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Does banking competition affect startup activity in the United States?

Evidence on recent startup measures.

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Abstract

The Interstate Banking and Branching Efficiency Act (IBBEA) of 1994 represented a major milestone in the U.S. banking deregulation process, as it permitted banks to open branches and buy other banks across state borders. We exploit the relaxation of IBBEA branching provisions with recent data from the Startup Cartography Project to estimate the impact of banking competition on entrepreneurship. The findings suggest that while there is a slight decrease in business entry and startup potential following the IBBEA implementation, the changes in branching provisions have a more permanent positive impact on both measures of entrepreneurship. This study argues that states that removed barriers later in the deregulation process experienced more pronounced effects on their startup ecosystems. Additionally, states with poor competitive banking markets benefited the most from these policies. This last finding supports the idea that banking competition plays an important role in fostering small business activities in the United States. Overall, these results provide further insights into how banking deregulation impacts states and how other factors can influence state responses to these policies.

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Main Abbreviations

CD	Cross-Sectional
DiD	Difference-in-differences
DK	Driscoll-Kraay (1998)
EQI	Entrepreneurship Quality Index
FE	Fixed Effects
GDP	Gross Domestic Product
HHI	Herfindahl-Hirschman Index
IBBEA	Interstate Banking and Branching Efficiency Act
IDUM	Interstate branching dummy
LFPR	Labor Force Participation Rate
OLS	Ordinary Least Squares
REAI	Regional Ecosystem Acceleration Index
RECPI	Regional Entrepreneurship Cohort Potential Index
RSIndex	Rice and Strahan (2010) index
SCP	Startup Cartography Project
SFR	Startup Formation Rate
UER	Unemployment Rate

General Introduction

Young businesses play a crucial role for the U.S. economy. The growth of these firms contributes significantly to both gross and net job creation in the United States (Haltiwanger, Jarmin & Miranda, 2013). Additionally, there is a growing recognition of the role of startups as major contributors to innovation (Chava, Oettl, Subramanian, and Subramanian, 2013; Van Praag & Versloot, 2007). Considering that innovation and job creation are key drivers of economic growth (Audretsch, 2007; Solow, 1957), it is important to understand the effects of regulations on small business activities. For these reasons, the literature has seen an increasing amount of research conducted by policymakers and researchers investigating the effect of various factors on entrepreneurial activity.

At the same time, the structure of the U.S. banking system also has a real impact on the growth of the economy (Allen, Carletti & Gu, 2019). Over the past century, the sector has experienced various periods of regulation and deregulation. An important milestone of these events has been the establishment of the Riegle-Neal Interstate Banking and Branching Efficiency Act (IBBEA) of 1994 in the United States. While the main focus of policymakers has been on controlling for competition and stability within the banking sector, it is important to recognize that these policies have broader implications beyond the financial sector itself, including for the activities of young firms.

In this thesis, we aim to explore the relationship between banking competition and startup ecosystems, specifically by studying startup formation during the staggered passage of the IBBEA. The underlying assumption of this work is based on evidence suggesting that the lifting of interstate branching restrictions by the Act has led to increased banking competition across state lines (Johnson & Rice, 2008). This increased competition has, in turn, brought various changes in the structure and the services of the sector, such as a decrease of the local market power of banks (Chava et al., 2013) and a decrease of the price of credit for small businesses (Rice & Strahan, 2010). As a result, the rising competition might have serious implications for young private firms.

The literature that studies the effect of banking competition on entrepreneurship is extensive, and several channels through which banks influence the survival and success of startups have been investigated. Given that the banking system is the main provider of credit to small businesses, existing evidence suggests that young firms are sensitive to changes in interest rates and credit volume (Mach, Wolken, Carter, Holmes & Hazelwood, 2006; Rice & Strahan, 2010). Kroszner and Strahan (2014) also argue that the role of bank lending for small businesses becomes even more crucial during time of economic distress or crisis.

The primary challenge in empirical research on startup ecosystems is the issue of endogeneity, as young firms are influenced by various company and market characteristics. Therefore, simply observing a correlation between startups and banking competition cannot explain the effect of banking competition *on* startups (Cornaggia, Mao, Tian & Wolfe, 2015). Based on the approach widely used in the literature, we address this endogeneity constraint by leveraging the relaxation of IBBEA branching provisions between 1994 and 2005.

To capture entrepreneurial activity, we adopt the framework of Andrews, Fazio, Guzman, Liu, and Stern (2022), who created four variables that reflect both quantitative and qualitative measures of startup ecosystems at the county-year level. Their work, known as the *Startup Cartography Project* (SCP), has shown various advantages in studying entrepreneurship dynamics. One of the main contributions of the SCP is its consideration of the skewness of startup outcome measures. This implies that, out of the vast number of firm births each year, a few of them will account for a long-lasting impact on the economy (Decker, Haltiwanger, Jarmin & Miranda, 2014). As a result, evaluating the effect of regulations on entrepreneurship requires not only considering the number of new companies founded, but also assessing their potential to generate meaningful growth outcomes (Guzman & Stern, 2020).

In our study, we combine data from the SCP, which tracks quantity, quality and potential performance of entrepreneurship, with data from the interstate branching provisions settled by the IBBEA. Our resulting dataset contains the startup statistics and an index created from the changes in provisions for fifty states from 1990 until

2005. Using a difference-in-differences approach at the state-year level with state and year fixed effects, our baseline model examines the possible relationship between banking competition and entrepreneurial activity.

To address dynamic issues, we extend our model in two ways. First, we suppose that changes in restrictions can have an impact on several years and not only on the present year of deregulation. For that, we adapt our baseline model by taking the total of the three next years of our outcome variables. This allows us to capture any potential time-dependent effect of banking competition. Second, we also examine possible differences across groups of states based on their IBBEA implementation year. This approach is evaluated using a difference-in-difference-in-differences approach that considers heterogenous effect for each state cohort.

From these three models, the significance of the results vary greatly depending on the computation of the standard errors. Overall, after accounting for spatial dependence, we find a direct negative impact of the initial (1994-1997) interstate branching laws on both the quantity and the quality-adjusted quantity of entrepreneurship. On the other hand, the relaxation of the provisions (1995-2005) have a more permanent positive impact on the measures of startup ecosystems. The aggregate effects still suggest a positive relationship between banking competition and startup activity.

We also observe a more pronounced positive effect for states that deregulated later (i.e., in 1997). This difference is attributed to the increasing expansion opportunities created by a larger network of deregulated states. The adoption of the IBBEA by more states has increased the potential for banks to expand operations across state borders, which has had a noticeable impact on the activity of small businesses. This also suggests that early deregulation may not have sufficiently fostered banking competition in those states to enhance small business activity.

When analyzing the robustness of the baseline regression by including the HHI into the model, we detect that banking concentration also significantly influences the impact of the interstate branching deregulation. States with poor banking competition have experienced more pronounced positive effects from the

deregulation on business entry and the startup potential than highly competitive states. This discovery suggests that banking competition is important for an active entrepreneurial ecosystem. As a result, states with a higher banking concentration prior the Riegle-Neal Act were disadvantaged to enhance startup activity compared to more competitive states.

These findings contribute to a more nuanced understanding of the impact of banking competition on startups and provide valuable insights for policymakers and researchers interested in studying entrepreneurship dynamics in the context of interstate branching provisions.

The central contribution of this work is the combination of recent empirical measures of startup ecosystems with established methods to proxy banking competition in the United States. The recent study of Andrews et al. (2022) has indeed opened up a new era of research on the factors influencing entrepreneurial activity in the United States. Henceforth, in this work, we decide to leverage their measures by combining them with the *RSIndex* developed by Rice and Strahan (2010), which assess the level of restrictiveness towards interstate branching deregulation at the state-year level.

Our conclusions from this study are informative for policy design. While interstate deregulation was initially intended to diversify the bank product lines and expand geographic markets (Kane, 1996), our main hypothesis suggests a positive relationship between interstate branching deregulation and both business entry and startup potential. Although we do find significant estimates, further research is needed to establish a precise quantitative relationship between these factors.

The remainder of this work proceeds as follows. Section 1 provides a brief review of the related literature examining the link between banking competition and entrepreneurship activity. Section 2 presents a description of the sources and construction of the data, along with a step-by-step explanation of the methodology. In Section 3, additional models and their corresponding empirical results are presented. The final section offers a brief conclusion, along with an identification of the limitations of our strategy and areas for further research.

1 Literature Review

This work examines several strands of literature. First, we will discuss the literature investigating banking competition through the U.S. deregulation events that started during the second half of the twentieth century. The gradual opening of states towards bank branching provides a unique natural experiment to understand the effect of banking competition on the economy. Before this liberalization, U.S. banks were facing geographic restrictions on both intrastate and interstate expansion¹. Then, the opening up of the banking system did not occur all at once. Although several states pulled out intrastate expansion restrictions before 1978, it is recognized as the beginning of a fifteen-year period during which states progressively allowed banking expansion (Jayaratne & Strahan, 1999). The Riegle-Neal Interstate Banking and Branching Efficiency Act of 1994 (IBBEA) is identified as the culminating step in the process towards unrestricted nationwide interstate banking and branching activities. It allows nationwide acquisition of banks without taking count of bilateral agreements, unless a state has expressly objected to them². Additionally, IBBEA also includes provisions to allow states to still erect barriers against out-of-state entry. These laws were not necessarily lifted at the same time by states. As a result, a growing body of literature, initiated by Johnson and Rice (2008) and Rice and Strahan (2010), has used these changes in provisions as a measure of banking competition at the state-year level.

Extensive literature has studied the real effects of these deregulation on the economy. Jayaratne and Strahan (1996) were among the first to study the impact of intrastate branch banking deregulation. They find evidence of a significant increase on the real, per capita growth in income and output. Strahan (2003) suggests that the removal of limits on bank entry and expansion was followed by better

¹ Kane (1996) carefully describes the role of these restrictions in the relationship between banks and customers.

² For better insight, Jayaratne and Strahan (1999) provide a detailed history of the U.S. branch banking deregulation events.

performance of the real economy. Jayaratne and Strahan (1999) observe a significant growth of state economies after deregulation.

Other studies have analyzed the influence of deregulation on the performance and efficiency of the banking industry. The impact appears to be ambiguous as research papers find a significant positive relationship between expansion reforms and banking efficiency (Carow & Heron, 1998; Jayaratne & Strahan, 1998; 1999; Stiroh & Strahan, 2003; Tirtiroglu, Daniels & Tirtiroglu, 2005), while other papers find mixed or minimal link between the two instances (Grabowski, Rangan & Rezvanian, 1994; Zou, Miller & Malamud, 2011). Overall, evidence suggests that the significant effects are driven by better-performing banks and their ability to leverage new expansion possibilities (Jayaratne & Strahan, 1999, Stiroh & Strahan, 2003).

There is also evidence that banking competition affects U.S. startup ecosystems. To grow and leverage their businesses, access to outside funding is crucial for startups with long-term perspectives (Stam & van de Ven, 2021). In that regard, banks play an important role (Berger & Black, 2019), typically small banks due to the limited options that startups face when seeking external funds (Berger & Udell, 2002; Petersen & Rajan, 1994). Researchers seem to agree that banking competition benefits new firms' access to credit (Cetorelli & Gambera, 2001; Elitcha, 2021; Love & Martinez Peria, 2015). Vera and Onji (2010) documents that IBBEA compressed the number of small banks, but not the credit supply to small businesses. Patel (2021) observes a significant effect of the IBBEA on the loan amount of small firms and their probability of default. Demyanyk (2008) studies the effect of intrastate banking deregulation on credit access for self-employed individuals and observe a positive link. Along with Demyanyk (2008), Rice and Strahan (2010) and Cetorelli and Strahan (2006) find a similar positive relationship using interstate branching and banking deregulation, respectively. Rice and Strahan do not find any significant effect on small firms' overall credit supply following the IBBEA, but document that, in less regulated states, small firms borrow at lower rates than in more restricted states.

Several articles have tried to identify the influence of U.S. banking reforms on innovation, as it represents a key factor for economic growth (Audretsch, 2007; Solow, 1957). The effects are mixed depending on the type of deregulation and whether we look at the impact on small, private or public businesses (Amore, Schneider & Zaldokas, 2013; Chava et al., 2013; Cornaggia et al., 2015; Hombert & Matray, 2017). When focusing on private firms, interstate deregulation decreases banks' bargaining power but it increases the level of innovation (Chava, et al., 2013; Cornaggia et al., 2015). However, intrastate deregulation presents the opposite effect; it raises the local market power of banks and therefore reduces the level of innovation for young, private firms (Chava et al., 2013; Hombert & Matray, 2017).

Different articles have investigated the relationship between banking competition and business creation but the effect appears to be uncertain. Several studies (Backman, 2015; Black & Strahan, 2002; Kerr & Nanda, 2009; Strahan, 2003) derive a positive relationship on new business formation, whereas Rogers (2012) finds a negative impact. Elitcha (2021) finds an overall positive link that attenuates as the stock market increases. Rogers (2012) shows that more small banks benefit new firm creation. Bonaccorsi Di Patti and Dell'Ariccia (2004) find a bell-shaped relationship which depends on the opaqueness of industries.

Other studies have analyzed the effect of changes in branching restrictions while accounting for spatial dependence between states. Huang (2008) assesses the difference in economic performance of counties separated by a state border and with different banking restrictions. He finds that five out of the twenty-three deregulation events present a significant difference in growth acceleration. Spierdijk, IJtsma and Shaffer (2021) observe a significant growth effect after intrastate deregulation but no effect following interstate deregulation. McCrory and Messer (2020) examine the economic growth induced by interstate deregulation while taking the timing and bilateral agreements between states into account. Chang (2017) investigates the effect of the IBBEA on the access to finance for small firms engaged in research and development. His approach categorizes states according to their IBBEA implementation year and find different results for each stage of the Riegle-Neal implementation process.

Since this work aims to analyze the impact of U.S. deregulation on startup activity, it is essential to mention recent studies that have worked with the data from the *Startup Cartography Project* by Andrews et al. (2022). For instance, Guzman and Kacperczyk (2019) have analyzed the difference between male and female founders' access to external financing. A study from Mostaghel, Oghazi and Patel (2023) investigates the impact of rugged individualism on entrepreneurial activity by focusing on both the quantity and the quality-adjusted quantity of entrepreneurship. While using the same startup measures, Fazio, Guzman and Stern (2020) observe a significant impact from tax credit on these variables. Other studies focus on the role of regional clusters of entrepreneurship (Guzman & Stern, 2020; Stam & van de Ven, 2021), the choices of founders at the early stage (Catalini, Guzman & Stern, 2019) or the proximity of research and university institutions (Tartari et al., 2021) on the performance of startups and their ability to achieve growth outcomes.

2 Methodology

This section presents a detailed exposition of the data used in our analysis, a description of our methodology, the summary statistics and the formulation of the baseline regression. We will first elaborate on the nature of the data characterizing banking competition, entrepreneurial activity and the control variables. Then, we discuss the transformation applied on these variables and expose summary statistics that provide a condensed overview of the trends in the data. The analytical approach adopted here aims to facilitate a comprehensive understanding of the key factors that might influence the startup ecosystems.

2.1 Data

2.1.1 Interstate branching deregulation: independent variable

Significant changes in restrictions have occurred in the U.S. banking sector for almost a century now. The first event that shook up the sector was the 1927 McFadden Act which formalized the authority of states for the regulation of in-state branching. After that, U.S. banks have gone through decades of restrictions regarding the geographic expansion of branches until the establishment of the IBBEA in September 1994 (Kroszner and Strahan, 2014).

Along the way, one can distinguish two levels of deregulation: *intrastate* deregulation, which allowed banks to expand within states, and *interstate* deregulation, which allowed banks to expand across states lines. Although some states decided to deregulate in the 1930s, most states enforced their within-state regulations until 1970. But in the 1970s, mindsets evolved and states progressively removed barriers. The number of states permitting intrastate branching increased from twelve to thirty-eight by the end of 1994 (Rice and Strahan, 2010).

The first step towards interstate banking came in 1978 when Maine permitted out-of-state entry if banks of Maine could also enter other states³. No state reciprocated until New York and Alaska adopted similar laws in 1982. Following these changes, by 1992, all states but Hawaii had passed laws in favor for interstate banking. However, these legislations did not allow banks to open out-of-state branches. The culmination to full interstate banking and branching was established by the Riegle-Neal Act (IBBEA) in 1994. This act effectively authorized banks and holding companies to enter other states without permission and command branches across state borders. Figure 1 presents the history of the important events of the banking deregulation process in a simple timeline.

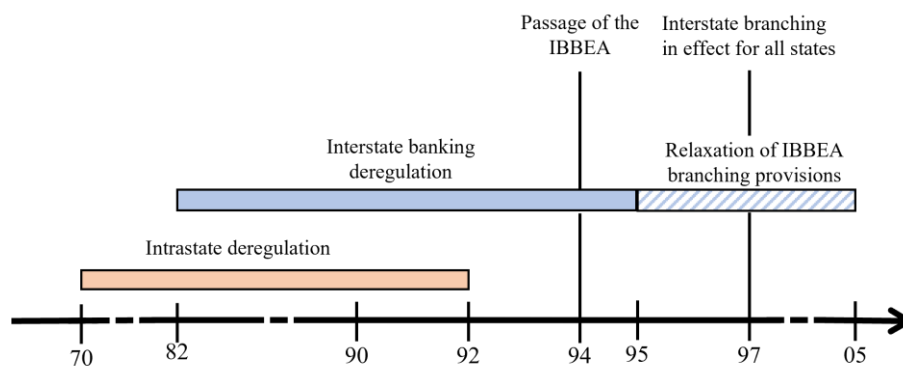


Figure 1: History of the banking and branching deregulation process from 1970 to 2005 (Rice and Strahan, 2010).

From the IBBEA, we can differentiate two phases: one involving interstate *banking*, and the other interstate *branching*. Although the IBBEA authorized expansion across state borders without permission, the Act included flexible provisions for both phases so that states could still erect barriers for out-of-state entry. As previously mentioned, several states had already permitted out-of-state entry with bilateral agreement before the IBBEA. Consequently, we will not examine the banking provisions as they do not constitute a major change from the status quo. However, the branching provisions represented a major relaxation of the interstate restrictions (Kroszner & Strahan, 2014). These laws empowered states to deviate from the initial settings of the IBBEA by stipulating additional barriers on: (i) de

³ Entry, in this case, means the ability to acquire existing whole banks. Entry via branching was still not permitted (Rice and Strahan, 2010).

novo branching; (ii) the minimum age of the target institution in case of mergers; (iii) the acquisition of individual branches without acquiring the entire bank; (iv) the maximum amount of statewide deposits controlled by a single bank or bank holding company⁴. These provisions were required to be implemented from June 1995 until the trigger date of June 1, 1997. We refer to this period (1995-1997) as the IBBEA implementation period for the rest of this work. After 1997, states could still remove any existing provisions in order to favor out-of-state entry (Johnson & Rice, 2008).

Following Rice and Strahan (2010), we use these changes of provisions to create an index, further called *Index*. This *Index* counts each time a state adds any of the above four barriers and ranges from zero to four the restrictiveness of states. The value zero indicates states completely open to interstate branching, and four indicates states with the most restrictive provisions. Specifically, we add one to the index if a state puts a minimum age of three years or above; does not allow de novo branching; does not allow partial branching or puts a statewide deposit cap smaller than thirty percent on branch acquisitions. Given that interstate branching was not exercised before the passage of the IBBEA, we assign the value of four to the *Index* before the implementation, as it is the highest level the index can take⁵. Table A.1 presents the timing of these changes retrieved from Rice and Strahan (2010). To get a better representation of the *Index*, Figure A.1 displays the evolution of the number of states clustered by their *Index* level.

We also build *IDUM*, a dummy variable that has a value of zero before the focal state implemented the IBBEA and one otherwise. This variable is used in our model to define the post-IBBEA implementation for each state. We include this dummy

⁴ De novo branching, as opposed to the acquisition of an existing branch, is the creation of a new branch.

⁵ A few states (Massachusetts, New York, Alaska, Oregon, Rhode Island, Nevada, North Carolina and Utah) allowed interstate branching before the IBBEA (Amel, 1993). But it was under the condition of national reciprocal, so almost no states exercised interstate branching before 1994 (Rice & Strahan, 2010). Moreover, we do not refer here to interstate branching via Multi-Bank Holding Companies (MBHC).

to ensure that the model estimates precisely the effect of changes in IBBEA provisions only.

Finally, we construct one additional variable HHI that we use to check the robustness of the baseline model. This Herfindahl-Hirschman Index (HHI) is computed on data from 1994 to 2006 as follows⁶:

$$HHI_{i,t} = \sum_b^N s_{b,i,t}^2,$$

where $s_{i,s,t}^2$ is the square of the market share of deposits of the bank institution b registered in state i during year t . The data is manually retrieved from the annual Summary of Deposits survey issued by the Federal Deposit Insurance Corporation (FDIC)⁷. This measure is constructed to capture the level of competition of each state throughout the passage of the IBBEA.

2.1.2 Entrepreneurial activity: dependent variable

We use data from the *Startup Cartography Project* (SCP) from Andrews et al. (2022) to represent entrepreneurial activity in the United States. This dataset offers both quantitative and qualitative measures of startup ecosystems in all fifty states from 1988 until 2016. In our analysis, we focus on two outcome variables: the *Startup Formation Rate (SFR)* and the *Regional Entrepreneurship Cohort Potential Index (RECPI)*. The *SFR* quantifies the number of new business registrants within a cohort of firms for a specific year⁸. SCP records a business registration when an individual registers a new business as a corporation, a Limited Liability Company (LLC), or partnership (Andrews et al., 2022). The *Entrepreneurship Quality Index*

⁶ The HHI is a widely accepted measure of concentration among a specific sector and geographic area. It takes the relative size distribution of the firms in a market (The United States Department of Justice, 2018).

⁷ While the HHI is based on market shares of deposits, it provides a good indicator of the competitive environment within each state throughout the IBBEA implementation. Also, the Summary of Deposits dataset does not provide data before 1994.

⁸ A cohort, as defined here, is a particular group of individuals under study.

(*EQI*) proposes an index of the average quality within a geographic region. In this context, ‘quality’ refers to the probability of achieving growth outcomes (i.e., achieving an initial public offering (IPO) or a high-value acquisition within six years of founding). This probability indicator is constructed from the aggregation of twelve variables, each representing startup characteristics⁹. The *RECPI* offers an indicator of the overall inherent potential for startups within an area. It is computed by combining the number of firms and the average ‘quality’ of startups for a state and a specific year. This calculation enables Andrews et al. to measure the quality-adjusted entrepreneurial activity within each state. While the *EQI* permits direct ecosystem comparison across states, *RECPI* refer to the expected number of growth outcomes based on the number of new startups and their probability of success:

$$RECPI = EQI * SFR$$

Finally, the *Regional Ecosystem Acceleration Index (REAI)* confronts the startup potential, given by *RECPI*, with the actual number of growth events, given by g . In other words, it is the ratio of realized to expected growth events:

$$REAI_{i,t} = \frac{\sum g_{jit}}{RECPI_{i,t}}$$

A value above one for the *REAI* means that there are more growth outcomes than expected within the focal state. This measure is not discussed in this study but it can be interpreted as the capacity for firms to perform better than expectations among a specific area. See Andrews et al. (2022) for a detailed discussion of these statistics and their ability to capture entrepreneurial ecosystem performance.

⁹ Two models are available from the SCP, the academic and the policy model. In the context of this work, we will use the academic model. The policy model is a modified version where only the startup characteristics that are connected with the “legal and intellectual property environment surrounding the firm” (Andrews et al., 2022, p. 2) are considered.

2.1.3 Control variables

In order to account for macroeconomic conditions and business cycles, we insert the *Gross Domestic Product* and the *Population* in the regression. We choose to use the population size rather than the population growth because we argue that valuable information is captured by the difference in population size across states. The same reasoning applies to *GDP*. These two variables are obtained from the U.S. Bureau of Economic Analysis (BEA).

As startups are led by individuals, we control for other demographic factors by including the average *Unemployment Rate* and the average *Labor Force Participation Rate (LFPR)* from the U.S. Bureau of Labor Statistics (BLS). Following Cornaggia et al. (2015), we also account for the effects of interstate banking deregulation by including the indicator variable *Inter*, which takes the value zero before a state allowed interstate banking and one thereafter (Black & Strahan, 2002). The timing used for constructing *Inter* are reported in Table A.2 (Kerr & Nanda, 2009). We observe all these controls for each state and each year.

2.1.4 Summary Statistics

To investigate the relationship between banking competition and startup activity in the United States, we collected observations of 49 states and the District of Columbia over a 17-year period from 1990 to 2006¹⁰. This time period is intended to cover the pre-reform period, the date of enactment of the IBBEA, and the last change in provisions in 2005¹¹.

Table 1 summarizes the statistics for all variables. The number of new business registrations varies greatly across states. It ranges from a minimum of 40 for Maine to over 287,000 for Florida, with an average of 21,848 new registrants per year. The

¹⁰ For the sake of simplicity, we will consider the District of Columbia as a state for the rest of this work.

¹¹ Only data for the state of Delaware is missing from the SCP dataset (Andrews et al., 2022)

potential of startups, given by *RECPI*, ranges from a minimum of 0.03 to a maximum level of 308. Figure A.6 shows the distribution of states based on both average *SFR* and average *RECPI*. From this graph, we clearly observe that five states outperform one or both outcome variables (i.e., California, Florida, Texas, New York and Massachusetts). We will exclude these states from the analysis to test the robustness of our results.

From the summary statistics, we suspect the distributions of *SFR* and *RECPI* to be positively skewed in each time series¹². Accounting for that, we take the natural logarithm of these variables in all regressions. The same reasoning applies to the variables *GDP*, *Population* and the *Labor Force Participation Rate (LFPR)*. The population varies significantly from 453 thousand to 36 million within a specific

Table 1: Summary statistics for all variables.

STATISTIC	MIN	PCTL(25)	MEDIAN	MEAN	PCTL(75)	MAX	ST. DEV.	N
SFR	40	4,543.5	12,143	21,847.9	26,225.2	287,591	30,323.1	850
EQI	0.0001	0.0003	0.0004	0.001	0.001	0.004	0.0004	850
RECPI	0.032	1.404	5.141	13.162	13.808	308.307	28.778	850
INDEX	0	1.2	3	2.788	4	4	1.509	850
GDP	12,952.3	51,411.9	123,783.4	213,076.7	262,127.7	2,002,437.0	267,983.3	850
POPULATION	453,690	1,585,106	3,804,203	5,487,022	6,295,567	36,021,202	6,014,906	850
UER	2.10	4.23	5.15	5.67	6.17	30.42	3.71	850
LFPR	54.84	64.94	67.70	67.47	70.55	75.33	3.82	850
INTER	0	1	1	0.992	1	1	0.090	850
HHI	114.760	457.037	698.183	895.961	1,061.438	6,400.970	693.614	650
IDUM	0	0	1	0.640	1	1	0.480	850

¹² In this context, "skewness" refers to the asymmetric distribution of the observations. Positive skewness indicates that observations are more densely concentrated towards the upper tail of the distribution (i.e., at the end of the time series). This can affect the accuracy of estimates when performing an OLS estimation, as OLS assumes the observations to be normally distributed.

state, and states have an average unemployment rate of 5.677% and an average labor force participation rate of 67.475%.

Figure A.1 displays the evolution of the number of states by *Index* level. We observe a distinct transition from total restriction (i.e. *Index* level four) to open conditions for interstate bank branching (i.e. *Index* level zero), with fourteen out of fifty states completely open to interstate bank branching by 2006. From Figure A.2 and Figure A.3 which respectively display the time series of average *SFR* and average *RECPI* of states grouped by quartile, we observe that it is necessary to account for the difference across states in our model formulation. This finding thus supports the inclusion of state fixed effects (FE) in the regression. Figure A.3 presents the time series of the average *SFR* for states grouped by IBBEA implementation year. Two notable elements can be highlighted from it. First, there appears to be a visual difference in slopes according to the year during which states have implemented the Riegle-Neal Act. This preliminary evidence indicates that the timing of implementation might influence the effect of the deregulation on *SFR*. Secondly, we observe that all states seem to be affected by year-specific shocks, as evidenced by a decline in *SFR* across all groups during the year 2000. This last finding supports the inclusion of year fixed effects in our analysis.

2.2 Model

Given that some states have gradually opened their borders to interstate branching while others have maintained strict restrictions, the interstate branching deregulation is a perfect natural experiment to analyze banking competition using a panel dataset with a difference-in-differences (DiD) approach. Before looking into the presentation of the regression, a brief introduction to panel data analysis is necessary to understand our methodology. The following theory on panel data is derived from Baltagi (2008). In our case, panel data refers to the pooling of observations on a cross-section of states over several years. The panel includes 850 observations ($N \times T$) from 50 states ($N = 50$) on a 17-year period ($T = 17$). Since

all variables are observed for all states and years, the panel is balanced. Please refer to Table A.3 for a sample of the panel dataset¹³.

2.2.1 Basic panel data model

When working with panel data, several settings can be analyzed depending on the format of the panels, the nature of the variables and the context of the observations. The basic formulation of a panel data model can be written as :

$$y_{it} = \alpha + X'_{it}\beta + u_{it}, \quad \forall i = 1, \dots, N ; t = 1, \dots, T \quad (1.1)$$

With i denoting individuals (e.g., states) and t denoting the time (e.g., year). The cross-section dimension is specified by the i subscript while t refers to the time-series dimension of the observations. y_{it} is the dependent variable, α is a scalar, β is of size $K \times 1$ and X'_{it} is the observation of state i in year t of the K independent variable.

The error component u_{it} can take two forms, either as a

$$\text{- one-way error component model:} \quad u_{it} = \mu_i + v_{it}, \quad (1.2)$$

or as a

$$\text{- two-ways error component model:} \quad u_{it} = \mu_i + \lambda_t + v_{it}, \quad (1.3)$$

where μ_i refers to the unobservable individual specific effect, which is time-invariant. It embraces any state effect that is not included in the regressors. λ_t is the unobservable year effect. v_{it} denotes the remainder disturbance not captured by the regression. The distinction between the two models lies in the omission of the time-specific effect, which is individual-invariant.

¹³ Panel data can either take the form of micro panels with a wide collection of individuals (N) and a short time period (T) or the form of macro panels with a smaller set of individuals an a longer time experiment (Baltagi, 2008).

2.2.2 Type of effects

In panel data analysis, the choice between fixed effects and random effects models depends on the specific research context. A fixed effects model is relevant when the analysis is restricted to a specific group of N individuals and the inference of the estimates (i.e. the relation that we draw between the regressors and the dependent variable) is limited to the behavior of these individuals. Simply put, fixed effects models are models that allow the regression coefficients to vary across individuals or over time (Balestra & Krishnakumar, 2008). On the other hand, a random effects model is appropriate when trying to use the inference based on N individuals (chosen randomly) on a larger group containing these individuals (Baltagi, 2008).

In our study, two elements indicate that a model with fixed effects is more pertinent. Firstly, based the theory of both effects described in Baltagi (2008) and Arellano (2003), the choice is self-explanatory. We are considering fifty states and we do not intend to derive our conclusions on other countries or on a wider population. Secondly, we rely on the literature that investigates the impact of the interstate branching deregulation (e.g., Burks, Cuny & Gerakos, 2018; Cornaggia et al., 2015; Favara & Imbs, 2015) and observe the constant presence of two-ways fixed effects in the regressions.

Particular attention must also be given to the implementation of the fixed-effects specification. Since we allow regression coefficients to vary across individuals or time, we can distinguish two types of coefficients: the intercept and the slope parameters (Balestra & Krishnakumar, 2008). When intercept coefficients are modelled, the regression is called a *covariance* model or dummy variable model. Only this type of model will be discussed throughout this study. There are two estimators that compute this model, the *within* estimator or the *Least Squares Dummy Variable* estimator (*LSDV*). The *within* estimator (Equation 1.4) subtracts individual means from the observations, while the *LSDV* estimator (Equation 1.5) is an OLS regression of y on x and a set of N dummy variables for each individual (Arellano, 2003). The first estimator considers the fixed effects in the error terms, while the second incorporates these effects into the dummy variables.

$$y_{it} - \bar{y}_{i.} = \beta(x_{it} - \bar{x}_{i.}) + (v_{it} - \bar{v}_{i.}) \quad \forall i = 1, \dots, N; t = 1, \dots, T \quad (1.4)$$

$$y_{it} = \alpha + X'_{it}\beta + \gamma_2 D2_i + \dots + \gamma_n Dn_i + v_{it} \quad \forall i = 1, \dots, N; t = 1, \dots, T \quad (1.5)$$

where $D2_i, \dots, Dn_i$ are dummy variables for each state i , y_{it} is the dependent variable, $\bar{y}_{i.}$ is the average of the focal variable over the time dimension¹⁴.

These two methods give the same coefficient estimates since they are numerically similar. However, both methods report different R^2 since the *LSDV* naturally incorporates the estimates of all fixed effects in its regression, while the within estimation does not. The R^2 of both methods is given for all regressions.

2.2.3 Panel Diagnostic Tests

To ensure the consistency of the regression, several tests must be performed when using panel data models. We first check if the inclusion of fixed effects effectively brings valuable information into our model. We also test for the presence of serial correlation and cross-sectional dependence in the panel dataset. Finally, to avoid spurious regressions, we test the existence of unit roots in the different time series of the observations.

2.2.3.1 Test for individual and time fixed effects

We conduct a Lagrange multiplier test developed by King and Wu (1997) to examine individual and time fixed effects. This test involves a 'locally mean most powerful (LMMP) one-sided test' (Baltagi, 2008) that coincides with Honda's (1985) test. It compares a pooled model with a fixed effects model¹⁵. The null hypothesis assumes that all fixed effects estimates are not significant, while the

¹⁴ Note that the within estimator cannot estimate the effect of time-invariant and individual-invariant variables because of the demeaning transformation. In other words, the within estimator does not provide an intercept estimate.

¹⁵ A pooled model refers to a model where observations are analyzed as a classic dataset without considering differences across cross sections (i.e., states) and time periods.

alternative hypothesis suggests the opposite. We report the results of the tests for both startup statistics in Table A.4. For *SFR*, we can reject the null hypothesis for all fixed effects. This indicates that both states and year dummy variables have significant fixed effects on the dependent variables. Regarding *RECPI*, we find that time FE do not bring significant effects. We explain the difference between *SFR* and *RECPI* by highlighting the nature of the *RECPI* variable. We argue that *RECPI* already captures the exogenous shocks present in the macroeconomic environments by multiplying it by *EQI*. Based on these results, we include both state and year fixed effects in all regressions and also provide results state FE as a matter of comparison.

2.2.3.2 Test for serial correlation

Serial correlation is usually not expected in panels with less than 20 years. However, if it occurs, it makes coefficient estimates inefficient. To ensure accuracy, the Wooldridge (2002) test for serial correlation in panel models is conducted on the Equation 2.1. The results in Table A.5 show that the null hypothesis is rejected with p-values smaller than five percent, indicating the presence of serial correlation among the residuals. This finding implies that we must account for serial correlation when computing the robust standard errors.

2.2.3.3 Test for cross-sectional dependence

Since individuals are represented by states, and states can have an influence on their neighboring states, it is necessary to examine the issue of cross-sectional or spatial dependence in the context of this study. While most papers currently report clustered robust standard error estimates, the literature sometimes ignores the presence of cross-sectional dependence, rendering consistent coefficient estimates but biased standard error estimates (Hoechle, 2007). To determine whether the residuals from the baseline regression with fixed effects are spatially independent, we conduct the Pesaran (2021) CD test. The null hypothesis suggests that the residuals are not correlated across cross-sectional units. Subsequently, the alternative hypothesis presumes the existence of spatial dependence. Statistics from Table A.5 indicate that there is indeed spatial dependence. As a result, we will report both clustered standard error estimates and Driscoll-Kraay (1998) standard

errors in all regressions. Driscoll-Kraay (1998) proposes a consistent estimation of standard errors for medium and large-scale panels that are cross-sectionally correlated. Hoechle (2007) provide a detailed discussion of the benefits of this estimation.

2.2.3.4 Panel unit root tests

It is also essential to check for nonstationary trends in time series when dealing with a relatively large number of states and a large time period (Baltagi, 2008). To do this, we performed the Im, Pesaran, and Shin (IPS) test to determine whether each individual time series (i.e., state time series) contains a unit root¹⁶. The alternative hypothesis is that the focal time series is stationary. Although results from Table A.6 present rejections of the null hypothesis for all variables, the interpretation is not straightforward. In our case, the best conclusion we can draw is that a significant fraction of the cross-sectional units are stationary, as emphasized by Breitung and Pesaran (2008). This implies that some panels may still contain a unit root, as shown by the results of the Hadri test (2000). The null hypothesis of Hadri is that every time series is stationary, while the alternative hypothesis suggests that at least one time series is non-stationary. For the rest of this work, we will assume that the potential non-stationary trends should not affect the precision of our estimates since the aggregation of our individual effects will remove the potential spurious effect of unit roots within our panel dataset.

¹⁶ The Im, Pesaran and Shin (2003) test is efficient for large panels with cross-sectional dependence. See Baltagi (2008) for a comprehensive review of unit root tests related to nonstationary panels.

2.2.4 Baseline Regression

Given the theoretical concepts presented above, we formulate a simple regression that computes the average change in startup statistics after changes in IBBEA provisions at the state-year level. We derive our DiD estimates using Ordinary Least Squares (OLS). Our regression can be written as:

$$\ln Startups_{i,t} = \alpha + \beta_1 Index_{i,t} + \beta_2 IDUM_{i,t} + X'_{it} \delta + State_i + Year_t + v_{it} \quad (2.1)$$

where $\ln Startups_{i,t}$ refers to the natural logarithm of the *SFR* or *RECPI* variable in state i in year t . The variable $Index_{i,t}$ is the variable of interest observed in state i in year t . Constructed as a continuous policy variable, it ranges from zero to four the restrictiveness of states towards interstate branching. $IDUM_{i,t}$ is a dummy variable that takes zero before a state implemented the IBBEA and one after. X'_{it} represents the matrix of control variables for state i at time t . The controls are the natural logarithm of *GDP*, the natural logarithm of the *Population*, the *Unemployment rate*, the natural logarithm of the *Labor force participation rate (LFPR)* and the *Inter* dummy variable.

The vectors $State_i$ and $Year_t$ represent state and year fixed effects, respectively. The state fixed effects capture, for each state, the time-invariant effect on *SFR*. It controls for difference across states. For instance, the state of California may have a higher *SFR* value compared to other states due to a highly developed startup ecosystem in that region. Therefore, including a state fixed effect for California would account for this difference and it ensures that we can compare states between each other and extract the real effect of the explanatory variables.

The year fixed effects capture exogenous impacts on the startup activity that do not come from a change in interstate deregulation and that affect all the individuals of the dataset (i.e., all states). It controls for business cycle effects or federal shocks. For instance, the effect of the dot-com crisis in 2000 might be incorporated in the year fixed-effect as it has affected all states at the same time and is exogenous to changes in the IBBEA provisions.

Although the Startup Formation Rate is a count variable, we use OLS estimation rather than a negative binomial or Poisson model for two reasons. First, recent papers that have worked with the SCP measures have also used a classic OLS estimation to analyze the startup statistics (Fazio, Guzman & Stern, 2020; Mostaghel, Oghazi & Patel, 2023). Second, Table A.7 shows that the mean of the SFR observations is greater than the standard deviation for all states, violating the assumptions of low conditional mean and the mean-variance equality from the theory of Poisson distribution. Based on these reasons, OLS estimation appears to be satisfactory (Cameron & Trivedi, 2014).

3 Results

3.1 Baseline analysis

Table 2 Panel A presents the results of the baseline regression including the *Startup Formation Rate* as the dependent variable. Our preferred OLS estimation with clustered standard errors, represented by model (4), reveals some interesting findings using the DiD approach. We observe that the coefficient estimate β_1 is not significantly different than zero at the ten percent level. This suggests that the changes in provisions do not have an impact on business entry. Furthermore, the effect of the passage of IBBEA, captured by *IDUM*, is also not significant at the ten percent level. It indicates that we cannot assert any specific relationship between interstate branching deregulation and the number of new business registrants at the state-year level. Regarding the control variables, we find that the size of the state population has a significant positive impact on the *SFR* at the one percent level. This makes sense because the SCP records a new startup when an individual creates a business registration. Therefore, it is very likely that the number of startups is increasing with the size of the population in a specific region.

To check the robustness of the estimates and the importance of fixed effects, we begin with a model without fixed effects and then gradually add state and year fixed effects in models (1) to (4). The variation of β_1 , while not significant, indicates that the inclusion of fixed effects in the model specification largely influences the estimated effect of changes in provisions. This conclusion is also supported by the variation of R^2 .

Table 2: Baseline regression results.

This table presents the results of OLS regression for the baseline analysis. In Panel A, the estimates for Equation 2.1a are reported. The dependent variable in columns 1-5 is the natural logarithm of the number of new business registrations at the state-year level. In Panel B, the dependent variable in columns 1-5 is the natural logarithm of the *RECPI* measure. The *Index* is the variable of interest. It ranges from zero to four the restrictiveness of states towards interstate bank branching. The control variables are defined in Appendix A. The standard errors are clustered by state, by year and by state-year for columns 2, 3 and 4, respectively in Panel A and Panel B. All columns 5 report Driscoll-Kraay (1998) standard errors. The level of significance is indicated by ***, **, and * for the 1%, 5%, and 10% levels, respectively. The data is observed at the state-year level from 1990-2006.

Panel A: Results for SFR					
Dependent Variable: <i>ln Startup Formation Rate</i>					
Model:	(1)	(2)	(3)	(4)	(5)
Constant	-0.2083 (4.270)				
Index	0.0359 (0.0620)	-0.0429 (0.0346)	0.0373 (0.0629)	-0.0198 (0.0330)	-0.0198** (0.0070)
IDUM	0.4299*** (0.1192)	0.0173 (0.0564)	-0.0079 (0.2432)	-0.0472 (0.0535)	-0.0472** (0.0221)
<i>ln GDP</i>	0.6495*** (0.2224)	0.7088*** (0.0742)	0.3868 (0.3959)	0.2400 (0.6118)	0.2400* (0.1182)
<i>ln Population</i>	0.4970** (0.2302)	3.135*** (0.5189)	0.7563* (0.4052)	2.654*** (0.7543)	2.654*** (0.2901)
<i>Unemployment Rate</i>	-0.0146 (0.0137)	-0.0030 (0.0163)	-0.0093 (0.0129)	-0.0259 (0.0277)	-0.0259* (0.0128)
<i>ln Labour Force Participation Rate</i>	-1.337 (1.068)	-1.052 (1.038)	-0.7301 (1.123)	0.0724 (1.256)	0.0724 (0.9117)
<i>Interstate Banking Dummy</i>	-0.2950 (0.2908)	0.1827*** (0.0573)	-0.4218 (0.2909)	0.0355 (0.0794)	0.0355* (0.0201)
State		Yes		Yes	Yes
Year			Yes	Yes	Yes
Observations	850	850	850	850	850
R ²	0.79168	0.96711	0.80974	0.97322	0.97322
Within R ²	-	0.77543	0.78572	0.22009	0.22009

Panel B : Results for RECPI					
Dependent Variable : <i>ln Regional Entrepreneurship Cohort Potential Index</i>					
Model:	(1)	(2)	(3)	(4)	(5)
Constant	-26.82*** (7.249)				
Index	-0.0504 (0.0717)	-0.0222 (0.0255)	-0.0466 (0.0759)	-0.0204 (0.0254)	-0.0204** (0.0072)
IDUM	-0.1551 (0.2081)	0.0332 (0.0469)	-0.2072 (0.2916)	-0.0664 (0.0503)	-0.0664** (0.0240)

(Continued)

Table 2: Baseline regression results (Continued)

<i>In GDP</i>	0.8083*	0.1520**	0.9568	0.0555	0.0555
	(0.4331)	(0.0748)	(0.6032)	(0.5437)	(0.2172)
<i>In Population</i>	0.6736	3.201***	0.5276	3.001***	3.001***
	(0.4837)	(0.4563)	(0.6456)	(0.6969)	(0.2897)
<i>Unemployment Rate</i>	-0.0247*	-0.0411***	-0.0276*	-0.0337	-0.0337**
	(0.0144)	(0.0132)	(0.0142)	(0.0219)	(0.0131)
<i>In Labour Force Participation Rate</i>	2.200	0.3455	1.935	-0.0893	-0.0893
	(1.564)	(0.9099)	(1.704)	(1.220)	(0.9690)
<i>Interstate Banking Dummy</i>	-0.2195	0.0438	-0.3616	-0.0312	-0.0312
	(0.2414)	(0.1097)	(0.2666)	(0.1220)	(0.0387)
State		Yes		Yes	Yes
Year			Yes	Yes	Yes
Observations	850	850	850	850	850
R ²	0.80205	0.98450	0.80507	0.98522	0.98522
Within R ²	-	0.68008	0.79950	0.28570	0.28570

Based on the results of the diagnostic tests, we know that spatial correlation is present in the regression, which means that standard error estimates might be biased when clustered by year and by state. In model (5), we report the same regression as in model (4) but we instead compute Driscoll-Kraay (DK) standard errors. Using this method, the estimates become significantly different from zero at the ten percent level for the *Index*, the *IDUM*, the *Population*, the *Unemployment Rate* and the *Interstate Banking Dummy* variables. The coefficient estimate of β_1 indicates that states completely open to interstate branching (i.e., *Index* level zero) generated an average of 7.9% more startups than the most restrictive states (i.e., *Index* level four), as calculated by multiplying the estimate (-0.0198) by four. On the other hand, the IBBEA implementation, captured by *IDUM*, is associated with a drop of 4.72% in new business entry. In other words, while the staggered relaxation of branching restrictions (1995-2005) has proven to boost startup activity, the initial phase of the IBBEA (1994-1997) is associated with less business entry in average.

In addition to the population size being significant, model (5) reports that the *Unemployment Rate* has a negative relationship with the *SFR* variable. Specifically, adding 1% to the unemployment rate is associated with an average 2.59% drop in the number of new business registrations. The economic interpretation would

suggest that we associate states with high business registration records with a low unemployment rate.

In Figure A.5, we construct a graph of the state fixed effects from model (5) and we find that three out of the five outliers reported in Figure A.6 have divergent state fixed effects¹⁷. This means that the intrinsic difference across states is well captured by the state dummies. We also examine the year fixed effects and find an almost linear increase of the year fixed effects with time. We argue that the year dummies capture an overall increase of the outcome variables over time. These two findings support our decision to include both state and year fixed effects in our model.

Moving on the effects of banking competition on the quality-adjusted quantity of startups, given by Table 2 Panel B. Looking at model (4) as the baseline regression with two-ways FE and clustered standard errors, the coefficient estimate of the branching index (β_1) does not show a significant relationship between changes in branching legislations and the startup potential. This finding is consistent with the results in Panel A. In the model including state FE only, we find no effect of the *Index* variable but the estimates of *GDP* and the *Unemployment rate* become statistically significant at the five and one percent level, respectively. To be more specific, the startup potential within a state is positively linked to an increase in *GDP* and negatively associated with a rise in the unemployment rate. This discovery is line with expectations.

The results from model (5) present Driscoll-Kraay (1998) standard errors and the significance of the estimates is consequently affected. As in Panel A, the estimates of *Index* and *IDUM* become significant at the five percent level. The magnitude of both β_1 and β_2 coefficients is slightly larger than in Panel A. Therefore, we argue that the effect of interstate branching deregulation has not only an impact on the quantity but also on the quality of startups. Since the *EQI*, used to construct the *RECPI* variable, is created from twelve business characteristics, it is difficult to

¹⁷ We refer here to outliers as states that outperform in the average level of *SFR*, the average level of *RECPI* or both. The outperformance is only based on the visual difference between these states and the rest of the observations on both axes of Figure A.5.

determine how exactly banking competition can affect the probability of success for new businesses. Nonetheless, we can still note that from the twelve dummy, two of them involve the number of patents issued and the number of citations. We also know that the literature has closely investigated these variables as proxies of the level of innovation. We therefore argue that the branching deregulation might have fostered innovation, which in turn has boosted the potential of startups within a geographic area.

Overall, our results in Table 2 with clustered standard errors do not provide evidence supporting the idea that banking competition has a significant impact on either the quantity or the potential of startups. However, when computing standard errors that account for spatial dependence, we find a positive effect from the relaxation of provisions (1995-2005) but a negative effect from the IBBEA initial deregulation phase (1994-1997). Based on these nuanced findings, we identify three elements that could influence the precision of the estimates.

3.1.1 Loss in degrees of freedom due to fixed effects

The two-way error component regression model presented in Equation 2.1 estimates $\{(N-1)+(T-1)\}$ dummy variables in the regression. When N and T are too large, this causes an important loss of degrees of freedom (Baltagi, 2008, p. 35). This loss can make it more difficult for the OLS model to provide statistically significant estimates. To address this issue, fixed effects are omitted in models (1) to (3) of Table 1. We notice that the range of the estimates varies greatly with the inclusion of fixed effects. Additionally, from the tests in Section 2.3.3.1, we know that valuable information is included in the state dummies. Therefore, it is only relevant to compare models with state and state-year fixed effects. For both models, we observe that the coefficient estimates lie in the same range and that their interpretation is similar. As a result, we argue that the loss of degrees of freedom due to the inclusion of too many fixed effects does not prevent us from finding better estimates for the branching index.

3.1.2 Choice of the standard error computation

Table 2 highlights that the choice between different standard error computations is determinant for interpreting the results of this study. Although clustered standard errors can handle heteroskedasticity and serial correlation, an alternative method of standard error estimation is necessary when residuals are correlated within and between groups. Several approaches can address this issue, but since the T dimension is smaller than the N dimension, we discarded the Parks–Kmenta method (Kmenta, 1971; Parks, 1967). Moreover, the T/N ratio is not small enough to consider the Beck and Katz’s (1995) panel-correlated standard errors (PCSE) method (Hoechle, 2007). Therefore, we argue that the Driscoll-Kraay (DK) standard errors are the most suitable method for the context of this study. We still report clustered standard errors for comparison purposes.

3.1.3 Analysis without outliers

The inclusion of outliers in the sample is another factor that can deteriorate the accuracy of our estimates. The OLS estimation is dependent on each observation and the inclusion of states with excessively different observations can significantly affect the precision of our estimates. To account for this, we perform a robustness analysis by removing the states of California, Florida, New York, Texas, and Massachusetts from the panel dataset, which reduces the number of observations from 850 to 765. Based on the visual evidence from Figure A.6, we selected these states as they are outliers of the full sample of observations. The results in Table A.8 indicate that removing those states does not alter the significance of the estimates for both the clustered standard errors regressions (columns 1 to 4) and DK standard errors regressions (columns 5 to 8). We conclude that including upper tail observations does not affect the consistency of our estimations in this case.

3.2 Additional analyses

3.2.1 Dynamic analysis

Up to this point, we have assumed in the construction of the *Index* that the IBBEA provisions were implemented at the beginning of the year and that their effects would be noticeable within the year. However, given that regulations generally have long-lasting impact and that the actual implementation dates were spread within the implementation period, the removal of restrictions could have affected observations over multiple years. For instance, Nevada implemented its restrictions on September 29, 1995. We will thus not only examine the deregulation impact in the year 1995 but also anticipate finding an effect in 1996. To address this issue, we follow Cornaggia et al. (2015) and report the results of an extended model in Table A.8. The new model formulation can be written as follows:

$$\ln Startups_{i,t+1 \text{ to } t+3} = \alpha + \beta_1 Index_{i,t} + \beta_2 IDUM_{i,t} + X'_{it} \delta + State_i + Year_t + v_{it}, \quad (2.2)$$

where $\ln Startups_{i,t+1 \text{ to } t+3}$ represents the natural logarithm of the total *SFR* or the total *RECPI* of state i in the three years following year t . The rest of the regression is similar to the baseline regression.

We first examine columns (1) to (4) which present estimates with clustered standard errors. We observe that the effect of changes in IBBEA provisions is negative and significantly different from zero at the ten percent level for *SFR* with state fixed effects only. Comparing this estimate (-0.0481*) to the one in the baseline regression (-0.0429), we conclude that taking the effect over the next three years allows us to find a significant impact of the branching provisions on business entry. In other words, although the data could not confirm that the relaxation of branching restrictions had an immediate positive effect on business entries, we find that the lagged effect over three years is significant and positive. For the dependent variable *RECPI*, we find that the estimate of β_1 is significant and negative for both state and state-year fixed effects models. These results support the findings of the baseline regression. The difference of magnitude of the estimates leads to the same

interpretation as for SFR. With clustered standard errors, the results show that, while banking competition does not have an immediate impact on the potential of startups, we notice a significant positive effect within three years. Following this finding, we argue that, due to the inertia of the financial sector in the United States, the effect of banking competition on the potential of startups is not immediately visible.

Regarding the coefficient estimate of *IDUM* for both outcome variables, we do not find a significant effect from the initial phase of the IBBEA (1995-1997) in the next three years after deregulation. At the same time, the estimates of the controls do not show any significant variation compared to the baseline results. However, when taking state fixed effects into account, we note that the *GDP* becomes significant and positive for business entry, while the unemployment rate is significant but negatively associated with the startup potential.

When examining the results with standard errors robust to cross-sectional dependence, the coefficient estimates naturally remain the same, but the standard errors vary significantly. We find that an increase in the *Index* is significantly associated with a decrease in both the total number of business registrations and the total startup potential in the three years following changes in provisions. The interpretation of the *IDUM* estimate is the same as in columns (1) to (4), except that we find a significant negative impact of the IBBEA deregulation on the number of business entries. The estimates of the controls and their interpretation remain the same as with clustered standard errors.

Table 3: Dynamic regression.

This table presents the results of OLS regression for Equation 2.2. The dependent variable in columns 1-2 and 5-6 is the natural logarithm of the sum of new business registration generated within a state in the following three years, while the dependent variable in columns 3-4 and 7-8 is the natural logarithm of the startup potential generated within a state in the following three years. The *Index* is the variable of interest. It ranges from zero to four the restrictiveness of states towards interstate bank branching. The *IDUM* is a dummy variable that takes one after the passage of the IBBEA by each state and zero otherwise. The control variables are defined in Appendix A. Columns (1) to (4) report standard errors clustered by state or by state and year simultaneously. Columns (5) to (8) report Driscoll-Kraay (1998) standard errors that are robust to heteroskedasticity, cross-sectional and time dependence. The level of significance is indicated by ***, **, and * for the 1%, 5%, and 10% levels, respectively. The data is observed at the state-year level from 1992-2006.

Dependent Variable:		$\ln SFR_{i,t+1 \text{ to } t+3}$		$\ln RECP_{i,t+1 \text{ to } t+3}$		$\ln SFR_{i,t+1 \text{ to } t+3}$		$\ln RECP_{i,t+1 \text{ to } t+3}$	
Model:		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Index		-0.0481* (0.0282)	-0.0244 (0.0252)	-0.0303** (0.0136)	-0.0287* (0.0146)	-0.0481*** (0.0109)	-0.0244*** (0.0079)	-0.0303*** (0.0096)	-0.0287** (0.0098)
IDUM		0.0472 (0.0472)	-0.0312 (0.0463)	0.0270 (0.0315)	-0.0279 (0.0374)	0.0472 (0.0536)	-0.0312* (0.0149)	0.0270 (0.0230)	-0.0279 (0.0211)
<i>ln GDP</i>		0.5608*** (0.0612)	-0.1786 (0.4330)	-0.0678 (0.0470)	0.0171 (0.2447)	0.5608*** (0.1698)	-0.1786 (0.1485)	-0.0678 (0.0386)	0.0171 (0.0960)
<i>ln Population</i>		2.839*** (0.4335)	2.465*** (0.6291)	3.095*** (0.3162)	2.752*** (0.3791)	2.839*** (0.5261)	2.465*** (0.3454)	3.095*** (0.2029)	2.752*** (0.1717)
<i>Unemployment Rate</i>		0.0082 (0.0136)	-0.0340* (0.0191)	-0.0178** (0.0083)	-0.0159 (0.0145)	0.0082 (0.0204)	-0.0340*** (0.0077)	-0.0178* (0.0096)	-0.0159 (0.0099)
<i>ln Labour Force Participation Rate</i>		-1.342 (0.8256)	-0.4255 (0.7814)	-0.3902 (0.6738)	-0.8644 (0.8073)	-1.342* (0.7157)	-0.4255 (0.5409)	-0.3902 (0.3007)	-0.8644* (0.4151)
<i>Interstate Banking Dummy</i>		0.1814*** (0.0403)	-0.0080 (0.0311)	0.0608** (0.0264)	-0.0015 (0.0298)	0.1814*** (0.0383)	-0.0080 (0.0263)	0.0608** (0.0224)	-0.0015 (0.0268)
State	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	700	700	700	700	700	700	700	700	700
R ²	0.97846	0.98455	0.99280	0.99338	0.97846	0.98455	0.99280	0.99338	0.97846
Within R ²	0.78093	0.19998	0.74308	0.43913	0.78093	0.19998	0.74308	0.43913	0.78093

3.2.2 Heterogenous analysis across state cohorts

From a different dynamic perspective, we made the assumption that the impact of interstate branching deregulation is homogenous across all states. Nevertheless, as highlighted by Chang (2017), this assumption may not hold for one important factor. The gradual implementation of the IBBEA has indeed led to an exponential increase of interstate expansion possibilities for states. In their respective first year of implementation, states from the 1995 cohort had less expansion options than states that deregulated in 1997¹⁸. As seen from the difference of slopes in Figure A.3, we reasonably assume that the timing of IBBEA implementation might be biased. To tackle this issue, we use a difference-in-difference-in-differences (DDD) estimation that should proxy the different effects across cohorts. The model can be written as:

$$\ln Startups_{i,t+1 \text{ to } t+3} = \alpha + \beta_1 Index_{i,t} + \beta_2 IDUM_{i,t} + \beta_3 (Index_{i,t} * ImplementYear_i) + \beta_4 (IDUM_{i,t} * ImplementYear_i) + X'_{it} \delta + State_i + Year_t + v_{it}, \quad (2.3)$$

where $\ln Startups_{i,t+1 \text{ to } t+3}$ is the natural logarithm of the total of *SFR* or the total of *RECPI* of state *i* in the three years following year *t*. *ImplementYear_i* is a categorical variable that classifies states according to the year during which they implemented the Riegle-Neal Act. To capture the individual effect of each cohort, we compute the interaction effect between the *ImplementYear* variable and *IDUM*¹⁹.

Table 3 displays the results using clustered standard errors in columns (1) to (4) and DK standard errors in columns (5) to (8). Interesting findings can be extracted from this table. In contrast to the previous regression, models (1) to (4) demonstrate that

¹⁸ In this context, we define cohorts as groups of states that passed the IBBEA in the same year (i.e., 1995, 1996 or 1997).

¹⁹ Notice that, in this two-ways FE model, adding a parameter *ImplementYear_i* alone is not possible since it is depending on the state dimension, and it would therefore be already integrated in the state fixed effects.

categorizing states does not provide additional information on the impact of changes in IBBEA provisions (1995-2005) on business formation and startup potential. The same applies to the effect of the IBBEA initial phase (1994-1997) across cohorts. Only the IBBEA implementation of states from the 1997 cohort appears to have a significant negative impact at the ten percent level on the total startup potential within three years. This implies an average decrease of 16.8% in startup potential from 1998 to 2000. The estimates of the controls in columns (1) to (4) do not provide additional information compared to previous regressions.

Looking at the estimates robust to cross-sectional dependence, two opposite conclusions can be drawn. First, the relaxation of branching legislations (1995-2005) has only a significant positive effect on both the total business entry and the total quality-adjusted quantity of entrepreneurship for states from the 1997 cohort. This result supports the insight of Chang (2017), who claims that states that deregulated in 1997 could benefit from a broader network of interstate expansion options than states from the 1995 cohort. The same network effect is found with the *IDUM* variable in columns (6) and (8). However, similar to the baseline results, the IBBEA initial phase (1994-1997) has a negative impact on both startup statistics. The same interpretation as for the *Index* can be applied here: if any effect is found from branching deregulation, then the state cohort that deregulated last should see the major effects.

On the other hand, we find that the changes in provisions are detrimental for states of the 1995 cohort. When considering state-year fixed effects, each removed branching restriction harm both the business entry and the startup potential in those states. We argue that the network formed by states that underwent deregulation in the first two years (i.e., 1995 and 1996) was insufficient to boost startup activity in those particular states.

In general, the findings suggest that the timing of the IBBEA implementation can provide valuable insights into how each state responds to interstate branching deregulation. The results demonstrate that states that implemented the policy later experienced more significant effects from the deregulation than those that deregulated earlier. Additionally, the study suggests that poor network effects may

have prevented some states from activating their startup environment during the beginning of the deregulation process. Despite this, the overall results suggest that the interstate branching network effect has been beneficial for both business entry and startup potential.

Table 3: DDD Regression.

This table presents the results of OLS regression for Equation 2.3. The dependent variable in columns 1-2 and 5-6 is the natural logarithm of the sum of new business registration generated within a state in the following three years, while the dependent variable in columns 3-4 and 7-8 is the natural logarithm of the startup potential generated within a state in the following three years. The *Index* is the variable of interest. It ranges from zero to four the restrictiveness of states towards interstate bank branching. The *IDUM* is a dummy variable that takes one after the passage of the IBBEA by each state and zero otherwise. The control variables are defined in Appendix A. Columns (1) to (4) report standard errors clustered by state or by state and year simultaneously. Columns (5) to (8) report Driscoll-Kraay (1998) standard errors that are robust to heteroskedasticity, cross-sectional and time dependence. The level of significance is indicated by ***, **, and * for the 1%, 5%, and 10% levels, respectively. The data is observed at the state-year level from 1992-2006.

Dependent Variable:		$\ln SFR_{i,t+1 to t+3}$		$\ln RECP I_{i,t+1 to t+3}$		$\ln SFR_{i,t+1 to t+3}$		$\ln RECP I_{i,t+1 to t+3}$	
Model:		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Index</i>		0.0171 (0.0319)	0.0301 (0.0274)	0.0186 (0.0314)	0.0256 (0.0300)	0.0171 (0.0161)	0.0301* (0.0152)	0.0186 (0.0126)	0.0256* (0.0136)
<i>Index</i> x <i>ImplementYear</i> = 1996		0.0037 (0.0463)	0.0440 (0.0515)	0.0064 (0.0459)	0.0084 (0.0444)	0.0037 (0.0366)	0.0440 (0.0305)	0.0064 (0.0159)	0.0084 (0.0180)
<i>Index</i> x <i>ImplementYear</i> = 1997		-0.0881 (0.0679)	-0.0650 (0.0670)	-0.0494 (0.0470)	-0.0515 (0.0477)	-0.0881*** (0.0241)	-0.0650** (0.0281)	-0.0494** (0.0195)	-0.0515** (0.0216)
<i>IDUM</i>		0.1461 (0.1012)	0.0491 (0.0805)	0.1371 (0.0985)	0.0473 (0.0858)	0.1461** (0.0679)	0.0491 (0.0320)	0.1371* (0.0783)	0.0473 (0.0362)
<i>IDUM</i> x <i>ImplementYear</i> = 1996		0.1508 (0.1447)	0.0966 (0.1451)	0.1718 (0.1462)	0.0626 (0.1353)	0.1508 (0.1424)	0.0966 (0.0865)	0.1718 (0.1034)	0.0626 (0.0652)
<i>IDUM</i> x <i>ImplementYear</i> = 1997		-0.0790 (0.0639)	-0.1473 (0.0892)	-0.0628 (0.0570)	-0.1680* (0.0860)	-0.0790 (0.0765)	-0.1473** (0.0609)	-0.0628 (0.0460)	-0.1680*** (0.0554)
<i>ln GDP</i>		0.7231*** (0.0740)	0.3239 (0.5420)	0.1758** (0.0770)	0.1334 (0.4852)	0.7231*** (0.2311)	0.3239** (0.1387)	0.1758** (0.0684)	0.1334 (0.2075)
<i>ln Population</i>		3.016*** (0.5275)	2.464*** (0.7629)	3.067*** (0.4478)	2.814*** (0.6567)	3.016*** (0.5662)	2.464*** (0.2573)	3.067*** (0.2153)	2.814*** (0.2451)
<i>Unemployment Rate</i>		-0.0048 (0.0184)	-0.0270 (0.0251)	-0.0409*** (0.0142)	-0.0331 (0.0198)	-0.0048 (0.0180)	-0.0270** (0.0110)	-0.0409*** (0.0102)	-0.0331** (0.0115)

(Continued)

Table 3: DDD regression (Continued)

	(0.0184)	(0.0251)	(0.0142)	(0.0198)	(0.0180)	(0.0110)	(0.0102)	(0.0115)
<i>In Labour Force Participation Rate</i>	-1.269	-0.2964	0.2594	-0.2818	-1.269	-0.2964	0.2594	-0.2818
	-1.142	-1.357	(0.9051)	-1.281	-1.129	-1.118	(0.9492)	-1.116
<i>Interstate Banking Dummy</i>	0.0981	-0.0436	-0.0273	-0.1111	0.0981**	-0.0436	-0.0273	-0.1111**
	(0.0689)	(0.0687)	(0.1110)	(0.1031)	(0.0449)	(0.0300)	(0.0516)	(0.0443)
State	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year		Yes		Yes		Yes		Yes
Observations	850	850	850	850	850	850	850	850
R ²	0.96797	0.97402	0.98494	0.98568	0.96797	0.97402	0.98494	0.98568
Within R ²	0.78133	0.24335	0.68930	0.30767	0.78133	0.24335	0.68930	0.30767

3.2.3 Analysis across competitive environments

From the baseline regression, we found that the effect of the interstate branching deregulation on the entrepreneurial activity is nuanced. It depends on whether we observe the effect of the relaxation of branching legislations (1995-2005) or the effect of the IBBEA implementation itself (1995-1997). To further check the robustness of the baseline results, we add the *HHI* variable and make it interact with the *Index* variable. The goal is to understand how the competitive environment of each state can have an impact on the effect of the branching deregulation. We categorized so far states according to their IBBEA implementation timing. Now, we distinguish each state according to its banking concentration. Formally, the Herfindahl-Hirschman Index (HHI) is used to capture the concentration of banks at the state-year level. Ranging from 0 to 10,000, a high index represents a state with fewer banks or with big banks that have a lot of market shares. Since branching deregulation is supposed to enhance banking competition (Johnson & Rice, 2008), we reasonably expect that states with a lower banking concentration (i.e., high competition) should be less affected by the branching deregulation, since the added competition should not change much from the status quo. On the other hand, states with a high *HHI* (i.e., low competition) should experience larger effects from the relaxation of branching restrictions.

The extended model described above can be written as:

$$\ln Startups_{i,t+1 \text{ to } t+3} = \alpha + \beta_1 Index_{i,t} + \beta_2 IDUM_{i,t} + \beta_3 (Index_{i,t} * HHI_{i,t}) + \beta_4 HHI_{i,t} + X'_{it} \delta + State_i + Year_t + v_{it}, \quad (2.4)$$

where $\ln Startups_{i,t+1 \text{ to } t+3}$ is the natural logarithm of the total of *SFR* or the total of *RECPI* of state *i* in the three years following year *t*. $HHI_{i,t}$ is a measure of the banking concentration of state *i* in year *t*. The other components of the regression remain the same as in Equation 2.1.

The results of the Equation 2.4 are presented in Table 4. Columns (1) and (3) report estimates with clustered standard errors, while models in columns (2) and (4) compute Driscoll-Kraay (1998) standard errors. Surprisingly, the statistical

significance of the estimates is almost the same across all models. The results also confirm our hypothesis that the effect of changes in provisions (1995-2005) is influenced by the level of banking concentration in each state, as the estimate of β_4 is significantly negative at the five percent level across all models. In other words, the branching deregulation benefits states with higher *HHI* for activating entrepreneurial ecosystems in the region. This discovery is in line with the conclusions of Zardkoohi and Fraser (1998), which suggest that only states with a low degree of competition have shown significant effects from banking deregulation. Additionally, the analysis of the controls gives similar conclusions than in the other regressions. They indicate a negative relationship between the outcome variables and the unemployment rate and a positive association between the startup measures and the population size.

These findings are informative for policymakers. Although we found an overall positive impact of the relaxation of branching provisions overall, the effects vary depending on the market conditions of each state. The results show that the impact of the deregulation was less pronounced in states with higher banking competitiveness compared to those with high banking concentration. This implies that startups in states with low competition in the pre-IBBEA period were disadvantaged compared to those in states with high banking competition. As a result, we argue that banking competition is crucial for enhancing both business entry and the long-term potential of startups in a specific area.

Table 4: Baseline regression with HHI.

This table presents the results of OLS regression analyses for the baseline model extended by the inclusion of the HHI variable. Models (1) and (3) consider the Startup Formation Rate as the dependent variable while models (2) and (4) take the RECPI variable as outcome variable. The *Index* is the variable of interest. It ranges from zero to four the restrictiveness of states towards interstate bank branching. The *HHI* variable ranges from 0 to 10000 the banking concentration at the state-year level. The control variables are defined in Appendix A, and all models are specified with both state and year fixed effects. The standard errors are clustered by state and by year in columns (1) to (2). The columns (3) to (4) report Driscoll-Kraay (1998) standard errors, which are robust to cross-sectional dependence. The level of significance is indicated by ***, **, and * for the 1%, 5%, and 10% levels, respectively. The data is observed at the state-year level from 1994 to 2006.

Dependent Variable:	<i>ln SFR</i>	<i>ln RECPI</i>	<i>ln SFR</i>	<i>ln RECPI</i>
Model:	(1)	(2)	(3)	(4)
<i>Index</i>	0.1487* (0.0687)	0.1889** (0.0781)	0.1487* (0.0687)	0.1889** (0.0781)
<i>ln HHI</i>	-0.0676 (0.0607)	-0.0704 (0.0518)	-0.0676 (0.0607)	-0.0704 (0.0518)
<i>IDUM</i>	-0.0333 (0.0300)	-0.0535 (0.0350)	-0.0333 (0.0300)	-0.0535 (0.0350)
<i>ln GDP</i>	0.4889 (0.2897)	0.7020* (0.3538)	0.4889 (0.2897)	0.7020* (0.3538)
<i>ln Population</i>	1.722*** (0.4658)	2.005*** (0.5425)	1.722*** (0.4658)	2.005*** (0.5425)
<i>Unemployment Rate</i>	-0.0403** (0.0161)	-0.0425*** (0.0139)	-0.0403** (0.0161)	-0.0425*** (0.0139)
<i>ln LFPR</i>	0.7632 (0.7880)	1.008 (0.9517)	0.7632 (0.7880)	1.008 (0.9517)
<i>Index x ln HHI</i>	-0.0247** (0.0109)	-0.0306** (0.0122)	-0.0247** (0.0109)	-0.0306** (0.0122)
State	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Observations	650	650	650	650
R ²	0.99074	0.99361	0.99074	0.99361
Within R ²	0.31727	0.38300	0.31727	0.38300

4 Conclusion

In this paper, we examine the impact of banking competition on startup activity in the United States using difference-in-differences estimation. In particular, we study the effects of the staggered interstate branching deregulation on the number of new business registrations and the expected number of successful startups. These two statistics provide relevant measures for assessing not only business entry but also the entrepreneurship potential of a certain geographical area. To answer our research question, we use panel data of 50 states over a time window covering the implementation of the Riegle-Neal Act. Both quantitative and qualitative tests are provided to support the choice of our model formulation and ensure its robustness.

Generally, banking deregulation is associated with positive outcomes for startups. The relaxation of branching restrictions is correlated with a decrease in the price of credit for small businesses (Rice & Strahan, 2010), while interstate banking deregulation enhances business formation and promotes innovation (Cornaggia et al., 2015; Chava et al., 2013; Kerr & Nanda, 2009). Overall, the literature supports the hypothesis that interstate branching deregulation has a positive impact on business entry and startup potential.

Using a classic DiD estimation as baseline model, we study this hypothesis and find different conclusions depending on the computation of the standard errors. Accounting for spatial dependence, the results show a positive link between the relaxation of interstate branching restrictions (1995-2005) and both startup statistics. However, the data still show that the passage of the IBBEA (1995-1997) has led to an immediate slight decrease in startup activity across states. To further examine these effects, we extend our baseline model to include two dynamic dimensions. We examine the total of the dependent variables for the three years following deregulation and find even more pronounced effects from interstate branching deregulation. Additionally, the negative impact found from the passage of the IBBEA (1995-1997) becomes nonsignificant when examining the total three years after deregulation. This indicates that, in the long run, banking competition contributes to a positive effect on startup activity.

We also differentiate states according to their implementation year and find that states that deregulated in 1997 have seen larger effects from the relaxation of branching restrictions than states that deregulated earlier. This difference is attributed to the network effect, which gives states wider interstate expansion possibilities at the end of the deregulation process than at the beginning.

Ultimately, we investigate the heterogeneous effects between states with different competitive environments. We find that highly concentrated states prior the IBBEA are associated with more startup activity boost than states that were already competitive. This last finding indicates that banking competition is an important aspect for states to foster business entry, but also for startups to be surrounded by the most attractive conditions for growth outcomes.

The findings of this study have significant implications for policymakers interested in promoting startup activity. In addition to providing evidence of nuanced positive impact of banking competition on the quantity and the quality-adjusted quantity of entrepreneurship, the results especially highlight that the context of the banking deregulation process and dynamic effects can greatly impact how states are affected by these policies.

4.1 Limitations and further research

When looking at the results, it is still important to highlight how competition has been measured. The *Index* variable, as retrieved from Rice and Strahan (2010), is a proxy of banking competition. The term ‘proxy’ is important here because the index does not reflect directly the level of competitiveness between banks. It is rather constructed on the assumption that removing interstate branching restrictions is associated with an increasing number of bank branches across states (Johnson & Rice, 2008). Another limit of the index is that it is considered as a continuous policy variable and therefore assumes that lifting any of the four IBBEA branching provisions has the same effect. This is naturally not true, as the nature of each provision is different, and their implication could be considered differently for banks when expanding across state borders.

Throughout this work, we voluntarily omit to mention the impact of the deregulation of Multi-Bank Holding Companies (MBHCs). As Amel (1993) reported, MBHCs had already benefited from intrastate and interstate deregulation before laws were passed to encourage the expansion of individual banks. Consequently, the effects captured by the branching index only represent a part of the impact of the global branching deregulation process that occurred in the U.S. economy.

Furthermore, the construction of the startup statistics retrieved from the SCP present two limits that must be considered when analyzing the results. Firstly, we used the academic dataset and not the policy dataset to construct the *EQI* and *RECPI* variables. The chosen dataset includes twelve dummy variables to create the *EQI*, while the policy index is more restrictive and omits non-formal variables (e.g. *eponymy*, *short name*, etc.). In the context of this thesis, the choice for the academic dataset is straightforward but we must note that further research with the policy dataset can provide additional insights or divergent results. Secondly, the interpretations presented in this work rely heavily on the fact that the *EQI* and the *RECPI* variables truly reflect the quality and the potential of a certain group of firms or a specific geographical area, respectively.

Another important caveat is that, in Equation 2.3, we differentiate states based on their IBBEA implementation year. However, this classification of states does not account for the increasing size of the interstate branching network from 1995 to 1997. While the current regression serves as a good proxy, future research could use a more consistent approach, such as the one proposed by Chang (2017). Similarly, we consider the implementation timings to be exogenous. However, Kroszner and Strahan (1999) report that the timing of interstate branching deregulation might be explained by interest group factors, which can introduce bias in our estimations. Although promoting startup activity is unlikely to be the major focus for politicians to deregulate, we note that if that were the case, the inference drawn from the model would be biased.

Moreover, this study does not fully discuss how exactly banking competition impacts entrepreneurial activity. Our primary objective has been to determine

whether there is a relationship between the branching index and the startup statistics. Further research is encouraged to investigate this link more thoroughly and develop more precise formulations to capture the quantitative relationship between the two factors. We also suggest incorporating the number of unit banks into the model. This variable, available in the Call Reports, can serve as a measure of the local banking size within a state and help explain the mechanism behind our baseline analysis. Further research on this mechanism can rely on the literature that studies the role of relationship lending in enabling small businesses to access external financing (Berger & Udell, 1995; 2002; Petersen & Rajan, 1994). It can also provide a more comprehensive understanding of the link between banking competition, relationship lending, and entrepreneurial activity.

The main takeaway of our analysis is the creation of a new link between well-established banking competition measures and recent startup statistics that considers both business entry and startup potential. From the data collected, we find a nuanced but positive effect of banking competition on these two measures of entrepreneurship ecosystems.

Appendices

Appendix A: Figures and Table

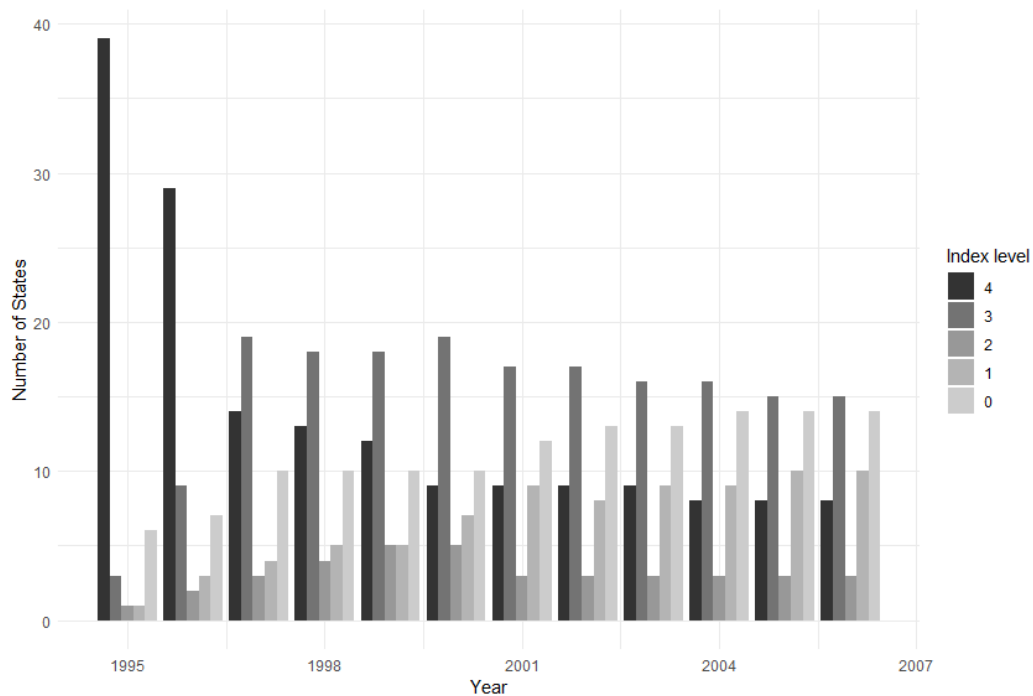


Figure A.1 – Evolution of the number of states clustered by *Index* level from 1995 to 2006.

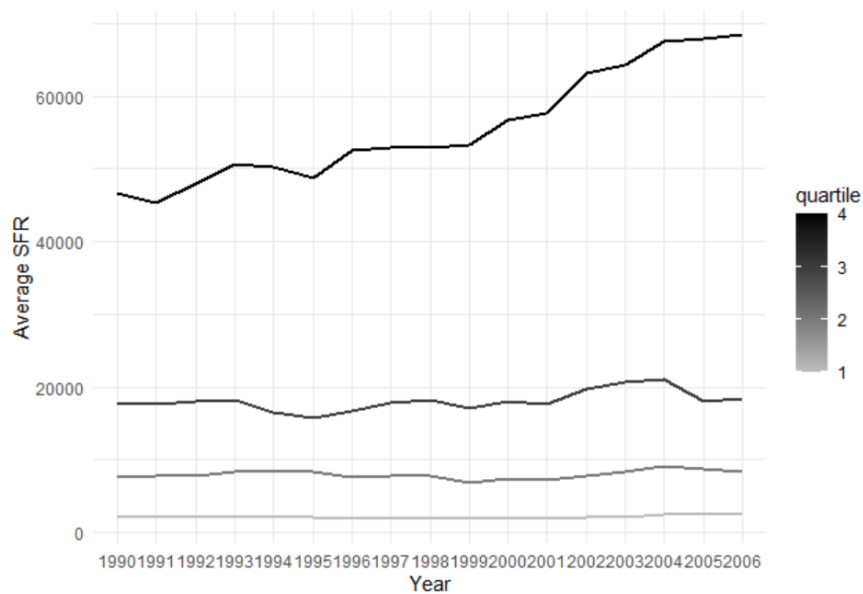


Figure A.2 - Time series of the average *SFR* grouped by quartile from 1995 to 2006.

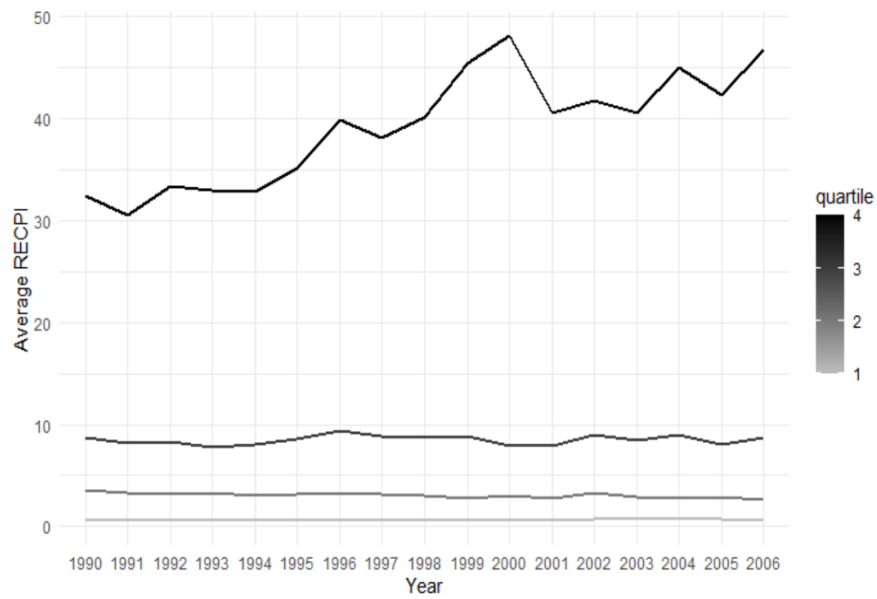


Figure A.3 - Time series of the average *RECPI* grouped by quartile from 1995 to 2006.

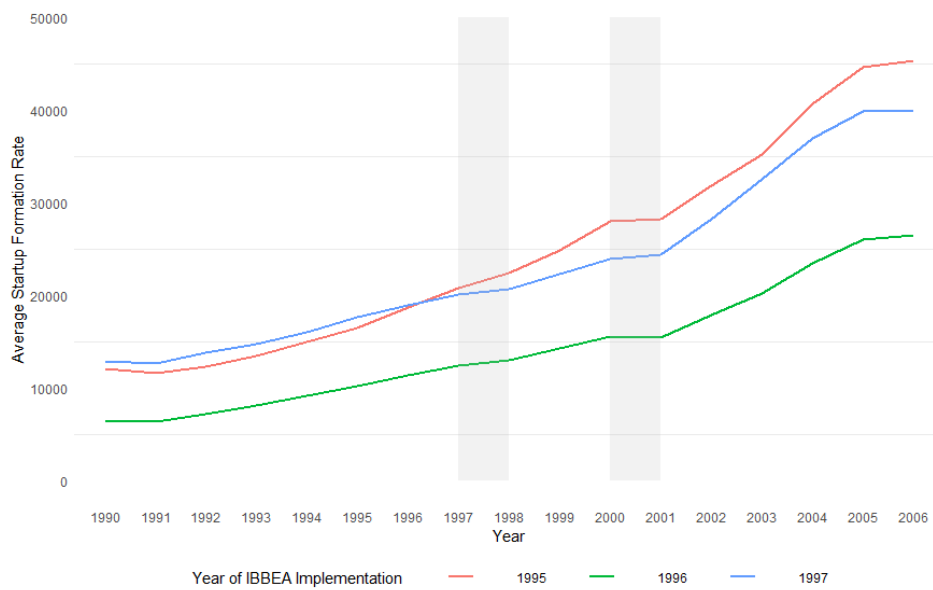


Figure A.4 - Time series of the average *SFR* by cohort (i.e. IBBEA implementation year) from 1995 to 2006.

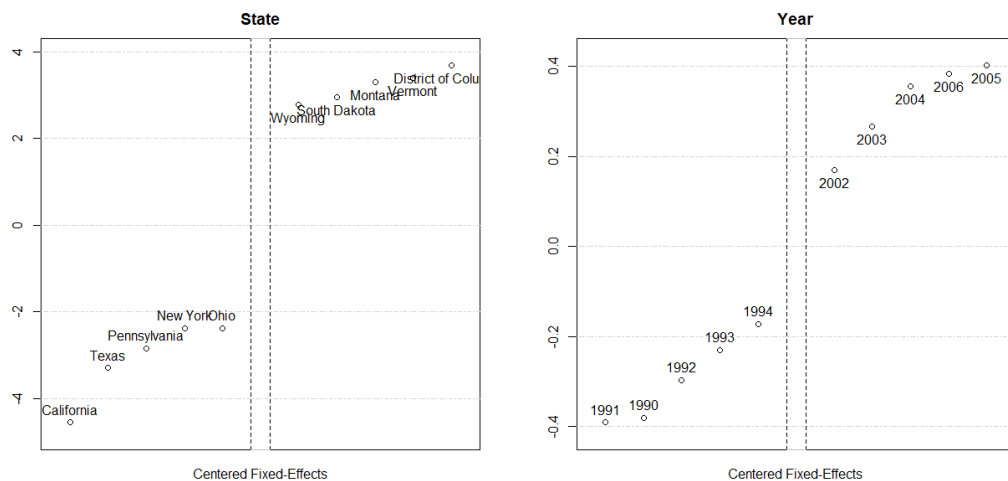


Figure A.5 - Distribution of the five highest and five lowest fixed effects for states (left) and years (right).

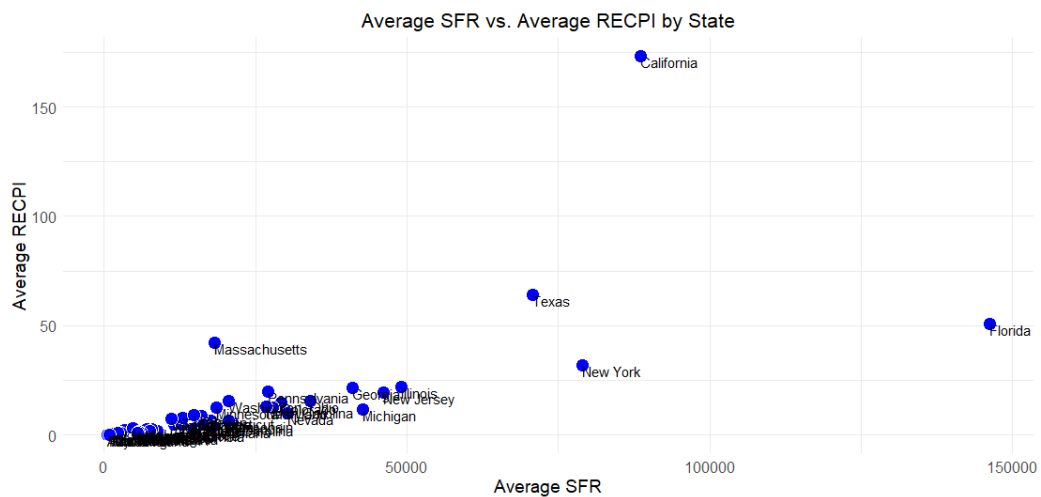


Figure A.6 - Scatter plot of the average *SFR* and average *RECPi* for each state from 1990 to 2006.

Table A.1: Interstate branching deregulation timing by state (Rice & Strahan, 2010)

This table displays the index of interstate branching restrictions and the effective date of interstate branching regulation changes. The index is based on the provisions settled by the IBBEA. These provisions include the minimum age of the institution for acquisition, the allowance of de novo interstate branching, the allowance of interstate branching by acquisition of a single branch or portions of an institution, and the statewide deposit cap on branch acquisitions. States that are most open to out-of-state entry have an index set to zero. The index increases by one when a state adds any of the four barriers mentioned earlier. For instance, the index increases by one if a state imposes a minimum age of 3 or more years on target institutions of interstate acquirers, if a state does not permit de novo interstate branching, if a state does not permit the acquisition of individual branches by an out-of-state bank, or if a state imposes a deposit cap less than 30%. The index, which ranges from zero to four is retrieved from Rice and Strahan (2010) but the original timing dates come from Johnson and Rice (2008).

STATE	BRANCHING RESTRICTIVNESS INDEX	EFFECTIVE DATE	STATE	BRANCHING RESTRICTIVNESS INDEX	EFFECTIVE DATE
ALABAMA	3	31-05-97	NEBRASKA	4	31-05-97
ALASKA	2	01-01-94	NEVADA	3	29-09-95
ARIZONA	2	31-08-01	NEW HAMPSHIRE	0	01-01-02
ARIZONA	3	01-09-96	NEW HAMPSHIRE	1	01-08-00
ARKANSAS	4	01-06-97	NEW HAMPSHIRE	4	01-06-97
CALIFORNIA	3	28-09-95	NEW JERSEY	1	17-04-96
COLORADO	4	01-06-97	NEW MEXICO	3	01-06-96
CONNECTICUT	1	27-06-95	NEW YORK	2	01-06-97
DELAWARE	3	29-09-95	NORTH CAROLINA	0	01-07-95
DISTRICT OF COLUMBIA	0	13-06-96	NORTH DAKOTA	1	01-08-03
FLORIDA	3	01-06-97	NORTH DAKOTA	3	31-05-97
GEORGIA	3	10-05-02	OHIO	0	21-05-97
GEORGIA	3	01-06-97	OKLAHOMA	1	17-05-00
HAWAII	0	01-01-01	OKLAHOMA	4	31-05-97
HAWAII	3	01-06-97	OREGON	3	01-07-97
IDAHO	3	29-09-95	PENNSYLVANIA	0	06-07-95
ILLINOIS	0	20-08-04	RHODE ISLAND	0	20-06-95
ILLINOIS	3	01-06-97	SOUTH CAROLINA	3	01-07-96
INDIANA	1	01-07-98	SOUTH DAKOTA	3	09-03-96
INDIANA	0	01-06-97	TENNESSEE	1	17-03-03
IOWA	4	04-04-96	TENNESSEE	1	01-07-01

(Continued)

Table A.1: Interstate branching deregulation timing by state (Continued)

KANSAS	4	29-09-95	TENNESSEE	2	01-05-98
KENTUCKY	3	22-03-04	TENNESSEE	3	01-06-97
KENTUCKY	3	17-03-00	TEXAS	2	01-09-99
KENTUCKY	4	01-06-97	TEXAS	4	28-08-95
LOUISIANA	3	01-06-97	UTAH	1	30-04-01
MAINE	0	01-01-97	UTAH	2	01-06-95
MARYLAND	0	29-09-95	VERMONT	0	01-01-01
MASSACHUSETTS	1	02-08-96	VERMONT	2	30-05-96
MICHIGAN	0	29-11-95	VIRGINIA	0	29-09-95
MINNESOTA	3	01-06-97	WASHINGTON	1	09-05-05
MISSISSIPPI	4	01-06-97	WASHINGTON	3	06-06-96
MISSOURI	4	29-09-95	WEST VIRGINIA	1	31-05-97
MONTANA	4	01-10-01	WISCONSIN	3	01-05-96
MONTANA	4	29-09-95			

Table A.2: Interstate banking deregulation timing by state (Kerr & Nanda, 2009)

STATE	INTERSTATE DEREGULATION	STATE	INTERSTATE DEREGULATION
ALABAMA	1987	MONTANA	1993
ALASKA	1982	NEBRASKA	1990
ARIZONA	1986	NEVADA	1985
ARKANSAS	1989	NEW HAMPSHIRE	1987
CALIFORNIA	1987	NEW JERSEY	1986
COLORADO	1988	NEW MEXICO	1989
CONNECTICUT	1983	NEW YORK	1982
DELAWARE	1988	NORTH CAROLINA	1985
DISTRICT OF COLUMBIA	1985	NORTH DAKOTA	1991
FLORIDA	1985	OHIO	1985
GEORGIA	1985	OKLAHOMA	1987
HAWAII	1997	OREGON	1986
IDAHO	1985	PENNSYLVANIA	1986
ILLINOIS	1986	RHODE ISLAND	1984
INDIANA	1986	SOUTH CAROLINA	1986
IOWA	1991	SOUTH DAKOTA	1988
KANSAS	1992	TENNESSEE	1985
KENTUCKY	1984	TEXAS	1987
LOUISIANA	1987	UTAH	1984
MAINE	1978	VERMONT	1988
MARYLAND	1985	VIRGINIA	1985
MASSACHUSETTS	1983	WASHINGTON	1987
MICHIGAN	1986	WEST VIRGINIA	1988
MINNESOTA	1986	WISCONSIN	1987
MISSISSIPPI	1988	WYOMING	1987
MISSOURI	1986		

Table A.3: Sample of the panel dataset.

OBS.	STATE	YEAR	SFR	EQI	RECPI	INDEX	GDP	POPULATION	UER	LFPR	INTER
1	AL	1990	5928	0.000560	3.321401	4	83766.4	4296800	6.825	62.158	1
2	AL	1991	6016	0.000536	3.230456	4	86115.5	4331102	7.325	61.766	1
3	AL	1992	6707	0.000501	3.366482	4	89813.9	4569805	7.525	62.266	1
.	.	.	.	.	.	.	.	.	.	.	.
849	WY	2005	1966	0.000140	0.275684	3	33484.0	514157	3.541	71.575	1
850	WY	2006	2024	0.000142	0.289066	3	37813.4	522667	3.008	71.916	1

Table A.4: Test results for the significance of fixed effects in Regression 2.1 (King & Wu, 1997).

KING AND WU (1997) TEST	STATE FE		TIME FE		TWO-WAYS FE	
	F STATISTIC	P-VALUE	F STATISTIC	P-VALUE	F STATISTIC	P-VALUE
LN(SFR)	86.32	2.2E-16	4.90	1.06E-9	81.03	2.2E-16
LN(RECPI)	190.44	2.2E-16	0.80	0.6855	148.19	2.2E-16

Table A.5: Results for serial correlation (Wooldridge, 2002) and cross-sectional correlation (Pesaran, 2021) tests on Regression 2.1.

EQUATION 2.1	WOOLDRIDGE (2002) TEST		PESARAN (2021) CD TEST	
	CHISQ STATISTIC	P-VALUE	F STATISTIC	P-VALUE
LN(SFR)	484.46	2.2E-16	51.18	2.2E-16
LN(RECPI)	372.53	2.2E-16	10.84	2.2E-16

Table A.6: Test results for Panel Unit Root Tests on the continuous time-series.

VARIABLE	IPS (2003) TEST	HADRI (2000) TEST
LN(SFR)	-6.22***	22.82***
LN(RECPI)	-2.02**	25.25***
LN(GDP)	-2.29**	19.06***
LN(POPULATION)	-4.34***	44.42***
UER	-4.60***	28.33***
LN(LBFR)	-3.16***	28.15***

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table A.7: Mean and standard deviation of the *Startup Formation Rate* for each state.

STATE	MEAN	S.D.	STATE	MEAN	S.D.
ALABAMA	13180.54	3834.04	MONTANA	7241	3289.4
ALASKA	666.85	319.21	NEBRASKA	2101.77	743.6
ARIZONA	18757.38	13926.59	NEVADA	38251.15	19129.09
ARKANSAS	9993.08	2667.31	NEW HAMPSHIRE	5432.23	2048.24
CALIFORNIA	102547	41890.64	NEW JERSEY	51490.38	13038.84
COLORADO	36008.54	16217.58	NEW MEXICO	6416	2305.35
CONNECTICUT	18653.15	5400.6	NEW YORK	86640.38	17218.26
DISTRICT OF COLUMBIA	3694.08	982.49	NORTH CAROLINA	31143.46	10154.02
FLORIDA	164967.69	71084.89	NORTH DAKOTA	1566.08	421.33
GEORGIA	47454.46	16803.28	OHIO	38360.92	9564.98
HAWAII	6028.85	1914.13	OKLAHOMA	14256.46	3696.73
IDAHO	6673	3251.65	OREGON	18261.46	6046.24
ILLINOIS	54017	13043.79	PENNSYLVANIA	30065.92	10302.55
INDIANA	19768.62	5023.02	RHODE ISLAND	1048.92	345.28
IOWA	8553	2181.77	SOUTH CAROLINA	20100.77	8287.33
KANSAS	8080.77	1986.52	SOUTH DAKOTA	3082.23	791.36
KENTUCKY	14413.23	3746.74	TENNESSEE	14215.54	2634.21
LOUISIANA	20242.46	6432.7	TEXAS	79861.54	26365.42
MAINE	1905.46	1424.19	UTAH	17655	6844.42
MARYLAND	30974.92	8591.16	VERMONT	2400.54	757.82
MASSACHUSETTS	19669.15	3555.42	VIRGINIA	13182.62	6970.38
MICHIGAN	48422.69	14407.28	WASHINGTON	24983	10633.28
MINNESOTA	21066.38	6739.32	WEST VIRGINIA	6072.54	1695.82
MISSISSIPPI	8630	2606.48	WISCONSIN	23795	8382.96
MISSOURI	24253.69	8428.54	WYOMING	1007.23	539.82

Table A.8: Baseline regression without outliers.

This table presents the results of OLS regression for the baseline model without the five outliers discussed in Section 3.1.3. The dependent variable in columns 1-2 and 5-6 is the natural logarithm of the SFR in year t , while the dependent variable in columns 3-4 and 7-8 is the natural logarithm of the startup potential generated within a state in year t . The *Index* is the variable of interest. It ranges from zero to four the restrictiveness of states towards interstate bank branching. The *IDUM* is a dummy variable that takes one after the passage of the IBBEA by each state and zero otherwise. The control variables are defined in Appendix A. Columns (1) to (4) report standard errors clustered by state or by both state and year. Columns (5) to (6) report Driscoll-Kraay standard errors that are robust to heteroskedasticity, cross-sectional and time dependence. The level of significance is indicated by ***, **, and * for the 1%, 5%, and 10% levels, respectively. The data is observed at the state-year level from 1990-2006.

Dependent Variable:		ln <i>SFR</i>			ln <i>RECP1</i>			ln <i>SFR</i>			ln <i>RECP1</i>		
Model:		(1)	(2)	(3)	(4)		(5)	(6)	(7)	(8)			
Index		-0.0422 (0.0351)	-0.0164 (0.0321)	-0.0210 (0.0260)	-0.0189 (0.0248)		-0.0422*** (0.0097)	-0.0164** (0.0069)	-0.0210*** (0.0059)	-0.0189** (0.0068)			
IDUM		0.0168 (0.0595)	-0.0365 (0.0501)	0.0263 (0.0501)	-0.0648 (0.0524)		0.0168 (0.0744)	-0.0365 (0.0235)	0.0263 (0.0511)	-0.0648** (0.0258)			
ln GDP		0.7294*** (0.0768)	0.2543 (0.5900)	0.1349* (0.0779)	0.1229 (0.5439)		0.7294*** (0.2306)	0.2543 (0.1578)	0.1349* (0.0669)	0.1229 (0.2140)			
ln Population		3.247*** (0.5399)	2.789*** (0.7303)	3.459*** (0.4333)	3.179*** (0.6737)		3.247*** (0.5703)	2.789*** (0.3560)	3.459*** (0.2412)	3.179*** (0.2949)			
Unemployment Rate		-0.0123 (0.0173)	-0.0428 (0.0277)	-0.0448*** (0.0125)	-0.0388* (0.0211)		-0.0123 (0.0205)	-0.0428*** (0.0094)	-0.0448*** (0.0100)	-0.0388*** (0.0125)			
ln Labour Force Participation Rate		-1.089 (0.025)	-0.1796 (0.0277)	0.1716 (0.9188)	-0.3595 (0.0211)		-1.089 (0.0205)	-0.1796 (0.0094)	0.1716 (0.0100)	-0.3595 (0.0125)			
Interstate Banking Dummy		0.1586*** (0.0537)	-0.0022 (0.0566)	0.0387 (0.1048)	-0.0388 (0.0956)		0.1586*** (0.0409)	-0.0022 (0.0205)	0.0387 (0.0362)	-0.0388 (0.0372)			
State	Yes	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes			
Year		Yes	Yes		Yes		Yes	Yes		Yes			
Observations	765	765	765	765	765		765	765	765	765			
R ²	0.96054	0.96807	0.98015	0.98103	0.98103		0.96054	0.96807	0.98015	0.98103			
Within R ²	0.77925	0.23867	0.68718	0.31857	0.31857		0.77925	0.23867	0.68718	0.31857			

Table A.9: Variable definitions.

Variable	Definition
<i>Measures of entrepreneurship (Andrews et al. 2022)</i>	
$SFR_{i,t}$	Number of new business registrations created in state i in year t solely by individuals in the form of a corporation, LLC, or partnership.
$EQI_{i,t}$	Probability of the startups in state i and year t to achieve growth outcomes within six years of founding, either through an IPO or a high-value acquisition. It is based on the average quality of a particular area and is calculated by aggregating twelve variables, each representing startup characteristics.
$RECPI_{i,t}$	Indicator of the overall inherent potential for startups within a geographic area. It combines the average quality, captured by EQI , with the quantity of entrepreneurship captured by SFR . It can also be defined as the quality-adjusted entrepreneurial activity or as the startup potential within a group of firms.
$\ln Startups_{i,t+1 to t+3}$	Natural logarithm of one plus state i 's total number of the outcome variable (SFR or $RECPI$) in years $t+1$ through $t+3$
$\ln SFR_{i,t+1 to t+3}$	Natural logarithm of one plus state i 's total number of the new business registrations in years $t+1$ through $t+3$
$\ln RECPI_{i,t+1 to t+3}$	Natural logarithm of one plus state i 's total expected number of startups that would achieve growth outcomes in years $t+1$ through $t+3$
<i>Measures of banking competition</i>	
$Index_{i,t}$	Index created by Rice and Strahan (2010) which ranges the restrictiveness of state i in year t towards interstate bank branching. A value of zero represent states that have lifted all restrictions towards out-of-state entry. A value of four indicates states with the most restrictive provisions towards interstate branching. Each lifting of an IBBEA branching provision decreases the <i>Index</i> with one unit.
$IDUM_{i,t}$	Dummy variable that has in year t a value of zero before the focal state i implemented the IBBEA and one otherwise.
$HHI_{i,t}$	Herfindahl-Hirschman Index is a measure of the banking concentration of state i in year t . It calculated by taking the sum over N of the square of the market share of deposits of the bank institution b registered in state i during year t .
$ImplementYear_t$	Categorical variable that classify state t according to the year during which it implemented the IBBEA. It can only take the value 1995, 1996 or 1997.
$Inter_{i,t}$	Dummy variable that has in year t the value zero before state i allowed interstate banking and one thereafter (Black & Strahan, 2002)
<i>Measures of other variables</i>	
$\ln GDP_{i,t}$	Natural logarithm of the real gross domestic product of state i in year t (U.S. Bureau of Economic Analysis).
$\ln Population_{i,t}$	Natural logarithm of the number of individuals that live in state i in year t (U.S. Bureau of Economic Analysis).
$Unemployment Rate_{i,t}$	Number of unemployed people as a percentage of the labor force in state i in year t (U.S. Bureau of Labor Statistics).
$\ln \frac{Labor Force}{Participation Rate}_{i,t}$	Natural logarithm of the ratio of the total number of individuals who are currently employed or in search of a job on the total working-age population in state i in year t (U.S. Bureau of Labor Statistics).

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