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Abstract

Economic theory offers competing hypotheses about how the cost and availability of finance influence labor market outcomes. Making use of the U.S. banking reforms between the 1970s and the 1990s as a quasi-natural experiment, this paper studies the impact of credit market development on employment. This paper documents the significant effects of these reforms on employment growth. Potential channels between finance and employment are also investigated. Changes in the growth of the number of self-employed individuals, the entry and exit of firms, and investment growth do *not* explain most of the employment growth following the reforms. The reforms had a substantially higher impact in industries with higher labor intensity, which is consistent with the idea that labor has fixed costs that need to be financed.

Keywords: Banking Reform, Employment
JEL Classification: D33, G21, J21

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1 Introduction

The large body of literature documenting the real effects of financial developments is mainly focused on developments' impact on output growth.[1] Surprisingly little, however, is known about the impact of financial developments on employment. The importance of this question is particularly clear following the recent financial crisis, which caused massive job destruction, and the following jobless recovery. In particular, the jobless recovery has highlighted the fact that increased output growth may not necessarily translate to higher employment.

Theoretically, the cost and availability of external finance have ambiguous effects on employment. On the one hand, easing financing constraints may allow firms to optimally substitute capital for labor (Garmaise (2008)) by investing in more capital-intensive technologies, thereby decreasing employment. On the other hand, because in the presence of capital market frictions investment is limited by the availability of internal funds, a decrease in the cost of external finance will increase firm level investment. Due to the fact that labor and capital are complement, the demand for labor goes up. Moreover, Acemoglu (2001) and Wasmer and Weil (2004) show that credit market imperfections lead to higher equilibrium unemployment by restricting firm entry.[2]

Investigating the causal effect of finance on labor market variables is, however, complicated by identification concerns of endogeneity if one uses outcome measures of financial development such as the size or the depth of financial markets. The same problem occurs if one uses measures of firms' financial health (such as net worth or leverage) or credit spreads since all these variables are also correlated with firms' demand for labor. As such, I use the U.S. banking reforms between the 1970s and the 1990s as a quasi-natural experiment to identify the impact of easing financial constraints on labor market outcomes. The removal of restrictions on geographic expansion resulted in better efficiency and pricing of banking services. Jayaratne and Strahan (1998) and Black and Strahan (2001) show that non-interest costs, wages, and loan losses all fell following reforms that removed restrictions on bank branching. These cost reductions led, in turn, to lower prices on loans although not on deposits (Kroszner and Strahan (2011)).[3] The simultaneous existence of cross-sectional and over-time variation concerning individual states' timing of the reforms represents a unique opportunity for identifying and assessing the causal impact of a positive shock to financial intermediation environment on employment and wages in the real sectors of the economy. As demonstrated in Figure 1, neither the rate of change in the aggregate wage bill nor employment growth before the reforms helps predict when a state removes restrictions on bank

[1] See Jayaratne and Strahan (1996), Rajan and Zingales (1998), Levine (2005), and Guiso, Sapienza, and Zingales (2004).

[2] These are not all potential links between finance and employment. In Section 6, these theories as well as other channels are explained and investigated.

[3] The mechanism for this better performance is changes in the market shares of banks following the reforms (Stiroh and Strahan (2003)). Prior to the reforms, well-run banks could not expand to new markets because banking was functioning as a local monopoly industry. When these constraints were lifted, however, better-run banks gained the opportunity to acquire other banks in new markets and therefore assets were reallocated towards the more efficient banks (see (Kroszner and Strahan (2011))).

branching, suggesting that the timing of branch deregulations at the state level is exogenous to labor market conditions.

Consequently, I employ a difference-in-differences estimation methodology that makes use of the cross-state, cross-year variation in the timing of bank branching reforms to assess the impact of finance on the growth of the aggregate wage bill. The results imply that the aggregate wage bill grew 0.74 percentage points more following the reforms, which is economically large since the average growth of aggregate wage bill in the sample is 2.35 percent. Further results show that employment growth (as opposed to the growth of wages) accounts for the growth of the wage bill. Specifically, while the growth of wages was unaffected, these reforms increased employment growth by 0.68 percentage points, which is translated to 32 percent increase of the average employment growth.

As a robustness check, I construct a reform index including all types of reforms that have made it easier for banks to expand geographically. In particular, other than lifting intrastate branching restrictions via mergers and acquisitions, states also removed restrictions on *de novo* branching and interstate banking between the 1970s and the 1990s. Combining all three reforms gives a reform index between 0 and 3, indicating the number of expansion types a state allows in each year. If banks' ability to enter new markets increases employment growth, one should expect to see higher employment growth in states with lower restrictions on the geographical expansion of banks. Indeed, the results indicate that employment grows at a higher rate in states that allow more types of expansion for banks. This finding increases confidence in the interpretation that the reforms have boosted employment by enhancing bank performance.

I finish by conducting an exploration of three possible explanations of the channels underlying the relationship between finance and employment. The first explanation focuses on reforms improving credit availability and promoting entrepreneurial activities. By increasing the number of entrepreneurs or by enabling them to start their business with a larger size, a reform that leads to a more efficient credit market could boost employment.

The second explanation relies on the idea that credit market developments decrease the cost of external finance, resulting in increased investment. Due to complementariness between labor and capital, the demand for labor also goes up. Finally, the third explanation highlights the first-order impact of financial constraints on the employment decision of firms. The direct channel is based on the idea that labor has aspects of fixed costs that require financing to bridge upfront costs (hiring costs, training, paying wages) and the subsequent revenues coming from selling goods. Easing financing constraints, therefore, allows firms to hire more employees and grow more in size. These channels, however, are not mutually exclusive and the aim is to shed light on the importance of each channel.

The employment growth that occured following these banking reforms is fully accounted

for by an increase in employment growth among salaried employees (as opposed to the self-employed). Moreover, intrastate branch deregulations did not affect firms' entry and exit rates. Therefore, the only possibility is that deregulations allowed entrants or incumbents to grow in size. Further investigation shows that the increased employment growth comes from higher rates of incumbents' job creation following the reforms. These results suggest that finance acts as a barrier to employment, but we still do not know through which channel easier access to finance increases the job creation rate of firms.

As the last step in exploring the channels between finance and employment, I investigate whether the increased employment is the result of higher investment or whether it is due to the direct impact of financial constraints on firms' employment decisions as discussed above. The results indicate that changes in investment cannot explain most of the increase in employment growth, which suggests that the first order impact of finance on employment is substantial. To find direct evidence on this channel, I use industry level data to be able to exploit cross-sectional differences in labor intensity across industries. Specifically, if easier access to finance helps firms to finance the fixed costs aspects of their labor and leads to increased employment, we should observe a larger effect on those industries in which the cost of labor is substantial. The triple difference estimation confirms that employment grows significantly more in labor intensive industries following banking reforms. More specifically, going from an industry on the first quartile of labor intensity (0.58) to the one on the third quartile (0.77), employment grows 0.63 percentage points more following the reforms. These results are in line with idea that labor, similar to capital, needs to be financed. The alternative view is based on the notion that labor expenses are variable costs (that are paid out of sales) and hence, unlike capital, does not require upfront investment. In most production activities, however, labor is not paid only upon the sale of goods in the market, but rather needs to be financed throughout the production process.[4] Overall, the results of this paper suggest that labor has fixed-costs aspects that require financing to bridge upfront costs and revenues.

The most important contribution of this paper is finding a strong link between finance and employment. The theoretical literature focusing on the employment consequences of the interaction between credit and labor markets is currently quite thin. Wasmer and Weil (2004) investigate this relation in a framework that includes job search, labor and credit matching frictions and negotiated mark-ups in the labor and credit markets. Their model generates a decomposition of unemployment into two parts, one depending on labor-market imperfections and the other related to credit-market imperfections. These imperfections exhibit interactions in the form of a credit multiplier such that credit market imperfections amplify the unemployment generated through imperfections in the labor market. Acemoglu (2001) shows that credit market frictions are an important constraint for job creation and provides empirical evidence suggesting that the presence of

[4]It is important to note that theoretical arguments in labor economics usually do take labor as a quasi-fixed factor (Oi (1962), Hamermesh (1989), and Hamermesh and Pfann (1996)).

more credit market frictions in Europe is one reason why unemployment in Europe is relatively higher than in the United States. Empirically, Pagano and Pica (2012) use the size of the credit market as a proxy for financial development and find that across countries employment growth is associated with financial development. However, the typical problems with cross-country studies and using outcome measures such as the size of countries' credit markets are omitted variable problem and reverse causality. Using the cross-sectional and temporal variation in the timing of states' banking deregulation, which was exogenous to the labor market, this paper evaluates the causal impact of local credit market developments on employment growth.

In addition, this paper investigates the channels through which the credit market might affect employment. The results suggest that labor, similar to capital, requires financing and hence a more efficient credit market increases employment even if investment levels remain constant. This is consistent with the result of Benmelech, Bergman, and Seru (2011), who show that the sensitivity of employment to cash flows remains high even after controlling for concurrent investment. [5] The first implication of this set of results is that credit market imperfections may play an important role in understanding the aggregate dynamics operating through labor as opposed to investment flows.[6] Moreover, this study sheds light on why the standard theory of equilibrium unemployment, which does not consider credit market frictions, fails to explain the large fluctuations of unemployment over business cycles (see Shimer (2005)). Indeed, some recent theoretical studies have incorporated financial frictions into standard models to help understand the large, unexplained employment fluctuations that are observed in the data (see Petrosky-Nadeau and Wasmer (2013), Petrosky-Nadeau (2011), and Petrosky-Nadeau (2010)).

Finally, this paper relates to a body of research on the effects of U.S. banking deregulations. Researchers have examined the impact of bank deregulation on states' income growth (Jayaratne and Strahan (1996)), output volatility (Morgan, Rime, and Strahan (2004); Demyanyk, Ostergaard, and Srensen (2007)); Acharya, Imbs, and Sturgess (2011)), wages in the finance industry (Black and Strahan (2001); Boustanifar (2014)), income inequality (Beck, Levine, and Levkov (2010)), small firm finance (Rice and Strahan (2010)) and economic integration through trade (Michalski and Ors (2012)). However, this literature has not looked at how the reforms affected labor market outcomes in the real sectors of the economy.

The remainder of the paper proceeds as follows. Section 2 describes a brief summary of U.S. banking reforms. Section 3 explains the identification strategy. Section 4 discusses the data sources used in the paper. Section 5 provides the core results, and Section 6 investigates the channels between finance and employment. Section 7 concludes.

[5] Benmelech, Bergman, and Seru (2011) also provide evidence that banking deregulation is associated with lower unemployment rates. However, they do not investigate causality and do not explore channels between deregulation and employment.

[6] For papers that study how credit market imperfections amplify shocks to the macroeconomy through investment channel see Kiyotaki and Moore (1997) and Bernanke, Gertler, and Gilchrist (1999).

2 The History of U.S. Banking Deregulation

The ability of banks to operate branch networks and holding company structures has been subject to state legislation since the 1920s.[7] Many states imposed restrictions on these banking activities, both within and across state borders. Between 1970 and 1994, however, such restrictions were gradually lifted in almost all states.

Although there was some deregulation of branching restrictions in the 1930s, about two-thirds of states continued to enforce restrictions on in-state branching until the 1970s. Only 12 states allowed unrestricted statewide branching in 1970. Between 1970 and 1997, however, 38 states eased their restrictions on branching.

These reforms of restrictions on intrastate branching typically occurred in a two-step process. First, states permitted multi-bank holding companies (MBHCs) to convert subsidiary banks (existing or acquired) into branches. MBHCs could then expand geographically by acquiring banks and converting them into branches. Second, states began permitting *de novo* branching, whereby banks could open new branches anywhere within state borders. Branching by mergers or acquisitions was often authorized earlier than *de novo* branching. In fact, as many as ten states did not permit statewide *de novo* branching well into the 1990s. Considerable consolidation therefore occurred, predominantly through mergers and acquisitions.

In addition to branching limitations, states also prohibited cross-state ownership of bank branches. Following passage of the McFadden Act, banks begun to undermine state branching restrictions by building multi-bank holding companies with operations in many states. The Douglas Amendment to the Bank Holding Company (BHC) Act of 1956 ended this practice by prohibiting a BHC from acquiring banks outside the state where it was headquartered unless the target bank's state explicitly permitted such acquisitions. Since no state allowed such acquisitions, holding companies were effectively prohibited from crossing state lines. Deregulation began in 1978, when Maine passed a law allowing entry by out-of-state BHCs if, in turn, banks from Maine were allowed to enter those states.[8] No state reciprocated, however, so the interstate deregulation process remained stalled until 1982, when Alaska and New York passed laws similar to Maine's. Other states then followed suit, and state deregulation of interstate banking was nearly complete by 1992, by which time all states but Hawaii had passed similar laws. These state changes, however, did not permit banks to open branches across state lines.[9] The transition to full interstate banking was completed with passage of the Interstate Banking and Branching Efficiency Act of

[7] The McFadden Act of 1927 essentially prohibited intrastate branching by subjecting the branches of national banks to state authority. The Douglas Amendment to the Bank Holding Company Act of 1956 further restricted interstate expansion by barring bank holding companies from acquisition in another state unless specifically authorized by that state.

[8] Entry in this case means the ability to purchase existing whole banks; entry via branching was still not permitted.

[9] With the exception of eight states (Alaska, Massachusetts, New York, Oregon, Rhode Island, Nevada, North Carolina and Utah), which allowed limited interstate branching. Despite interstate branching being allowed in these states, however, it was not exercised except in a few cases prior to the passage of IBBEA in 1994.

1994 (IBBEA), which effectively permitted bank holding companies to enter other states without permission and to operate branches across state lines.

This paper explits the staggered timing of deregulations to identify the effect of credit market developments on employment in the real sectors of the economy. More specifically, the deregulation experiment allows for a difference-in-difference strategy that estimates the changes in the state's employment growth after treatment (deregulation) controlling for changes in employment growth in the control group (regulated states). Section 3 explains the details of the identification strategies used in the paper.

3 Identification Strategy

3.1 State-Level Specification

The main identification strategy used in this paper is as follows:

$$Y_{st} = \text{Constant} + \beta.\ \text{Deregulation}_{st} + \mathbf{A_s} + \mathbf{B_t} + \varepsilon_{t,s} \quad (1)$$

where in separate regressions Y_{st} equals aggregate wage bill growth, employment growth, or the growth of wages in state s over time t. Deregulation_{st} is a deregulation indicator which is equal to 1 for states without restrictions on branching via M&A and 0 otherwise. In this specification, $\mathbf{B_t}$ (year-specific dummy variables) controls for nation-wide shocks and trends that shape the dependent variables over time, such as business cycles, national changes in regulations and laws, long-term trends in dependent variables, and so on. $\mathbf{A_s}$, which represents state-specific dummy variables, controls for time-invariant differences in long-run labor market variables due to unexplained factors that differ across states. The coefficient of interest, β, gives us a difference-in-differences estimator that shows the changes in the dependent variable following the reform in the treatment state, controlling for changes in the dependent variable among the control group (states that did not deregulate).

The literature has shown that aggregate trends in technology affected all financial services firms and created increasingly strong pressures for regulatory regime change, and that interest-group factors within financial services account for differences in the *timing* of state-level deregulations (Kroszner and Strahan (1999)). Hence, a cross-sectional comparison of banking structure and its relationship with states' labor market outcomes might be misleading. By including state fixed effects in the model, all of the cross-sectional variation (such as when a state deregulates) gets removed; coefficients are driven by changes in variables after a state alters its regulations. Also, persistent differences across states (e.g., those dominated by large vs. small banks) do not

affect the results. Moreover, there is no evidence that *changes* in economic conditions lead (or predict) deregulation (Kroszner and Strahan (2011)). Further evidence will be shown later that strongly suggests that the timing of states' deregulation was exogenous to labor market outcomes.

3.2 Industry-Level Specification

When investigating the channels between finance and employment, I use industry level data to make use of cross-industry differences in labor intensity. Specifically, I will be testing whether the impact of the reforms is larger for more labor-intensive industries. To do so, I will use the following specification:

$$\begin{aligned} Y_{jst} &= \text{Constant} + \beta.\ \text{Deregulation}_{st} \times \text{Labor intensity}_{j} \\ &+ \delta.\ \text{Employment share}_{js} + \mathbf{A_{st}} + \mathbf{B_{jt}} + \varepsilon_{jst} \end{aligned} \tag{2}$$

where Y_{st} equals employment growth of industry j in state s over time t. The deregulation indicator is similar to before. Labor intensity is a measure of industries' dependence on labor, or the unit cost of labor. It is calculated by dividing the aggregate wage bill over value added for each industry. Industry j's share of total employment in state s at the beginning of the sample (*Employment share*$_{js}$) controls for the relative size of a given sector in a market.[10] $\mathbf{A_{st}}$ controls for any time-variant state-specific factor that might affect employment, whereas $\mathbf{B_{jt}}$ captures all time-variant industry-specific factors that drive employment growth in each industry which is not related to the reforms.

Note that the direct effect of *Labor intensity*$_j$ is not identified in the above specification because its effect is fully captured by the set of industry-level fixed effects. Similarly, the direct effect of *Deregulation*$_{st}$ is captured by state-time fixed effects. In fact, in this specification, β is a triple-differences estimator. The difference-in-difference-in-differences (DDD) estimate starts with the time change in averages for the labor intensive industries in the treatment states and then nets out the changes in means for labor-intensive industries in the control states and the changes in means for the non-labor-intensive industries in the treatment states. The hope is that this will control for two kinds of potentially confounding trends: changes in the growth status of labor intensive industries across states (which would have nothing to do with the reform) and changes in the growth status of all industries in policy-change states (possibly due to other state policies that affect all industries, or state-specific changes in the economy that affect all industries).

[10] Using employment shares that are time-variant does not change the results. However, I use employment shares at the beginning of the sample to avoid the endogeneity of employment shares and banking reforms.

4 Data

I use several datasets at different levels: state, industry, and individual. To focus on the episode of bank branching deregulation from the 1970s to the mid-1990s, the sample spans from 1976 to 1997. I start in 1976 because some of the data, as will be explained later, start in that year. I stop in 1997, three years after the full transition to interstate banking. All the analyses in this paper are robust to ending the sample a few years earlier or a few years later.

Labor market variables (state-level)

I construct a panel data set of aggregate wage bill growth, employment growth, and wage growth at the state-level between 1976 and 1997. The aggregate wage bill is provided as a component of GDP and is available at the state level on an annual basis from the Bureau of Economic Analysis (BEA). It is the sum of wages and salaries and supplements to wages and salaries.[11] However, aggregate wage bill data omit the labor income of self-employed people. I follow Gollin (2002) to adjust for self-employment income. Specifically, self-employed income is treated as containing the same mix of labor and capital income as the rest of the economy.[12]

The BEA also provides total employment as well as wages-and-salary employment for each state by industry in each year. Total employment is equal to wages-and-salary employment plus proprietors employment. Data availability restricts the analysis to the two-digit level of aggregation by industry. From this data set, I compute (total) employment growth and average wage growth in each state and year. Average wages are obtained by dividing the aggregate wage bill over total employment.

Employment Dynamics (state-level)

As part of the investigation of channels between banking reforms and employment, I use the Business Dynamic Statistics (BDS) dataset. The dataset includes all establishments with paid employees in the United States. It reports establishment openings and closings, employment, job creation and destruction, and job expansions and contractions. The information is compiled (by BDS) from a database of establishments and firms tracked over time known as the Longitudinal Business Database.

Individual-level data

[11] More specifically, it includes wages and salaries (cash and in-kind), commissions, bonuses, tips, cost of living adjustments, vacation and sick leave allowances. Also it includes employer contributions to social security programs and pension schemes, employer contributions to insurance funds, employers' paid and imputed contributions to pensions, family allowances, layoff and severance pay, health plans, and other benefit packages.

[12] Gollin (2002) shows that it is crucial to adjust for self-employed income specifically when dealing with data from poor countries, where self-employed constitute 50 percent of workers on average. In the case of the U.S. there is not much difference because, as noted by Gollin (2002), fewer than two percent of the manufacturing workers are self-employed. The results of this paper remain unchanged with or without the adjustment, both quantitatively and qualitatively.

I use individual data to examine the impact of deregulation on employment status of observationally equivalent individuals before and after deregulation. My individual data are from the March Supplement of Current Population Survey (CPS), which is an annual survey of about 60,000 households across the United States. CPS is the standard database used by labor economists and is the official source of the U.S. unemployment rate. It is a repeated, representative sampling of the population, but it does not trace individuals through time. Information provided by CPS includes employment status, type of employment, industry, education, income, and a wide-array of demographic characteristics in the year prior to the survey. Most importantly for this study, I start with the 1977 survey because the exact state of residence is unavailable prior to this survey. I restrict my attention to individuals in the labor force who are between 15 and 65 years old. As I am interested in the real impact of the reforms, I drop all individuals employed in the finance sector.

Industry level variables including labor intensity

To test for the direct impact of finance on employment, I obtain data on value added, the aggregate wage bill, and employment by state, industry, and year from the BEA. The sample is from 1976 to 1997, includes 50 states plus te District of Columbia, and contains 32 industries. I drop the finance industry to focus on the real sectors of the economy.[13] Using this data, following the literature, I construct a measure of labor intensity at industry level (see, for example, Simintzi, Vig, and Volpin (2012)). To do so, I divide the aggregate wage bill by value added for each industry in each state in the year 1980. This gives a measure of labor intensity for each industry in each state. The median of this measure across states is taken as the labor intensity for each industry.[14] The results found later are robust to the different methods of constructing labor intensity. In particular, the choice of reference year or aggregation at the country level rather than state level does not materially change the results. In general, the ranking of labor intensity across industries does not vary much across states and over years.

Unfortunately, investment data do not exist by state and industry. Chirinko and Wilson (2009) provide data on investment by state only for total manufacturing. Therefore, when investigating the impact of deregulation through changes in investment I replicate all other results using total manufacturing data and then include investment in the analysis.

Banking reforms

Consistent with most of the literature, I choose the date of reforms as the date on which a state permitted branching via M&As through the holding company structure. The literature has shown that deregulation via M&A is the only type of branching deregulation that consistently affected banking structure and bank efficiency (see Jayaratne and Strahan (1996) and Kroszner

[13] I exclude the finance sector to reduce the concern that the results are driven by changes in employment within the finance sector following deregulations. As the primarily aim of the paper is to investigate the impact of the financial market on real sectors of the economy, excluding the finance sector is important.

[14] This measure is sometime called *unit labor cost* since it could be written as the ratio between wage rate and labor productivity, $\frac{w}{VA/N}$.

and Strahan (1999)). I also construct a reform index combining all three types of deregulations and investigate the impact of the index on labor market outcomes as a robustness check. Table 1 presents deregulation dates for each state.

Table 2 presents summary statistics for all variables. Table 3 reports labor intensities for all 32 industries.

5 Reforms and Labor Market Outcomes

In this section, I start by showing that the timing of branching reforms was not affected by pre-existing labor market conditions. Consequently, I address the impact of banking reforms on the growth of employment and wages.

5.1 Labor Market Outcomes and the Timing of Reforms

I start with an analysis of state-level data to investigate whether or not the timing of banking reform at the state level was affected by labor market outcomes. I address this issue in several ways. First, I examine whether aggregate wage bill growth and employment growth before the year of reform can help predict the timing of states' reforms. Figure 1 shows that neither the growth of the wage bill, nor employment growth before deregulation, can explain the timing of bank deregulations. The t-statistics for the correlations are -0.99 and 0.26, respectively.

Additional evidence that labor market outcomes did not affect the timing of branch deregulation emerges from a hazard model study of deregulation. Following Kroszner and Strahan (1999), Table 4 reports tests of whether labor market outcomes influence the likelihood that a state deregulates in a specific year given that it has not deregulated yet.

Table 4 indicates that the timing of branch deregulation does not vary with the degree of pre-existing labor market outcomes. Column 1 reports the results of a regression with only employment and wage growth, while other columns provide regression results controlling for numerous state-level control variables, including those state characteristics employed by Kroszner and Strahan (1999). As shown, the growth of employment and wages do not enter significantly in any of the Table 4 regressions. Therefore, the results show that pre-existing labor market outcomes have no explanatory power in explaining the timing of banking reform.

Finally, I run a set of placebo regressions with the following procedure. I randomly draw deregulation dates with replacement from the empirical distribution of the actual dates. Then, I re-run the regression similar to the one in column 3 of Table 5 with the simulated deregulation times. I perform this procedure for 100 times and retrieve the point estimate corresponding to

Deregulation variable. While the actual estimate is 0.681, the average estimate from placebo regressions is 0.004 and only in 5 percent of times the null hypothesis (that the coefficient is zero) is rejected. Therefore, the placebo regressions also strongly confirm validity of the identification strategy used in the paper.

5.2 Reforms and Labor Market Outcomes

I use a difference-in-differences methodology similar to Equation 1 to evaluate the impact of banking reforms on labor market outcomes at the state level. Specifically, I estimate Equation 1 for three different dependent variables: the growth of the aggregate wage bill, employment growth, and wage growth. All regressions include state and year fixed effects. I also run a specification including state trends. Standard errors are clustered by state to address the serial correlation concerns of Bertrand, Duflo, and Mullainathan (2004) for differences-in-differences estimations.

Table 5 presents the results, indicating that bank deregulation substantially increased the growth of the aggregate wage bill by boosting employment growth. Specifically, Column (1) suggests that the aggregate wage bill grows 0.74 percentage point more following intrastate branching deregulation. The effect is also economically large, as the average growth of wage bill in the sample is 2.35 percent. To put it differently, the reform increased the growth of aggregate wage bill by about 31 percent.

Columns 3, 4, 5, and 6 of Table 5 investigate the cause of increases in the aggregate wage bill. Specifically, the regressions examine whether the wage bill grows more because of higher employment or because of higher wage growth. The results strongly suggest that deregulation increased the aggregate wage bill by boosting employment growth. The coefficient on intrastate deregulation in the regression of employment growth is 0.68 and statistically significant at the 1 percent level. As average employment growth in the sample is 2.15 percent, the economic magnitude of the effect is substantial. As shown in Columns 5 and 6, however, average wages do not grow more in deregulated states. Overall, the results of Table 5 strongly suggest that bank deregulation has had a positive and significant effect on local employment growth.

5.3 Dynamics of Reforms and Employment

Although the results of Table 5 demonstrate that employment growth increased following episodes of banking deregulation, the analysis does not yet provide information on whether the impact was temporary or long-term. In addition, despite the fact that Figure 1 and Table 4 suggest that employment growth did not precede deregulation, they do not explicitly document the dynamics of changes in employment before and after deregulations. To clarify the timing of the impact of deregulation, I examine the dynamics of the relationship between deregulation and employment.

I do this by including a series of dummy variables in the regression (Equation 1) to trace out the year-by-year effects of bank deregulation on employment growth:

$$\text{Employment growth}_{st} = \text{Constant} + \beta_1 D_{st}^{-4} + \beta_2 D_{st}^{-3} + ... + \beta_4 D_{st}^{+4} + \mathbf{A_s} + \mathbf{B_t} + \varepsilon_{st}.$$

where deregulation dummy variables, the "D's", equal zero, except as follows: D^{-j} equals one for states in the jth year before deregulation, while D^{+j} equals one for states in the jth year after deregulation. I exclude the year of deregulation, thus estimating the dynamic effect of deregulation on employment growth relative to the reform year. The vectors $\mathbf{A_s}$ and $\mathbf{B_t}$ are vectors of state and year dummy variables, respectively. At the end points, D_{st}^{-4} equals one for all years that are four or or more years before deregulation, while D_{st}^{+4} equals one for all years that are 4 or more years after deregulation. Figure 1 plots the year by year estimates of deregulation on employment growth and the 95 percent confidence intervals, which are adjusted for state-level clustering.

Figure 2 illustrates two key points: First, changes in employment growth did not precede deregulations, and the impact of reforms on employment growth becomes significant at 5 percent within two years of enactment. As shown, the coefficients on the deregulation dummy variables are not significantly different from zero for all years before deregulation. Furthermore, employment growth increases immediately after deregulation and becomes significant at the 5 percent level in the second year following the reform. The positive and significant impact of deregulation on employment remains for several years. In sum, changes in employment do not precede deregulation and employment grows substantially following deregulation.

5.4 Reforms and Unemployment Rate (Individual Data)

In this section, I use individual data to investigate the impact of deregulation on the probability of individuals being unemployed following deregulation after controlling for all demographic and other individual characteristics. This analysis complements the previous results and minimizes the concern regarding the omitted variable problem. For example, one might argue that changes in demographic variables or education levels at a state might have an effect on changes in regulation as well as changes in employment. As such, using CPS data and applying a Probit model, I assess how deregulation affected the probability of an individual being unemployed, conditional on all individual characteristics.

I estimate a Probit model, where the dependent variable is one if an individual is unemployed and zero otherwise. I am interested in the coefficient on the deregulation indicator, after controlling for all observable individual characteristics as well as state and year fixed effects. The controls include sex, age, age squared, race, marital status, and education. In this way, I am comparing

the probability of observationally equal individuals being unemployed in a state before and after deregulation and then netting out the changes of unemployment probability for similar people in states that did not change their regulation status.

Table 6 presents the average marginal effects of the Probit regression. Note that all regressions control for the individual characteristics mentioned above. While Columns 1 and 2 investigate the impact of intrastate branching deregulation on the probability of individuals being unemployed, Columns 2 and 4 examine the impact of the constructed deregulation index on the probability of unemployment. Regardless of the choice of specification, the results indicate that deregulation decreased unemployment significantly. For example, the coefficient on intrastate deregulation implies that unemployment rate declined by between 0.54 and 0.63 percentage point following deregulation. This translates to a reduction of the sample mean of between eight and nine percent, which is a sizeable effect.

6 Channels Between Finance and Employment

Having found that banking deregulation increased employment (and labor income) growth by affecting bank performance, I now explore three potential channels underlying these findings. The first explanation relies on an indirect impact of deregulation on employment through investment. Jayaratne and Strahan (1998) show that branch deregulation reduced the cost of capital. Although a reduction in the cost of capital encourages firms to substitute capital for labor, it may also increase firm-level investment. As mentioned previously, since labor and capital are complements, as investment increases the demand for labor goes up.

The second explanation focuses on deregulation improving the ability of individuals to access credit and become entrepreneurs. Obtaining adequate access to capital is one of the biggest hurdles to starting a new business (Kerr and Nanda (2009), Lelarge, Sraer, and Thesmar (2010)). Bank deregulation that eases financing constraints could promote entrepreneurship and the rate of firms' entry. This, in turn, could increase employment. The third explanation highlights the first-order impact of financial constraints on firms' demand for labor. When labor needs to be paid throughout the production process before it generates cash flow, firms must be able to finance their costs related to the search for labor, training it, and paying wages throughout the production process (Greenwald and Stiglitz (1987)). As such, when access to credit improves, firm employment should increase.

So far, in the regressions, I have treated employees as a homogeneous group. The employment variable, used in all previous regressions, is the sum of wages-and-salary employees (hereafter wage earners) and self-employed individuals. To provide an assessment of the entrepreneurship channel, I investigate the impact of bank deregulation on wage-earners and entrepreneurs separately.

The results are shown in Table 7. Columns 1 and 4 investigate the impact of intrastate branching on different types of employment. Columns 2 and 4 use the deregulation index as an alternative for intrastate branching. No matter which measure of deregulation is used, the results indicate that deregulation substantially increased the growth of wage-earners whereas the growth of the number of self-employed was not affected.[15] Intrastate deregulation increased the growth of wage earners by 0.73 percentage point. As the sample mean of wage earners' growth is 2.05 percent, this is a significant effect.

The growth of the number of wage-earners following deregulation could be as a result of changes in the rate of entry and exit of firms, or it could be due to changes in the size of existing and/or entrants. Table 8 investigates whether deregulation altered the rate of entry or exit of firms. The results indicate that neither intrastate deregulation nor the deregulation index is associated with changes in the rate of entry and exit of firms. This shows that the increased employment following banking reform does not come from extensive, but rather from intensive, margins. That is, the reforms led to either higher job creation or lower job destruction within firms. Panel A of Table 9 investigates how job creation and destruction were affected by intrastate deregulation. Columns (1) and (4) of Table 9 show that while the rate of job destruction was unaffected, branch deregulation substantially increased the rate of job creation. To provide a better understanding of the channel, I assess the impact separately for the entrants and incumbents. Columns (2) and (3) of Table 9 show that the increased employment comes from incumbents but not entrants. Similar results are found in Panel B of Table 9, which provides results from the same tests using the deregulation index instead of intrastate bank branching. The result that increased employment is due to higher rates of job creation by incumbents following deregulation has an important implication: restrictions on geographical activities of banks was serving as a barrier to employment, preventing firms to grow in size.

Finally, I investigate the exact channel behind deregulation and the job creation of incumbents. The first hypothesis is that firms' employment increased due to increased investment following deregulation. According to this channel, financial constraints do not affect labor directly, since, unlike capital, labor does not require financing. Instead, relaxing financial constraints allow firms to increase investment and, in turn, labor is adjusted for the increase in capital. As investment data by state and industry are not available, this part of the analysis is based on total manufacturing data, for which I have obtained investment data from Chirinko and Wilson (2009). Due to the change in sample, I replicate the impact of deregulation on employment growth using total manufacturing data. Column 1 of Table 10 shows that deregulation increased employment growth of total manufacturing sector by 0.95 percentage point, which is both economically and statistically

[15] This result seems to be in contrast to Guiso, Sapienza, and Zingales (2004), who find that local financial development increases entrepreneurship. However, note that their measure of financial development is an inverse function of the conditional probability of obtaining loans for households, which is expected to increase entrpreneurship. However, there is no evidence that banking deregulations increased access to credit for households who did not have access to loans. Therefore, the measures of banking efficiency used in this paper and financial development used in Guiso, Sapienza, and Zingales (2004) capture two distinct aspects of improvement in financial systems.

significant.

Having replicated the previous result using manufacturing data, I seek to understand whether employment grows more because of higher investment rates. I do this in two ways. First, I control for investment growth post-deregulation in the regressions. If the channel is only through investment, controlling for changes in investment should make the link between deregulation and employment disappear. Columns 2 and 3 control for investment growth and the interaction of investment growth and deregulation, respectively. As shown, however, the economic and statistical significance of the effect of deregulation on employment growth changes only slightly, suggesting that only a small part of the link between deregulation and employment growth can be explained by changes in investment. Second, since investment itself might be endogenous to bank deregulations, perhaps a better way is to investigate the impact of bank reforms on investment in a regression where investment growth is used as the dependent variable. In such a regression, the coefficient of deregulation is not significant, indicating that investment growth was not affected by bank reform. The results of this regression are not reported but are available upon request.

As there is not much evidence supporting the investment channel, I examine the importance of the direct impact of finance on employment. The direct channel is based on the idea that labor has fixed costs that require financing to bridge upfront costs (such as hiring costs, training, paying wages) and the subsequent revenues coming from sales. Easing financing constraints following deregulation, therefore, allows firms to hire more employees and become larger. If this is the underlying channel between deregulation and employment, we should see a larger effect on those industries in which the unit cost of labor is higher. To test this, I run regressions similar to Equation 2, and the coefficient of interaction between deregulation and labor intensity is the coefficient of interest.

Columns 4, 5, and 6 of Table 10 report the results on the direct impact of finance on labor. The columns are different due to the choice of fixed effects. As shown, the coefficient of (Intrastate x Labor intensity) is positive and varies little across all specifications. This means that deregulation has a larger impact on employment in industries that are more labor-intensive. The magnitude of the effect is also economically very large. For example, going from the Hotels industry, with a labor intensity of 0.58 (in the first quartile), to Health Services industry, with a labor intensity of 0.77 (in the third quartile), employment grows 0.63 percentage point more following deregulation. Given that the sample mean of employment growth at industry level is 2.29 percent, this is a quite significant effect. Thus, the results support the idea that labor, similar to capital, has fixed-cost aspects that require financing to bridge upfront costs and revenues.

7 Concluding Remarks

The recent financial crisis and the following labor market disruptions underscore the importance of a deeper understanding of the impact of financial markets on labor market outcomes. This paper documents a strong positive link between credit-market efficiency and employment. The results also suggest that labor, similar to capital, needs to be financed and this explains most of the positive relation between banking reforms and the subsequent increases in employment. The finding that labor has aspects of fixed costs has an important implication: when firms experience even a temporary decline in the demand for their goods, they will not be able to maintain their highly trained employees in presence of financial constraints. This implies that financial constraints can potentially amplify variation in employment levels over the business cycle, which sheds light on why recessions associated with financial crises tend to be unusually severe. The same argument could also potentially explain jobless recoveries following the recent financial crisis. As long as access to credit (especially for younger firms) is not back to the pre-crisis conditions, they will not be able to incur the fixed costs of hiring new employees. This interpretation is consistent with Campello, Graham, and Harvey (2010) who find that financially constrained firms planned deeper cuts in employment in the midst of the recent financial crisis.

This paper has a more general implication for the corporate finance literature. In most of the corporate finance literature, labor is implicitly treated as a purely variable factor of production. When labor markets are frictionless, employees can be hired and fired costlessly and always earn their marginal product. In such a setting labor has no effect on firm value or firm policies. In the presence of labor market frictions, however, labor becomes a quasi-fixed factor of production and contributes to firm value. Such frictions may include the inability of workers to insure their labor income, costly search and matching, or the acquisition of firm-specific human capital. Fixed costs of labor produce a match-specific surplus associated with the employment relationship, which must be divided between workers and employers. When part of firms' value comes from rents earned from keeping their labor, a value maximizing firm will consider the effects of its policies on labor. Having said that, much more needs to be learned about how financial markets and labor markets are related.

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Table 1: Year of Deregulation of Restrictions on Geographical Expansion, by State

Intrastate bank branching via M&A allows banks to branch statewide via mergers and acquisitions only. *de novo* branching deregulation allows banks to enter new markets in their states by opening new branches. Interstate banking refers to the year in which a state entered into an interstate banking agreement with other states. ** indicates that the state was not deregulated by 1997.

State	via M&A	via *De novo*	Interstate
Alabama	1981	1990	1987
Alaska	1960	$<$ 1970	1982
Arizona	1960	$<$ 1970	1986
Arkansas	1994	**	1989
California	1960	$<$ 1970	1987
Colorado	1991	**	1988
Connecticut	1980	1988	1983
Delaware	1960	$<$ 1970	1988
District of Columbia	1960	$<$ 1970	1985
Florida	1988	1988	1985
Georgia	1983	**	1985
Hawaii	1986	1986	1997
Idaho	1960	$<$ 1970	1985
Illinois	1988	1993	1986
Indiana	1989	1991	1986
Iowa	*	*	1991
Kansas	1987	1990	1992
Kentucky	1990	**	1984
Louisiana	1988	1988	1987
Maine	1975	1975	1978
Maryland	1960	$<$ 1970	1985
Massachusetts	1984	1984	1983
Michigan	1987	1988	1986
Minnesota	1993	**	1986
Mississippi	1986	1989	1988
Missouri	1990	1990	1986
Montana	1990	**	1993
Nebraska	1985	**	1990
Nevada	1960	$<$ 1970	1985
New Hampshire	1987	1987	1987
New Jersey	1977	**	1986
New Mexico	1991	1991	1989
New York	1976	1976	1982
North Carolina	1960	$<$ 1970	1985
North Dakota	1987	**	1991
Ohio	1979	1989	1985
Oklahoma	1988	**	1987
Oregon	1985	1985	1986
Pennsylvania	1982	1990	1986
Rhode Island	1960	$<$ 1970	1984
South Carolina	1960	$<$ 1970	1986
South Dakota	1960	$<$ 1970	1988
Tennessee	1985	1990	1985
Texas	1988	1988	1987
Utah	1981	1981	1984
Vermont	1970	1970	1988
Virginia	1978	1987	1985
Washington	1985	1985	1987
West Virginia	1987	1987	1988
Wisconsin	1990	1990	1987
Wyoming	1988	**	1987

Table 2: Summary Statistics

This table presents summary statistics for variables used in the paper. The sample is from 1976 to 1997 and includes 50 states plus District of Columbia. *Wage bill growth* is the growth of aggregate wage bill, while *Wage growth* is the growth of average wages. Data on these variables together with *Wages earners* and *Proprietary employment* is from BEA. Data on firm entry, firm exit, job creation and job destructions is from Business Dynamic Statistics. *Investment growth* is from Chirinko and Wilson (2009), which is an estimate of investment growth in the manufacturing sector for each state. All growth variables are reported in percentage change. *Unemployment rate* is constructed using data from Current Population Surveys. *Branching deregulation* is an indicator that gets the value of 1 in all years that a state allows intrastate branching via M&A. *Deregulation index* equals the number of expansion types (branching via M&A, *de novo* branching, interstate banking) a state allows in each year. *Labor intensity* is the ratio of each industry's aggregate wage bill to its value added.

	Mean	SD	Min	Max	Observation
Wage bill growth	2.35	3.67	-17.45	18.76	1122
Employment growth	2.15	2.14	-4.82	12.67	1122
Wage growth	0.19	2.50	-17.78	13.12	1122
Firm entry rate	13.32	2.52	7.50	28.80	1122
Firm exit rate	11.12	1.75	7.80	25.90	1122
Job creation rate	18.08	3.45	12.00	45.10	1122
Job creation rate (birth)	6.81	1.75	3.70	18.80	1122
Job creation rate (continuers)	11.28	2.29	7.27	38.37	1122
Job destruction rate	15.44	2.99	10.00	40.50	1122
Job desdruction rate (deaths)	5.55	1.31	3.20	18.30	1122
Job destruction rate (continuers)	9.89	2.25	6.13	36.57	1122
Proprietory employment growth	2.78	3.68	-15.81	25.05	1122
Wages earners growth	2.05	2.36	-6.61	13.46	1122
Investment growth	6.98	26.24	-73.80	182.63	1100
Unemployment rate	6.76	25.10	0	1	1084422
Branching deregulation	0.66	0.47	0.00	1.00	1122
Deregulation index	1.69	1.24	0.00	3.00	1122
Employment growth (industry)	2.29	9.42	-81.43	182.43	31913
Labor intensity	0.67	0.17	0.10	0.91	31913

Table 3: Labor Intensity

Labor intensity is constructed by, first, dividing wage bill to value added for each 32 industries at each state in year 1980. This gives a measure of labor intensity for each industry at each state. Then, labor intensity for each industry is constructed as the median of labor intensity for that industry across all states. The raw data is from BEA.

Industry	Labor intensity
Agriculture, forestry, and fishing	.0995
Tobacco products	.402
Petroleum and coal products	.419
Legal services	.434
Personal services	.492
Mining	.501
Transportation and public utilities	.518
Wholesale trade	.563
Hotels and other lodging places	.579
Business services	.614
Lumber and wood products	.631
Chemicals and allied products	.633
Retail trade	.641
Food and kindred products	.697
Construction	.701
Paper and allied products	.716
Primary metal industries	.742
Printing and publishing	.745
Industrial machinery and equipment	.749
Stone, clay, and glass products	.756
Fabricated metal products	.756
Miscellaneous manufacturing	.756
Leather and leather products	.758
Health services	.767
Electronic and other electric equipment	.780
Instruments and related products	.786
Rubber and misc. plastics products	.808
Furniture and fixtures	.817
Textile mill products	.832
Apparel and other textile products	.854
Motor vehicles and equipment	.878
Educational services	.908

Table 4: Timing of Reforms and Pre-existing Labor Market Outcomes: The Duration Model

The model is a Weibul hazard model where the dependent variable is the log expected time to bank branching deregulation. The sample period is from 1976 to 1997 and the sample comprises 37 states that deregulated after 1976. States drop from the sample once they deregulate. The control variables are taken from Kroszner and Strahan (1999). Standard errors are adjusted for state-level clustering and corresponding t-values appear in parentheses.

	(1)	(2)	(3)
Employment growth	-.05	-.04	-.01
	(-1.26)	(-1.33)	(-.57)
Wage growth	.00	.03	.01
	(.13)	(1.09)	(.37)
Growth rate of per capita Gross State Product (2000 dollars)		-.58	.36
		(-.44)	(.27)
Small bank asset share of all banking assets in the state		6.86***	8.0 2***
		(2.89)	(4.11)
Capital ratio of small banks relative to large in the state		12.33**	11 .14**
		(2.40)	(2.13)
Relative size of insurance in states where banks may sell insurance, 0 otherwise		3.84*	1.22
		(1.74)	(.54)
Indicator is 1 if banks may sell insurance in the state		-2.12**	-.78
		(-2.33)	(-.84)
Relative size of insurance in states where banks may not sell insurance, 0 other		-1.80***	-.69
		(-2.71)	(-1.08)
Small firm share of the number of firms in the state		-10.11***	-22.02***
		(-3.51)	(-4.01)
Share of state government controlled by Democrats		.24	.15
		(1.10)	(.73)
Indicator is 1 if state controlled by one party		.08	.32*
		(.56)	(1.89)
Average yield on bank loans in the state minus Fed funds rate		-4.73	-6.13
		(-.54)	(-1.05)
Indicator is 1 if state has unit banking law		.27**	.45***
		(2.23)	(2.73)
Indicator is 1 if state changes bank insurance powers		-.02	-.24
		(-.10)	(-.99)
Regional indicators	No	No	Yes
Observations	408	408	408

Table 5: The Impact of Reforms on Labor Market Outcomes

Each column in this table reports statistics from a regression similar to Equation 1, where the dependent variable is the columns' label. Data on aggregate wage bill, employment and wages are from the Bureau of Economic Analysis. Intrastate deregulation indicator equals one during all years in which a state permits intrastate branching and zero otherwise. Deregulation index is an indicator which equals the number of expansion types a state allows in each year and varies between 0 and 3. The number of observations in each regression corresponds to 50 states plus District of Columbia for years 1976 to 1997. All regressions control for state and year fixed effects. Standard errors are clustered at state level. Values of t-statistics are in parentheses, where *** indicates significance at 1% level, ** indicates significance at 5% level and * indicates significance at 10% level.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Aggregate wage bill growth		Employment growth		Wage growth	
Intrastate Deregulation	0.739**		0.681***		0.054	
	(2.255)		(2.745)		(0.384)	
Deregulation Index		0.440**		0.393**		0.045
		(2.249)		(2.461)		(0.524)
Observations	1,122	1,122	1,122	1,122	1,122	1,122
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.518	0.519	0.540	0.541	0.506	0.506

Table 6: The Impact of Reforms on Unemployment

This table reports the effect of banking reforms on unemployment. Each column in this table reports statistics from a Probit regression, where the dependent variable is a binary variable: it is equal 1 if the person is unemployed and 0 if employed. The data is from Current Population Surveys between 1977 and 1997. The sample includes individuals between 15 and 65 years old who have been part of the labor force during the year before the survey. Intrastate deregulation indicator equals one during all years in which a state permits intrastate branching via M&A. Deregulation index is an indicator which equals the number of expansion types a state allows in each year and varies between 0 and 3. All regressions include individual controls. The controls include sex, age, age square, race, marital status, and education. Standard errors are clustered at state level. Values of t-statistics are in parentheses, where *** indicates significance at 1% level, ** indicates significance at 5% level and * indicates significance at 10% level.

	(1)	(2)	(3)	(4)
VARIABLES	Unemployment			
Intrastate Deregulation	-0.536**		-0.625*	
	(-2.191)		(-1.746)	
Deregulation Index		-0.312***		-0.401**
		(-2.932)		(-2.461)
Observations	1,084,422	1,084,422	1,084,422	1,084,422
Individual Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
State trends	No	No	Yes	Yes

Table 7: The Impact of Reforms on Different Types of Employment

Each column in this table reports statistics from a regression similar to Equation 1, where the dependent variable is the columns' title. Columns 1 and 3 investigates the impact of deregulations on self-employed growth, whereas Columns 2 and 4 examines the impact of deregulations on wage earners' growth. The data is from Bureau of Economic Analysis, includes 50 states plus District of Columbia and is from 1976-1997. Intrastate deregulation equals one during all years in which a state permits intrastate branching via M&A. Deregulation index is an indicator which equals the number of expansion types a state allows in each year and varies between 0 and 3. Standard errors are clustered at state level. Values of t-statistics are in parentheses, where *** indicates significance at 1% level, ** indicates significance at 5% level and * indicates significance at 10% level.

	(1)	(2)	(3)	(4)
VARIABLES	Self-Emp Growth		Wage Earners' Growth	
Intrastate deregulation	0.498		0.732***	
	(1.212)		(2.744)	
Deregulation Index		0.377		0.390**
		(1.419)		(2.415)
Observations	1,122	1,122	1,122	1,122
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
R-Squared	0.219	0.220	0.560	0.560

Table 8: The Impact of Reforms on Entry and Exit of Firms

Each column in this table reports statistics from a regression similar to Equation 1, where the dependent variable is the column's label. The data is from Business Dynamic Statistics, includes 50 states plus District of Columbia and is from 1976 to 1997. Intrastate deregulation indicator equals one during all years in which a state permits interstate banking. Deregulation index is an indicator which equals the number of expansion types a state allows in each year and varies between 0 and 3. All regressions include state and time fixed effects. Standard errors are clustered on state level. Values of t-statistics are in parentheses, where *** indicates significance at 1% level, ** indicates significance at 5% level and * indicates significance at 10% level.

	(1)	(2)	(3)	(4)
VARIABLES	Entry rate	Entry rate	Exit rate	Exit rate
Intrastate deregulation	0.05		-0.29	
	(0.226)		(-1.363)	
Deregulation Index		0.15		-0.02
		(1.069)		(-0.145)
Observations	1,122	1,122	1,122	1,122
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
R-Squared	0.784	0.785	0.715	0.713

Table 9: The Impact of Reforms on Employment Dynamics

Each column in this table reports statistics from a regression similar to Equation 1, where the dependent variable is the rate of change of the column's label. The data is from Business Dynamic Statistics, includes 50 states plus District of Columbia and is from 1976 to 1997. Intrastate deregulation indicator equals one during all years in which a state permits intrastate branching via M&A. Deregulation index is an indicator which equals the number of reforms a state allows in each year and varies between 0 and 3. Creation birth is the job creation rate of entrant firms, while creation expand stands for job creation rate of existing firms. Similarly, destruction death is the rate of job destruction of firms that disappear, while destruction contraction stands for the rate of job destruction of coming from contractions. All regressions include state and time fixed effects. Standard errors are clustered on state level. Values of t-statistics are in parentheses, where *** indicates significance at 1% level, ** indicates significance at 5% level and * indicates significance at 10% level.

Panel A: Intrastate deregulation

VARIABLES	(1) Job creation	(2) Creation birth	(3) Creation expand	(4) Job destruction	(5) Destruction death	(6) Destruction contraction
Intrastate deregulation	0.65**	0.10	0.56***	-0.18	-0.13	-0.05
	(2.277)	(0.563)	(3.237)	(-0.440)	(-0.792)	(-0.172)
Observations	1,122	1,122	1,122	1,122	1,122	1,122
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.675	0.693	0.520	0.538	0.597	0.401

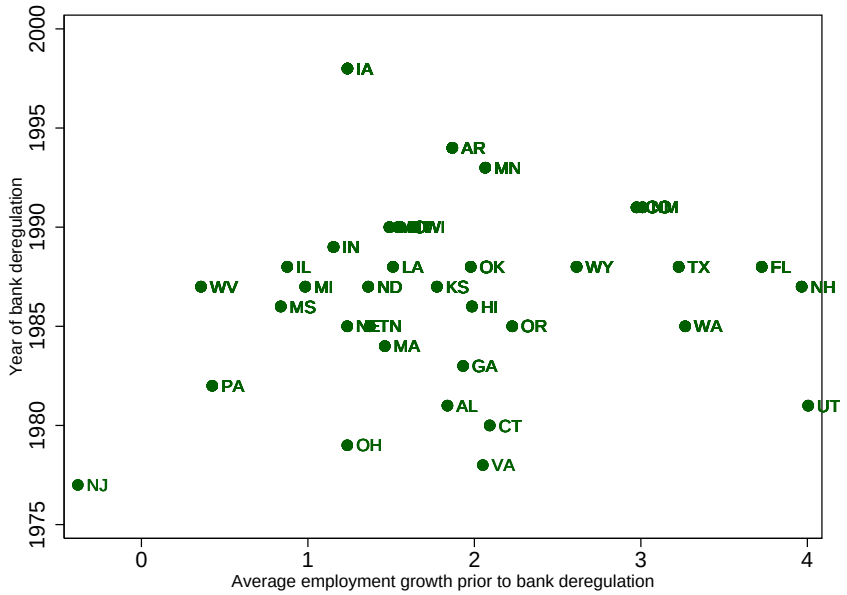
Panel B: Deregulation Index

VARIABLES	(1) Job creation	(2) Creation birth	(3) Creation expand	(4) Job destruction	(5) Destruction death	(6) Destruction contraction
Deregulation Index	0.45***	0.12	0.33***	0.10	0.05	0.05
	(2.879)	(1.296)	(3.442)	(0.487)	(0.557)	(0.357)
Observations	1,122	1,122	1,122	1,122	1,122	1,122
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.677	0.694	0.521	0.538	0.597	0.401

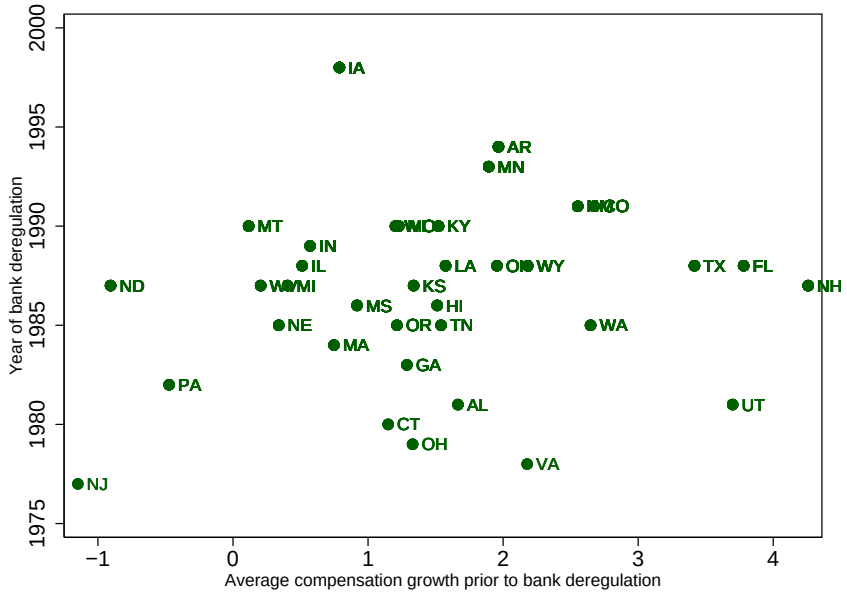
Table 10: Reform and Employment Growth: The Direct Channel

This table investigates the direct impact of deregulation on firms' employment. The dependent variable in all regressions is employment growth. The data for the first three columns includes total manufacturing in 50 states plus Districts of Columbia and from 1976 to 1997. Employment data is from the Bureau of Economic Analysis, while investment data is from Chirinko and Wilson (2009). The sample used in regressions of the last three columns is at industry level, includes 32 industries, and is from 1976 to 1997. Data section explains details on the construction of *labor intensity*. Intrastate deregulation indicator equals one during all years in which a state permits intrastate branching via M&A. Standard errors are clustered at state level. Values of t-statistics are in parentheses, where *** indicates significance at 1% level, ** indicates significance at 5% level and * indicates significance at 10% level.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Employment growth					
Intrastate	0.952**	0.941**	0.904**			
	(2.175)	(2.137)	(2.014)			
Investment growth		0.008				
		(1.558)				
Intrastate x Investment growth			0.006			
			(0.841)			
Intrastate x Labor intensity				2.000**	1.913**	1.872*
				(2.469)	(2.325)	(1.708)
Observations	1,122	1,100	1,100	32,156	32,156	32,156
R-squared	0.541	0.556	0.553	0.221	0.322	0.388
Sample	Manuf.	Manuf.	Manuf.	All ind.	All ind.	All ind.
State FE	Yes	Yes	Yes	Yes	No	No
Industry FE	No	No	No	Yes	Yes	No
Time FE	Yes	Yes	Yes	Yes	No	No
State x Time	No	No	No	No	Yes	Yes
Industry x Time	No	No	No	No	No	Yes



(a)



(b)

Figure 1: Timing of Reform and Pre-existing Labor Market Outcomes: Graphical Analysis

Figure (a) shows a scatter plot of the average employment growth prior to intrastate branching reform and the year of reform. Figure (b) shows a scatter plot of the average growth of aggregate wage bill prior to intrastate branching reform and the year of reform. In constructing averages, I require to have at least two data points. The t-statistics for the correlations in Figure (a) and (b) are -0.99 and 0.26, respectively.

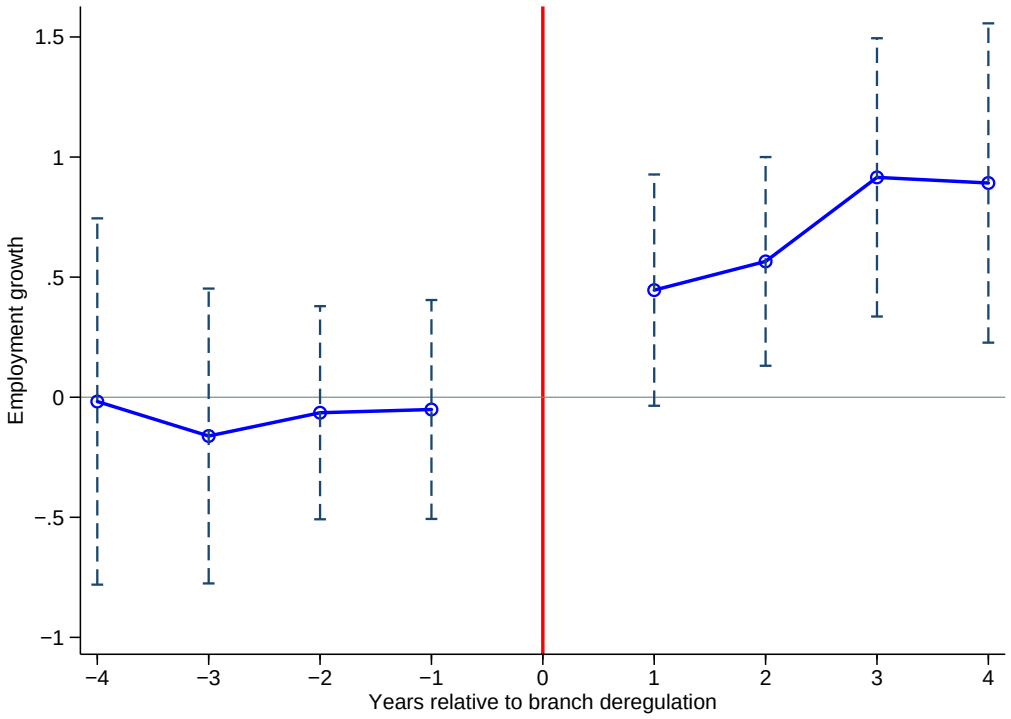


Figure 2: The Dynamic Impact of Reform on Employment

This figure shows the dynamic impact of bank reform on employment growth. I consider a 8-year window, spanning from 4 years before deregulation until 4 years after deregulation. The dashed lines represent 95% confidence intervals, adjusted for state-level clustering. Specifically, I report estimated coefficients from the following regression:

$$\text{Employment growth}_{st} = \alpha + \beta_1 D_{st}^{-4} + \beta_2 D_{st}^{-3} + ... + \beta_4 D_{st}^{+4} + \mathbf{A_s} + \mathbf{B_t} + \varepsilon_{st}.$$

The D's equal zero, except as follows: D^{-j} equals one for states in the jth year before deregulation, while D^{+j} equals one for states in the jth year after deregulation. I exclude the year of deregulation, thus estimating the dynamic effect of deregulation on employment growth relative to the year of reform. A_s and B_t are vectors of state and year dummy variables that account for state and year fixed effects, respectively.