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 populations. 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 rule out demand-based explanations for the entry of new branches showing that branching deregulation has no effect either on a placebo sample of non-deregulated institutions, or on overall county economic prosperity. Using household level data, we find that this exogenous supply-shock on branch density increases the likelihood for low-income households to hold a bank account. The effect is even stronger for populations that are more likely to be rationed by banks, such as black households living in "high racial bias" states, or for households living in rural areas where branch density is initially low.

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, a large distance between branches and the proliferation of formal steps to open an account result in costs that may be too high for poor households to manage (Washington (2006), Barr and Blank (2008)). Furthermore, bank financial services may not be tailored to low-income households. These two polar explanations have different policy implications. Whereas the demand-side view predicts interventions at the household level through financial literacy programs, for example, 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. Rice and Strahan (2010) construct a time-varying

index to capture these state-level differences in regulatory constraints. 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 deregulations 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 deregulations on economic activity in poor counties. We find that branching deregulations have 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. This 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. Our regressions confirm this result. The share of low income households with a bank account increases by 4 percentage points after a state fully deregulates, which corresponds to a

[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. There is no reason why the real effects observed in earlier periods in response to different shocks would have the same effect in 1994.

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.

Third, we show that the effect of intensified bank competition is stronger for populations that are *ex ante more likely to be rationed by banks*, which reinforces the identification of supply effects. First, we find that black households benefit more from branching deregulations than do non-black households *only* in states with a history of discrimination. For the same level of income, black households are indeed 20% less likely than white households to hold a bank account in states with a history of discrimination, but this gap narrows to only 15% after deregulation, to the level observed in states with no history of discrimination. Second, the effect of branching deregulations increases when the level of income decreases. Whereas branching deregulations have no significant impact for middle-income households, whose income is above twice the poverty line, deregulating results on average in a 4 percentage points increase in the probability of holding a bank account among low-income households, and the effect increases up to 6 percentage points for poor households, whose income is below the poverty line. Third, the magnitude of the effect is significantly larger for households living in rural areas, where branch coverage is lower ex ante. Finally, we differentiate between households with lower and higher levels of education. We find that the effect of deregulation is stronger for more educated households. For these households, being unbanked is less likely to be driven by sophistication because they have relatively higher financial literacy (Lusardi and Mitchell (2007)).

Finally, we show that having access to bank accounts improves wealth accumulation but does not translate into higher levels of indebtedness. First, deregulation increases the share of low-income households with interest-earning assets in both banks and other financial institutions. Second, we show that deregulation impacts neither the probability to own debt nor the debt to income ratio of low income households, which mitigates the fear that banking competition fosters "predatory lending".

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. (2014) show that by offering a savings account with lottery-like features, banks can motivate the opening of savings accounts. This paper provides evidence consistent with the literature showing that geographic proximity matters, even more for low income households (Nguyen (2014)). The debate on the determinants of being unbanked also raises the question of the role played by the development of alternative financial services (see, for instance, Morgan and Strain (2008), Melzer (2011), Bertrand and Morse (2011), Morse (2011), Morgan et al. (2012), Carrell and Zinman (2014)).

More generally, our paper relates to the literature that shows the large and positive effect of access to banking accounts on savings rates (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)). Holding a bank account can also protect households from predatory lending.

Finally, our paper adds to the literature that evaluates the effect of intensified competition on racial or gender discrimination. 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 deregulation expanded access to credit, particularly among blacks.[2]

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]However, as shown by Ouazad and Rancière (2013), the relaxation of credit standards can also lead to more black segregation by giving white households the opportunity to relocate in white neighborhoods.

2 Data

2.1 Banking Deregulation

Restrictions on interstate banking and branching have their historical roots in the 1789 Constitution (Johnson and Rice (2008)).[3] 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 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.

[3]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.

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.[4]

Interstate branching deregulation has fostered the development of multi-state banking. As Figure 1 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 1

2.2 Household Data

Data on households comes from the SIPP and covers the 1993-2010 period. The SIPP is a running panel that collects detailed information about income and demographics for 20,000 to 30,000 households over 2 to 3 years. Most importantly, the SIPP includes topical modules focusing on household asset allocation and the use of banking services. 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 free checking or savings account, and 0 otherwise. The large size of the 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.[5] We work at the household rather than the individual level because households often pool

[4]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.

[5]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.

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. This leaves us with a total sample of 135,524 low-income households living in 45 states plus the District of Columbia over the 1993-2010 period.[6]

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 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.3). 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.[7]

Table 1 shows the summary statistics for banked and unbanked households in our sample. On average, banked households are less likely to be black and to receive transfer income and are richer than unbanked households.

INSERT TABLE 1

2.3 County-Level Data

Bank and Credit Union Location Data

[6]To ensure the confidentiality of the data, the SIPP aggregates five states in two groups. These states are: Maine and Vermont (first group) and North Dakota, South Dakota and Wyoming (second group), this explains why we do not have 50 +1 states. Unfortunately, there is a gap in the data between 2006 and 2008 because no topical module on asset allocation was administered during these years.

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

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.[8]

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.[9] 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 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

We want to test whether the effect of deregulations on black households is higher in states with a history of discrimination. Following Chatterji and Seamans (2012), we build four discrimination dummies to indicate states with a history of discrimination.

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

[8]data can be downloaded here: http://www.ncua.gov/DataApps/QCallRptData/Pages/CallRptData.aspx
[9]data can be accessible via the National Historical Geographic Information System (NHGIS)

(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 Effects of Deregulation on Branch Density

This section establishes the positive effect of deregulation on the density of bank branches in poor counties and in black counties in states with a history of discrimination. We then show that branching deregulations have no effect on the density of the placebo sample of financial institutions that are not affected by the change in regulation, i.e credit unions, and on county economic prosperity.

3.1 Bank Branch Density in Poor Counties

We start by showing that branching deregulation has a positive impact on the number of branches per inhabitant in poor counties. We define poor 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:

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

where $BranchDensity_{cst}$ is the ratio of the number of bank branches in a county to the

number of inhabitants in this county (in thousands), $Deregulation_{st}$ is the deregulation index in state s at time t, $CountyControl_{ct}$ are county time-varying characteristics and δ_t and η_c are year and county fixed effects, respectively. Controls at the county level include personal income in level and growth, unemployment rate and total population. Standard errors are clustered at the state level to account for serial correlation within states.

Panel A of Table 2 presents the results. Column (1) shows that bank branch density increases significantly following deregulation. The coefficient of our *Deregulation* variable implies that states where all branching restrictions were lifted experienced a 30% increase in the density of bank branches.

In a second step of our analysis, we also use the log of total deposits in banks within the county as a dependent variable. Results in columns (4) confirm that bank branches have collected more deposits following deregulation. Because we control for personal income in the county, this result implies that following branching deregulation, each dollar in the county translates into more deposits. However, this result is in aggregate, and therefore the paper's further investigation at the individual level is needed to conclude on household access to bank accounts (Section 4).

INSERT TABLE 2

3.2 Bank Branch Density in Black Counties

We now test if branching deregulation has a stronger impact on bank branch density in states where populations are more likely to be rationed by banks, which would be consistent with supply effects driving the increase in the density of bank branches. We make the assumption that black households are more likely to be rationed by banks, and even more in states with a history of discrimination. We know indeed from the literature that norms and institutions have a long-term impact.

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.[10] Columns (2) and (5) indicate that the effect of deregulation on density is almost the same in "Black counties" in states without a history of discrimination as in "Poor counties". However, the effect of deregulation is more than twice higher in states with a history of deregulation (columns (3) and (6)). The coefficient of our *Deregulation* variable indicates that branch density would have increased by 40% after a state fully deregulated.

3.3 Ruling-out Demand-based Explanations

Effect on Credit Union Branch Density

To mitigate endogeneity concerns, we investigate whether deregulation had any impact on financial institutions that were not subject to the deregulation. More specifically, we look at credit unions, and estimate the same models as before on this "placebo sample". Panel B of Table 2 shows that deregulation has not effect on the density of credit unions and the total volume of deposits they collected. As such, interstate branching deregulation seems to provide a valid exogenous shock to the supply of bank accounts to low-income households.

Effect on Overall Economic Activity

The absence of any significant consequence of deregulation in our placebo sample already suggests that the positive effect we find for bank branches is not driven by the deregulation improving overall economic activity and so, the demand for banking services. We now test directly this assertion and estimate the effect of our deregulation on various dimension of local economic prosperity: income per capita, unemployment rate and poverty rate. We also study household leverage for the subsample period where data from the Consumer Credit Panel/Equifax at the NewYork FED are available (1999-2010). For each dimension of economic prosperity, we run our regression on the sample of all counties, poor counties and counties with a high fraction of black households and control for the log of total population.

[10]Table A.9 in online appendix indicates that the results are the same for the four indexes.

Table A.4 in the appendix reports the results of the regressions. 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 household leverage (columns (10 to 12)). While these results may seem at odds in the light of the literature that argues that banking deregulation affects the real economy directly (e.g. Jayaratne and Strahan (1996)), it should be reminded that the deregulation episodes considered in this paper have little connection with those that were previously studied. These deregulations episodes indeed cover the period 1975-1994. In fact, Rice and Strahan (2010) studying our deregulation shows that while the increase in banking competition leads to a decrease in interest rates for small firms, they find no effect on the amount that small firms borrow, which is consistent with the fact that this deregulation has no macroeconomic effect.

4 The Effect of 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 better 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. The controls at the state level come from data from the Bureau of Economic Analysis and include state-level GDP growth, unemployment and a log of the total population. 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.[11][12] 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. All the other regressions rely on the same identification strategy.

Table A.5 in the appendix reports the estimated coefficients when we regress the *BankAccount* dummy on both the household and state-level control variables. The coefficients have the expected signs. Holding a diploma, whether it is from elementary school, high school or college, increases the likelihood of holding a bank account, whereas being poor decreases it. The coefficient on the *Black* dummy is -0.16, which implies that being black decreases the likelihood of holding a bank account by 16 percentage points. Given that we control for many socio-economic determinants, this result may suggest that black households suffer from discrimination (see Blanchflower et al. (2003) for evidence of racial discrimination on the credit market). Finally, the coefficients of state-level controls

[11] 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.

[12] Our results still hold in logit regressions

are not significant, which may be explained by the fact that macroeconomic factors do not matter once we control for socio-economic variables at the household level. To save space and facilitate the reading of the results, the coefficients of the control variables are reported only in Table A.5.

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.[13] 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. 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 0.012 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 4.8 percentage points. The second column introduces household controls and the third column introduces time-varying state controls. The coefficient on *Deregulation index* subsequently remains stable.

[13]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).

INSERT TABLE 3

To further address endogeneity concerns we first introduce a large set of household and state level controls in the second and third columns of Table 3. These controls aim to capture factors that would foster the demand for banking services at the household level and the economic conditions that may drive deregulation. We observe that the coefficient of our deregulation index is even slightly reinforced.

Second, we analyze the dynamics of the share of banked households around deregulation. Figure 2 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 households 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 FIGURE 2

Finally, in section 5.1, we investigate the timing of deregulation following the method of Krozner and Strahan (1999) and find that deregulation does not seem to be driven by variables that also affect access to banking services. As such, interstate branching deregulation seems to provide a valid exogenous shock to the supply of bank accounts to low-income households.

4.3 Heterogeneous Treatment Effects

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

Table 4 examines the impact of banking deregulation among black households. We make the assumption that black households are more likely to be rationed by banks in states with a history of discrimination, because we know from the literature that norms and institutions have a long-term impact. Thus, following Chatterji and Seamans (2012), we build four discrimination dummies that indicate states with a history of discrimination. 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%.

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* $\times$*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.

INSERT TABLE 4

We find that the effect of deregulation on the share of banked households is larger among black households than among non-black households, but only in states with a history of discrimination. The first column of Table 4 shows no significant difference in the impact of deregulation between black and non-black households, because the coefficient of the *Deregulation* $\times$*Black* interaction is positive but not significant. However, the second, third, fourth and fifth columns suggest that the effect of deregulation is larger for black households in states with a history of discrimination. 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. Table A.7 in the Appendix reports the results when we split our sample along our four measures of discrimination. We find again that the impact of deregulation is larger for black households in states with a history of discrimination.

Next, the first three columns of Table 5 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), 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). Table 5 shows 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 2% increase in the probability of holding a bank account among poor households (column (1)) against a 0.9% 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 5

Columns (4) and (5) in Table 5 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 twice 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.

Finally, the last two columns in Table 5 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 Mitchell, 2011). To do so, we split our sample between households with low education in column (6) (none or only elementary) and households with at least a high school degree in column (7). We find that the effect of deregulation appears mostly for more educated households (column (6)).

4.4 Banking Deregulation, Asset Accumulation and Debt

This section investigates the impact of banking deregulation on households' debt and savings. If banking deregulation results in an increase in the likelihood of holding a bank account among low-income households, we could expect the latter not only to accumulate more interest-earning savings given the key role of transaction accounts in asset accumulation (Carney and Gale, 2001), but also to have easier access to debt financing.

Table 6 examines the detailed impact of banking deregulation on households' savings. The table shows estimates of the baseline model, where the dependent variables include the two components of our *BankAccount* dummy. *Checking*, in columns (1), (3) and (4), and *Savings*, in columns (2), (5) and (6), indicate whether the household holds a non-interest bearing checking account and a savings account, respectively. The positive and significant coefficients of the deregulation index in columns (1) and (2) show that deregulation significantly increases the likelihood of holding both a checking *and* a savings account to a similar degree. Banking deregulation may therefore foster savings accumulation on interest bearing accounts.

When splitting the sample between poor households and low-income households in columns (3) to (6), the coefficient of our deregulation index indicates that poor households are much more likely to open a checking account (column (3)) than a savings account (column (4)) following deregulation, whereas the opposite result is found for low-income households (column (5) and (6)). This finding is consistent with the intuition that households that are below the poverty line do not have sufficient income to accumulate savings and that savings accounts may better meet the needs of low-income households.

The final columns of Table 6 reports estimates of our basic model on the log of household total net worth, which is the sum of financial assets, home equity, vehicle equity, and business equity, net of unsecured debt holding. The coefficient of our deregulation index is again positive and significant. Because we control for several income variables in our regression, as well as state macroeconomic conditions, this result implies that for an equal amount of income, low-income households are more likely to accumulate wealth when they have access to bank accounts, which confirms the considerable role of bank accounts in fostering asset accumulation. The result is robust to excluding bank assets to total net worth (column (8)).

INSERT TABLE 6

Next, Table 7 turns to the relationship between deregulation, bank accounts and households' access to debt and investigates whether the increased probability of holding

a bank account following deregulation translates into increased access to debt. We begin by mitigating the risk of reverse causality in columns (1) to (3). 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. Column (1) focuses on the subsample of banked households and estimates the baseline model in which where the dependent variable is a dummy indicating whether the household holds debt. The coefficient of the deregulation index is not significant and close to zero, which indicates that the credit supply does not appear to increase after deregulation for low-income households with a bank account. Columns (2) and (3) estimate the baseline model in which the dependent variable is our dummy *BankAccount*, but the sample is split into households without debt (in column (2)) and households with debt (in column (3)). The positive and significant coefficient of our deregulation index in both columns (2) and (3) suggests that deregulation strongly increases access to bank accounts regardless of whether the household takes a loan.

Finally, the last column of Table 7 estimates our basic model, where the dependent variable is the debt to income ratio. The negative but not significant coefficient of the deregulation index indicates that deregulation has no impact on the debt-to-income ratio. This result mitigates the fear that deregulation increases the risk of over-indebtedness.

INSERT TABLE 7

5 Robustness

5.1 Evidence of Racial Discrimination across Income Groups

Given that the impact of banking deregulation on the poorest households is relatively large and given that black households are poorer on average, our results for racial discrimination may only reflect an income distributional effect. However, there are two reasons why this should not be the case.

First, we find that banking competition has an impact on the racial gap in access to

banking services *only* in states with a history of discrimination. This finding contradicts the view that we simply capture a reduction in the gap between poor and middle-income households.

Second, we show that deregulation has a larger impact on black households than on non-black households at each point of the income distribution. We split our initial sample into *very poor* (below half the poverty line), *poor* (between half the poverty line and the poverty line), *low-income* (between one and two times the poverty line) and *middle-income* households (between two and three times the poverty line). Columns (2) to (5) in Table A.6 report the results of this decomposition and show that deregulation has an impact on the racial gap in each income group in states with a history of discrimination. In addition, we find no significant effect on the racial gap in the rest of the sample. These results suggest that banking competition reduces the gap between black and non-black households in states with a history of discrimination.

5.2 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 equals 0. In column (2) we re-run our baseline regression and directly add *State* x *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.

We then run two other types of robustness checks. First, we check that our results are robust to the sample period. Column (3) starts the sample in 1997 (the date at which the IBBEA becomes effective), and column (4) ends it in 2006 (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 (5) we replace our index with a simple dummy variable that takes the value 1 if a state has adopted at least one of the four deregulations. By contrast, in column (6), 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 7, 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

5.3 The Timing of Bank Deregulation

This section strengthen the robustness of our results to several potentially confounding influences resulting from the timing of deregulation. First, one might be concerned that the causal link between deregulation and the share of banked households is reversed. States may have more incentives to deregulate *when the share of banked households is low*. Following deregulation, the share of banked households would then mechanically increase. Another plausible explanation for our results is that states deregulate when their economies are doing well and therefore when the demand for bank accounts is high, because banks are less vulnerable to deregulation during these periods . This phenomenon would translate into a subsequent increase in the share of banked households after deregulation.

We test whether the share of banked households or the macro-economic conditions at the state-level drive the timing of deregulation with a Weibull proportional hazards model (Kroszner and Strahan (1999)). The hazard rate function takes the following form:

$$h(t, X_t, \beta) = h_0(t) \exp[X_t' \beta], \tag{3}$$

where X_t is a vector of covariates; β is a vector of unknown parameters; and the baseline hazard rate, $h_0(t)$, is pt^{p-1} with shape parameter p. The parameters β and p are estimated with maximum likelihood. Because we consider four steps of deregulation (the amount of bank deposits, de novo branching, the acquisition of a single branch and the minimum age of a targeted bank), the covariates vector includes an indicator variable for each type of deregulation. We include all state-deregulation step pairs in the analysis . We keep state-deregulation step pairs even when the state has still not deregulated in 2010, in which case the duration is right-censored. We are left with 204 state-deregulation step pairs of which 172 are not censored (i.e., deregulation is observed during the sample period).[14] For each state-deregulation step pair we have one observation for each year up to and including the year of deregulation, which gives us a total of 1,773 observations.

First, to investigate whether the initial level of the share of banked households influences the timing of deregulation, we introduce three new variables: the share of unbanked households, the share of low-income unbanked households and the share of black unbanked households at the beginning of the period (1994). Second, to estimate the effect of macro-economic conditions on the deregulation date we include three broad state variables: the share of black people in the state population, the unemployment rate and real GDP per capita. Third, we include the main variables that are used by Kroszner and Strahan (1999) and Rice and Strahan (2010): the share of small banks in the state, their relative capital ratio, the size of the insurance sector and the share of small firms in total employment of the state.[15] Finally, we include a proxy for political ideology with a

[14]Excluding the right-censored state-deregulation step pairs from the analysis yields similar results.

[15]Data for the share of small banks and their relative capital ratio comes from the Call Reports. The share of small banks is the fraction of total assets held by banks with assets below the state median, and the relative capital ratio is the difference in the capital-to-asset ratio of small banks that of large ones.

dummy "Republican" that equals one if the majority of the voters chose the Republican candidate in the last presidential election.

Table A.8 in the Appendix reports the results of the analysis. Reassuringly, the first three columns indicate that the different measures of the share of unbanked households have no significant impact on the timing of deregulation. The fourth column shows that among the macro-economic variables, only GDP per capita has a positive and significant coefficient, suggesting that richer states tend to deregulate earlier. The fifth column reports the coefficients of the Krozner-Strahan variables and shows that the factors that had an impact on the timing of intrastate deregulation in the 1970s and 1980s (Kroszner and Strahan (1999)) also affect interstate deregulation. For instance, a larger share of small banks delays deregulation,whereas a large insurance sector leads to earlier deregulation. However, contrary to the first waves of deregulation, the share of small firms appears to have no effect. Finally, column (6) shows the results when we include all of the variables and confirms that overall, the timing of deregulation does not seem to be related to the share of unbanked households, the share of black households, state unemployment or GDP per capita.

6 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 at the impact of bank competition on access to bank accounts across

The size of the insurance sector is defined as the ratio of value added from insurance to value added from insurance plus banking. The share of small firms is defined as the fraction of employees in firms with fewer than 20 employees. Data for value added come from the Bureau of Economic Analysis and data for employment by state-firm size come from the Bureau of Dynamic Statistics.

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 bank competition improves savings for low-income households but not debt to income ratios, 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. 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 minorities 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

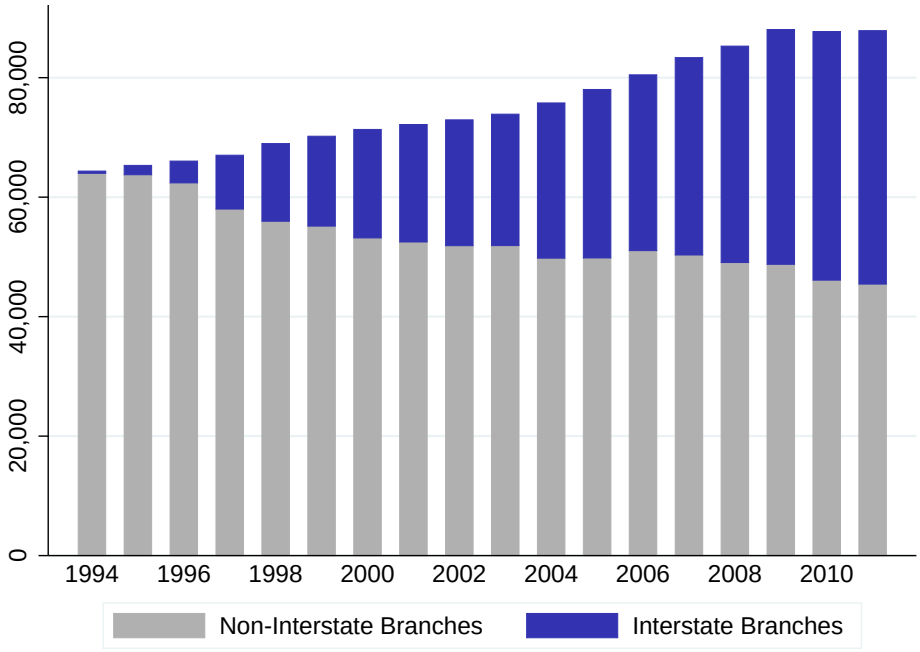


Figure 1. Number of Branches Operated by FDIC-insured Commercial Banks

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

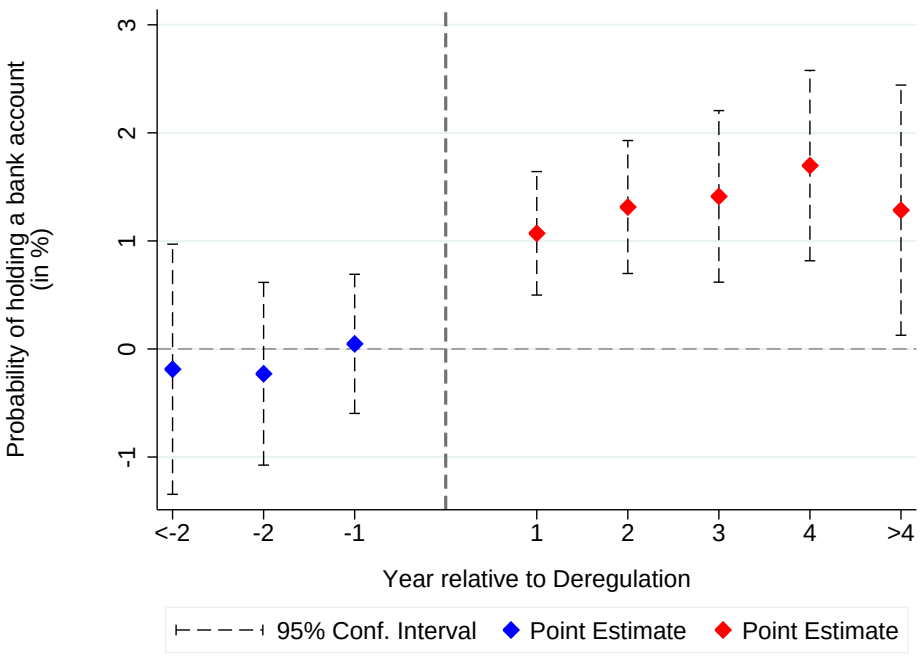


Figure 2. The Impact of Banking Deregulation on the Share of Banked Households

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	**Banked Households**	**Unbanked Households**	*Test*
Black (%)	13	30	***
Married Couple (%)	42	32	***
Single Female-Headed (%)	43	50	***
Household Size	2.5	2.7	***
Age (year)	53	48	***
Elementary Education (%)	22	38	***
High School Education (%)	35	36	***
College Education (%)	42	26	***
Monthly Household Income	1,403	1,297	***
Recepients of Social Security (%)	47	45	***
Recepients of Transfer Income (%)	25	34	***
Unemployed Head of Household (%)	7.7	9	***
Observations	*83,856*	*51,668*	-

This table contains summary statistics on banked and unbanked low-income household socio-demographic characteristics, SIPP (1993 - 2010). The first column displays the mean value of these characteristics for the sample of banked households, whereas the second column displays the mean value of these characteristics for the sample of unbanked households. The test column displays the level of statistical significance of a t-test between the mean values of the right column minus the left column. *, **, and *** represent statistical significance at the 10%, 5%, and 1% confidence levels, respectively.

Table 2. Interstate Branching Deregulation and Bank Branch Coverage

A. Commercial Banks

Dependent Variable	**Branch Density**			**Total Deposits**		
Counties	Poor	Black		Poor	Black	
Sample		Non-discr.	Discr.		Non-discr.	Discr.
	(1)	(2)	(3)	(4)	(5)	(6)
Deregulation	0.071**	0.074**	0.179***	0.078*	0.093**	0.187***
	(0.030)	(0.030)	(0.042)	(0.043)	(0.041)	(0.063)
County-Year	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	11,325	4,904	6,836	11,261	4,902	6,808

B. Credit Unions

Dependent Variable	**Branch Density**			**Total Deposits**		
Counties	Poor	Black		Poor	Black	
Sample		Non-discr.	Discr.		Non-discr.	Discr.
	(1)	(2)	(3)	(4)	(5)	(6)
Deregulation	0.000	-0.001	-0.001	0.027	-0.008	0.003
	(0.001)	(0.001)	(0.001)	(0.053)	(0.005)	(0.021)
County-Year	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	11,325	4,904	6,836	11,325	4,904	6,836

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 controls whereas columns (2), (3) and (4) include household controls, plus state-year 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 and state-year controls include controls for family type, race, age, size of the household, education, receipt of Social Security income or transfer income, monthly income and state unemployment, population (log), GDP growth and a republican dummy. Standard errors are clustered by state.

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

Dependent variable	**=1 if the household holds a bank account**			
	(1)	**(2)**	**(3)**	**(4)**
Deregulation Index	0.012***	0.012***	0.012***	
	(0.004)	(0.004)	(0.004)	
Deregulation ($\leq$ t-4)				-0.012
				(0.016)
Deregulation (t-3,t-1)				-0.012
				(0.012)
Deregulation (t+1,t+3)				0.033***
				(0.012)
Deregulation ($\geq$ t+4)				0.037**
				(0.017)
Household Controls	-	Yes	Yes	Yes
State-Year Controls	-	-	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Observations	136,176	136,176	136,176	136,176

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 controls whereas columns (2), (3) and (4) include household controls, plus state-year 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 and state-year controls include controls for family type, race, age, size of the household, education, receipt of Social Security income or transfer income, monthly income and state unemployment, population (log), GDP growth and a republican dummy. Standard errors are clustered by state.

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

Dependent Variable	=1 if the household holds a bank account				
Discrimination Dummy	**-**	**Former Slave State**	**Antimiscegenation Law**	**No Fair Housing Law**	**Share of interacial marriage**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Deregulation Index	0.010**	0.014**	0.012**	0.014**	0.013***
	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)
Index x Black	0.009	-0.002	0.003	-0.005	-0.001
	(0.006)	(0.008)	(0.008)	(0.007)	(0.006)
Index x Black x Discr.		0.024**	0.019*	0.028***	0.023***
		(0.010)	(0.010)	(0.009)	(0.008)
Household Controls	Yes	Yes	Yes	Yes	Yes
State-Year Controls	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	136,176	135,934	134,718	136,176	136,176

This table reports linear probability regressions of the Interstate Branching Deregulation Index on access to bank accounts, its interaction with a black dummy not interacted and interacted with racial discrimination 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. From column (2) to (5) four racial discrimination dummies are interacted first, with black, second, with black and the deregulation index: slaves state in the year immediately prior to Civil war (1 if yes, 0 if not), anti-miscegenation law not repealed until after the US Supreme Court's 1967 decision in *Loving v.Virginia* (1 if yes, 0 if no), no fair housing law until federally mandated by the Fair Housing Act of 1968 (1 if yes, 0 if no), racial bias index, as measured by the interracial marriage rate (1 if below median). All regressions include black*discrimination, index*deregulation, black*deregulation controls as well as 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 rate, population (log), GDP growth and a republican dummy. Standard errors are clustered by state.

Table 5. Heterogenous Effect of Bank Deregulation across Household Types

Dependent Variable	**=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***	0.010**	0.003	0.018***	0.010*	0.012**	0.007
	(0.004)	(0.004)	(0.003)	(0.007)	(0.005)	(0.005)	(0.006)
Household Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Year 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	52,470	83,706	48,343	37,550	98,626	97,873	38,303

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 between households living in rural and urban areas. Columns (6) and (7) split the sample between low educated (less than high school) and highly educated (high school or higher) households. Household and state controls are the same as previously described. Standard errors are clustered by state.

Table 6. The Effect of Bank Deregulation on Asset Accumulation

Dependent variable	**=1 if the household holds**						**Household Net Worth**	
	Checking Account	**Savings Account**	**Checking Account (only)**		**Savings Account**		**Total**	**Excluding Banking Assets**
Sample	**All**	**All**	**Poor**	**Low Inc.**	**Poor**	**Low Inc.**	**All**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Deregulation Index	0.008**	0.007***	0.014***	0.009	0.004	0.008**	0.036*	0.039*
	(0.004)	(0.002)	(0.004)	(0.006)	(0.003)	(0.003)	(0.019)	(0.021)
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	136,176	136,176	38,737	46,282	52,470	83,706	115,817	114,837

This table reports the effect of the Interstate Branching Deregulation Index on asset accumulation. In columns (1) to (6) the dependent variable is dichotomous and takes the value 1 if the household owns a non interest bearing checking account (columns (1) and (3)-(4)), a savings account (columns (2) and (5)-(6)) and interest earning assets in financial institutions other than a bank in column (columns (7)). We also split the effect of owning a checking and savings account across income groups. "Poor" is below the poverty line and "Low" is between once and twice the poverty line. Household and state controls are the same as previously described. Standard errors are clustered by state.

Table 7. The Effect of Bank Deregulation on Low-Income Household Debt

Dependent variable	**Any Debt**	**Debt to Income Ratio**	**Bank Account**	
Sample	**All**	**All**	**Debt-free HH**	**In debt HH**
	(1)	**(2)**	**(3)**	**(4)**
Deregulation Index	0.003	0.013	0.015***	0.007**
	(0.003)	(0.014)	(0.005)	(0.003)
Household Controls	Yes	Yes	Yes	Yes
State-Year Controls	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Observations	135,699	131,778	63,039	72,660

This table reports the effect of owning a bank account on access to debt. We estimate linear probability regressions in which dependent variables are a dichotomous variable that take the value 1 if the household owns debt in column (1), the debt to income ratio in columns (2), a dichotomous variable that take the value 1 if the household owns a bank account in columns (2) and (3). The deregulation index ranges from 0 to 4, 0 is least, 4 is most deregulated. All regressions include state and year fixed effects. Household and state controls are the same as previously described. Standard errors are clustered at the state level.

Table 8. Ruling Out Demand Factors

Dependent variable	**=1 if the household holds a bank account**					
Sample	**All**				Likely unemployed	Not likely unemployed
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Deregulation Index	0.012***	0.012***	0.012***	0.010***	0.013***	0.011***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)
Unemployed HH Head	0.012***		0.012***	0.012***		
	(0.004)		(0.004)	(0.004)		
Any HH Member Unemployed		0.017***				
		(0.004)				
UE rate - Poor			-0.085			
			(0.051)			
UE rate - Low Inc.			-0.037			
			(0.095)			
UE rate - Other			-0.056			
			(0.311)			
UE rate - Poor & Black				0.015		
				(0.013)		
UE rate -Low Income & Black				0.031		
				(0.023)		
UE rate - Poor & White				-0.143**		
				(0.058)		
UE rate - Low Income & Black				-0.105		
				(0.106)		
UE rate - Other & Black				-0.005		
				(0.041)		
UE rate - Other & White				0.000		
				(0.345)		
Household Controls	Yes	Yes	Yes	Yes	Yes	Yes
State-Year Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	136,176	136,176	136,176	136,176	67,455	68,721

This table reports the effect of Interstate Branching Deregulation Index on the likelihood to have a bank account depending on various measure of the unemployment rate in the the state of location. In column (5) and (6), we estimate the probability to be unemployed based on household and state characteristics and split the sample between households with a probability below the median sample (column (5)) and above median sample (column (6)). Standard errors are clustered by state.

Table 9. Robustness Checks

Dependent Variable	**=1 if the household holds a bank account**						
	All		**Periods**		**States**		
Sample			**After 1997**	**Before 2005**	**All**	**Only Deregulated**	**Largest**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Deregulation Index		0.011**	0.015***	0.013***		0.011**	0.014**
		(0.005)	(0.005)	(0.004)		(0.005)	(0.005)
Placebo Index	-0.002						
	(0.004)						
Deregulation Dummy					0.020*		
					(0.011)		
Household Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Year 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
State × Year Trend	-	Yes	-	-	-	-	-
Observations	136,176	136,176	108,116	112,339	136,176	122,005	72,716

This table reports results from linear probability model regressions on access to bank accounts. Columns (1) and (2) and (5) include the whole sample. In columns (3) and (4) data are split into two sub-periods: (1997-2010) and (1993-2006). In column (6) the sample is restricted to states with at least one deregulation over the 1993-2010 period, and in column (7) to the largest 10 states. The dependent variable is 1 if the household holds a checking or a savings account (SIPP 1993 - 2010). The explanatory variable is the deregulation index in each column, except in column (1) and in column (5). In column (1) the dependant variable is a placebo index. In column (5) the dependant variable is a dummy with value 1 if the deregulation index is strictly higher than 0. All regressions include state and year fixed effects. Column (2) also includes state*trend 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) and unemployment rate. Standard errors are clustered by state.

Appendix A Tables

Table A.1. 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. 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	1997	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.2. 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	1996	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

Table A.3. 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.4. The Impact of Bank Deregulation on County Prosperity

Dependent variable	**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)
County-Year	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
Obs.	59,742	14,920	14,920	59,700	14,906	14,918	59,685	14,914	14,914	27,375	8,073	6,578

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 controls whereas columns (2), (3) and (4) include household controls, plus state-year 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 and state-year controls include controls for family type, race, age, size of the household, education, receipt of Social Security income or transfer income, monthly income and state unemployment, population (log), GDP growth and a republican dummy. Standard errors are clustered by state.

Table A.5. Standard Determinants of Banked Households

Dependent Variable	**=1 if the household holds a bank account**	
Household Controls		
Black	-0.162***	(0.010)
Married Couple	0.091***	(0.006)
Single Female-Headed	0.035***	(0.006)
Household Size	-0.020***	(0.002)
Age	0.004***	(0.000)
Elementary Education	0.091	(0.080)
High School Education	0.193**	(0.080)
College Education	0.308***	(0.082)
Monthly Household Income	0.00***	(0.00)
Income < Poverty Threshold	-0.059***	(0.005)
Receive Social Security	0.013*	(0.007)
Receive transfer income	-0.139***	(0.007)
Head unemployed	0.012**	(0.004)
State-Year Controls		
GDP Growth	-0.103	(0.129)
Population	-0.037	(0.132)
State Unemployment	-0.003	(0.006)
Year Fixed Effects	Yes	
State Fixed Effects	Yes	
Observations	136,176	

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.6. The Impact of Bank 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.7. Racial Discrimination Across Income Groups

	Very Poor HH	**Poor HH**	**Low Income HH**	**Middle Income HH**
	(1)	**(2)**	**(3)**	**(4)**
Deregulation Index	0.019***	0.015*	0.012**	0.006
	(0.007)	(0.008)	(0.006)	(0.004)
Index x Black x Discr.	0.023*	0.029***	0.030***	0.023**
	(0.012)	(0.010)	(0.011)	(0.012)
Household Controls	Yes	Yes	Yes	Yes
State-Year Controls	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Observations	18,069	34,401	83,706	48,343

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. Data are split into four exclusive samples of households based on their annual income: *very poor* (below half the poverty line), *poor* (between half and once the poverty line), *low-income* (between once and twice the poverty line) and *middle income* households (between twice and three times the poverty line). In column (1) we estimate linear probability regressions of the Interstate Branching Deregulation Index on access to banking and its interaction with a *poor household* (including *very poor* households) dummy and with a *low-income household* dummy. Regressions also include black x discrimination, index x deregulation, black*deregulation controls. 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) and unemployment rate. Standard Errors are clustered by state.

Table A.8. 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. 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.9. Deregulation and Branch Density: Alternative Proxies of Discrimination

Dependent variable	**Branch Density**					
Proxy Discrimination	**Antimiscegenation Law** *Discrimination*		**No Fair Housing** *Discrimination*		**Interracial Marriage** *Discrimination*	
	No	Yes	No	Yes	No	Yes
Deregulation	0.073**	0.180***	0.012***	0.089***	0.078**	0.128**
	(0.030)	(0.043)	(0.004)	(0.032)	(0.031)	(0.056)
County-Year	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	5,032	6,692	4,154	7,602	5,095	6,661

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*.