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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 US 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 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 difficult to identify ex ante, which is why democratizing entry is an important trait of well-functioning capital markets.

1. Introduction

A number of recent studies find a positive relation between financial development and economic growth across countries (e.g., Beck, Levine, and Loayza, 2000; Levine, 1997; Levine, Loayza, and Beck, 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 such as cronyism, scarce financing is reallocated to the most qualified entrepreneurs,

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inefficient incumbents are displaced, and product markets improve due to Schumpeterian creative destruction (e.g., King and Levine, 1993a, 1993b; Rajan and Zingales, 2003; Aghion, Fally, and Scarpetta, 2007; Chun, Kim, Morck, and Yeung, 2008).

While the cross-country relation between finance and growth is well documented, empirical work at the firm level evaluating 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, Hubbard, and Petersen, 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, Sapienza, and Zingales, 2004; Cetorelli and Strahan, 2006; Zarutskie, 2006; Bertrand, Schoar, and Thesmar, 2007).

We study how US branch banking deregulations impacted entrepreneurship rates and incumbent firm displacement. These reforms, enacted by individual states from the 1970s onward, 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, Holtz-Eakin, and Rosen, 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 shows substantial increases in start-up 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, not incumbents. The greatest increase in entry occurred among very small start-ups 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 Longitudinal Business Database (LBD) of the US Census Bureau. The LBD provides annual employment data for every US establishment from 1976 onward. 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 an important part of the entrepreneurial process. Roughly half of start-ups 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 the general understanding of how product markets are influenced by improved financial sector efficiency (e.g., Bertrand, Schoar, and Thesmar, 2007; Cetorelli and Strahan, 2006; Cetorelli, 2004; Cetorelli and Gambera, 2001; Beck and Levine, 2002, Beck, Levine, and Levkov, 2008; Levine, Levkov, and Rubinstein, 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, Schoar, and Thesmar (2007) study of the French banking deregulations' impact for firms with more than one hundred employees and the Guiso, Sapienza, and Zingales (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 start-up births and deaths with facility openings and closures by existing firms. We argue 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 start-ups from general economic conditions. This technique could 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 could 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 capture changes on both the intensive and extensive margins. 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 with 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 that 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 could have also improved economies of scale, although Berger, Klapper, and Udell (2001) argue that subsequent bank mergers resulted in few cost savings on average. In part due to the 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. Fig. 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 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 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 Section 3 that the timings of these reforms are not correlated with pre-existing rates of entrepreneurship in states.

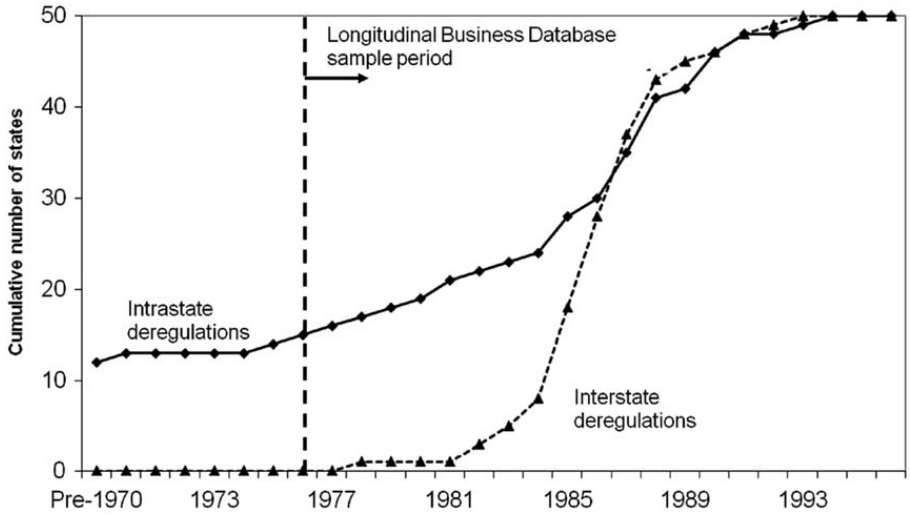

Fig. 1. Cumulative states passing US branch banking deregulation. This figure plots the cumulative number of states passing the intrastate and interstate reforms by year.

Table 1
Descriptive statistics on changes in US banking industry from 1977 to 1994. Panel A shows data from the Longitudinal Business Database (LBD) for banking industry (602 in 1987 Standard Industrial Classification). Panel B shows asset-based descriptive statistics taken from Berger, Klapper, and Udell (2001).

Panel A. Change in number of banks and bank branches	1977	1994
Total number of banking organizations	12,810	8,547
Percent with fewer than 50 employees (%)	79	70
Percent with branches in multiple states (%)	0	3
Total number of banking branches	38,231	64,155
Percent owned by banks with 500+ employees (%)	50	66
Percent owned by banks with mean 500+ employees before 1985 (%)	52	62
Percent owned by banks originally located in other states (%)	2	25
Panel B. Change in bank assets	**1979**	**1994**
Total number of banking organizations	12,463	7,926
Percent small banks (less than $100 million in assets) (%)	80	71
Real gross industry assets (in trillions of 1994 dollars)	3.26	4.02
Percent assets in megabanks (more than $100 billion in assets) (%)	9	19
Percent assets in small banks (less than $100 million in assets) (%)	14	7

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). Panel A of Table 1 shows 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 five hundred 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, Klapper, and Udell (2001) find that the decline in the number of banks is almost completely due to reduced numbers of small banks with assets under $100 million. Moreover, the percentage of industry assets managed by megabanks (i.e., with more than $100 billion in assets) almost doubled from 1977 to 1994, while the share managed by small banks halved, as shown in Panel B of Table 1.

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 toward projects yielding highest returns. 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 start-ups face substantial credit constraints. Moreover, because entrepreneurs have fewer nonbank 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 start-up entry relative to facility expansions by existing firms.

However, there are two channels through which these reforms could instead harm start-ups. First, Petersen and Rajan (1995) argue that start-ups 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 start-ups because they are better at screening on soft versus hard information (e.g., Stein, 2002; Berger, Miller, Petersen, Rajan, and Stein, 2005). If lending decisions at larger banks are based on more hierarchical decision processes, ultimate adjudication decisions could 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. However, 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 could give local loan officers a comparative advantage for entrepreneurial finance. Because US banking reforms led to a shift in industry structure from small banks toward large banks, this could have had a direct negative effect on lending to start-ups relative to established firms with a history of audited accounts.

The net theoretical effect of these competing channels is therefore ambiguous. Fig. 1 shows that introductions of intrastate and interstate deregulations are sufficiently independent across states that we can jointly investigate their effect on start-up 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 onward. The underlying data are sourced from US tax records and Census Bureau surveys, and approximately four million establishments and 70 million 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 important for our analysis of entry patterns following banking deregulations. The LBD also lists physical locations of establishments instead of states of incorporation, circumventing issues such as higher incorporation rates in 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 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 in which 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 onward. 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 409 thousand new establishments opened in each year within these sectors are new start-ups. Fifty-eight percent of these entering establishments survive for four or more years. Survival rates are higher for multi-unit facility expansions at 73% versus 55%.

Fig. 2 plots relative entry counts over time for start-ups and facility expansions, with entry counts normalized by 1977–1981 levels. While start-ups 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 start-up entrants over the 20-year period, despite a 20% overall

Table 2
Table presents descriptive statistics on the formation of new establishments outside of the financial sector using data from the Longitudinal Business Database (LBD). Statistics are calculated for entrants between 1977 and 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 (four), 1984 (four), 1985 (one), 1986 (one), 1989 (one), and 1993 (two).

	Entering establishments			Churning entrants (survive ≤ 3 years)			Long-term entrants (survive ≥ 4 years)		
	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 (thousands)	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

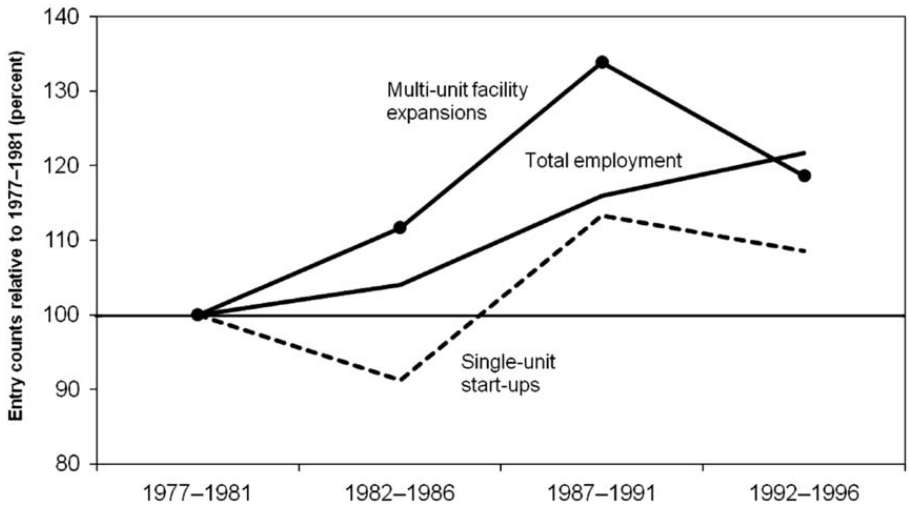

Fig. 2. US establishment entry patterns by entrant type. This figure plots establishment entry rates over the 1977–1996 period calculated from the Longitudinal Business Database (LBD). These entry counts are relative to the 1977–1981 period. Total US employment in the LBD is also given as reference.

growth in LBD employment. Measured in terms of rates, Davis, Haltiwanger, Jarmin, and Miranda (2006) show a substantial reduction in business entry and exit from the late 1970s to the late 1990s using the LBD. Fig. 2 also shows 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, Klapper, and Udell, 2001; Zarutskie, 2006).

These aggregate trends are important when interpreting upcoming panel estimation results. We control separately for aggregate entry or exit rates of start-ups 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 start-up entry and exit rates for states that have deregulated versus those that have not.

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

Manufacturing accounts for about 10% of entry; manufacturing, services, wholesale trade, and retail trade jointly account for 75%. Exclusions noted in Table 2 caption lower our sample's share of services relative to overall economic activity. While the sector distributions of start-ups and facility expansions are generally comparable, they are different for retail trade and construction. Our core estimations control for these differences across Standard Industrial Classification (SIC2) industries, and we further confirm 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 such as Silicon Valley and Boston's Route 128, the broad entry and exit rates we consider are more evenly spread across US regions. Also, no substantial differences exist in the extent to which start-ups 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, Roberts, and Samuelson (1989), Davis, Haltiwanger, and Schuh (1996), and Glaeser and Kerr (2009) provide additional details on US entry patterns. Dumais, Ellison, and Glaeser (2002) and Ellison, Glaeser, and Kerr (2009) consider the agglomeration and coagglomeration of start-ups and facility expansions, respectively.

With these descriptive statistics, 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 is systematically associated with pre-existing establishment entry rates. Fig. 3, Panel A, 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 relation evident. Fig. 3, Panel B, 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, Panels C and D of Fig. 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 start-ups and facility expansions. These estimations provide the most intuitive presentation of our results. We then turn to stricter frameworks that isolate start-up 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 start-up entrants and facility expansions, and the same empirical specification equation (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 such as 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 afterward. The LBD is collected on March 1 of each year. We thus date the reforms such that a passage of *TRA* in 1987, for example, is coded as changing from zero to one 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, Duflo, and Mullainathan (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]

[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, Kerr, and Kugler (2007) for further details.

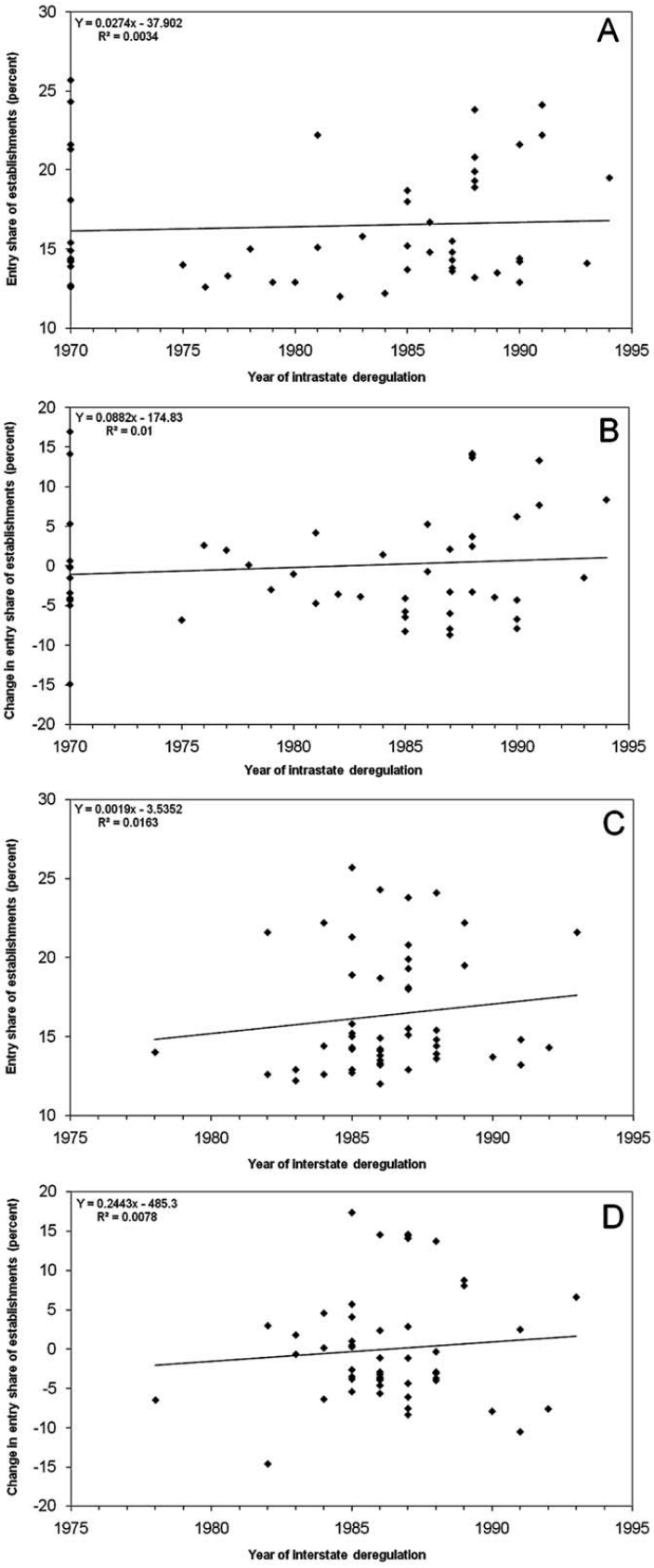

Fig. 3. Intrastate and interstate reform timing. The figure plots pre-existing entry rates for establishments against years of deregulation. Levels of entry rates in Panels A and C are calculated as entering establishments divided by total establishment counts during 1977–1980. Changes in entry rates in Panels B and D are calculated as change from 1977–1978 to 1979–1980 compared with the national average.

Table 3
Panel estimations of establishment entry and exit at the state-year level using data from the Longitudinal Business Database (LBD) for 1977–1998. Coefficients report estimates of the effect of intrastate and interstate banking deregulations on the entry and exit of establishments. 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 Washington, DC, excepting 25 state-year cells in which 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 to 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.

	Log total entrants		Log churning entrants		Log long-term entrants		Log establishment closures	
	New single-unit start-ups (1)	Multi-unit facility expansions (2)	New single-unit start-ups (3)	Multi-unit facility expansions (4)	New single-unit start-ups (5)	Multi-unit facility expansions (6)	Single-unit firms (7)	Multi-unit facility closures (8)
Panel A. Pre-post specifications								
Intrastate banking deregulation post indicator	0.002 (0.024)	−0.010 (0.035)	−0.027 (0.035)	−0.007 (0.042)	0.027 (0.016)	−0.011 (0.033)	−0.059 (0.033)	−0.033 (0.033)
Interstate banking deregulation post indicator	0.060 (0.020)	0.032 (0.017)	0.051 (0.030)	0.044 (0.030)	0.071 (0.018)	0.028 (0.018)	0.018 (0.025)	−0.017 (0.019)
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,097	1,097	1,097	1,097	1,097	1,097	1,097	1,097
Panel B. Linear treatment effect specifications								
Number of years since intrastate banking reform	−0.005 (0.010)	0.004 (0.011)	−0.007 (0.012)	0.004 (0.012)	−0.003 (0.009)	0.004 (0.010)	−0.008 (0.011)	−0.001 (0.010)
Number of years since interstate banking reform	0.050 (0.010)	0.026 (0.009)	0.063 (0.013)	0.037 (0.010)	0.041 (0.011)	0.022 (0.008)	0.037 (0.009)	0.006 (0.010)
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,097	1,097	1,097	1,097	1,097	1,097	1,097	1,097

These pre-post results are reported in Panel A of Table 3. Column 1 of Table 3 finds a strong increase in start-up births after the interstate deregulations. The elasticity of 6% is statistically significant and economically large in size. However, 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 and Bradstreet incorporations data to interstate and intrastate deregulations, respectively. This result is also confirmed in Levine, Levkov, and Rubinstein (2008). Several data sources thus point 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 start-up entry response in Column 1 likely combines specific benefits for entrepreneurship with more general economic development that indirectly increased new firm entry, too. We formally compare start-up 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 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 equation (1), it is among single-unit firms themselves. Pre-post specifications

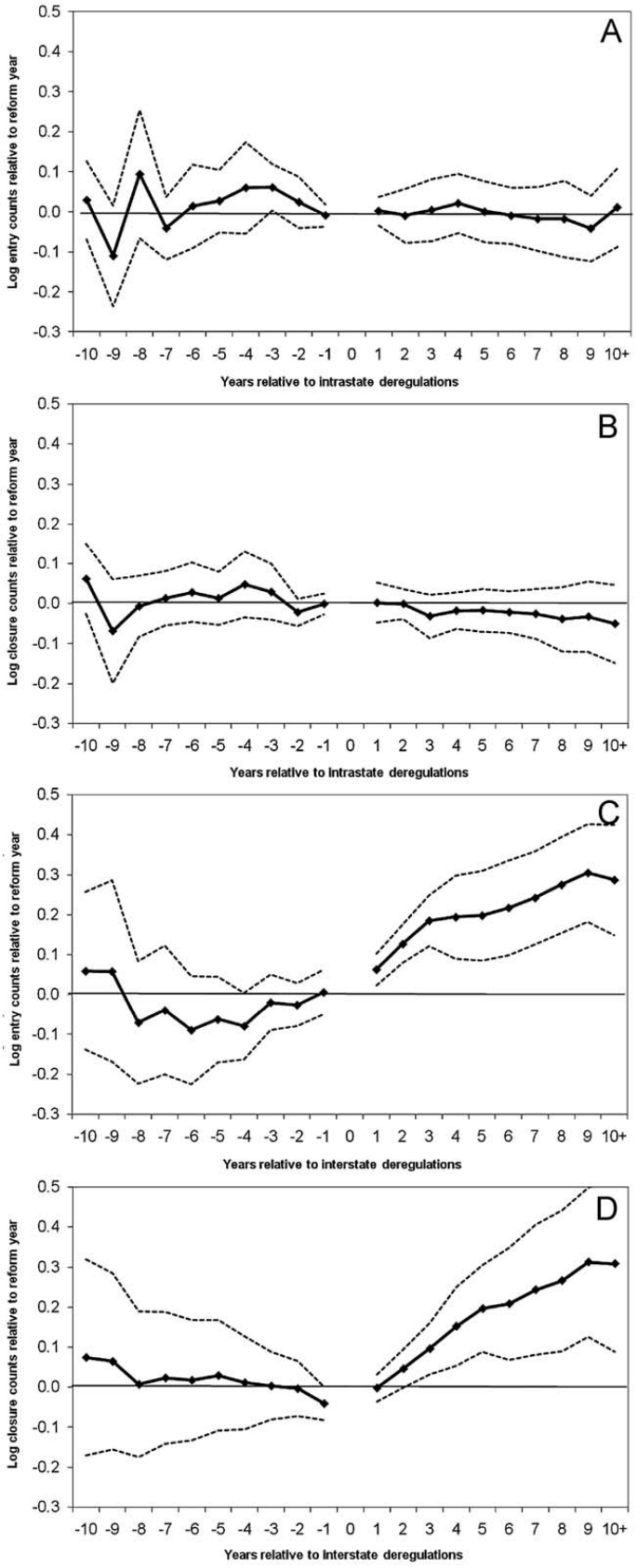

Fig. 4. Intrastate and interstate dynamics for entry and closures. The figure plots coefficients from regressions of log entry counts or closure counts on a series of indicator variables extending from 10 years before the reform's passage to 10 years afterward. The end points include all earlier and 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 intrastate and interstate 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. Panel A. Intrastate dynamics for entry, Panel B. Intrastate dynamics for closures, Panel C. Interstate dynamics for entry, Panel D. Interstate dynamics for closures.

are blunt instruments, however, for measuring these extended effects on the product markets that often take several years to materialize. Dynamic specifications offer richer characterizations.

4.2. Dynamic specifications by state-year

The panels of Fig. 4 show 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 20 separate indicator variables modeling the passage of the *TRA* reform. These dummy variables take a value of one in the qth 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 20-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 equation (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. A slight rise in entry could be evident 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 10-year window for states that deregulated early. Looking after the *TRA* reform, no changes in entry or closures are evident. However, 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 Fig. 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. Start-up 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 start-ups 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 start-ups 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 nonparametric approach is a parsimonious way of capturing the major features of the raw dynamics in Fig. 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 start-up entry four or more years after the reform, compared with 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 start-ups, respectively. This 19% estimate might be overstated, too, as a sizable forward effect is evident in Column 5. However, 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 perform a number of robustness checks on these basic state-year outcomes. Tables A2 and A3 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 10th from their full sample values. While these tests provide

Table 4
Dynamic panel estimations of establishment entry and exit at the state-year level using data from the Longitudinal Business Database (LBD) for 1977–1998. Coefficients report estimates of the effect of intrastate and interstate banking deregulations on the entry and exit of establishments. 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 3 provides further details on the specification.

	Log total entrants		Log churning entrants		Log long-term entrants		Log establishment closures	
	New single-unit start-ups (1)	Multi-unit facility expansions (2)	New single-unit start-ups (3)	Multi-unit facility expansions (4)	New single-unit start-ups (5)	Multi-unit facility expansions (6)	Single-unit firms (7)	Multi-unit facility closures (8)
Response to intrastate branch banking deregulations								
Years 1–2 before reform	−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)
Reform year and one after	−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)
Years 2–3 after reform	−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)
Years 4+ after reform	−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 interstate branch banking deregulations								
Years 1–2 before reform	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)
Reform year and one after	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)
Years 2–3 after reform	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)
Years 4+ after reform	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 fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,097	1,097	1,097	1,097	1,097	1,097	1,097	1,097

added confidence, the census data allow greater empirical leverage than state-year estimations for showing identification.[2]

[2] We also test the mechanism implied by our reduced-form indicators for interstate deregulations. Using the LBD, we show that 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 five hundred or more employees over the period 1977–1985. These simple estimates confirm deregulation's role in the descriptive statistics outlined in Table 1. While we prefer the reduced-form approach of modeling 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 in the data.

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. Fig. 3 did not find a pre-existing relation between state-level entrepreneurship rates and the timing of the reforms. Nevertheless, other secular changes at the state-year level could bias the parameter estimates. To address this concern, 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

Table 5
Differences-in-differences estimates of establishment entry and exit using data from the Longitudinal Business Database (LBD) for 1977–1998. Coefficients report estimates of the effect of intrastate and interstate banking deregulations on the entry and exit of new firms relative to state-industry-type-year establishment expansions and contractions for existing multi-unit firms. Dependent variables are indicated in column headers. Cells are constructed by state-industry-type-year, where type indicates whether the entrant is a new single-unit firm or a new establishment of a multi-unit firm. All regressions include cross-sectional fixed effects for state-industry-type and longitudinal fixed effects for type-year and state-industry-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 to 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.

	Log entry counts (1)	Log churning entry (2)	Log long-term entry (3)	Log closure counts (4)
Panel A. Pre-post specifications				
Start-up response relative to multi-unit facilities				
Intrastate banking deregulation post indicator × start-up firms	−0.012	−0.038	0.006	−0.020
	(0.032)	(0.038)	(0.030)	(0.028)
Interstate banking deregulation post indicator × start-up firms	0.027	0.041	0.029	0.046
	(0.019)	(0.023)	(0.017)	(0.014)
State-industry-type fixed effects	Yes	Yes	Yes	Yes
State-industry-year fixed effects	Yes	Yes	Yes	Yes
Type-year fixed effects	Yes	Yes	Yes	Yes
Number of 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 intrastate banking reform × start-up firms	−0.010	−0.015	−0.006	−0.007
	(0.006)	(0.008)	(0.007)	(0.005)
Number of years since interstate banking reform × start-up firms	0.021	0.038	0.013	0.036
	(0.007)	(0.009)	(0.007)	(0.009)
State-industry-type fixed effects	Yes	Yes	Yes	Yes
State-industry-year fixed effects	Yes	Yes	Yes	Yes
Type-year fixed effects	Yes	Yes	Yes	Yes
Number of observations	111,894	111,894	111,894	111,894

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 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 contrast the entry of start-up 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 shown in Fig. 2. The dynamic state-year regressions in Table 4 suggest this identification strategy is reasonable. Facility expansion patterns are similar to start-ups prior to reforms, and dynamic growth patterns for facility expansions following deregulations are reasonable.

[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 could bias estimates due to the nonproportional allocation of industries across states.

Table 6
Dynamic differences-in-differences estimates of establishment entry and exit using data from the Longitudinal Business Database (LBD) for 1977–1998. Coefficients report estimates of the effect of intrastate and interstate banking deregulations on the entry and exit of new firms relative to state-industry-type-year establishment expansions and contractions for existing multi-unit firms. 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 5 provides further details on the specification.

	Log entry counts (1)	Log churning entry (2)	Log long-term entry (3)	Log closure counts (4)
Start-up response relative to multi-unit facilities following intrastate branch banking deregulations				
Years 1–2 before reform	−0.013	−0.020	−0.016	−0.028
	(0.023)	(0.035)	(0.019)	(0.032)
Reform year and one after	−0.020	−0.039	−0.009	−0.049
	(0.036)	(0.045)	(0.028)	(0.037)
Years 2–3 after reform	−0.084	−0.135	−0.059	−0.079
	(0.036)	(0.048)	(0.032)	(0.041)
Years 4+ after reform	−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 interstate branch banking deregulations				
Years 1–2 before reform	0.021	0.040	0.025	0.006
	(0.034)	(0.037)	(0.033)	(0.022)
Reform year and one after	0.038	0.061	0.044	0.045
	(0.034)	(0.040)	(0.030)	(0.028)
Years 2–3 after reform	0.071	0.115	0.067	0.097
	(0.039)	(0.054)	(0.036)	(0.040)
Years 4+ after reform	0.109	0.220	0.059	0.173
	(0.037)	(0.054)	(0.035)	(0.045)
State-industry-type fixed effects	Yes	Yes	Yes	Yes
State-industry-year fixed effects	Yes	Yes	Yes	Yes
Type-year fixed effects	Yes	Yes	Yes	Yes
Number of observations	111,894	111,894	111,894	111,894

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 start-up 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 (e.g., Berger, Kashyap, and Scalise, 1995). It is much more challenging to construct competing explanations when variations within state-industry-year cells are exploited. Our identification strategy also has a useful substantive interpretation in that it teases out differential responses of start-ups to banking deregulations over and above heightened facility expansions of existing firms. Because start-ups 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 start-up firm or not. We denote industries with i and entrant types with x. The addition of 51 SIC2 industries and two entrant types results in more than 100,000 state-industry-type-year cells. As an analog to the state-year pre-post estimation equation (1), we first examine

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

The fixed effects are 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 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 start-ups 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 Fig. 2. Finally, state-industry-year fixed effects $\eta_{s,i,t}$ fully absorb secular changes in local industrial conditions common to start-ups and facility expansions.

The *TRA* and *TER* deregulation indicators from equation (2) are interacted with whether the entrant type is a start-up firm or not ($Type_x$). As state-industry-year fixed effects saturate the model, the dynamic coefficients for start-up firms become relative to responses of facility expansions. 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 (e.g., dividing by local industry size).[4]

[4] The Appendix shows how dropping state-industry-year fixed effects from equation (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 start-up 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.

Table 5 presents these relative models in a format similar to Table 3. Consistent with the impression given by the state-year panels, start-ups 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 in 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 modeling. The first column finds that entry of start-ups 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 start-ups 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 start-up entry, with a transitory dip evident in the second and third lags.

In comparing coefficients across Table 6 columns, the higher relative magnitude for business closures (17%) compared with entry (11%) clearly does not suggest an absolute decline in small businesses. Table 3 shows otherwise. The differential for start-up 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 test jointly our relative start-up differentials with differences across industries in dependence on external finance. Following Rajan and Zingales (1998), Cetorelli and Strahan (2006), and Aghion, Fally, and Scarpetta (2007), we construct measures of industry-level dependence on external finance from Compustat data. Relative entry elasticities for financially dependent sectors are typically higher than those for nondependent 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.

Table 7
Table disaggregates churning and long-term entry by employment size of entrant in the first year of operation. Coefficients report estimates of the effect of intrastate and interstate banking deregulations on the entry and exit of new firms relative to state-industry-type-year establishment expansions and contractions for existing multi-unit firms. Table 5 provides further details on the specification.

	Log churning entrants by initial employment				Log long-term entrants by initial employment			
	1–5 (1)	6–20 (2)	21–100 (3)	101+ (4)	1–5 (5)	6–20 (6)	21–100 (7)	101+ (8)
Panel A. Pre-post specifications								
Intrastate banking deregulation post indicator × start-up firms	0.005 (0.026)	−0.044 (0.032)	0.015 (0.035)	−0.034 (0.042)	0.048 (0.018)	−0.013 (0.024)	−0.009 (0.032)	−0.034 (0.031)
Interstate banking deregulation post indicator × start-up firms	0.047 (0.019)	0.040 (0.023)	−0.026 (0.027)	0.032 (0.030)	0.028 (0.023)	0.046 (0.022)	−0.003 (0.026)	0.005 (0.022)
State-industry-type fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Type-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of 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 intrastate banking reform × start-up firms	−0.002 (0.008)	−0.014 (0.007)	−0.006 (0.007)	−0.013 (0.010)	0.002 (0.007)	−0.008 (0.007)	−0.005 (0.009)	−0.008 (0.008)
Number of years since interstate banking reform × start-up firms	0.038 (0.010)	0.053 (0.012)	0.027 (0.010)	0.012 (0.010)	0.023 (0.010)	0.029 (0.009)	0.022 (0.012)	0.003 (0.013)
State-industry-type fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Type-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	111,894	111,894	111,894	111,894	111,894	111,894	111,894	111,894

Table 8
Table disaggregates churning and long-term entry by employment size of entrant in the first year of operation. Coefficients report estimates of the effect of intrastate and interstate banking deregulations on the entry and exit of new firms relative to state-industry-type-year establishment expansions and contractions for existing multi-unit firms. Table 6 provides further details on the specification.

	Log churning entrants by initial employment				Log long-term entrants by initial employment			
	1–5 (1)	6–20 (2)	21–100 (3)	101+ (4)	1–5 (5)	6–20 (6)	21–100 (7)	101+ (8)
Start-up response relative to multi-unit facilities following intrastate branch banking deregulations								
Years 1–2 before reform	−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)
Reform year and one after	−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)
Years 2–3 after reform	−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)
Years 4+ after reform	−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 interstate branch banking deregulations								
Years 1–2 before reform	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)
Reform year and one after	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)
Years 2–3 after reform	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)
Years 4+ after reform	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-industry-type fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Type-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	111,894	111,894	111,894	111,894	111,894	111,894	111,894	111,894

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 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 could 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 could 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 more than 100 employees. Coefficients on banking reform indicators in these regressions estimate the relative elasticity of start-up 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

Table 9
Panel estimations of incumbent market shares and concentration at state-industry-year level using data from the Longitudinal Business Database (LBD) for 1977–1998. Coefficients report estimates of the effect of intrastate and interstate banking deregulations on industrial concentrations. Dependent variables are indicated in column headers. Cells are constructed by state-industry-year. Regressions include cross-sectional fixed effects for state-industry and longitudinal fixed effects for industry-year. Regressions include unreported interactions of explanatory variables with census-year indicators. Regressions are weighted by average birth employment in cells from 1977 to 1985. Standard errors are clustered at the cross-sectional state-industry level. Incumbent firms are defined as the 10 largest firms in 1980 by state-industry. Market shares are calculated through employments.

	Log top ten incumbent market share (1)	Log top ten incumbent market share (2)	Log top ten market share (3)	Log top ten market share (4)	Normalized Herfindahl–Hirschman index (5)	Normalized Herfindahl–Hirschman index (6)
Panel A. Pre-post specifications						
Intrastate banking deregulation post indicator	−0.019 (0.026)	−0.009 (0.012)	0.001 (0.009)	−0.001 (0.005)	−0.001 (0.002)	−0.001 (0.001)
Interstate banking deregulation post indicator	−0.047 (0.033)	−0.044 (0.022)	−0.005 (0.007)	−0.003 (0.006)	0.002 (0.001)	0.001 (0.001)
State-industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State time trend		Yes		Yes		Yes
Number of observations	55,947	55,947	55,947	55,947	55,947	55,947
B. Linear treatment effect specifications						
Number of years since intrastate banking reform	0.008 (0.009)	−0.003 (0.010)	0.000 (0.004)	−0.001 (0.004)	0.001 (0.001)	−0.001 (0.001)
Number of years since interstate banking reform	−0.008 (0.010)	−0.009 (0.005)	−0.007 (0.002)	−0.007 (0.002)	−0.001 (0.001)	−0.001 (0.001)
State-industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State time trend		Yes		Yes		Yes
Number of observations	55,947	55,947	55,947	55,947	55,947	55,947

entrants with 21–100 employees, and no growth in churning occurred for entrants with more than 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 suggest. 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. Because 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, start-up firms could 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 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 could 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, Holtz-Eakin, and Rosen, 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. It is

Table 10
Dynamic panel estimations of incumbent market shares and concentration at state-industry-year level using data from the Longitudinal Business Database (LBD) for 1977–1998. Coefficients report estimates of the effect of intrastate and interstate banking deregulations on industrial concentrations. 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 provides further details on the specification.

	Log top ten incumbent market share (1)	Log top ten incumbent market share (2)	Log top ten market share (3)	Log top ten market share (4)	Normalized Herfindahl–Hirschman index (5)	Normalized Herfindahl–Hirschman index (6)
Response to intrastate branch banking deregulations						
Years 1–2 before reform	0.027	−0.003	−0.004	0.005	0.003	0.001
	(0.019)	(0.013)	(0.008)	(0.009)	(0.001)	(0.001)
Reform year and one after	0.023	0.001	0.009	0.016	0.001	−0.001
	(0.025)	(0.018)	(0.012)	(0.010)	(0.002)	(0.001)
Years 2–3 after reform	0.005	−0.016	−0.009	0.002	0.001	−0.002
	(0.044)	(0.030)	(0.016)	(0.016)	(0.002)	(0.002)
Years 4+ after reform	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 interstate branch banking deregulations						
Years 1–2 before reform	−0.045	−0.035	−0.008	−0.009	0.000	0.000
	(0.031)	(0.016)	(0.010)	(0.008)	(0.001)	(0.001)
Reform year and one after	−0.085	−0.069	−0.006	−0.005	0.002	0.002
	(0.055)	(0.029)	(0.011)	(0.009)	(0.002)	(0.001)
Years 2–3 after reform	−0.092	−0.078	−0.027	−0.028	0.000	0.000
	(0.065)	(0.032)	(0.013)	(0.010)	(0.002)	(0.001)
Years 4+ after reform	−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-industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State time trend		Yes		Yes		Yes
Number of observations	55,947	55,947	55,947	55,947	55,947	55,947

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 start-ups, 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.

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 simultaneous changes exist 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, show 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. This test is reported in Kerr and Nanda (2009), and we highlight the central findings here. 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, start-up firms and facility expansions enter at 68% and 75% of their maximum three-year sizes, respectively.

These lower relative entry sizes for start-ups could directly reflect financing constraints on the intensive margin, but the differential could include other factors such as increased caution due to greater uncertainty. To assess whether financing constraints play an important role, we test whether start-ups enter closer to their maximum three-year sizes after the banking deregulations using the relative framework equation (3). This approach provides a more direct metric of financing constraints on the intensive margin by looking within-establishment instead of 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 start-up entry sizes compared with 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.

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 10 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. However, null results are found in unweighted or nonlogarithm 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 10 firms by state-industry does decline by 2–3% in the long-run after interstate deregulations. This decline is robust across specification variants. However, 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 discussed above. These differentials, versus specific elasticities in Tables 9 and 10, are what we hope to emphasize. Bertrand, Schoar, and Thesmar (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 such as "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 could be so important for the link between well-functioning capital markets and creative destruction. Using Census Bureau data, this paper shows 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 start-ups 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

[5] In particular, start-ups have different hazard functions of failure relative to facility expansions, and this could 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).

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 showing a fall in credit extended to small businesses in the early 1990s (e.g., Berger, Klapper, and Udell, 2001; Zarutskie, 2006), we also find a dip in start-up activity over that period. We further show how the relative growth of start-up entry has lagged behind the growth of establishment openings by existing firms since the late 1970s (e.g., Davis, Haltiwanger, Jarmin, and Miranda, 2006). The positive elasticities of our panel estimations, however, suggest that increases in banking competition in part dampened national declines in start-up 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 leads to higher failure rates. The concentration of failures among small start-ups, 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 could 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 could have led to different lending strategies (e.g., Berger and Udell, 1995; Erel, 2009; Berger, Miller, Petersen, Rajan, and Stein, 2005; Sah and Stiglitz, 1986) or weakened the ability of banks to evaluate small business projects, with negative consequences for the survival of start-ups. However, the higher churning result could 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 (e.g., markets for corporate control, allocation of credit, technology diffusion) that should be investigated in the entrepreneurial context.

Such an approach could 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 could 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 could 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 start-ups, this could 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, Haltiwanger, and Krizan, 2001). It is puzzling that the productivity growth shown 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, Haltiwanger, Jarmin, and Miranda (2006) note that the aggregate US trend toward 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. Clearly, much more must be learned about how banking competition, entrepreneurship, creative destruction, and productivity growth all tie together.

Appendix A

Fig. A1 provides separate estimations for single-unit and multi-unit establishments. Tables A1–A5 report extended empirical results.

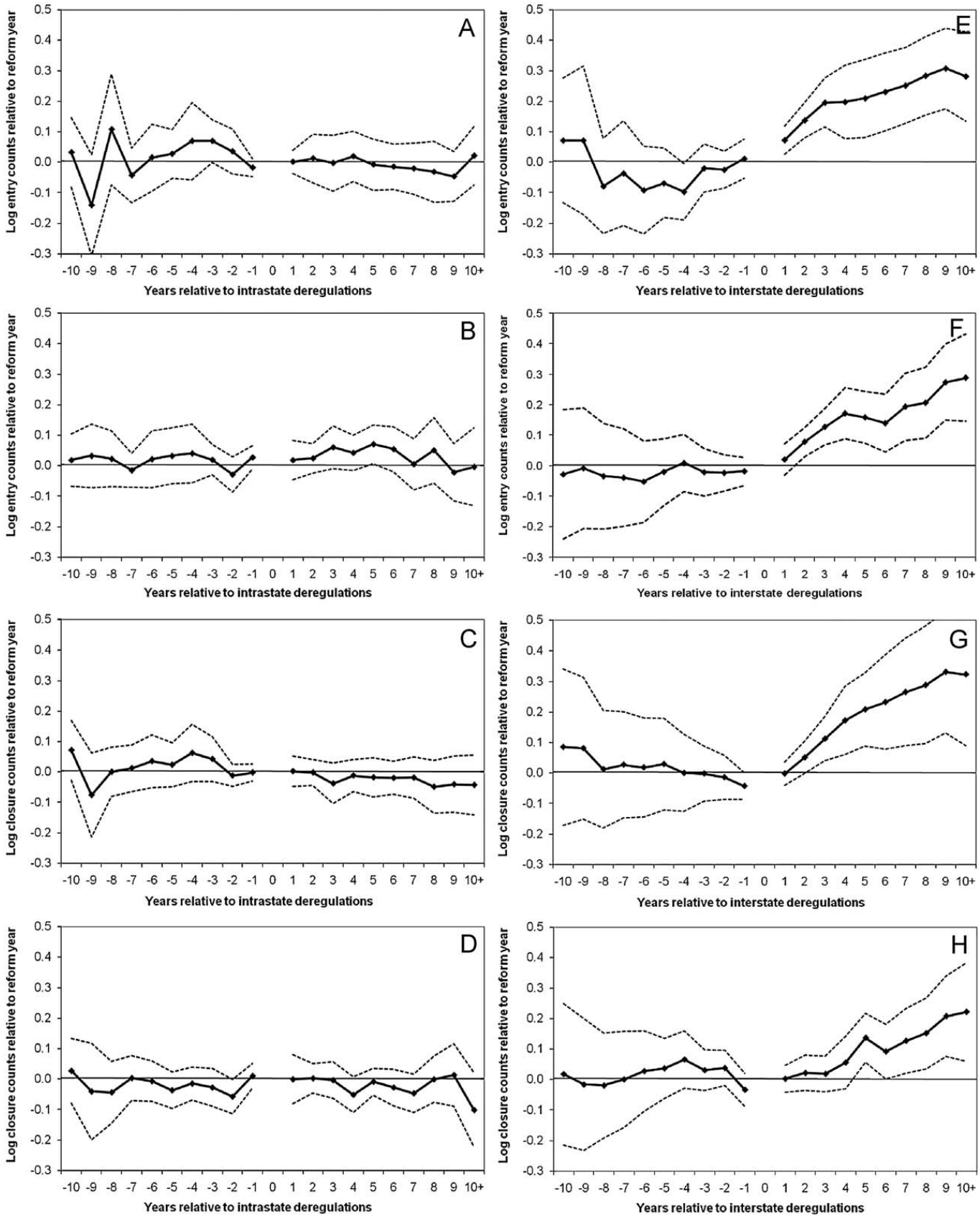

Fig. A1. Single-unit and multi-unit dynamics for entry and closures. Panel A. Intrastate, SU birth, Panel B. Intrastate, MU birth, Panel C. Intrastate, SU closures, Panel D. Intrastate, MU closures, Panel E. Interstate, SU birth, Panel F. Interstate, MU birth, Panel G. Interstate, SU closures, Panel H. Interstate, MU closures.

Table A1
Timing of state branch banking deregulations. Deregulations prior to 1970 are listed as 1970.

State	Intrastate de novo deregulation	Intrastate mergers and acquisitions deregulation	Interstate 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

Table A2
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 (1)	Multi-unit facility expansions (2)	New single-unit start-ups (3)	Multi-unit facility expansions (4)	New single-unit start-ups (5)	Multi-unit facility expansions (6)	Single-unit firms (7)	Multi-unit facility closures (8)
Panel A. Pre-post specifications								
Intrastate banking deregulation post indicator	0.038 (0.022)	−0.034 (0.027)	0.014 (0.035)	−0.031 (0.039)	0.062 (0.021)	−0.033 (0.024)	−0.033 (0.032)	−0.059 (0.029)

Table A2 (*continued*)

	Log total entrants		Log churning entrants		Log long-term entrants		Log establishment closures	
	New single-unit start-ups (1)	Multi-unit facility expansions (2)	New single-unit start-ups (3)	Multi-unit facility expansions (4)	New single-unit start-ups (5)	Multi-unit facility expansions (6)	Single-unit firms (7)	Multi-unit facility closures (8)
Interstate banking deregulation post indicator	0.057 (0.021)	0.019 (0.015)	0.052 (0.032)	0.028 (0.034)	0.065 (0.018)	0.016 (0.016)	0.014 (0.026)	−0.036 (0.023)
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,097	1,097	1,097	1,097	1,097	1,097	1,097	1,097
Panel B. Linear treatment effect specifications								
Number of years since intrastate banking reform	−0.002 (0.011)	0.001 (0.010)	−0.004 (0.013)	−0.002 (0.011)	0.000 (0.013)	0.002 (0.010)	−0.003 (0.013)	−0.007 (0.015)
Number of years since interstate banking reform	0.047 (0.012)	0.021 (0.009)	0.061 (0.015)	0.031 (0.011)	0.037 (0.012)	0.016 (0.009)	0.032 (0.009)	0.000 (0.010)
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,097	1,097	1,097	1,097	1,097	1,097	1,097	1,097

Table A3
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 (1)	Multi-unit facility expansions (2)	New single-unit start-ups (3)	Multi-unit facility expansions (4)	New single-unit start-ups (5)	Multi-unit facility expansions (6)	Single-unit firms (7)	Multi-unit facility closures (8)
Response to intrastate branch banking deregulations								
Years 1–2 before reform	−0.046 (0.032)	−0.011 (0.026)	−0.063 (0.045)	−0.020 (0.047)	−0.026 (0.024)	−0.004 (0.023)	−0.018 (0.027)	0.006 (0.023)
Reform year and one after	−0.057 (0.045)	−0.018 (0.049)	−0.074 (0.067)	−0.020 (0.076)	−0.039 (0.034)	−0.016 (0.040)	−0.016 (0.048)	0.009 (0.043)
Years 2–3 after reform	−0.063 (0.049)	0.006 (0.052)	−0.078 (0.066)	0.005 (0.065)	−0.044 (0.051)	0.003 (0.052)	−0.022 (0.057)	0.004 (0.054)
Years 4+ after reform	−0.055 (0.051)	0.015 (0.058)	−0.069 (0.068)	0.005 (0.085)	−0.037 (0.058)	0.021 (0.053)	−0.006 (0.063)	−0.022 (0.065)
Response to interstate branch banking deregulations								
Years 1–2 before reform	0.054 (0.034)	−0.014 (0.023)	0.034 (0.038)	−0.032 (0.032)	0.075 (0.035)	−0.002 (0.022)	−0.028 (0.022)	−0.053 (0.018)
Reform year and one after	0.089 (0.035)	−0.005 (0.029)	0.069 (0.046)	−0.016 (0.050)	0.111 (0.034)	0.002 (0.029)	−0.009 (0.035)	−0.076 (0.029)
Years 2–3 after reform	0.201 (0.045)	0.058 (0.037)	0.208 (0.053)	0.079 (0.055)	0.201 (0.053)	0.051 (0.036)	0.053 (0.046)	−0.073 (0.046)
Years 4+ after reform	0.234 (0.073)	0.081 (0.058)	0.292 (0.079)	0.101 (0.064)	0.188 (0.076)	0.076 (0.067)	0.148 (0.053)	−0.035 (0.051)
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,097	1,097	1,097	1,097	1,097	1,097	1,097	1,097

Table A4
Transition estimations from state-year panels to state-industry-type-year panels. Column 1 presents state-industry-type-year estimations for entry counts without state-industry-year fixed effects. Column 1 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-industry-year fixed effects. In these saturated models, the start-up response is estimated relative to facility expansions, and separate coefficients for expansion establishments are not estimated. Tables 4 and 6 provide further details on the specifications.

	Log entry counts (1)	Log entry counts (2)
Single-unit start-ups interactions		
Years 1–2 before intrastate reform	−0.034 (0.030)	−0.013 (0.023)
Intrastate reform year and one after	−0.040 (0.038)	−0.020 (0.036)
Years 2–3 after intrastate reform	−0.051 (0.028)	−0.084 (0.036)
Years 4+ after intrastate reform	−0.032 (0.037)	−0.044 (0.030)
Years 1–2 before interstate reform	0.032 (0.027)	0.021 (0.034)
Interstate reform year and one after	0.056 (0.027)	0.038 (0.034)
Years 2–3 after interstate reform	0.153 (0.032)	0.071 (0.039)
Years 4+ after interstate reform	0.201 (0.039)	0.109 (0.037)
Multi-unit facility expansions interactions		
Years 1–2 before intrastate reform	−0.022 (0.030)	Absorbed
Intrastate reform year and one after	−0.020 (0.048)	
Years 2–3 after intrastate reform	0.033 (0.044)	
Years 4+ after intrastate reform	0.012 (0.052)	
Years 1–2 before interstate reform	0.011 (0.034)	
Interstate reform year and one after	0.018 (0.037)	
Years 2–3 after interstate reform	0.082 (0.036)	
Years 4+ after interstate reform	0.092 (0.048)	
State-industry-type fixed effects	Yes	Yes
State-industry-year fixed effects		Yes
Type-year fixed effects	Yes	Yes
Number of observations	111,894	111,894

Table A5
Size distribution of aggregate relative start-up entry. Table 8 provides further details on the specification.

	Log entry count by initial employment			
	1–5 (1)	6–20 (2)	21–100 (3)	101+ (4)
Start-up response relative to multi-unit facilities following intrastate branch banking deregulations				
Years 1–2 before reform	−0.024	−0.016	−0.044	−0.037
	(0.031)	(0.031)	(0.027)	(0.043)
Reform year and one after	0.006	−0.051	−0.046	−0.021
	(0.030)	(0.039)	(0.046)	(0.052)
Years 2–3 after reform	−0.086	−0.078	−0.049	−0.129
	(0.039)	(0.036)	(0.042)	(0.051)
Years 4+ after reform	−0.029	−0.066	−0.059	−0.056
	(0.036)	(0.036)	(0.030)	(0.051)
Start-up response relative to multi-unit facilities following interstate branch banking deregulations				
Years 1–2 before reform	0.040	0.049	0.034	0.084
	(0.036)	(0.038)	(0.038)	(0.035)
Reform year and one after	0.047	0.091	0.000	0.060
	(0.024)	(0.027)	(0.029)	(0.048)

Table A5 (*continued*)

	Log entry count by initial employment			
	1–5 (1)	6–20 (2)	21–100 (3)	101+ (4)
Years 2–3 after reform	0.122 (0.035)	0.144 (0.038)	0.054 (0.048)	0.117 (0.039)
Years 4+ after reform	0.149 (0.045)	0.227 (0.038)	0.076 (0.046)	0.079 (0.045)
State-industry-type fixed effects	Yes	Yes	Yes	Yes
State-industry-year fixed effects	Yes	Yes	Yes	Yes
Type-year fixed effects	Yes	Yes	Yes	Yes
Number of observations	111,894	111,894	111,894	111,894

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