Credit Constraints and Demand for Higher Education: Evidence from Financial Deregulation

Teng Sun, Stephen and Yannelis, Constantine

Stanford University, Stanford University

30 July 2013

Credit Constraints and Demand for Higher Education: Evidence from Financial Deregulation*

Stephen Teng Sun† Constantine Yannelis‡

July 2013

PRELIMINARY AND INCOMPLETE – NOT FOR CIRCULATION OR CITATION

Abstract

This paper uses staggered bank branching deregulation across states in the United States to examine the impact of the resulting increase in the supply of credit on college enrollment from the 70s to early 90s. A significant advantage of our research design is that it produces estimates that are not confounded by wealth effects. We find that lifting branching restrictions raises college enrollment by about 2 percentage points (4%). Our results rule out alternative interpretations to the credit constraints channel. First, the effects are largest for low and middle income families, while insignificant for upper income families as well as bankrupt families who would have been unaffected by the increased access to private credit. Second, the effect of lifting branching restrictions subsided immediately following periods of increased loan limit through government student loan programs. We also show that household educational borrowing increased as a result of lifting branching restrictions. Our results provide novel evidence that credit constraints play an important role in determining household college enrollment decisions in the United States.

*We especially wish to thank Caroline Hoxby for guidance and helpful comments. We also thank Nick Bloom, Tim Bresnahan, Brianna Cardiff, Andres Drenik, Pascaline Dupas, Dan Grodzicki, Davide Malacrino, Fred Panier, Luigi Pistaferri and seminar participants at Stanford University, the SAET conference at École des Mines and the China Meeting of the Econometric Society for helpful comments. We also wish to thank Nicole Fortin for making data publicly available. Constantine Yannelis is grateful to the Alexander S. Onassis foundation for generous funding.

†Department of Economics, Stanford University, 579 Serra Mall, Stanford, CA 94305-6072. doubles@stanford.edu.

‡Department of Economics, Stanford University, 579 Serra Mall, Stanford, CA 94305-6072. yannelis@stanford.edu.

1 Introduction

Human capital is the ultimate force that drives technological innovation and economic development, and post-secondary education plays a crucial role in fostering human capital accumulation. With an increasing wage gap between college and non-college educated workers driven by skill-biased technical change, enrollment in post-secondary educational institutions affects social mobility and the distribution of income. Economists have long recognized that credit constraints, a particular form of frictions resulting from incomplete markets, could affect an individual's investment in human capital by distorting incentives and limiting the ability to invest in education.[1]

However, analyzing the effect of credit constraints on demand for higher education has presented substantial empirical challenges. While there is clear evidence that family income is strongly correlated with college enrollment in the US, this fact is also consistent with children from low-income families having long-term educational disadvantages because they could attend lower quality primary and secondary schools and have access to fewer educational resources. Furthermore, earlier works that attempt to identify the effects of credit constraints are often confounded by the presence of wealth effects, as if education is a consumption good wealthier families may increase college enrollment for reasons unrelated to credit access.

This paper aims to examine the causal relationship between credit constraints and demand for college education by exploiting exogenous increases in credit available to households coming from staggered financial deregulation. While many existing studies have focused on student aid, family income or wealth, our paper complements the current literature by considering another form of financial resources available to households–credit from commercial banks. Several recent studies have highlighted the importance of federal aid and loans on college enrollment. Brown, Scholz, and Seshadri (2011) find that financial aid increases the educational attainment of children whose families would typically underinvest in education. Bettinger, Long, Oreopoulos, and Sanbonmatsu (2012) also find that assistance in filling out applications for federal student aid and loans increases college enrollment. Bond and Turner (2002) find evidence that the G.I. bill, which financed college education for returning soldiers, increased college enrollment for veterans substantially. Relaxing liquidity constraints for parents is fundamentally different from grant aid, as it does not involve a subsidy. Increasing subsidies may result in low ability student enrolling and dropping out of college, for example, see Stinebrickner and Stinebrickner (2011). However, when liquidity constraints are relaxed as a result of increased credit, funds are not given directly to students who enroll in college. Rather, unconstrained students are able to determine whether or not education investments are sensible given their abilities.

While direct private student loans from banks to students were virtually non-existent before 1996,[2] private borrowing has been and continues to be a significant means of financing post-

[1] See, among others, Becker, Duesenberry, and Okun (1960); Becker (1975); Schultz (1961); Friedman (1962).

[2] Knapp and Seaks (1992) indicate that the lack of direct private student loans is due to the high risk involved. See McSwain, Price, and Cunningham (2006) for a discussion of direct private student loans, which began in earnest in the mid-1990s and increased greatly after 2005 when holders of student loan debt were prohibited from defaulting on private student loans.

secondary education through parental contributions towards their children's tuition and living expenses. In the US, several channels of support are available to relieve the financial burden of college education for families.[3] It is important to note that these loans or aid programs have various caps and restrictions. As a result, many families turn to the private market after exhausting the limits. Families with good credit scores might also find it cheaper to borrow on the private market. During the period of financial deregulation, a significant portion of college students relied on private loans for the gap between college costs and financial resources available from grants and their families.[4]

The staggered deregulation of the banking industry across states in the United States provides us with a natural setting for identifying the effect of credit constraints on demand for college education. Starting from the late 1970s to the mid 1990s, most states deregulated the banking industry by allowing banks to open branches within and across state borders. As argued in Jayaratne and Strahan (1996), states did not deregulate their banks in anticipation of future good growth prospects. Krozner and Strahan (1999) provide a detailed analysis of the economic and political reasons for the exact timing of state branching deregulations. We therefore exploit the cross-state, cross-time exogenous variations in credit available to households from the banking deregulation to examine the causal impact of credit constraints on individual level college enrollment decisions. Following Jayaratne and Strahan (1996), several studies have documented that bank branch deregulation leads to an improvement in the efficiency of the banking industry. In particular, Dick and Lehnert (2010) find that deregulation increased competition among lenders, allowing previously excluded households to enter the market and receive loans. We argue that households residing in states which deregulated the banking industry benefited from increased credit supply and were more likely to send their children to college. We use data from Federal Housing Finance Association (FHFA) and Federal Reserve to show that the banking deregulation was accompanied by higher loan volume, lower bank fees and mortgage loan interest rates. In order to provide direct evidence on loan for college enrollment, we use data from the National Longitudinal Survey of Youth (NLSY) 1979. Our results indicate that the deregulation led to a significant increase in the use of loans to finance college.

We test for the empirical significance of deregulation on college enrollment with data from the Panel Study of Income Dynamics (PSID),[5] and find that college enrollment increases significantly in response to an increase in available credit after the banking deregulation. Using both micro data and state level aggregates, we estimate that college enrollment increased by roughly 2.2 percentage points (4.1%) following the lifting of branching restrictions, out of an average college enrollment rate of 54 percentage points in our sample. The access to increased credit following the branch

[3] Federal and state aid such as Pell Grants targeted at low-income families, federal government provided loans including Stafford Loans, at the same time private and public institutions have their own grant aid programs.

[4] Choy, Henke, and Schmitt (1992) document that, between 1986 and 1987, 63% of students were financially dependent on their parents and that 14% of students had parents using loans to provide support. In particular, 11% of students' parents took non-federal, state or institutional loans such as signature loans, home equity loans, lines of credit or loans against life insurance policies. Stiglitz, Tyson, Orszag, and Orszag (2000) also note that 7% of families took out a second mortgage to finance college in 1993.

[5] We also present results from NLSY 79, which are of similar magnitude.

deregulation accounts for 20% of the total increase in college enrollments between 1972 and 1992. This result is robust to a number of alternative specifications and adding various controls, including changes in the state level college wage premium. Among all potential interpretations of this causal effect, we argue that the increase in college enrollment following the branching deregulation is due to the relaxation of credit constraints, rather than an increase in expected return to college education from state-level financial development. Consistent with a credit constraints interpretation, the observed effect is largest among lower and middle income families. We observe no significant effects of branching deregulation for upper income families, while the college enrollment rates for these households increased from 56% to 64% in our sample. We also examine a group of households that are excluded from private credit markets due to parents having gone through bankruptcy as a placebo test. Our finding that there is no significant effect for this group of households again points toward a credit constraints interpretation, rather than a returns to education interpretation. We also find a smaller effect of lifting branching restrictions immediately following increased availability of credit through Guaranteed Student Loans. We also provide a number of sensitivity checks. These empirical findings are consistent with credit constraints being the primary mechanism through which this financial deregulation affected enrollments.

Credit constraints have drawn considerable attention in the education literature. However, so far no concensus has been reached regarding the empirical importance of credit constraints on college enrollment. Controlling for ability proxied by Armed Forces Qualification Test (AFQT) scores, Carneiro and Heckman (2002) find that a positive but small relationship between family income and college attendance using National Longitudinal Survey of Youth (NLSY) 1979 data. They attribute the effect of income on college attendance largely to early life educational resource disadvantages as opposed to credit constraints. Keane and Wolpin (2001) also find a small impact of credit constraints on college attendance, using the same NLSY 1979 data. On the other hand, Acemoglu and Pischke (2001) exploit the change in the distribution of income from 70s to 90s and find a substantial effect of family income on college enrollment. Similar to Carneiro and Heckman (2002), Lochner and Belley (2007) use the 1997 cohort of the NLSY and find the effect of family income on college has been increasing over time in the US.

Lovenheim (2011) uses recent fluctuations in housing prices in the 2000s and finds a significant effect of housing wealth fluctuations on college enrollment.[6] Lovenheim (2011) notes that the results could be potentially confounded by an increase in household wealth leading to more consumption including college education or increased credit access, and calls for further work on separating the two effects. Our research design is different from previous work in that the exogenous variation which we exploit does not involve an increase in *net* wealth, thus our estimates are not subject to the wealth effects concern. Our results add further evidence that the credit constraints channel has played an important role in determining households' college enrollment decisions in the US in a broader temporal horizon.

This paper also joins a growing body of work, mainly in the finance literature, that evaluates the

[6]Lovenheim and Reynolds (2012) presents further evidence using the National Longitudinal Survey of Youth 97 that housing wealth increases college enrollment.

impact of banking deregulation on economic development (Jayaratne and Strahan (1996), Huang (2008)), entrepreneurship formation (Black and Strahan (2002), Kerr and Nanda (2009)), state business cycles (Morgan, Rime, and Strahan (2003), Hoffman and Shcherbakova-Stewen (2011)), Demyanyk, Ostergaard, and Sørensen (2007)), income distribution (Beck, Levine, and Levkov (2010)), neighborhood crime rates (Garmaise and Moskovitz (2006)), rent-sharing and discrimination in the banking industry (Black and Strahan (2001)) and personal bankruptcy rates (Dick and Lehnert (2010)). In section 5 we will provide strong evidence that the channel through which the reform affected college enrollment was the relaxation of credit constraints and rule out alternative explanations. It is useful to emphasize that independent of the interpretations of the causal effect of banking deregulation on household college attendance, this is an important policy parameter in and of itself. Our estimates imply that the banking deregulation across states in the US had a positive effect on college enrollment through an increase in available credit to households and could have positive long-term effects on the human capital accumulation and regional development in the United States, apart from the effects identified in the existing banking literature.

The paper is organized as follows. Section 2 provides regulatory background on banking deregulation and justifies the validity of using this policy reform. We also discuss the related theoretical perspective on the college attendence decision. Section 3 explains our research design and empirical model. Section 4 describes our dataset. Section 5 presents and discusses the main results of our paper. We first provide direct evidence that deregulation lowered the cost of household mortgage financing and increased the proportion of students taking up college loans. We then show that banking regulation had a significant effect on individual enrollment decisions. This section also discusses evidence pointing to the credit constraints interpretation. Section 6 presents various robustness checks and section 7 concludes and offers suggestions for future research.

2 Institutional and Theoretical Background

2.1 Institutional Background on Bank Deregulation

Most U.S. states historically restricted banks from expanding geographically within and across state borders, which dates back to colonial times (Kroszner and Strahan (2001)). Only 12 states deregulated intra-state bank branching while no state allowed inter-state bank expansion before the wave of deregulation that began in the 1970s. The historical restriction on banking favored small and poorly capitalized banks over large and well-capitalized banks as it gave local banks monopoly power, which they used to extract economic rent.[7] As shown in Jayaratne and Strahan (1996), states did not deregulate their banks in anticipation of future growth prospects. Krozner and Strahan (1999) provide further analysis of the economic and political reasons for the exact timing of state deregulations.

Starting from the early 1970s, states lifted restrictions on banks' geographic expansion. States typically deregulated intra-state banking first and then moved to deregulate inter-state banking.

[7]See Black and Strahan (2001) and Economides, Hubbard, and Palia (1996).

Several studies have found significant effects of deregulation on market structure in local banking markets, for example, Amel and Liang (1992) show that the number of new entrants increased and Calem (1994) as well as McLaughlin (1995) show that existing banks consolidated. Following the passage of the Riegle-Neal Interstate Banking and Branching Efficiency Act of 1994, the culmination of the deregulation wave came by 1996, as all states except for Arkansas, Iowa and Minnesota had lifted intrastate branching restrictions.

We choose the date of intra-state deregulation as the date on which a state permitted branching via mergers and acquisitions (M&A) through the holding company structure, which was the first step in the deregulation process, followed by *de novo* branching, i.e., allowing a bank holding company to establish a new branch in a different geographical area.[8] Figure 1 shows the spread of intrastate bank branching deregulation allowing branching via merger and acquisition (M&A) between 1972 and 1992.

2.2 Effects of Bank Deregulation on Household Credit

The lifting of state branching laws would have directly affected the price of credit for families with college age children. Knapp and Seaks (1992) mention that a direct private student loan market was practically nonexistent during the time period in question due to the high risk involved. However, many students borrowed indirectly from banks via their parents. Students' parents typically have assets to use as collateral and these loans were consequently less risky investments for banks. During the time period in question, various sources estimate that between 10 and 20 percent of families took on private loans to help finance college costs. Government student loans were available to both parents and students throughout the time period in question, however they were capped requiring some families to borrow on the private market.[9] Some families with good credit scores may also have received lower interest rates in the private loan market.[10] The bulk of private loans for college financing were through signature loans, home equity loans, lines of credit or loans against life insurance policies.

Inter-university Consortium for Political and Social Research High School and Beyond data indicates that in 1982, 18.1 percent of families took non-federal, state or institutional loans to finance college, with 5.1 percent of families borrowing more than $2,000. Choy, Henke, and Schmitt (1992) document that in 1987, 11 percent of student parents took non-federal, state or institutional loans such as signature loans, home equity loans, lines of credit or loans against life insurance policies.[11] Stiglitz, Tyson, Orszag, and Orszag (2000) note that in 1993, 7 percent of families took

[8] This definition is consistent with many previous studies, for example, see Jayaratne and Strahan (1996).

[9] Before 1981, aggregated Stafford loans over a four year period were capped at $7,500, which was below the average *yearly* cost of tuition, room and board for a full time student in the same year. The aggregate rates were raised to $12,500 in 1981 and again to $17,250 in 1987 and finally to $23,000 in 1993 (US Department of Education (2012)).

[10] Government student loans were indexed to treasury bill auction rates plus a flat fee, and capped at eight to twelve percent throughout the time period.

[11] The decrease in private loan borrowing between 1982 and 1987 may have been due to increases of subsidized Government Student Loan caps in 1987.

out a second mortgage to finance college. As a magnitude check, it is useful to compare the rates of families that took out private student loans to the results of this study on college enrollment, namely that enrollment increased by 4.1% following financial deregulation. If between 10 and 20 percent of families took on private loans for college, between 20 and 40 percent of the families taking private loans were constrained and would have been unable to attend otherwise.

Given that the rollback of state branching regulation increased competition between banks and lowered the costs of private loans, branching deregulation would have made college more affordable for many families. Dick and Lehnert (2010) find that banks in general allowed previously excluded households to enter the market and increased overall personal borrowing including credit card loans. There also exists indirect evidence of lower costs resulting from increased competition, for example Black and Strahan (2001) find that wages fell for bank employees following regulation. Savings may have been passed on to consumers. In section 5.1, we provide evidence consistent with the findings in the finance literature that total loan volume increases, bank fees decrease, and mortgage loan interest rates decrease following branching deregulation taking effect. We also provide direct evidence from household longitudinal surveys that households borrowed more to finance education following the lifting of branching restrictions.

2.3 Theoretical Background

Two theoretical perspectives are typically presented when interpreting the relation of family financial wealth and education choice. These two views are not mutually exclusive. One perspective treats college education purely as an investment good in the lens of human capital investment models, as in Becker (1962). This branch of models predicts that one invests in college if and only if the rate of return from attending college is greater than or equal to the market rate of return from investing the forgone earnings and the direct cost of college. Under the investment good perspective, there could exist two major channels through which a state's deregulation policy can lead to an increase in college enrollment. The first is a credit constraint channel: a state's deregulation could lead to a lower interest rate for borrowing, at the same time, banks could begin lending money to those previously excluded from the credit market. The second is a returns to education channel: if a state's deregulation policy leads to an increase in the college wage premium, as shown by Larrain (2012), we might still observe that households in those states that deregulate their banking sector are more likely to send children to college. In fact, this could happen even in a frictionless market where households do not face borrowing constraints. In our main regression, we include state-level college wage premium as a control for the returns to education channel. In section 5.2 we present substantial evidence consistent with a credit constraints interpretation, while not lending support to the returns to education interpretation. However, it is important to note that, given our previous argument that the state deregulates its banking sector regardless of future economic prospects, our empirical exercise estimates a *causal* impact of the state deregulation on a household's college enrollment decision. This is an important policy issue in its own right, given recent discussion on the effect of banking competition on issues as diverse as local crime rates in Garmaise and Moskovitz (2006) and credit supply to poor income groups as in Melzer (2011).

An alternative perspective treats college education as a consumption good, for example, Lazear (1979). In this case, college enrollment increases due to wealth effects. As pointed out by Lovenheim (2011), many earlier empirical papers faced a challenge in evaluating the credit constraints channel by examining the relationship between family income or housing wealth and the college enrollment decision. If college attendance is a consumption good, then an increase in family income or housing wealth will lead the household to consume more of all goods, including college education. We are able to deal with this challenge, as being able to borrow more from credit markets adds to a household's debt liability while not changing *net* family wealth.

Our research design is novel in comparison to existing studies as the effect that we identify comes entirely from change in prices, rather than an accompanied change in wealth.[12] Previous studies have examined changes in family income or housing wealth and these changes naturally involve wealth effects in the estimates, in addition to price effects. Our identified change in college enrollment decision comes only from changes in prices, and not changes in wealth. To the extent that changes in price will automatically introduce both income and substitution effects, we, as the existing literature, cannot separately identify the income effect from the substitution effect, unless strong assumptions about family preferences are imposed.[13]

3 Empirical Methodology

3.1 Empirical Model

The principal challenge faced by researchers aiming to estimate the effects of financial constraints on college enrollment is the fact that financial resources are non-randomly assigned across households. Children from high income households may be endowed with greater abilities and enjoy better educational resources in early childhood, and hence will be more likely to attend college. Other factors which are correlated with access to credit, such as a history of personal bankruptcy, may also be correlated with family background, which will affect college enrollment decisions. However, we can circumvent this difficulty by exploiting exogenous changes in state financial regulations which had the effect of increasing credit available to households. These policies would have relaxed credit constraints for individuals living in a state where deregulation took place.

We use the dates of legislative changes to construct an indicator variable equal to one for states permitting M&A branching and zero otherwise. We restrict the sample to individuals who are of typical college-going age, which we define as 17 to 23, between 1972 and 1992 during which the vast majority of states went from restricting to permitting M&A branching.[14] We also restrict the

[12] It's illustrative to regard Y_{it}, household college education decision, as a function of P_{it}, the relative prices of education and other goods, and W_{it}, family wealth, i.e., $Y_{it} = Y(P_{it}, W_{it})$. Naturally, changes in education decision could come from direct changes in P_{it} and direct changes in W_{it}, which we call price effects and wealth effects respectively; i.e., $dY_{it} = \frac{\partial Y_{it}}{\partial P_{it}} dP_{it} + \frac{\partial Y_{it}}{\partial W_{it}} dW_{it}$.

[13] Lovenheim (2011) discusses how an increase in housing wealth could bring down the average interest rate a family has to pay for loan while an increase in household income could also lead to lower mortgage rates, see for example, Mian and Sufi (2009).

[14] The results are robust to a number of sensitivity checks and altering our definition of college age, the years

sample to individuals who completed middle school.[15] We estimate the effects of relaxing credit constraints on college enrollment with the following linear probability model:

$$Y_{it} = \alpha_t + \alpha_i + \beta_1 Z_{it} + \gamma X_{it} + \epsilon_{it}$$

where Y_{it} is our object of interest, an indicator of whether or not an individual i has attended college or university at time t and Z_{it} is an indicator of whether or not a state has restrictions on branching. We include year fixed effects α_t to absorb any economy-wide temporal shocks and state fixed effects α_i to control for any state-specific factors in college enrollment. We also include X_{it}, a vector of controls including gender, race, parents' marital status, household income, home value, state unemployment rate, median state income, state population and state-level college wage premium. Our main coefficient of interest is β_1, which measures the increase in college enrollment stemming from relaxing credit constraints via lifting branching restrictions. The specification is a generalization of the differences-in-differences approach where the effect of deregulation is estimated as the difference in college enrollment before and after deregulation with the difference in enrollment from a control group which did not experience financial deregulation. We estimate the specification using OLS to avoid the incidental parameters problem using logit or probit model, and cluster standard errors at the state by year level.[16]

3.2 Identification

Our source of identifying variation is the passage of state laws deregulating bank branching which relaxed credit constraints for families. This state-level deregulation was driven by exogenous differences in the timing of the adoption of branching deregulation laws, not by an anticipation of future good growth prospects. Following the early work of Jayaratne and Strahan (1996), a large body of research has emerged that utilizes the exogenous variation in state banking deregulation which we reviewed earlier. A particular advantage of our research design is that state-wide deregulation is uncorrelated with individual level characteristics that may be determinants of college enrollment. If there were unobserved determinants of college enrollment which were correlated with our identifying source of variation, our estimates would be biased. Controlling for geographic and temporal factors in college enrollment using state and year fixed effects allows us to compare individuals with similar observable characteristics who were affected by exogenous changes in credit supply from bank branching deregulation. In section 6 we include a robustness check where we use another closely related set of branching laws, the repeal of *de novo* branching restrictions which prohibited banks from opening affiliated branches anywhere within state borders.

We control for state level labor market condition by including state level unemployment rates, real income per capita, the size of college-going population in the state and the state college wage

included, and our definition of being enrolled in college.

[15] We view dropping out of middle or high school as indicative of problems such as learning disabilities or adverse family shocks that would make college enrollment difficult regardless of credit constraints. The results are robust to the inclusion of high school dropouts.

[16] See Moulton (1990). We also block bootstrap standard errors in the robustness check.

premium. We wish to draw special attention to the state college wage premium control. One of the most significant concerns in regards to our interpretation of the main results is that the observed effects of the branching deregulation are due to changes in the college wage premium rather than being driven by the credit constraint channel. Businesses may have taken advantage of the banking deregulation to invest in new technologies, which could have driven up the wage premium for a college education. Unconstrained individuals would then respond by increasing enrollment rates. We can control for changes in the distribution of the college wage premium directly.[17]

Our identifying assumption is that the specific timing of state banking deregulation is conditionally uncorrelated with unobserved factors that affect individual college enrollment decision. In section 2.1, we present institutional details documented by the banking literature on the political economy of the banking deregulation which supports this assumption. Several empirical exercises in section 5.3 also test and refute any pre-trend in college enrollment and simultaneous banking deregulation, adding to the validity of our identifying assumptions. We are able to identify the average treatment effect (ATE) brought about by the banking deregulation. Specifically, there are two groups of households affected: those who could not borrow enough to go to college without the deregulation or chose not to borrow to go to college at the then relatively high interest rate, but switched to borrow credit to fund college education once given access to credit supply at a lower price after the deregulation. Broadly speaking, these two groups of households are all facing a decrease in prices of debt: in particular, the price of a loan in the first case goes from infinity to some finite number.[18]

4 Data

Our main source of individual-level data is the Panel Study of Income Dynamics (PSID). The PSID is a nationally representative sample of households which began in 1968 and followed individuals and their descendants annually since that time. The PSID crucially contains data on educational attainment and state of residence, as well as rich demographic controls including family income and home value, which we take as a proxy for wealth.[19] An individual is defined as having been enrolled in college or university if they completed more than 12 grades of schooling. We match the PSID data to indicator variables of whether or not the state permits branching via merger and acquisition through holding company structure and whether or not banks can expand through *de novo* branching.

We obtain a second source of individual data in the National Longitudinal Survey of Youth 1979 (NLSY). The NLSY 79 was a nationally representative survey of American youths between the ages of 14 and 22 in 1979. The NLSY data is valuable to us for two reasons. First, the data validates our main findings from the PSID. Second, the NLSY includes data on whether or not families took

[17]We obtain data on the state level college-high school wage gap from Fortin (2009), which started in 1979 and was generated from the Merged Outgoing Rotation Groups of the Current Population Survey (CPS).

[18]These two groups of families are referred to as being credit-constrained, for example, see Lovenheim (2011).

[19]The PSID did not begin collecting detailed data on wealth until 1985. Unfortunately this period is after the vast majority of states deregulated.

out loans to finance educational expenses. Thus the NLSY allows us to provide direct evidence that lifting the branching restrictions was associated with an increase in obtaining educational loans. Crucially, we obtained the NLSY restricted access geocode which allowed us to determine whether or not the branching restrictions were in effect for an individual in a given year. The primary drawback of the NLSY comes from statistical power. There is variation in unit banking laws from 1972 until 1996, whereas the NLSY begins in 1979 and by 1989 most individuals have made their college enrollment decisions. We present summary statistics for key variables in both PSID and NLSY 79 data set in Table 1.

Our state-level controls are drawn from various sources. We obtain per capita income for each state from U.S. Bureau of Economic Analysis and adjust for inflation using the CPI-U. State-level unemployment data are from the Bureau of Labor Statistics Local Area Unemployment Statistics compilation. The size of the state's college-age population, collected from U.S. Census Bureau population estimates, is defined as individuals from 18 to 22 years old.

We obtain several sources of data pertaining to the effect of banking deregulation on real interest rates and personal loans. First, we obtain information on whether or not laws permitting M&A and *de novo* branching are in effect from Jayaratne and Strahan (1996) and Strahan (2003). Average interest rates and bank fees at the state level for mortgage loans are obtained from the Federal Housing Finance Agency's Monthly Survey of Rates and Terms on Conventional Single Family Non-farm Mortgage Loans. We obtain data on the volume of personal loans from the Federal Reserve Reports of Condition and Income. We finally obtain data on state level college enrollment from the National Center for Education Statistics Integrated Postsecondary Education Data System.

5 Results

Here we present the results of our empirical exercise. In section 5.1, we present indirect evidence on increased credit supply from the banking deregulation by examining change in mortgage loan interest rate, bank fees and the volume of private loan. We provide further direct evidence in section 5.2 by showing that household educational borrowing for college increased following the deregulation. One can compare the exercise in section 5.1 and 5.2 similarly to the first stage in an instrumental variable estimation. The main results for this paper is shown in section 5.3, we find the financial deregulation led to a 2.2 percentage points increase in college enrollment rate during our sample period. We provide substantial evidence supporting the credit constraints interpretation in section 5.4.

5.1 Effects on Household Credit Availability

Table 2 presents evidence of the effects of branching deregulation on private loans. We separately regress interest rates, mortgage loan fees, and total private loan volume on an indicator equal to one if bank branching deregulation is in effect.[20] Consistent with an increased competition leading

[20]Data is from the FHFA and Federal Reserve. See section 4.

to lower prices and higher quantities, we find that total private loan volume increases, bank fees decrease, and mortgage loan interest rates decrease following branching deregulation taking effect.

The evidence presented in Table 2 is consistent with both Dick and Lehnert (2010) and Black and Strahan (2002). Dick and Lehnert (2010) find that banks allowed previously excluded households to enter the market by increasing overall personal borrowing including credit card loans and reducing cost of credit as a result of the branching deregulation. Black and Strahan (2002) show that banking deregulation increased the rate of entrepreneurship in a state, which is well-known to depend crucially on bank lending. They provide indirect evidence that the deregulation led to increased credit supply, even though data on lending to small and young firms over a relatively long span of time is not available. The increasing loan volumes, decreasing interest rates and bank fees that came about as a result of branch deregulation relaxed credit constraints for families with college age children. We can thus exploit the variation resulting from lifting state branching prohibitions to determine whether or not credit constraints affected college enrollment.

5.2 Are Individuals Taking out More Loans to Finance College?

Table 3 provides direct evidence that financial deregulation increased the fraction of individuals taking loans to finance college. Each column presents the result of a linear probability model, where the dependent variable is an indicator of whether or not an individual took out loans to finance post-secondary study. We are able to obtain this information from questionnaires in NLSY 79. The results indicate that the deregulation caused a four percentage point increase in families taking out loans to finance their children's university studies. This is larger than the effect we see on college enrollment in the following subsection, suggesting that either many unconstrained families borrowed to increase consumption or completion rates increased due to credit availability.[21] The observed effect is robust to including both individual demographic controls, and state economic controls. The effect is similar for both branching via merger and acquisition, as well as *de novo* branching. In all specifications, the effect is significant at the .01 level for lifting the the M&A branching restrictions. While the coefficient is similar for allowing *de novo* branching, the effect is much less precisely estimated, being significant only at the .1 level. This is likely due to the fact that we have less variation in *de novo* branching laws in the NLSY sample. The individuals sampled in the NLSY were between the ages of 14 and 22 in 1979, and thus we can only exploit variation in branching laws from the early 1980s when the majority of the young adults sampled in the NLSY were making enrollment decisions. Regardless, states' allowing both types of branching significantly increased the share of families taking educational loans.

5.3 Results on College Enrollment

Table 5 presents the results of a linear probability model using the PSID data, with the dependent variable being an indicator of whether or not an individual attended college. Our main variable of interest is an indicator of whether or not a state has lifted branching regulations. We find evidence

[21] See Stinebrickner and Stinebrickner (2008) for a discussion of credit constraints and the college dropout decisions.

that state branching deregulation, and the subsequent increase in credit supply, increases college enrollment. When branching regulations are lifted, the probability of college enrollment increases by roughly 2.2 percentage points. In our sample average college enrollment is 54 percentage points, and thus the increase in enrollment probability represents a 4.1 percent increase in enrollment resulting from relaxing credit constraints via branching deregulation. This magnitude of this increase accounts for 20% of the total 10 percentage points increase in college enrollments between 1972 and 1992. Our results are significantly different from zero at the .01 level in all specifications, and is robust to controlling for demographics and family background. As will be shown in the next subsection, these results are driven by the lower and middle income families who would have been most affected by credit constraints. We also find no effect for individuals who would not have been affected by relaxing credit constraints, and we find smaller effects in time periods immediately following increases in student loan limits. Our baseline estimates are also robust to a number of alternative specifications and sensitivity checks. See section 6 for more information on the robustness of our results.

To further investigate the dynamic effect of the banking deregulation on college enrollment, we estimate a model similar to our main regression except that we replace the treatment dummy with a set of dummy variables indicating the number of years before and after each state's banking deregulation and omit the first year of deregulation as the base year. The point estimate as well as confidence band at 95% level for each of these year-specific treatment effects ranging from 10 years before and after the deregulation are plotted in Figure 2. Clearly, findings from this empirical exercise show that our main results are not driven by a pre-determined trend that coincides with the specific timing of state banking deregulation and change in college enrollment. Apart from our previous argument using the political economy of the exogeneity of state banking deregulation and college enrollment, our results further strengthen this identifying assumption. At the same time, Figure 2 also illustrates the effect from banking deregulation is quite persistent over time.

Our results from the PSID are confirmed by analogous analysis using data from the NLSY 79. The lifting of branching restrictions led to a significant increase in college enrollment. The second and third to last rows of Table 5 consist of specifications roughly analogous to those from the PSID. Two specifications were altered slightly, the marital status of parents and home value were not available in all years of the NLSY. Column five replaces an indicator of whether or not an individuals' parents are married with an interaction between black and female, and column six replaces home value with a square term for household income. The results are significantly different from zero at the .05 level in all specifications. While our results are still significant, the lack of significance at the .01 level could be due to the fact that in the NLSY we have fewer years of data and consequently less variation in branching laws.

When we use another data source containing aggregated data on college student enrollment, we find very similar results both qualitatively and in terms of magnitude. Table 4 presents similar results at the state level using the aggregated National Center for Education Statistics (NCES) data. The dependent variable is the total state college enrollment. The results indicate that lifting state branching regulations increases college enrollment by between roughly 9,000 and 16,000. The

state level NCES data indicates that the rollback of M&A branching restrictions increases college enrollment by 8.8 percent, which is broadly consistent with our micro-results using the PSID data given the large standard errors.

Instrumental variables estimates also indicate that the observed effects on college enrollment are through the borrowing channel. The second row of Panel B of Table 5 show linear log instrumental variable regressions of college enrollment on total educational borrowing. The results indicate that a 10% increase in educational borrowing is associated with a .025 point (5 percent) increase in college enrollment. All results are significant at the .05 or .1 level. The lack of further significance is likely due to the few years in the NLSY 79 sample. The NLSY 79 only records educational borrowing after 1983, after many of the sampled individuals would have finished college. We are thus identifying the effect of borrowing on enrollment off of only a few years' variation in state laws and a subsample of the NLSY 79. The instrumental variables results indicate that borrowing increased college enrollment, providing further evidence that lifting the branching laws affected college enrollment through a credit channel.

The results indicate that states' lifting restrictions on state branching increased higher education enrollment among college age individuals. At the same time, borrowing costs were significantly reduced for families through lower interest rates and bank fees brought about by deregulation. Since this would have reduced the costs of borrowing, the results are consistent with credit constraints affecting college enrollment. Further evidence of the channel through which branch deregulation affected college enrollment is provided by the lack of any observable effect for groups which were not affected by credit constraints.

5.4 Evidence for the Credit Constraints Interpretation

One significant concern for the validity of our results is that the observed effects are not due to relaxing credit constraints, but rather due to rational expectations resulting from changes in economic growth following branch deregulation. In fact, Jayaratne and Strahan (1996) found that growth rates increased following the lifting of state branching prohibitions. In our main regression, we include state-level college wage premium to control for the returns to education channel, to investigate the issue further, here we conduct some heterogeneity analyses. In particular, we show that for those groups whose credit constraints were not relaxed by the branching deregulation, we do not observe any increase in college enrollment. We argue that this is consistent with relaxing credit constraints being the main channel through which financial deregulation affected college enrollment.

Columns 1-4 of Table 6 present a linear probability model with the dependent variable being an indicator of whether or not a college age individual is enrolled in post-secondary education on a branching indicator and controls, broken down by income quartiles.[22] The results provide evidence that our earlier results are driven by poor and middle class families. The largest observed effect comes from the second income quartile. Branching deregulation increases the probability of enrollment by 3.91 percentage points. The coefficient is statistically different from zero at the .01

[22]The number of individuals is not the same in each column due to the fact that families had different numbers of children.

level. The average rate of lifetime college enrollment for this group in our sample is 43.2 percent, indicating that college enrollment increased by 9.1 percent following branching deregulation. This result is unsurprising, as individuals in the second income quartile are the group most likely to be able to benefit from access to credit. This group is relatively low income, and hence paying for college directly may be unaffordable. However individuals in the second quartile are likely to have assets which can be used as collateral to access credit markets. The results are similar but of smaller magnitude for the first and third income quartiles, with lifting branching restrictions increasing the probability of enrollment by 2.26 and 2.62 percent respectively, representing approximately 6.3 and 5.6 percent increases in enrollment for these groups. The results are significant at the 10 percent level. These groups would have also been constrained by access to credit, so the increase in credit supply resulting from lifting branching regulations would have made college more accessible for these groups.

For the top income quartile, the coefficient is positive but of smaller magnitude than the other quartiles, and moreover the effect of branching deregulation on college enrollment is not significant at the 10 percent level. For individuals in the highest income quartile, branching deregulation is associated with a 2.55 percentage point increase in the probability of college enrollment and thus in this income group college enrollment increased by 4.44 percent. The results indicate that there may have been some effect on upper income families. University fees are substantial in the United States, and given PSID over-samples lower income households, even upper income families in our sample may have benefitted from access to credit. However, the results are not significantly different from zero suggesting that if there was any increase from branch deregulation on college enrollment, the effect was small.

We can use the PSID to construct another group of individuals who faced significant credit constraints both before and after states' lifted branching regulations. Individuals who have gone through bankruptcy face great difficulty in accessing credit markets, and this would have remained true both before and after the banking deregulation.[23] Column 5 of Table 6 presents the results of a linear probability model with the dependent variable being college enrollment, restricted to individuals whose parents had declared bankruptcy at the time of their college-going age. Consistent with the effect of branching deregulation on college enrollment being through credit constraints, we find no effect of lifting branch regulations on college enrollment for individuals whose parents went through bankruptcy.

The results from the group of individuals whose parents declared bankruptcy are important in distinguishing the role of access to credit in explaining the observed increase in college enrollments. An alternative explanation for our results is that businesses borrowed and invested more following the repeal of unit branching laws, and that these investments increased the returns to education differentially for low income individuals. If this were the causal mechanism behind the increase in enrollment, and indeed individuals were not credit constrained, we would expect to see an increase in college enrollments for individuals whose parents had declared bankruptcy. This is not the case.

[23] In 1996 the PSID asked individuals if they have ever declared bankruptcy, as well as the year of their previous two bankruptcies.

Column 6 of Table 6 provides an additional placebo test. We replace the dependent variable with an indicator of whether or not an individual completed high school and did not go on to college. As high school is generally free in the United States, we should not see a large effect of access to credit on high school enrollment. This is precisely what is indicated in column 6. The coefficient on the branching restrictions is a precisely estimated zero.

Finally, we find the effect of intra-state branching deregulations subsided in years following the increase of Federal Guaranteed Student Loan borrowing limits. During the time period studied Stafford loan limits were increased four times, in 1973, 1981, 1987 and 1992 in order to keep up with increasing tuition inflation. Given that many families borrowed from private sources after exhausting federal student loan borrowing limits, in periods immediately after Stafford loan increases, there would have been less of a need for families to borrow from private sources, although this effect would gradually decrease as tuition inflation increased. We would thus expect branching restrictions to have a smaller effect immediately following increases in federal borrowing limits. This is exactly what our results in column 6 of Table 6 indicate. We interact the indicator of lifting branching restrictions with an indicator of whether or not the year is within two years of Stafford loan limits being increased. The interaction term is negative and significant at the 10 percent level, indicating that lifting branching restrictions and the subsequent increase in credit supply had a smaller effect in periods when there was greater federal access to student loans.

Table 3 provides evidence from the NLSY 79 that families increased borrowing to finance educational expenses following the repeal of unit branching restrictions. Table 5 indicates that the lifting of state branching restrictions was accompanied by a roughly 4 percent increase in college enrollments. The results in Table 6 indicate that our baseline results are driven by lower and middle income families, whose college enrollment choices would have been most affected by credit constraints. Furthermore, we find no effects for upper income families who presumably had the financial resources to send their children to college both before and after the lifting of branching restrictions. We also find no effects for a placebo group of individuals whose families had undergone bankruptcy, and thus who would have been excluded from credit markets both before and after deregulation. Finally, we find that the effects of lifting branch restrictions are smaller in periods when families would have had greater access to federal student loans. We view the sum of this evidence as being strongly in favor of the effect of branching deregulation operating through the credit constraints channel.

6 Robustness

Table 7 presents a number of robustness and sensitivity checks to confirm the robustness of our main results. In the main results, we restrict the sample to individuals who have completed middle school, viewing earlier dropouts as unlikely to have the opportunity to enroll in college. The first two columns of Table 7 restrict the sample to individuals who completed at least one year of high school and completed high school respectively. In both cases the results remain qualitatively similar. The magnitude of the results increases, suggesting that if anything our results underestimate the

effect of relaxing unit banking laws on college enrollment. In the main results, we also define college age as being between 17 and 23 while the age range has been changed to 17 to 22 in the first two columns of Table 7.[24] In column 3 we change the age range to individuals between the ages of 18 and 21. We also examine the age bracket of 16-22 in column 4. The results suggest that our estimates are not driven by a specific choice of the age range.

The intra-state banking deregulation wave took place in U.S. as both M&A branching deregulation and *de novo* branching deregulation. Consistent with most of the banking literature, we have used the date of M&A branching deregulation in the main regression to avoid collinearity. Column 5 of Table 7 uses the lifting of *de novo* branching restrictions instead, which would have had similar effects to the lifting of M&A branching restrictions. Our results are qualitatively and quantitatively similar to the main results, suggesting our estimates are not driven by the specificities of the timing of the M&A branching deregulation.

In column 6 we also present results dropping observations in Delaware and South Dakota, as is common in several prior studies, for example Black and Strahan (2002). Delaware and South Dakota passed laws to attract financial corporations to their states more or less at the time that the branching deregulation came into effect, leading to potential concerns that this effect could lead to changing returns to education. Dropping these states does not affect our results significantly. We also restrict the sample to years following 1976 and the conclusion of the Vietnam War. In the US many individuals enrolled in college to avoid conscription during the Vietnam War era, and this could potentially drive our results. Column 7 presents the results when years after 1976 are dropped. When we drop the Vietnam War era years from our sample the results are again quite similar to the main specifications.

In column 8 of Table 7 we block bootstrap our standard errors to allow for potential serial correlation. Our results are again significant at the 1 percent level, suggesting that this is not a major concern for our results. We also alter our definition of college enrollment. In our main specification, we consider individuals who completed more than 12 years of schooling to have enrolled in college. We alter this definition to individuals who have completed more than 14 years of schooling, to exclude individuals who enrolled in much cheaper community colleges and dropouts. The results of this check are presented in column 9. The magnitude of our coefficient increases, suggesting that our results are not driven by how we define the dependent variable. The results are also robust to changing the dependent variable to having completed four years of post-secondary education. Finally in column 10, we include a linear time trend interacted with each state to better control for any state-specific trend in college enrollment that could coincide with deregulation. Our result shows that it is significant at the 5 percent level and does not change much in magnitude compared with result from our main specification.

[24] The results are robust to using the age range 17 to 23.

7 Concluding Remarks

Classical human capital investment theory predicts that investment in college education should be independent of a family's financial resources, absent credit constraints or wealth effects. Whether or not credit constraints exist and how they affect a household's college enrollment decision in the US turns out to be an empirical question. A large body of existing work has examined this issue focusing on the role of family income or housing wealth fluctuations. Our work adds to this literature by examining the effect of an increase in commercial credit available to households, exploiting the exogeneous timing of states' lifting branching restrictions from 1970s to 1990s. This research design allows us to differentiate between wealth effects and the credit constraints channel, providing clearly identified evidence that credit constraints affect college enrollment.

Using the Panel Study of Income Dynamics (PSID) and the National Longitudinal Survey of Youth (NLSY 79) we estimate that lifting of intrastate branching restrictions increased college enrollment by roughly 4%, out of an average 54 percentage points. This accounts for 20% of the total increase in college enrollments between 1972 and 1992. The effect is largest for low and middle income families, and insignificant for upper income families. Furthermore, no significant effect is found for families who have gone through bankruptcy and hence would be unable to access credit markets. This evidence points to the relaxation of credit constraints as being the primary mechanism through which lifting intrastate branching regulations affected college enrollment.

These results are particularly relevant to policies on both promoting higher education enrollment and financial deregulation. While earlier research finds that the effect of credit constraints on college enrollment seems to be limited in the early 1980s, our analysis shows that an increase in credit supply promoted an increase in college enrollments from the 1970s to 1990s. Our paper adds to the work of Beck, Levine, and Levkov (2010) that shows financial deregulation leads to a tightening of the distribution of income. While the recent paper of Melzer (2011) finds some negative effects of providing credit to low-income groups, our research finds relieving the credit constraints of low-income households could increase college enrollment for this group. At the same time, the welfare effects of this increase in college enrollment are ambiguous as individuals must take on debt. Further research could estimate the returns to education for individuals most affected by credit constraints, who chose to borrow to finance post-secondary studies. As student loan debt surpasses credit card debt as the main source of American households' debt burdens, and tuition costs continue to rise, this becomes an especially timely question. Our work is admittedly indirect evidence about the effect of credit constraints on households' college decision. Future research is needed to exploit potential individual loan data matched with household information to corroborate our findings and to evaluate the efficiency of the investment and the impact on inter-generational mobility.

References

Acemoglu, D., and J.-S. Pischke (2001): "Changes in the Wage Structure, Family Income and Children's Education," *European Economic Review*, 45(4), 890–904.

Amel, D. F., and J. N. Liang (1992): "The Relationship between Entry into Banking Markets and Changes in Legal Restrictions on Entry," *Antirust Bulletin*, 37, 621.

Beck, T., R. Levine, and A. Levkov (2010): "Big Bad Banks? The Winners and Losers from Bank Deregulation in the United States," *The Journal of Finance*, 65(4).

Becker, G. (1962): "Investment in Human Capital: A Theoretical Analysis," *Journal of Political Economy*, 70(5), 9–49.

——— (1975): "Front Matter, Human Capital: A Theoretical and Empirical Analysis, with Special Reference to Education," 2nd ed. NBER, 22-0.

Becker, G., J. Duesenberry, and B. Okun (1960): "An Economic Analysis of Fertility," Demographic and economic change in developed countries. pp.225-256 Columbia University Press.

Bettinger, E. P., B. T. Long, P. Oreopoulos, and L. Sanbonmatsu (2012): "The Role of Application Assistance and Information in College Decisions: Results from the HR Block Fafsa Experiment," *The Quarterly Journal of Economics*, 127(3), 1205–1242.

Black, S. E., and P. E. Strahan (2001): "The division of spoils: rent-sharing and discrimination in a regulated industry," *The American Economic Review*, pp. 814–831.

——— (2002): "Entrepreneurship and bank credit availability," *The Journal of Finance*, 57(6), 2807–2833.

Bond, J., and S. Turner (2002): "Going to War and Going to College: Did World War II and the G.I. Bill Increase Educational Attainment for Returning Veterans?," *The Journal of Labor Economics*, 20(4), 784–815.

Brown, M., J. K. Scholz, and A. Seshadri (2011): "A New Test of Borrowing Constraints for Education," *The Review of Economic Studies*, 79(2), 511–538.

Calem, P. (1994): "The impact of geographic deregulation on small banks," *Business Review*, Nov, 17–31.

Carneiro, P., and J. Heckman (2002): "The Evidence on Credit Constraints in Post-Secondary Schooling," *Economic Journal*, 112(428), 705–735.

Choy, S., R. Henke, and C. Schmitt (1992): "Parental Financial Support for Undergraduate Education," *National Postsecondary Student Aid Study, National Center for Education Statistics*.

Demyanyk, Y., C. Ostergaard, and B. E. Sørensen (2007): "US banking deregulation, small businesses, and interstate insurance of personal income," *The Journal of Finance*, 62(6), 2763–2801.

Dick, A., and A. Lehnert (2010): "Personal Bankruptcy and Credit Market Competition," *Journal of Finance*, 65(2), 655–686.

Economides, N., R. G. Hubbard, and D. Palia (1996): "The Political Economy of Branching Restrictions and Deposit Insurance: A Model of Monopolistic Competition among Small and Large Banks," *National Bureau of Economic Research*, (w5210).

Fortin, N. (2009): "Higher-Education Policies and the College Wage Premium: Cross-State Evidence from the 1990s," *The American Economic Review*, 96(4), 959–987.

Friedman, M. (1962): "Capitalism and Freedom," Chicago: The University of Chicago Press.

Garmaise, M. J., and T. J. Moskovitz (2006): "Bank Mergers and Crime: The Real and Social Effects of Credit Market Competition," *The Journal of Finance*, 61(2), 495–538.

Hoffman, M., and I. Shcherbakova-Stewen (2011): "Consumption Risk Sharing over the Business Cycle: The role of Small Firm' Access to Credit Markets," *The Review of Economics and Statistics*, 93(4), 1403–1416.

Huang, R. R. (2008): "Evaluating the real effect of bank branching deregulation: Comparing contiguous counties across US state borders," *Journal of Financial Economics*, 87(3), 678–705.

Jayaratne, J., and P. Strahan (1996): "The Finance Growth Nexus," *The Quarterly Journal of Economics*, 111(3), 639–670.

Keane, M., and K. Wolpin (2001): "The Effect of Parental Transfers and Borrowing Constraints on Educational Attainment," *International Economic Review*, 42(4), 1051–1103.

Kerr, W., and R. Nanda (2009): "Banking deregulations, financing constraints, and firm entry size," *National Bureau of Economic Research*, (w15499).

Knapp, L. G., and T. G. Seaks (1992): "An Analysis of the Probability of Default on Federally Guranteed Student Loans," *The Review of Economics and Statistics*, 74(3), 404–411.

Kroszner, R. S., and P. E. Strahan (2001): "Bankers on Boards: Monitoring, Conflicts of Interest, and Lender Liability," *Journal of Financial Economics*, 62(3), 415–452.

Krozner, R. S., and P. E. Strahan (1999): "What Drives Deregulation? Economics and Politics of the Relaxation of Bank Branching Restrictions," *The Quarterly Journal of Economics*, 114(4), 1437–1467.

Larrain, M. (2012): "Does Financial Liberalization Contribute to Wage Inequality? The Role of Capital-Skill Complementarity," *Unpublished Mimeo*.

Lazear, E. (1979): "Education: Consumption or Production?," *Journal of Political Economy*, 85(3), 569–598.

Lochner, L., and P. Belley (2007): "The Changing Role of Family Income and Ability in Determining Educational Achievement," *Journal of Human Capital*, 1(1), 1–31.

Lovenheim, M. (2011): "The Effect of Liquid Housing Wealth on College Enrollment," *Journal of Labor Economics*, 29(4), 741–771.

Lovenheim, M., and L. Reynolds (2012): "The Effect of Housing Wealth on College Choice: Evidence from the Housing Boom," *Journal of Human Resources*, pp. 1–35.

McLaughlin, S. (1995): "The impact of interstate banking and branching reform: evidence from the states," *Current Issues in Economics and Finance*, 1(2), 1–5.

McSwain, C., D. Price, and A. Cunningham (2006): "The Future of Private Loans: Who is Borrowing and Why?," *The Institute for Higher Education Policy*.

Melzer, B. T. (2011): "The Real Costs of Credit Access: Evidence from the Payday Lending Market," *The Quarterly Journal of Economics*, 126(1).

Mian, A., and A. Sufi (2009): "The Consequences of Mortgage Credit Expansion: Evidence from the US Mortgage Default Crisis," *The Quarterly Journal of Economics*, 124(4), 9–49.

Morgan, D., B. Rime, and P. E. Strahan (2003): "Bank Integration and State Business Cycles," *National Bureau of Economic Research*, (w9704).

Moulton, B. R. (1990): "An Illustration of a Pitfall in Estimating the Effects of Aggregate Variables on Micro Unit," *The Review of Economics and Statistics*, 72(2), 334–38.

Schultz, T. (1961): "Investment in Human Capital," *The American Economic Review*, 51(1), 1–17.

Stiglitz, J., L. Tyson, P. Orszag, and J. Orszag (2000): "The Impact of Paying for College on Family Finances," *SEBTAG*.

Stinebrickner, T., and R. Stinebrickner (2008): "The Effect of Credit Constraints on the College Drop-Out Decision: A Direct Approach Using a New Panel Study," *The American Economic Review*, 98(5), 2163–84.

——— (2011): "Learning about Academic Ability and the College Dropout Decision," *The Journal of Labor Economics*, 30(4), 707–748.

Strahan, P. E. (2003): "The Real Effects of US Banking Deregulation," *Review- Federal Reserve Bank of St. Louis*, 85(4), 111–128.

US Department of Education (2012): "National Center for Education Statistics," *Digest of Education Statistics*, 001(3).

Figure 1: Spread of M&A Branching Deregulation

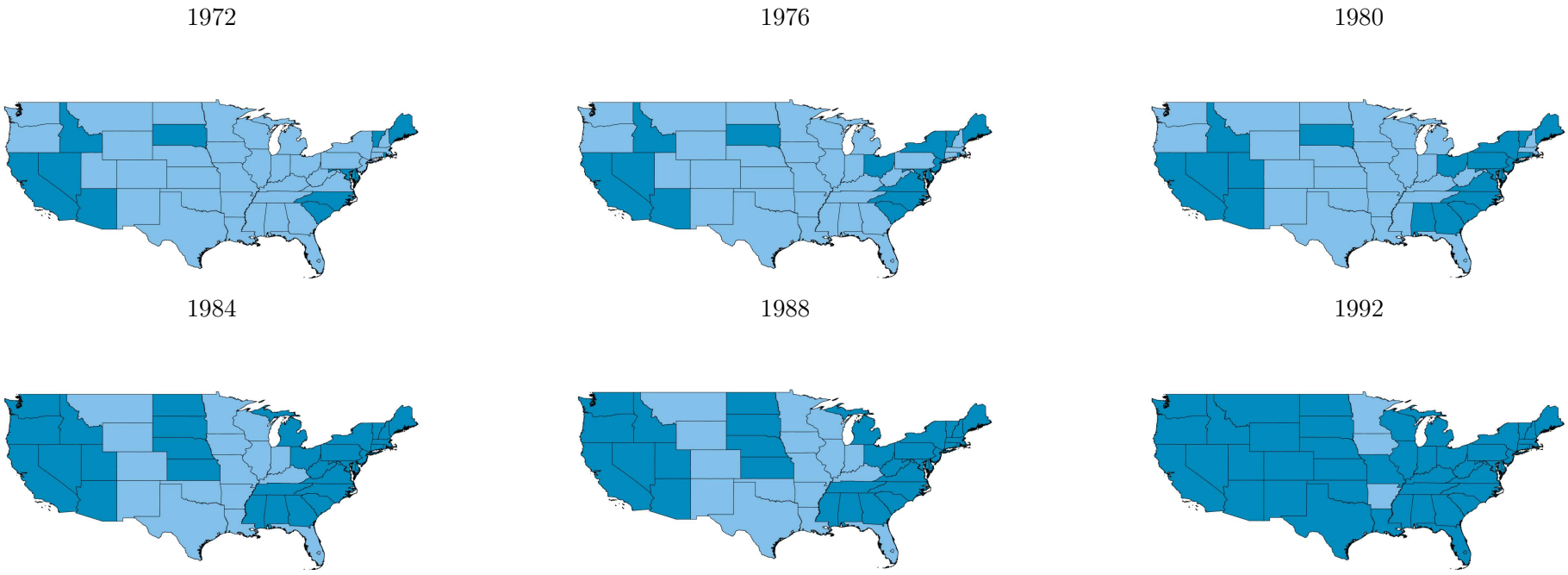
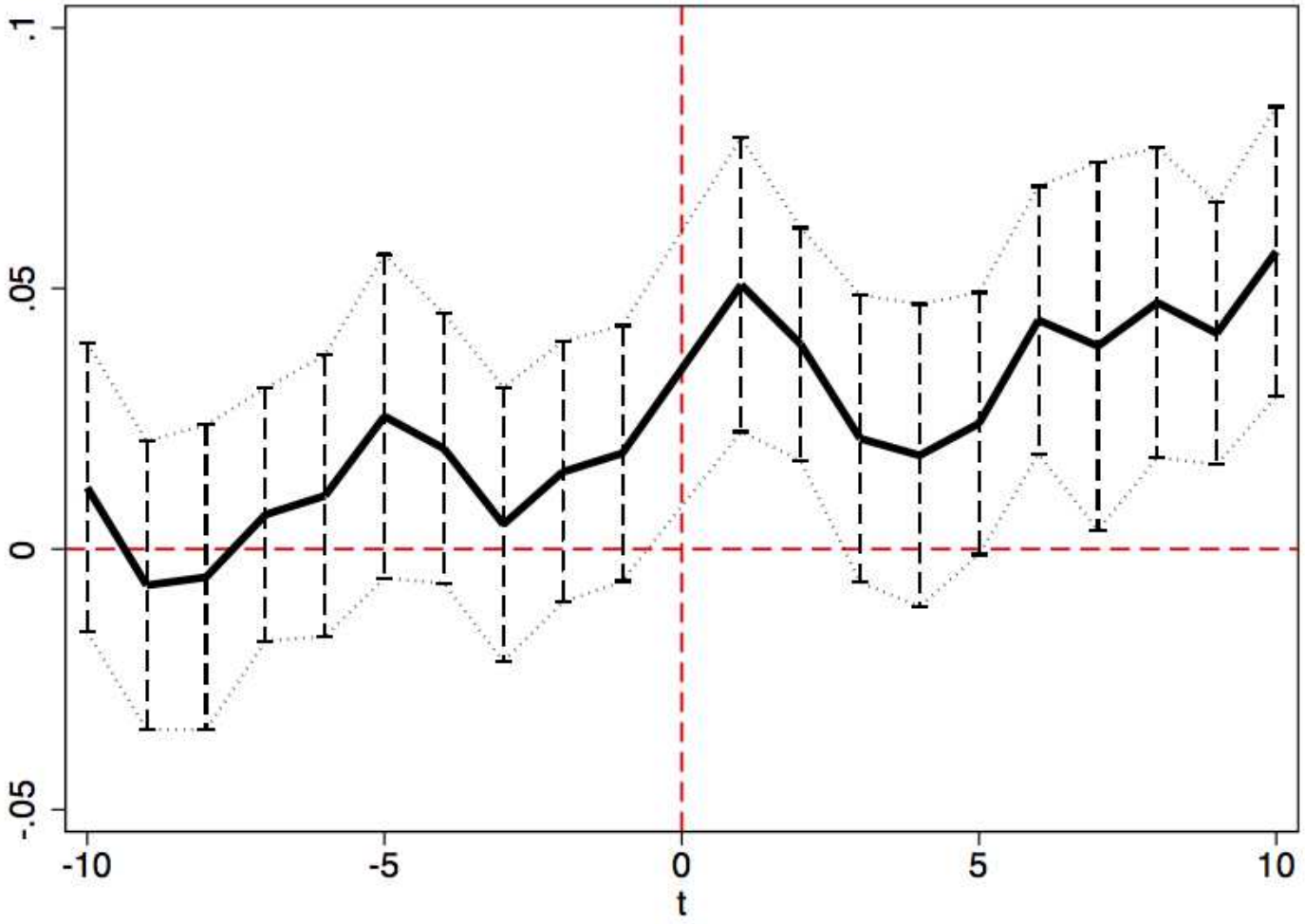


Figure 2: Treatment Effect Over Time



Notes: The solid black line plots the coefficient β_y from the specification $Y_{it} = \alpha_t + \alpha_i + \sum_{y=0}^{T} \beta_y \mathbb{1}_{y=t} * Z_{it} + \gamma X_{it} + \epsilon_{it}$. Z_{it} is an indicator of whether or not a state has lifted restrictions on branching. We include year fixed effects α_t to absorb any economy-wide temporal shocks and state fixed effects α_i to control for any state-specific factors in college enrollment. We also include X_{it}, a vector of controls including gender, race, parents' marital status, household income, home value, state unemployment rate, median state income, state population and state-level college wage premium. Standard errors are clustered at the state by year level. A 95% confidence interval is plotted in dotted lines.

Table 1: Summary Statistics

	PSID		NLSY 79	
	Mean	Std. Dev	Mean	Std. Dev.
M&A Dereg.	.540	.498	.450	.498
College	.532	.499	.471	.499
Age	20.088	2.022	18.284	1.827
Female	505	.500	.509	.500
Black	.126	.332	.249	.432
Married	.825	.380	..	..
Fam. Inc.	3.071	4.005	1.729	2.843
Home Val.	4.145	6.514	..	..
Bankrupt	.011	.105	..	..
N	37,467		30,916	

Notes: Family Income and Home Value are measured in $10,000s. Age is measured in years. All other variables are indicators.

Table 2: Effects on Credit Supply

	(1)	(2)	(3)
	Interest Rates	Bank Fees	Personal Loans
Branching	-0.0903**	-0.00707*	0.0477***
Deregulation	(0.0353)	(0.00426)	(0.00426)
Constant	0.279**	2.247***	7.740***
	(0.108)	(0.00881)	(0.268)
Observations	949	949	1,723,348

Notes: *$p < .1$, ** $p < .05$, *** $p < .01$. The dependent variables in column one and two are respectively the logarithm of average mortgage loan interest rates and fees by state. In column three the dependent variable is the logarithm of the total volume of personal loans for individual bank branches. The mean of log interest rates is 2.12, the mean log of fees is .062 and the mean log of personal loans is 8.21. All specifications include state and year fixed effects. Standard errors are clustered at the state by year level. Average interest rates and bank fees at the state level for mortgage loans are obtained from the Federal Housing Finance Agency's Monthly Survey of Rates and Terms on Conventional Single Family Non-farm Mortgage Loans. Data on the volume of personal loans is from the Federal Reserve Reports of Condition and Income.

Table 3: Lifting Branching Restrictions Resulted in an Increase in Educational Loan Borrowing

	(1)	(2)	(3)	(4)	(5)	(6)
Type of Restriction	M&A	M&A	M&A	*de novo*	*de novo*	*de novo*
Restriction Lifted	0.0401***	0.0404***	0.0392***	0.0422*	0.0410*	0.0442**
	(0.0117)	(0.0114)	(0.0115)	(0.0216)	(0.0217)	(0.0221)
Fixed Effects	YES	YES	YES	YES	YES	YES
Demographic Controls	NO	YES	YES	NO	YES	YES
State Controls	NO	NO	YES	NO	NO	YES
Observations	36,263	36,263	36,263	36,263	36,263	36,263

Notes: $*p < .1$, $**$ $p < .05$, $***$ $p < .01$. All specifications are a linear probability model where the dependent variable is an indicator of whether or not an individual took out educational loans to finance college. The sample consists of individuals in the NLSY 79 data set between the ages of 17 and 23. We add state and year fixed effects, demographic controls and state unemployment rate, median income and population sequentially across specifications for either M&A or *de novo* branching deregulation. Standard errors are clustered at the state by year level.

Table 4: Effects on Total State College Enrollment

	(1)	(2)
	M&A	*de novo*
Branching	92.33**	156.7***
Deregulation	(37.62)	(44.40)
Constant	1048.66***	1065.09***
	(93.61)	(92.76)
Observations	1,377	1,377

Notes: *$p < .1$, ** $p < .05$, *** $p < .01$. The dependent variable is state level college enrollment in hundreds. In the first column branch deregulation is measured by the repeal of M&A branching restrictions, while in the second column branch deregulation is measured using the repeal of *de novo* branching restrictions. All specifications include state and year fixed effects. Standard errors are clustered at the state by year level.

Table 5: Main Results

Panel A: PSID	(1)	(2)	(3)	(4)	(5)	(6)
M&A Branching	0.0224***	0.0223***	0.0214***	0.0215***	0.0220***	0.0219***
	(0.00725)	(0.00728)	(0.00732)	(0.00730)	(0.00732)	(0.00739)
Age	0.0564***	0.0561***	0.0560***	0.0550***	0.0557***	0.0577***
	(0.00113)	(0.00113)	(0.00113)	(0.00112)	(0.00113)	(0.00115)
Female		0.0360***	0.0361***	0.0368***	0.0370***	0.0379***
		(0.00391)	(0.00391)	(0.00389)	(0.00387)	(0.00386)
Black			-0.0198***	-0.0218***	-0.0165***	-0.00968*
			(0.00572)	(0.00575)	(0.00578)	(0.00572)
Parents Married				-0.0408***	-0.0452***	-0.0523***
				(0.00648)	(0.00655)	(0.00653)
Family Income					1.390***	0.733***
					(0.214)	(0.179)
Home Value						0.705***
						(0.0701)
Observations	34,145	34,145	34,145	34,145	34,145	34,145
Panel B: NLSY79						
M&A Branching	0.0289**	0.0290**	0.0289**	0.0289**	0.0291**	0.0276**
	(0.0125)	(0.0126)	(0.0125)	(0.0125)	(0.0125)	(0.0125)
Observations	31,392	31,392	31,392	31,392	31,392	31,392
Log Ed. Loans	0.235*	0.235*	0.237*	0.237*	0.233*	0.233*
	(0.126)	(0.127)	(0.124)	(0.124)	(0.122)	(0.122)
Observations	1,973	1,973	1,973	1,973	1,973	1,973

Notes: *$p < .1$, ** $p < .05$, *** $p < .01$. In all specifications the dependent variable is an indicator of whether or not an individual is enrolled in college. The sample consists of individuals between the ages of 17 and 23 in the PSID in top Panel A and NLSY 79 in the bottom Panel B. All specifications include state and year fixed effects as well as household demographic controls listed above and state-level controls including state unemployment rate, median income, population and college wage premium. In the final row of column B, total educational loans in natural logarithms are instrumented using branching deregulation law indicators. Standard errors are clustered at the state by year level.

Table 6: Results by Income Level

	(1) First Quartile	(2) Second Quartile	(3) Third Quartile	(4) Fourth Quartile	(5) Went Bankrupt	(6) Completed High School	(7) GSL Increase
M&A Branching	0.0226*	0.0391***	0.0262*	0.0255	-0.0175	0.00125	0.0239***
	(0.0135)	(0.0149)	(0.0159)	(0.0167)	(0.0195)	(0.00937)	(0.00792)
M&A Branching X GSL Limit Increase							-0.0193*
							(0.0114)
Observations	7,649	9,216	8,617	8,404	3,107	33,886	36,121

Notes: *$p < .1$, ** $p < .05$, *** $p < .01$. All specifications are a linear probability model where the dependent variable is an indicator of whether or not an individual is enrolled in college. Families are broken into income quartiles. Noting that number of observations vary across income quartiles as families have different number of children. All specifications include controls for female, race, marital status of parents, household income, home value, state income, state unemployment rate, population, state-specific college wage premium as well as state and year fixed effects. Standard errors are clustered at the state by year level.

Table 7: Robustness

	(1) Began HS	(2) Finish HS	(3) Age 18-21	(4) Age 16-22	(5) De Novo	(6) Drop States	(7) After 1976	(8) Bootstrap	(9) Four Year College	(10) State Trend
Branching	0.0198**	0.0342***	0.0144**	0.0181**	0.0195**	0.0241***	0.0270***	0.0243***	0.0254***	0.0221**
	(0.00801)	(0.0105)	(0.00825)	(0.00708)	(0.00904)	(0.00742)	(0.00753)	(0.00885)	(0.00842)	(0.00860)
Obs.	30,518	21,525	27,880	32,803	34,145	34,019	32,518	34,145	27,880	34,145

Notes: *$p < .1$, ** $p < .05$, *** $p < .01$. All specifications are a linear probability model where the dependent variable is an indicator of whether or not an individual is enrolled in college. The sample consists of individuals in the PSID between the ages of 17 and 23. All specifications include controls for female, race, marital status of parents, household income, home value, state income, state unemployment rate, population, state-specific college wage premium as well as state and year fixed effects. Standard errors are clustered at the state by year level. Column (1) restricts sample to individuals had began high school while column (2) restricts to individuals finished high school. We define college-going age to be 18-21 in column (3) and 16-22 in column (4), respectively. Column (5) uses the lifting of *de novo* branching restrictions. We dropped observations in states of Delaware and South Dakota in column (6). Column (7) restricts sample to years following 1976, the conclusion of Vietnam war. In column (8) we block bootstrapped our standard errors to allow for potential serial correlation in the error term. Column (9) restricts to sample finishing 4-year college. In column (10) we control for state-specific linear time trends.