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Democratizing Entry: Banking Deregulations, Financing Constraints, and Entrepreneurship*

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Abstract

We examine entrepreneurship and creative destruction following US banking deregulations using Census Bureau data. US banking reforms brought about exceptional growth in both entrepreneurship and business closures. Most of the closures, however, were the new ventures themselves. Although we do find evidence for the standard story of creative destruction, the most pronounced impact was a massive increase in churning among new entrants. We argue that creative destruction requires many business failures along with the few great successes. The successes are very difficult to identify *ex ante*, which is why democratizing entry is an important trait of well-functioning capital markets.

JEL Classification: E44, G21, L26, L43, M13.

Key Words: banking, financial constraints, entrepreneurship, creative destruction, growth.

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1 Introduction

A number of recent studies find a positive relationship between financial development and economic growth across countries (e.g., Beck et al. 2000; Levine 1997; Levine et al. 2000). This research argues that better financing environments are associated with higher economic growth, at least in part, because more efficient financial sectors facilitate better *ex ante* allocation of capital across investment opportunities. By reducing distortions like cronyism, scarce financing is reallocated to the most qualified entrepreneurs, inefficient incumbents are displaced, and product markets improve due to Schumpeterian creative destruction (e.g., King and Levine 1993a,b; Rajan and Zingales 2003; Aghion et al. 2007; Chun et al. 2008).

While the cross-country relationship between finance and growth is well documented, empirical work evaluating at the firm level how entrepreneurship and creative destruction follow from improved financial conditions is sparse. Most research on financing constraints considers established firms (e.g., Banerjee and Duflo 2004; Fazzari et al. 1988; Kaplan and Zingales 1997, 2000; Moyen 2004; Paravisini 2008) or the transition of individuals into entrepreneurship (e.g., Evans and Jovanovic 1989; Gentry and Hubbard 2000; Hurst and Lusardi 2004; Nanda 2008). Only a handful of studies examine how changes in financial markets impact firm entry and exit in product markets (e.g., Black and Strahan 2002; Guiso et al. 2004; Cetorelli and Strahan 2006; Zarutskie 2006; Bertrand et al. 2007).

We study how US branch banking deregulations impacted US entrepreneurship rates and incumbent firm displacement. These reforms, enacted by individual states from the 1970s onwards, allowed bank entry across state borders and ended local banking monopolies. Bank debt comprises the majority of US firm borrowings, and new ventures are especially sensitive to local banking conditions due to their limited options for external finance (e.g., Petersen and Rajan 1994; Fluck et al. 1998; Berger and Udell 2002). Reducing distortions in the banking sector can thus have first-order effects on entrepreneurship and creative destruction in product markets. Prior work for the US documents substantial increases in startup activity and to some degree productivity growth following branch banking deregulations (e.g., Jayaratne and Strahan 1996; Black and Strahan 2002; Cetorelli and Strahan 2006; Huang 2008).

Our central contribution is establishing the close link between firm entry and exit patterns following US banking reforms. While we find some evidence that supports the standard mechanism espoused for creative destruction, the US experience was much, much messier than the *ex ante* story would suggest. US banking reforms brought about exceptional growth in both entrepreneurship rates and business closures. Most of these closures, however, were new ventures themselves, rather than incumbents. Indeed, the greatest increase in entry occurred among very

small startups that failed within three years of founding. Certainly, some entrants did go on to challenge incumbents *ex post*, but these were only a fraction of new firm foundings.

Separating this churning entry from long-term entry is possible due to the micro-data from the US Census Bureau's Longitudinal Business Database (LBD). The LBD provides annual employment data for every US establishment from 1976 onwards. The panel structure of the data affords calculations of entry rates, entrant sizes, and subsequent survival of new companies. We also track employment shares for incumbent firms by state and industry to quantify realized displacement effects following from entrepreneurship.

This churning entry helps explain why prior work has found that interstate reforms resulted in entry increasing by over 10% a year (e.g., Black and Strahan 2002) but no measured effects on the firm size distribution (e.g., Cetorelli and Strahan 2006). Likewise, short-lived entrants partially explain why Jayaratne and Strahan (1996) do not find that economic growth accelerates after the interstate deregulations, while they do find growth effects following intrastate reforms. More generally, our results emphasize that failure is a very real part of the entrepreneurial process. Roughly half of startups close within five years of entry, even among entrants selected and supported by sophisticated venture capitalists in well-developed capital markets. It would thus take exceptionally strong improvements by banks in *ex ante* project selection to have growth in entry rates and displacement effects occur in lockstep. Instead, the data argue for a more mundane story of creative destruction. US financial reforms democratized entrepreneurship by facilitating widespread entry. While US reforms did lead to enhanced competition from longer-term entrants and a reduction in incumbent market power, the most pronounced impact was a massive increase in churning among the smallest entrants.

Linking entry with exit also contributes to our general understanding of how product markets are influenced by improved financial sector efficiency (e.g., Bertrand et al. 2007; Cetorelli and Strahan 2006; Cetorelli 2004; Beck et al. 2008; Levine et al. 2008). Our analysis of the entry size distribution and long-term survival provides evidence consistent with US deregulations reducing the importance of being 'insiders' or privileged clients for receiving financing (e.g., Jayaratne and Strahan 1996; Laeven 2000; Rajan and Zingales 2003). These results complement the Bertrand et al. (2007) study of the French banking deregulations' impact for firms with over 100 employees and the Guiso et al. (2004) study of financial development in Italy. Our close attention to smaller firms and failure rates, however, also emphasizes that a substantial share of product market gains come *ex post* by simply encouraging the general entrepreneurial process. These findings therefore paint a more nuanced picture of how financial market deregulations engender creative destruction.

Our second contribution comes through comparisons of startup births and deaths with facility openings and closures by existing firms. We argue in Section 4 that new establishments

being opened by multi-unit firms provide a natural baseline against which to measure impacts for entrepreneurship. We thus use a differences-in-differences empirical approach to identify the effects of deregulations using variation within state-industry-year cells only. From an econometric perspective, this technique provides better identification than prior studies due to reduced scope for results being driven by omitted variables. From a substantive perspective, we better isolate how reforms impacted startups from general economic conditions. This technique may find application in other settings, too.

Our final contribution is to study separately the intensive and extensive margins of entry. Average entry size is a blunt measure for whether eased financing constraints yielded larger entrants. Lower financing constraints may facilitate larger entry sizes for firms that would have entered regardless (e.g., Evans and Jovanovic 1989; Cabral and Mata 2003), an intensive margin effect that would promote higher average entry sizes. If deregulations also influence entry rates, however, average entry sizes will capture changes on both the intensive and extensive margins. In fact, we show massive entry of very small firms that would tend to decrease average entry size. We thus study intensive margin effects through the entry size distribution and the size of entrants in their first year compared to subsequent growth. Better financing environments helped promote larger entrants among those that survived more than three years.

Section 2 provides an overview of US branch banking deregulations and theoretical predictions of how banking competition should affect entrepreneurship. Section 3 introduces the LBD and describes US entry patterns. Section 4 outlines our identification strategy and presents the results. Section 5 concludes our study by identifying further how our results fit into the literature and areas for future research.

2 US Branch Banking Deregulations

Our empirical approach exploits cross-state variation in the timing of US branch banking deregulations. Prior to these liberalizations, US banks faced multiple restrictions on geographic expansion both within and across states. The 1970s through the mid 1990s experienced a significant liberalization in the ability of banks to establish branches and to expand across state borders, either through new branches or acquisitions. This section describes these deregulations and discusses theoretical impacts for entrepreneurship due to greater bank competition.

States historically restricted banking within their borders as a means of public finance. The McFadden Act of 1927 required national banks obey state-level restrictions on branching, effectively prohibiting cross-state banking. In addition, many states developed stringent rules governing the conduct of branch banking within their territories. The most restrictive of these,

known as unit banking, limited each bank to a single branch. Although banks responded to these restrictions by forming multibank holding companies (MBHCs) that owned more than one bank, states in turn restricted activities of MBHCs. Restrictions on intrastate branching for MBHCs focused on the market share and concentration of these holding companies, while the Douglas Amendment of 1956 prevented a MBHC from owning banks across state borders.

Two classes of restrictions were eased in the 1970s through 1990s. First, intrastate deregulations for branch banking allowed banks to expand within the passing state if they were licensed to operate there. One version of this reform facilitated expansion via mergers and acquisitions, while a second version allowed the opening of *de novo* branches. Most states introduced these two variants at about the same time, and we model their leading edge for each state. The ability to expand within states allowed for more competition in local banking markets, in some cases even breaking-up effective monopolies that existed prior to these liberalizations.

Second, interstate deregulations allowed banks to acquire branches in other states with which their 'home state' had negotiated such a bilateral agreement. This class of reforms further reduced the monopoly power of local banks, in particular due to the significant improvements in the market for corporate control. Interstate deregulations may have also improved economies of scale, although Berger et al. (2001) argue that subsequent bank mergers resulted in few cost savings on average. In part due to reciprocal nature of these agreements, most states undertook interstate deregulations in the mid 1980s to early 1990s.

These state-level reforms culminated in the Riegle-Neal Interstate Banking and Branching Efficiency Act of 1994. The Riegle-Neal Act opened up nationwide acquisition of banks across state lines, regardless of bilateral agreements, unless a state explicitly opted out. In effect, the Riegle-Neal Act put out-of-state banks on par with local banks in every state, with important implications for capital reserves and banking efficiency across the industry. In addition, the Riegle-Neal Act allowed banks to set up new branches across state borders without the need to acquire a subsidiary bank, and MBHCs could convert subsidiaries into branches. Kane (1996) carefully discusses the Riegle-Neal Act.

Only 12 states had some form of intrastate deregulation prior to 1970, and no state allowed interstate branch banking. Starting in the 1970s, and especially in the 1980s, most states passed both forms of deregulations. Figure 1 plots the cumulative number of states adopting each reform by year. The appendix lists branch banking deregulations for each state.

Accounts of the political economy of these reforms suggest their passage are mostly exogenous to product markets, driven in part by federal actions and state-level structures of the banking industry. Black and Strahan (2001) argue that some of the impetus for intrastate deregulations came from initiatives taken by the Office of the Comptroller of the Currency that put banks

with national charters on par with Savings and Loans (S&Ls) and savings banks that could branch freely within states. Interstate deregulations were driven in part by the S&L crisis in the early 1980s when federal legislators allowed failed banks and thrifts to be acquired by banks in any state, regardless of state laws governing these transactions. These changes paved the way for bilateral negotiations between states to allow interstate banking in order to foster larger, diversified banks that were less susceptible to failure. Kroszner and Strahan (1999) carefully model how the timing of state deregulations were driven by the relative strength of state interest groups for or against deregulation. After introducing our data, we show in the next section that the timings of these reforms are not correlated with pre-existing rates of entrepreneurship in states.

Interstate liberalizations led to an expansion of large MBHCs across state borders and a significant decline in small, local banks (e.g., Janicki and Prescott 2006). Table 1A documents aggregate changes in the banking sector taken from the LBD. The total number of banks fell by 30% from the mid-1970s to the mid-1990s. The share of large banks, defined as having more than 500 employees, and the share of branches controlled by large banks, increased over the same period. The fraction of branches controlled by out-of-state banks also grew from 2% to 25%, suggesting robust cross-state merger activity. These trends are mirrored in studies using bank assets to measure bank size. Berger et al. (2001) find that the decline in the number of banks is almost completely due to reduced numbers of small banks with assets under $100m. Moreover, the percentage of industry assets managed by 'megabanks' (i.e., with more than $100b in assets) almost doubled from 1977 to 1994, while the share managed by small banks halved.

Stronger bank competition and markets for corporate control due to US deregulations are thought to have improved allocative efficiency by allowing capital to flow more freely towards projects yielding highest returns. Moreover, although the number of banks fell over this period, the number of bank branches increased considerably, reflecting greater competition and increased consumer choice in local markets. From a theoretical perspective, these reforms could have had a strong positive effect on entrepreneurship if startups face substantial credit constraints. Moreover, since entrepreneurs have fewer, non-bank options for financing their projects relative to existing firms (e.g., internal cash flow, bond markets), more efficient allocation of capital within the banking industry should lead to larger increases in startup entry relative to facility expansions by existing firms.

However, there are two channels through which these reforms may instead harm startups. First, Petersen and Rajan (1995) argue that startups benefit from concentrated banking markets because a monopolist bank can engage in inter-temporal cross-subsidization of loans. As a monopolist bank can charge above-market interest rates to mature firms, they can in turn charge below-market rates to potential entrepreneurs. By doing so, the monopolist bank can maximize

the long-term pool of older firms to which they lend. Increased competition weakens the market power of local banks for mature firms, reducing their ability to charge above-market rates, and thereby weakens their incentives for charging below-market rates to new entrants as well.

Second, several studies argue that small banks have a comparative advantage relative to large banks at making lending decisions for startups because they are better at screening on 'soft' versus 'hard' information (e.g., Stein 2002; Berger et al. 2005). If lending decisions at larger banks are based on more hierarchical decision processes, ultimate adjudication decisions may come from officers who do not know potential borrowers personally. These decisions are more likely to be based on credit scoring models that inherently focus on hard information. On the other hand, local loan officers at small banks know information about borrowers that cannot be condensed into a credit score. This ability to lend and monitor based on soft information may give local loan officers a comparative advantage for entrepreneurial finance. Since US banking reforms led to a shift in industry structure from small banks towards large banks, this could have had a direct negative effect on lending to startups relative to established firms with a history of audited accounts.

The net theoretical effect of these competing channels is ambiguous, requiring empirical quantification. Figure 1 shows that introductions of intrastate and interstate deregulations are sufficiently independent across states that we can jointly investigate their effect on startup entry. We prefer to model the reforms jointly to isolate better their respective impacts, but our results are robust to treating them separately. Intrastate deregulations capture trade-offs between allocative efficiency from increased competition and potential costs to entrepreneurs from a loss of concentrated markets. Interstate deregulations capture trade-offs between efficiencies and potential costs to entrepreneurs due to shifts away from small banks. Our study therefore also tests for the presence of financing constraints in entrepreneurship.

3 Longitudinal Business Database

The LBD provides annual employments for every private-sector, US establishment with payroll from 1976 onwards. The underlying data are sourced from US tax records and Census Bureau surveys, and approximately 4m establishments and 70m employees are included in the average year. This study uses micro data spanning the period 1976-2001. The LBD's complete accounting of very small firms and establishments, which are often excluded or subsampled in corporate surveys, is very important for our analysis of entry patterns following banking deregulations. The LBD also lists physical locations of establishments rather than states of incorporation, circumventing issues like higher incorporation rates in states like Delaware. Jarmin and Miranda (2002) provide further details on the LBD construction.

The LBD assigns a firm identifier to each establishment that allows us to distinguish stand-alone firms from facilities of multi-unit firms. We develop panels of entry and exit by these two establishment types at state-year and state-industry-year levels. Entrepreneurship is defined as the entry of new, stand-alone firms. In various analyses, we further separate entrants by establishment size in year of entry and/or how long the establishment survives. The latter breakdowns are possible due to unique, time-invariant identifiers for each establishment that can be longitudinally tracked.

For each establishment, we define its years of entry and exit as the first and last years of positive employment, respectively. We do not count cases where a plant temporarily suspends operations to be an exit and re-entry. We likewise exclude corporate spin-offs. The data start in 1976, so we can define entry cohorts from 1977 onwards. In our survival analyses, we consider whether establishments survive four years or longer. To maintain consistent sample sizes across specifications, we thus close our analysis with the 1998 entry cohort. Ending in the mid 1990s is also appropriate given the passage of the Riegle-Neal Act. All of our basic entry and exit results easily extend to including the 1999-2001 cohorts.

Table 2 provides descriptive statistics on entrants in our sample. Included sectors are manufacturing, services, retail trade, wholesale trade, mining, transportation, and construction. Over 80% of the 409k new establishments opened in each year within these sectors are new startups. 58% of these entering establishments survive for four or more years. Survival rates are higher for multi-unit facility expansions at 73% versus 55%.

Figure 2 plots relative entry counts over time for startups and facility expansions, with entry counts normalized by 1977-1981 levels. While startups constitute most new establishments, their relative entry has consistently lagged that of facility expansions since the early 1980s. There is only a 10% increase in the raw number of startup entrants over the twenty-year period, despite a 20% overall growth in LBD employment. Measured in terms of rates, Davis et al. (2006) document a substantial reduction in business entry and exit from the late 1970s to the late 1990s using the LBD. Figure 2 also documents a broad decline in entry during the early 1990s. This decline is consistent with the decline in credit available to firms during this period (e.g., Berger et al. 2001; Zarutskie 2006).

These aggregate trends are important when interpreting upcoming panel estimation results. We separately control for aggregate entry or exit rates of startups and facility expansions by year to remove secular changes that differentially affect these groups (e.g., different cyclical volatilities). These aggregate trends, however, include overall movements in credit access that are partly due to deregulations. The inference of panel estimations using cross-state banking variation comes in part from greater or weaker relative declines in startup entry and exit rates for states that have deregulated versus those that have not.

While startups account for most new establishments, existing firms open new establishments at larger sizes. Facility expansions start on average with four times the employment of startups, at 24 versus 6 employees. As can be seen in Table 2, 76% of new startups begin with five or fewer employees, versus 44% for facility expansions. Churning establishments tend to enter at smaller sizes compared to long-term entrants.

Manufacturing accounts for about 10% of entry; manufacturing, services, wholesale trade, and retail trade jointly account for 75%. Exclusions noted in the comments of Table 2 lower our sample's share of services relative to overall economic activity. While the sector distributions of startups and facility expansions are generally comparable, they are quite different for retail trade and construction. Our core estimations control for these differences across SIC2 industries, and we have further confirmed that our results are robust to excluding these sectors entirely. Industrial compositions for churning versus long-term entrants are relatively similar.

Despite the well-documented concentration of high-tech entrepreneurship within regions like Silicon Valley and Boston's Route 128, the broad entry and exit rates we consider are more evenly spread across US regions. There are also no substantial differences in the extent to which startups versus existing firms open new establishments across states. These geographic regularities aid our using of cross-state variation in banking deregulations to study entrepreneurship. Dunne et al. (1989), Davis et al. (1996), and Glaeser and Kerr (2008) provide additional details on US entry patterns. Dumais et al. (2002) and Ellison et al. (2007) consider the agglomeration and coagglomeration of startups and facility expansions, respectively.

With these data, we now return to the timing of the deregulations. The exogeneity of the banking deregulations for our study would be questionable if the timing of the reforms across states were systematically associated with pre-existing establishment entry rates. The first panel of Figure 3 plots establishment entry rates of states for 1977-1980, the first four years of our sample, against the years when states passed the intrastate reform. There is no relationship evident. Figure 3b likewise shows that changes in entry rates by states from 1977-1978 to 1979-1980 are not related to the timing of the intrastate deregulations. Finally, the last two panels of Figure 3 find the same holds true for the interstate reforms. In all cases, the t-statistics for the trend lines are less than 0.8. This lack of predictive power gives us additional confidence in the empirical design.

4 Empirical Results

This section reports our empirical results. We first consider state-year panel estimations that separately examine entry and exit patterns for startups and facility expansions. These estimations provide the most intuitive presentation of our results. We then turn to stricter frameworks

that isolate startup entry and exit relative to facility expansions. We close with an analysis of market concentration.

4.1 Pre-Post Reform Analysis by State-Year

We first analyze simple panel data models at the state-year level that are traditional for this literature. These specifications take the form,

$$\ln(BIR_{s,t}) = \phi_s + \tau_t + \beta_{TRA} TRA_{s,t} + \beta_{TER} TER_{s,t} + \varepsilon_{s,t}. \quad (1)$$

$BIR_{s,t}$ are counts of entering establishments in state s and year t. We run the specification separately for startup entrants and facility expansions, and the same empirical specification (1) tests exit, churning, and long-term entry patterns as well. ϕ_s and τ_t are vectors of state and year fixed effects, respectively. State fixed effects control for fixed differences in entry across states due to factors like California's larger economic size. Year effects account for aggregate changes in entry rates over time due to business cycles, national policy changes, and so on.

The variables TRA and TER model intrastate and interstate banking deregulations, respectively, through dichotomous indicator variables. Each indicator variable takes a zero value up to the year of deregulation in state s and unit value afterwards. The LBD is collected on March 1st of each year. We thus date the reforms such that a passage of TRA in 1987, for example, is coded as changing from 0 to 1 in 1988. As $BIR_{s,t}$ is measured in logs, the β coefficients measure the mean percentage increase in a state's annual births after the specified deregulation. We cluster standard errors by state to address the serial correlation concerns for differences-in-differences estimations of Bertrand et al. (2004). We weight regressions by the log of 1977-1985 birth employment in the state; these weights do not change across specifications. Weights afford population estimations of treatment effects, but similar results are obtained in unweighted regressions.[1]

These pre-post results are reported in Panel A of Table 3. Column 1 of Table 3 finds a strong increase in startup births after the interstate deregulations. The elasticity of 6% is statistically significant and economically large in size. On the other hand, the intrastate reforms did not lead to a change in entry patterns. In general, we rarely find that intrastate reforms had consistent, material effects on this study's outcomes. We further discuss this null result in the conclusions. As a comparison, Black and Strahan (2002) find 11% and 3% elasticities using Dun & Bradstreet incorporations data to interstate and intrastate deregulations, respectively. This result is also

[1] We also include in each regression an interaction of the reforms with an indicator for an Economic Census year (i.e., 1977, 1982, ..., 1997). In these years, more resources are devoted to updating the business registry. As a result, longitudinal bumps occur in establishment entry counts for both types of firms. These interactions flexibly accommodate these shifts, although the interactions are insignificant, and their coefficients are not informative. They can be excluded without impacting the results. See Autor et al. (2007) for further details.

confirmed in Levine et al. (2008). Several data sources are thus pointing to a large impact on US entrepreneurship from the interstate deregulations.

Column 2 of Table 3 finds a 3% increase in facility expansions after interstate deregulations. This elasticity, which is also statistically significant and economically important in size, suggests that the startup entry response in Column 1 likely combines specific benefits for entrepreneurship with more general economic development that indirectly increased new firm entry, too. We will formally compare startup and facility expansion responses below to tease out the causal effect for entrepreneurship itself.

While Columns 1 and 2 could be consistent with the standard mechanism espoused for creative destruction — more efficient financial sectors promoting higher quality entrants that will displace incumbents — the remaining columns of Panel A demonstrate that the US experience was also about democratizing the entry process. Columns 3-6 separate entrants by the number of years they survive. Churning entrants, defined to be those that close within three years of founding, rose in step with entrants that survived longer. Likewise, to the extent that business closures are found to increase after the interstate reforms with specification (1), it is among single-unit firms themselves. Pre-post specifications are blunt instruments, however, for measuring these extended effects on the product markets that often take several years to materialize. We thus turn next to more nuanced specifications that portray the underlying dynamics.

4.2 Dynamic Specifications by State-Year

The panels of Figure 4 document the raw dynamics of entry associated with the banking reforms through the specification,

$$\ln(BIR_{s,t}) = \phi_s + \tau_t + \sum_{q=-10}^{10} \beta_{TRA,t+q} \Delta TRA_{s,t+q} + \sum_{q=-10}^{10} \beta_{TER,t+q} \Delta TER_{s,t+q} + \varepsilon_{s,t}. \quad (2)$$

The variables $\Delta TRA_{s,t+q}$ are twenty separate indicator variables modelling the passage of the TRA reform. These dummy variables take a value of one in the q^{th} year before or after the TRA deregulation and are zero otherwise. The -10 and +10 year endpoints include all years earlier and later than our twenty-year window. We do not include an indicator for the year of the deregulation itself, so that the β coefficients measure the year-by-year dynamics of entry relative to reform years. The TER lag structure is similarly defined. While we split the intrastate and interstate patterns into two graphs, they are estimated jointly. The dashed lines plot 95% confidence intervals for the point estimates. We also test the extended dynamics of establishment closures through model (2).

The patterns are striking. Lead effects for both reforms are relatively small, especially just prior to the reform's passage, and are not statistically different from zero. There may have been a slight rise in entry over the seven years prior to the intrastate reforms, while the opposite is true for the interstate reforms. It should be noted, however, the panel is unbalanced for earlier lead effects as our data start in 1977, well within the ten-year window for states that deregulated early. Looking after the TRA reform, no changes in entry or closures are evident. On the other hand, large increases in establishment births and closures are evident after the interstate deregulations. Moreover, entry increases after the reforms at a rate consistent with growing financial access due to greater bank competition. The appendix further reports the extended dynamics separately for single-unit and multi-unit entrants and closures. The patterns are similar across the two types of firms, with the single-unit responses to the interstate reforms exceeding the multi-unit responses and coming earlier.

We now turn to two specifications that summarize the major features of these dynamics. Panel B of Table 3 quantifies the growing treatment effects evident in Figure 4 through linear treatment effect specifications. In these specifications, TRA and TER continue to take a zero value up to the year of deregulation. They then take a value of one in the year of the reforms, a value of two in the second year after the reforms, and so on. As the treatment effects visibly flatten after four years, we cap the linear treatment at four years.

The results in Panel B are much more precisely estimated than those in Panel A. Accounting for growing treatment effects after the reforms is clearly important. Startup entry is again found to increase more after the interstate reforms than facility expansions, although the latter does increase too. The treatment effects for churning startups are substantial, rising 6% per year through the first four years. This growth effect is stronger than the long-term entrants evident in Column 5. The last two columns find that closures for startups grow with time, while the establishment closures of multi-unit firms continue to be weakly affected.

Table 4 provides a more flexible specification than the linear treatment effects model. We include four indicator variables for each reform. The first indicator variable is for the two years prior to the reform's passage. The second indicator is for the year of the reform and the following year. The third indicator is for the second and third year after the reform. The final indicator variable is for the fourth year after the reform and later. Elasticities measured through this approach are relative to the period three years or earlier before the given deregulation. This non-parametric approach is a parsimonious way of capturing the major features of the raw dynamics in Figure 4. It is also more appropriate for analyzing the LBD given the short window prior to the earliest of the reforms.

The pattern of entry effects after the interstate reforms is consistent with the earlier results. This technique estimates a 23% higher startup entry four or more years after the reform,

compared to 12% for facility expansions. Separating entrant types, the interstate reforms are associated with a 28% and 19% increase in churning and long-run entry for startups, respectively. This 19% estimate might be overstated too, as a sizeable forward effect is evident in Column 5. It should be noted, however, that forward effects are hard to interpret in churning and long-term entrant estimations. Establishments are categorized based upon survival, and changes in banking conditions and associated product market environments in period t can clearly influence whether entrants in the $t-1$ cohort survive for four years or not. Heightened closures of single-unit firms are evident four or more years after the interstate reforms, when the churning entrants begin exiting.

We have performed a number of robustness checks on these basic state-year outcomes. Appendix Tables 2 and 3 show that entry patterns are robust to including linear state time trends that center identification on discontinuities surrounding the reforms. Unreported estimations also consider responses within each sector. The basic patterns are economically and statistically important sector-by-sector, with somewhat stronger effects evident in wholesale and retail trade than manufacturing or services. The patterns are also robust to excluding influential states (e.g., Wall 2004). Excluding California has the largest effect, but point estimates only decline by about a tenth from their full sample values. While these tests provide added confidence, the Census data allow greater empirical leverage than state-year estimations for showing identification. We take this up next.[2]

4.3 Relative Entry Analysis by State-Industry-Type-Year

State-year analyses provide an intuitive presentation of our findings, but omitted variable biases are a natural concern with this estimation technique. Figure 3 did not find a pre-existing relationship between state-level entrepreneurship rates and the timing of the reforms. Nevertheless, other secular changes at the state-year level may still be biasing the parameter estimates. To address this liability, recent research exploits industry-level variation within states (e.g., Cetorelli and Strahan 2006). These studies follow Rajan and Zingales (1998) by grouping industries according to the degree to which they are dependent upon external finance or not. This additional variation allows researchers to control for state-year and industry-year fixed effects. Effects for industries dependent upon external finance are contrasted with less dependent industries before and after the reforms. While this industry differential is more robust than state-year panels, it

[2]We have also tested the mechanism implied by our reduced-form indicators for interstate deregulations. Using the LBD, we document sharp growth in out-of-state banks occurred in states after their interstate deregulations. Moreover, much of this growth was driven by large banks with an average of 500 or more employees over the period 1977-1985. These simple estimates confirm deregulation's role in the descriptive statistics outlined in Tables 1A and 1B. While we prefer the reduced-form approach of modelling deregulations, due to LBD data collection limitations for the financial sector prior to 1992, evidence for the expected mechanism of out-of-state banks is evident in the data.

naturally cannot address omitted factors that operate at the state-industry-year level. These more granular factors are particularly apt to emerge in agglomerated industries (e.g., high-tech in California, automotive in Michigan).[3]

The detailed establishment-level data in the LBD afford an even stronger approach. We specifically contrast the entry of startup firms with the entry of facility expansions by multi-unit firms. We use facility expansions, rather than firm growth through employment adjustments at existing plants, to create a baseline with similar discontinuous financing requirements. We believe that facility expansions can serve as an appropriate control group conditional on removing the aggregate differences and trends documented in Figure 2. The dynamic state-year regressions in Table 4 suggest this identification strategy is reasonable. Facility expansion patterns are very similar to startups prior to reforms, and dynamic growth patterns for facility expansions following deregulations are quite reasonable.

This differences-in-differences approach also enables us to control for state-industry-year effects. While this test is substantially more powerful than previous approaches, it is clearly not foolproof either. For example, states could have passed other reforms in parallel to banking deregulations that differentially influenced startup firms from facility expansions. While acknowledging this issue, we also believe these concerns are mitigated by both cross-state variation in the timing of deregulations and the overall economic importance of the banking deregulations. It is much more challenging to construct competing explanations when variations with state-industry-year cells are exploited. Our identification strategy also has a useful substantive interpretation in that it teases out differential responses of startups to banking deregulations over and above heightened facility expansions of existing firms. Since startups are particularly dependent on banks for external finance, these results can also be interpreted as quantifying how much more important changes in banking competition are for entrepreneurship relative to existing firms.

To implement this technique, we organize entrant counts in the LBD to be by state-industry-type-year, where "type" indicates whether the entrant is a startup firm or not. We denote industries with i and entrant types with x. The addition of 51 SIC2 industries and 2 entrant types results in over 100k state-industry-type-year cells. As an analog to the state-year pre-post estimation (1), we first examine

$$\ln(BIR_{s,i,t,x}) = \phi_{s,i,x} + \tau_{t,x} + \eta_{s,i,t} + \beta_{TRA}^{Startup} TRA_{s,t} \cdot Type_x + \beta_{TER}^{Startup} TER_{s,t} \cdot Type_x + \epsilon_{s,i,t,x}. \quad (3)$$

The fixed effects are very important for understanding this estimation. $\phi_{s,i,x}$ is a vector of cross-sectional fixed effects at the state-industry-type level similar to the state vector ϕ_s in the

[3]Variations in how external finance is defined or the time period studied can also lead to different industry groupings. Regardless, it is important that cross-sectional fixed effects be included along with state-industry-year effects in estimations. Several studies model state-year and industry-year longitudinal effects, but omit cross-sectional controls. This omission may bias estimates due to the non-proportional allocation of industries across states.

state-year analyses. Likewise, $\tau_{t,x}$ extends the earlier vector of year fixed effects τ_t to be instead by type-year. These two extensions allow startups and facility expansions to have independent panel effects as in the separated regressions of Tables 3 and 4. By doing so, we fully control for levels differences and secular changes like those noted in Figure 2. Finally, state-industry-year fixed effects $\eta_{s,i,t}$ fully absorb secular changes in local industrial conditions common to startups and facility expansions.

The TRA and TER deregulation indicators from (2) are interacted with whether the entrant type is a startup firm or not ($Type_x$). As state-industry-year fixed effects saturate the model, the dynamic coefficients for startup firms become relative to responses of facility expansions. Indeed, this specification is only possible by contrasting entrant types within state-industry-year cells, and separate coefficients for facility expansions are no longer estimated. This structure also demonstrates the comparability of our count-based estimations with entry-rate formulations relative to local cell sizes.[4]

Table 5 presents these relative models in a format similar to Table 3. Consistent with the impression given by the state-year panels, startups exhibit higher entry and exit increases after the interstate deregulations than facility expansions. The relative increase in churning is especially pronounced, which ties the growth of entry and exit together. Relative increases long-term entry, while present, are more modest in comparison to increases in churning. These results are also confirmed in the linear treatment effects specification in Panel B of Table 5.

Table 6 extends the dynamic specifications in a similar manner. These are our preferred estimates given the robustness of the estimation technique and the dynamic modelling. The first column finds that entry of startups was 11% higher than facility expansions in the long-run after interstate deregulations. This elasticity is statistically significant and economically important, and the dynamic structure of effects is also reasonable. The fourth column of Table 6 demonstrates that growth in closures for single-unit firms substantially exceeded growth in plant closures by multi-unit firms. The long-term exit differential is 17%. The second column shows again how much of this death was entrants themselves, with churning entry increasing by 22% for startups relative to facility expansions. The third column shows a more muted 6% response for long-term entry. The intrastate deregulations are again found to have limited long-term effects on startup entry, with a transitory dip evident in the second and third lags.

[4]The appendix shows how dropping state-industry-year fixed effects from (3) returns results similar to the separated state-year regressions. The only difference is the added industry dimension.
Similar to the earlier specifications, we include interactions for Economic Census years and weight the regressions by the 1977-1985 birth employments in the state-industry cell. While all state-year observations have startup and facility expansions, this is not true at the industry level. To maintain a consistent observation count in log specifications, we recode a zero entry count as one and include unreported dummies for zero count observations by type. The results are robust to dropping these observations entirely; in general, these cells receive very small weight.

In comparing coefficients across Table 6's columns, the higher relative magnitude for business closures (17%) compared to entry (11%) clearly does not suggest an absolute decline in small businesses. Table 3 shows otherwise. The differential for startup exits, relative to multi-unit facility closures, is higher due to the churning outcome. In the growing US economy, firm starts are greater in number than firm closures throughout the period studied. Adding these churning establishments to these starts and exits results in a higher elasticity for exits.

As a final note, we have tested jointly our relative startup differentials with differences across industries in dependence on external finance. Following Rajan and Zingales (1998), Cetorelli and Strahan (2006), and Aghion et al. (2007), we constructed measures of industry-level dependence on external finance from Compustat data. Relative entry elasticities for financially-dependent sectors are typically higher than those for non-dependent industries, but the differentials are often not economically large or statistically significant. Differences within manufacturing — the typical sector studied in these dependency tests — are stronger and statistically different.

4.4 Churning and Long-Term Entrant Size Distribution

We next examine the size distribution of churning and long-term entrants. Characterizing relative entry effects across the establishment size distribution provides a richer description of whether and how creative destruction followed from US deregulations. This analysis also identifies whether the extensive margin effects discussed thus far are complemented by intensive margin effects, too.

Theoretical models suggest that even if potential entrepreneurs are not precluded from starting new businesses due to financing constraints, they may still start firms that are smaller than optimal for the projects at hand (e.g., Evans and Jovanovic 1989). As increased relative entry rates for entrepreneurs point to financing constraints for potential entrepreneurs being eased, we may also find effects in the intensive margin of initial firm employment.

Tables 7 and 8 present the relative entry count specification with entrants grouped into four size categories based upon employment in the year of entry: 1-5 employees, 6-20 employees, 21-100 employees, and over 100 employees. Coefficients on banking reform indicators in these regressions estimate the relative elasticity of startup entry to facility expansions by size group. We report specifications for churning and long-term entrants. Overall entry responses are a blend of these two types and are reported in the appendix.

The results are striking. Relative churning entry increased dramatically among entrants with 20 employees or fewer. The long-term effect is estimated to be 23% and 29% for the 1-5 and 6-20 entrant size categories, respectively. The relative increase was only 10% for entrants with 21-100

employees, and no growth in churning occurred for entrants with over 100 employees. These churning differences across entrant size categories are statistically significant. This substantial increase in the entry of new establishments that fail within three years was not just a consequence of banks learning about different markets following the deregulations. The dynamic pattern suggests the churning effect grew over time after deregulations were introduced. This pattern is also much messier than a model of improved *ex ante* allocative efficiency on the part of banks would have suggested. We believe that interstate deregulations also 'democratized' entry. Many, many more firms were started, some of which ultimately competed with and displaced incumbents. A large number of these entrants, however, failed along the way.

By contrast, growth for long-term entrants was much more uniform across the entrant size distribution. The contrast to the skewed churning distribution is visibly evident. The largest relative increase in entry was again among 6-20 employees at 17%, but the responses in the other categories are comparable at 4%-9%. We take this uniformity as evidence for the standard model of creative destruction. Since establishments entering in these larger size categories are not as likely to be credit constrained on the extensive margin, these results are consistent with improvements in allocative efficiency following the deregulations. That is, startup firms may have received financing for projects that they would not have prior to the deregulations because they were not 'insiders' or past clients of banks.

In addition to characterizing the channels of creative destruction, these distributions also suggest both extensive and intensive margin effects from financing constraints for entrepreneurship. Extensive margins effects clearly lie behind the greater entry increases among the smallest firms. The peak within the 6-20 employee category is particularly suggestive of bank lending. Entrants with fewer than six employees may be able to substitute personal savings and funds from friends and family for bank loans, but this is less likely to be true for those trying to enter at somewhat larger firm sizes (e.g., Fluck et al. 1998).

Although weaker, the growth in long-term entrants among the larger size groups is also indicative of interstate reforms having an intensive margin effect as well. It is unlikely that changes in bank financing conditions would have produced extensive margin effects in these larger groups. Instead, the increased relative entry in these largest categories likely follows from startups, which would have entered regardless of the reforms, entering at larger sizes. The distribution of effects is thus consistent with financing constraints impacting both extensive and intensive margins of entrepreneurship. We next discuss a more formal test of intensive margin effects through employment growth in establishments.

4.5 Intensive Margin Effects of Entrant Size

Empirically identifying the effect of changes in financing constraints on the intensive margin of entry is complicated by the fact that there are simultaneous changes in both the extensive and the intensive margins. The ideal estimations would compare entry sizes before and after the reforms for firms that would have entered regardless of the banking deregulations. In this case, average entry size could be an appropriate metric. Earlier estimations, however, document that greater entry is facilitated by deregulations, and we do not have a way of distinguishing which firms would have entered in the counterfactual. This is particularly true at the lower end of the size distribution, where we might expect to see the strongest effects on both the intensive and extensive margins of greater financial access.

To confirm this intensive margin effect, we undertake a second test with our long-term entrants that survive four years or longer. We calculate for each of these entrants the ratio of their initial employment size to the maximum employment size obtained by the establishment in the first three years of operation. We then calculate the mean of this entry size ratio by state-industry-type-year cells. Examining the unweighted means across these cells, startup firms and facility expansions enter at 68% and 75% of their maximum three-year sizes, respectively.

These lower relative entry sizes for startups may directly reflect financing constraints on the intensive margin, but the differential may include other factors like increased caution due to greater uncertainty. To assess whether financing constraints play an important role, we test whether startups enter closer to their maximum three-year sizes after the banking deregulations using the relative framework (3). This approach provides a more direct metric of financing constraints on the intensive margin by looking within-establishment rather than at the cross-section of entry. It is potentially limited, however, by the conditioning on survival for three years.[5]

These estimations again find no measurable impact on the intensive margin following intrastate deregulations. Following interstate deregulations, however, there was a 2% increase in startup entry sizes compared to three-year maximums. This estimate is economically and statistically significant. This estimation is again a relative comparison to facility expansions, providing evidence that entrepreneurs in particular are able to enter closer to their optimal project sizes following deregulations. While a full analysis of entry sizes requires a broader investigation of the firm size distribution, this result again suggests that effects of financing constraints for entrepreneurship are present on both the extensive and intensive entry margins.

[5]In particular, startups have different hazard functions of failure relative to facility expansions, and this may introduce some bias in the mean ratios. The three-year window trades off this survival bias with allowing more time for new establishments to reach their desired size (e.g., due to internal cash flows or better external financing opportunities).

4.6 Incumbent Displacement Analysis

We now test whether the massive entry subsequent to interstate reforms resulted in incumbent displacement along the lines of the creative destruction story. Tables 9 and 10 test this prediction using state-industry-year data. Summing across establishments, we identify the ten largest firms for each state-industry in 1980. We then track the employment market share of these firms in ensuing years. All specifications include state-industry and industry-year fixed effects and weight by initial employments in the state-industry cell.

Column 1 finds a 5% decline in the log market share of these incumbents after interstate deregulations. Evaluated at the sample mean, this would be a modest decline of about 1% of the state-industry's employment in these incumbent firms. This effect is not precisely measured, although it is when adding a linear state trend in Column 2. A variety of robustness checks suggest this decline in incumbent concentration is modestly stable. Similar results, for example, are found when looking at the market shares of the top three or five incumbents. On the other hand, null results are found in unweighted or non-logged regressions. As an alternative, Columns 3 through 6 test whether overall market concentration changed after the reforms, ignoring the incumbent distinction. The log market share of the top ten firms by state-industry does decline by 2%-3% in the long-run after interstate deregulations. This decline is quite robust across specification variants. On the other hand, no significant change in market concentration is evident with a normalized Herfindahl-Hirschman index.

Looking across the specifications, we believe a modest decline in incumbent and market concentration occurred after interstate deregulations. These changes in overall market leadership were, however, much smaller than entrepreneurship growth due to the churning result documented above. These differentials, versus specific elasticities in Tables 9 and 10, are what we hope to emphasize. Interestingly, Bertrand et al. (2007) find stronger effects of banking reforms on incumbents in France. Differences between our studies are likely tied to pre-reform banking conditions in the two countries. Future research needs to connect initial conditions with how banking reforms operate; this will be a key input for policy makers.

5 Conclusions

Theoretical models and policy discussions often describe how more competitive financial sectors improve product markets with phrases like greater efficiency, better investment choices, reduced cronyism, replacement of unproductive incumbents, and so on. We find evidence for these effects in the US experience. But, we also believe the inherent messiness and *ex ante* unpredictability of the process is under-appreciated. Although we find evidence that US banking deregulations

led to increased competition through longer-term entry, the reforms led to an even larger amount of churning. Entrepreneurship and creative destruction require many, many business failures along with the few great successes. Who the few great successes will be is rarely known *ex ante* even to venture capitalists, which is why democratizing entry may be so important for the link between well-functioning capital markets and creative destruction. Using Census Bureau data, this paper documents this through several findings.

First, entrepreneurship grew substantially after interstate banking deregulations. This was true even when compared against the baseline of facility expansions by multi-unit firms. Second, business closures grew after the deregulations, too. This second fact is tightly linked to the first, as most closures were new startups themselves. Our examination of the entrant size distribution shows that this increased churning was concentrated among very small entrants. Third, deregulations did promote long-term entry as well. Moreover, these long-term entrants were able to enter at larger employment sizes upon founding. This provides evidence for both extensive and intensive margin effects of financing constraints on entrepreneurs. Finally, incumbent concentration declined somewhat after the reforms. These concentration changes were much weaker, however, than the entrepreneurship response due to the churning element.

The macroeconomic trends presented in this paper also shed light on why studies regarding the effects of banking competition on small businesses have had somewhat contradictory results. Consistent with the literature documenting a fall in credit extended to small businesses in the early 1990s (e.g., Berger et al. 2001; Zarutskie 2006), we also find a dip in startup activity over that period. Indeed, we further document how the relative growth of startup entry has lagged behind the growth of establishment openings by existing firms since the late 1970s (e.g., Davis et al. 2006). The positive elasticities of our panel estimations, however, suggest that increases in banking competition in part dampened national declines in startup entry in states that deregulated interstate branch banking relative to states that did not.

Our analysis raises important questions for future research. First, what factors lie behind the greater churn? Certainly, greater competition will lead to higher failure rates. The concentration of failures among small startups, however, suggests that there is more to the story. Possible explanations can be found on the entrepreneur and bank sides. For entrepreneurs, lower financing constraints may lead to weaker or more frivolous entry (e.g., de Meza 2002). Nanda (2008) finds evidence for this in the context of Danish entrepreneurs. Understanding the role of consumption entrepreneurship is important for evaluating how well increases in entry rates after policy changes measure lasting economic effects. Moreover, a better understanding of entrant types is important for welfare evaluation, about which we are silent.

A second hypothesis is that the churning results from structural changes in the banking sector. The repeated emphasis on entrepreneurship following from interstate deregulations, but

not from intrastate reforms, suggests that such structural changes would be linked to the growth of large, cross-state banks. Decline in relationship banking is a very prominent candidate. Changes in bank organization may have led to different lending strategies (e.g., Berger et al. 2005; Sah and Stiglitz 1986) or weakened the ability of banks to evaluate small business projects, with negative consequences for the survival of startups. On the other hand, the higher churning result may imply greater efficiency in that banks were less likely to ration credit following the reforms (e.g., Canales and Nanda 2008) or quicker to terminate weaker firms (e.g., Gine and Love 2006). The interstate reforms brought significant changes to several aspects of banking — markets for corporate control, allocation of credit, technology diffusion — that should be investigated in the entrepreneurial context.

Such an approach may also be fruitful in helping to understand the mechanisms through which the banking sector impacts changes in the product market. While both the intrastate and the interstate reforms brought about some measure of competition in the banking sector, our study suggests that the former did not have a substantive impact on the real economy. One explanation may hinge on the extent of competition that was generated through the intrastate reform. It is conceivable that the market for corporate control must be larger than an individual state to be effective. A second explanation may hinge on the kind of technology used in bank lending to small businesses. If larger, multi-state banks were more likely to invest in technology that would better serve startups, this may explain why the interstate deregulations had a much more profound impact on entry than the intrastate reforms. The differential effect of these reforms remains a puzzle, however, and further work on untangling these differences is critical to understand the mechanisms connecting financial sector reforms to changes in the real economy.

A second important area for future research is better linking entrepreneurship with aggregate productivity changes. Our entry and exit results help reconcile apparent contradictions in the finance literature around the US banking reforms. Interstate banking deregulations have been associated with massive entry but little change in the firm size distribution and productivity growth (e.g., Black and Strahan 2002; Cetorelli and Strahan 2006; Jayaratne and Strahan 1996). While seemingly at odds, the churning growth ties these findings together nicely.

Questions remain for the Schumpeterian creative destruction story, however. A number of studies regarding aggregate productivity growth emphasize the importance of production reallocations to more efficient firms versus within-establishment growth (e.g., Foster et al. 2001). It is puzzling that the productivity growth documented in Jayaratne and Strahan (1996) is associated with intrastate deregulations, which are not associated with increases in entrepreneurship or business turnover. In an important recent study, Huang (2008) finds that the productivity growth associated with intrastate reforms is concentrated among several states that also closely passed interstate deregulations. By carefully identifying where entrepreneurship and productivity effects exist, perhaps the creative destruction story behind the US banking deregulations will

become even clearer. It is interesting, however, that this puzzle extends beyond banking. Davis et al. (2006) note that the aggregate US trend towards declining firm volatility from the 1970s onward is difficult to reconcile with large US aggregate productivity gains over the same period using standard Schumpeterian theories. We clearly have much more to learn about how banking competition, entrepreneurship, creative destruction, and productivity growth all tie together.

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Fig. 1: US Branch Banking Deregulations
Cumulative States Passing Reforms

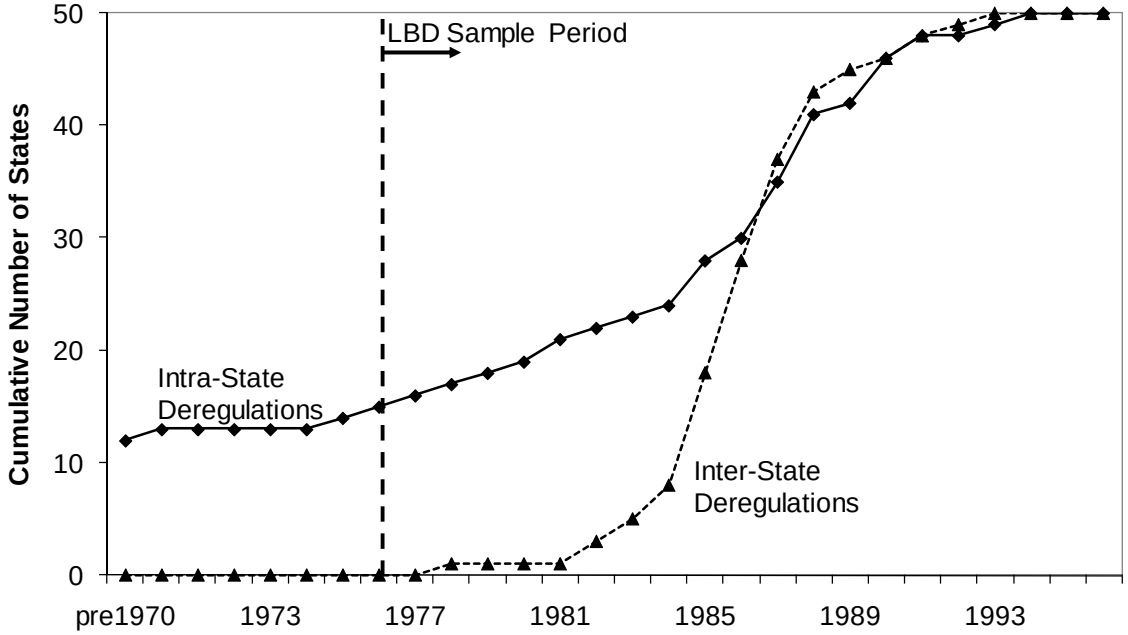
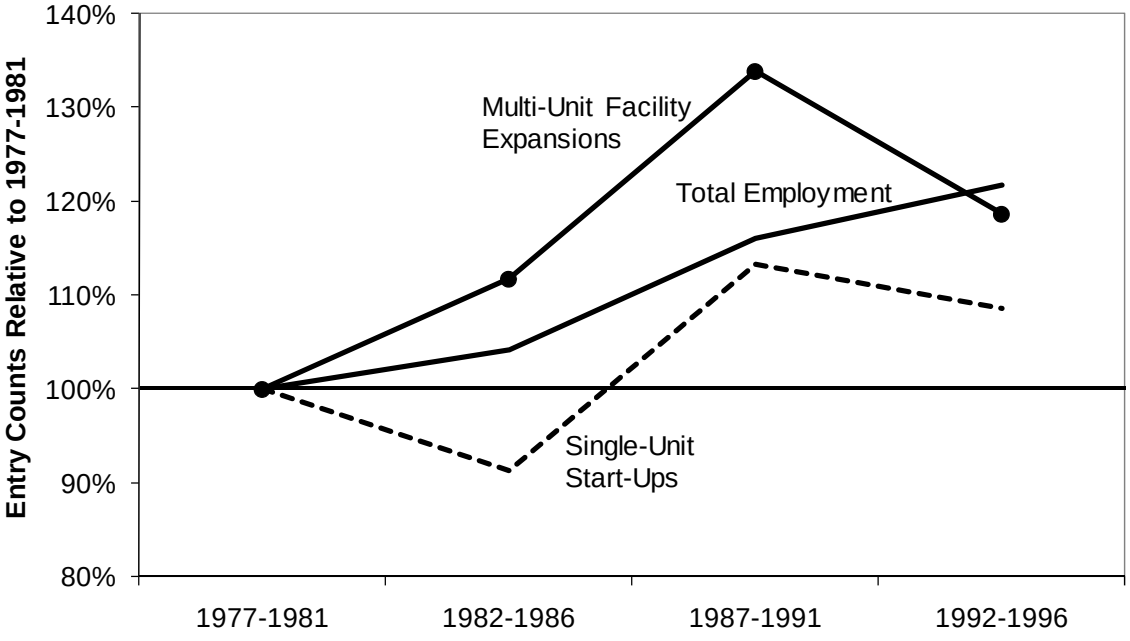


Fig. 2: US Establishment Entry Patterns
By Entrant Type



Notes: Figure 1 plots the cumulative number of states passing the Intra-State and Inter-State reforms by year. Figure 2 plots establishment entry rates over the 1977-1996 period calculated from the LBD. These entry counts are relative to the 1977-1981 period. Total US employment in the LBD is also given as reference.

Fig. 3a: Intra-State Reform Timing
Against 1977-1980 Entry Rates

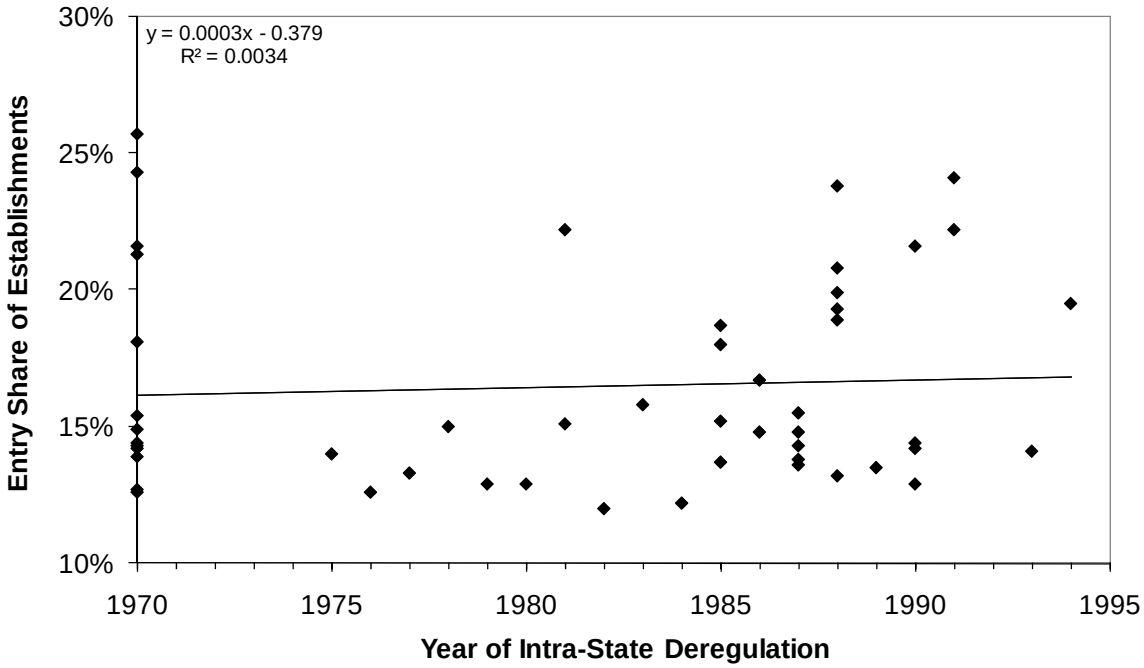
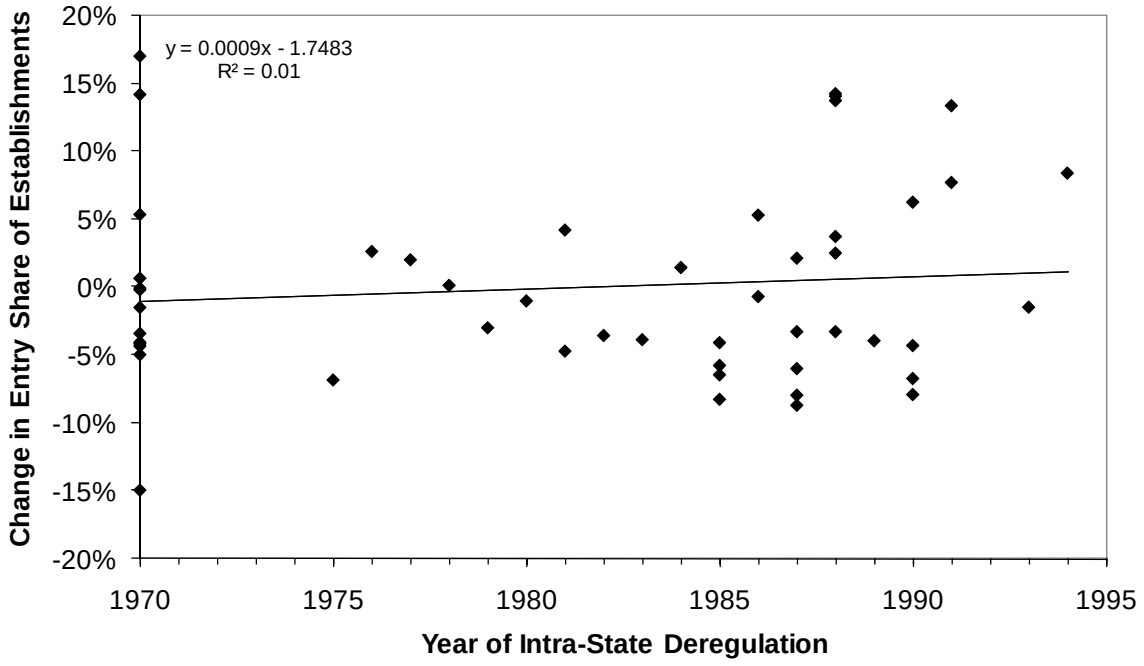


Fig. 3b: Intra-State Reform Timing
Against 1977-1978 to 1979-1980 Entry Rate Changes



Notes: Figures plot pre-existing entry rates for establishments against years of deregulation. Levels of entry rates in Figures 3a and 3c are calculated as entering establishments divided by total establishment counts during 1977-1980. Changes in entry rates in Figures 3b and 3d are calculated as change from 1977-1978 to 1979-1980 compared to the national average.

Fig. 3c: Inter-State Reform Timing
Against 1977-1980 Entry Rates

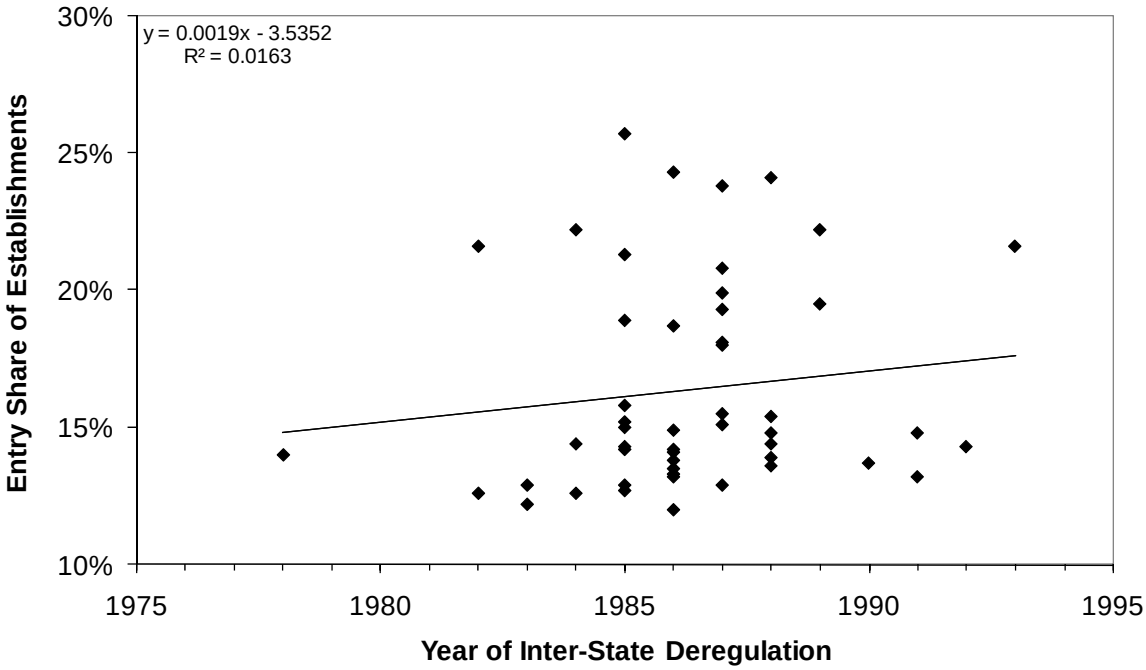
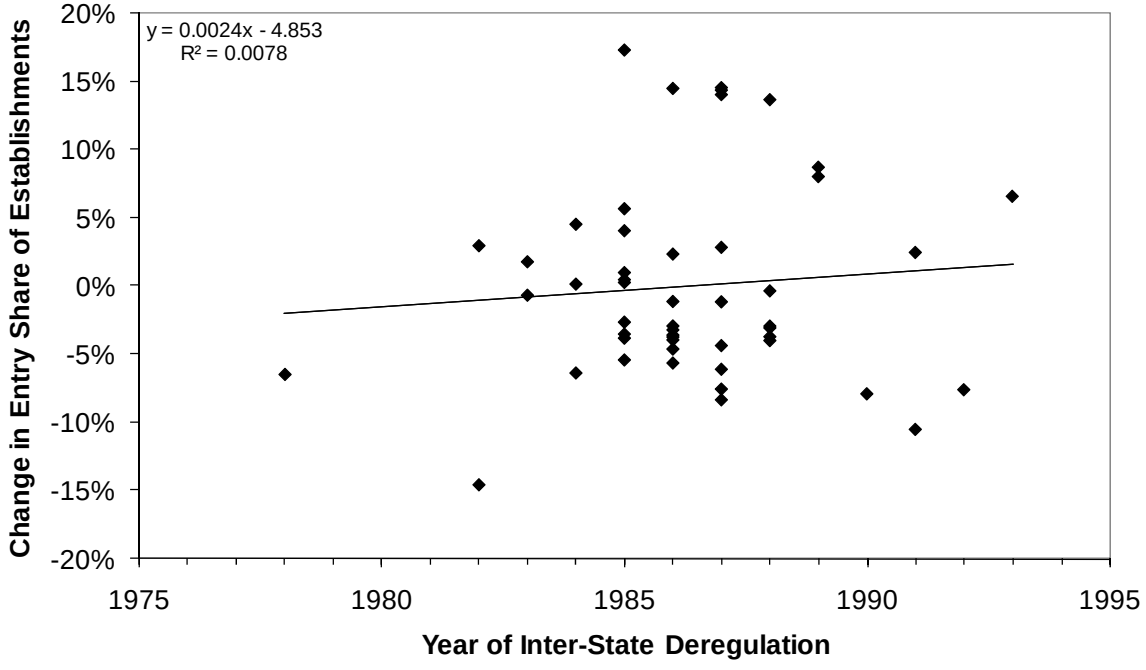


Fig. 3d: Inter-State Reform Timing
Against 1977-1978 to 1979-1980 Entry Rate Changes



Notes: Figures plot pre-existing entry rates for establishments against years of deregulation. Levels of entry rates in Figures 3a and 3c are calculated as entering establishments divided by total establishment counts during 1977-1980. Changes in entry rates in Figures 3b and 3d are calculated as change from 1977-1978 to 1979-1980 compared to the national average.

Fig 4a: Intra-State Dynamics, Entry

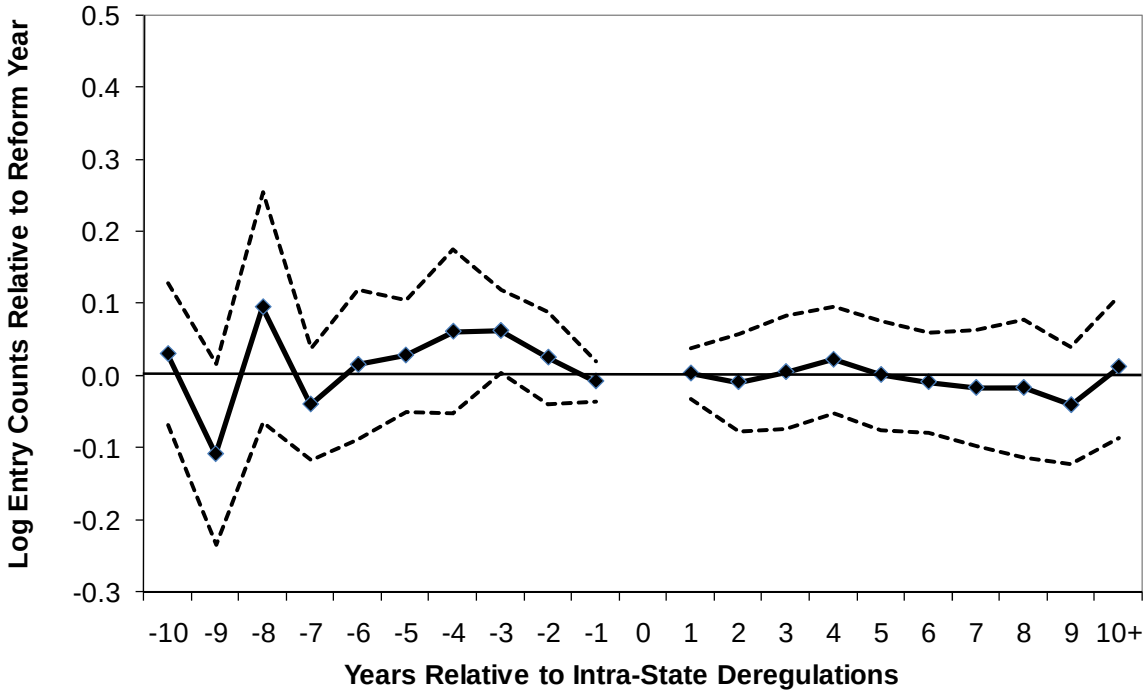
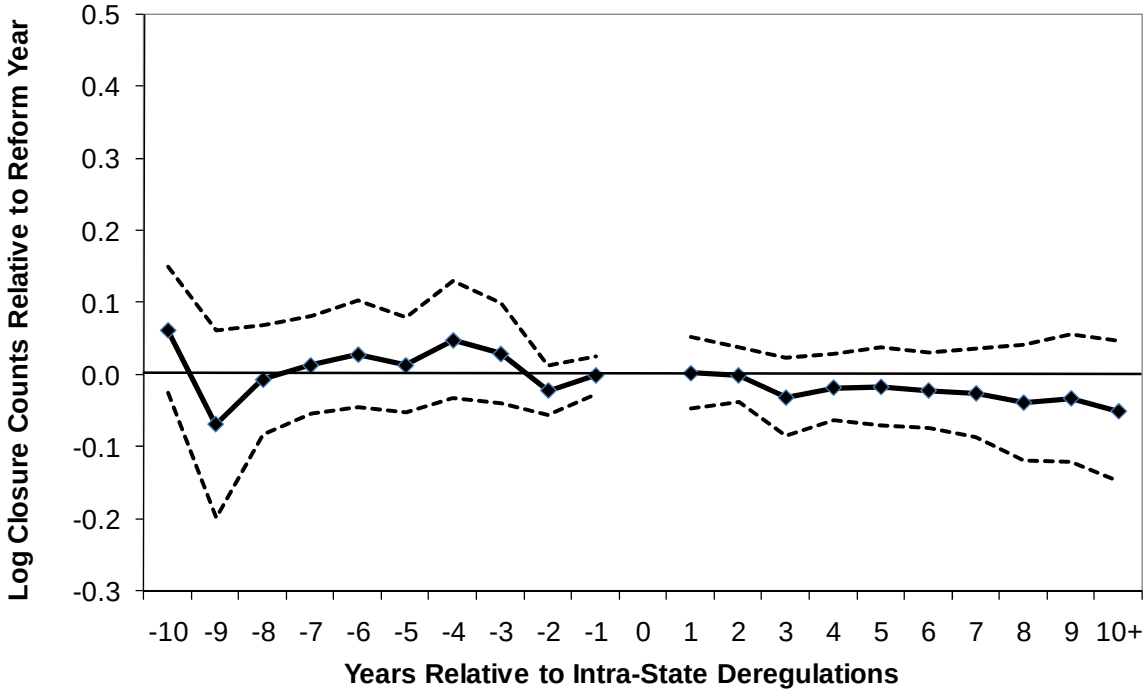


Fig 4b: Intra-State Dynamics, Closures



Notes: Figures plot coefficients from regressions of log entry counts or closure counts on a series of indicator variables extending from 10 years before the reform's passage to 10 years afterwards. The end points include all earlier and later years. The indicator variable for the year of the reform is omitted, so that coefficients are measured relative to entry or closure rates in the year of the reform. State and year effects are included in regressions. While split between two graphs, the raw dynamics surrounding the passage of the intra-state and inter-state reforms are estimated jointly. The dashed lines present 95% confidence intervals, with standard errors clustered by state. The appendix provides separate estimations for single-unit and multi-unit establishments.

Fig. 4c: Inter-State Dynamics, Entry

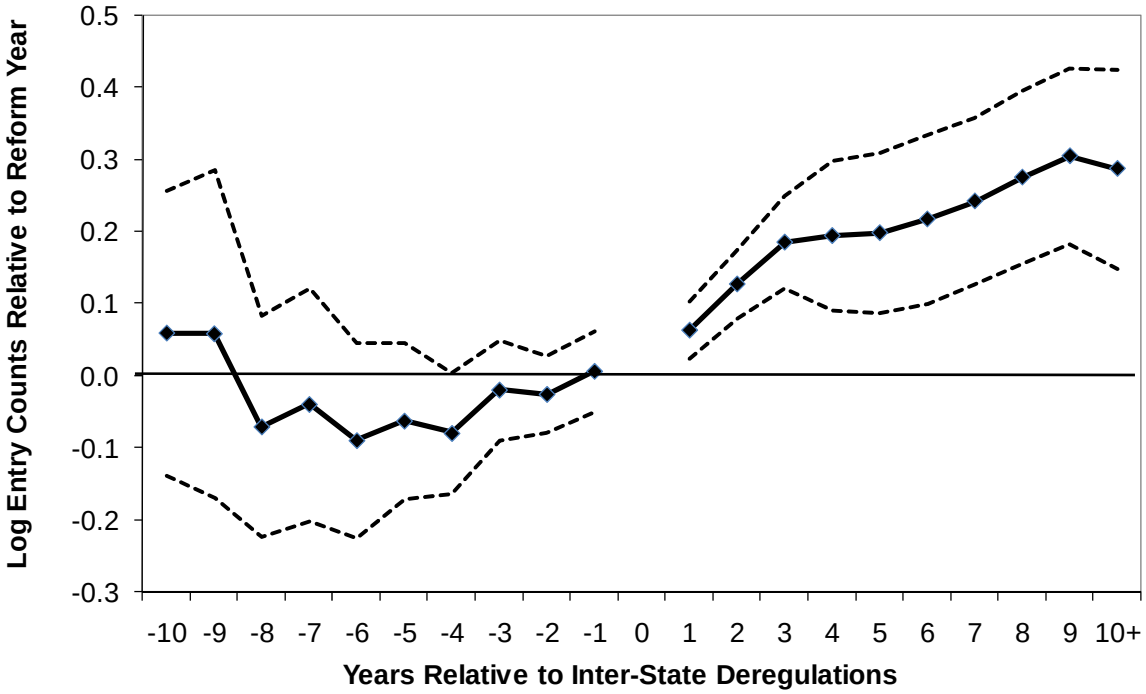
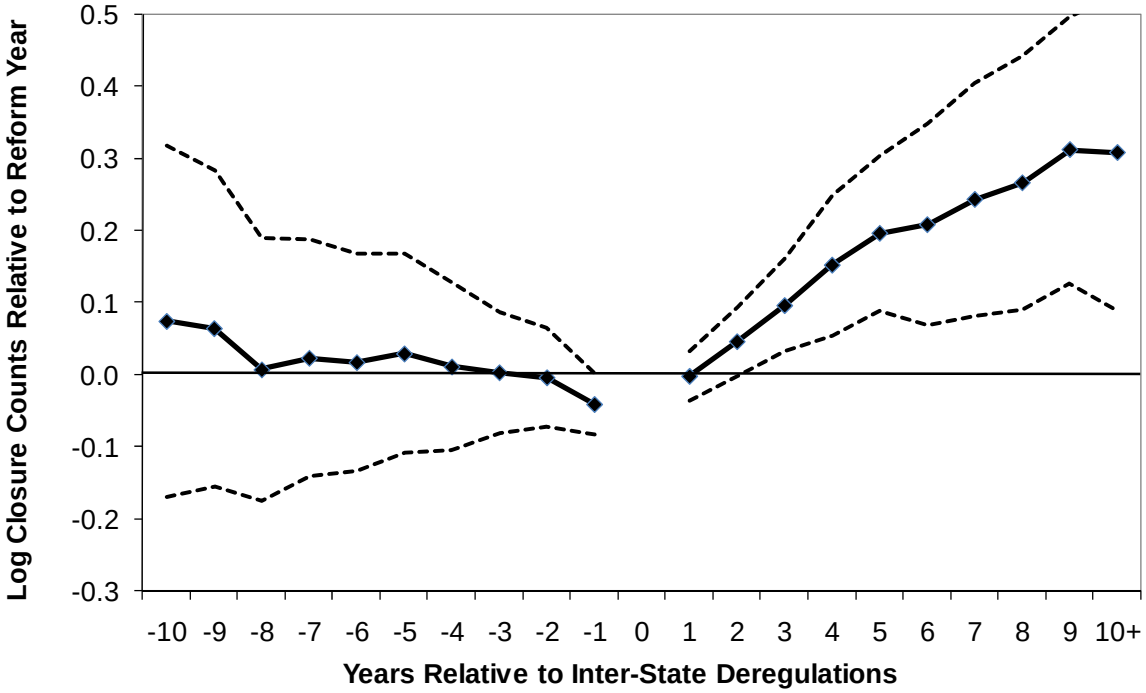
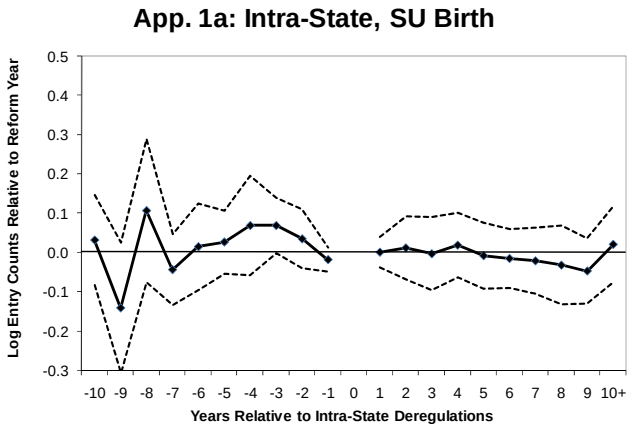
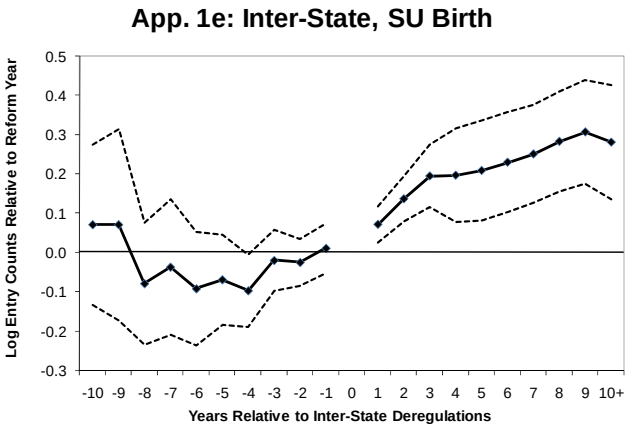


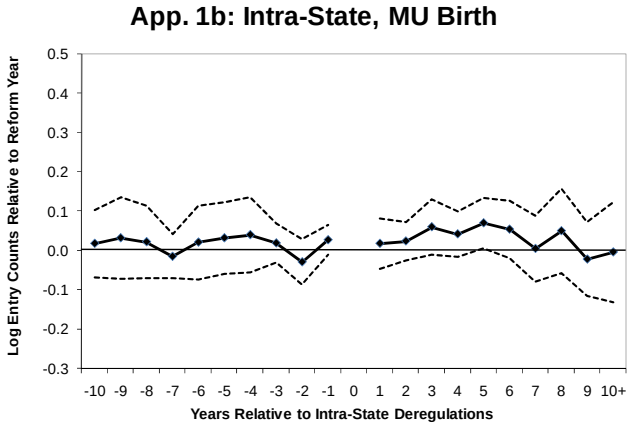
Fig 4d: Inter-State Dynamics, Closures

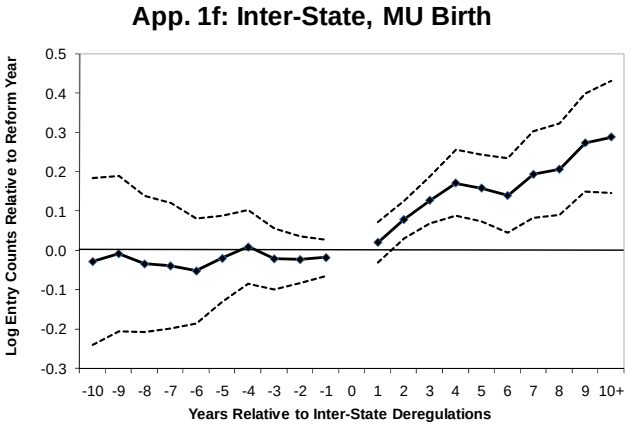


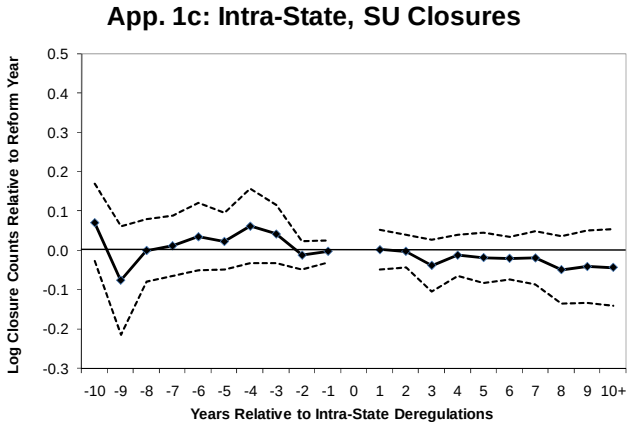
Notes: Figures plot coefficients from regressions of log entry counts or closure counts on a series of indicator variables extending from 10 years before the reform's passage to 10 years afterwards. The end points include all earlier and later years. The indicator variable for the year of the reform is omitted, so that coefficients are measured relative to entry or closure rates in the year of the reform. State and year effects are included in regressions. While split between two graphs, the raw dynamics surrounding the passage of the intra-state and inter-state reforms are estimated jointly. The dashed lines present 95% confidence intervals, with standard errors clustered by state. The appendix provides separate estimations for single-unit and multi-unit establishments.

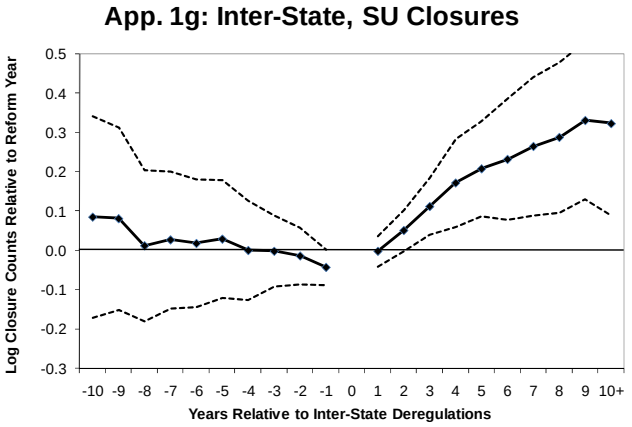


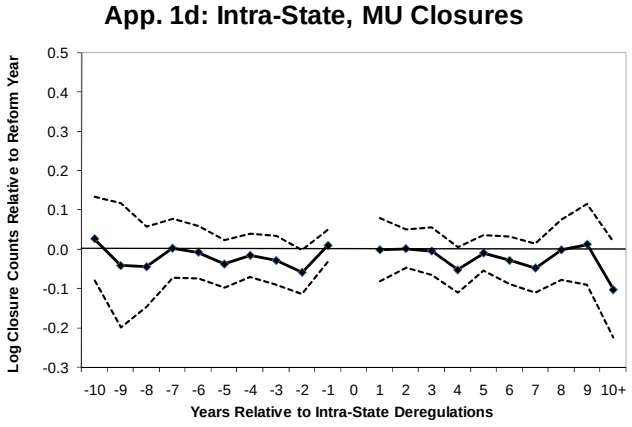


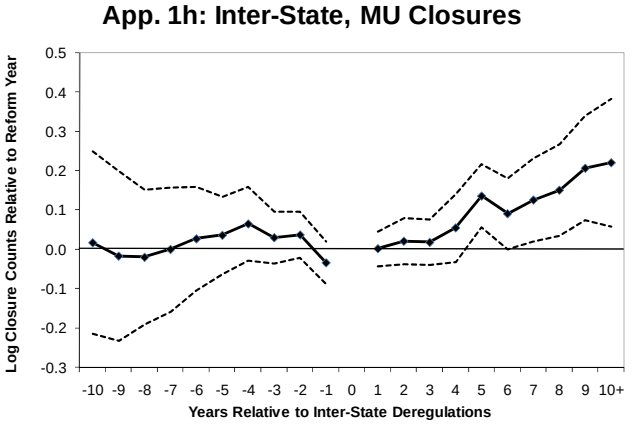












Notes: See Figure 4. Figures report single-unit (SU) and multi-unit (MU) entry and closures separately.

Table 1A: LBD-Based Descriptive Statistics on US Banking Industry

	1977	1994
Total Number of Banking Organizations	12,810	8,547
% with fewer than 50 employees	79%	70%
% with branches in multiple states	0%	3%
Total Number of Banking Branches	38,231	64,155
% owned by banks with 500+ employees	50%	66%
% owned by banks with mean 500+ employees before 1985	52%	62%
% owned by banks originally located in other states	2%	25%

Notes: Descriptive details are taken from LBD for SIC 602 (1987 classifications).

Table 1B: Asset-Based Descriptive Statistics on US Banking Industry

	1979	1994
Total Number of Banking Organizations	12,463	7,926
Small Banks (less than $100m in assets)	80%	71%
Real Gross Industry Assets (in trillions of 1994 dollars)	3.26	4.02
Industry Assets in Megabanks (more than $100b in assets)	9%	19%
Industry Assets in Small Banks (less than $100m in assets)	14%	7%

Notes: Data taken from Berger et al. (2001).

Table 2: LBD Descriptive Statistics on US Product Markets

	Entering Establishments			Churning Entrants (Survive ≤ 3 Yrs.)			Long-Term Entrants (Survive ≥ 4 Yrs.)		
	All Entrants	New Single-Unit Start-Ups	Multi-Unit Facility Expansions	All Entrants	New Single-Unit Start-Ups	Multi-Unit Facility Expansions	All Entrants	New Single-Unit Start-Ups	Multi-Unit Facility Expansions
Mean Annual Entry (k)	409	336	73	173	153	20	236	183	53
Share of Entrants		82%	18%	42%	37%	5%	58%	45%	13%
Size Distribution									
1-5 Employees	70.3%	76.0%	44.1%	76.3%	79.5%	51.6%	65.9%	73.0%	41.3%
6-20 Employees	22.8%	19.7%	36.9%	18.6%	16.9%	31.7%	25.9%	22.1%	38.9%
21-100 Employees	5.8%	3.8%	14.9%	4.4%	3.2%	13.3%	6.9%	4.3%	15.6%
100+ Employees	1.1%	0.4%	4.1%	0.8%	0.4%	3.4%	1.3%	0.5%	4.3%
Sector Distribution									
Manufacturing	9%	9%	6%	8%	9%	7%	9%	10%	6%
Services	28%	29%	23%	30%	30%	24%	27%	29%	22%
Wholesale Trade	12%	11%	17%	11%	10%	18%	12%	12%	17%
Retail Trade	25%	22%	42%	24%	23%	38%	26%	21%	44%
Mining	1%	1%	1%	1%	1%	1%	1%	1%	1%
Construction	17%	20%	1%	18%	20%	2%	16%	21%	1%
Transportation	7%	7%	10%	7%	7%	11%	7%	7%	9%
Geographic Distribution									
Northeast	19%	20%	17%	18%	18%	17%	20%	21%	17%
South	36%	35%	37%	37%	37%	37%	35%	34%	37%
Midwest	22%	21%	24%	20%	20%	23%	23%	22%	24%
West Coast	24%	24%	22%	24%	25%	23%	23%	23%	22%

Notes: Table documents descriptive statistics for establishments outside of the financial sector in the Longitudinal Business Database. Statistics are calculated for entrants between 1977-1998 using data extending to 2001. Single-unit start-ups are new firm formations. Multi-unit facility expansions are new establishment openings by existing firms. Churning entrants are establishments closing within three years of entry. Long-term entrants are establishments surviving four or more years. Entry size distributions are calculated from year of establishment entry. Jarmin and Miranda (2002) describe the construction of the LBD. Sectors not included in the LBD are agriculture, forestry and fishing, public administration, and private households. We also exclude the US postal service, restaurants and food stores, hospitals, education services, and social services. These exclusions lower the services share relative to other sectors. Incomplete LBD records require dropping 25 state-year files: 1978 (12 states), 1983 (4), 1984 (4), 1985 (1), 1986 (1), 1989 (1), and 1993 (2).

Table 3: Entry and Exit Patterns at State-Year Level

	Log Total Entrants		Log Churning Entrants		Log Long-Term Entrants		Log Establishment Closures	
	New Single-Unit Start-Ups	Multi-Unit Facility Expansions	New Single-Unit Start-Ups	Multi-Unit Facility Expansions	New Single-Unit Start-Ups	Multi-Unit Facility Expansions	Single-Unit Firms	Multi-Unit Facility Closures
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	A. Pre-Post Specifications							
Intra-State Banking	0.002	-0.010	-0.027	-0.007	0.027	-0.011	-0.059	-0.033
Dereg. Post Indicator	(0.024)	(0.035)	(0.035)	(0.042)	(0.016)	(0.033)	(0.033)	(0.033)
Inter-State Banking	0.060	0.032	0.051	0.044	0.071	0.028	0.018	-0.017
Dereg. Post Indicator	(0.020)	(0.017)	(0.030)	(0.030)	(0.018)	(0.018)	(0.025)	(0.019)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,097	1,097	1,097	1,097	1,097	1,097	1,097	1,097
	B. Linear Treatment Effect Specifications							
Number of Years Since	-0.005	0.004	-0.007	0.004	-0.003	0.004	-0.008	-0.001
Intra-State Banking Reform	(0.010)	(0.011)	(0.012)	(0.012)	(0.009)	(0.010)	(0.011)	(0.010)
Number of Years Since	0.050	0.026	0.063	0.037	0.041	0.022	0.037	0.006
Inter-State Banking Reform	(0.010)	(0.009)	(0.013)	(0.010)	(0.011)	(0.008)	(0.009)	(0.010)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,097	1,097	1,097	1,097	1,097	1,097	1,097	1,097

Notes: Panel estimations consider establishment entry and exit patterns taken from the LBD for 1977-1998. Dependent variables are indicated in column headers. Single-unit start-ups are new firm formations. Multi-unit facility expansions are new establishment openings by existing firms. Churning entrants are establishments closing within three years of entry. Long-term entrants are establishments surviving four or more years. The sample includes all states and DC, excepting 25 state-year cells where LBD files are not available, for 1,097 observations per regression. Regressions include state and year fixed effects. Regressions include unreported interactions of explanatory indicators with a Census-year indicator. Regressions are weighted by average birth employment in states from 1977-1985. Standard errors are clustered at the state cross-sectional level. Pre-Post specifications compare annual entry rates before and after the state-level banking deregulation indicated. Linear Treatment Effect specifications allow for linear growth in treatment effects over time by modeling the number of years after the indicated deregulation's passage, with a long-term effect at four years. The appendix reports these estimations with linear state time trends incorporated.

Table 4: Dynamic Specifications of Entry and Exit at State-Year Level

	Log New Entrants		Log Churning Entrants		Log Long-Term Entrants		Log Establishment Closures	
	New Single-Unit Start-Ups	Multi-Unit Facility Expansions	New Single-Unit Start-Ups	Multi-Unit Facility Expansions	New Single-Unit Start-Ups	Multi-Unit Facility Expansions	Single-Unit Firms	Multi-Unit Facility Closures
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Response to Intra-State Branch Banking Deregulations:								
Fwd. 1-2 Yrs.	-0.038	-0.015	-0.053	-0.023	-0.023	-0.009	-0.037	-0.001
	(0.034)	(0.023)	(0.041)	(0.033)	(0.030)	(0.022)	(0.026)	(0.035)
Lag 0-1 Yrs.	-0.040	-0.016	-0.058	-0.012	-0.026	-0.016	-0.037	0.017
	(0.044)	(0.044)	(0.057)	(0.055)	(0.036)	(0.040)	(0.042)	(0.043)
Lag 2-3 Yrs.	-0.044	0.015	-0.063	0.027	-0.028	0.008	-0.050	0.020
	(0.035)	(0.042)	(0.042)	(0.048)	(0.036)	(0.044)	(0.038)	(0.046)
Lag 4+ Yrs.	-0.029	0.007	-0.039	0.003	-0.022	0.010	-0.038	-0.006
	(0.047)	(0.050)	(0.054)	(0.062)	(0.043)	(0.046)	(0.048)	(0.044)
Response to Inter-State Branch Banking Deregulations:								
Fwd. 1-2 Yrs.	0.028	-0.016	0.000	-0.031	0.057	-0.005	-0.043	-0.043
	(0.032)	(0.027)	(0.037)	(0.036)	(0.032)	(0.024)	(0.022)	(0.019)
Lag 0-1 Yrs.	0.062	0.004	0.028	-0.001	0.096	0.010	-0.020	-0.049
	(0.034)	(0.030)	(0.048)	(0.046)	(0.030)	(0.027)	(0.037)	(0.027)
Lag 2-3 Yrs.	0.175	0.078	0.167	0.106	0.189	0.068	0.047	-0.036
	(0.037)	(0.034)	(0.052)	(0.052)	(0.042)	(0.029)	(0.045)	(0.042)
Lag 4+ Yrs.	0.227	0.124	0.272	0.152	0.194	0.115	0.163	0.015
	(0.057)	(0.046)	(0.065)	(0.060)	(0.060)	(0.049)	(0.050)	(0.046)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,097	1,097	1,097	1,097	1,097	1,097	1,097	1,097

Notes: See Table 3. Dynamic specifications model a series of leads and lags for each reform. Leads and lags are consolidated into two-year increments extending from two years prior to the deregulations to four or more years after the deregulations. Coefficient values for dynamic leads and lags are relative to the period three years before reforms and earlier. The appendix reports these estimations with linear state time trends incorporated.

Table 5: Relative Start-Up Entry and Exit Patterns at State-Industry-Type-Year Level

	Log Entry Counts	Log Churning Entry	Log Long-Term Entry	Log Closure Counts
	(1)	(2)	(3)	(4)
		A. Pre-Post Specifications		
Start-Up Response Relative to Multi-Unit Facilities:				
Intra-State Banking Dereg. Post	-0.012	-0.038	0.006	-0.020
Indicator x Start-Up Firms	(0.032)	(0.038)	(0.030)	(0.028)
Inter-State Banking Dereg. Post	0.027	0.041	0.029	0.046
Indicator x Start-Up Firms	(0.019)	(0.023)	(0.017)	(0.014)
State-SIC2-Type FE	Yes	Yes	Yes	Yes
State-SIC2-Year FE	Yes	Yes	Yes	Yes
Type-Year FE	Yes	Yes	Yes	Yes
Observations	111,894	111,894	111,894	111,894
		B. Linear Treatment Effect Specifications		
Start-Up Response Relative to Multi-Unit Facilities:				
Number of Years Since Intra-State	-0.010	-0.015	-0.006	-0.007
Banking Reform x Start-Up Firms	(0.006)	(0.008)	(0.007)	(0.005)
Number of Years Since Inter-State	0.021	0.038	0.013	0.036
Banking Reform x Start-Up Firms	(0.007)	(0.009)	(0.007)	(0.009)
State-SIC2-Type FE	Yes	Yes	Yes	Yes
State-SIC2-Year FE	Yes	Yes	Yes	Yes
Type-Year FE	Yes	Yes	Yes	Yes
Observations	111,894	111,894	111,894	111,894

Notes: Panel estimations consider establishment entry and exit patterns taken from the LBD for 1977-1998. Dependent variables are indicated in column headers. Annual cells are constructed by State-SIC2-Type, where Type includes single-unit firms and establishments of multi-unit firms. All regressions include cross-sectional fixed effects for State-SIC2-Type and longitudinal fixed effects for Type-Year and State-SIC2-Year. In these saturated models, single-unit responses are estimated relative to multi-unit responses. Regressions include unreported indicator variables for cells with zero births and unreported interactions of explanatory variables with Census-year indicators. Regressions are weighted by average birth employment in cells from 1977-1985. Standard errors are clustered at the cross-sectional State-Type level. Pre-Post specifications compare annual entry rates before and after the state-level banking deregulation indicated. Linear Treatment Effect specifications allow for linear growth in treatment effects over time by modeling the number of years after the indicated deregulation's passage, with a long-term effect at four years.

Table 6: Dynamic Specifications of Relative Start-Up Entry and Exit Responses

	Log Entry Counts	Log Churning Entry	Log Long-Term Entry	Log Closure Counts
	(1)	(2)	(3)	(4)
Start-Up Response Relative to Multi-Unit Facilities following Intra-State Branch Banking Deregulations:				
Fwd. 1-2 Yrs.	-0.013	-0.020	-0.016	-0.028
	(0.023)	(0.035)	(0.019)	(0.032)
Lag 0-1 Yrs.	-0.020	-0.039	-0.009	-0.049
	(0.036)	(0.045)	(0.028)	(0.037)
Lag 2-3 Yrs.	-0.084	-0.135	-0.059	-0.079
	(0.036)	(0.048)	(0.032)	(0.041)
Lag 4+ Yrs.	-0.044	-0.061	-0.035	-0.028
	(0.030)	(0.037)	(0.030)	(0.023)
Start-Up Response Relative to Multi-Unit Facilities following Inter-State Branch Banking Deregulations:				
Fwd. 1-2 Yrs.	0.021	0.040	0.025	0.006
	(0.034)	(0.037)	(0.033)	(0.022)
Lag 0-1 Yrs.	0.038	0.061	0.044	0.045
	(0.034)	(0.040)	(0.030)	(0.028)
Lag 2-3 Yrs.	0.071	0.115	0.067	0.097
	(0.039)	(0.054)	(0.036)	(0.040)
Lag 4+ Yrs.	0.109	0.220	0.059	0.173
	(0.037)	(0.054)	(0.035)	(0.045)
State-SIC2-Type FE	Yes	Yes	Yes	Yes
State-SIC2-Year FE	Yes	Yes	Yes	Yes
Type-Year FE	Yes	Yes	Yes	Yes
Observations	111,894	111,894	111,894	111,894

Notes: See Table 5. Dynamic specifications model a series of leads and lags for each reform. Leads and lags are consolidated into two-year increments extending from two years prior to the deregulations to four or more years after the deregulations. Coefficient values for dynamic leads and lags are relative to the period three years before reforms and earlier.

Table 7: Size Distribution of Relative Start-Up Churning and Long-Term Entry Responses

	Log Churning Entrants				Log Long-Term Entrants			
	1-5 Empl.	6-20 Empl.	21-100 Empl.	101+ Empl.	1-5 Empl.	6-20 Empl.	21-100 Empl.	101+ Empl.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				A. Pre-Post Specifications				
Intra-State Banking Dereg. Post	0.005	-0.044	0.015	-0.034	0.048	-0.013	-0.009	-0.034
Indicator x Start-Up Firms	(0.026)	(0.032)	(0.035)	(0.042)	(0.018)	(0.024)	(0.032)	(0.031)
Inter-State Banking Dereg. Post	0.047	0.040	-0.026	0.032	0.028	0.046	-0.003	0.005
Indicator x Start-Up Firms	(0.019)	(0.023)	(0.027)	(0.030)	(0.023)	(0.022)	(0.026)	(0.022)
State-SIC2-Type FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-SIC2-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Type-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	111,894	111,894	111,894	111,894	111,894	111,894	111,894	111,894
				B. Linear Treatment Effect Specifications				
Number of Years Since Intra-State	-0.002	-0.014	-0.006	-0.013	0.002	-0.008	-0.005	-0.008
Banking Reform x Start-Up Firms	(0.008)	(0.007)	(0.007)	(0.010)	(0.007)	(0.007)	(0.009)	(0.008)
Number of Years Since Inter-State	0.038	0.053	0.027	0.012	0.023	0.029	0.022	0.003
Banking Reform x Start-Up Firms	(0.010)	(0.012)	(0.010)	(0.010)	(0.010)	(0.009)	(0.012)	(0.013)
State-SIC2-Type FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-SIC2-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Type-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	111,894	111,894	111,894	111,894	111,894	111,894	111,894	111,894

Notes: See Table 5. Table disaggregates churning and long-term entry by employment size of entrant in the first year of operation. Coefficients report effects of start-up firms relative to facility expansions by multi-unit firms.

Table 8: Dynamic Specifications of Size Distribution of Relative Start-Up Churning and Long-Term Entry Responses

	Log Churning Entrants				Log Long-Term Entrants			
	1-5 Empl.	6-20 Empl.	21-100 Empl.	101+ Empl.	1-5 Empl.	6-20 Empl.	21-100 Empl.	101+ Empl.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Start-Up Response Relative to Multi-Unit Facilities following Intra-State Branch Banking Deregulations:								
Fwd. 1-2 Yrs.	-0.018	-0.016	-0.040	0.001	-0.023	-0.012	-0.061	-0.073
	(0.034)	(0.040)	(0.030)	(0.037)	(0.024)	(0.036)	(0.032)	(0.039)
Lag 0-1 Yrs.	-0.028	-0.041	-0.015	-0.014	0.011	-0.045	-0.084	-0.070
	(0.029)	(0.046)	(0.058)	(0.068)	(0.022)	(0.033)	(0.033)	(0.035)
Lag 2-3 Yrs.	-0.081	-0.104	-0.051	-0.140	-0.051	-0.045	-0.038	-0.058
	(0.030)	(0.043)	(0.040)	(0.053)	(0.032)	(0.033)	(0.038)	(0.032)
Lag 4+ Yrs.	-0.009	-0.068	0.045	-0.024	-0.004	-0.048	-0.053	-0.054
	(0.035)	(0.031)	(0.033)	(0.049)	(0.027)	(0.035)	(0.038)	(0.040)
Start-Up Response Relative to Multi-Unit Facilities following Inter-State Branch Banking Deregulations:								
Fwd. 1-2 Yrs.	0.053	0.073	0.031	0.012	0.033	0.043	0.063	0.054
	(0.022)	(0.031)	(0.046)	(0.046)	(0.031)	(0.035)	(0.038)	(0.031)
Lag 0-1 Yrs.	0.073	0.066	-0.022	0.032	0.036	0.072	0.031	0.040
	(0.022)	(0.031)	(0.042)	(0.053)	(0.032)	(0.026)	(0.028)	(0.028)
Lag 2-3 Yrs.	0.135	0.165	0.066	0.049	0.105	0.124	0.092	0.045
	(0.036)	(0.045)	(0.058)	(0.084)	(0.042)	(0.033)	(0.048)	(0.037)
Lag 4+ Yrs.	0.226	0.289	0.107	-0.039	0.074	0.174	0.086	0.038
	(0.048)	(0.058)	(0.061)	(0.063)	(0.051)	(0.041)	(0.056)	(0.069)
State-SIC2-Type FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-SIC2-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Type-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	111,894	111,894	111,894	111,894	111,894	111,894	111,894	111,894

Notes: See Table 7. Dynamic specifications model a series of leads and lags for each reform. Leads and lags are consolidated into two-year increments extending from two years prior to the deregulations to four or more years after the deregulations. Coefficient values for dynamic leads and lags are relative to the period three years before reforms and earlier.

Table 9: Incumbent Market Shares and Concentration at State-Industry-Year Level

	Log Top 10 Incumbent Market Share	Log Top 10 Incumbent Market Share	Log Top 10 Market Share	Log Top 10 Market Share	Normalized Herfindahl Hirschman Index	Normalized Herfindahl Hirschman Index
	(1)	(2)	(3)	(4)	(5)	(6)
			A. Pre-Post Specifications			
Intra-State Banking	-0.019	-0.009	0.001	-0.001	-0.001	-0.001
Dereg. Post Indicator	(0.026)	(0.012)	(0.009)	(0.005)	(0.002)	(0.001)
Inter-State Banking	-0.047	-0.044	-0.005	-0.003	0.002	0.001
Dereg. Post Indicator	(0.033)	(0.022)	(0.007)	(0.006)	(0.001)	(0.001)
State-SIC2 FE	Yes	Yes	Yes	Yes	Yes	Yes
SIC2-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State Time Trend		Yes		Yes		Yes
Observations	55,947	55,947	55,947	55,947	55,947	55,947
			B. Linear Treatment Effect Specifications			
Number of Years Since	0.008	-0.003	0.000	-0.001	0.001	-0.001
Intra-State Banking Reform	(0.009)	(0.010)	(0.004)	(0.004)	(0.001)	(0.001)
Number of Years Since	-0.008	-0.009	-0.007	-0.007	-0.001	-0.001
Inter-State Banking Reform	(0.010)	(0.005)	(0.002)	(0.002)	(0.001)	(0.001)
State-SIC2 FE	Yes	Yes	Yes	Yes	Yes	Yes
SIC2-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State Time Trend		Yes		Yes		Yes
Observations	55,947	55,947	55,947	55,947	55,947	55,947

Notes: Panel estimations consider concentration patterns taken from the LBD for 1977-1998. Dependent variables are indicated in column headers. Annual cells are constructed by State-SIC2. Regressions include cross-sectional fixed effects for State-SIC2 and longitudinal fixed effects for SIC2-Year. Regressions include unreported interactions of explanatory variables with Census-year indicators. Regressions are weighted by average birth employment in cells from 1977-1985. Standard errors are clustered at the cross-sectional State-SIC2 level. Incumbent firms are defined as the ten largest firms in 1980 by State-SIC2. Market shares are calculated through employments.

Table 10: Dynamic Analysis of Incumbent Market Shares and Concentration

	Log Top 10 Incumbent Market Share	Log Top 10 Incumbent Market Share	Log Top 10 Market Share	Log Top 10 Market Share	Normalized Herfindahl Hirschman Index	Normalized Herfindahl Hirschman Index
	(1)	(2)	(3)	(4)	(5)	(6)
Response to Intra-State Branch Banking Deregulations						
Fwd. 1-2 Yrs.	0.027	-0.003	-0.004	0.005	0.003	0.001
	(0.019)	(0.013)	(0.008)	(0.009)	(0.001)	(0.001)
Lag 0-1 Yrs.	0.023	0.001	0.009	0.016	0.001	-0.001
	(0.025)	(0.018)	(0.012)	(0.010)	(0.002)	(0.001)
Lag 2-3 Yrs.	0.005	-0.016	-0.009	0.002	0.001	-0.002
	(0.044)	(0.030)	(0.016)	(0.016)	(0.002)	(0.002)
Lag 4+ Yrs.	0.044	-0.008	-0.002	0.000	0.002	-0.001
	(0.037)	(0.038)	(0.017)	(0.017)	(0.003)	(0.001)
Response to Inter-State Branch Banking Deregulations						
Fwd. 1-2 Yrs.	-0.045	-0.035	-0.008	-0.009	0.000	0.000
	(0.031)	(0.016)	(0.010)	(0.008)	(0.001)	(0.001)
Lag 0-1 Yrs.	-0.085	-0.069	-0.006	-0.005	0.002	0.002
	(0.055)	(0.029)	(0.011)	(0.009)	(0.002)	(0.001)
Lag 2-3 Yrs.	-0.092	-0.078	-0.027	-0.028	0.000	0.000
	(0.065)	(0.032)	(0.013)	(0.010)	(0.002)	(0.001)
Lag 4+ Yrs.	-0.074	-0.073	-0.025	-0.026	0.000	0.001
	(0.077)	(0.041)	(0.016)	(0.012)	(0.002)	(0.002)
State-SIC2 FE	Yes	Yes	Yes	Yes	Yes	Yes
SIC2-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State Time Trend		Yes		Yes		Yes
Observations	55,947	55,947	55,947	55,947	55,947	55,947

Notes: See Table 9. Dynamic specifications model a series of leads and lags for each reform. Leads and lags are consolidated into two-year increments extending from two years prior to the deregulations to four or more years after the deregulations. Coefficient values for dynamic leads and lags are relative to the period three years before reforms and earlier.

App. Table 1: Timing of State Branch Banking Deregulations

State	Intra-State *de novo* Deregulation	Intra-State M&A Deregulation	Inter-State Deregulation
Alabama	1990	1981	1987
Alaska	1970	1970	1982
Arizona	1970	1970	1986
Arkansas	Not deregulated	1994	1989
California	1970	1970	1987
Colorado	Not deregulated	1991	1988
Connecticut	1988	1980	1983
Delaware	1970	1970	1988
District of Columbia	1970	1970	1985
Florida	1988	1988	1985
Georgia	Not deregulated	1983	1985
Hawaii	1986	1986	Not deregulated
Idaho	1970	1970	1985
Illinois	1993	1988	1986
Indiana	1991	1989	1986
Iowa	Not deregulated	Not deregulated	1991
Kansas	1990	1987	1992
Kentucky	Not deregulated	1990	1984
Louisiana	1988	1988	1987
Maine	1975	1975	1978
Maryland	1970	1970	1985
Massachusetts	1984	1984	1983
Michigan	1988	1987	1986
Minnesota	Not deregulated	1993	1986
Mississippi	1989	1986	1988
Missouri	1990	1990	1986
Montana	Not deregulated	1990	1993
Nebraska	Not deregulated	1985	1990
Nevada	1970	1970	1985
New Hampshire	1987	1987	1987
New Jersey	Not deregulated	1977	1986
New Mexico	1991	1991	1989
New York	1976	1976	1982
North Carolina	1970	1970	1985
North Dakota	Not deregulated	1987	1991
Ohio	1989	1979	1985
Oklahoma	Not deregulated	1988	1987
Oregon	1985	1985	1986
Pennsylvania	1990	1982	1986
Rhode Island	1970	1970	1984
South Carolina	1970	1970	1986
South Dakota	1970	1970	1988
Tennessee	1990	1985	1985
Texas	1988	1988	1987
Utah	1981	1981	1984
Vermont	1970	1970	1988
Virginia	1987	1978	1985
Washington	1985	1985	1987
West Virginia	1987	1987	1988
Wisconsin	1990	1990	1987
Wyoming	Not deregulated	1988	1987

Notes: Data taken from Jayaratne and Strahan (1996). Deregulations prior to 1970 are listed as 1970.

App. Table 2: Table 3 with Linear State Time Trends

	Log Total Entrants		Log Churning Entrants		Log Long-Term Entrants		Log Establishment Closures	
	New Single-Unit Start-Ups	Multi-Unit Facility Expansions	New Single-Unit Start-Ups	Multi-Unit Facility Expansions	New Single-Unit Start-Ups	Multi-Unit Facility Expansions	Single-Unit Firms	Multi-Unit Facility Closures
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				A. Pre-Post Specifications				
Intra-State Banking	0.038	-0.034	0.014	-0.031	0.062	-0.033	-0.033	-0.059
Dereg. Post Indicator	(0.022)	(0.027)	(0.035)	(0.039)	(0.021)	(0.024)	(0.032)	(0.029)
Inter-State Banking	0.057	0.019	0.052	0.028	0.065	0.016	0.014	-0.036
Dereg. Post Indicator	(0.021)	(0.015)	(0.032)	(0.034)	(0.018)	(0.016)	(0.026)	(0.023)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,097	1,097	1,097	1,097	1,097	1,097	1,097	1,097
				B. Linear Treatment Effect Specifications				
Number of Years Since	-0.002	0.001	-0.004	-0.002	0.000	0.002	-0.003	-0.007
Intra-State Banking Reform	(0.011)	(0.010)	(0.013)	(0.011)	(0.013)	(0.010)	(0.013)	(0.015)
Number of Years Since	0.047	0.021	0.061	0.031	0.037	0.016	0.032	0.000
Inter-State Banking Reform	(0.012)	(0.009)	(0.015)	(0.011)	(0.012)	(0.009)	(0.009)	(0.010)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,097	1,097	1,097	1,097	1,097	1,097	1,097	1,097

Notes: See Table 3.

App. Table 3: Table 4 with Linear State Time Trends

	Log New Entrants		Log Churning Entrants		Log Long-Term Entrants		Log Establishment Closures	
	New Single-Unit Start-Ups	Multi-Unit Facility Expansions	New Single-Unit Start-Ups	Multi-Unit Facility Expansions	New Single-Unit Start-Ups	Multi-Unit Facility Expansions	Single-Unit Firms	Multi-Unit Facility Closures
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Response to Intra-State Branch Banking Deregulations:								
Fwd. 1-2 Yrs.	-0.046	-0.011	-0.063	-0.020	-0.026	-0.004	-0.018	0.006
	(0.032)	(0.026)	(0.045)	(0.047)	(0.024)	(0.023)	(0.027)	(0.023)
Lag 0-1 Yrs.	-0.057	-0.018	-0.074	-0.020	-0.039	-0.016	-0.016	0.009
	(0.045)	(0.049)	(0.067)	(0.076)	(0.034)	(0.040)	(0.048)	(0.043)
Lag 2-3 Yrs.	-0.063	0.006	-0.078	0.005	-0.044	0.003	-0.022	0.004
	(0.049)	(0.052)	(0.066)	(0.065)	(0.051)	(0.052)	(0.057)	(0.054)
Lag 4+ Yrs.	-0.055	0.015	-0.069	0.005	-0.037	0.021	-0.006	-0.022
	(0.051)	(0.058)	(0.068)	(0.085)	(0.058)	(0.053)	(0.063)	(0.065)
Response to Inter-State Branch Banking Deregulations:								
Fwd. 1-2 Yrs.	0.054	-0.014	0.034	-0.032	0.075	-0.002	-0.028	-0.053
	(0.034)	(0.023)	(0.038)	(0.032)	(0.035)	(0.022)	(0.022)	(0.018)
Lag 0-1 Yrs.	0.089	-0.005	0.069	-0.016	0.111	0.002	-0.009	-0.076
	(0.035)	(0.029)	(0.046)	(0.050)	(0.034)	(0.029)	(0.035)	(0.029)
Lag 2-3 Yrs.	0.201	0.058	0.208	0.079	0.201	0.051	0.053	-0.073
	(0.045)	(0.037)	(0.053)	(0.055)	(0.053)	(0.036)	(0.046)	(0.046)
Lag 4+ Yrs.	0.234	0.081	0.292	0.101	0.188	0.076	0.148	-0.035
	(0.073)	(0.058)	(0.079)	(0.064)	(0.076)	(0.067)	(0.053)	(0.051)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,097	1,097	1,097	1,097	1,097	1,097	1,097	1,097

Notes: See Table 4.

App. Table 4: Transition Estimations from State-Year to State-Industry-Type-Year

	Log Entry Counts	Log Entry Counts
	(1)	(2)
Single-Unit Start-Ups Interactions:		
Intra-State Banking Dereg. Fwd. 1-2 Yrs	-0.034 (0.030)	-0.013 (0.023)
Intra-State Banking Dereg. Lag 0-1 Yrs	-0.040 (0.038)	-0.020 (0.036)
Intra-State Banking Dereg. Lag 2-3 Yrs	-0.051 (0.028)	-0.084 (0.036)
Intra-State Banking Dereg. Lag 4+ Yrs	-0.032 (0.037)	-0.044 (0.030)
Inter-State Banking Dereg. Fwd. 1-2 Yrs	0.032 (0.027)	0.021 (0.034)
Inter-State Banking Dereg. Lag 0-1 Yrs	0.056 (0.027)	0.038 (0.034)
Inter-State Banking Dereg. Lag 2-3 Yrs	0.153 (0.032)	0.071 (0.039)
Inter-State Banking Dereg. Lag 4+ Yrs	0.201 (0.039)	0.109 (0.037)
Multi-Unit Facility Expansions Interactions:		
Intra-State Banking Dereg. Fwd. 1-2 Yrs	-0.022 (0.030)	Absorbed
Intra-State Banking Dereg. Lag 0-1 Yrs	-0.020 (0.048)	
Intra-State Banking Dereg. Lag 2-3 Yrs	0.033 (0.044)	
Intra-State Banking Dereg. Lag 4+ Yrs	0.012 (0.052)	
Inter-State Banking Dereg. Fwd. 1-2 Yrs	0.011 (0.034)	
Inter-State Banking Dereg. Lag 0-1 Yrs	0.018 (0.037)	
Inter-State Banking Dereg. Lag 2-3 Yrs	0.082 (0.036)	
Inter-State Banking Dereg. Lag 4+ Yrs	0.092 (0.048)	
State-SIC2-Type FE	Yes	Yes
State-SIC2-Year FE		Yes
Type-Year FE	Yes	Yes
Observations	111,894	111,894

Notes: See Tables 4 and 6. Column 1 documents State-SIC2-Type-Year estimations for entry counts without State-SIC2-Year FE. Column 1's coefficients parallel the State-Year analysis in Table 4. The difference between these is due to the added industry dimension. Column 2 further incorporates the State-SIC2-Year FE. In these saturated models, the start-up response is estimated relative to facility expansions, and separate coefficients for expansion establishments are not estimated.

App. Table 5: Size Distribution of Aggregate Relative Start-Up Entry

	Log Entry Count			
	1-5 Empl.	6-20 Empl.	21-100 Empl.	101+ Empl.
	(1)	(2)	(3)	(4)
Start-Up Response Relative to Multi-Unit Facilities				
Intra-State Branch Banking				
Fwd. 1-2 Yrs.	-0.024	-0.016	-0.044	-0.037
	(0.031)	(0.031)	(0.027)	(0.043)
Lag 0-1 Yrs.	0.006	-0.051	-0.046	-0.021
	(0.030)	(0.039)	(0.046)	(0.052)
Lag 2-3 Yrs.	-0.086	-0.078	-0.049	-0.129
	(0.039)	(0.036)	(0.042)	(0.051)
Lag 4+ Yrs.	-0.029	-0.066	-0.059	-0.056
	(0.036)	(0.036)	(0.030)	(0.051)
Start-Up Response Relative to Multi-Unit Facilities				
Inter-State Branch Banking				
Fwd. 1-2 Yrs.	0.040	0.049	0.034	0.084
	(0.036)	(0.038)	(0.038)	(0.035)
Lag 0-1 Yrs.	0.047	0.091	0.000	0.060
	(0.024)	(0.027)	(0.029)	(0.048)
Lag 2-3 Yrs.	0.122	0.144	0.054	0.117
	(0.035)	(0.038)	(0.048)	(0.039)
Lag 4+ Yrs.	0.149	0.227	0.076	0.079
	(0.045)	(0.038)	(0.046)	(0.045)
State-SIC2-Type FE	Yes	Yes	Yes	Yes
State-SIC2-Year FE	Yes	Yes	Yes	Yes
Type-Year FE	Yes	Yes	Yes	Yes
Observations	111,894	111,894	111,894	111,894

Notes: See Table 8.