences of deregulation on house prices continue to be muted in counties where construction is elastic. By analogy with Table OA5, online Appendix Table OA7 verifies the absence of any effect when house prices between 1990 and 1994 are regressed on subsequent deregulation, between 1996 and 2000. If anything, the effect is slightly negative, and significant at the 10 percent confidence level: there is no evidence that the increase in house prices documented in the body of the paper preexisted the period considered there.

Finally, online Appendix Table OA8 considers the question of arbitrage. When state s deregulates and mortgage rates fall there, properties that are purchased across, but near the border could be financed by loans originated in state s. In other

[23] The bank-specific variables are: bank equity capital to assets, liquid assets to total assets, and total loans to assets. The county-specific controls continue to be: current and lagged log change in county's income per capita, population, house price, and the Herfindahl index of loan concentration.

Table 6—Commercial Banks: Balance Sheet Data

	Mortgage rate	ROA	Cost of deposits	NPL	Deposit growth
Panel A. Full sample					
Deregulation	−0.00483***	0.00003	−0.00016***	0.00013	0.0436**
	(0.00123)	(0.00005)	(0.00006)	(0.00016)	(0.0167)
Observations	11,136	11,147	11,147	10,607	10,020
Number of counties	1,018	1,018	1,018	1,018	1,018
Number of states	50	50	50	50	50
R^2 within	0.531	0.357	0.955	0.070	0.274
Panel B. Border sample					
Deregulation	−0.00336***	0.00001	−0.00023***	0.00002	0.0533***
	(0.00078)	(0.00004)	(0.00008)	(0.00027)	(0.01736)
Observations	2,921	2,924	2,924	2,837	2,631
Number of counties	267	267	267	267	267
Number of states	35	35	35	35	35
R^2 within	0.543	0.382	0.946	0.122	0.299

Notes: Dependent variables are mortgage rate (the ratio of interest and fee income on mortgage loans to total mortgage loans), ROA (net income over assets), cost of deposits (interest expenses on deposit to total deposits), NPL (nonperforming mortgage loans to total mortgage loans), deposit growth (log change in total deposits). The regressors are: the Rice and Strahan (2010) Index of Interstate Branching Deregulation, bank equity capital to assets, liquid assets to total assets, total loans to assets, as well as current and lagged log change in county's income per capita, population, house price, and the Herfindahl index of loan concentration. All variables are defined in online Appendix Table OA1. Panel A reports regressions for the full sample of US counties in urban areas. Panel B reports regression results for US counties in MSAs straddling two or more US states. The index of interstate branching deregulation ranges from 0 (most restricted) to 4 (least restricted). All regressions include county and year fixed effects. Standard errors are clustered by state.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

words, borrowers could cross the border to contract a loan at better terms than in their home state. As this happens, credit should increase on both sides of the border since the data are collected according to the location of the purchase. Credit should also increase in state s' where regulation has not changed, and so should house prices. Since this paper's estimations are identified on the basis of differential responses across state borders, coefficient estimates should be insignificant. Of course, arbitrage will only happen in the vicinity of the state border. Online Appendix Table OA8 presents estimations of equations (1) and (2), performed on the reduced sample of border counties, but splitting further the sample into loans that were originated to finance properties that are located at different distances from the border. Interestingly, estimates of β_1 are insignificant for properties located less than 15 miles from the border; they become significant for properties between 15 and 30 miles, and they are both significant and larger in values for properties located more than 30 miles from the border. This suggests arbitrage does happen for properties located less than 15 miles from the border, so much so that no differential effects can be observed. Arbitrage may still happen for distances above 30 miles, but it is not so prevalent as to completely obscure the differential effects of interest in this paper.

IV. Conclusion

There is a causal chain going from an expansion in credit to house prices. This is illustrated using the lifting of branching restrictions that has taken place in the United States since 1994, and examining its consequence on the mortgage market. Causality is established within two placebo samples. First, the only banks that expanded credit in response to the deregulation were those within the purview of the law. Independent mortgage companies, thrifts, and credit unions did not react. Second, the only banks that expanded credit were headquartered out of the deregulating state *and* opened branches in it. Lenders headquartered in the deregulating state did not respond. Both differentiated responses rule out the possibility that credit expanded because of (expected) soaring demand, which then could have motivated the deregulation. If it had, all banks would have expanded credit.

It must be that commercial banks could expand credit because the deregulation allowed them to improve the geographic diversification of their portfolio. Indeed, the balance sheets of banks operating in deregulating states suggest that they experienced significantly higher deposit growth, and lower deposit costs. They also charged significantly lower rates, presumably because some of the cost savings were passed through to borrowers. Credit terms improved, more borrowing happened, and the demand for housing increased. In areas where housing supply is inelastic, the response of house prices was pronounced. It was muted in areas where housing supply is elastic.

The causal link going from deregulation to an expansion of credit and house prices is economically meaningful. Deregulation can explain up to two-thirds of the observed increase in mortgage loans originated by commercial banks in the United States between 1994 and 2005. And in an instrumental variable sense, the increase in credit due to deregulation can explain up to one half of the changes in house prices observed over the same period.

Appendix A

This Appendix presents the main estimation results on the sample of counties in MSAs bordering two or more states (sample of contiguous counties).

Table A1—Commercial Banks versus Placebo Samples in Contiguous Counties

	Number of originations	Volume	Number of denials	Loan to income ratio	Number sold
Panel A. Commercial banks					
Deregulation	0.038***	0.042***	−0.003	0.043***	0.020**
	(0.010)	(0.012)	(0.018)	(0.012)	(0.010)
Observations	2,885	2,885	2,866	2,885	2,829
Number of counties	267	267	267	267	267
Number of states	35	35	35	35	35
R^2 within	0.106	0.119	0.378	0.112	0.117
Panel B. Independent mortgage companies, thrifts, and credit unions					
Deregulation	0.008	0.010	0.006	0.011*	−0.006
	(0.005)	(0.006)	(0.017)	(0.007)	(0.010)
Test	[0.000]	[0.001]	[0.920]	[0.000]	[0.113]
Observations	2,796	2,796	2,796	2,796	2,630
Number of counties	266	266	266	266	265
Number of states	35	35	35	35	35
R^2 within	0.345	0.262	0.605	0.257	0.382

Notes: Dependent variables are the log change in the number or volume of mortgages originated, the number of applications denied, the loan to income ratio, and the number of mortgages originated and securitized. Regressors are: the Rice and Strahan (2010) Index of Interstate Branching Deregulation, a lagged dependent variable, current and lagged log change in county's income per capita, population, house price, and the Herfindahl index of loan concentration. All variables are defined in online Appendix Table OA1. The sample includes all US counties in MSAs straddling two or more US states, and for which mortgage data are available for the period 1994–2005. Panel A reports regression results for mortgage loans originated by commercial banks. Panel B reports regression results for the placebo sample of mortgage loans originated by independent mortgage companies, thrifts, or credit unions, The index of interstate branching deregulation ranges from 0 (most restricted) to 4 (least restricted). All regressions include county and year fixed effects. Standard errors are clustered by state. "Test" reports p-values associated with the null hypothesis that the coefficients in panel A are zero and equal to those in panel B.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table A2—The Importance of Bank Location and of Bank Branches in Contiguous Counties

	Number of originations	Volume	Number of denials	Loan to income ratio	Number sold	Number local branches
Panel A. Out-of-state banks: local branches						
Deregulation	0.162*	0.186**	0.071	0.187**	0.118	0.103**
	(0.083)	(0.090)	(0.088)	(0.089)	(0.139)	(0.039)
Observations	1,348	1,348	1,192	1,348	897	1,423
Number of counties	225	225	215	225	199	230
Number of states	32	32	32	32	31	33
R^2 within	0.174	0.150	0.228	0.148	0.249	0.177
Panel B. Out-of-state banks: no branches						
Deregulation	0.013	0.013	−0.011	0.013	0.017	—
	(0.009)	(0.010)	(0.023)	(0.011)	(0.011)	
Test	[0.114]	[0.096]	[0.659]	[0.092]	[0.238]	
Observations	2,630	2,630	2,613	2,630	2,586	—
Number of counties	267	267	267	267	267	—
Number of states	35	35	35	35	35	—
R^2 within	0.143	0.126	0.448	0.115	0.159	—
Panel C. In-state banks						
Deregulation	0.030	0.026	0.003	0.027	−0.030	−0.008
	(0.018)	(0.020)	(0.029)	(0.020)	(0.030)	(0.011)
Test	[0.049]	[0.084]	[0.720]	[0.078]	[0.188]	[0.025]
Observations	2,583	2,583	2,473	2,583	2,345	2,424
Number of counties	266	266	264	266	262	259
Number of states	35	35	35	35	35	35
R^2 within	0.07	0.145	0.141	0.105	0.164	0.061

Notes: Dependent variables are the log change in the number or volume of mortgages originated, number of applications denied, loan to income ratio, number of mortgages originated and securitized, and the log change in the number of local branches (in panels A and C). Regressors are: the Rice and Strahan (2010) Index of Interstate Branching Deregulation, a lagged dependent variable, current and lagged log change in county's income per capita, population, house price, and the Herfindahl index of loan concentration. All variables are defined in online Appendix Table OA1. The sample includes all US counties in MSAs straddling two or more US states, for which mortgage data are available for the period 1994–2005. Panel A reports results for out-of-state-banks with local branches, i.e., lending in a location outside of the state where they are headquartered, and where they have at least one branch. Panel B reports results for out-of-state-banks without local branches, i.e., lending in a location outside of the state where they are headquartered, and where they have no branch. Panel C reports regression results for in-state banks, i.e., lending for a property in the state they are headquartered. The index of interstate branching deregulation ranges from 0 (most restricted) to 4 (least restricted). All regressions include county and year fixed effects. Standard errors are clustered by state. "Test" reports p-values associated with the null hypothesis that the coefficients in panel A are zero and equal to those in panels B and C.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table A3—House Prices, Housing Stock, and Deregulation in Contiguous Counties

	House prices			Housing stock	
	(1)	(2)	(3)	(4)	(5)
Index of interstate branching deregulation	0.0105***	0.0234***	0.0124***	0.00139***	0.00035
	(0.003)	(0.006)	(0.002)	(0.0004)	(0.0002)
Index of interstate branching deregulation × house supply elasticity		−0.007***	−0.004***		
		(0.002)	(0.000)		
Index of interstate branching deregulation × house supply inelasticity				−0.00203***	−0.00099***
				(0.00051)	(0.00026)
Lagged house price			0.515***		
			(0.041)		
Lagged housing stock					0.455***
					(0.132)
Income per capita			0.1486**		0.015***
			(0.048)		(0.003)
Lagged income per capita			0.057		0.0051
			(0.047)		(0.004)
Population			0.543***		0.780***
			(0.176)		(0.021)
Lagged population			0.355		−0.436***
			(0.175)		(0.114)
Herfindahl			−0.002		0.001*
			(0.003)		(0.000)
Lagged Herfindahl			−0.002		0.0005*
			(0.002)		(0.0002)
Observations	3,204	3,048	2,794	3,048	2,794
Number of counties	267	254	254	254	254
Number of states	35	33	33	33	33
R^2	0.184	0.235	0.537	0.082	0.750

Notes: County level linear regressions of the log change in House Prices (columns 1 to 3) or the log change in the Housing Stock (columns 4 and 5) on the Rice and Strahan (2010) Index of Branching Deregulation and its interaction with the Saiz's Index of Housing Supply Elasticity (columns 1 to 3) or its inverse (columns 4 and 5). All variables are defined in online Appendix Table OA1. The sample includes all US counties in MSAs straddling two or more US states, and for which mortgage and house price data are available for the period 1994–2005. The index of interstate branching deregulation ranges from 0 (most restricted) to 4 (least restricted). Regressions include county and year fixed effects. Standard errors are clustered by state.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table A4—Instrumental Variable Regressions for House Prices in Contiguous Counties

	House prices		
	(1)	(2)	(3)
Instrumented number of loans	0.123***		
	(0.044)		
Instrumented loan volume		0.123***	
		(0.045)	
Instrumented loan to income ratio			0.120***
			(0.044)
Lagged house price	0.492***	0.447***	0.448***
	(0.038)	(0.048)	(0.046)
Income per capita	−0.027	−0.050	0.019
	(0.097)	(0.101)	(0.086)
Lagged income per capita	−0.001	−0.046	−0.018
	(0.058)	(0.071)	(0.064)
Population	−0.241	−0.246	−0.172
	(0.297)	(0.312)	(0.285)
Lagged population	0.428**	0.319	0.331
	(0.184)	(0.209)	(0.206)
Herfindahl	0.008	0.005	0.005
	(0.006)	(0.007)	(0.007)
Lagged Herfindahl	0.011	0.013*	0.013*
	(0.007)	(0.007)	(0.007)
First stage F-test of excluded instruments	15.74	12.12	12.34
Underidentification test (p-values)	0.000	0.000	0.000
Observations	2,914	2,914	2,914
Number of counties	267	267	267
Number of states	35	35	35

Notes: Second stage county level linear regressions of an IV specification of the log change in house prices on the number of loans, the loan volume, or the loan to income ratio of commercial banks. Number of loans, loan volume, and loan to income ratio are instrumented with the Rice and Strahan (2010) Index of Branching Deregulation. All variables are defined in online Appendix Table OA1. The sample includes all US counties in MSAs straddling two or more US states, and for which mortgage and house price data is available for the period 1994–2005. Regressions include county and year fixed effects. Standard errors are robust to heteroskedasticity and autocorrelation.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Appendix B

This Appendix contains two figures that illustrate the geographic dispersion of the Rice and Strahan (2010) branching deregulation index over three-year intervals (Figure B1), and the geographic coverage of the urban counties used in the main regression analysis (Figure B2).

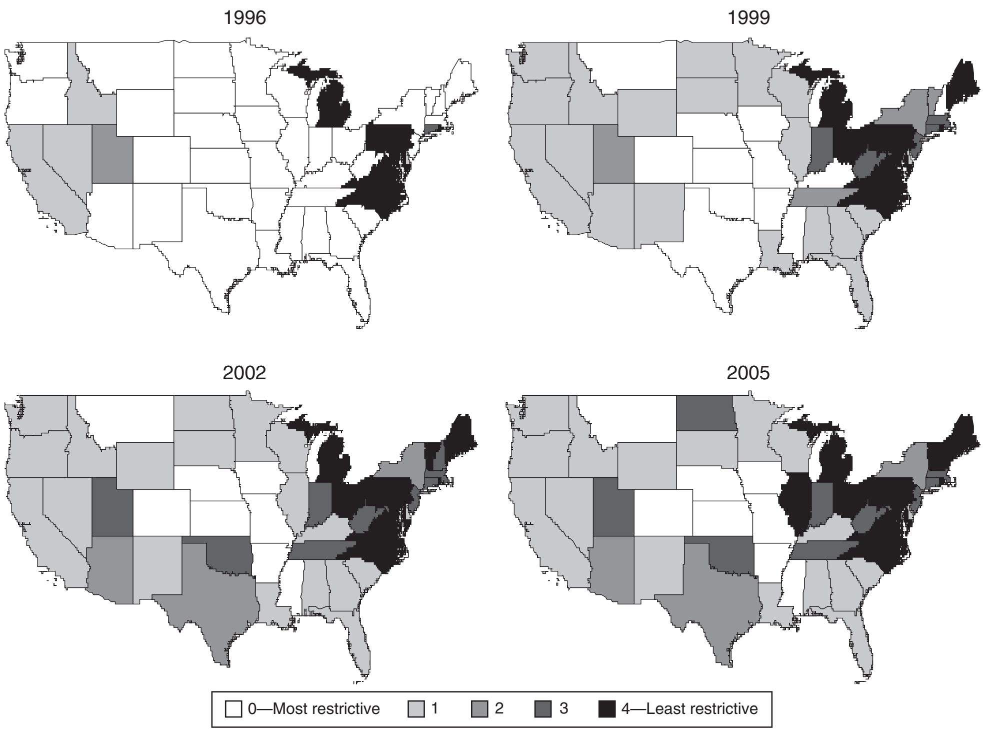

Figure B1. Rice-Strahan (2010) Deregulation Index by State and Year

Source: Rice and Strahan (2010).

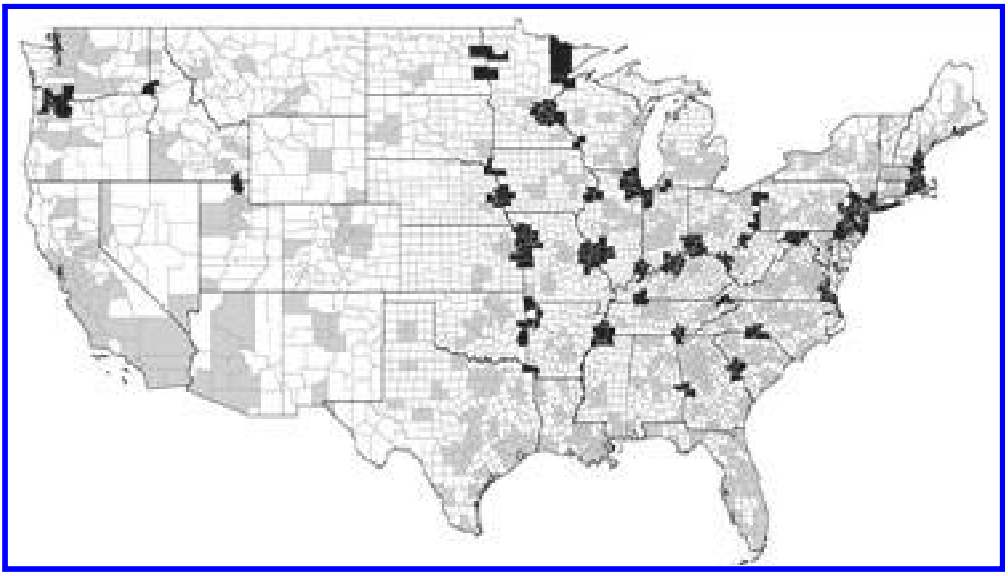

Figure B2. Sample of US Urban Counties (*Gray*)
and Counties in MSA Bordering Two or More States (*Black*)

Source: HMDA and Moody's.com.

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