Unbanked Households: Evidence of Supply-Side Factors [*]

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Abstract

This paper provides evidence that supply-side factors significantly drive the high share of unbanked households. Using interstate branching deregulation in the U.S. after 1994 as an exogenous shock, we show that an increase in bank competition is associated with a large drop in the share of unbanked households. 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. The improved access to bank accounts leads to higher savings rates but does not translate to higher levels of indebtedness.

Keywords: Banks, Regulation, Imperfect Competition, Household Finance, Discrimination

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

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., they 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 presents evidence that supply-side factors significantly drive the share of unbanked households among low-income households, defined as having income below twice the poverty line. Similar to Rice and Strahan (2010), we exploit interstate branching deregulation in the U.S. after 1994 as an exogenous shock on bank competition. We combine this shock with 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 focuses on low-income American

households. Coupled with its yearly frequency, these data are particularly well suited for our analysis.[1]

Our first set of empirical evidence shows that as bank competition intensifies, the share of unbanked households decreases. We find 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 households with a bank account increases by 4 percentage points after a state fully deregulates.

Moreover, we show that the effect of intensified bank competition is stronger for populations that are *more likely to be rationed by banks*. First, we differentiate states along several measures of discrimination. We find that black households benefit more from interstate-branching deregulation 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 deregulation increases when the level of income decreases. Whereas deregulation has no significant impact for middle-income households, whose income is above twice the poverty line, deregulating results on average in a 2 percentage points increase in the probability of holding a bank account among low-income households, and the effect increases up to 3.5 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 competition is expected to be 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

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[1]Although we are not running a "horse race" between the demand-side and the supply-side views, we focus on a clean supply shock and use a wide array of economic variables at the state and household levels to control for demand factors.

sophistication because they have relatively high financial literacy.

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 owning a bank account improves access to credit without translating into a higher ratio of debt to income, which mitigates the fear that banking competition fosters "predatory lending".

Our results are robust to controlling for the demand-side factors identified in the literature. Banking deregulation, by decreasing unemployment among low-income households through growth (Jayaratne and Strahan (1996)) and easier firm access to credit (Black and Strahan (2001), Rice and Strahan (2010)), may increase *demand* for bank accounts. However, 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 that capture the effect of deregulation on GDP growth or unemployment. In addition, we find that the effect of deregulation is not higher for households that are more likely to benefit from deregulation through an increase in income.

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. 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.

Our paper also complements the literature on the impact of bank competition on household finance. This literature has focused on implications in terms of household debt, such as mortgages (Favara and Imbs (2014)) or credit cards (Dick and Lehnert (2010)), but not household savings. A more developed stream of literature has used interstate and intrastate deregulation in the U.S. to investigate the impact of bank competition on the financing of firms (Rice and Strahan (2010), Zarutskie (2006), Cetorelli and Strahan (2006)), economic growth (Jayaratne and Strahan (1996)) and economic volatility (Morgan et al. (2004)).

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 provides theoretical explanations for the impact of intensified bank competition on the share of unbanked households. Section 3 describes the data and the empirical strategy. Section 4 presents the results. Section 5 runs various robustness checks. Section 6 concludes.

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[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 Background

2.1 Theoretical Discussion

How can bank competition increase the share of households with a bank account among low-income populations or minorities? Several forces are potentially at play.

First, concentration in banking may lead to excessively low supply of bank accounts and high prices. Low-income households whose level of wealth is low are more likely to be harmed by this lack of competition. Intensified competition should drive prices closer to marginal cost, which should favor low-income households.

Second, intense competition may give banks incentives to cope with the higher costs that low-income households generate. Low-income households are more likely to have low balances, to overdraw accounts, and may require more time from customer services, making them less profitable as consumers. In addition, the small amount of these households' savings and loans reduces future expected revenues, which may be relatively small relatively to the fixed cost of opening a bank account. Banks may be less reluctant to pay this fixed entry cost in a highly competitive environment.

Third, when competition intensifies, banks may have more incentives to invest in expertise toward low-income or black populations. Acquiring expertise in offering services to these groups may indeed partially insulate banks from pure price competition.[3] Moreover, intensified competition may increase the market size of the most efficient banks, and so decrease the marginal cost of acquiring expertise.

Fourth, decrease in entry barriers may give banks that already have expertise in offering services to specific populations access to new markets, in which such expertise may be lacking. For example, bank expertise in offering bank accounts to black households may be low in states with a history of discrimination. The entry of specialized banks into these markets would induce a decrease in the share of unbanked households among black populations.

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[3]A similar mechanism is described in Boot and Thakor (2000). In their model, banks invest in acquiring expertise in the market for loans through relationship banking.

Beyond profit-maximizing reasons for why intensified bank competition may be associated with a decrease in the share of unbanked households, competition may also reduce "taste-based" discrimination toward minorities. In a seminal work published in 1957, Gary Becker argues that over the long run, competition drives discrimination out of the market-place. The application of Becker's model to the market for financial services can be described in relatively general terms: banks with a "taste for discrimination" will forego profits to indulge their desire to offer bank accounts to a specific type of depositor. For example, banks in states with a history of discrimination may offer less than the profit-maximizing number of bank services to black households or to households living in "black areas". This practice in the market for loans has partly resulted in the "redlining" phenomenon and was one of the main reasons for the adoption of the Community Reinvestment Act in 1977. CohenCole (2011) finds some evidence that this practice has even persisted in recent years. Thus, in a perfectly competitive market, non-discriminating banks should gain a cost advantage and ultimately drive discriminating banks out of business, which should result in a decrease in the share of unbanked households among minorities.

2.2   Banking Deregulation

Restrictions on interstate banking and branching have their historical roots in the 1789 Constitution (Johnson and Rice (2008)).[4] 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

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[4]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.

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. 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.[5]

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

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[5]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.

of small banks, resulting in a strong appreciation of bank density.

INSERT FIGURE 1

3 Data and Empirical Strategy

3.1 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.[6] 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. 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.[7]

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

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[6]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.

[7]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.

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.[8]

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

3.2 Identification Strategy

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

$$P(BankAccount_{ist}) = \alpha + \beta Deregulation_{st} + \theta X_{ist} + \lambda StateControl_{st} + \delta_t + \eta_s + \epsilon_{ist} \quad (1)$$

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

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

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.[9][10] 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 2 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 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

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[9]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.

[10]Our results still hold in logit regressions

Table 2.

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.[11] 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   Results

4.1   Basic Model

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.

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[11]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).

The second column introduces household controls and the third column introduces time-varying state controls. The coefficient on *Deregulation index* subsequently remains stable.

INSERT TABLE 3

Two concerns regarding endogeneity arise from our baseline model. First, the relationship between banking deregulation and the share of households holding a bank account 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 regulator responds to increasing demand for banking services. Second, unobserved factors such as changes in economic conditions could drive both deregulation and the demand for bank accounts.

We first address these endogeneity concerns with the large set of household and state level controls that we introduce in our specification in the second and third columns of Table 3. These controls aim to capture factors that 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.

4.2   Heterogeneous Treatment Effect

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

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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.

<div align="center">INSERT TABLE 4</div>

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.5 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

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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

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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)).

4.3   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 column of Table 6 reports estimates of our basic model on a dummy indicating whether the household has accumulated interest-earning assets in other financial institutions such as savings and loans, credit unions and mutual funds. 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.

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

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5 Robustness

5.1 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:

$$h(t, X_t, \beta) = h_0(t) \exp[X_t'\beta], \tag{2}$$

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).[12] 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.[13] Finally, we include a proxy for political ideology with a dummy "Republican" that equals one if the majority of the voters chose the Republican candidate in the last presidential election.

Table A.6 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

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

[13]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. 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.

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.

5.2   Ruling out Demand Factors

Although the timing of deregulation seems to be exogenous to the share of unbanked households, there may be concern that demand-side factors are driving our results.

One alternative explanation is that banking competition, by decreasing unemployment through growth (Jayaratne and Strahan (1996)) and providing easier firm access to credit (Black and Strahan (2001), Rice and Strahan (2010)), would in fact *foster the demand for bank accounts.*

However there are three facts that suggest that our result is not driven solely by demand effects. First, in all of our previous specifications, we control for a large number of covariates that capture demand effects. At the household level, we control for several dimensions of income, skills and labor status. At the state level, we control for the main state macroeconomic variables such as GDP growth or unemployment. However, to strengthen our specification, we include more detailed controls for unemployment. Table 8 reports the results. Columns (1) and (2) show estimates of our main specification after controlling for whether the head of the family is unemployed or whether one of the adults in the household is unemployed. In both cases, our result holds. Next, columns (3) and (4) include detailed controls for unemployment at the state level. In column (3), we replace *State Unemployment* with three variables: *Poor Unemployment*, the unemployment rate of households living below the poverty line; *low-income Unemployment*, the unemployment rate of households with an income between one and two times the poverty line; and *Unemployment Other Income*, the unemployment rate of households whose income is above twice the poverty line. In column (4), we decompose these three unemployment rates by race (black and non-black), resulting in six different unemployment rates.[14]   In

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[14]To construct each unemployment rate we use the CPS (Current Population Survey). A detailed

both cases, our result holds, which mitigates the concern that our effect is driven only by demand effects through a reduction in the unemployment rate.

Second, we consider the effect of deregulation depending on the likelihood of a household being unemployed. If, following banking deregulation, households are more likely to find jobs, and therefore to hold a bank account, households that are initially more at risk of being unemployed (but have jobs) should be more affected by deregulation. To test this prediction, we generate a predicted probability to be unemployed based on household characteristics and location. Columns (5) and (6) present the results of splitting the sample into households with a probability of being unemployed that is *below* the median and households with a probability of being unemployed that is *above* the median. The effect of deregulation is roughly the same; if anything, the point estimate of our deregulation index for households with a lower probability of being unemployed (column 5) is slightly higher.

INSERT TABLE 8

Finally, Rice and Strahan (2010), examining the same deregulation, shows that although the increase in competition resulted in a decrease in the cost of credit, it did not translate into an increase in the volumes of loans. This finding suggests that deregulation had a limited effect on demand from firms.

5.3   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

description of how we construct the variables is provided in the Appendix.

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.4 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.

5.4 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 un-banked 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

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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.

<center>INSERT TABLE 9</center>

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. 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 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

<center>24</center>

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, in all of our specifications, we control for a large set of covariates that capture demand effects at both the household and state levels. Second, we find that the effect is not higher for households that are more likely to benefit from an increase in revenue due to deregulation.

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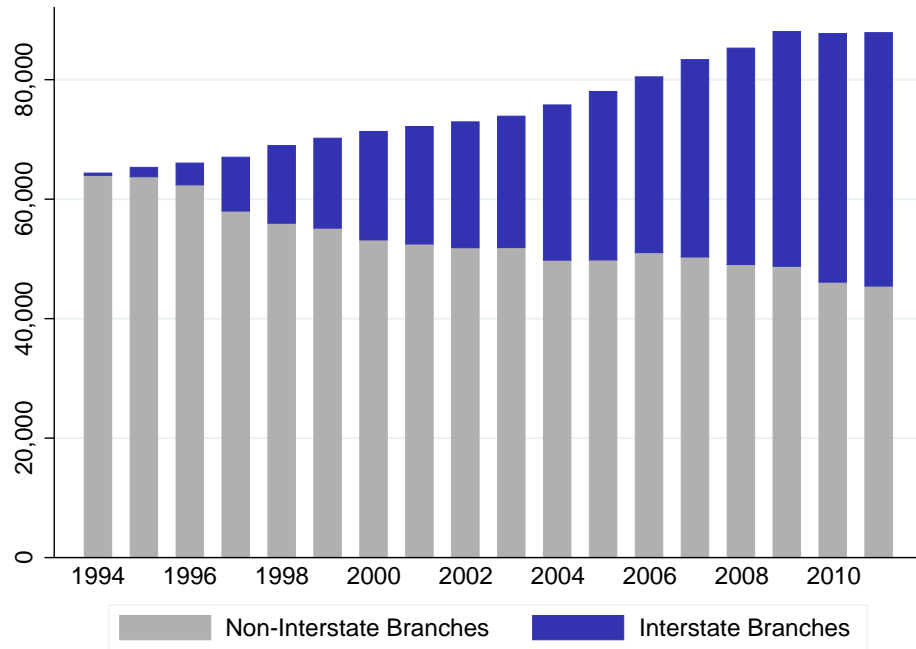
Figure 1. Number of Branches Operated by FDIC-insured Commercial Banks

This figure shows the number of interstate and non interstate branches operating in the U.S. over the years. Data are form the FDIC.
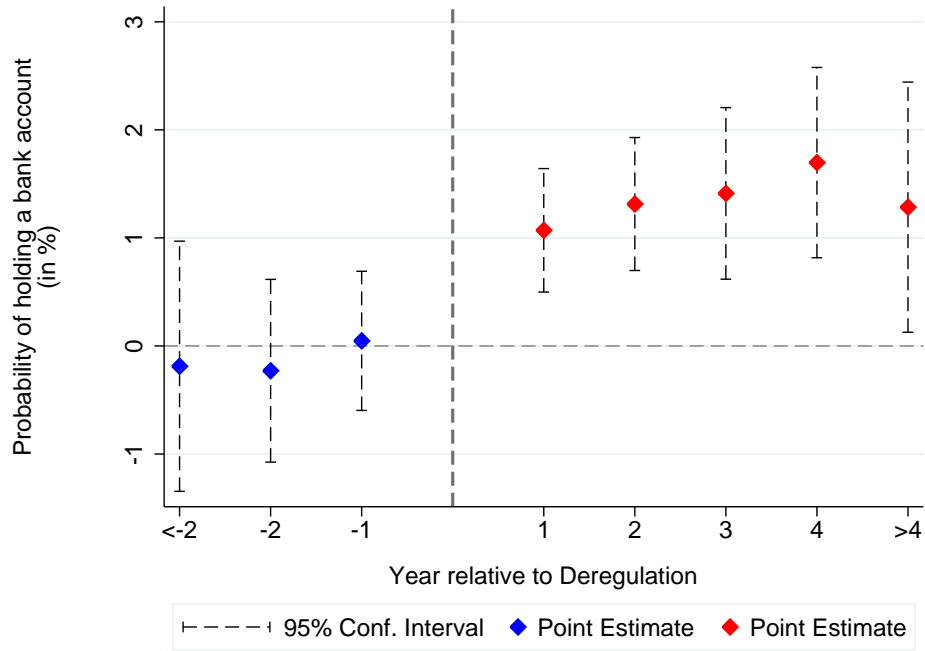
Figure 2. The Impact of Banking Deregulation on the Share of Banked Households

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. 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 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**	0.018*	0.028***	0.023***
		(0.010)	(0.009)	(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	136,176	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						
	Checking Account	**Savings Account**	**Checking Account (only)**		**Savings Account**		**Savings in o. Institutions**
Sample	**All**	**All**	**Poor**	**Low Inc.**	**Poor**	**Low Inc.**	**All**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Deregulation Index	0.007**	0.006***	0.014***	0.008*	0.004	0.009**	0.002**
	(0.003)	(0.002)	(0.003)	(0.005)	(0.003)	(0.003)	(0.001)
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	136,176	136,176	38,737	46,282	52,470	83,706	136,176

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 Debt

Dependent variable	=1 if the household holds			Debt-to-Inc. R.
	Debt	Bank Account		
Sample	Banked HH	Debt-free HH	In debt HH	All
	(1)	(2)	(3)	(4)
Deregulation Index	0.001	0.014***	0.007**	0.013
	(0.002)	(0.005)	(0.003)	(0.020)
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	84,089	63,039	72,660	135,699

This table reports the effect of owning a bank account on access to debt. We estimate linear probability regressions in which dependent variables are dichotomous variables that take the value 1 if the household owns debt in column (1), a bank account in columns (2) and (3) and the debt to income ratio in columns (4). 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***	0.012***	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	68,088	68,088

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** (0.005)	0.015*** (0.005)	0.013*** (0.004)		0.011** (0.005)	0.014** (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 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	0.008	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	78,371	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.5. 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.6. Addressing Endogeneity Concerns

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

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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 $wtsupp$.