Bank Branch Supply and the Unbanked Phenomenon*

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Abstract

An exogenous increase in the density of bank branches reduces the share of unbanked households among low-income households. This finding is established using US interstate branching deregulation between 1994 and 2010 as an exogenous shock to the entry of new branches in poor counties. We exploit household level data, and show that the effect is stronger for populations that are more likely to be rationed by banks, such as black households living in "high racially-bias" states, or for households living in rural areas where branch density is initially low. We then use deregulation to instrument the likelihood of holding a bank account and subsequently explore the effect on wealth accumulation. Holding a bank account helps poor household to accumulate wealth.

Keywords: Banks, Unbanked Households, Household Finance, Discrimination

JEL codes: G21, G28, D14, D43, J15

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The fact that poor families often rely on informal means to manage their financial lives suggests that the formal sector is not meeting their needs.

National Poverty Center, 2008

1 Introduction

There is a large debate about the reasons why so many low-income households - 35 to 45% in the United States - are unbanked, i.e., possess neither a checking nor a savings account. One question is whether being unbanked is driven by supply or demand-side factors (see for instance, Bertrand et al. (2004) or Barr and Blank (2008)). The "demand-side" view attributes the unbanked phenomenon to cultural determinants – the poor may distrust financial institutions or may not have a culture of saving – or to a lack of financial literacy. Alternatively, the "supply-side" view suggests that standard bank practices create hurdles for the poor. Minimum account balances, overdraft fees, and, in particular, the low presence of bank branches in poor areas may result in costs that are too high for poor households to manage (Washington, 2006; Barr and Blank, 2008; Ho and Ishii, 2011). These two polar explanations have different policy implications. For example, whereas the demand-side view predicts interventions at the household level through financial literacy programs, the supply-side view suggests that banking regulation, by giving banks incentives to change their behavior, may reduce the share of unbanked households.

This paper shows that an exogenous increase in the density of bank offices in poor counties has a positive impact on the share of low-income households with a bank account. To do so, we exploit interstate branching deregulation in the U.S. after 1994 as an exogenous shock on branch entry. While the passage of the Interstate Banking and Branching Efficiency Act (IBBEA) in 1994 made interstate branching fully legal, states kept the right to erect barriers to the entry of interstate branches, and partially lifted these barriers over the following years in a staggered way. Rice and Strahan (2010) construct a time-varying index to capture these state-level differences in regulatory con-

straints. We combine this index with bank office location data from the FDIC to study bank coverage, and micro data on households from the Survey of Income and Program Participation (SIPP) from 1993 to 2010 to identify low-income households with or without a bank account (Washington, 2006). The SIPP unique focus on low-income American households, coupled with its yearly frequency, make the data particularly well suited for our analysis.

We first establish the positive effect of interstate branching deregulation on the density of bank branches in poor counties. We find that the density of bank branches increases by around 30% in poor counties after a state fully deregulates. We then ensure that this increase is not driven by demand for credit and banking services by looking at 1) the density of credit union branches that were unaffected by the deregulation because of their legal status and, 2) the effect of deregulation on economic activity in poor counties. We find that branching deregulation has no effect either on the coverage of credit unions or on personal income growth, unemployment and household leverage. The absence of effect on the placebo sample of credit unions confirms that branching deregulation constitutes a supply shock that does not reflect contemporaneous or expected changes in the demand for banking services. In addition, the absence of effect of branching deregulation on county economic prosperity makes it an ideal laboratory for our research question by allowing us to isolate supply effects (Favara and Imbs, 2015).[1]

Second, we show that interstate branching deregulation is associated with a significant drop in the rate of unbanked households among low-income populations. Figure 2 plots the change in the likelihood of holding a bank account in the years before and after deregulation relative to a control group of states that do not deregulate. We observe a significant increase in the share of banked households following deregulation but no effect before, validating the parallel trend assumption of our diff-in-diff design. Our regressions confirm this result. The share of low income households with a bank account increases by

[1]This absence of effect on county economic activity could seem surprising given the large literature on the real effects of previous banking deregulations (Jayaratne and Strahan (1996) among others). However, all these papers explore different deregulation episodes that preceded and were completed *before* the interstate branching deregulation we are looking at. The real effects observed in earlier periods in response to different shocks are not observed in 1994 (Favara and Imbs, 2015; Rice and Strahan, 2010).

4 percentage points after a state fully deregulates, which corresponds to a 15% increase in relative term. In all of our specifications, we control for a large number of household covariates that capture several dimensions of income, skills and labor status and for main state macroeconomic variables to further control for demand effects.

The effect of intensified bank competition is stronger for populations that are ex ante more likely to be rationed by banks. Motivated by the over-representation of black households among the unbanked population – over 60% for black low-income are unbanked–, we study whether branching deregulation reduces the gap between black and non-black low-income households. We find that deregulation has an effect at least twice as large on both the density of bank branches in black counties, and on the share of black households with a bank account, *only* in states with a history of discrimination. While, before deregulation, black households are 20% less likely than white households to hold a bank account in states with a history of discrimination, this gap narrows to only 15% after deregulation, to the level observed in states with no history of discrimination.[2] The effect of branching deregulation is also larger for households living in rural areas, where branch coverage is lower ex ante, and for poorer households. While deregulation results on average in a 4 percentage points increase in the probability of holding a bank account among low-income households, the effect increases up to 6.5 percentage points for poor households, whose income is below the poverty line.

Finally, we investigate the effect of holding a bank account on household wealth accumulation. We use our interstate branching deregulation as an instrument for the probability of holding a bank account. Because branching deregulation has no impact either on the macroeconomic activity of poor counties or on household income and employment status in our SIPP sample, the exclusion restriction is satisfied. We first find that having a banking account translates into holding a significant amount of bank assets, which relieves the concern that unbanked low-income households open a bank account but do not use it to accumulate savings. Second, we find that holding bank account has

[2]This result holds across the full wage distribution, relieving concerns that it simply reflects the observation that deregulation has more effect on the poor and that the poor are disproportionately black.

a large and significant effect on total wealth *and* net worth, even within income deciles. These results show the positive effect of holding a bank account beyond access to debt, as we find a positive effect on net worth, but also on the probability of holding a bank account without any debt.

Our paper contributes to the literature on the determinants of being unbanked. This literature has been scarce mainly as a result of the challenge of disentangling demand-side from supply-side factors (see Barr and Blank (2008) for a broad survey of the literature). Socio-economic characteristics, which may capture both demand- or supply-side effects, are often noted as the most influential determinants of holding a bank account (Rhine et al., 2006; Barr, 2005; Barr et al., 2011; Hogarth and O'Donnell, 1999). On the demand side, Kearney et al. (2010) and Cole et al. (2016) show that by offering a savings account with lottery-like features, banks can motivate the opening of savings accounts. This paper provides evidence that the local presence of bank branches matters, extending the results of Brown et al. (2016), and Nguyen (2016) on small business lending.

Our paper adds to the literature that investigates the effect of access to bank accounts on savings behavior (Ashraf et al., 2006; Schaner, 2013), on investment in preventative health (Dupas and Robinson, 2013b) and in education (Prina, 2014), and on starting a business (Dupas and Robinson, 2013a). While this literature has mostly focused on randomized control experiments in developing countries, we address this question using a natural experiment in the U.S., where holding a bank account may also protect households from the use of costly alternative financial services (see Melzer (2011), Carrell and Zinman (2014) on the costs of access to payday loans, and Morse (2011) for the opposite view).[3]

Finally, our paper extends the literature that evaluates the effect of intensified competition on racial discriminations. Increased competition has been found to reduce the black-white wage gap in the trucking industry (Peoples and Talley, 2001), in the economy overall (Levine et al., 2013) and between genders (Black and Strahan, 2001). Our results are also in line with Chatterji and Seamans (2012), who shows that credit card

[3]Suri and Jack (2016) estimate that access to mobile money has lifted 2% of Kenyan households out of poverty.

deregulation expanded access to credit, particularly among blacks.[4]

The rest of the paper proceeds as follows. Section 2 explains the data and the empirical strategy. Section 3 presents the empirical strategy and the results on branch entry. Section 4 presents the results on household-level data. Section 5 runs various robustness checks. Section 6 concludes.

2 Data

2.1 Banking Deregulation

Restrictions on interstate banking and branching have their historical roots in the 1789 Constitution (Johnson and Rice, 2008).[5] Although the Constitution prevented states from issuing fiat money and taxing interstate commerce, it gave them the right to charter and regulate banks. Since then, states have used banks as a source of revenue by charging fees for granting charters, levying taxes and owning shares. These revenues have given states incentives to restrict competition from out-of-state banks and to create local monopolies. In 1927, the McFadden Act implicitly prohibited interstate branching by commercial banks. In the following years, however, bank holding companies were created to circumvent the law and they acquired branches across states. In 1956, the Bank Holding Company Act ended this development, preventing banks from acquiring banks or branches outside their state unless the state of the targeted bank permitted such acquisitions. The first step toward interstate banking came in 1978 when Maine began to allow out-of-state bank holding companies to acquire banks on a reciprocal basis. Other states followed beginning in 1982, but interstate branching was still not allowed until 1994.

In 1994, the Interstate Banking and Branching Efficiency Act (IBBEA), also known as

[4]However, as shown by Ouazad and Rancière (2016), the relaxation of credit standards can also lead to more black segregation by giving white households the opportunity to relocate in white neighborhoods.

[5]Interstate banking refers to the control by bank holding companies of banks across state lines, whereas interstate branching means that a single bank may operate branches in more than one state without requiring separate capital and corporate structures for each state.

the Riegel-Neal Act, effectively permitted bank holding companies to enter other states and operate branches. However, it also allowed states to erect barriers to out-of-state entry with regard to four dimensions: (i) the minimum age of the targeted bank (5 years, 3 years or less), (ii) de-novo branching without an explicit agreement by state authorities, (iii) the acquisition of individual branches without acquiring the entire bank and (iv) a statewide deposit cap, that is, the total amount of statewide deposits controlled by a single bank or bank holding company. Following the passage of the IBBEA in 1997, states had the opportunity to modify each of these provisions, and many states did so. In fact, 43 states have relaxed the protection of their banking market since then.

Following Rice and Strahan (2010), we construct a deregulation index that ranges from 0 to 4 to capture each dimension of state-level branching restrictions: 0 for fully regulated and 4 for fully deregulated states. Therefore, an increase in the index value implies greater competition.[6]

Interstate branching deregulation has fostered the development of multi-state banking. As Figure 2 shows, not only has the total number of branches increased since 1994, but each local market has also experienced a strong penetration of "out-of-state" branches, which have challenged local incumbents. Analyzing the other dimension of IBBEA, the interstate *banking* deregulation, Dick (2006) finds that it has translated into a dramatic decrease in the number of regional dominant banks and a slight increase in the number of small banks, resulting in a strong appreciation of bank density.

INSERT FIGURE 2 AROUND HERE

2.2 Household Data

Data on households comes from the SIPP and covers the 1993-2010 period.[7] The SIPP is a running panel that collects detailed information about income and demographics for

[6]We reverse Rice and Strahan (2010)'s index to facilitate the description of our results. The index takes the value 4 before the deregulation year.

[7]Data are available on line: $http://www.nber.org/data/survey-of-income-and-program-participation-sipp-data.html$

20,000 to 30,000 households over 2 to 3 years. Most importantly, the SIPP includes topical modules providing a complete list of all assets and liabilities held by each household, and their value .[8] We exploit the data from these topical modules to create a dummy variable *BankAccount* that takes the value 1 if at least one member in the household holds either a checking or a savings account, and 0 otherwise. We consider as a savings account any interest earning account in a banking institution, which includes savings accounts, interest earning checking accounts, money market deposits and certificates of deposits. We also collect data on household total wealth, debt, and net worth.[9].

The large size of the SIPP sample allows us to focus on low-income households, i.e., those below 200% of the poverty threshold, which is key for our analysis because low-income households are more likely to be rationed by banks.[10] We work at the household rather than the individual level because households often pool resources; a bank account in one member's name can provide access to banking services to other members of the same household. We collapse each household observation at the year level. We then drop households whose head is less than 20, and with strong inconsistencies in their asset declaration, mainly households declaring holding bank debt but no bank accounts, or negative wealth. This leaves us with a total sample of 130,125 low-income households living in 45 states plus the District of Columbia over the 1993-2010 period.

Finally, we exploit the very detailed information on socio-demographics that the SIPP provides to control for a large set of variables in our identification strategy. These controls include family type (size of the households, whether the household head is single and female, and whether the head is married), the socio-demographic characteristics of the head of household (age, race, three dummies for education: elementary, high school or college degree, employment status) and the household's economic characteristics (monthly income, dummy for receiving social security, dummy for transfer income).

Based on the SIPP data, we find that 36.3% of low-income households are unbanked in

[8]See the online appendix for one example of these topical module questionnaires

[9]Household total wealth and net worth are available in all asset and liability topical modules except in the 1992 panel, wave 7 and the 1993 panel, wave 4

[10]The poverty threshold is defined in the SIPP and varies with the number of adults and children in the household and, for some household types, the age of the household head.

1993. This rate increases up to more than 40% in 2002. We observe the same increasing trend in the Panel Study of Income Dynamics data (Table A.6 in the appendix). One potential explanation would be the rapid development of alternative financial services over this period. The 2011 National Survey of Unbanked and Underbanked Households from the FDIC indicates that the proportion of unbanked households has also increased slightly during the recent financial crisis.[11]

Table 1 shows the summary statistics for banked and unbanked households in our sample. While we make no causal statement here, several patterns emerge. First, consistent with the role of financial inclusion in asset accumulation, "banked" households have a total wealth almost four times higher than unbanked households, despite having a monthly household income that is on average only 1.3 times higher. Second, one of the largest gap in the characteristics of unbanked households relatively to banked households relies in the share of black households. On average, black households are three times more likely to be unbanked.

INSERT TABLE 1 AROUND HERE

The racial gap in household access to banking services persists when controlling for household characteristics in a linear probability model where we predict the likelihood for a household to hold a bank account. Table A.1 in the appendix reports the estimated coefficients when we regress the *BankAccount* dummy on both the household and state-level control variables. Even after controlling for all the different socio-demographics and income characteristics, we still observe that black households are 16 percentage point less likely to have access to a bank account, which is almost a 50% increase in the probability to be unbanked relative to the sample mean. Given that we control for many socio-economic determinants, this result suggests that black households suffer from discrimination in access to banking services (see Blanchflower et al. (2003) for evidence of racial discrimination on the credit market).

[11] http://www.fdic.gov/householdsurvey/

2.3 County-Level Data

Bank and Credit Union Location Data

Both data on bank branch location and deposit holdings come from the Sum of Deposits (SOD) maintained by the Federal Deposit Insurance Fund (FDIC). The FDIC provides annual branch-level data on total deposits outstanding from June 1994 to June 2014. The data set has also information on branch characteristics such as the branch ownership, the branch address at the zipcode level and the total amount of deposits in the branch. The data covers the universe of bank branches in the U.S. and contains a unique office identifier, branch identifier, bank identifier, and county identifier.

Data on credit union location and deposit holdings come from the National Credit Union Administration. The data provides annual information on total deposits and branch location at the county level for the years 1994 to 2014.[12]

County Characteristics

We collect data on county characteristics from several sources. The data on total population and statistics about communities come from the population estimates produced by the Census Bureau. Both poverty and urbanization rates are obtained from the decennial census.[13] Information on unemployment rate is from the Bureau of Labor and Statistics. Finally, information on personal income comes from the Bureau of Economic Analysis (BEA) Regional Tables.

Based on these data, we identify poor and black counties as counties where respectively the poverty rate and the fraction of black households are both at the top quartiles of the distribution in 1993.

2.4 Identifying states with a history of discrimination

To investigate the effect of interstate branching deregulation on racial discrimination, we build four dummies that indicate states with a history of discrimination (Chatterji and

[12]data can be downloaded here: http://www.ncua.gov/DataApps/QCallRptData/Pages/CallRptData.aspx
[13]Data are available via the National Historical Geographic Information System (NHGIS)

Seamans, 2012).

The first index, "slave state", is equal to one if states allowed slavery before the civil war of 1861-1865. The second index, "banning interracial marriages", comes from Fryer (2007) and identifies states that still banned interracial marriage before 1967, the date when the US Supreme Court's 1967 decision in Loving v. Virginia repealed such anti-miscegenation laws. The third index, "fair housing law", is based on Collins (2004) and identifies states that did not curb discriminatory practices by sellers, renters, real estate agents, builders, and lenders until the federal Fair Housing Act of 1968. Finally, for the fourth index, "interracial marriage bias", we use the racial bias index reported in Levine et al. (2013), which measures the difference between actual and predicted interracial marriage rates in 1970 and classifies states as above or below the median for interracial marriage bias. Not surprisingly, the correlation between these four measures is fairly high and ranges from 40% to more than 90%.

3 The Effect of Branching Deregulation on Branch Density

This section establishes the effect of interstate branching deregulation on the density of bank branches in low income counties and in black counties in states with a history of discrimination. We then show results for the placebo sample of financial institutions that are not affected by the change in regulation, i.e credit unions. This section finally investigates the (absence of) effect of the deregulation on county prosperity, hence comforting the use of interstate branching deregulation as an exogenous shock to the supply of banking services.

3.1 Bank Branch Density in Poor Counties

As motivating evidence, Figure 2 first documents a negative correlation between county poverty, as measured by the county poverty rate, and the number of bank branches

(number of branches scaled by population in the county). In term of economic magnitude, the elasticity amounts to -0.11, implying that a relative increase of 10% of the poverty rate is associated with a decrease of 1% of bank branch density. Low-income counties therefore face a lower supply of bank branches at the beginning of our sample period. We then assess whether interstate branching deregulation leads to a positive supply shock on branch density in low-income counties. We define low-income counties as counties where the fraction of households living below the poverty line is in the top quartile of the distribution.

To estimate the effect of deregulation, we run the following model:

$$Log(BankBranchDensity_{cst}) = \alpha + \beta Deregulation_{st} + \lambda CountyControl_{ct} + \delta_t + \eta_c + \epsilon_{ct} \quad (1)$$

where *Bank Branch Density* $_{cst}$ is the number of bank branches in a county scaled either by the number of inhabitants in this county (in thousands) or the number of square miles, $Deregulation_{st}$ is the deregulation index in state s at time t, $CountyControl_{ct}$ are county time-varying characteristics (log population, log personal income, personal income growth, population growth, unemployment rate and poverty rate) and δ_t and η_c are year and county fixed effects, respectively. Standard errors are clustered at the state level to account for serial correlation within states.

Panel A of Table 2 presents the results. Columns (1), (2), (5) and (6) show that bank branch density increases significantly following deregulation, and that the effect is slightly higher in low-income counties (Columns (2) and (6)). The coefficient of our *Deregulation* variable implies that states where all branching restrictions were lifted experienced a (4 $\times$7.2% =) 30% increase in the density of bank branches.

INSERT TABLE 2 AROUND HERE

3.2 Bank Branch Density in Black Counties

Motivated by the overrepresentation of black households among the unbanked population that we observe in the aggregate data, we now test whether interstate branching deregulation has a stronger impact on bank branch density in counties where with a higher fraction of black households in particular in states with a history of discrimination.

Figure 2 shows the number of bank branches per capita, across quartiles of counties based on the fraction of black households in 1994. Because the fraction of black households in a given county is strongly correlated with county average personal income, we first split our sample across quartiles of income. The figure reveals a decreasing pattern in branch density along county share of black households. Within each county income quartile, an increase in the fraction of black households is associated with a decrease in the number of bank branches per capita.

To test whether branching deregulation has a larger effect in black counties, we run a specification similar to Equation 1. We first identify black counties as counties where the fraction of black households is in the top quartile of the distribution. We then use the first of our four discrimination dummies ("former slavery state") to identify states with a history of discrimination. Table 2 presents our results. Columns (3) and (7) indicate that the effect of deregulation on density is almost the same in "black counties" in states without a history of discrimination as in "low income counties". However, the effect of deregulation is more than twice higher in states with a history of deregulation (columns (2) and (8)). The coefficient of our *Deregulation* variable indicates that branch density in black counties would have increased by 40% in states with a history of discrimination after a state fully deregulated.

3.3 Interstate Branching Deregulation as an Exogenous Shock to Bank Branch Supply

There are three concerns that could arise when using interstate deregulation as an exogenous shock to the supply of banking services. First, the relationship between branch

deregulation and branch density may be subject to reverse causality. By studying the previous waves of deregulation in the 1970s and 1980s, Kroszner and Strahan (1999) show that the timing of deregulation is not random across states but related to interest group factors such as the prevalence of small banks and small firms. In our case, our identification would be compromised if for instance, the states deregulate in response to an increase in demand for banking services. Second, unobserved factors such as changes in economic conditions could drive both deregulation and the demand for bank accounts. Third, interstate branching deregulation, as previous deregulation, may directly affect the real economy, hence enhancing the demand for bank accounts.

Effect on Credit Union Branch Density

To mitigate these concerns, we first investigate whether interstate branching deregulation had any impact on financial institutions that were not subject to changes in branching regulations. More specifically, we look at credit unions, and estimate the same models on this "placebo sample". Panel B of Table 2 shows that branching deregulation has no effect on the density of credit union branches. As such, interstate branching deregulation seems to provide a valid exogenous shock to the supply of bank accounts to low-income households. If deregulation were endogenous and simply responding to expected large increases in the demand for banking services, the coefficient of the deregulation index should be significant also in this sample.

Effect on Overall Economic Activity

The absence of any significant effect of deregulation in our placebo sample suggests that interstate branching deregulation has not improved overall economic activity and so, the demand for banking services. We confirm this finding by testing directly whether interstate branching deregulation had real effects in term of employment and income, first using our SIPP sample, and second using aggregate county level data from the Census.

Columns (1) and (2) in in Table 8 show the effect of interstate branching deregulation on household income and probability to be unemployed in our SIPP sample of low income households. The coefficient of the *Deregulation* variable is always insignificant,

alleviating the concern that deregulation has improved household economic conditions.

Second, Table A.3 in the appendix reports the effect of interstate branching deregulation on different measures of county prosperity in a differences-in-difference setting similar to Equation 1.[14] The coefficient of the *Deregulation* variable is always insignificant for all the proxies of economic prosperity we consider: income per capita (columns (1) to (3)), unemployment rate (columns (4) to (6)), poverty rate (columns (7) to (9)) and also on household leverage (columns (10) to 12)), even when restricting the sample to low-income or black counties. These results confirm the absence of direct effects of deregulation on the overall economic activity at the county level.

This finding may seem surprising in light of the literature showing that intrastate branching and interstate banking deregulation affected the real economy directly.[15] However, the deregulation episodes we consider in this paper have little connection with those that were documented to have real effects (Favara and Imbs, 2015). The index of restrictions used here starts after 1994, once all the deregulation waves documented as having direct real effects were completed. In addition, Rice and Strahan (2010), using the same deregulation, show that while the increase in banking competition leads to a decrease in interest rates for small firms, there is no effect on the amount that small firms borrow, which is consistent with the absence of macroeconomic effect.

Motives for Deregulation

Finally, we investigate the timing of deregulation following the method of Kroszner and Strahan (1999) and find that our interstate branching deregulation does not seem to be driven by variables that may also affect access to banking services. We show in particular that the share of unbanked households (in the total population, among low-income or among black households) at the beginning of the sample period (1994) does not predict the timing of deregulation in a Weibull proportional hazards model (Kroszner and Strahan, 1999). We detailed the methodology of this analysis in Appendix and report

[14]Data for leverage comes from the Consumer Credit Panel/Equifax of the NewYork FED and are available over the 1999-2010 period

[15](e.g. Jayaratne and Strahan (1996), Morgan et al. (2004), Cetorelli and Strahan (2006), and Kerr and Nanda (2010))

the results in Table A.4. As such, interstate branching deregulation seems to provide a valid exogenous shock to the supply of bank accounts to low-income households.

4 The Effect of Branching Deregulation on the Share of Unbanked Households: Household level Data

We now turn to micro-data from the SIPP to estimate at the household level whether the effect of deregulation on bank branch density converts into a higher household access to bank accounts.

4.1 Specification

The baseline model estimates the effect of deregulation on the probability of holding a bank account:

$$P(BankAccount_{ist}) = \alpha + \beta Deregulation_{st} + \theta X_{ist} + \lambda StateControl_{st} + \delta_t + \eta_s + \epsilon_{ist} \quad (2)$$

where $BankAccount_{ist}$ equals 1 if household i in state s holds a bank account at time t, $Deregulation_{st}$ is the deregulation index in state s at time t, X_{ist} is a vector of household characteristics, $StateControl_{st}$ are state characteristics and δ_t and η_s are year and state fixed effects, respectively. Household controls include socio-demographic controls (race, marital status, sex, age and age polynomial of the household head), controls for the household head level of education (elementary, high-school, college) and economic controls (household head employment status, and dummies indicating whether the household received any social security income or social transfer income). In order to control in a non-parametric way for household income and household size, we include a set of dummies for income deciles and categories of household size – 1, 2, 3, 4 and 5 or more–. This non parametric estimation allows us to better control for household economic conditions that could drive household demand for a bank account.

Finally, time-varying state controls include state-level GDP growth, unemployment and a log of the total population.[16] Although our dependent variable is binary, the use of a non-linear model such as probit or logit is not suitable given the numerous fixed effects we are using. Therefore, following Angrist and Pischke (2008) we use a linear probability model.[17][18] Standard errors are clustered at the state level to account for serial correlation within states.

The parameter of interest is β, which measures the incremental effect of one step of deregulation out of four possible steps on the likelihood of holding a bank account. State fixed effects capture time-invariant determinants of access to banking services in each U.S. state, such as the size of the state, the initial structure of the local banking market and the level of education. Year fixed effects control for aggregate shocks and common trends in access to banking services. The identification of β therefore relies on comparing the probability of a household holding a bank account in a state before and after deregulation relative to a control group of states that do not experience a change in regulation.

One concern with our identification strategy is that we may capture the effect of the Community Reinvestment Act (CRA) on unbanked households rather than the effect of banking deregulation. The IBBEA stipulates that meeting the credit needs of communities, as defined by the CRA, is a condition for the operation of interstate branches.[19] However, the CRA's focus on access to credit rather than on access to basic bank accounts alleviates this concern. In addition, even if the CRA had an effect through the IBBEA, our results on the impact of banking deregulation would be even stronger than reported.

[16] To save space and facilitate the reading of the results, the coefficients of the control variables are reported only in Table A.1

[17] In addition, Angrist and Pischke (2008) argue that once raw coefficients from non-linear estimators are converted to marginal effects, they offer little efficiency or precision gains over linear specifications. The other main advantage of linear probability models is that the coefficient can be interpreted directly in term of percentage points.

[18] Our results still hold in logit regressions

[19] The CRA was enacted in 1977 to fight the problem of "redlining" namely, the existence of discrimination in loans and access to banking services to individuals and businesses from low- and moderate-income neighborhoods (see, for instance, Barr (2005) for a review of the CRA and Agarwal et al. (2012) for a recent application on the effect of CRA on bank lending).

Indeed, a bank that wants to operate interstate branches in a newly deregulated state must meet the requirements of the CRA *in its home state*. Therefore, the bank may increase the supply of bank accounts to low-income households in its home state (the control state) before entering the newly deregulated state (the treated state).

4.2 Results

We begin by investigating whether and to what extent banking deregulation affects the share of unbanked households.

Table 3 reports four versions of our baseline regression, which all indicate a large and positive impact of banking deregulation on the share of banked households. The first column does not include any control. The coefficient on *Deregulation index* is 0.011 and significant at the 1% level. That is, when a state fully deregulates, we observe an increase in the share of households with a bank account of almost 4.5 percentage points. To further address endogeneity concerns, we then introduce our large set of household and state level controls in the second and third columns of Table 3. These controls capture factors that would foster the demand for banking services at the household level and the economic conditions that may drive deregulation. The coefficient on *Deregulation index* subsequently remains stable, suggesting that the deregulation is not correlated with other households and state-level characteristics that may affect the decision to open a bank account.

Finally, we analyze the dynamics of the share of banked households around deregulation and show graphically that the parallel trend assumption is verified. Figure 3 plots the change in the likelihood of holding a bank account in the years before and after a state deregulates (i.e., it relaxes at least two out of the four restrictions to out-of-state entry). The figure shows that the probability of holding a bank account is relatively high after deregulation and, most importantly, that there is no discernible pattern before the deregulation date. The fourth column of Table 3 confirms this result. We interact four dummy variables indicating four periods around the deregulation date with our deregulation index: more than 3 years before, less than 3 years before, 0 to 3 years after, and

more than 3 years after. We observe that only the interaction terms with the dummies indicating years after deregulation have a positive and significant coefficient. Therefore, we observe no pre-deregulation trend, and the share of banked household increases only after deregulation takes place. These findings suggest that deregulation is not endogenous to the share of unbanked households but *causes* an increase in the share of banked households.

INSERT TABLE 3 AROUND HERE

By combining the results in Table 2 and Table 3, we compute a back-of-the-envelop overall elasticity of bank-account to bank branch density in a county of roughly 30%.[20]

INSERT FIGURE 3 AROUND HERE

4.3 Branching Deregulation and Racial Discrimination

In this section, we investigate whether the effect of interstate branching deregulation is higher for households that are more likely to be rationed by banks.

Table 4 examines the impact of interstate branching deregulation among black households. Motivated by the overrepresentation of black households among the unbanked population that we observe in the aggregate data, and the effect of deregulation on branch density, we make the assumption that black households are more likely to be rationed by banks, in particular in states with a history of discrimination, as we know from the literature that norms and institutions have a long-term impact. Thus, following Chatterji and Seamans (2012), we exploit our four discrimination dummies – *Slave State*, *Banning Interracial Marriages*, *Absence of Fair Housing Law* and *Low Interracial Marriage Rate* – described in Subsection 2.4, to identify states with a history of discrimination.

Table 4 reports the result of the basic model after introducing the double interaction *Deregulation* $\times$*Black* in the first column, plus the triple interaction *Deregulation*

[20]Branch density increases by 28% (0.072$\times$4) after a state fully deregulates, while bank account increases by 8% (0.048$\times$0.61), 0.61 being the initial share of bank accounts among low-income households, hence leading to a total elasticity of $8/28 = 0.29$

$\times$*Black* $\times$ *Discrimination* for our four discrimination dummies in the final four columns. The coefficient of the double interaction *Deregulation* $\times$*Black* in the first column indicates whether the effect of deregulation is larger for black households than for non-black households. The coefficient of the triple interaction *Deregulation* $\times$*Black* $\times$ *Discrimination* in the other columns indicates whether the gap between black and non-black households reduces more in states with a history of discrimination. All the specifications also include *Deregulation* $\times$ *Discrimination* and a *Black* $\times$ *Discrimination* interactions.

INSERT TABLE 4

We first find that the effect of deregulation on the share of banked households is larger among black households than among non-black households (column (1)) as the coefficient of the *Deregulation* $\times$*Black* interaction is positive and significant. However, when we zoom-in and exploit differences across states, we find that this positive effect of deregulation on the white-black gap is mainly driven by states with a history of discrimination (columns (2) to (5)). The coefficient of the triple interaction *Deregulation* $\times$*Black* $\times$ *Discrimination* is always positive and significant for our four discrimination dummies. Furthermore, the coefficient of *Deregulation Index*, which measures the effect of banking deregulation on non-black households, does not decrease and is still highly significant in all the specifications of the table. This result suggests that the large effect of deregulation on black households does not drive our main result alone and that the entire population of low-income households also benefits from the reform.

These results are consistent with the ones presented in Subsection 3.2 where we find that interstate branching deregulation has a relatively larger effect on bank branch density in black counties in states with a history of discrimination.

One concern might be that black households are on average poorer than white households and therefore that our differential effect is driven by the fact that deregulation may have a bigger impact on the lowest part of the income distribution. To ensure that this is not the case, we re-run the same specification by splitting our sample across four income categories: "very poor" households (people living with less than half the poverty line),

"poor" households (below the poverty line), "low-income" (between once and twice the poverty line) and "middle income" (between twice and three time the poverty line). We report the results in Table 5. We find that the effect of deregulation keeps having a larger impact on black households in states with a history of discrimination across the whole income distribution considered.

4.4 Heterogeneous Treatment Effects

Table 6 investigates the heterogeneity of the effect of deregulation across various household characteristics. In the first three columns, we present the impact of deregulation along income distribution and test whether the poorest households, which are more likely to be rationed by banks' standard practices (e.g., minimum account balance) and more likely to live in poorer areas with a lower geographical coverage of branch density, are more impacted by deregulation.

We split our sample into three groups: poor households (below the poverty line), low-income households (between one and two times the poverty line) and middle income households (between two and three times the poverty line). Columns (1) to (3) of Table 6 show that the effect of deregulation is higher for poor households than for low income households and that there is no effect for middle-income households. More specifically, each step in the deregulation index induces a 1.6% increase in the probability of holding a bank account among poor households (column (1)) against a 0.8% increase among low-income households (column (2)). By contrast, deregulation has no significant impact on middle-income households (column (3)), which seems logical because middle-income households are less likely to face hurdles or entry barriers to opening a bank account. The absence of a significant effect on middle-income households also confirms that our main result does not simply capture a general decreasing trend in the share of unbanked households in the deregulated states.

INSERT TABLE 6 AROUND HERE

Columns (4) and (5) in Table 6 focus on the heterogeneous impact of deregulation

across geographical areas. We assume here that the effect of deregulation is higher in rural areas, where households are more likely to be rationed due to lower bank competition ex-ante. To test this hypothesis, we split our sample into "rural" (column (4)) and "urban" households (column (5)). We find that the coefficient of our deregulation index is 1.5 as large for households living in rural areas. This result is consistent with the idea that since rural areas are more likely to be dominated by few local banks, they experience the strongest competitive shocks.

Columns (6) and (7) investigate whether the impact of deregulation is larger for more educated household. Being unbanked is less likely to be driven by sophistication for these households because they have a higher level of financial literacy (Lusardi and Mitchelli, 2007). To do so, we split our sample between households with at least a high school degree in column (6) and household with only elementary education in column (7). We find that the effect of deregulation appears mostly for more educated households (column (6)), consistent with the notion that there is an existing demand for bank account that was constrained by the lack of bank branch presence.

5 Banking Deregulation and Asset Accumulation

This section investigates the impact of branching deregulation on households' savings and asset accumulation.

5.1 Instrument Variable Estimation

Table 1 indicates a positive correlation between access to bank account and household wealth: banked low-income households are roughly four time wealthier than unbanked low-income household. However, causality could go in both directions. Households that are more prone to saving are likely to both be wealthier and own a bank account. We therefore use our branching deregulation index as an instrument to estimate causally whether being unbanked limit wealth accumulation. Because deregulation has no effect either on county prosperity or on SIPP household income or unemployment, the exclusion

restriction is satisfied.

Table 7 reports the results. We run the same baseline specification as in equation 2, using as dependent variables the total amount of bank assets, (columns (3)–(4)), the log of total wealth (columns (5)–(6)) and net worth (columns (7)–(8)). For each specification, we first estimate an ordinary least square model (OLS), where *Bank Account* is the endogenous variable, and we then instrument *Bank Account* with *Deregulation Index*. In each case, we find that the predictive power of *Deregulation Index* is sufficiently high to be a valid strong instrument (F-test of around 10), and that the instrument variable estimate comforts the result of the OLS one.

The large effect of holding a bank account on bank assets (columns (3)–(4)) is somewhat mechanical but reassuring. This result relieves the concern that unbanked households open a bank account only, for example, to have access to mortgage debt, and not to accumulate savings. By contrast, given the limited alternative formal saving instruments available to low-income, we should expect their bank assets to be higher when they have a bank account if financial inclusion has an impact of wealth accumulation. This is precisely what we observe.

When looking at total wealth, we find that having a bank account translates into a multiplication by around three of household wealth, whether we use the (endogenous) OLS (column (5)) or the IV specification (column (6)). While this difference may seem large, it is actually smaller than the raw difference in total wealth in the summary statistics where we find that banked households have five time the wealth of unbanked households.

In columns (7) and (8), we look at net worth that we defined as total wealth minus total debt, as having a bank account may simply has an effect on household total wealth because now low-income households can borrow more. We actually find that having a bank account has a very similar effect on net worth, where *Bank Account* multiplies by around three household net worth. This result suggests that our result on total asset is not driven only by easier access to credit and higher debt accumulation.

INSERT TABLE 7 AROUND HERE

5.2 Branching Deregulation and Debt

One concern with our results would be that they are driven only by household better access to debt. While access to debt may lift household out of poverty, excess debt can have negative effect.

We start by estimating the baseline model 2 and split the sample between households with a bank account (column (1)) and debt and households with a bank account and no debt (column (2)). The objective is to show that deregulation can have an effect on holding a bank account also above and beyond debt accumulation. It may be the case that intensified bank competition provides banks with incentives to increase the credit supply for low-income households and to subsequently offer them the opportunity to open bank accounts.[21] We find a similar point estimate for both samples and in particular still find that deregulation increases by 1.1 p.p the likelihood for low-income households to open a bank account with *no debt in it*. Therefore, it does not seem to be the case that our effect is driven by the fact that credit supply for low-income households increases after deregulation and as a result, low-income households open a bank account.

6 Robustness

6.1 Alternative story: better job outcome

One alternative explanation for our result is that interstate branching deregulation improves economic conditions for low-income households and in particular help them getting a job, which leads them to open a bank account. Higher financial inclusion would simply be a nice "collateral benefit" of better employment status.

We already alleviate this concern in two ways. First, the point estimate of banking deregulation does not change when we control for many economic characteristics at the

[21] Access to formal credit for previously excluded population is likely to be welfare improving given that implied annual percentage rates charged by payday loans are usually well over 400% (Bertrand and Morse, 2011). However, as noted by Dick and Lehnert (2010), improving credit availability to borrowers who are poorer, likely to be less educated and therefore more likely to make borrowing mistakes, raises the concern of excess lending to this category.

household and state level, including non-parametric control for household income. Second, we show in Subsection 3.3 that our deregulation (unlike the first waves of the 70s – 80s) has *no* effect on county level personal income, unemployment rate, poverty rate, personal income growth.

To alleviate further this concern, we test directly on our SIPP sample if banking deregulation has an impact on household unemployment, income and if our effect varies for households more or less likely to be unemployed at some point. Table 8 shows that interstate branching deregulation has no impact on the likelihood for the household to be unemployed (column (1)) or on the log of household income (column (2)), consistent with the absence of aggregate effect at the county level we find in Table A.3. We also estimate our baseline specification on two sub-sample: households likely and unlikely to be unemployed. To do so, we first regress household employment status on all the covariates used so far and then predict the probability for a household to be unemployed. We then consider a household to be "likely unemployed" if its probability is above the sample median. If branching deregulation was improving job prospects for low-income households, we would expect more vulnerable households (i.e. households more likely *a priori* to be unemployed) to react more to the deregulation. Columns (3) and (4) show that *Deregulation Index* has roughly the same effect on the probability of having a bank account and that if anything, the point estimate for households less likely to be unemployed is stronger.

INSERT TABLE 8 AROUND HERE

6.2 The Effect of Deregulation across Periods and States

In this section, we run a set of standard robustness checks.

First, we show that our result does not capture a general trend in the share of unbanked households in states that deregulate. To do so, we perform a placebo test and randomly change the date of each state deregulation in column (1) in Table 9. If the effect we are measuring simply results from a trend, by randomly changing the deregulation

date we should still observe a positive and significant impact of deregulation. Column (1) in Table 9 shows that the coefficient of the deregulation index is no longer significant and that the point estimate is actually close to 0. In column (2) we re-run our baseline regression and directly add *State* $\times$ *Trend* control variables, such that the effect of the reform is identified purely by a deviation from a trend that differs for each state. Column (2) indicates that such a variable does not affect our results.

In column (3), we interact the current deregulation index with a dummy equal to one if branch density at the county level in 1994 in a given state is above the median sample. We find that the effect is smaller for states where branch density was higher at the beginning of the period, consistent with the idea that the effect of the new deregulation produced a positive supply shock in particular in states where households are more likely to be rationed due to initial lower bank branch coverage.

We then run two other types of robustness checks. First, we check that our results are robust to the sample period. Column (4) starts the sample in 1997 (the date at which the IBBEA becomes effective), and column (5) ends it in 2005 (the date before our gap in the data). Second, we consider what happens when we use different control groups. Because our dependent variable is an index, the identification comes both from the comparison between states that never deregulate with states that deregulate and from the comparison between states that deregulate *more* than others (for instance the comparison between states that move from an index of 1 to 2 as opposed to a state that stays at 1). In column (6) we replace our index with a simple dummy variable that takes the value 1 if a state has adopted at least two of the four possible deregulation. By contrast, in column (7), we restrict our sample to states that have already deregulated at least once and use our index variable such that the identification comes purely from the increment of the index and the control group is always composed of states that have deregulated at least once. Reassuringly, our results hold in both cases.

Finally, in column 8, we restrict the sample to the largest 11 states (California, Florida, Georgia, Illinois, Michigan, Missouri, New York, North Carolina, Ohio, Pennsylvania and Texas) to ensure that our results are not driven only by small states. We find that our

results still hold.

INSERT TABLE 9 AROUND HERE

7 Conclusion

In this paper, we investigate whether intensified bank competition can have a positive impact on the share of banked households among low-income populations. We exploit interstate bank branching deregulation in the U.S. after 1994 as an exogenous shock on branch entry. We find that the share of unbanked households decreases in the years following deregulation. This result is consistent with the hypothesis that supply-side factors contribute to the unbanked phenomenon.

By examining the impact of bank competition on access to bank accounts across household types, we confirm the robustness of our results. We find that the effect of intensified bank competition is stronger for populations that are more likely to be restricted by banks. Hence, black households benefit more from deregulation than do non-black households in states with a history of discrimination. The effect of deregulation is also higher for households below the poverty threshold that are more likely to face entry barriers, such as minimum account balances for opening a bank account.

We also find that the increase in the likelihood of holding a bank account resulting from intensified geographical bank branch density improves total wealth and net worth of low-income households, which suggests that having access to the formal banking sector plays a role in asset accumulation.

Finally, we rule out the alternative interpretation of our result that bank competition decreases the share of unbanked households by fostering demand for bank accounts. First, deregulation has no impact either on the sample of non-deregulated institutions or on county prosperity or individual income and employment status. Second, in all of our specifications, we control for a large set of covariates that capture demand effects at both the household and state levels.

Our paper shows that an intensification of bank competition promotes access to banking services for low-income households. It suggests that changes in banking regulation could impact minority access to financial services. Because households with no bank accounts turn to alternative financial services, this raises the question of how bank competition interacts with this sector. We leave this question for future research.

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A Figures

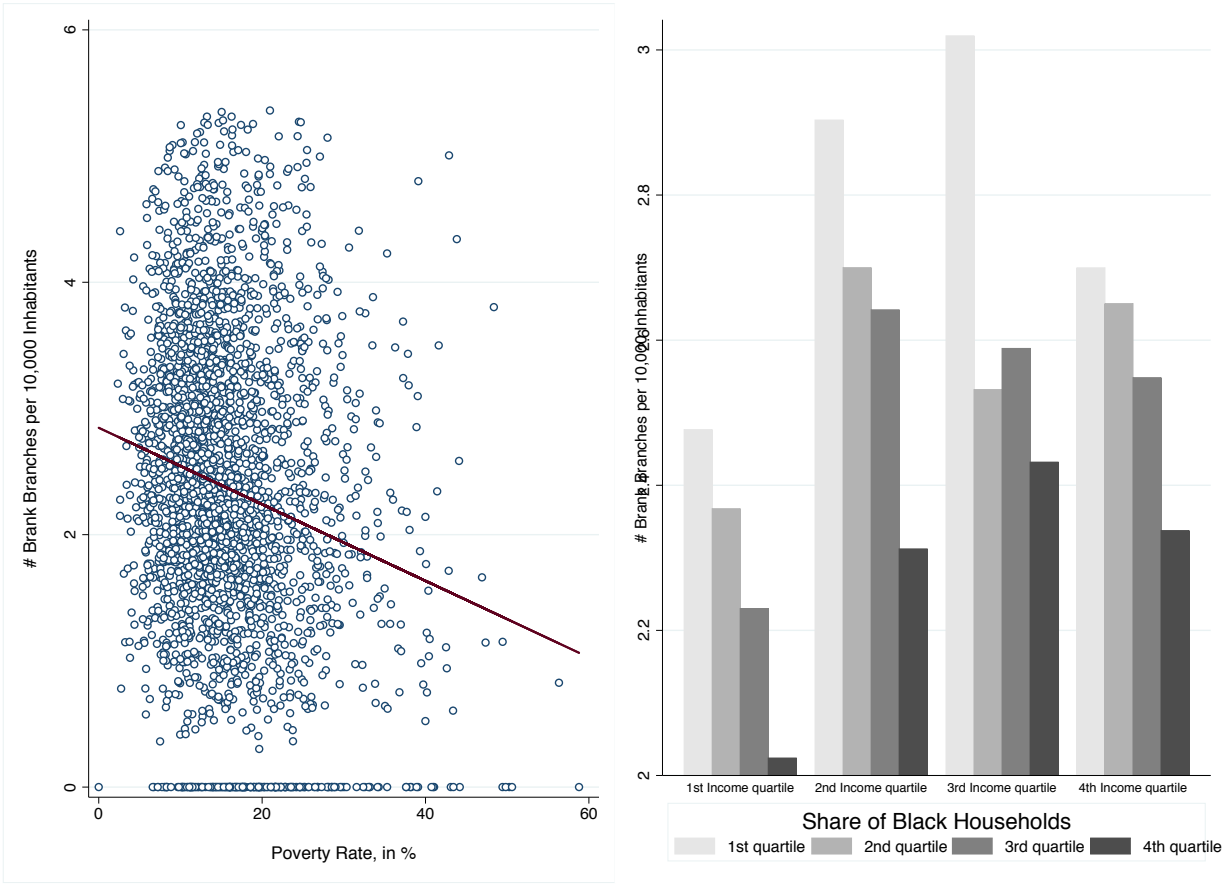

Figure 1. Bank Branch Density and County Characteristics, 1994

This figure shows bank branch density at the county level, calculated as the number of branches per 10,000 inhabitants over county poverty rate (left-hand side) and county share of black households in the population (right-hand side). For the figure in the right-hand side, counties are divided in quartiles of income and bank branch density is computed for each quartile of the share of black households within each quartile of income. Data are form the FDIC and BEA.

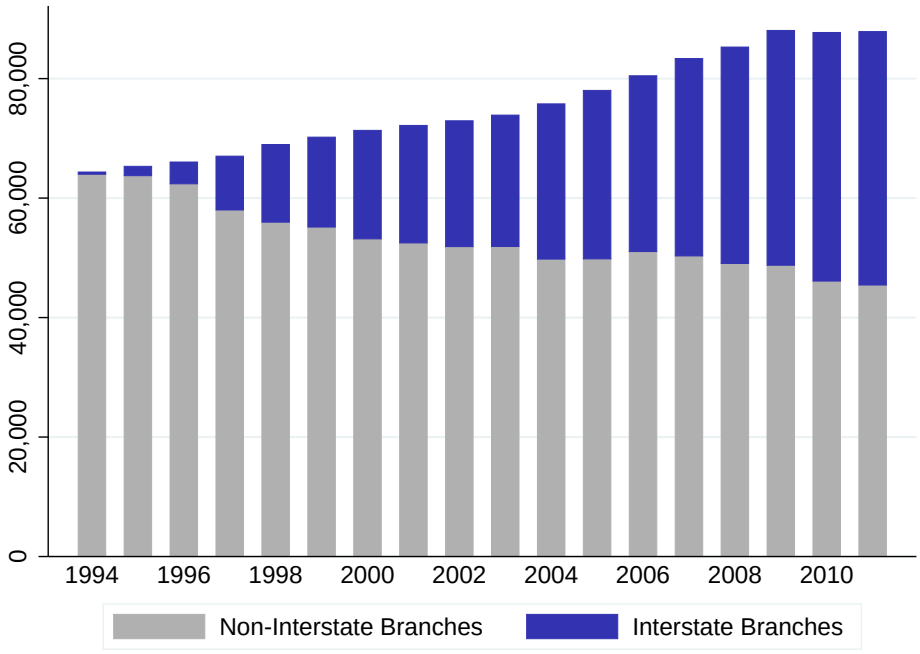

Figure 2. Number of Branches Operated by FDIC-insured Commercial Banks

This figure shows the number of interstate and non interstate branches operating in the U.S. over the years. Data are form the FDIC.

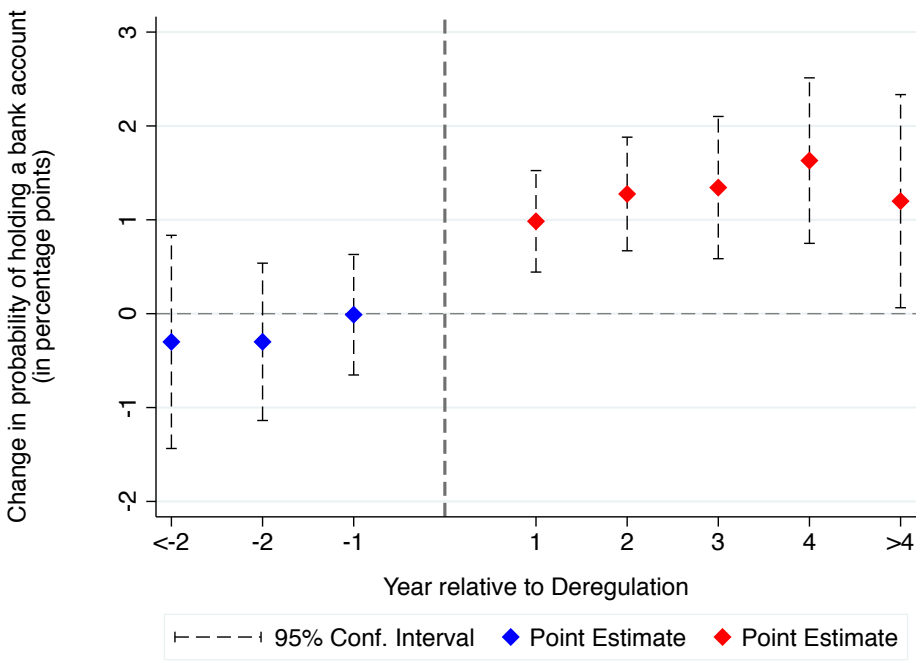

Figure 3. The Impact of Banking Deregulation on the Share of Banked Households

This figure shows the relative change in odd ratios of holding a bank account around deregulation dates among low-income households , where deregulation is defined as a state removal of at least two interstate branching restriction. The specification is the same as equation (1) except that the deregulation index is replaced by dummy variables I(k) equal to one exactly k years after (or before if k is negative) interstate branching deregulation. The point estimates of the dummy variables I(k) and the 95% confidence intervals are plotted. Standard errors are clustered at the state-level.

B Tables

Table 1. Summary Statistics

Sample	**Unbanked Households**				**Banked Households**			
	Mean	*p10*	*p50*	*p90*	*Mean*	*p10*	*p50*	*p90*
Sociodemographics								
Monthly Household Income	890	418	723	1,596	1,169	469	1,027	2,131
Black (%)	32	0	0	100	13	0	0	100
Age (year)	48	26	45	77	53	28	51	81
Elementary Education (%)	40	0	0	100	23	0	0	100
High School Education (%)	37	0	0	100	36	0	0	100
College Education (%)	24	0	0	100	42	0	0	100
Married Couple (%)	26	0	0	100	42	0	0	100
Single Female-Headed (%)	54	0	100	100	43	0	0	100
Household Size	2.5	1	2	5	2.5	1	2	5
Recipients of Social Security (%)	44	0	0	100	46	0	0	100
Recipients of Transfer Income (%)	52	0	100	100	25	0	0	100
Unemployed Head of Household (%)	11	0	0	100	7.8	0	0	0
Assets								
Wealth	17,959	0	602	56,206	67,840	145	22,842	188,545
Bank Assets	0	0	0	0	4,682	0	459	12,600
Home Equity	13,181	0	0	48,186	36,681	0	7,853	110,956
Net Worth	17,342	-260	443	55,347	65,011	-2,345	19,694	185,134
Credit Card Debt and Bills	246	0	0	236	870	0	0	3,212
Bank Loans	0	0	0	0	182	0	0	1
Mortgages	0	0	0	0	14,620	0	0	57,636
Observations	*40,248*				*89,668*			

This table reports summary statistics on the socio-demographic characteristics of banked and unbanked low-income households and their assets. Data comes from the 1992, 1993, 1996, 2001, 2004 and 2008 panels of the Survey of Income and Program Participation (SIPP). The left-hand side of the table displays the mean, 10th percentile, median and 90th percentile values of these characteristics for the sample of unbanked households, whereas the right-hand side of the table displays the mean, 10th percentile, median and 90th percentile values of these characteristics for the sample of unbanked households. Banked households hold a checking or a savings account. All nominal variables are deflated using the CPI in 1993.

Table 2. Interstate Branching Deregulation and Bank Branch Coverage

	Branch Density Per Inhabitants				**Branch Density Per Square Miles**			
Counties	All	Low Income	Black History of Discrimination		All	Low Income	Black History of Discrimination	
			No	Yes			No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Commercial Banks								
Deregulation	0.068***	0.072***	0.059*	0.106**	0.066***	0.071***	0.053	0.105**
	(0.023)	(0.026)	(0.034)	(0.041)	(0.023)	(0.025)	(0.033)	(0.041)
Time-Varying County Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	46,350	11,034	3,722	5,349	46,350	11,034	3,722	5,349
Panel B. Credit Unions								
Deregulation	0.002	-0.000	-0.007	0.008	0.000	-0.003	-0.012	0.011
	(0.009)	(0.008)	(0.007)	(0.008)	(0.008)	(0.007)	(0.008)	(0.011)
Time-Varying County Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	23,601	4,747	4,044	3,705	23,601	4,747	4,044	3,705

This table reports OLS regressions of the Interstate Deregulation Index on bank and credit union density (Panels A and B respectively). The dependent variable is the log of the number of branches in each county per 1,000 inhabitants in columns (1) to (3) and per square miles in columns (4) to (6). The deregulation index ranges from 0 to 4, 0 is least, 4 is most deregulated. In columns (2) and (6) the sample is restricted to counties in the top quartile of the distribution in terms of poverty rate, in columns (3) and (7) to counties both in the top quartile of the distribution in the ratio of black households and in states with no history of discrimination, and in columns (4) and (8) to counties both in the top quartile of the distribution in the ratio of black households and in states with a history of discrimination. States with a history of discrimination are the states that did not repeal anti-miscegenation law until after the US Supreme Court's 1967 decision in *Loving v. Virginia*. All regressions include county and year fixed effects. Time-varying county controls include controls for the log and the delta log of per capital income and population, the poverty rate and the unemployment rate. Standard errors are clustered by state.

Table 3. The Impact of Interstate Branching Deregulation on the Share of Banked Households

Dependent variable	**=1 if the household holds a bank account**			
	(1)	**(2)**	**(3)**	**(4)**
Deregulation Index	0.011***	0.011***	0.011***	
	(0.004)	(0.004)	(0.004)	
Deregulation ($\leq$ t-4)				-0.014
				(0.014)
Deregulation (t-3,t-1)				-0.013
				(0.011)
Deregulation (t+1,t+3)				0.027**
				(0.013)
Deregulation ($\geq$ t+4)				0.031*
				(0.017)
Household Controls				
Income Deciles FE	-	Yes	Yes	Yes
Houshold Size FE	-	Yes	Yes	Yes
Sociodemographics	-	Yes	Yes	Yes
Time-Varying State Controls	-	-	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Observations	130,125	130,125	130,125	130,125
R^2	0.028	0.206	0.206	0.206

This table reports linear probability regressions of the Interstate Branching Deregulation Index on access to bank accounts. The dependent variable equals 1 if the household holds a checking or a savings account (SIPP 1993 – 2010). The deregulation index ranges from 0 to 4, 0 is least, 4 is most deregulated. Column (1) does not include any control, while columns (2), (3) and (4) include household controls, plus time-varying state controls in columns (3) and (4). All regressions include state and year fixed effects. In column (4) the deregulation index is split into four sub-periods: more than 3 years before deregulation, less than 3 years before deregulation, 0 to 3 years after deregulation, and more than 3 years after deregulation, where deregulation corresponds to the removal of at least two out of the four possible restrictions. Household controls include monthly income decile fixed effects, household size fixed effects, family type, dummies for whether the household receives Social Security income or transfer income, household head education dummies (elementary, high school, college), unemployment dummy, *Black* dummy indicating whether the household head is black, and age as a cubic polynomial. State time varying controls include the unemployment rate, population (log), GDP growth and a republican dummy. Standard errors are clustered by state.

Table 4. The Impact of Interstate Branching Deregulation on the Share of Banked Households: Evidence on Racial Discrimination

	=1 if the household holds a bank account				
Discrimination Dummy	**-**	**Former Slave State**	**Antimi-scegenation** Law	**No Fair Housing Law**	**Share of interacial marriage**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Deregulation Index	0.009**	0.011**	0.010**	0.010**	0.011**
	(0.004)	(0.005)	(0.004)	(0.005)	(0.004)
Index x Black	0.009*	-0.001	0.003	-0.004	-0.001
	(0.005)	(0.006)	(0.006)	(0.006)	(0.005)
Index x Black x Discrimination		0.021***	0.018**	0.025***	0.024***
		(0.007)	(0.007)	(0.007)	(0.007)
Household Controls					
Income Decile FE	Yes	Yes	Yes	Yes	Yes
Houshold Size FE	Yes	Yes	Yes	Yes	Yes
Sociodemographics	Yes	Yes	Yes	Yes	Yes
Time-Varying State Controls	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	130,125	130,125	130,125	130,125	130,125
R^2	0.207	0.207	0.207	0.207	0.207

This table reports linear probability regressions where the dependent variable is a dummy indicating whether the household owns a checking or a savings account. *Deregulation Index* is interacted with the dummy *Black* that values 1 when the household head is black. In columns (2) to (5), four racial discrimination dummies are interacted first, with the *Black* dummy, second, with the interaction *Deregulation* $\times$ *Black*. The first discrimination dummy - column (2) - indicates whether the state was a slave state in the year immediately prior to Civil war (1 if yes, 0 if not), the second one - column (3) - whether anti-miscegenation law was not repealed until after the US Supreme Court's 1967 decision in *Loving v.Virginia*, the third one - column (4) - whether no fair housing law was voted until federally mandated by the Fair Housing Act of 1968, and the fourth one - column (5) - whether the interracial marriage rate is below the median. All regressions include *Deregulation* $\times$ *Discrimination*, *Black* $\times$ *Discrimination* controls, as well as household controls, time varying state controls, and state and year fixed effects. Household controls include the *Black* dummy, monthly income decile fixed effects, household size fixed effects, family type, dummies for whether the household receives Social Security income or transfer income, household head education dummies (elementary, high school, college), unemployment dummy and age as a cubic polynomial. State time varying controls include the unemployment rate, population (log), GDP growth and a republican dummy. All household and state controls are interacted with *Black*. Standard errors are clustered by state.

Table 5. Racial Discrimination Across Income Groups

	=1 if the household holds a bank account			
Sample	**Very Poor Households (1)**	**Poor Households (2)**	**Low Income Households (3)**	**Middle Income Households (4)**
Deregulation Index	0.021***	0.010	0.008*	-0.000
	(0.008)	(0.007)	(0.004)	(0.003)
Index x Black	-0.008	-0.010	0.001	-0.003
	(0.009)	(0.009)	(0.007)	(0.007)
Index x Black x Discrimination	0.026*	0.028**	0.019**	0.019*
	(0.013)	(0.011)	(0.009)	(0.010)
Household Controls				
Income Decile FE	Yes	Yes	Yes	Yes
Houshold Size FE	Yes	Yes	Yes	Yes
Sociodemographics	Yes	Yes	Yes	Yes
Time-Varying State Controls	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Observations	17,209	32,694	80,222	35,220
R^2	0.285	0.159	0.117	0.082

This table reports linear probability regressions where the dependent variable is a dummy indicating whether the household owns a checking or a savings account. *Deregulation Index* is interacted with the dummy *Black* that values 1 when the household head is black. The Discrimination dummy indicates whether no fair housing law was voted until federally mandated by the Fair Housing Act of 1968 and is interacted first, with the *Black* dummy, second, with the interaction *Deregulation* × *Black*. Data are split into four exclusive samples of households based on their annual income: *very poor* (below half the poverty line) in column (1), *poor* (between half and once the poverty line) in column (2), *low-income* (between once and twice the poverty line) in column (3) and *middle income* households (between twice and three times the poverty line) in column (4). All regressions include *Deregulation* × *Discrimination*, *Black* × *Discrimination* controls, as well as household controls, time varying state controls, and state and year fixed effects. Household controls include the *Black* dummy, monthly income decile fixed effects, household size fixed effects, family type, dummies for whether the household receives Social Security income or transfer income, household head education dummies (elementary, high school, college), unemployment dummy and age as a cubic polynomial. State time varying controls include the unemployment rate, population (log), GDP growth and a republican dummy. Standard errors are clustered by state.

Table 6. Heterogenous Effect of Interstate Branching Deregulation across Household Types

	=1 if the household holds a bank account						
	Income Group			Residence		Education	
Sample	**Poor**	**Low**	**Middle**	**Rural**	**Urban**	**High**	**Low**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Deregulation Index	0.016***	0.008*	-0.001	0.014**	0.009**	0.012**	0.005
	(0.004)	(0.004)	(0.002)	(0.006)	(0.004)	(0.004)	(0.006)
Household Controls							
Income Decile FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Houshold Size FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sociodemographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-Varying State Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	49,903	80,222	35,220	35,503	94,621	93,822	36,303
R^2	0.200	0.116	0.082	0.210	0.209	0.166	0.199

This table investigates the effect of banking deregulation on access to bank accounts across various types of households. In columns (1) to (3) we split the sample into three groups based on income level: "Poor" is below the poverty line, "Low" is between once and twice the poverty line and "Middle" is between two and three times the poverty line. Columns (4) and (5) split the initial sample of low income households between households living in rural and urban areas. We define as *Urban*, the Metropolitan areas of the Census, which contains a core urban area of 50,000 or more population. Finally, columns (6) and (7) split the initial sample between low educated (less than high school) and highly educated (high school or higher) households. Standard errors are clustered by state.

Table 7. The Effect of Financial Inclusion on Asset Accumulation

							=1 if HH holds a bank account	
	Bank Assets		**Log(Total Wealth)**		**Log(Net Worth)**		**Without Any Debt**	**Joint With Debt**
Estimation	OLS (1)	IV (2)	OLS (3)	IV (4)	OLS (5)	IV (6)	OLS (7)	OLS (8)
Savings or Checking Account	3,738***	10,733*	3.036***	2.931**	3.028***	3.463**		
	(125)	(5937)	(0.192)	(1.398)	(0.197)	(1.421)		
Deregulation Index							0.011**	0.011**
							(0.004)	(0.004)
Household Controls								
Income Decile FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Houshold Size FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sociodemographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-Varying State Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	130,125	130,125	122,232	122,232	110,475	110,475	64,083	97,913
R^2	0.106	0.062	0.436	0.435	0.432	0.430	0.252	0.267
F-test								

This table reports the effect of financial inclusion on asset accumulation. Columns (1) and (2) show the effect of deregulation on the probability of holding a bank account without any debt (column (1)) and joint with debt (column (2)). The sample includes households without neither debt nor bank account in columns (1) and (2) and is extended to households with bank account alone in column (1) and with bank account and debt in column (2). Columns (3) to (8) investigate the effect of holding a bank account on the amount of bank assets (columns (3) and (4)), the log of household wealth (columns (5) and (6)) and the log of household net worth (columns (7) and (8)). Columns (3), (5), and (7) are OLS regressions, while in columns (4), (6), and (8) we instrument the bank account dummy with our deregulation index. Household and state controls are the same as previously described. Standard errors are clustered by state.

Table 8. Ruling Out Demand Factors

Dependent variable	**=1 if HH Head is Unemployed**	**Log(HH Income)**	**=1 if the HH holds a bank account**	
Sample	All		HH Head Likely Unemployed	
			No	*Yes*
	(1)	(2)	(3)	(4)
Deregulation Index	0.002	0.001	0.013***	0.009**
	(0.001)	(0.002)	(0.005)	(0.004)
Household Controls				
Income Decile FE	-	-	Yes	Yes
Houshold Size FE	Yes	Yes	Yes	Yes
Sociodemographics	Yes	Yes	Yes	Yes
Time-Varying State Controls	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Observations	130,125	130,125	68,993	61,132
R^2	0.054	0.383	0.174	0.237

This table reports the effect of Interstate Branching Deregulation on the likelihood the household head is unemployed (column (1)) and on the log of the household total income (column (2)). In column (3) and (4), we estimate the household head probability to be unemployed based on household and state characteristics and split the sample into two: the probability is below the median (column (3)) or above the median (column (4)). Household and state controls are the same as previously described, except that income controls are excluded in columns (1) and (2). Standard errors are clustered by state.

Table 9. Robustness Checks

Dependent Variable	**=1 if the household holds a bank account**							
Sample	**All**			**Time Periods**		**States**		
				1997 -2010	**1993 -2005**	**All**	**Only Deregulated**	**Largest**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Placebo Index	0.001							
	(0.003)							
Deregulation Index		0.010**	0.017***	0.013***	0.018***		0.012**	0.012**
		(0.005)	(0.004)	(0.004)	(0.004)		(0.005)	(0.005)
Deregulation			-0.009**					
× High Initial Density			(0.004)					
Deregulation Dummy						0.037***		
						(0.010)		
Household Controls								
Income Decile FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Houshold Size FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sociodemographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-Varying State Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × Year Trend	–	Yes	–	–	–	–	–	–
Observations	127,096	127,096	127,096	105,534	100,329	127,096	113,666	69,667
R^2	0.212	0.217	0.212	0.217	0.203	0.212	0.211	0.215

This table reports robustness check estimations. In Column (1) the dependent variable is a placebo index where deregulation dates are randomly generated for each state. In Column (2) the dependent variable is the deregulation index and state×trend effects are included. In Column (3) *Deregulation* is interacted with a dummy equal one if the branch density at the county level in 1994 is above the median sample. Columns (4) and (5) split the sample into two sub-periods: (1997–2010) and (1993–2006). In Column (6) the dependent variable is a dummy equal one if at least two different deregulation have been implemented. In Column (7) the sample is restricted to states with at least one deregulation over the 1993–2010 period, and in Column (8) to the 10 largest states. All regressions include state and year fixed effects. Household and state controls are the same as previously described. Standard errors are clustered by state.

Appendix A Tables

Table A.1. Standard Determinants of Access to Bank Accounts

Dependent Variable	**=1 if the HH holds a bank account**	
Household Sociodemographic Controls		
Black	-0.161***	(0.010)
Married Couple	0.114***	(0.006)
Single Female-Headed	0.035***	(0.007)
Age	-0.004	(0.003)
Age^2	0.0002***	(0.0000)
Age^3	-0.0001***	(0.0000)
High School Education	0.103**	(0.009)
College Education	0.213***	(0.012)
Household Size	-0.026***	(0.003)
Household Income Controls		
Receive Social Security	-0.003	(0.007)
Receive transfer income	-0.143***	(0.006)
Head unemployed	0.022***	(0.004)
2nd Income Decile	0.023***	(0.008)
3rd Income Decile	0.065***	(0.005)
4th Income Decile	0.109***	(0.007)
5th Income Decile	0.130***	(0.006)
6th Income Decile	0.156***	(0.006)
7th Income Decile	0.174***	(0.008)
8th Income Decile	0.202***	(0.010)
9th Income Decile	0.266***	(0.009)
10th Income Decile	0.338***	(0.012)
Time Varying State Controls		
GDP Growth	-0.175	(0.115)
Population	-0.003	(0.128)
State Unemployment	-0.002	(0.006)
Republican State	-0.001	(0.006)
Year Fixed Effects	Yes	
State Fixed Effects	Yes	
Observations	*130,125*	
R^2	*0.205*	

This table reports a linear probability regression of household and state-year controls on access to bank accounts. The dependent variable equals 1 if the household holds either a checking or savings account (SIPP 1993 - 2010). The regression includes state and year fixed effects. Standard errors are clustered by state.

Table A.2. Branching Deregulations and Branch Density: Alternative Proxies for Discrimination

	Log(Branch Density)					
Discrimination Proxy	**Antimiscegenation Law** *Discrimination*		**No Fair Housing** *Discrimination*		**Interracial Marriage** *Discrimination*	
	No	Yes	No	Yes	No	Yes
Deregulation	0.054	0.106**	-0.001	0.071***	0.062	0.059
	(0.034)	(0.044)	(0.017)	(0.026)	(0.038)	(0.037)
Time-Varying County Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,749	5,268	4,170	7,260	4,204	7,226

Table A.3. The Impact of Branching Deregulations on County Prosperity

	Personal Income			**Unemployment Rate**			**Poverty Rate**			**Household Leverage**		
Counties	All	Black	Poor	All	Black	Poor	All	Black	Poor	All	Black	Poor
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Deregulation	0.001	0.005	0.012	0.000	-0.000	-0.002	-0.000	0.002	-0.003	-0.008	-0.005	-0.007
	(0.006)	(0.006)	(0.007)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)	(0.011)	(0.011)	(0.023)
Log(County Population)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	59,742	14,920	14,920	59,700	14,906	14,918	59,685	14,914	14,914	27,375	8,073	6,578
R^2	1.00	1.00	1.00	0.79	0.78	0.76	0.78	0.74	0.63	0.96	0.96	0.95

This table reports OLS regressions of the Interstate Branching Deregulation Index on measures of county prosperity and household leverage. The deregulation index ranges from 0 to 4, 0 is least, 4 is most deregulated. Data are from the Census. Standard errors are clustered by state.

Table A.4. Addressing Endogeneity Concerns

	Duration Model for the Time until Deregulation					
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Unbanked HH (%)	-1.85					-4.14
	(1.916)					(3.033)
Unbanked Low income HH (%))		-3.98				
		(2.778)				
Unbanked Black HH (%)			0.38			
			(3.524)			
Black HH (%)				-0.26		2.64
				(1.496)		(2.380)
UE rate (%)				0.12		0.21
				(0.121)		(0.186)
GDP per capita				0.03**		0.01
				(0.014)		(0.019)
Republican dummy					-0.18	0.22
					(0.372)	(0.439)
Share of small banks					-19.57***	-18.97***
					(5.134)	(6.172)
Relative capital ratio of small banks					0.00	0.00
					(0.000)	(0.000)
Relative size of insurance					1.01***	1.17***
					(0.369)	(0.323)
Observations	1,773	1,773	1,773	1,773	1,773	1,773

The hazard model is Weibull, where the dependent variable is the log expected time to deregulation (Kroszner and Strahan, 1999). All variables are measured for each state in each year. The share of unbanked households, low-income unbanked households or black unbanked households are measured at the state level in 1994. The share of black people in the state population, unemployment rate and real GDP per capita is at the state-year level. Share of small banks is the percent of banking assets in the state held by banks below the median size of banks in each state in each year. Relative capital ratio of small banks is the capital to assets ratio of small banks minus that of large banks. Relative size of insurance relative to banking plus insurance in the state is measured as gross state product from insurance divided by gross state product from insurance plus banking. Republican is equal to one if the majority of the voters chose the Republican candidate in the latest presidential election.

Table A.5. The Impact of Branching Deregulation on the Share of Banked Households: Evidence on Racial Discrimination (2)

	Slave Territory		**Antimisce -genation Law**		**No Fair Housing Law**		**Share of interacial marriage**	
	No	**Yes**	**No**	**Yes**	**No**	**Yes**	**>Median**	**<Median**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Deregulation Index	0.005	0.009	0.007	0.008	0.002	0.013**	0.008	0.011*
	(0.006)	(0.006)	(0.005)	(0.007)	(0.007)	(0.006)	(0.005)	(0.007)
Index x Black	-0.002	0.022***	0.003	0.022***	-0.005	0.023***	-0.001	0.022***
	(0.008)	(0.005)	(0.008)	(0.006)	(0.007)	(0.005)	(0.006)	(0.006)
Household Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Year Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	66,017	69,917	76,913	57,805	66,779	69,397	77,960	58,216

This table reports linear probability regressions of the Interstate Branching Deregulation Index on access to bank accounts and its interaction with a black dummy. The dependent variable equals 1 if the household holds a checking or savings account (SIPP 1993 - 2010). The deregulation index ranges from 0 to 4, 0 is least, 4 is most deregulated. For each set of regressions, the data are split into two mutually exclusive samples: slave state in the year immediately prior to the Civil War (yes or no), anti-miscegenation law not repealed until after the US Supreme Court's 1967 decision in *Loving v.Virginia* (yes or no), no fair housing law until federally mandated by the Fair Housing Act of 1968 (yes or no), racial bias rate, as measured by the interracial marriage rate (below or above median). All regressions include state and year fixed effects. Household and state controls include controls for family type, race, age, size of the household, education, receipt of Social Security income or transfer income, monthly income, unemployed status and state unemployment, population (log), GDP growth and a republican dummy. Standard Errors are clustered by state.

Table A.6. Percent of Unbanked Households by Data Source and Year

Year	Survey of Income and Program Participation	Panel Study of Income Dynamics	SIPP (low-income Households)
1993	16.4		36.3
1994	16.5	22	35.5
1995	16.8		35.6
1996	17.8		36.8
1997	19.2		39.2
1998	19.1		38.6
1999	19.4	23.3	38.8
2000	19.5		38.5
2001	20.1	24.8	38.6
2002	21.4		40.6
2003	22.4	25.7	40.6
2004	19.0		38.6
2005	18.6	25.8	36.7
2009	19.0	26.9	36.7
2010	20.6		38.4

This table reports the share of unbanked households. Percentages are authors' calculations except for the 1994 Panel Study and Income Dynamics one, which is from Hogarth and O'Donnell (1999).

Table A.7. State Interstate Branching Laws: 1994-2010

This table shows for every state the year in which the deregulation reforms came into effect and gives the deregulation index resulting from these changes. Rice and Strahan (2010) construct a time-varying index to capture these state-level differences in regulatory constraints. The index ranges from 0 to 4, 4 indicating maximum openness to out-of-state branching.

State	Effective Year	No Minimum Age on target	Allows De Novo Branching	Allows Single Branch Acquisition	Deposit cap higher than 30%	Index
Alabama	1997	0	0	0	1	1
Alaska	1994	0	0	1	1	2
Arizona	1996	0	0	0	1	1
Arizona	2001	0	0	1	1	2
Arkansas	1997	0	0	0	0	0
California	1995	0	0	0	1	1
Colorado	1997	0	0	0	0	0
Connecticut	1995	0	1	1	1	3
Delaware	1995	0	0	0	1	1
DC	1996	1	1	1	1	4
Florida	1997	0	0	0	1	1
Georgia	1997	0	0	0	1	1
Georgia	2002	0	0	0	1	1
Hawaii	1997	0	0	0	1	1
Hawaii	2001	1	1	1	1	4
Idaho	1995	0	0	0	1	1
Illinois	1997	0	0	0	1	1
Illinois	2004	1	1	1	1	4
Indiana	1997	1	1	1	1	4
Indiana	1998	0	1	1	1	3
Iowa	1996	0	0	0	0	0
Kansas	1995	0	0	0	0	0
Kentucky	1997	0	0	0	0	0
Kentucky	2000	1	0	0	0	1
Kentucky	2004	1	0	0	0	1
Louisiana	1997	0	0	0	1	1
Maine	1997	1	1	1	1	4
Maryland	1995	1	1	1	1	4
Massachussets	1996	1	0	0	1	2
Michigan	1995	1	1	1	1	4
Minnesota	1997	0	0	0	1	1
Mississipi	1997	0	0	0	0	0
Missouri	1995	0	0	0	0	0
Montana	2001	0	0	0	0	0

Table A.8. State Interstate Branching Laws: 1994-2010 (End)

This table shows for every state the year in which the deregulation reforms came into effect and gives the deregulation index resulting from these changes. The index ranges from 0 to 4, 4 indicating maximum openness to out-of-state branching.

State	Effective Year	No Minimum Age on target	Allows De Novo Branching	Allows Single Branch Acquisition	Deposit cap higher than 30%	Index
Nebraska	1997	0	0	0	0	0
Nevada	1995	0	1	1	1	3
New Hampshire	1997	0	0	0	0	0
New Hampshire	2000	0	1	1	1	3
New Hampshire	2002	1	1	1	1	4
New Jersey	1996	1	0	1	1	3
New Mexico	1996	1	0	0	0	1
News York	1997	0	0	1	1	2
North Carolina	1995	1	1	1	1	4
North Dakota	1997	1	0	0	0	1
North Dakota	2003	1	1	1	0	3
Ohio	1997	1	1	1	1	4
Oklahoma	1997	0	0	0	0	0
Oklahoma	2000	1	1	1	0	3
Oregon	1997	0	0	0	0	1
Pennsylvania	1995	1	1	1	1	4
Rhode Island	1995	1	1	1	1	4
South Carolina	1996	0	0	0	1	1
South Dakota	1996	0	0	0	1	1
Tennessee	1997	0	0	0	1	1
Tennessee	1998	0	0	1	1	2
Tennessee	2001	0	1	1	1	3
Tennessee	2003	0	1	1	1	3
Texas	1995	0	1	1	0	2
Texas	1995	0	0	0	0	0
Texas	1999	1	1	1	0	3
Utah	1995	0	0	1	1	2
Utah	2001	0	1	1	1	3
Vermont	1996	0	0	1	1	2
Vermont	2001	0	1	1	1	3
Virginia	1995	1	1	1	1	4
Washington	1996	0	0	0	0	1
Washington	2005	0	1	1	1	3
West Virginia	1997	0	1	1	0	2
Wisconsin	1996	0	0	0	1	1
Wyoming	1997	0	0	0	1	1

Appendix B Variables Definitions

State-level variables

State GDP Growth: annual growth rate in gross state product (GSP) using data obtained from the U.S. Bureau of Economic Analysis Region Tables.

Population: log of total state population from the U.S. Bureau of Economic Analysis Region Tables.

State Unemployment: the state unemployment rate, obtained from the U.S. Bureau of Labor Statistics.

CPS Data

To construct unemployment rate by racial and income categories, we use publicly-available microdata from IPUMS-CPS (Ruggles et al. 2010) for the years 1994 to 2010. We select the sample as follow. We drop the population not in the labor force ($labforce = 13$) and in military ($empstat$=13) and keep persons between the age of 16 and 64. We identify families below the poverty line if their total family income $ftotval$ is below the threshold given by the CPS (variable *cutoff*). Finally, we collapse the data at the state-year level using population weight *wtsupp*.