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Effects of U.S. Financial Regulations on Labor Markets

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EFFECTS OF U.S. FINANCIAL REGULATIONS ON LABOR MARKETS

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Dedicated to Sanjoy K. Sarker and Krishna D. Sarker

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Table of Contents

Abstract

This thesis is based on a collection of three essays which study the effects of financial policy on labor markets. The first two essays investigate the effects of U.S. bank branching deregulation on labor markets. The first essay studies how these regulations impacted wages and working hours of traditional payroll workers. The second essay studies the impact of these policy reforms on the occupational choice to engage in self-employed work. The main finding of this set of studies is credit access has real effects on the workforce. Estimates from the econometric analysis used in this study suggest that credit access that stemmed from various policy changes lead to increases in entry and exit rates among self-employed workers, especially among those who started incorporated businesses. Furthermore, these effects are stronger among groups that typically face tougher credit constraints such as younger workers, minorities. The results of this study also show that increases in credit, stemming from policy reforms, lead to increases in the annual working hours and growth rate of the real wage rate for private sector workers. Further analysis suggests that increases in the annual labor supply response is higher for workers with lower earning rates, which lead to decreases in income inequality in the absence of a decline in earnings inequality. The third essay studies the effects of bankruptcy reforms on self-employed workers classified by their needs for external finance. We find that after a national tightening of the bankruptcy code, there is a decline in the rates of entry among unincorporated self-employed workers in states which offer a lower degree of bankruptcy protection.

Chapter 1. Introduction

This dissertation consists of three distinct works that lie at the intersection of financial economics and labor economics. The purpose of this work is to investigate a better understanding of how changes in credit constraints stemming from financial policy reforms impact the workforce and small business entrepreneurship. The first two essays examine the impact of U.S. state-level banking deregulation. The third essay examines how changes in the personal bankruptcy code, through federal legislation, impacts entrepreneurship.

For much of the 20th century, U.S. laws prohibited or restricted the geographic expansion of commercial banks. Federal laws gave U.S. states a broad authority to regulate branching and bank mergers within their border. Advances in technology and finance fundamentally changed the role of banks in the economy, and thus from the 1970s through the 1990s, states removed these prohibitions on banking sector. Banking deregulation reduced banking costs and increased efficiency, which in turn increased local credit supply (Krozner and Strahan 2014).

Chapter 2 of this dissertation exploits the staggered-in-time nature of these policy reforms to conduct a differences-in-differences analysis. We study the banking deregulation on the wages and labor input responses of U.S. private sector workers. From 1970s through the 1990s, states elected to remove prohibitions on branching, which resulted in large structural changes on U.S. financial markets and increased local credit supply. We show that interstate banking deregulation led to increases in the growth rate of the real hourly wages, annual labor incomes and annual labor input responses, as measured by annual hours and weeks. We show that policy reforms lead to declines annual labor income inequality and not wage rate inequality. These findings suggest that the declines in income inequality due to banking deregulation came from increases in the quantity of the labor input.

Chapter 3 uses state-level bank branch deregulation to study the impact of changes

in credit on entrepreneurship at the individual-owner level.[1] Self-employed individuals are classified into incorporated and unincorporated business owners. Exploiting the variation in the staggered timing of banking deregulation, the analysis yields that branching reforms affected the entry and exit rates of the incorporated self-employed. Further, the branching reforms encouraged unincorporated businesses to incorporate. Finally, the effects of reforms are different across groups based on gender, race, and age. We find stronger effects on incorporated business creation among minorities, and higher exit rates among the young and minorities.

In the last chapter, we study how a major change in U.S Bankruptcy policy, the Bankruptcy Abuse and Consumer Protection Act of 2005 (BAPCPA), affects individual-owner entrepreneurship. This reform reduced the ability of borrowers to file for chapter 7 bankruptcy, a procedure which allows borrowers to default on debts in exchange for assets above an exemption level. We classify the self-employed into SIC industry categories and match each industry category with a common measure of external finance. We find that this policy reduced entry into unincorporated entrepreneurship. This effect is stronger in states which offer the low home-equity exemptions. Furthermore, in states with high levels of home-stead exemption, the effect is directly linked to a particular industries need for external finance. This suggests that a reduction in risk sharing between borrowers and lenders, due to the passage of BAPCA, lead to a lower level of entrepreneurship.

[1]This chapter mainly draws from a joint paper co-authored with my advisor Prof. Unel (see Sarker and Unel 2017).

Chapter 2. Impact of U.S. Bank Expansion on Labor Markets

2.1 Introduction

Through a large part of the twentieth century, U.S national and state-level financial policies heavily restricted the ability of commercial banks to expand geographically. For example, prior to 1970 no state allowed a bank chartered in another state to acquire banks or open branches within its border. Most states prohibited banks chartered within the state to acquire or open new branches. Beginning 1970s through 1990s, many U.S. states elected to lift policy restrictions prohibiting bank mergers and branching, which had transformative effects on the U.S financial sector. By the late 1990s, the banking sector had heavily integrated with 35.5 percent of the nation's assets held by the eight largest banks, and the number of distinct banking organizations fell by 30 percent from 1988 to 1997 (Berger et al. 1999).

Since these reforms constitute a major episode of financial liberalization, a large body of research has been carried out to study their impact on economic activity. A number of studies have documented that banking deregulation affected the availability of credit by increasing competition and improving efficiency from integration (Stiroh and Strahan 2003, Jayaratne and Strahan 1998). Consequently, banking reforms led to increases in local credit supply (Strahan 2003) and improvements in the allocation of credit (Dick and Lehnart 2010).

In this chapter, we study the effects of a positive financial shock on the labor market. Specifically, using U.S. census data, we exploit the time variation from the staggered nature of these policy changes to explore the causal impact of deregulation on hourly wage rates, real incomes and labor input responses of U.S. private sector wage and salary workers over the period 1976–2006. First, we provide evidence that banking reforms lead to an increase in the growth rate of real hourly wages and annual labor incomes. we show that these increases stem from workers who are in the top and the middle of the wage distribution. Second, we provide estimates that suggest deregulation lead to a robust decline in annual labor income

inequality, but not in wage rate inequality.

Third, we show that this reduction in labor income inequality stems from the time that workers spend working. we find that interstate banking deregulation has led to an increase in the average number of hours individuals worked. Estimates suggest that workers worked an average of 25 hours longer per year in states where interstate banking reforms have been instituted, and this increase mainly stems from the increase in the average number of weeks worked. We show that the additional hours worked is concentrated among those who are in the bottom and middle earning quartiles.

The availability of credit is rarely an exogenous event, as it is affected by many confounding factors such individual borrower characteristics, past and future macroeconomic states, monetary policy, asset prices and beliefs. Furthermore, these factors are also influenced by the availability of credit, which introduces reverse causality problems. For this reason researchers often employ economic models with strong structural assumptions in order to understand the economic effects credit shocks. Because banking reforms were instituted in different states at different times, which are determined by political climate and lobbying activities of big banks (Krozner and Strahan 2014), these policy changes are often used to empirically assess the causal impact of a change in credit on the real economy.

The main advantage of the design used in this chapter is that the timing of banking deregulation is unlikely to be related to individual level characteristics that influence wealth, credit worthiness and earnings. However, this does not circumvent that other macroeconomic factors could influence the timing of deregulation. In order to address this issue, we conduct event studies in order to understand the dynamics before and after deregulation. In the cases where potential existing trends can explain the timing of deregulation, we present two estimates: one estimate that presumes the trend would have continued in the absence of banking deregulation, and one which assumes there are no trends once state-specific trends are controlled for. This creates a plausible boundary for the true casual impact of deregulation. In this manner, the analysis here identifies the causal impact of deregulation,

while making only a limited number of structural assumptions.

This chapter contributes to a large and growing literature that has investigated the impact of U.S. bank deregulations on various economic outcomes such as growth and productivity (Jayarante and Strahan 1996, Jerzmanowski 2017), entrepreneurship (Black and Strahan 2002, Kerr and Nanda 2009, Sarker and Unel 2017), economic volatility (Morgan et al. 2004, Krishnamurthy 2013), inequality (Black and Strahan 2001, Beck et al. 2010), mortgage loans and homeownership (Tewari 2014), labor's share of income (Leblebicioglu and Weinberger 2018), among many others. Full coverage of this literature is beyond the scope of this chapter. Our paper relates closely to Beck et al. (2010) and Dao and Ume (2016), who investigate the impact of deregulation on different aspects of labor markets.

Beck et al. (2010) show that banking deregulation reduced income inequality by increasing incomes in the lower part of the income distribution while having negligible impact on incomes above the median. This chapter complements theirs in two key aspects. First, we focus on how deregulation affected the labor input and wage rates (rather than annual total income) across different wage groups. Second, our analysis suggests that banking deregulation may have caused a decline in income inequality through an increase in the quantity of the labor input supplied by workers in lower earnings groups, rather than declines in hourly wage inequality.

Bui and Ume (2016) find that workers report a small decline in the number of weekly hours they worked after the introduction of intrastate deregulation. This effect is driven predominantly by lower-middle income workers. These authors focus on the labor input response of hours worked one week prior to the census survey. we consider a broader set of labor input measures (e.g., weeks and usual hours worked last year). In addition, our analysis predominantly focuses the effects of deregulation on the growth of hourly wage rates, and incomes. Finally, we examine the effects of both intrastate and interstate reforms, while they only consider intrastate reforms. we find that when both reforms are considered, interstate banking reforms rather than intrastate reforms drive labor input responses.

The rest of the chapter is organized as follows. The next section provides further background on banking deregulation in the US. Section 2.3 describes the data and its construction. Section 2.4 describes the details of the econometric methodology of this study. Section 2.5 presents the results. Section 2.6 concludes.

2.2 Background on Banking Deregulations

Until early 1970s, the creation and expansion of commercial banks were heavily restricted in the U.S. The McFadden Act, which was passed in 1927, gave states the ability to regulate creation of bank branches within their state borders. The Bank Holding Act of 1956 barred bank holding companies from acquiring banks across state lines unless the target state had a law explicitly allowing interstate acquisitions. Bank expansions largely occurred through multi-bank holding companies (MBHC) which operated individual commercial bank branches as separate institutions. Strong branching regulations were maintained in part because they led to the creation of local bank monopolies who lobbied to maintain restrictions. Furthermore, monopolistic banking charters were an important source of tax revenue (Kroszner and Strahan, 1999). In the 1970s, the introduction of technologies such as the ATM, as well as the creation of chequable money market funds lead to a decline in the relative importance of relationship lending among banks. Expansion minded banks and special interest groups lobbied for the removal of banking restrictions (Krozner and Strahan 2014). Deregulation involved three types of reforms. The first reform allowed multi-bank holding companies (MHBC) chartered in a state to merge or acquire existing branches. MHBCs could consolidate their existing branches into a single network and purchase branches from other MHBCs. The second type of reform, allowed 'De novo' branching, which allowed MHBCs to open new branches within their charter state. The third reform allowed out of state banks to purchase branches within a states borders.

Prior to 1980, the only state to allow interstate banking was Maine. By 1993, every state except Hawaii allowed some degree of interstate banking. In 1994 the U.S federal govern-

ment passed Riegle-Neal Interstate Banking and Branching Efficiency Act (IBBEA). This legislation eliminated the Douglas Amendment from the Bankholding Act of 1956 and the Mcfadden Act, and allowed well capitalized banks to acquire branches in any U.S. state. In most states 'de novo' branching deregulation was implemented soon after, or simultaneously branching by M&A was also allowed, and thus it is hard to estimate the impact of these two deregulations separately. Following other researchers, we use the year a state allowed branching by M&A.

As discussed in the previous section, branching deregulation led to profound structural changes in the banking sector. Banking deregulation led to a rapid period of bank consolidation, with a larger share of assets being held by larger multi-state banks. Despite this rapid consolidation of the banking sector, deregulation decreased local market concentrations through a rapid expansion of new bank branches. Deregulation allowed new banks to open branches in previously restricted markets as well (Strahan, 2003). Branch deregulation increased efficiency in the banking sector by reducing costs and increasing the market share of more efficient banks (Stiroh and Strahan, 2003). Jayaratne and Strahan (1998) have found that non-interest costs such as salaries and loan losses fell after deregulation.

Deregulation of branch expansion increased the availability of credit. The period following the deregulation of both intrastate and interstate mergers saw increases in loan origination, credit cards, chapter 7 personal bankruptcies, and a reduction in bank charge-offs (Dick and Lehnert, 2010). Furthermore, empirical studies such as Tewari (2014) and Favara and Imbs (2015) show, by exploiting the Home Mortgage Disclosure Act which requires that financial institutions make mortgage data public, that increased lending occurred solely in commercial banks affected by deregulation. Favara and Imbs further show that the degree to which states deregulated interstate banking after the passage of the IBBEA, expanded the availability of the number of mortgage loans, dollar amounts of mortgage lending and value of mortgages.

2.3 Data

The main source of labor market data comes from the March Current Population Surveys (CPS) from 1977 to 2007 (covering earnings from 1976 to 2006). Each CPS file provides annual estimates based on a survey of more than 60,000 households. We use the data complied by Flood et al. (2018), which are publicly available at Integrated Public Use Micro Samples (IPUMS) website. The sample is a repeated cross section, and does not trace household over time. Each observation is assigned a probability weight reflecting how closely it represents households within the population. We use these sampling weights throughout all of our analysis. The survey contains a broad range of information about income, occupation, industry, employment, education and demographics. For this analysis, we use variables which report information about income and time spent working in the year prior to the survey date. For the primary analysis, we consider only employed private sector workers between the ages of 18 and 64. We exclude both the self-employed and the public sector workers.

The variables used from these surveys are total number of weeks worked, usual number of hours worked per week, annual earnings from wages and salaries. Our analysis excludes all observations with missing and allocated values. In constructing wage series, we also correct top-coded earners following Autor et al. (2008) and Unel (2010). We adjust earnings for inflation by using the Personal Consumption Index (PCE) from the Bureau of Economic Analysis (BEA). The real hourly wage rate for a given year is calculated as the total income from wage and salaries divided by total annual hours worked. We drop from the sample workers with hourly earnings considerably below the real minimum wage.

Table 2.1 Summary Statistics about the CPS-March, 1976–2007

	Private Sector Workers, 18-64 1	All Full-time Prime Age Workers 2
Age	37.3	38.2
Male (%) .	52.9	52.7
Female (%)	47.1	47.3
White (%)	86.9	85.7
Black (%)	8.2	9.0
College Graduate (%)	22.3	28.5
Manufacturing (%)	22.5	21.5
Retail and Services (%)	39.1	41.3
Observations	1,437,324	1,445,579

Notes: Column 1 presents summary statistics about primary sample; Private sector U.S. workers between ages 18 and 64. Public sector and Self-employed are excluded. Column 2 presents summary statistic on appendix sample. Sample consists of public sector and private sector workers between the ages of 25 and 54, who worked at least 35 hours a week and 40 weeks a year.

Table 2.1 reports summary statistics about key variables for wage and salary workers. Column 1 reports information about the primary sample used in this status, the employed private sector workers. It contains about 1.5 million observations. The average age is 37, and males constitute about 53 percent of the sample. Around 86 percent of the sample are white and 20 percent are college educated. Column 2 reports summary statistics about prime-aged workers. A major difference between the two samples is that a considerably larger group of the prime-aged workers is college educated, reflecting that prime aged sample is concentrated among workers who are above the traditional college age.

Information on Banking Deregulation is taken from Amel (2008). As noted by Strahan and Black (2002), banking activities in Delaware and South Dakota are skewed by the presence of credit card banks. Therefore, our analysis covers 48 states over the 1976-2006 period.

2.4 Econometric Model

This study uses a difference-in-differences (DD) framework to examine the effects of intrastate branching and interstate banking on the labor market. We make use of the fact that states deregulated banking at different times in order to estimate the impact of banking policies on labor market variables. The main specification used in this study is

$$Y_{ist} = \alpha \text{Intra}_{st} + \beta \text{Inter}_{st} + \gamma X_{ist} + \eta_s + \eta_t + \eta_s t + \varepsilon_{ist}, \tag{2.1}$$

where Intra_{st} (Inter_{st}) are dummy variables which identify if intrastate (interstate) banking deregulation is in effect in state s and year t. Each dummy variable equals zero up to the year of deregulation and one afterward. The main coefficients of interest are α and β, which denote the marginal impact of these two types of deregulation. The dependent variable Y_{ist} for an individual i working in state s in year t could be:

- the log of real hourly earnings from employment
- the log of weekly earnings from employment
- the log of real annual earnings from employment
- reported number of weeks spent working over an year.
- usual number of hours per week spent working at all jobs
- total annual hours

In most specifications, We include X_{ist}, a vector of individual level controls for race (black and other dummies), gender, age and age square.[1] The variable η_s denotes state fixed effects to control for any state-specific factors. Year fixed effects are included via η_t to control for

[1]We do not include variables such as unemployment rate, corporate tax rate, real personal income, growth rate of gross state product, education and experience etc. in our controls. These are potential outcome variables, and thus considered bad controls (Angrist and Pischke 2009). However, including them in our specification does not have a significant impact on our estimates (results are available upon request).

common macroeconomic shocks. State-specific time trends ($\eta_s t$) are included to account for other trending factors. All regressions are weighted by the CPS individual-level weights. Regressions use heteroskedasticity robust standard errors clustered at the state-level.

A key identification assumption of the DD research design is an assumption of no pre-treatment trends. In this study, we include state-specific time trends in order to control for this problem. However, we also conduct the following dynamic analysis that allows us to understand the impact of deregulation over time,

$$Y_{ist} = \sum_{\tau=-10}^{10} \alpha_\tau \text{Intra}_{st}^{\tau} + \sum_{\tau=-10}^{10} \beta_\tau \text{Inter}_{st}^{\tau} + \gamma X_{ist} + \eta_s + \eta_t + \varepsilon_{ist}, \tag{2.2}$$

where we extend the main regression equation to include a set of twenty dummies for each type of deregulation. Each dummy takes a value of one the τth year before or after deregulation, and is zero otherwise. The end points take a value of one for all earlier and later years. We exclude the year of deregulation. This allows us to explore the dynamic effects of deregulation and provides a check of 'Granger' causality to see if the dependent variable predicts deregulation. Here, if α_τ and β_τ are not statistically different from zero for $\tau < 0$, then the likelihood that pre-treatment trends are present is reduced.

2.5 Results

This section reports the results of the empirical exercises. The effects of intrastate and interstate banking deregulation on the wages are discussed in Section 5.1. The next subsection investigates the impact of deregulation on wage inequality. Finally, how banking reforms have affected labor input (measured by the number of weeks worked, usual weekly hours, and total annual hours) is discussed in Section 5.3.

2.5.1 Effects of Bank Deregulation on Wages

We first present results obtained from event studies to see whether there are any pre-treatment effects. Figures 2.1 and 2.2 show the plots of the lag and lead coefficients estimates

from equation (2.2) and their corresponding confidence interval, which allows us to visualize the dynamic effects on the policy on the log of hourly wages. Each point on the dotted line depicts the coefficient estimate and the vertical lines show the corresponding 95% confidence interval. The dashed line represents a counter-factual line which is fitted to the points prior to the policy change, this provides a visual interpretation for the impact of the policy change. The counter-factual line represents the change in the outcome variable, if the pre-treatment trend were to continue.

Figure 2.1, which corresponds to intrastate deregulation, shows a gradual decline in the growth of real wages in the years prior to deregulation, but none of these coefficients is statistically significant. However, the pattern of the data does not fully reject the possibility that states which were experiencing a fall in earnings growth deregulated the banking sector anticipating better growth prospects. This is often referred to in the labor literature as an Ashenfelter dip, a common problem in program evaluation studies.[2] Notice that the coefficients after intrastate deregulation are also not statistically significant, and that the counter-factual line runs through the confidence interval for at 5 years after deregulation. This suggests that intrastate deregulation did not have much of an effect on the wage rate. While the counter-factual suggests that intrastate deregulation may have caused wages to rise 5 years after the policy implemented, by this point most states had deregulate interstate deregulation which makes it hard to conclude that this effect is the result of intrastate branching policies.

Figure 2.2, corresponds to interstate deregulation, the pattern is similar to the previous figure. The coefficients prior to deregulation are mostly insignificant statistically, however there exists a negative pattern in the data. This suggests that on average the growth rate of wages is falling prior to deregulation and has become negative prior to the policy change, in a potentially confounding manner. If one considers the pre-treatment estimates to not

[2]The classic example of a Ashenfelter's dip comes from difficulties in evaluating the effects job training problems. Workers entering into a jobs training program experience a dip in earnings prior to starting program. This introduces a self-selection bias, as the dip in earnings may be their reason for entering the program. This undermines an assumption of random assignment.

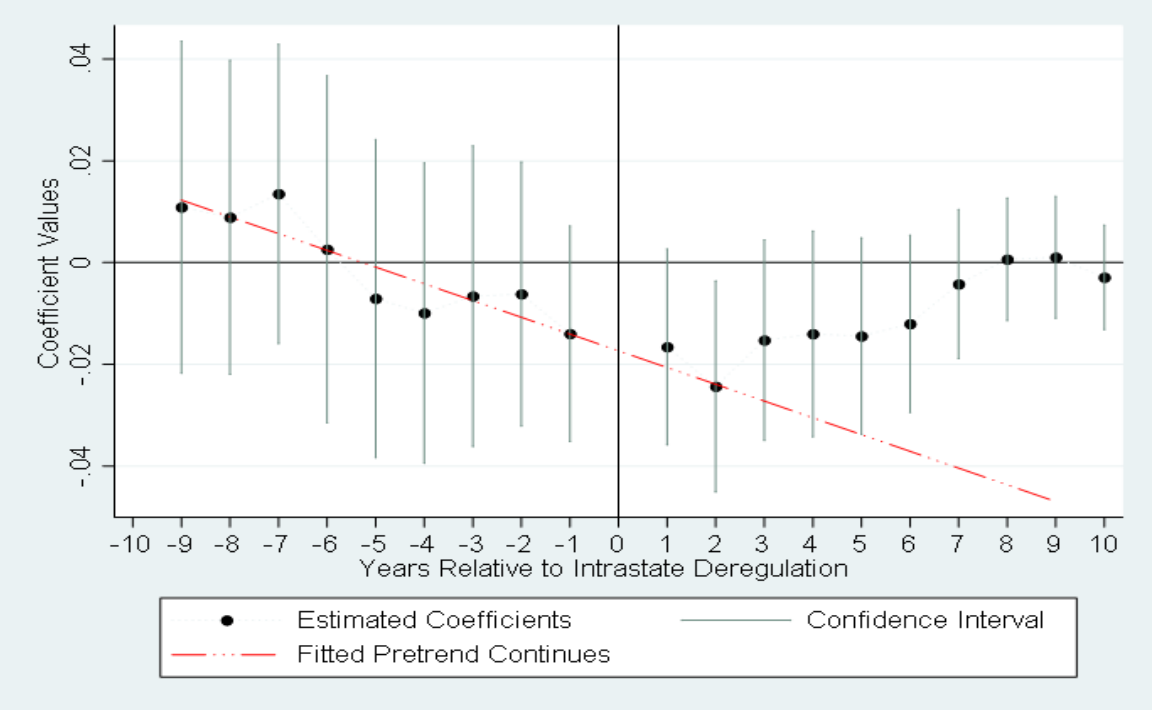


Figure 2.1: Change in Log Wages,Years Before or After Intrastate Bank Deregulation

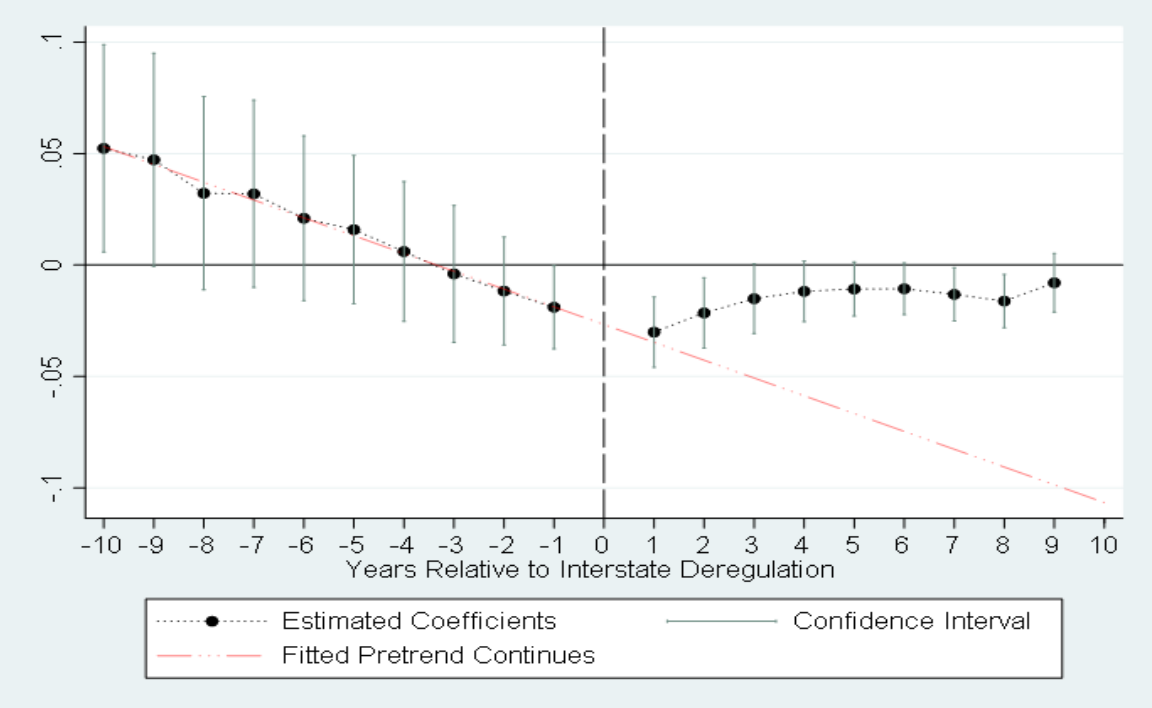


Figure 2.2: Change in Log Wages,Years Before or After Interstate Bank Deregulation

be statistically different from 0 this suggests that banking deregulation caused a persistent negative decline in the wages. However, the counter-factual line, which plots the scenario where pre-existing trend continues, suggests that banking deregulation had a positive impact on the growth of real wages.

In order to account for the potential trending factors in figures 2.1 and 2.2, we include state-specific time-trends into the main differences-in-differences (DD) specification presented in equation (2.1), which is often used to control for mild trending factors. We also consider a second specification which exploits the approximately linear nature of the pre-treatment trend. The key idea is to compare the slopes between the pre and post-trends. This requires making an assumption that pre-treatment trend would continue in the post-period in the absence of the policy change. The framework is similar to the approach in Willage (2018) who uses changes in health insurance rates stemming from the affordable health care act to study ex-ante moral hazard.

We extend equation (2.1) in the following fashion:

$$\begin{aligned} Y_{ist} =& \alpha_1 \text{Inter-Trend}_{st} + \beta_1 \text{Intra}_{st} + \beta_2 \text{Intra Post-trend}_{st} + \\ & \delta_1 \text{Inter}_{st} + \delta_2 \text{Inter Post-trend}_{st} + \gamma X_{ist} + \eta_s + \eta_t + \varepsilon_{ist}, \end{aligned} \tag{2.3}$$

where Inter-Trend_{st} accounts for the counter-factual trend. It takes the value of the number of years before or after a state deregulated, where the before values are negative. Note that the variable is also equal to the difference between the observation year and the year of deregulation, thus specification does not admit for state specific time trends. The $\text{Inter Post-trend}_{st}$ ($\text{Intra Post-trend}_{st}$) variable is the number of years after interstate (intrastate) deregulation and 0 for all years prior to the deregulation. The other variables are defined as in equation (2.1). Here, our parameters of interest are δ_1, δ_2. Where δ_1 corresponds to the immediate impact from interstate deregulation and δ_2 corresponds to the trend.

Notice that the standard DD model is equivalent to equation (2.3) if α, β_2 , and δ_2 are set equal to zero. This implies that the standard DD model is a special case of this model

Table 2.2 Impact of Deregulation on Log of Hourly Earnings, 1976–2006

	DD 1	DD Slope 2	DD Slope 3	DD Slope 4
Intra	−0.0058	0.0054		0.0047
	(0.0104)	(0.0140)		(0.0122)
Inter	0.0106		0.0031	0.0027
	(0.0077)		(0.00950)	(0.0098)
Intra Trend		0.0031**		−0.0045*
		(0.0015)		(0.0026)
Intra Post-Trend		.00373*		0.0012
		(0.0019)		(0.0019)
Inter Trend			−0.0039	
			(0.0024)	
Inter Post-Trend			0.0142***	0.0134***
			(0.0036)	(0.0041)
Time Trends	Yes	No	No	No
Observations	1,437,324	1,437,324	1,437,324	1,437,324

Notes: Numbers in parentheses are robust standard errors clustered at the state level, and ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively. Controls are not reported for brevity.

where the slope of pre-treatment trends are zero and there is no time-varying treatment effect. The estimated effect of deregulation from derived from equation (2.1) and equation (2.3) are the impact of the policy change for the two different scenarios presented in figure 2.2. One which presumes that treatment effects are not statistically different from zero, the other which presumes the linear trend in the figures are present.

Table 2.2 reports the impact of banking policy reforms on log hourly real wages based on the standard DD model specified in equation (2.1) and the slope change specification in equation (2.3). Through out this chapter, all regressions include controls, state and year fixed effects and use sampling weights. For brevity, we do not report the impact of controls. Column 1 presents estimates from equation (2.1) and includes state-specific time trends. The estimates are not statistically significant, indicating neither intrastate or interstate caused

a growth in the real wage rate. However, it is worth noting that the value of the coefficient from interstate banking is positive and much larger than the effect of intrastate. In column 2, we present a slope change model using intrastate deregulation as the treatment variable. The estimate associated with the intrastate dummy is not statistically significant, and suggests that there is no immediate effect from the passage of intrastate deregulation. The estimate associated with the post-trend (in row 4) suggests that there may have been a small impact on wage growth over time, but this is only significant at the 10% level. Consistent with the first figure, there seems to be no real change in the wage rate due to intrastate deregulation.

Column 3 presents the estimates of equation (2.3) when considering interstate reforms. The estimates suggest there is no immediate impact from interstate banking on the log of the wage rate, however there is a statistically significant increase on the growth of the wage rate over time due to reforms. Column 4 presents estimates from a specification of equation (2.3) that considers both possible policies. Note that the specification only allows for the correction of either a intrastate policy trend or an interstate policy trend, but not both. However, this does not effect the core result, which is consistent with columns 2 and 3, intrastate reforms had no real impact on hourly wages, while interstate reforms lead to an increase in the growth of real wages over time. The findings in this table suggest that if banking deregulation had any effect on hourly wages it is positive.

Table 2.3 reports the effects of deregulation on the log of weekly wages and log of annual wages. We have not presented event study figures for these two variables, their dynamics behave similarly to the effect of deregulation on the hourly wages depicted in figures 2.1 and 2.2. Column 1 shows the estimates from the standard DD model, the findings suggests that interstate banking lead to a 3.5 % increase in annual earnings, the finding is statistically significant at the 5% level. These estimates are consistent with column 2, which presents the estimates from equation (2.3). Here we see that both deregulation lead to increases the growth of annual wages over time, though the effects are smaller and not as significant for intrastate deregulation. When considering log of weekly wages, the standard DD model

Table 2.3 Impact of Deregulation on Log of Annual and Weekly Earnings, 1976–2006

	Log Annual Wages		Log Weekly Wages	
	DD	DD Slope	DD	DD Slope
Variable	1	2	3	4
Intra	−0.0047	−0.0013	−0.0050	−0.0021
	(0.0155)	(0.0142)	(0.0114)	(0.0089)
Inter	0.0355**	0.0193	0.0163*	0.0011
	(0.0134)	(0.0144)	(0.0082)	(0.00891)
Inter Trend		−0.0060*		−0.0093***
		(0.00348)		(0.00233)
Intra Post-Trend		0.0056**		0.0043**
		(0.0024)		(0.0017)
Inter Post-Trend		0.0224***		0.0216***
		(0.0052)		(0.0037)
Time Trends	Yes	No	Yes	No
Observations	1,437,324	1,437,324	1,385,148	1,385,148

Notes: Numbers in parentheses are robust standard errors clustered at the state level, and ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively. Controls are not reported for brevity.

estimates (column 3) shows a positive impact from interstate deregulation. However, this is only significant at the 10% level. In column 4 estimates from the slope change model shows an increase in the growth of weekly earnings over time.

Table 2.4 presents estimates of equation (2.3) on the log of hourly earnings across different earnings groups. We divide the sample observations into each of the following hourly earnings groups: the bottom quartile, the middle 25%–75%, and the top quartiles. Regressions reported here include controls, state fixed effects and an interstate banking time-trend. The estimates show that the interstate banking deregulation had an impact on the growthof hourly earnings, predominantly for workers which are in the top and the middle of the income distribution. For brevity, we do not report the results from equation (2.1), however, the finding is consistent with Table 2.2. DD estimates are positive across all income groups, but not statistically significant.

Table 2.4 Impact of Deregulation on Log of Hourly Earnings, by Quartile

Variable	Bottom Quartile 1	Middle 25th - 75th 2	Top Quartile 3
Inter Trend	−0.0003	−0.0041	0.0004
	(0.0027)	(0.0031)	(.0025)
Intra	0.0151	0.0092	−0.0061
	(0.0142)	(0.015)	(0.0097)
Inter	0.0076	−0.0036	−0.0090
	(0.0123)	(0.0119)	(0.0078)
Intra Post-Trend	−0.0021	0.00038	0.0029
	(0.0019)	(0.00220)	(0.0020)
Inter Post-Trend	0.0083*	0.0154***	0.0135***
	(0.0043)	(0.0049)	(0.0039)
Observations	354,756	709,257	354,867

Notes: Numbers in parentheses are robust standard errors clustered at the state level, and ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively. Controls are not reported for brevity.

2.5.2 Bank Deregulation and Wage Inequality

In this section we assess the impact of banking deregulation on income and wage inequality. We analyze the effects of deregulation on the difference between the natural log of earnings for those who are high earners and for those who are low-earners. For every year in the sample we calculate the incomes in each state which are at the 10th, 25th, 75th and 90th percentile. We then calculate earning ratios for the 75th percentile to the 25th percentile (75-25 ratio) and 90th percentile to the 10th percentile (90-10 ratio) separately for hourly wages or incomes. We then estimate equation (2.1) using the natural logs of these variables as the response variable. The results of this exercise is reported in Table 2.5.

Columns 1 and 2 report the effects of deregulation on income inequality and show that income inequality fell after deregulation. This finding is only statistically significant for interstate banking, suggesting that most of decline in income inequality are a result of

Table 2.5 Impact of Deregulation on Income and Wage Inequality, 1976–2006

	Income		Wage	
	Log 90/10	Log 75/25	Log 90/10	Log 75/25
Variable	1	2	3	4
Intra	0.0129	−0.0062	0.0125	0.0001
	(0.0229)	(0.0143)	(0.0093)	(0.00715)
Inter	−0.0511**	−0.0426***	−0.0169*	−0.0117
	(0.0208)	(0.0119)	(0.0098)	(0.00909)
Observations	1,488	1,488	1,488	1,488

Notes: All regressions include controls, state fixed effects, year fixed effects, and state-specific time trends. Numbers in parentheses are robust standard errors clustered at the state level, and ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively. Controls are not reported for brevity.

interstate banking reforms and not intrastate reforms. Column 3 and Column 4 report the effects of deregulation on wage inequality. Column 3 suggests that interstate deregulation may have led to a small decline in wage inequality, but the estimate is statistically significant at the 10% level. Column 4, reports findings for the log of 75-25 ratio of wages, and the effects are not significant for either intrastate or interstate reforms.

The exercises here is similar to Beck, Levine and Levkov (2010), who show that intrastate banking deregulation lead to tightening of the overall income distribution.[3] However, as we will show in the next section, our analysis suggests that this tightening of the distribution did not come from declines in wage inequality and are partially driven by increases in the amount of time spent working.

2.5.3 Effects on Time Spent working

We first present results obtained from event studies to see whether there are any pre-treatment effects. Figures 2.3 and 2.4 show plots of the lag and lead coefficients' estimates from equation (2.2) and their 95% confidence interval, which allows me to visualize the dynamic impact of deregulation on the number of weeks worked. Notice that Figure 2.3, which

[3]Beck, Levine and Levkov (2010) analyzes the effects of deregulation on a number of different measures of inequality including state Gini Coefficients, Theil Index and the natural Log Income Percentile Differences.

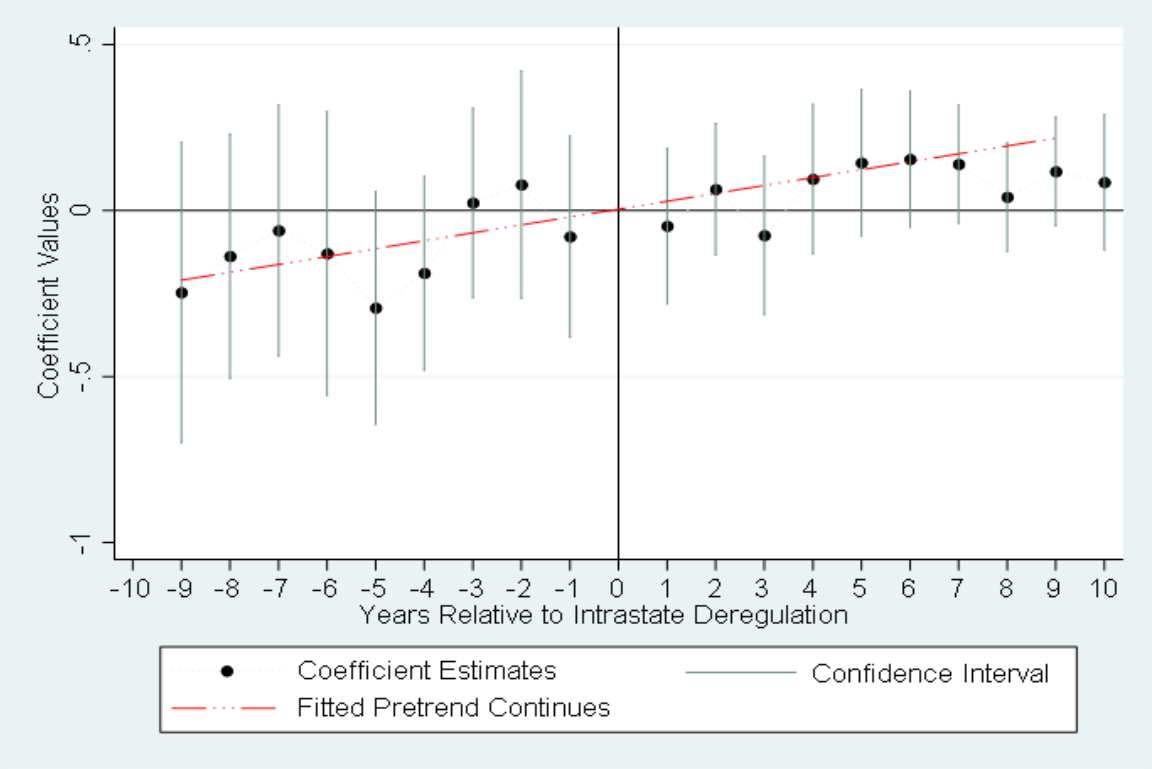


Figure 2.3: Change in Weeks, Years Before or After Intrastate Bank Deregulation

relates to intrastate reforms, shows a generally noisy pattern prior to deregulation, which rejects the likelihood of strong pre-treatment trends. Notice also that the post deregulation coefficients are not statistically different from 0. The dashed line represents a counter-factual line which is fitted to the points prior to the policy change, this provides a visual interpretation for the impact of the policy change. The counter-factual reference line, which is fitted to the pre-treatment estimate, runs through the post-treatment confidence interval. This supports the conclusion that intrastate reforms did not have an impact on the number of weeks individuals spent working.

In Figure 2.4, the coefficients prior to interstate deregulation are associated with lower number of working weeks, however they are mostly statistically insignificant. Although the pattern of the data is generally flat, it does not fully reject the possibility of pre-treatment trends. For this reason, we incorporate state-specific time trends in all regression estimates based on equation (2.1). The counter-factual reference line runs below the confidence interval

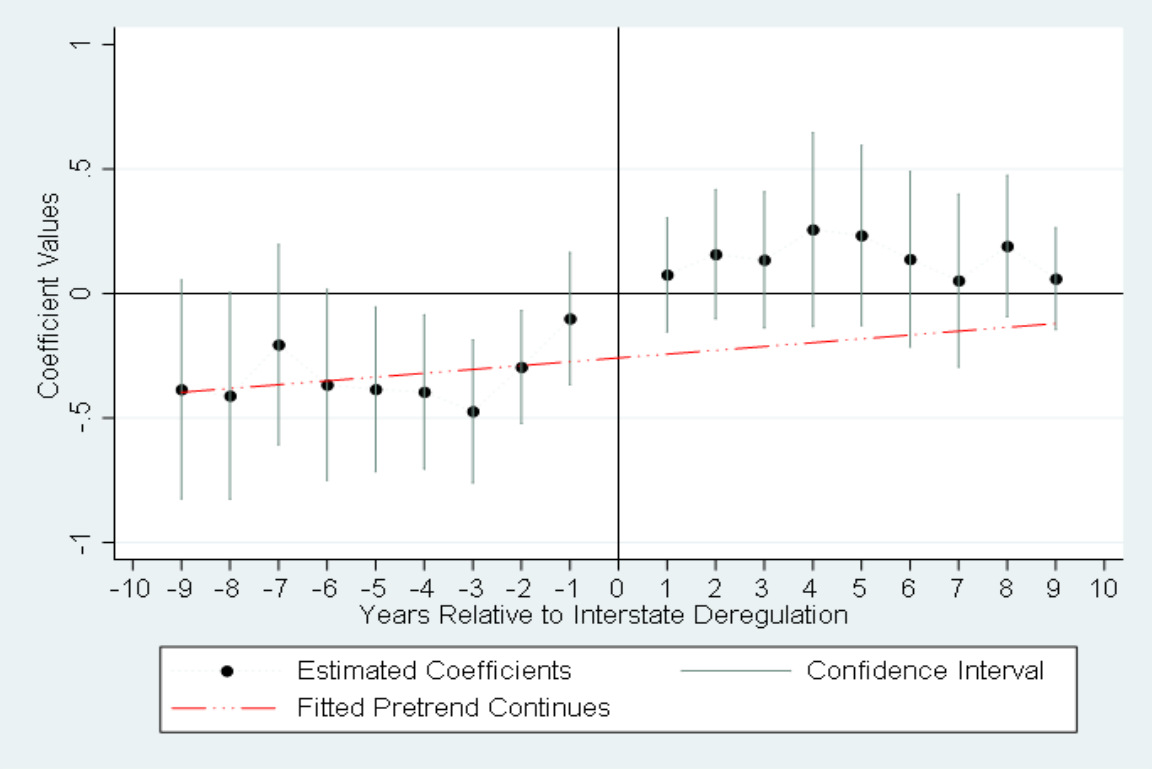


Figure 2.4: Change in Weeks, Years Before or After Interstate Bank Deregulation

of coefficient estimates after treatment. This suggests that there is an increase in the number of weeks individuals worked, as seen from a gradual upward trend, suggesting that these effects were ongoing and persistent following deregulation.

Figures 2.5 and 2.6 show plots of the coefficient estimates and their respective confidence intervals from equation (2.2) for total annual hours worked. The pattern is similar to the total number of working weeks. The coefficient estimates for intrastate reforms are insignificant and noisy, indicating the policy reform had little effect on hours people worked. Similar to figure 2.3, the post treatment effects are not statistically different from 0 and the counterfactual line runs through their confidence interval. Suggesting that intra-state reforms did not much effect annual hours individual's worked.

In Figure 2.6, which corresponds to interstate deregulation, post deregulation coefficients suggest that there is a gradual persistent increase in the number of hours prior to deregulation. Most of the coefficients prior to deregulation are not significant, but their exists an

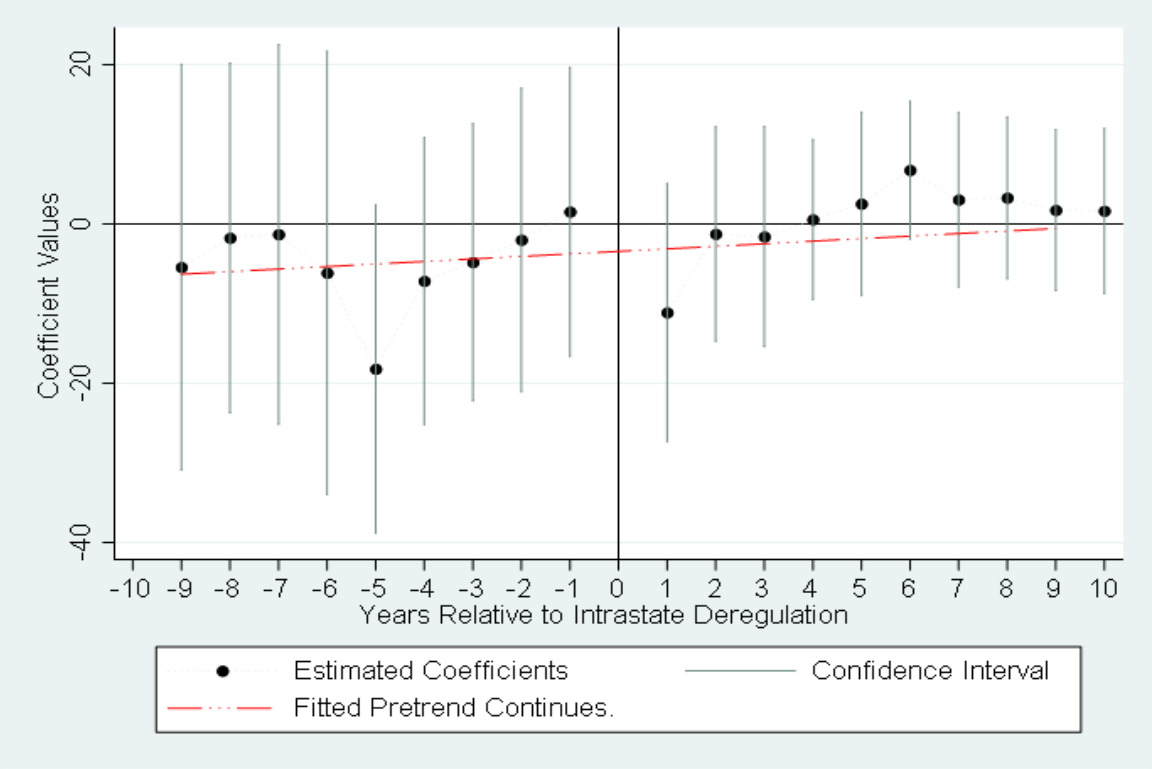


Figure 2.5: Change in Hours, Years Before or After Intrastate Bank Deregulation

Ashenfelters Dip. However, the counter factual reference line would run below the confidence interval, if one fits it to points prior to the dip. This suggests an increase in the total hours worked, though the standard DD model may not fully capture this effect.

Table 2.6 presents estimates from equation (2.1) of the number of weeks worked over the course of a year for each of the following earnings groups: the bottom quartile, the middle 25% - 75%, and the top quartiles. The regressions reported include all fixed effects, state-specific time trends, and other controls X specified in equation (2.1). Column (1) shows that interstate banking reforms had a positive and statistically significant impact on the number of weeks worked. The point estimate (.315) is economically meaningful as it suggests that the average worker spent approximately 1.5 more days working each year. Columns 2–4 report the estimates for the bottom quartile, the middle 25% - 75%, and the top quartile, respectively. Column (3) suggests that the reforms mainly impacted workers in the middle earnings group, as the effects are only statistically significant for this group. The effect of

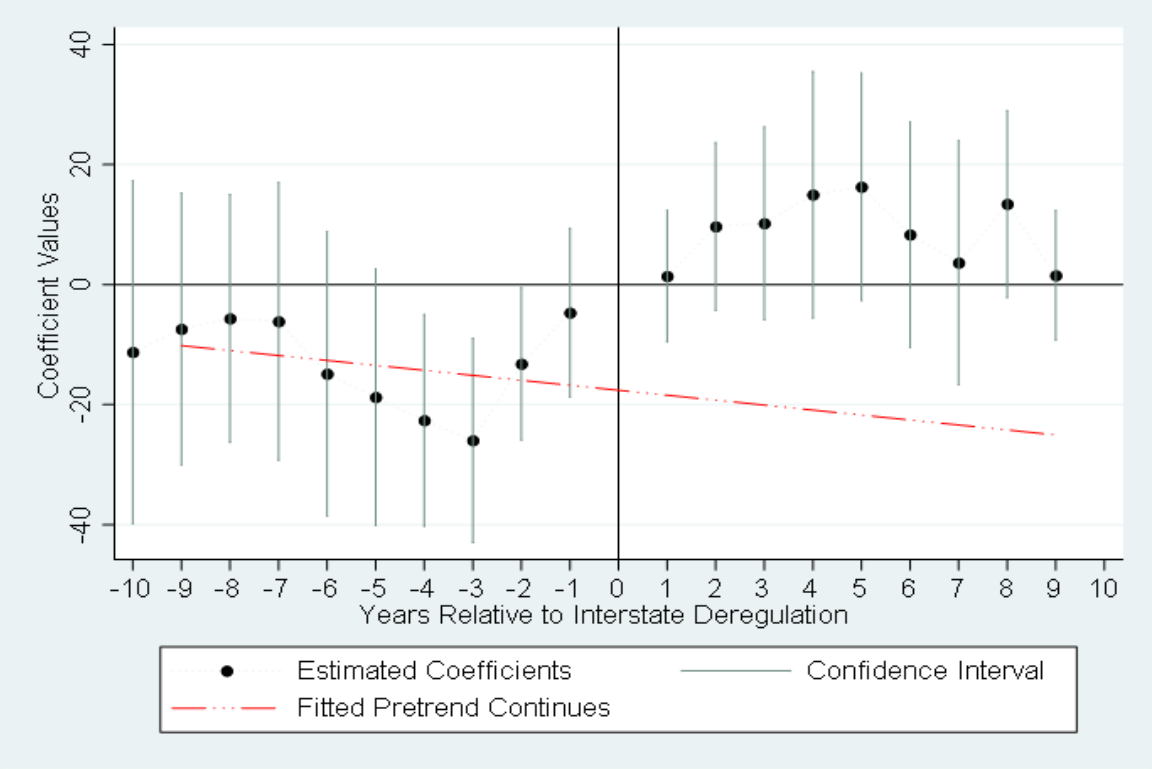


Figure 2.6: Change in Hours, Years Before or After Interstate Bank Deregulation

intrastate banking is statistically insignificant for all groups.

Table 2.7 reports the effects of deregulation on the number of usual weekly hours worked for different earning groups. The findings are similar to the effects of reforms on the number of weeks worked. Interstate banking reforms shows a statistically significant effect on weekly hours, while the estimates are insignificant for intrastate reforms. Furthermore, columns 2 and 3 show that the effect is driven by workers who lie in the bottom and middle of the wage distribution, and not the top quartile. Note that the estimate is almost 40% larger for the bottom of the income (column 2) distribution than those on the middle quartile (column 3).

The effects of deregulation on total hours worked annually is reported in Table 2.8. The total number of hours is constructed by taking the average number of hours worked per week and multiplying it by the number of weeks, the effects are consistent with previous estimates. The exercise here provides a concrete magnitude for the impact of banking reforms on individual labor input. The finding suggests that after interstate banking reforms were

Table 2.6 Impact of Deregulation on Weeks Worked, by Earnings Group

Variable	All 1	Bottom Quartile 2	Middle 25th - 75th 3	Top Quartile 4
Intra	0.0593	−0.0366	0.0832	0.1215
	(0.1370)	(0.2062)	(0.1402)	(0.1604)
Inter	0.3142**	0.4431	0.3723***	0.1496
	(0.1448)	(0.2823)	(0.1206)	(0.1916)
Observations	1,437,324	354,756	709,257	354,867

Table 2.7 Impact of Deregulation on Usual Weekly Hours, by Earnings Group

Variable	All 1	Bottom Quartile 2	Middle 25th - 75th 3	Top Quartile 4
Intra	−0.0527	−0.0030	0.0686	−0.1263
	(0.1330)	(0.2011)	(0.1192)	(0.1141)
Inter	0.3911***	0.4016***	0.2917***	0.1737
	(0.0963)	(0.1452)	(0.0922)	(0.1129)
Observations	1,437,324	354,756	709,257	354,867

passed workers increased the annual quantity of labor input by 25 hours. The effect is driven by the bottom and middle earning groups of the wage distribution, and workers in the bottom quartile of the distribution show an average increase approximately 22% higher than those who are at the middle of the distribution (33.6 hours vs 27.5 hours). Note that the estimates reported in column 4 indicate that deregulation had no impact on workers in the top earnings group.

2.6 Conclusion

This chapter investigates how changes in credit markets effects the labor market, through the lens of a major change in U.S. financial policy. We assess the impact of state-level banking reforms, which are generally thought to increase the availability of credit, on a nationally representative sample of U.S. workers. The evidence presented here suggests that

Table 2.8 Impact of Deregulation on Annual Hours, by Earnings Group

Variable	All 1	Bottom Quartile 2	Middle 25th - 75th 3	Top Quartile 4
Intra	4.097	1.394	7.253	0.341
	(8.588)	(13.162)	(9.345)	(9.483)
Inter	24.611***	33.622**	27.547***	12.133
	(7.471)	(14.890)	(7.387)	(8.747)
Observations	1,437,324	354,756	709,257	354,867

Notes: Numbers in parentheses are robust standard errors clustered at the state level, and ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively. Controls are not reported for brevity.

interstate banking, which allowed banks to enter into new markets by acquiring branches across state borders, lead to a growth in real wages and had at the least non-negative impact on overall wages. The results here show that there does not appear to be any strong relationship between deregulation and wage inequality, in spite of generally robust findings that deregulation lead to declines in income inequality as shown in (Beck, Levine and Levkov, 2009). This suggests that banking deregulation lead to declines income inequality due to increase in the labor input supplied by workers who are in lower earnings groups. This is supported by the finding that interstate deregulation lead to increases in the time individuals reported working among workers who are not within the top earnings quartile and point estimates suggests that this response is highest for those who are the lowest on the earnings quartile.

This chapter has also suggested that interstate deregulation has larger macroeconomic consequences for the labor markets than intrastate banking reforms. This finding is consistent with other studies which consider the effects of deregulation on comprehensive samples on pay-roll firms. Ceterolli and Strahan (2006) , Kerr and Nanda (2009) find that predominantly interstate banking deregulations rather than intrastate deregulations effect pay-roll firms, mainly benefited firms with smaller establishment sizes. This is also consistent with Leblebiciolu and Weinberger (2017) who find interstate reforms lead to decline in labors

share of income.

We note that there are limitations to the results presented by the analysis in this chapter. The presence of potential confounding pre-treatment trends makes it difficult to identify precise magnitudes for the causal impact of deregulation on wages and labor input. This study approaches this issue by modeling the potential pre-treatment trend and then comparing it to estimates which presumes few pre-treatment trends. This creates a plausible boundary for the true casual effect, which the results suggest is either non-negative or positive. Empirically identifying impact of credit shocks is generally difficult, as the availability of credit is rarely exogenous. It is affected by current and future macroeconomic states, policy, beliefs and underlying performance of assets in credit markets. The approach used in this study is largely one which aims to identify the causal impact of the shock making the fewest possible structural assumptions.

Chapter 3. The Impact of Bank Expansion on Self-Employed Business Owners: Evidence from US States

3.1 Introduction

Entrepreneurship has long been considered a key determinant of growth and economic prosperity through its effects on technological progress, market competition, and job creation. Recent empirical studies show that growth in labor productivity can be often attributed to replacement of lower productivity firms by higher productivity entrants.[1] They also document that expansions of start-ups are major sources of gross job creation in the US. Because of its importance to other economic activities, understanding the determinants of entrepreneurship has been a subject of considerable interest for economists and policy makers alike.

Many scholars have argued that the limited access to credit is one of the major factors behind the low level of entrepreneurship in many economies (Buera et al. 2015). According to the Small Business Credit Survey (2015), nearly 40 percent of respondents said that they sought credit primarily to expand their business. The majority of small firms (under $1 million in annual revenues) and startups (under 5 years in business) were unable to obtain any credit. In another survey, conducted by the World Bank (2015), a significant fraction of business owners (especially those in developing countries) chose credit constraints as the main obstacle for expanding their production.[2] In the light of these surveys, one can reasonably conclude that individuals wishing to create their own businesses are more likely to face tougher credit constraints.

In this chapter, we study the causal impact of changes in credit availability due to US

[1]Important contributions are Foster et al. (2006), Haltiwanger et al. (2013), Fairlie (2014) among many others. Decker et al. (2014) provides a comprehensive review of the role of entrepreneurship in US job creation and economic dynamism.

[2]Small Business Credit Survey is conducted by the Federal Reserve Banks of New York, Atlanta, Cleveland, and Philadelphia, and available at https://www.newyorkfed.org/smallbusiness/Spring2014/index.html. The World Bank's (2015) entrepreneurship survey can be found at http://databank.worldbank.org/data/reports.aspx?source=enterprise-surveys#.

banking deregulation on individual entrepreneurship over the 1980–2007 period. We exploit the variation in the timing of intrastate and interstate banking deregulation to study the effects of the resulting credit change. As mentioned in the previous chapter, a main advantage of our approach is that the timing of branching deregulation are unlikely to be related to factors such as wealth, education, attitudes to risk; as a result, these regulatory changes provide a natural setting for identifying the causal effects of credit constraints on an individual's decision to become an entrepreneur. Using the Current Population Survey Outgoing Rotation Group (CPS-ORG) files, we consider self-employed individuals (overwhelmingly small business owners), and classify them into incorporated and unincorporated. Our analysis distinguishes between the incorporated self-employed and unincorporated individuals, as key differences between these two groups were highlighted in a recent study by Levine and Rubinstein (2017).[3]

Using this classification, we identify business creation and closure at individual-owner level exploiting the rotating nature of the CPS data. We also identify newly incorporated self-employed individuals who were previously unincorporated, which allows us to study whether changes in credit encourage the unincorporated to become incorporated.[4] An advantage of our data is that we are also able to explore heterogeneity across individuals as several studies have found discrimination in credit markets based on gender or race (Asiedu et al. 2012).

Our main findings can be summarized as follows. First, access to credit is a determinant of entrepreneurship –entry and exit rates of the incorporated self-employed increased significantly after branching deregulation. However, we do not find any effects on the entry into unincorporated self-employment from non-business owners. Second, we find that credit access influences organizational structure. For example, reforms increased incorpo-

[3]Previous studies identify *all* self-employed individuals as entrepreneurs (Borjas and Bronas 1989, Fairlie 2014, among many others). However, Levine and Rubinstein (2017) argue that incorporated self-employed is a better proxy for entrepreneurship, because their cognitive and non-cognitive traits are more consistent with what one expects from an entrepreneur (see Section 3 for more details).

[4]When a business becomes incorporated, it will have a separate legal identity and limited liability (i.e. the firm can enter into contracts and own property independently of its owners, and its owners are not fully responsible for the firm's debts). However, becoming an incorporated business involves both direct and indirect costs such as annual fees, higher tax rates, and organizational costs.

ration among previously unincorporated self-employed. Finally, banking deregulation had heterogeneous effects on different groups, especially on those who are likely to face binding credit constraints. We find that deregulation had a stronger impact on entry and exit rates of minorities and the young into business ownership.

This chapter relates to a growing literature that investigates why individuals engage in entrepreneurship given its risky nature (Moskowitz and Vissing-Jørgensen 2002). One view is that entrepreneurs are different from wage and salary workers by having different attitudes towards risk (Hall and Woodward 2010) and/or placing a greater value on non-pecuniary benefits (Hurst and Pugsley 2017). A second, and not mutually exclusive, viewpoint is that individuals' likelihood of engaging in entrepreneurship would change had they faced different economic conditions. In this strand of research, our chapter relates to studies that have investigated whether financial constraints impede entrepreneurship. Evans and Javanovic (1989) develop an occupational choice model where individuals can become entrepreneurs, and show that liquidity constraints hinder individuals with insufficient funds from starting their businesses. Holtz-Eakin et al. (1994) show that entrepreneurs who received large inheritances are more likely to stay in business (see also Blanchflower and Oswald 1998, Cagetti and De Nardi 2006). Hurst and Lusardi (2004) show that having wealth increases the propensity to become a business owner only at the top of wealth distribution, otherwise it has no effect.

Using wealth to draw inferences about liquidity constraints may potentially suffer from endogeneity as wealth itself may be linked to unobserved attributes such as talent, ability, and work ethic. Further, individuals' occupational decisions resulting from a change in wealth do not necessarily reflect their behavior when there is a change in credit access. When individuals use their own assets to engage in entrepreneurship, they undertake all of the risk associated with the venture. In contrast, when they are able to use a credit agreement, the lender shares the underlying risk with the borrower. Our approach has two advantages. First, we do not use wealth to draw inferences about liquidity constraints, and the timing

of branching deregulation are unlikely to be related to an individual's characteristics, which may affect her decision to become a business owner. Second, we test how changes in credit access affect entrepreneurship.

Within the bank deregulation literature, this chapter relates to Black and Strahan (2002) and Kerr and Nanda (2009 & 2010), who investigate the impact of US banking deregulation on entrepreneurship. Black and Strahan (2002), using data on new business incorporation compiled by Dun and Bradstreet over the 1976–1994, find that the rate of new incorporation increased following deregulation of branching restrictions. Kerr and Nanda (2009) use US Census Bureau data on establishments between 1977 and 1998, and investigate the impact of bank deregulation on business creation and closure. They find that the greatest increase in entry occurred among small start-ups, and most of business closures occurred among small, young firms.

This study differs from the above studies in the following key aspects. First, consistent with the above literature on occupational choice and entrepreneurship (Evans and Javanovic 1989, Hurst and Pugsley 2017), we examine the impact of bank deregulation on an individual's choice to engage in entrepreneurship. In contrast, Black and Strahan (2002) use the number of new incorporation per capita as a measure of entrepreneurship, and Kerr and Nanda (2009 & 2010) use the number of new establishments created by the payroll firms. Thus these studies exclude the majority of self-employed, which constitutes over 60 percent of US businesses.[5] Second, we distinguish between incorporated and unincorporated business owners, and show that the impact of reforms varies significantly across these groups. Equally important, we show that reforms affected businesses' organizational forms (e.g., increased incorporation among previously unincorporated business owners). Finally, we investigate how reforms affected business ownership among different groups, especially among those who are likely to face credit constraints.

The rest of the chapter is organized as follows: Section 3.2 discusses the data and explains

[5]This statistics is based on the Federal Reserve's 2015 Small Business Credit Survey (see also Fairlie 2014).

the construction of the data on entrepreneurial activity. Section 3.3 describes the details of the econometric methodology that we employ. Section 3.4 presents our results, and Section 3.5 concludes.

3.2 Data

The data used in this chapter are drawn from several sources covering the period between 1980–2007. Entrepreneurial activity is measured by business creation and closure at individual-owner level. Using the Current Population Survey Outgoing Rotation Group (CPS-ORG) files from Unicon Research Corporation (2015), we identify self-employed workers (who are predominantly small business owners).[6] The CPS-ORG is a monthly household survey where each household is interviewed for four consecutive months in one year, followed by four consecutive months one year later (after which they leave the sample permanently). In order to identify entry and exit of entrepreneurs in each state and year, we use this rotating feature of the data. The CPS-ORG files start in 1979, so we can identify entry and exit cohorts from 1980 onward. Our sample ends in 2007, because we do not want our estimates to be influenced by financial regulations passed during the Great Recession.[7]

Self-employed individuals in the CPS files are classified into two categories: incorporated and unincorporated. Previous studies used all self-employed individuals as a measure for entrepreneurship (Borjas and Bronars 1989, Fairlie 2014, among many others). However, this measure generates some puzzling outcomes in the sense that entrepreneurship does not appear to offer economic rewards. For example, studies have documented that the median self-employed worker has lower initial earnings and slower earning growth than wage and salary workers (Hamilton 2000, Hurst and Pugsley 2017). Levine and Rubinstein (2017) show

[6]Unicon Corporation cleaned up the problems in the raw CPS files provided by the Census Bureau and recorded variables so that the surveys became more comparable across years. In addition, as we shall discuss shortly, it also provides (publicly unavailable) variables that we exploit in our analysis.

[7]In 1994, the Census Bureau redesigned the CPS to improve the quality and quantity of the data collected, which led to changes in the population shares of some variables (Hipple 2010). As a robustness check, we conduct analysis using only data prior to 1994. Our results qualitatively remain the same (see Table A.2 in the appendix).

that these two groups have significantly different traits and earning profiles: the incorporated self-employed generally are more educated, work more hours, and earn much more per hour than salaried and unincorporated ones.[8] Therefore, our analysis separate these two groups, but mainly focuses on the incorporated who are more entrepreneurial.

Our sample includes all individuals between the ages of 25 and 55, but excludes those with imputed/missing worker class and inconsistent reports (Levine and Rubinstein 2017).[9] Prior to 1994, in the publicly available CPS files all incorporated self-employed individuals were classified as wage and salary workers. However, the CPS-ORG files from Unicorn Research Corporation include an unedited and unallocated worker-class variable through which we are able to identify incorporated self-employed correctly for the years prior to 1994. The CPS provides information on individuals' age, gender, race, marital status, and education level as well as their employment status, worker class, industry worked, and weekly hours worked.[10]

Table 3.1 reports summary statistics on key variables for self-employed and wage & salary workers, which are mainly in line with Levine and Rubinstein (2017).[11] Compared to wage & salary workers, most of self-employed individuals are male, white, and work longer hours. The percent of individuals who have at least some college education is very comparable across both groups. The percent of individuals who work in manufacturing is substantially higher for wage & salary workers, whereas the share of self-employed working in the private service sector is very similar to that of wage & salary workers in this sector. However, a

[8]Levine and Rubinstein use the March CPS files for years between 1995 and 2012. We do not use the March CPS, because the data do not distinguish incorporated self-employed from the unincorporated in the survey years prior to 1988.

[9]Following Levine and Rubinstein (2017), we also exclude individuals who work in public administration sector, because almost no entrepreneurial activity takes place in this sector.

[10]Industry classification over the sample period has changed three times, and thus we aggregate industries under the following 11 broad sectors: Agriculture & Mining, Construction, Manufacturing, Transportation/Utility/Information, Wholesale, Retail, Finance & Insurance, Professional, Repair, and Personal & Entertainment, and Public Administration. Analysis based on a detailed classification with 22 sectors yields very similar results. As we mentioned above, our final sample excludes Public Administration.

[11]Statistics in Table 1.A are based on the final dataset that we used in our regressions. Our original data have about 3.8 million observations on two rotating groups, each having about 1.9 million observations. After matching process described below, we have about 1.1 million observations (see the last row in Table 3.1). However, statistics based on the original sample yields very similar results to those in Table 3.1

Table 3.1 Summary Statistics on Self-employed and Wage Workers, 1980–2007

	Self-employed			Salary
	All	Incorporated	Unincorporated	Workers
Female (%)	29.9	23.1	33.0	46.8
	(45.8)	(42.2)	(47.0)	(49.9)
Age	41.6	42.4	41.2	39.6
	(7.8)	(7.4)	(7.9)	(8.2)
White (%)	92.2	92.9	91.9	85.4
	(26.8)	(25.7)	(27.3)	(35.3)
Some College (%)	58.3	70.9	52.7	53.8
	(49.3)	(45.4)	(49.9)	(49.9)
Hours Worked	44.7	48.0	43.2	40.6
	(17.5)	(15.7)	(18.1)	(11.3)
Manufacturing (%)	15.3	12.5	16.5	24.0
	(36.0)	(33.1)	(37.1)	(42.7)
Service (%)	68.6	72.9	66.7	69.9
	(46.4)	(44.4)	(47.1)	(45.8)
Sample Size	147,903	43,925	103,978	960,624

Notes: The data draw on the CPS-ORG Files from Unicon Corporation (2015). Some College represents fraction of individuals who have at least some college education, and Hours Worked represents total hours worked last week. Numbers in parentheses are standard deviations, and the CPS weights are used in all calculations.

Table 3.2 Entrepreneurial Activity at Individual-Owner Level, 1980–2007

	Entry Rate (%)		Exit Rate (%)		Switching
	Incorp.	Unincorp.	Incorp.	Unincorp.	Rate (%)
All Sample	1.4	2.5	33.3	26.2	7.3
	(11.8)	(15.6)	(47.1)	(44.0)	(26.0)
Female	0.8	1.9	39.1	28.9	5.1
	(9.2)	(13.8)	(48.8)	(45.3)	(21.9)
White	1.5	2.6	32.5	25.4	7.2
	(12.1)	(15.9)	(46.9)	(43.5)	(25.8)
Some College	1.8	2.6	32.7	28.1	9.0
	(13.3)	(15.8)	(46.9)	(45.0)	(28.7)
Manufacturing	0.5	0.5	31.6	43.0	8.8
	(7.2)	(6.8)	(46.5)	(49.5)	(28.3)
Service	1.5	2.6	34.3	28.7	8.1
	(12.2)	(15.8)	(47.5)	(45.2)	(27.2)

Notes: The data draw on the CPS-ORG Files from Unicon Corporation (2015). Some College represents individuals who have at least some college education. Numbers in parentheses are standard deviations, and the CPS weights are used in all calculations.

comparison of incorporated self-employed with other groups reveals that this group is mostly white, male, who are significantly more educated, and work longer hours.[12]

Upon this classification, we can easily determine entry to and exit from entrepreneurship. New entrepreneurs in year t are individuals who *changed* their worker class to self-employed from time $t-1$ to t. Similarly, *exiting* entrepreneurs in year t are self-employed individuals who *changed* their worker class to non-business owners from time $t-1$ to t.[13] We also identify *switchers* in year t as those unincorporated self-employed individuals who changed their worker class to incorporated self-employed from time $t-1$ to t.

This process clearly requires tracking of individuals over time. However, the CPS is a household survey, and does not have individual identifiers. Following Madrian and Lefgren (2000) and Ziliak et al. (2011), we uniquely match pairs using identical household ID, household number, record lines, sex, survey month, and race. We only consider individuals with age and schooling difference in two successive years less than two, and dropped all unmatched individuals from the sample.[14]

Table 3.2 reports summary statistics on the average entry and exit rates of each group as well as the average rate of switching from unincorporated to incorporated for different groups. The average annual entry (exit) rate is 1.4 (33.3) percent for incorporated self-employed, whereas it is 2.5 (26.2) percent for the unincorporated. About 7.3 percent of the unincorporated self-employed became incorporated. A comparison of the first row with rows 2–4 indicates that the average entry and switching rates are higher among white, male, and college educated individuals. Similarly, the average exit rate is generally smaller among white, male, and college educated individuals. Finally, the entry, exit, and switching rates

[12]As noted by many others (e.g., Acemoglu and Autor 2011), the reported earnings of self-employed in the CPS files are not reliable. A significant portion of self-employed individuals reported zero weekly earnings.

[13]We find that about 57 percent of exiting entrepreneurs become wage & salary workers, 41 percent unincorporated self-employed, and the remaining 2 percent unemployed.

[14]Consistent with Ziliak et al. (2011), this process usually yields 60 percent matching success, which leaves us about 1.1 million observations. Household IDs assigned in 1985 are problematic and the CPS had a major design change in 1994, and thus matching rates in these years were around 30-40 percent. Excluding these years in the analysis does not have any appreciable effects on our results.

are significantly higher in the private service sector than those in manufacturing.

Data on the timing of bank deregulation comes from Amel (2008). Banking activities in Delaware and South Dakota are skewed by the presence of credit card banks. Therefore, our analysis covers 48 states over the 1980–2007 period.

3.3 Econometric Specifications

Following the approach outlined in the previous chapter, we use a difference-in-difference model to investigate the effects of banking deregulation on entrepreneurship. We begin our analysis by estimating the following linear probability model:

$$Y_{isjt} = \alpha \text{Intra}_{st} + \beta \text{Inter}_{st} + \gamma X_{ist} + \eta_s + \eta_j + \eta_t + \eta_s t + \eta_j t + \varepsilon_{st}, \tag{3.1}$$

where Intra_{st} ,Inter_{st}, and X_{ist} remained as previously defined in chapter 2. Intra_{st} (Inter_{st}) is a dummy variable that identifies whether intrastate (interstate) banking deregulation is in effect in state s and year t. The dependent variable Y measures either entry or exit of entrepreneurs at individual level. Y_{isjt} is a dummy variable, which equals one if individual i in state s and industry j becomes an entrepreneur in year t and zero otherwise; or equals one if an *entrepreneur* i in year t becomes non-entrepreneur and zero otherwise. Our coefficients of interest are α and β.

State and industry fixed effects (η_s and η_j) are included to control for any time invariant state- and industry-specific factors that can affect entrepreneurship, and year fixed effects (η_t) to control for common shocks to economies. Finally, state-specific and industry-specific time trends ($\eta_s t$ and $\eta_j t$) are included to account for other trending factors that can influence entrepreneurship. Consistent with the approach in the previous chapter, we use heteroskedasticity robust standard errors clustered at the state level.

As in chapter 2, we conduct an event study, in which we estimate the following dynamic

equation:

$$Y_{isjt} = \sum_{\tau=-8}^{15} \alpha_\tau \text{Intra}_{st}^{\tau} + \sum_{\tau=-8}^{15} \beta_\tau \text{Inter}_{st}^{\tau} + \gamma X_{ist} + \eta_s + \eta_j + \eta_t + \varepsilon_{st}. \tag{3.2}$$

As in the previous chapter, we extend the original difference-in difference model by including a set of dummies that take a value of one in the τth year before or after the banking deregulation and zero otherwise, and the end points include all earlier and later years.[15] We exclude the year of deregulation, and thus the coefficients measure yearly performance of entrepreneurial activity relative to reform years. If our identification assumption is valid, the estimated coefficients on α_v and β_v for $v < 0$ should not be statistically different from zero. This dynamic approach also allows us to see if there are any lagged effects of the banking reforms on entrepreneurship.

3.4 Results

3.4.1 Benchmark

Table 3.3 reports the effects of banking reforms on entry and exit of the incorporated self-employed based on equation (3.1).[16] All regressions include state, industry, and year fixed effects. Columns 1 and 2 report the impact of deregulation on the likelihood that a non-business owner (i.e., wage and salary workers) subsequently enters into incorporated self-employment, and note that the interstate deregulation has positive and highly significant effects on the likelihood that a non-business owner enters into incorporated self-employment. Since the entry rate prior to the interstate reform was about 0.63 percent, results reported

[15] Our dynamic equation is similar to Beck et al. (2010). As shown in Table A.1 in the appendix, most of the banking deregulation happened during the 1980s, and in our data the first entry cohort is 1980; as a result, we include only 8 years before the deregulation. Kerr and Nanda (2009) and Beck et al. (2010) estimate their dynamic models without any control variables. Excluding X from equation (3.2), however, yields similar results.

[16] As discussed above, the Census Bureau redesigned the CPS in 1994, which led to changes in the population shares of some variables (Hipple 2010). Table A.2 in the appendix reports results using the data over 1980–1993, and for brevity, we only report estimates on the Intra and Inter variables (although all regressions include all variables specified in equation (3.1)). A comparison with Table 3.3 indicates that results qualitatively remain mostly the same.

Table 3.3 Impact of Deregulation on Incorporated Self-employed, 1980–2007

	Entry to Incorporated from				Exit from Incorporated to			
	Non-Business		Unincorporated		Non-Business		Unincorporated	
Variable	1	2	3	4	5	6	7	8
Intra	0.0009	0.0007	0.0111*	0.0009	0.0183**	0.0111	0.0143	0.0168
	(0.0007)	(0.0005)	(0.0062)	(0.0051)	(0.0081)	(0.0101)	(0.0113)	(0.0116)
Inter	0.0022***	0.0024***	0.0040	0.0040	0.0054	0.0068	0.0350**	0.0405***
	(0.0007)	(0.0007)	(0.0064)	(0.0055)	(0.0111)	(0.0130)	(0.0133)	(0.0152)
Female		−0.0033***		−0.0455***		0.0761***		0.0136**
		(0.0003)		(0.0034)		(0.0059)		(0.0064)
Black		−0.0038***		0.0106		0.1027***		0.0971***
		(0.0004)		(0.0087)		(0.0307)		(0.0179)
Other		0.0015		0.0330***		0.0675***		0.0431***
		(0.0010)		(0.0120)		(0.0131)		(0.0160)
Married		0.0040***		0.0236***		−0.0260***		−0.0550***
		(0.0003)		(0.0036)		(0.0079)		(0.0070)
Age		0.0009***		0.0008		−0.0251***		−0.0168***
		(0.0001)		(0.0015)		(0.0032)		(0.0040)
Age^2		−0.0000***		−0.0000		0.0002***		0.0002***
		(0.0000)		(0.0000)		(0.0000)		(0.0000)
High School		0.0038***		0.0301***		−0.0192*		−0.0907***
		(0.0004)		(0.0037)		(0.0100)		(0.0163)
Some College		0.0070***		0.0418***		−0.0290***		−0.1252***
		(0.0004)		(0.0051)		(0.0107)		(0.0179)
College		0.0137***		0.0837***		−0.0194		−0.1388***
		(0.0007)		(0.0062)		(0.0117)		(0.0188)
$Entrp_{t-1}$		0.0409***		0.1937		0.0631		0.0553
		(0.0126)		(0.1628)		(0.3867)		(0.3228)
Time Trends	No	Yes	No	Yes	No	Yes	No	Yes
Observations	945,440	930,166	85,252	84,179	36,476	36,075	34,784	34,391

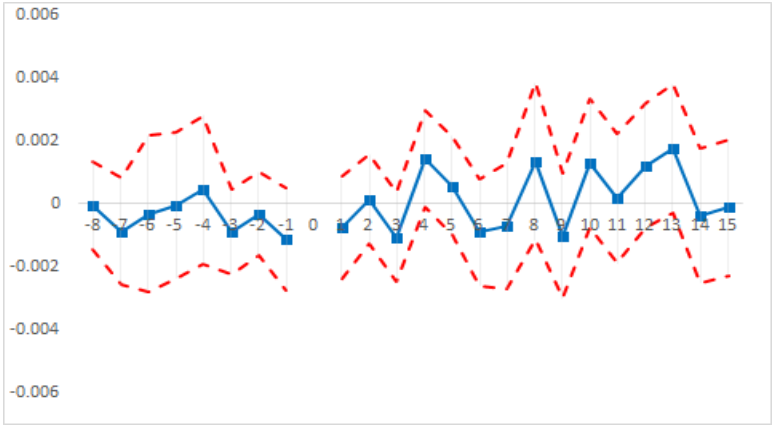
Notes: All regressions include state, industry, and year fixed effects; and regressions are weighted by the CPS weights. Numbers in parentheses are the robust standard errors clustered at the state level, and ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively.

in column 2 imply that deregulation increased the entry rate by 38 percent.

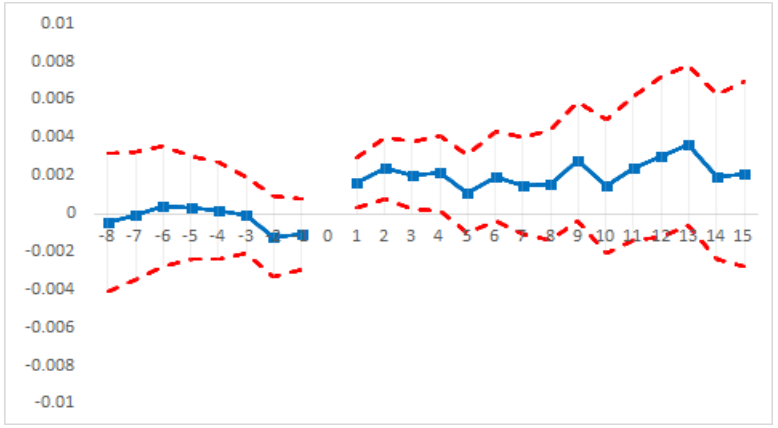
Columns 3 and 4 show the effects on the likelihood that an unincorporated self-employed worker incorporates. The estimated coefficients imply that the reforms did not have any significant effect on their entry. Consistent with the summary statistics reported in Table 3.2, the estimated coefficients on the control variables in columns 2 and 4 imply that educated, married males are more likely to become an entrepreneur. Similarly, compared to whites, blacks are less likely to enter into self-employment.

Columns 5–8 report the impact on the likelihood of exit from incorporated-self employment. An incorporated business owners may become a wage and salary worker (columns 5 and 6) or may simply become unincorporated (columns 7 and 8). Reforms had no significant impact on incorporated self-employed individuals becoming wage and salary workers. However, interstate banking reforms increased the likelihood that an incorporated self-employed individual becomes an unincorporated business owner by 4 percentage points, which is substantial given that the exit rate among this group prior to the reforms was about 18.5 percent. Observe that exit rates are higher among females, non-whites, young, and the less-educated.

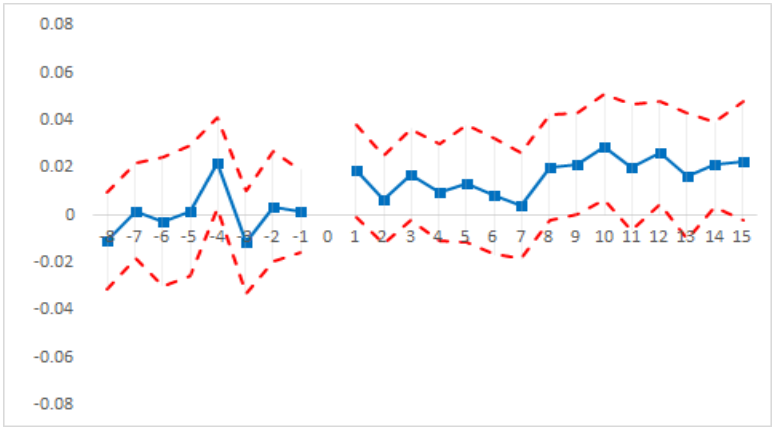
The validity of these results depends on our assumption that there are no pre-treatment trends in the outcome variables. Figures 3.1 and 3.2 show the estimated coefficients on lag and lead variables in equation (3.2) along with their 95-percent confidence intervals. Observe that the pre-treatment effect is usually small and statistically insignificant, which suggests that the identification assumption is not violated. There is a jump in estimates on the lagged values of the interstate deregulation for non-business owners (Figure 3.1 top right panel), and the estimates for the first four years are statistically significant at the 5-percent level. Further, the bottom right panel of 3.1 shows an upward trend in post-reform years .



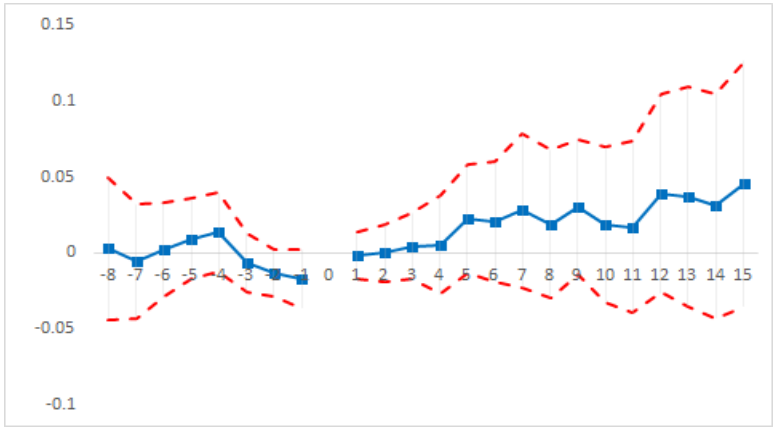
Intrastate: Entry from Non-Business Owners



Interstate: Entry from Non-Business Owners

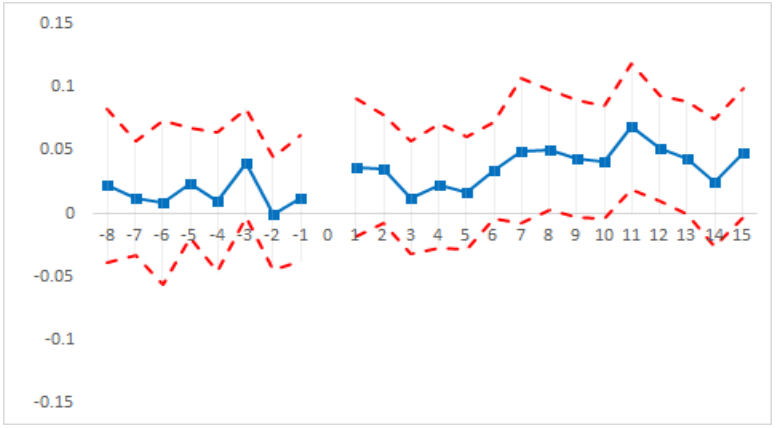


Intrastate: Entry from Unincorporated Self-employed

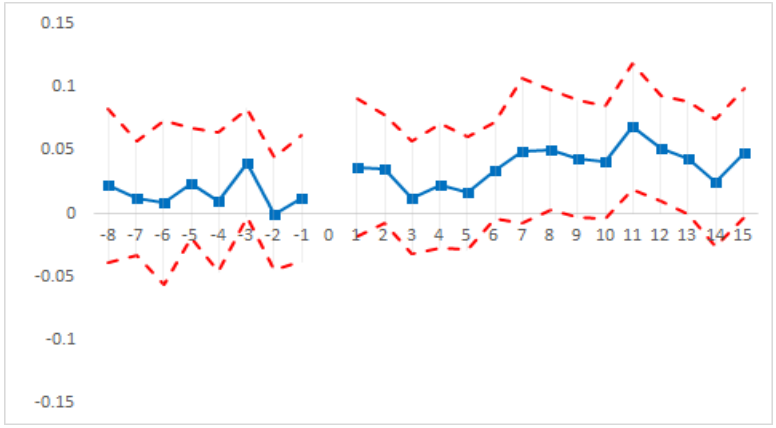


Interstate: Entry from Unincorporated Self-employed

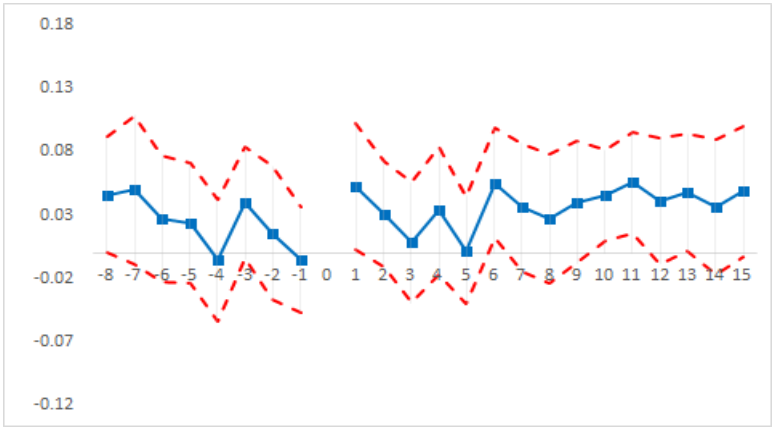
Figure 3.1: Estimated Coefficients from Equation (2) for Entry into Incorporated Self-employed.



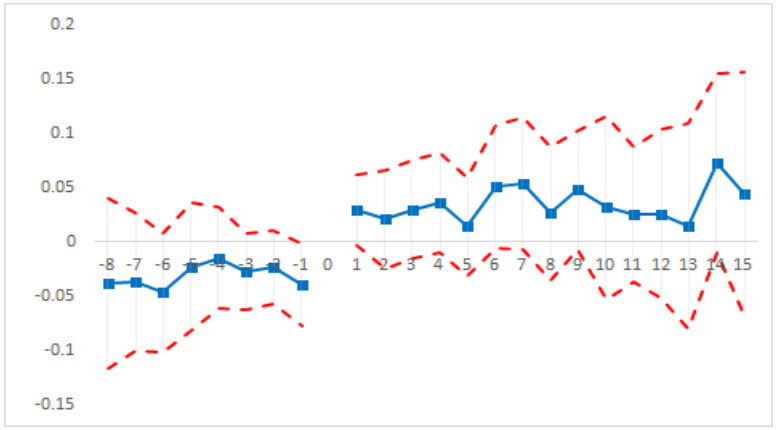
Intrastate: Exit to Non-Business Owners



Interstate: Exit to Non-Business Owners



Intrastate: Exit to Unincorporated Self-employed



Interstate: Exit to Unincorporated Self-employed

Figure 3.2: Estimated Coefficients from Equation (2) for Exit from Incorporated Self-employed.

These effects can not be captured by equation (3.1); thus, we extend equation (3.1) by including three dummy variables for each reform:

$$Y_{isjt} = \alpha_1 \text{Intra}_{\{1,2\}} + \alpha_2 \text{Intra}_{\{3,4\}} + \alpha_3 \text{Intra}_{\{5+\}} + \beta_1 \text{Inter}_{\{1,2\}} + \beta_2 \text{Inter}_{\{3,4\}} + \beta_3 \text{Inter}_{\{5+\}} + \gamma X_{ist} + \eta_s + \eta_j + \eta_t + \eta_s t + \eta_j t + \varepsilon_{st}, \tag{3.3}$$

where, for clarity, we dropped state and time indexes in reform variables. Here, for example, $\text{Intra}_{\{1,2\}}$ equals one for the first two years of the intrastate reform and zero otherwise, $\text{Intra}_{\{3,4\}}$ equals one for the third and fourth years of the reform, and $\text{Inter}_{\{5+\}}$ equals one for the fifth year after the reform or later.

Table 3.4 represent the results based on equation (3.3). For brevity, we do not report estimates on controls, but they are similar to those reported in Table 3.3. Note that the estimated coefficients on interstate deregulation are very similar to those reported in columns 1 and 2 in Table 3.3. The interstate reform also increased the likelihood that the unincorporated self-employed incorporates. Specifically, according to column 4, the entry rate among this group is higher by 40 percent 5 years after the reform (the pre-reform entry rate was about 7.2 percent).

Column 5–8 show the effects of reforms on the probability of exit from incorporated self-employment. The effects of the interstate deregulation are mostly insignificant on those who become non-business owners, and positive and significant on those who become unincorporated. Increased competition in the market after banking reforms increase failure rates among the incorporated self-employed, but the fact that deregulation has an impact on those who became unincorporated self-employed indicates that there is more to the story. In the subsequent section, we shall explore heterogeneity across different groups, and find that the reforms increased the likelihood of exit from business ownership among incorporated minorities and the young.

Table 3.4 Impact of Banking Deregulations on Incorporated Self-employed, 1980–2007

	Entry to Incorporated from				Exit from Incorporated to			
	Non-Business		Unincorporated		Non-Business		Unincorporated	
Variable	1	2	3	4	5	6	7	8
$\text{Intra}_{\{1,2\}}$	0.0003	0.0005	0.0087	0.0034	0.0175	0.0172	0.0185	0.0239*
	(0.0006)	(0.0006)	(0.0059)	(0.0062)	(0.0125)	(0.0133)	(0.0148)	(0.0141)
$\text{Intra}_{\{3,4\}}$	0.0008	0.0009	0.0082	−0.0023	−0.0042	−0.0026	−0.0007	0.0062
	(0.0007)	(0.0005)	(0.0058)	(0.0067)	(0.0112)	(0.0128)	(0.0157)	(0.0181)
$\text{Intra}_{\{5+\}}$	0.0013	0.0013*	0.0123	−0.0023	0.0223**	0.0198	0.0194	0.0206
	(0.0009)	(0.0008)	(0.0088)	(0.0071)	(0.0101)	(0.0166)	(0.0133)	(0.0214)
$\text{Inter}_{\{1,2\}}$	0.0021***	0.0023***	0.0018	0.0027	−0.0048	−0.0011	0.0332**	0.0383**
	(0.0007)	(0.0007)	(0.0064)	(0.0059)	(0.0124)	(0.0133)	(0.0135)	(0.0150)
$\text{Inter}_{\{3,4\}}$	0.0022***	0.0025***	0.0075	0.0120	0.0249	0.0307*	0.0374**	0.0452*
	(0.0008)	(0.0007)	(0.0087)	(0.0079)	(0.0148)	(0.0162)	(0.0185)	(0.0226)
$\text{Inter}_{\{5+\}}$	0.0022*	0.0023**	0.0252*	0.0297**	0.0218	0.0275	0.0329*	0.0413*
	(0.0012)	(0.0010)	(0.0143)	(0.0129)	(0.0137)	(0.0186)	(0.0182)	(0.0233)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Time Trends	No	Yes	No	Yes	No	Yes	No	Yes
Observations	945,440	930,166	85,252	84,179	36,476	36,075	34,784	34,391

Notes: All regressions include all fixed effects and controls specified in equation (3); and regressions are weighted by the CPS weights. Numbers in parentheses are the robust standard errors clustered at the state level, and ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively.

Table 3.5 Impact of Deregulation on Unincorporated Self-employed, 1980–2007

	Entry		Exit	
Variable	1	2	3	4
Intra	−0.0009	0.0005	−0.0068	−0.0033
	(0.0007)	(0.0010)	(0.0073)	(0.0064)
Inter	−0.0010	−0.0007	−0.0093	−0.0023
	(0.0009)	(0.0010)	(0.0062)	(0.0064)
Female		−0.0017***		0.0703***
		(0.0005)		(0.0049)
Black		−0.0048***		0.1007***
		(0.0008)		(0.0093)
Other		0.0020		0.0460***
		(0.0021)		(0.0100)
Married		0.0018***		−0.0302***
		(0.0005)		(0.0034)
Age		0.0012***		−0.0214***
		(0.0002)		(0.0027)
Age^2		−0.0000***		0.0002***
		(0.0000)		(0.0000)
High School		0.0007		−0.0291***
		(0.0013)		(0.0067)
Some College		0.0024*		−0.0305***
		(0.0013)		(0.0075)
College		0.0039**		−0.0374***
		(0.0019)		(0.0089)
$Entrp_{t-1}$		0.0055		−0.0139
		(0.0169)		(0.1550)
Time Trends	No	Yes	No	Yes
Observations	957,273	941,811	96,324	95,028

Notes: All regressions include state, industry, and year fixed effects; and regressions are weighted by the CPS weights. Numbers in parentheses are the robust standard errors clustered at the state level, and ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively.

Table 3.6 Impact of Banking Deregulation on Unincorporated Self-employed, 1980–2007

	Entry		Exit	
Variable	1	2	3	4
$\text{Intra}_{\{1,2\}}$	−0.0004	0.0009	−0.0064	−0.0034
	(0.0010)	(0.0010)	(0.0080)	(0.0078)
$\text{Intra}_{\{3,4\}}$	−0.0015	−0.0001	−0.0069	−0.0016
	(0.0010)	(0.0013)	(0.0076)	(0.0076)
$\text{Intra}_{\{5+\}}$	−0.0011	0.0012	−0.0064	−0.0071
	(0.0007)	(0.0013)	(0.0096)	(0.0085)
$\text{Inter}_{\{1,2\}}$	−0.0010	−0.0007	−0.0065	0.0006
	(0.0009)	(0.0010)	(0.0073)	(0.0073)
$\text{Inter}_{\{3,4\}}$	−0.0008	−0.0007	−0.0165**	−0.0076
	(0.0012)	(0.0014)	(0.0068)	(0.0075)
$\text{Inter}_{\{5+\}}$	0.0011	0.0013	−0.0100	0.0006
	(0.0015)	(0.0020)	(0.0097)	(0.0095)
Controls	No	Yes	No	Yes
Time Trends	No	Yes	No	Yes
Observations	957,273	941,811	96,324	95,028

Notes: All regressions include all fixed effects and controls specified in equation (3); and regressions are weighted by the CPS weights. Numbers in parentheses are the robust standard errors clustered at the state level, and ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively.

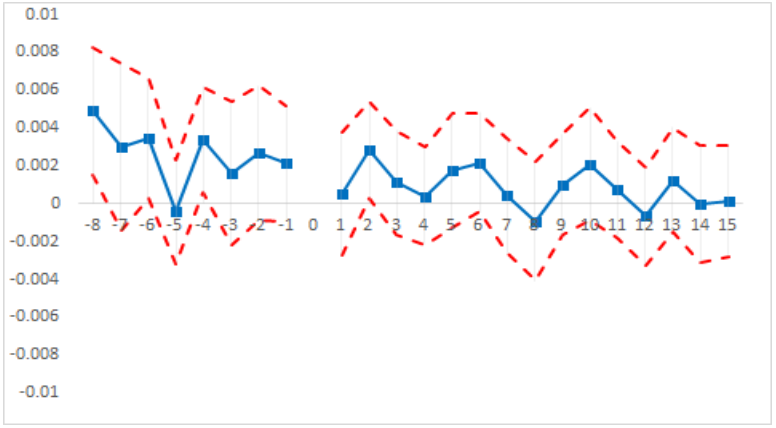
We now turn to investigate the effects of banking deregulation on the business dynamics of the unincorporated self-employed. Table 3.5 reports the regression results based on equation (3.1), and Figure 3.3 plots the estimated coefficients on lag and lead variables in equation (3.2) along with their 95-percent confidence intervals.[17] According to Tables 3.5 and Figure 3.3, reforms had no effect on the entry or exit of unincorporated self-employed. We obtain similar results using the more flexible model (3.3) as shown in Table 3.6.

Our analysis indicates that branching has different effects on the entry and exit rates of incorporated versus unincorporated self-employed. We show that most of these effects are concentrated among incorporated business owners. Thus, consistent with Levine and Rubinstein (2017), distinguishing between these two groups is important. Using all self-employed as a measure of entrepreneurship misses insights stemming from the differences between these two groups. Furthermore, we find that bank deregulation had an impact on organizational structure. The reforms increased propensity of a previously incorporated (unincorporated) business to transition into an unincorporated (incorporated) business. These effects are not captured in the previous studies, which relied on measures of entrepreneurship among mostly incorporated businesses (Black and Strahan 2002, Kerr and Nanda 2009).

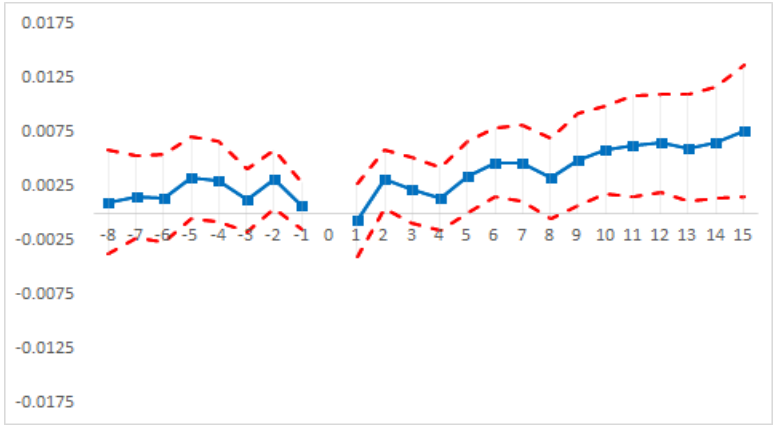
Black and Strahan (2002), using state-level Dun and Bradstreet incorporation data over the 1976–1994 period, find that the rate of new incorporation increased following banking deregulation. Our finding is similar to theirs, although we define entrepreneurship at the individual-owner level. In addition, our analysis indicates that some of the increase in incorporation comes from previously unincorporated businesses. Kerr and Nanda (2009) use data from the Longitudinal Business Database (LBD) of US Census Bureau over 1977–1998 to examine entrepreneurship and creative destruction following banking deregulation.[18] They find that interstate bank deregulation has a positive and significant impact on small start-ups, while intrastate deregulation has no effect on firm entry. They also find that

[17]We only consider the effects on non-business owners and also exclude all individuals becoming an incorporated self-employed.

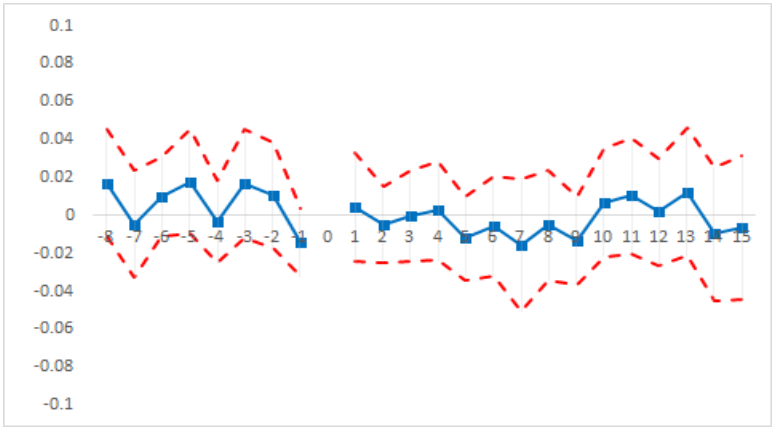
[18]LBD database covers only establishments with payroll, and thus excludes most of the businesses operated by self-employed individuals.



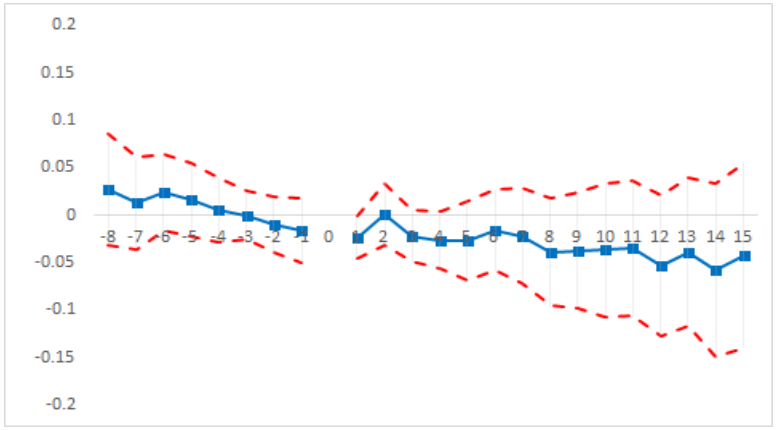
Intrastate: Entry from Non-Business Owners



Interstate: Entry from Non-Business Owners



Intrastate: Exit to Non-Business Owners



Interstate: Exit to Non-Business Owners

Figure 3.3: Estimated Coefficients from Equation (2) for Entry/Exit of Unincorporated Self-employed.

interstate deregulation increases business closures significantly among small start-ups. However, we show that the reforms did not have a uniform impact on all small businesses. Branching predominantly affected incorporated businesses, and have no observed impact on newly unincorporated businesses. Thus, branching did not have significant effect on the majority of unincorporated self-employed, which covers over 60 percent of US businesses.

3.4.2 Effects by Gender, Race and Age

Several studies have shown that certain groups (e.g., women and minorities) face higher barriers in credit markets to get loans. Using data from the Survey of Small Business Finances, Asiedu et al. (2012) find that the denial rate in a sample of loan applications in 2003 is about 30 percent higher for minority-owned firms compared to white males. Branching deregulation may alleviate the discrimination against these groups in two ways. First, increased competition in credit markets may induce banks to extend credits to previously excluded individuals. For example, Sun and Yannelis (2016) show that lifting intrastate banking restrictions raised college enrollment by about 2.6 percentage points. Second, banking reforms couple with technological innovations may induce banks to develop a more standard screening process where face-to-face communications will be minimum. Tewari (2014), for example, finds that following the branching deregulation mortgage access increased for lower-middle income groups, young, and black households; and she argues that banks' new screening technologies may have been responsible for this expansion.

We investigate how branching reforms have affected entrepreneurship among different groups, and exploit the detailed nature of the CPS data in order to address this problem. We explore heterogeneity by gender, race, and age. Table 3.7 reports the effects of deregulation on entrepreneurship among females and males.[19] Regressions include controls, fixed effects, state-specific and industry-specific time trends. There are three findings to note: first, the effects of interstate deregulation on the entry among non-business owners are

[19]In this section, results based on the standard model (3.1) are reported in the appendix. See Tables A.3, A.4.

Table 3.7 Impact of Deregulation on Entrepreneurship (1980–2007), by Gender

	Incorporated				Unincorporated	
	Entry from		Exit to			
	Non-Bus.	Uninc.	Non-Bus.	Uninc.	Entry	Exit
Variable	1	2	3	4	5	6
Panel A. Females						
$\text{Intra}_{\{1,2\}}$	0.0001	−0.0076	0.0043	0.0181	0.0005	−0.0004
	(0.0005)	(0.0087)	(0.0321)	(0.0364)	(0.0012)	(0.0137)
$\text{Intra}_{\{3,4\}}$	0.0005	−0.0069	−0.0298	−0.0240	−0.0014	−0.0032
	(0.0006)	(0.0107)	(0.0321)	(0.0401)	(0.0017)	(0.0158)
$\text{Intra}_{\{5+\}}$	0.0009	−0.0050	0.0362	−0.0234	0.0003	0.0094
	(0.0007)	(0.0125)	(0.0357)	(0.0460)	(0.0017)	(0.0191)
$\text{Inter}_{\{1,2\}}$	0.0010	0.0009	−0.0256	0.0589	−0.0006	0.0058
	(0.0006)	(0.0064)	(0.0380)	(0.0402)	(0.0017)	(0.0144)
$\text{Inter}_{\{3,4\}}$	0.0016**	0.0140	0.0403	0.0927**	−0.0023	−0.0201
	(0.0006)	(0.0117)	(0.0382)	(0.0434)	(0.0018)	(0.0193)
$\text{Inter}_{\{5+\}}$	0.0014**	0.0167	0.0332	0.0918**	−0.0002	−0.0104
	(0.0006)	(0.0150)	(0.0407)	(0.0440)	(0.0024)	(0.0219)
Obs.	445,537	26,168	8,319	7,329	450,975	32,372
Panel B. Males						
$\text{Inter}_{\{1,2\}}$	0.0008	0.0081	0.0178	0.0256*	0.0012	−0.0041
	(0.0010)	(0.0070)	(0.0147)	(0.0133)	(0.0015)	(0.0084)
$\text{Inter}_{\{3,4\}}$	0.0011	0.0001	−0.0007	0.0106	0.0011	0.0007
	(0.0008)	(0.0074)	(0.0137)	(0.0187)	(0.0015)	(0.0111)
$\text{Inter}_{\{5+\}}$	0.0016	−0.0013	0.0115	0.0282	0.0020	−0.0137
	(0.0011)	(0.0086)	(0.0184)	(0.0218)	(0.0015)	(0.0112)
$\text{Inter}_{\{1,2\}}$	0.0034***	0.0038	0.0036	0.0317**	−0.0007	−0.0002
	(0.0011)	(0.0071)	(0.0157)	(0.0130)	(0.0013)	(0.0080)
$\text{Inter}_{\{3,4\}}$	0.0032**	0.0105	0.0255	0.0339	0.0008	−0.0002
	(0.0013)	(0.0078)	(0.0189)	(0.0219)	(0.0020)	(0.0088)
$\text{Inter}_{\{5+\}}$	0.0031*	0.0338**	0.0241	0.0305	0.0027	0.0077
	(0.0017)	(0.0129)	(0.0218)	(0.0237)	(0.0021)	(0.0100)
Obs.	484,629	58,019	27,756	27,062	490,836	62,656

Notes: All regressions include all fixed effects and controls specified in equation (3); and regressions are weighted by the CPS weights. Numbers in parentheses are the robust standard errors clustered at the state level, and ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively.

stronger for males. Second, the likelihood that the unincorporated self-employed becomes an incorporated business owner increased among males, but not females. Third, the exit rate of the incorporated self-employed after deregulation is higher among females.

Next, we investigate the impact of banking reforms on entrepreneurship among racial minorities relative to whites, and the results are reported in Table 3.8. First, according to column 1, the likelihood that a non-business owner enters into incorporated self-employment is higher among non-whites. Since non-whites generally have lower initial wealth (Fairlie and Robb 2008), the marginal value of an increase in credit is higher for them. Consequently, relaxing credit constraints have stronger effects for minorities. For whites, we observe that there is an increase in the incorporation rate among previously unincorporated businesses (see column 2). Second, the reforms increased business closure rate substantially among incorporated non-white entrepreneurs, while having no such effects on whites. However, following reforms, incorporated whites are more likely to become unincorporated. Finally, the reforms increased exit rate among unincorporated non-whites, but decreased it among whites (column 6).

Higher exit rates after the deregulation can be explained by the increased competition created by these reforms. Why do minorities choose to exit the business entirely, while whites typically become unincorporated? One possibility is that whites do business in sectors where transitioning from a corporate to a non-corporate entity is relatively easier. However, our data does not seem to support this view. Because differences in the distributions of industries across these groups are not particularly large.[20] A plausible explanation is that whites run businesses with more assets that can induce them to stay in business. According to the 2012 Small Business Owners (SBO) survey, white owned firms have average sales of about $500,000 compared with $365,000 for those owned by Asians and $58,000 for those owned by blacks. For example, Fairlie and Robb (2008) find that black-owned businesses

[20]For example, among whites exiting from incorporated self-employment, 23 percent were doing business in wholesale and retail sectors and 9 percent in finance, insurance, and real estate sectors. The corresponding statistics for non-whites are 31 percent and 5 percent, respectively.

Table 3.8 Impact of Deregulation on Entrepreneurship (1980–2007), by Race

	Incorporated				Unincorporated	
	Entry from		Exit to			
	Non-Bus.	Uninc.	Non-Bus.	Uninc.	Entry	Exit
Variable	1	2	3	4	5	6
Panel A. Non-whites						
$\text{Intra}_{\{1,2\}}$	−0.0004	0.0596*	0.1099	0.0087	−0.0032	0.0895**
	(0.0012)	(0.0337)	(0.0890)	(0.0703)	(0.0020)	(0.0389)
$\text{Intra}_{\{3,4\}}$	0.0013	−0.0054	0.2181**	0.0144	−0.0040	0.0770*
	(0.0012)	(0.0291)	(0.0986)	(0.0848)	(0.0026)	(0.0387)
$\text{Intra}_{\{5+\}}$	0.0003	−0.0070	0.1447*	0.0373	−0.0031	0.0443
	(0.0014)	(0.0331)	(0.0830)	(0.1025)	(0.0030)	(0.0378)
$\text{Inter}_{\{1,2\}}$	0.0039**	−0.0373	0.0920	−0.0117	0.0040	0.0341
	(0.0015)	(0.0275)	(0.0698)	(0.0822)	(0.0025)	(0.0203)
$\text{Inter}_{\{3,4\}}$	0.0027***	−0.0147	0.1742***	−0.0157	0.0016	0.0911**
	(0.0010)	(0.0295)	(0.0595)	(0.0997)	(0.0023)	(0.0353)
$\text{Inter}_{\{5+\}}$	0.0035**	−0.0131	0.2048***	0.0738	0.0045	0.1268***
	(0.0014)	(0.0258)	(0.0639)	(0.1133)	(0.0031)	(0.0284)
Obs.	117,595	5,179	2,121	1,973	118,891	6,321
Panel B. Whites						
$\text{Intra}_{\{1,2\}}$	0.0007	0.0004	0.0138	0.0239	0.0014	−0.0083
	(0.0007)	(0.0061)	(0.0121)	(0.0142)	(0.0011)	(0.0080)
$\text{Intra}_{\{3,4\}}$	0.0009	−0.0022	−0.0101	0.0043	0.0004	−0.0062
	(0.0006)	(0.0067)	(0.0138)	(0.0177)	(0.0014)	(0.0079)
$\text{Intra}_{\{5+\}}$	0.0015*	−0.0026	0.0160	0.0182	0.0017	−0.0095
	(0.0008)	(0.0078)	(0.0177)	(0.0202)	(0.0015)	(0.0082)
$\text{Inter}_{\{1,2\}}$	0.0021**	0.0055	−0.0051	0.0413***	−0.0014	−0.0018
	(0.0008)	(0.0058)	(0.0135)	(0.0137)	(0.0013)	(0.0073)
$\text{Inter}_{\{3,4\}}$	0.0025***	0.0131*	0.0225	0.0470**	−0.0012	−0.0144**
	(0.0009)	(0.0077)	(0.0174)	(0.0206)	(0.0016)	(0.0066)
$\text{Inter}_{\{5+\}}$	0.0021*	0.0316**	0.0162	0.0386*	0.0007	−0.0082
	(0.0011)	(0.0133)	(0.0205)	(0.0211)	(0.0023)	(0.0088)
Obs.	812,571	79,000	33,954	32,418	822,920	88,707

Notes: Regressions include all fixed effects, time trends, and controls as specified in equation (3), and are weighted by the CPS weights. Numbers in parentheses are the robust standard errors clustered at the state level, and ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively.

Table 3.9 Impact of Deregulation on Entrepreneurship (1980–2007), by Age

	Incorporated				Unincorporated	
	Entry from		Exit to			
	Non-Bus.	Uninc.	Non-Bus.	Uninc.	Entry	Exit
Variable	1	2	3	4	5	6
Panel A. Young (Age < 40)						
$\text{Intra}_{\{1,2\}}$	−0.0008	0.0086	0.0396	0.0314	0.0003	0.0078
	(0.0008)	(0.0084)	(0.0292)	(0.0257)	(0.0016)	(0.0132)
$\text{Intra}_{\{3,4\}}$	−0.0007	−0.0014	0.0240	0.0069	−0.0004	0.0126
	(0.0008)	(0.0067)	(0.0261)	(0.0299)	(0.0019)	(0.0102)
$\text{Intra}_{\{5+\}}$	−0.0006	−0.0084	0.0448	0.0255	0.0003	0.0112
	(0.0008)	(0.0061)	(0.0270)	(0.0409)	(0.0019)	(0.0128)
$\text{Inter}_{\{1,2\}}$	0.0024***	0.0048	0.0084	0.0646**	−0.0019	0.0082
	(0.0008)	(0.0084)	(0.0209)	(0.0266)	(0.0013)	(0.0101)
$\text{Inter}_{\{3,4\}}$	0.0026**	0.0249**	0.0673**	0.0790***	−0.0022	−0.0090
	(0.0011)	(0.0110)	(0.0256)	(0.0269)	(0.0018)	(0.0114)
$\text{Inter}_{\{5+\}}$	0.0032**	0.0377**	0.0648**	0.0964***	−0.0000	−0.0091
	(0.0013)	(0.0172)	(0.0307)	(0.0339)	(0.0025)	(0.0105)
Obs.	481,673	35,390	12,853	12,039	488,462	41,490
Panel B. Old (Age ⩾ 40)						
$\text{Intra}_{\{1,2\}}$	0.0023**	−0.0020	0.0025	0.0218	0.0017	−0.0130*
	(0.0010)	(0.0072)	(0.0185)	(0.0170)	(0.0010)	(0.0073)
$\text{Intra}_{\{3,4\}}$	0.0029***	−0.0036	−0.0210	0.0087	0.0002	−0.0138
	(0.0010)	(0.0102)	(0.0188)	(0.0231)	(0.0014)	(0.0104)
$\text{Intra}_{\{5+\}}$	0.0039***	0.0007	0.0017	0.0218	0.0022*	−0.0228*
	(0.0013)	(0.0106)	(0.0245)	(0.0235)	(0.0011)	(0.0117)
$\text{Inter}_{\{1,2\}}$	0.0022**	0.0004	−0.0062	0.0229	0.0010	−0.0083
	(0.0010)	(0.0076)	(0.0172)	(0.0137)	(0.0014)	(0.0119)
$\text{Inter}_{\{3,4\}}$	0.0024**	0.0003	0.0065	0.0225	0.0011	−0.0087
	(0.0011)	(0.0091)	(0.0202)	(0.0265)	(0.0017)	(0.0111)
$\text{Inter}_{\{5+\}}$	0.0012	0.0214	0.0066	0.0070	0.0029	0.0061
	(0.0014)	(0.0127)	(0.0203)	(0.0214)	(0.0021)	(0.0154)
Obs.	448,493	48,789	23,222	22,352	453,169	53,538

Notes: Regressions include all fixed effects, time trends, and controls as specified in equation (3), and are weighted by the CPS weights. Numbers in parentheses are the robust standard errors clustered at the state level, and ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively.

start with substantially lower level of capital than white-owned firms. They also show that the white/non-white disparity in start-up capital is the major factor to racial disparities in closure rates and profits.

Finally, we explore heterogeneity across different age groups, and the results are reported in Table 3.9. We define young as individuals who are less than forty years old, the rest as old. The reforms had positive and significant effects on the entry of non-business owners into incorporated self-employment, although effects were somewhat stronger for the old. The deeregulation increased incorporation among previously unincorporated young, but had no impact on the corresponding old. The impact on the exit from the incorporated is different across these groups. The reforms had a positive and significant effects on the exit rate of the young incorporated self-employed, but had no appreciably significant effect on that among those who are old. The intuition behind these findings is similar to that for whites versus minorities. For life-cycle reasons, the young will be generally less wealthy, and thus less able to sustain unproductive businesses.

3.5 Conclusion

Why people engage in entrepreneurship is a puzzle for researchers. Entrepreneurship is a risky activity with low expected return, and only a small number of people choose to engage in entrepreneurship. Researchers have argued that access to credit is a major factor that can explain low rates of entrepreneurship. This chapter assesses if there is a causal link between credit and entrepreneurship. We focus on the effects of a major change in financial policy, the ability of banks to own and operate multiple branches in multiple jurisdictions in the US. Using the micro-level data from the Current Population Surveys (CPS) over the 1980–2007 period, we investigated how branching reforms affected entrepreneurship at individual owner-level.

Our analysis yields several interesting findings. First, changes in credit affects entrepreneurship. We find that entry and exit rates of the incorporated self-employed increased

after bank expansions. Second, we find that branching reforms encouraged unincorporated self-employed individuals to incorporate. Finally, the effects of banking deregulation are different across groups. Particularly, we find stronger effects on incorporated business creation among minorities, and higher exit rates among the young and minorities –two groups likely to face binding credit constraints.

Chapter 4. The Impact of Bankruptcy on Entrepreneurship

4.1 Introduction

In financial markets a common market imperfection is that credit arrangements suffer from problems of limited enforcement. Creditors may lend to borrowers who are not fully willing or able to repay debts. Borrowers may enter into credit contracts they are not fully able to repay, due to unexpected negative economic outcomes. Bankruptcy is a legal process through which borrowers that have defaulted on debt, either restructure or default on existing credit agreements. Consequently these policies seek to influence the degree to which limited enforcement problems occur.

The strength and weakness of bankruptcy laws themselves can alter the nature of credit frictions, which in turn effects households and firms. These policies may have significant implications for the rates of entrepreneurship in an economy. Lenient bankruptcy procedures may create moral hazard, and encourages borrowers to default on debt contracts. Adverse selection in these markets may then lead to a reduction in the availability of credit, which is of significant concern to entrepreneurs. For example, Berkowitz and White (2004) show that U.S. states with higher bankruptcy exemptions have higher interest rates and loan denial rates for small businesses. Credit access remains a central challenge for many small businesses. A recent survey conducted by Federal Reserve suggests that nearly a third of the self-employed firms report credit access as their main financial challenge, irrespective of the firm's revenue. However, when there is a high degree uncertainty about future economic status, a lenient bankruptcy policy creates risk sharing between borrowers and lenders. Entrepreneurship is a risky economic activity, where BLS reports that nearly forty percent of new U.S. establishments do not survive four years of continuous operation. Fan and White (2003) show that there is more entrepreneurship in states with higher bankruptcy exemptions due to partial wealth insurance.

In this chapter, we study how a major change in the U.S. Bankruptcy code has effected small business entrepreneurship. we study how Bankruptcy Abuse Prevention and Consumer Protecion Act, dubbed BAPCA, has impacted the self-employed. This policy strengthened the bankruptcy code in favor of creditors and increased the costs of credit default for borrowers. A main difference between the analysis here and existing studies, such as Paik (2013), is that we consider an individual entrepreneur's need for financing based on their particular industry. We use the March U.S. Current Population Survey and classify the self-employed according to their two digit Standard Industry Classification (SIC) code. We then match these codes a to a common measure of external finance first proposed by Rajan and Zingales (1998). We then explore how BAPCA effected entry and exit rates into self-employment, based on their industry's need for external financing. We further exploit the rich nature of CPS data to explore the effects of BAPCA on sub-samples based on race, gender or age.

The major finding of this chapter is that the passage of BAPCA seems to have decreased entry rates to unincorporated entrepreneurship. Incorporated businesses are protected by limited liability, which separates a firm's financial assets from its owners. Consequently, incorporation provides the self-employed with a degree of partial wealth insurance. This suggests that BAPCA, which reduced the degree of risk sharing between borrowers and lenders had pronounced effects, with fewer financial protections in the event of a default. This is further supported by evidence that BAPCA's effects were strongest in states which offer lower degree of bankruptcy exemptions. Finally, in states which offer greater bankruptcy protections, BAPCA led to a reduction in the entry rate among mostly self-employed individuals who were in industries with a greater need for financing.

This chapter relates to multiple areas of literature. The first focuses on the determinants of entrepreneurship. This literature seeks to understand what drives individuals to enter into entrepreneurship given its risky nature, and has been covered in detail in the previous chapter (Sarker and Unel, 2017). This study relates most closely to literature that seeks to understand whether credit is a key determinant, where Buera (2015) provides a recent

survey.

This chapter is also related to an emerging literature on BAPCA. A number of works have studied the impacts of this policy on household credit default rates, especially in mortgage markets, which had implications for the 2009 financial crisis (Li et al. 2011, Mitman 2016). The present work relates to Paik (2013), who treats BAPCA as a one time shock and studies entry rates into self-employment. Using logistic regression, he finds that BAPCA encouraged unincorporated firms to incorporate. The present study differs in a number of key aspects.

We consider how BAPCA effects entrepreneurship based on an individual's need for credit. We classify the self-employed into SIC industry categories in order to consider a particular industry's need for external finance. We then study both entry and exit rates. Furthermore, we consider how the degree of bankruptcy protection offered across different states influence the impact of the policy. Last, we explore how BAPCA impacted different subgroups of the population. We consider the effects of the reform on people of different races, gender and age.

The rest of this chapter is organized as follows. The next section reviews bankruptcy policy in the US and summarizes the changes to the code made by BAPCA. Section 3 discusses the data used in this chapter and construction of key variables. Section 4 describes the econometric methodology employed in this chapter. Section 5 presents the results of this analysis, and section 6 concludes.

4.2 Background on Bankruptcy Reform in the U.S.

In the U.S., if an individual declares bankruptcy he or she can file for one of two personal bankruptcy procedures, either chapter 7 or chapter 13. When either type of bankruptcy is filed, creditors are required to stop collecting debt. Under a chapter 7 procedure, an individual surrenders their non-exempt [1] assets to a trustee, their debts are discharged, and future earnings are protected from collection. The trustee liquidates surrendered assets and

[1]Individual states can exempt certain types of financial assets from bankruptcy. Most states exempt a portion of home-equity which is discussed later in the chapter.

repays creditors. In a chapter 13 bankruptcy procedure, individuals are not obligated to surrender financial assets and instead are required to repay a portion of their debt from future earnings over a span of three to five years. Individuals are required to propose a plan which offers repayment equivalent to what they would pay under a chapter 7 procedure and their remaining debts are only discharged if they repay debts. (see Fay et al. 2002,Berkowitz and White 2004, Paik 2013).

Because a chapter 7 bankruptcy protects future earnings and discharges debts, it is referred to as a 'fresh start'. Individuals typically filing for bankruptcy have greater incentive to select this type of bankruptcy procedure, unless they have significant amounts of financial assets. Indeed, approximately 70% of bankruptcies are chapter 7 bankruptcies and 95% of filers have no non-exempt assets to surrender for repayment to the credit (Paik 2013).

The Bankruptcy Abuse Prevention and Consumer Protection Act of 2005, was a major policy reform of the existing U.S. bankruptcy code. It introduced three major changes to the code. First, it placed income restrictions on individuals wishing to file for chapter 7 bankruptcy. Second, it lengthened the time which a debtor must wait before filing for a successive bankruptcy. Third, it placed geographic restrictions on exemptions debtors can use in a chapter 7 bankruptcy procedure in the cases where a debtor recently moved to another state.

The new policy introduced income restrictions by requiring that debtors pass a series of complex means test which are designed to prevent high income borrowers from filing for a chapter 7 bankruptcy. These tests are effectively used to demonstrate that their average, size adjusted, family income for six months preceding the filing is below their state's median monthly family income level (Paik 2013). If debtors failed the means test, their case is either dismissed or converted to a chapter 13 reorganization plan with the debtors consent.

BAPCA also increased the length of time which a debtor must wait before they are eligible to file for another bankruptcy. Under the new law, a debtor who has filed for a chapter 7 bankruptcy must wait 8 years, rather than 6 years, before filing for a second

chapter 7 bankruptcy. Furthermore, the debtor must wait 4 years before filing a chapter 13 bankruptcy, whereas prior to the reform they could file almost immediately. Debtors who have filed for a chapter 13 bankruptcy must wait at least 2 years before they are eligible to file for another chapter 13 bankruptcy; prior to the reform they only needed to wait 6 months.

As stated previously, a chapter 7 bankruptcy requires that a debtor surrenders his/her assets above an exemption level, the largest of which are home equity exemptions, called homestead exemptions. Here home equity refers to the fair market value of a property minus outstanding mortgage debt. The homestead is considered the primary residence of a person. If the value of home equity is below a state's exemption level, creditors cannot force the sale of a house. The size of the exemption varies widely, with some states offering virtually unlimited exemption levels.

This makes it attractive for debtors to move their primary residence to high-exemption states in order to protect their assets. The new policy reform introduced rules to discourage this behavior. If a debtor who is filing for chapter 7 bankruptcy moved their primary residence between different states within a two year period of their filing date, they are ineligible to use their current state of residence's homestead exemption. Instead, the debtor would use the exemption from whichever domicile they resided the longest for over a 6 month (180 day) period prior to two years before their filing date. The reform also set a maximum exemption for properties owned for less than three years and four months to 125,000$ irrespective of an individual's states homestead exemption level[2].

4.3 Data

The data used for this chapter is drawn from the March Current Population Survey covering the period from 2003–2008. Entrepreneurial activity is measured by changes in employment status to or from self-employment at the individual level. The CPS is a monthly cross-

[2]This threshold is lifted periodically by a Judicial Conference of the United States. As of 2010 the value is 146,450$

sectional survey conducted by the U.S. Census Bureau and is used to construct labor force statistics such as the unemployment rate. The March supplement is an annual survey which contains additional information about household demographics and economic conditions for both the current time period and the previous calendar year. We exploit this feature to identify individuals who either enter or exit from business ownership.

Within the CPS files workers are classified as working for either private sector, public sector, non-profit or as self-employed. We classify *entering* entrepreneurs as individuals who report that they are currently self-employed, whereas in the previous year they were private-sector (wage & salary) workers. Similarly, *exiting* entrepreneurs are those who report that they are currently private sector (wage & salary) workers, but were self-employed the previous calendar year. The self-employed workers are further classified into two categories: incorporated and unincorporated. This analysis will treat these groups as distinct, but also consider the effects of policy on all self-employed workers. A large number of academic studies have used all self-employed workers as a proxy for entrepreneurship (Borjas and Bronars 1989, Evans and Leighton 1989, Hamilton 2000 and others). However, a recent study by Levine and Rubinstein (2017) highlights that there are differences between incorporated self-employed workers and those who run unincorporated businesses. The incorporated self-employed are generally more educated, work longer hours and earn more per hour than traditional wage workers and unincorporated workers.

The sample considers all workers in the labor force (18-64 years old), and similar to the previous chapter, excludes those with imputed/missing worker class and inconsistent reports. We also exclude individuals who work in public administration sector since almost no entrepreneurial activity takes place in this sector. The sample contains information about each individual's age, gender, race, education level, marital status and industry worked. We reclassify industries according to two-digit SIC (Standard Industry Classification) codes.

Table 4.1 reports summary statistics for self-employed and wage & salary workers. Compared to wage & salary workers, a higher proportion of self-employed individuals are male,

Table 4.1 Summary Statistics on Self-employed and Wage Workers, 2002-2008

	Self-employed			Salary
	All	Incorporated	Unincorporated	Workers
Female (%)	37.1	28.2	39.5	49.2
	(47.8)	(45.0)	(48.7)	(49.9)
Age	44.6	45.5	44.3	39.9
	(10.0)	(9.4)	(7.9)	(11.3)
White (%)	86.7	88.1	86.9	80.8
	(33.11)	(32.3)	(34.4)	(39.4)
Some College (%)	63.8	72.6	58.5	58.1
	(48.2)	(44.5)	(49.3)	(49.3)
Sample Size	53,966	19,045	34,921	440,268

Notes: The data draw on the March CPS Files from Ipums (2017). Some College represents individuals who have at least some college education. Numbers in parentheses are standard deviations, and the CPS weights are used in all calculations.

white and more educated. Notice that the percentage of individuals who have at least some college education is quite comparable between unincorporated and traditional workers. In contrast, the incorporated self-employed workers are significantly more educated. In Table 4.2, we report entry (exit) rates into (from) both categories of entrepreneurship. Notice that there is a higher entry rate into unincorporated self-employment group than the incorporated group. Women are also less likely to start incorporated businesses. Finally, the incorporated self-employed have higher exit rates, than the unincorporated self employed.

Data on external finance is taken from Duygan-Bump et al. (2015). They calculate measures of external finance following an approach outlined in Rajan and Zingales (1998) and Ceterolli and Strahan (2006). Using the Compustat Database they identify firms which appear in the database for at least 10 years. They then calculate a measure of external finance which is the proportion of capital expenditures financed by external finance. For each firm they calculate a measure of free cash flow which is equal to the total capital expenditure (Compustat Item 128) minus the cash flow from operations (Compustat Item 110) summed over the 1980 to 2006 period. They divide this by the total capital expenditure. They then take the median value for each industry, categorized according to the two-digit

Table 4.2 Entrepreneurial Activity at Individual-Owner Level, 2002-2008

	Entry Rate (%)		Exit Rate (%)	
	Incorp.	Unincorp.	Incorp.	Unincorp.
All Sample	.59	.79	8.3	3.2
	(7.6)	(8.9)	(27.7)	(17.8)
Female	.40	.83	8.9	3.5
	(6.3)	(9.1)	(28.5)	(18.4)
White	.62	.82	8.0	3.2
	(7.8)	(9.0)	(27.2)	(17.7)
Some College	.71	.84	7.6	3.1
	(8.4)	(9.1)	(26.6)	(17.5)

Notes: The data draw on the March CPS Files from Ipums (2017). Some College represents individuals who have at least some college education. Numbers in parentheses are standard deviations, and the CPS weights are used in all calculations.

SIC code. Note that negative values indicate that firms have free cash flow and are not heavily dependent on external finance.

As a robustness check, we construct an alternative measure which is also considered in Ceterolli and Strahan (2006). We use data from the 1998 Survey of Small Business Finance (SSBF), which was conducted by the Federal Reserve. This is a nationally representative sample of small firms, defined as firms with fewer than 500 employees. For each two-digit SIC category, we take the loans to asset ratio for the firm at the median of the distribution. Loans here are mostly from commercial banks, credit unions and other thrifts. This measure captures bank dependence by smaller firms, as COMPUSTAT data reflects only large publicly traded firms. For consistency, we consider industry categories that are included in both data sets. As a result, the analysis here excludes forestry, fishing, tobacco, rail, oil pipelines, insurance and security brokerage. These industries consist mostly of larger firms and are unlikely to meaningfully alter the results, which focuses on the self-employed. Furthermore, Table 4.1 shows that some of these industries (e.g. forestry or insurance) have extremely large values of free cash flow, which can lead to confusing results. When these industry categories are omitted, the correlation between the two different measures is 50 percent.

4.3.1 Homestead Exemption

In bankruptcy proceedings, homestead exemptions are the amounts of equity that homeowners can keep if they declare bankruptcy. This exemption represents the largest portion of financial assets which are protected when a individual declares a chapter 7 bankruptcy (Fan and White, 2003). The exemptions are determined at a state level, and the amount of equity that homeowners can exempt vary widely from state to state. For example, Florida offers a unlimited exemption, while Maryland offers no exemption. Furthermore, exemption levels may vary based on marital status, the number of children, and a person's age. Since the CPS identifies these characteristics, we construct a variable that identifies the homestead exemption level for each individual within our sample [3]. Data on homestead exemption level for each case prior to the passage of BAPCA is taken from Elias (2009). Note that a portion of BAPCA reform limited the homestead exemption in most circumstances to 125,000$, so we use this value for states with exemption levels higher than 125,000$ once the reform is in effect.

4.4 Econometric Specification

We use the following linear probability model to investigate the effects of BAPCA on entrepreneurship:

$$Y_{isjt} = \alpha + \beta \text{Bapca}_t + \delta \text{Bapca}_t \times \text{EFD}_j + \text{HS}_{ist} + \gamma X_{ist} + \eta_j + \varepsilon_{isjt}, \tag{4.1}$$

where Y measures either entry or exit of entrepreneurs at individual level as in the previous chapter. Bapca_t is a dummy variable that identifies if the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005 is in effect in year t. The dummy variable equals zero up to the year the law is introduced, and one afterward. EFD is a measure of external finance dependence in industry j, and HS denotes homestead exemption in state s for individual i. Our coefficient of interest is β.

[3]Note that we report values in millions of dollars. This is to avoid overly small point estimates. A value of .125 would correspond to 125,000 $.

As in Chapter 2, X_{ist} is the set of observed covariates including dummies for gender, marital status, race, education, and a quadratic for age. State and industry fixed effects (η_s and η_j) are included to control for any time invariant state- and industry-specific factors that can affect entrepreneurship. Heteroskedasticity robust standard errors clustered at the state level are used to mitigate the potential serial correlation in the error term. All regressions are weighted by the CPS individual-level weights.

4.5 Results

4.5.1 Benchmark

Table 4.3 reports the effects of the bankruptcy reform on the entry and exit of incorporated, unincorporated and all self-employed, based on the regression specification. Regression includes all controls, state fixed effects, industry fixed effects and is weighted according to CPS weights. Column 2 reports that after the passage of BAPCA there is a decline in the probability that a traditional worker (i.e. wage and salary worker) enters into unincorporated entrepreneurship, which is statistically significant at the one percent level. Since the entry rate into unincorporated entrepreneurship is 0.79 percent, the coefficient estimate of -.00101 reflects a 12 percent decline in the entry rate. Note that column 2 also shows that BAPCA led to a relatively higher decline for firms with greater needs for external finance, but this effect is only statistically significant at the 10 percent level. The results indicate that BAPCA did not have an economically meaningful impact on exit rates of any category of self-employed workers, and no effect on the entry of incorporated workers.

Table 4.2 Impact of Bankruptcy Reform (2003–2008)

	Entry		Exit from		All	
	Incorporated	Uninc.	Incorporated	Uninc.	Entry	Exit
Variable	1	2	3	4	5	6
Bapca	−0.00009	−0.00101***	−0.00715	0.00192	−0.00108*	−0.00079
	(0.00039)	(0.00034)	(0.00517)	(0.00237)	(0.00060)	(0.00235)
Bapca × EFD	−0.00022	−0.00094*	0.01610	−0.00195	−0.00113	0.00529
	(0.00104)	(0.000508)	(0.0122)	(0.00709)	(0.00111)	(0.00672)
Age	0.00035***	0.00011	−0.00206	−0.00374***	0.00045***	−0.00244***
	(0.00010)	(0.00010)	(0.00224	(0.00091)	(0.000140)	(0.00091)
Age^2	−0.00000*	−0.00001	0.00001	0.00003***	−0.00001**	0.000013
	(0.000001)	(0.00000)	(0.00002)	(0.00001)	(0.00000)	(0.00000)
Female	−0.00316***	−0.00005	−0.00251	0.00249	−0.00317***	−0.00580
	(0.000435)	(0.00026)	(0.00753)	(0.00332)	(0.00048)	(0.00400)
1em] Marital status	0.00254***	0.00042	−0.00806	−0.0120***	0.00292***	−0.00469
	(0.00034)	(0.00026)	(0.00688)	(0.00217)	(0.000398)	(0.00293)
HS	−0.00025	−0.00016	−0.00614	−0.00188	−0.00041	−0.00373**
	(0.00035)	(0.00023)	(0.00459)	(0.00124)	(0.00039)	(0.00153)
Obs.	419,676	419,665	17,783	32,049	421,834	49,915

Notes: Numbers in parentheses are robust standard errors clustered at the state level, and ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively. Controls not reported for brevity.

This result makes sense, as corporations have limited liability which separates a firm's debt from its owner. Thus the incorporated self-employed are somewhat shielded against creditors in the event of a business failure.

Table 4.4 Impact of Bankruptcy Reform, by Homestead Exemption (2003–2008)

	Entry		Exit from		All	
	Incorporated	Uninc.	Incorporated	Uninc.	Entry	Exit
Variable	1	2	3	4	5	6
Panel A. Below Median Level						
Bapca	−0.000076	−0.00124***	−0.01340**	0.00110	−0.00130**	−0.00434
	(0.00032)	(0.00032)	(0.00614)	(0.00382)	(0.00049)	(0.00311)
Bapca × EFD	0.00034	−0.00014	0.02680	0.00069	0.00021	0.01170
	(0.00083)	(0.00079)	(0.02110)	(0.00893)	(0.00113)	(0.00945)
Obs.	213,782	213,908	8,278	14,996	214,848	23,315
Panel B. Above Median Level						
Bapca	−0.00017	−0.00065	0.00160	0.00335	−0.00080	0.00398
	(0.000922)	(0.000621)	(0.00776)	(0.00282)	(0.00123)	(0.00342)
Bapca × EFD	−0.000875	−0.00191***	0.00463	−0.00574	−0.00274	−0.00196
	(0.00204)	(0.000564)	(0.0129)	(0.0111)	(0.00197)	(0.00694)
Obs.	203,376	203,235	9,414	16,869	204,456	26,324

Notes: Numbers in parentheses are robust standard errors clustered at the state level, and ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively. Controls not reported for brevity.

Table 4.4 reports the impact of policy based on a states homestead exemption level. As stated previously, the homestead exemption reflects the degree an individual's home-equity is protected from bankruptcy. Individuals who reside in states with high exemptions can potentially shield themselves from a loss of financial assets by investing into their home-equity and avoid some of the costs associated with bankruptcy. Thus the effect of bankruptcy reform could potentially be different depending on the exemption level an individual faces. In order to investigate this issue we examine how the policy reform affected entrepreneurship based on whether an individual faces a high or a low exemption level. Here we divide the

sample based on the median exemption level prior to the passage of BAPCA. States with higher exemption levels offer a greater degree of bankruptcy protection against creditors, which reflects a more lenient credit enforcement regime.

Here, the top panel reports the estimates for states with lower exemption levels. Column 2 shows that the effects of BAPCA on the unincorporated are concentrated in states with a lower degree of homestead protection. This suggests that strengthening of the bankruptcy code had a larger negative effect on entrepreneurship in states that offer less bankruptcy protection. Note that this effect is large enough to drive a decline in the overall entry rate into self-employment as reported in column 5.

Another striking result is that in these states BAPCA seems also to have led to a decline in the exit rate from incorporated business ownership. The coefficient estimate of -.01340 suggests that BAPCA has led to a 16 percent decline in the exit rate. Because corporations are protected by limited liability, the incorporated self-employed are offered a higher level of protection when entering a credit contract. In states with relatively weak homestead exemption, a further tightening of personal bankruptcy procedures have increased incentives for the incorporated self-employed to maintain their incorporated status.

The bottom panel of the table reports the effects on states with higher exemption levels. The direct effect of the reform on both entry and exit rates are statistically insignificant. However, column 2 suggests that BAPCA reduced the rate of entry into unincorporated entrepreneurship in industries with relatively higher needs of external finance. This implies that in states with lenient credit enforcement policies, a national tightening of the bankruptcy code lead to a reduction in entrepreneurship in sectors which are more likely to require external borrowing. This is consistent with the idea that a reduction in risk sharing due to the change in the policy leads to less entrepreneurship.

4.5.2 Effects by Race, Gender and Age

Several studies have documented that credit access, that is the ability to enter into credit contracts, differs greatly depending on age, race and gender (Fairlie and Robb, 2008). In

one example, Asiedu et al. (2012) show that the denial rates of loan applications is about 30 percent higher for minority-owned firms compared to white males. In the previous chapter of this dissertation (Sarker and Unel, 2019) it was shown that credit supply increases due to banking deregulation have stronger effects on entrepreneurship rates among young workers and minorities. Thus bankruptcy policy which alters the nature of credit contracts may have different effects on different segments of the population.

Table 4.5 Impact of Bankruptcy Reform by Race (2003–2008)

	Entry		Exit from		All	
	Incorporated	Uninc.	Incorporated	Uninc.	Entry	Exit
Variable	1	2	3	4	5	6
Panel A. Whites						
Bapca	−0.00017	−0.00110***	−0.00614	0.00161	−0.00128**	−0.00062
	(0.00041)	(0.00035)	(0.00569)	(0.00284)	(0.00062)	(0.00259)
Bapca × EFD	−0.00046	−0.00088	0.01350	0.00229	−0.00132	0.00630
	(0.00119)	(0.00068)	(0.01250)	(0.00668)	(0.00122)	(0.00631)
Obs.	341,773	341,760	15,723	27,697	43,491	26,324
Panel B. Non-whites						
Bapca	0.00018	−0.00039	−0.01800	0.00321	−0.00021	−0.00026
	(0.00070)	(0.000542)	(0.01280)	(0.00468)	(0.00079)	(0.00531)
Bapca × EFD	0.00114	−0.00113	0.03660	−0.03690*	0.00001	−0.00652
	(0.00118)	(0.00127)	(0.05320)	(0.01950)	(0.00169)	(0.02150)
Obs.	77,903	77,905	2,060	4,352	4,352	6,424

Notes: Numbers in parentheses are robust standard errors clustered at the state level, and ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively. Controls not reported for brevity.

To explore this possibility we exploit the rich nature of the CPS. In Table 4.5, we study how BAPCA effects entrepreneurship based on an individual's race. Panel A. of the table shows the effects on whites. Column 2, reports that their is a decline in the entry rate into unincorporated entrepreneurship. This result is consistent with the previous table, but suggests BAPCA mostly impacted Whites. There appears to be no real impact from the policy on the entry and exit rates into entrepreneurship of minority workers. This finding is

Table 4.6 Impact of Bankruptcy Reform by Sex (2003–2008)

	Entry		Exit from		All	
	Incorporated	Uninc.	Incorporated	Uninc.	Entry	Exit
Variable	1	2	3	4	5	6
Panel A. Males						
Bapca	−0.00050	−0.00113**	−0.00648	−0.000733	−0.00159**	−0.00442
	(0.00050)	(0.00043)	(0.00683)	(0.00317)	(0.000692)	(0.00388)
Bapca × EFD	−0.000002	−0.00097	0.01750	0.00593	−0.00096	0.01660*
	(0.00157)	(0.00095)	(0.0153)	(0.01000)	(0.00203)	(0.00938)
Obs.	214,797	214,447	12,898	18,890	215,980	31,838
Panel B. Female						
Bapca	0.00035	−0.00089*	−0.00369	0.00247	−0.00054	0.00128
	(0.00033)	(0.00046)	(0.00863)	(0.00296)	(0.00060)	(0.00263)
Bapca × EFD	−0.00009	−0.00084	0.00652	−0.00712	−0.00091	−0.00702
	(0.00072)	(0.00083)	(0.02710)	(0.01540)	(0.00096)	(0.01410)
Obs.	204,879	205,218	4,885	13,159	205,854	18,077

Notes: Numbers in parentheses are robust standard errors clustered at the state level, and ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively. Controls not reported for brevity.

by and large consistent with documented facts about minorities. Blacks, the second, largest minority group, have significantly lower access to startup capital and loans. Consequently, black entrepreneurship is less likely to be effected by changes in bankruptcy policy.

Table 4.6 explores the effect of the reform based on Gender. The top panel reports results for males and the bottom panel for females. Consistent with the benchmark result, bankruptcy reform only has an effect on the entry rate into unincorporated self-employment. This effect is stronger for male workers with a coefficient that is statistically significant at the 5 percent level. For females the coefficient estimate of -.00089 is smaller than the benchmark, and the effect is statistically significant only at the 10 percent level. However, this reflects a 10.8 percent decline in the entry rate (.82 percent) for females.

Table 4.6 reports the effects of the reforms based on Age. We divide the sample of workers into two groups. Workers under the age of forty are categorized as young. Those

older than forty are classified as old. The top panel reports the effects of reform on young worker. The results indicate that BAPCA mostly effected the entry rate of young workers into unincorporated business ownership. Consistent with the benchmark estimates the

Table 4.7 Impact of Bankruptcy Reform by Age (2003–2008)

	Entry		Exit from		All	
	Incorporated	Uninc.	Incorporated	Uninc.	Entry	Exit
Variable	1	2	3	4	5	6
Panel A. Young (Age < 40)						
Bapca	0.00011	−0.00131***	−0.0147	0.00224	−0.00118*	−0.00212
	(0.00035)	(0.00043)	(0.0131)	(0.00575)	(0.000644)	(0.00530)
Bapca × EFD	−0.00013	−0.00110	0.0389	−0.00494	−0.00121	0.00740
	(0.00082)	(0.00095)	(0.02360)	(0.01530)	(0.00121)	(0.01300)
Obs.	200,788	201,155	4,739	10,415	201,887	15,186
Panel B. Old (Age ⩾ 40)						
Bapca	−0.00043	−0.00067	−0.00796	0.00133	−0.00108	−0.00158
	(0.00054)	(0.00047)	(0.0061)	(0.0023)	(0.00073)	(0.00289)
Bapca × EFD	−0.00109	−0.00083	0.01340	−0.000485	−0.00189	0.00538
	(0.00172)	(0.00091)	(0.0145)	(0.00641)	(0.00158)	(0.00724)
Obs.	206,142	205,777	12,435	20,585	207,129	33,066

Notes: Numbers in parentheses are robust standard errors clustered at the state level, and ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively. Controls not reported for brevity.

policy has no effect on exit rates or entry rate into incorporated entrepreneurship. The bottom panel shows the effects of BAPCA on older workers, the policy appears to have no effect on either entry or exit rates among this group.

4.6 Conclusion

High rates of entrepreneurship are seen as a sign of economic progress because of its effects on competition, innovation, employment and economic growth. However, at the individual level entrepreneurship remains a risky activity, subsequently credit default becomes a means of insurance in the event of poor economic outcomes. The bankruptcy code is a legal procedure through which credit defaults occur or credit contracts are restructured. Consequently,

the strictness or leniency of these laws impact the degree of risk sharing between borrowers and lenders in risky entrepreneurial projects. In this chapter we have studied how a change in the bankruptcy code, one which reduces the ability of borrowers to default, effects entrepreneurship.

The analysis presented in this chapter suggests that BAPCA reduced the likelihood that individuals start unincorporated businesses. As the unincorporated self-employed are not protected by limited liability, the tightening of the bankruptcy code results in a decline risk sharing for this group. The estimates here show that in the U.S., states which offer a greater degree of protection for the borrower's assets through homestead exemptions, the tightening of the bankruptcy code appears to have a greater impact on self-employed businesses with relatively higher needs for external borrowing. Finally, in the states with lower homestead exemptions, there seems to be a reduction in the exit rates of incorporated businesses. This suggests that in states in which BAPCA has a larger negative impact on borrowers, individuals are more likely to maintain incorporation status as a form of insurance. Incorporation separates a firm's assets from the owner's assets, thus offers a greater degree of financial protection in the event of a credit default.

Appendix. Supplementary Tables for Chapter 3

Table A.1. Impact of Bank Deregulations on Entrepreneurship, 1980–1993

	Incorporated					
	Entry from		Exit to		Unincorporated	
Variable	Non-Bus.	Uninc.	Non-Bus.	Uninc.	Entry	Exit
Intra	−0.0000	0.0065	0.0301*	0.0551***	0.0009	0.0029
	(0.0007)	(0.0086)	(0.0161)	(0.0129)	(0.0014)	(0.0096)
Inter	0.0027***	0.0044	−0.0015	0.0390**	0.0001	0.0011
	(0.0007)	(0.0076)	(0.0153)	(0.0175)	(0.0011)	(0.0072)
Obs.	472,413	49,059	17,006	17,357	479,461	54,055

Notes: All regressions include all fixed effects and controls specified in equation (3); and regressions are weighted by the CPS weights. Numbers in parentheses are the robust standard errors clustered at the state level, and ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively.

Table A.2. Impact of Bank Deregulations on Entrepreneurship (1980–2007), by Gender

	Incorporated					
	Entry from		Exit to		Unincorporated	
Variable	Non-Bus.	Uninc.	Non-Bus.	Uninc.	Entry	Exit
Panel A. Females						
Intra	0.0004	−0.0062	−0.0014	−0.0002	−0.0004	−0.0020
	(0.0004)	(0.0080)	(0.0249)	(0.0324)	(0.0012)	(0.0125)
Inter	0.0012**	0.0040	−0.0086	0.0658*	−0.0010	0.0002
	(0.0006)	(0.0066)	(0.0360)	(0.0378)	(0.0016)	(0.0145)
Obs.	445,537	26,168	8,319	7,329	450,977	32,371
Panel B. Males						
Intra	0.0010	0.0046	0.0110	0.0197*	0.0013	−0.0029
	(0.0008)	(0.0062)	(0.0115)	(0.0111)	(0.0013)	(0.0083)
Inter	0.0035***	0.0039	0.0086	0.0332**	−0.0004	−0.0019
	(0.0011)	(0.0062)	(0.0156)	(0.0135)	(0.0012)	(0.0070)
Obs.	484,628	58,012	27,755	27,061	490,836	62,659

Notes: All regressions include all fixed effects and controls specified in equation (3); and regressions are weighted by the CPS weights. Numbers in parentheses are the robust standard errors clustered at the state level, and ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively.

Table A.3. Impact of Bank Deregulations on Entrepreneurship (1980–2007), by Race

	Incorporated					
	Entry from		Exit to		Unincorporated	
Variable	Non-Bus.	Uninc.	Non-Bus.	Uninc.	Entry	Exit
Panel A. Non-whites						
Intra	0.0003	0.0237	0.1534*	0.0132	−0.0037*	0.0782**
	(0.0010)	(0.0277)	(0.0776)	(0.0600)	(0.0021)	(0.0320)
Inter	0.0037***	−0.0329	0.1227*	−0.0026	0.0035	0.0520**
	(0.0013)	(0.0248)	(0.0612)	(0.0765)	(0.0022)	(0.0254)
Obs.	117,595	5,179	2,121	1,973	118,891	6,321
Panel B. Whites						
Intra	0.0008	−0.0003	0.0058	0.0158	0.0011	−0.0081
	(0.0006)	(0.0054)	(0.0105)	(0.0118)	(0.0011)	(0.0067)
Inter	0.0022***	0.0061	0.0021	0.0435***	−0.0015	−0.0054
	(0.0008)	(0.0053)	(0.0131)	(0.0136)	(0.0012)	(0.0061)
Obs.	812,570	79,001	33,953	32,417	822,922	88,709

Notes: All regressions include all fixed effects and controls specified in equation (3); and regressions are weighted by the CPS weights. Numbers in parentheses are the robust standard errors clustered at the state level, and ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively.

Table A.4. Impact of Bank Deregulations on Entrepreneurship (1980–2007), by Age

	Incorporated				Unincorporated	
	Entry from		Exit to			
Variable	Non-Bus.	Uninc.	Non-Bus.	Uninc.	Entry	Exit
Panel A. Young (Age < 40*)*						
Intra	−0.0007	0.0044	0.0375	0.0227	0.0000	0.0091
	(0.0008)	(0.0063)	(0.0258)	(0.0245)	(0.0016)	(0.0096)
Inter	0.0024***	0.0079	0.0216	0.0664***	−0.0020	0.0043
	(0.0007)	(0.0072)	(0.0192)	(0.0243)	(0.0012)	(0.0097)
Obs.	447,994	32,193	11,432	10,688	454,541	37,907
Panel B. Old (Age $\geqslant 40$*)*						
Intra	0.0027***	−0.0021	−0.0066	0.0161	0.0011	−0.0140*
	(0.0008)	(0.0073)	(0.0154)	(0.0155)	(0.0010)	(0.0070)
Inter	0.0024**	−0.0003	−0.0025	0.0244	0.0010	−0.0101
	(0.0009)	(0.0071)	(0.0170)	(0.0157)	(0.0013)	(0.0100)
Obs.	482,171	51,987	24,642	23,702	487,272	57,123

Notes: All regressions include all fixed effects and controls specified in equation (3); and regressions are weighted by the CPS weights. Numbers in parentheses are the robust standard errors clustered at the state level, and ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively.

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