Bank-Branch Supply, Financial Inclusion and Wealth Accumulation*

Claire Celerier † Adrien Matray ‡

Abstract

This paper studies the impact of financial inclusion on wealth accumulation. Exploiting the US interstate branching deregulation between 1994 and 2010, we find that an exogenous shock to bank branch supply improves low-income household financial inclusion. We then show that financial inclusion leads to the accumulation of both liquid and durable assets through higher investment and a better access to vehicle loans. This increase in asset accumulation translates into a higher net worth and a lower probability of facing economic hardship. The results suggest that promoting financial inclusion for low-income populations can improve household wealth accumulation and financial security.

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†**Claire Celerier - Rotman School of Management, University of Toronto**, E-mail: claire.celerier@rotman.utoronto.ca.

‡**Adrien Matray - Princeton University**

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

Close to 40% of the world's population and 30% of the United States' low-income population are unbanked, i.e., possess neither a checking nor a savings account. Policy makers and regulators around the world are beginning to make expanding financial inclusion a priority given the potential benefits of financial inclusion. This paper addresses the following question: does financial inclusion spur wealth accumulation, and if yes, how? Answering this question is critical. If low-income households rely on informal or alternative financial services that are as efficient as standard financial services to accumulate wealth, forcing financial inclusion might be inefficient, or even harmful. Oppositely, if unbanked households would benefit from financial inclusion, but are constrained by the supply of banking services – through limited coverage of branches in poor areas, minimum account balances or large overdraft fees –, there is room, and maybe even a need, for policy intervention.

The objective of this paper is to understand whether in a developed economy – the United States – financial inclusion is constrained by the supply of banking services and whether financial inclusion can promote wealth accumulation. The private cost of being unbanked is likely to increase with a country level of financial development for two reasons. First, in a financially developed country, people have to rely more on financial services

to consume, invest and save than in developing countries.[1] Second, the use of informal finance is likely to be less pervasive in a developed economy, as transactions are more impersonal and personal networks less important.

Estimating the effect of financial inclusion on household wealth accumulation is challenging. First, it requires a shock on the supply of financial services that is exogenous to both household demand for financial services and local economic conditions. Second, we need detailed household-level data on household spending and wealth, as well as information on their socio-demographic conditions and usage of standard financial services. Our paper exploits the passage of the Interstate Banking and Branching Efficiency Act (IBBEA) in the United States in 1994 as an exogenous shock to the supply of banking services. We explore the effect of the resulting increase in financial inclusion on household wealth using micro data on households from the Survey of Income and Program Participation (SIPP) that we complement with data from the Bureau of Labor Statistics' Consumer Expenditure Survey (CEX). The SIPP's unique focus on low-income American households, coupled with its yearly frequency, make the data particularly well suited for our analysis.

First, using bank branch location from the FDIC, we establish that interstate branching deregulation increased the density of bank branches. While the passage of the IBBEA in 1994 made bank branching across states legal, states kept the right to erect barriers to the entry of interstate branches, and partially lifted these barriers over the following years in a staggered way. Following Rice and Strahan (2010), we construct a time-varying index to capture these state-level differences in regulatory constraints to investigate the

[1]This might sound counterintuitive given the literature on finance and growth, but it should be noted that when borrowing and lending are less important for the functioning of the economy, the private benefit of an extra liberalization may not be large, at least initially.

effect of the staggered deregulation across states and years on household financial inclusion. We find that the density of bank branches increases by around 30% in poor counties after a state fully deregulates.

Second, using the SIPP from 1993 to 2010 to identify low-income households with or without a bank account, we show that interstate branching deregulation is associated with a significant drop in the rate of unbanked households among low-income populations. Exploring the dynamic effects of deregulation, we show that there is no pre-deregulation trend. The share of low-income households with a bank account increases by 4 percentage points after a state fully deregulates, which corresponds to a 15% increase in relative term. In all of our specifications, we control for a large number of household covariates that capture several dimensions of income, skills and labor status and for main state macroeconomic variables to further control for potential demand effects. This result suggests that even in a well-developed financial market, low-income households are partly rationed by the supply of banking services.

Third, we develop a battery of tests suggesting that the interstate branching deregulation is a reliable instrument for the probability of holding a bank account. We first show that the increase in financial inclusion following deregulation is not driven by the demand of financial services by looking at the density of credit union branches that are unaffected by the deregulation because of their legal status. The absence of effect on this placebo sample confirms that branching deregulation constitutes a supply shock that does not reflect contemporaneous or expected changes in the demand for banking services. Second, we look at the effect of deregulation on personal income and employment using Census data but also CEX and SIPP data. In all these databases, and at any level of aggregation – states, MSAs, counties, households – we find that deregulation has

no measurable impact on income and employment, both for the whole population and low-income populations. While 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), all these papers explore different deregulation episodes that preceded and were completed *before* the interstate branching deregulation we are looking at. Rice and Strahan (2010) and Favara and Imbs (2015) show that the real effects observed in earlier periods in response to different shocks are not observed in 1994. Finally, we show that deregulation affects households within the same MSA after controlling for MSA times year fixed effects. This specification suggests that if deregulation had any effect on economic conditions that should be only at a very local level, which seems unlikely.

With this instrument in hand, we start by investigating the effect of holding a bank account on the flow of household investment in durable assets and savings. Using the CEX data, we find that households with a bank account invest $5,000 more per year on durable assets. They spend $4,000 more in the financing and repair of their vehicles, and are significantly more likely to invest in the maintenance and repair of their home. These increases in investment hold when we control for a rich set of household demographics, as well as income decile. We also find that they are not explained by differences in total expenditures.

Next, consistent with the effect of financial inclusion on household investment and saving behaviour, we find that having a banking account translates into holding a larger amount of assets, not only of bank assets but also of vehicle assets. This results in a higher amount of total assets. Asset accumulation is fostered by better access to vehicle loans and credit card debt and leads to an increase in household net worth.

Finally, we find that households with a bank account are less likely to face economic hardship when they are exposed to an exogenous negative income shock produced by the layoff of one of the household members. While the probability of economic hardship as measured by the failure to pay important bills - food, mortgage, rent and utilities - or obtain needed medical care increases around 50% for unbanked households after a layoff, it remains stable for households with a bank account. This result suggests that financial inclusion allows households to smooth consumption.

Our paper contributes to the literature on the determinants of being unbanked. This literature has been scarce primarily as a result of the challenge of disentangling the different factors (see Barr and Blank (2008) for a broad survey of the literature). Socio-economic characteristics 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. (2011) and Cole et al. (2016) show that by offering a savings account with lottery-like features, banks can motivate the opening of savings accounts. While there are several studies that evaluate the real effects of bank competition and access to finance for firms, this paper provides new evidence on the real effects on households.[2]

Our paper also adds to the literature that investigates the effect of financial inclusion in developing economies. Using randomized control experiments, Ashraf et al. (2006) and Schaner (2017) find a positive effect of financial inclusion on saving behavior, Dupas and Robinson (2013b) on investment in preventive health, Prina (2015) on education and Dupas and Robinson (2013a) on starting a business.Dupas et al. (2017) find a limited effect on wealth accumulation when households are directly *offered* a bank account. We

[2]e.g. Brown et al. (2016), and Nguyen (2016) on small business lending

address this question using a natural experiment in the U.S., where households may decide first, whether or not to open a bank account, second, where the use of informal finance is less pervasive, and third, where holding a bank account may also reduce household usage of costly alternative financial services (see Melzer (2011), Melzer (2017), Carrell and Zinman (2014) on the costs of access to payday loans, and Morse (2011) for the opposite view). Our results suggest that the individual benefits from financial inclusion increase as a country's financial development increases.

More generally, our paper complements the literature on the real effects of access to local financial institutions. A growing literature evaluates the real effects on firms (Brown et al., 2016; Nguyen, 2016). There is, however, little evidence on how local finance affects households. Bruhn and Love (2014) find a sizeable effect of an exogenous increase in access to finance on labor market activity, Suri and Jack (2016) on local poverty, Brown et al. (2016) on financial health in the future. In a recent paper, Agarwal et al. (2017) document that an Indian program enforcing the creation of bank accounts alongside financial literacy training and the provision of insurance facilities led to an increase in liquid savings over time. Our paper shows how an increase in branch density fosters the take up of bank accounts for households, which results in higher household total asset accumulation through saving and investment in durable goods.

The rest of the paper proceeds as follows. Section 2 presents the effects of branching deregulation on financial inclusion, Section 3 describes the identification strategy to study wealth accumulation for households. Section 4 presents the results on investment, wealth and access to debt. Section 5 presents results of the effect of financial inclusion on household financial security. Section 6 concludes.

2 Branching Deregulation, Branch Density and Financial Inclusion

This section first describes the nature of the changes to bank branching regulations experienced in the United States since 1994. The section then establishes the effect of interstate branching deregulations on the supply of bank branches in low-income counties, and then on financial inclusion.

2.1 Branching Deregulation

Restrictions on interstate banking and branching have their historical roots in the 1789 Constitution, which gave states the right to charter and regulate banks (Johnson and Rice, 2008).[3] Since then, states had been collecting revenues from banks by charging fees for granting charters and levying taxes, giving them incentives to restrict competition from out-of-state banks. This lead to the adoption of the McFadden Act in 1927 that implicitly prohibited interstate branching by commercial banks. In 1956, the Bank Holding Company Act ended the development of bank holding companies that were circumventing the existing law and acquiring branches across states. The Bank Holding Company Act prevented 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.

[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 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.[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.

INSERT FIGURE 1 AROUND HERE

2.2 The Effect on Bank Branch Density

We investigate the effect of interstate branching deregulation on the density of bank branches in low income counties using data from the Sum of Deposits (SOD) maintained

[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.

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. We also collect data on county population, poverty and urbanisation from the Census Bureau, unemployment rates from the Bureau of Labor and Statistics, and on personal income from the Bureau of Economic Analysis. Based on these data, we identify poor counties as counties where the poverty rate is at the top quartiles of the distribution in 1993.

As motivating evidence, Figure 2 first documents a negative correlation between county poverty, as measured by the county poverty rate, and the number of bank branches scaled by population in the county in 1994. In term of economic magnitude, the elasticity amounts to -0.11, implying that a relative increase of 10% of the poverty rate is associated with a decrease of 1% in bank branch density. Counties with higher poverty rate therefore face a lower supply of bank branches at the beginning of our sample period.

INSERT FIGURE 2 AROUND HERE

We then assess whether interstate branching deregulation leads to a positive supply shock on branch density. To estimate the effect of deregulation, we run the following model:

$$Log(BankBranchDensity_{c,t}) = \alpha + \beta Deregulation_{s,t} + \lambda CountyControl_{c,t} + \delta_t + \eta_c + \epsilon_{c,t} \quad (1)$$

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

For each measure of branch density, we look both at all the counties and specifically at "low-income counties", that we define as counties where the fraction of households living below the poverty line is in the top quartile of the poverty rate distribution within each state such that every state is represented.

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

INSERT TABLE 2 AROUND HERE

2.3 The Effect on Financial Inclusion

2.3.1 Measuring Financial Inclusion: SIPP data

To assess whether the effect of deregulation on bank branch density translates into a higher household access to bank accounts, we use data from the SIPP covering the 1993-

2010 period.[5] 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 providing a complete list of all assets and liabilities held by each household, and their value.[6] We exploit the data from these topical modules to create a dummy variable *BankAccount* that takes the value 1 if at least one member in the household holds either a checking or a savings account, and 0 otherwise. We consider as a savings account any interest earning account in a banking institution, which includes savings accounts, interest earning checking accounts, money market deposits and certificates of deposits. We also collect data on household total wealth, debt, and net worth.[7]

The large size of the SIPP sample allows us to focus on low-income households, i.e., those below 200% of the poverty threshold, which is key for our analysis because low-income households are more likely to be rationed by banks.[8] We work at the household rather than the individual level because households often pool resources; a bank account in one member's name can provide access to banking services to other members of the same household. We collapse each household observation at the year level. We then drop households whose head is less than 20 and households with strong inconsistencies in their asset declaration - mainly households declaring holding bank debt but no bank accounts, or households declaring negative wealth. This leaves us with a total sample of 130,125 low-income households living in 45 states plus the District of Columbia over the 1993-2010 period.

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

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

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

[8]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.

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 household 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 10 in the appendix). One potential explanation would be the rapid development of alternative financial services over this period. The 2011 National Survey of Unbanked and Underbanked Households from the FDIC indicates that the proportion of unbanked households has also increased slightly during the recent financial crisis.[9]

Table 1 shows the summary statistics for banked and unbanked households in our sample. While we make no causal statement here on the role of financial inclusion in asset accumulation, a clear correlation emerges: "banked" households have a total wealth almost four times higher than unbanked households, despite having a monthly household income that is on average only 1.3 times higher.

INSERT TABLE 1 AROUND HERE

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

2.3.2 Deregulation and Financial Inclusion

We estimate the effect of deregulation on the probability of holding a bank account by estimating the following linear probability regression:[10]

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

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

[10] 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. In addition, Angrist and Pischke (2009) 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. Therefore, following Angrist and Pischke (2009) we use a linear probability model. However, our results still hold in logit regressions

1 in the online appendix reports the coefficients of the control variables.

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.

Table 3 reports eight versions of our baseline regression, which all indicate a large, positive and robust impact of banking deregulation on the share of banked households. The first column does not include any control. The coefficient on *Deregulation index* is 0.011 and significant at the 1% level. That is, when a state fully deregulates, we observe an increase in the share of households with a bank account of almost 4.5 percentage points. To further address endogeneity concerns, we then introduce our large set of household and state level controls in the second column of Table 3. These controls capture factors that would foster the demand for banking services at the household level and the economic conditions that may drive deregulation. The coefficient on *Deregulation index* subsequently remains stable, suggesting that the deregulation is not correlated with other households and state-level characteristics that may affect the decision to open a bank account. Column 3 includes *State* $\times$ *Trend* as a control variable, such that the effect of the reform is identified purely by a deviation from a trend that differs for each state. In column (4), we include MSA $\times$ Year fixed effects, such that the effect is purely

identified using households living in the same MSA but from two different sides of a state border. We find that *within the same MSA*, households who live in a state that has deregulated are more likely to hold a bank account relative to households in the same MSA but who live on the other side of the border. One important limitation of this strategy is that it strongly reduces the sample size, both because MSAs do not map perfectly the US territory and because the SIPP stopped collected information about MSAs after 2005. Still, it is noticeable that our point estimate is barely affected, despite being less significant. We test for pre-trend in column (5). We interact four dummy variables indicating four periods around the deregulation date with our deregulation index: more than 3 years before, less than 3 years before, 0 to 3 years after, and more than 3 years after. We observe that only the interaction terms with the dummies indicating years after deregulation have a positive and significant coefficient. Therefore, we observe no pre-deregulation trend, and the share of banked household increases only after deregulation takes place. These findings suggest that deregulation is not endogenous to the share of unbanked households but *causes* an increase in the share of banked households.

Finally in columns (6) to (8), we check the robustness of our result. Column (6) starts the sample in 1997 (the date at which the IBBEA becomes effective), and column (7) ends it in 2005 (the end of the deregulation wave). In column (8), we restrict the sample to the largest 11 states (California, Florida, Georgia, Illinois, Michigan, Missouri, New York, North Carolina, Ohio, Pennsylvania and Texas) to ensure that our results are not driven only by small states. We find that our results still hold.

By combining the results in Table 2 and Table 3, we compute a back-of-the-envelop overall elasticity of bank-account to bank branch density in a county of roughly 30%.[11] In

[11]Branch density increases by 28% (0.072×4) after a state fully deregulates, while bank account increases by 8% (0.048×0.61), 0.61 being the initial share of bank accounts among low-income households,

other words, when the density of bank branches increase by 10%, the share of low-income households that are financially included increases by more than 3%.

INSERT TABLE 3 AROUND HERE

Finally, we analyze the dynamics of the share of banked households around deregulation using a different way to test the parallel trend assumption by incorporating each deregulation steps. In Figure 3, we re-estimate our baseline model, where the deregulation index is replaced by dummy variables for each year around *each* deregulation step. More specifically, we decompose each of the four components of the deregulation index into four dummy variables associated with each year around the deregulation. Then, we sum over the four components of the deregulation index to obtain dummy variables indicating the year around the deregulation. The deregulation year is the reference year. The figure plots the change in the likelihood of holding a bank account in the years three years before and three years. 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.

INSERT FIGURE 3 AROUND HERE

3 Financial Inclusion and Household Wealth: Empirical Strategy

This section first presents the data we exploit to investigate the effect of financial inclusion on household wealth accumulation. The main specification is then introduced. We finally address the endogeneity concerns that might affect the validity of our empirical strategy.

hence leading to a total elasticity of $8/28 = 0.29$

3.1 Data on Household Investment and Wealth

To study the effect of financial inclusion on wealth accumulation, we rely on two sources of data: First, the Consumer Expenditure Survey (CEX), to measure the flow of household investment and saving; Second, the SIPP, to measure the stock of wealth, asset and liabilities.

CEX

We turn to the Interview Survey of the CEX to get a picture of annual consumption over the period 1996–2010 as the SIPP only includes information on household balance sheets. The CEX is a rotating survey conducted by the BLS, where households are observed for four quarters.[12] The survey contains around 7,500 distinct households and detailed information about the flow of consumption across several categories.

For our analysis, we distinguish the consumption of non durable goods - which includes food and beverage, clothing and personal care, utilities, domestic services, nondurable transportation, nondurable entertainment - from the consumption of durable goods - which includes home repairmen and improvement and car financing and repair.

The CEX also contains information about socio-demographic characteristics, income and the use of banking services. All the socio-demographic characteristics that we use in our analysis are available in the CEX.

Similar to Aguiar and Hurst (2013), we drop households who reports no food consumption during a year. We also drop households whose head is younger than 20 year old and those living in states we cannot identify.[13]

[12]More precisely each household is observed five times but the first survey is a "warm-up" in the sense that the BLS asks households about their expenditures over the last month more for the sake of instructing them to record or remember these items for the subsequent surveys.

[13]Similar to the SIPP, the CEX bundles states that are too small for confidentiality reasons.

SIPP

The SIPP collects very detailed information on household balance sheet (both assets and liabilities) in the wealth inventory realized in the Wealth Topical Modules. We also exploit the Adult Well-being Topical Modules to obtain data about financial hardship.

3.2 Model

Studying how financial inclusion affects household wealth accumulation raises the issue of causality. Households that are more prone to investing, saving and have more wealth are also more likely to be both rich and to own a bank account. To address this issue, we use interstate branching deregulation to instrument our bank account dummy. Table 3 indicates that the t-stat of branching deregulation on bank account is sufficiently high to have a strong instrument (Stock and Yogo, 2005).

We therefore estimate two-stage least square regressions of the following sort:

$$Y_{ist} = \alpha + \beta \widehat{BankAccount}_{ist} + \theta X_{ist} + \lambda StateControl_{st} + \delta_t + \eta_s + \epsilon_{ist} \qquad (3)$$

where $\widehat{BankAccount}_{ist}$ is the dummy variable $BankAccount_{ist}$ equal to one if household i in state s at time t hold a bank account, instrumented by the deregulation index varying at the state-year level and estimated as in Equation 2. 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. Unless otherwise specified in a table, we include the same household controls as the one described to estimate Equation 2. The large set of state time-varying and household controls helps alleviate the concern that our identification strategy might capture the direct effect of the interstate branching deregulation on household economic prosperity. We cluster standard errors at the state level.

Y_{ist} is a household level outcome variable that measures investment, savings, wealth or debt. The parameter of interest is β, which measures the effect of holding a bank account on the outcome variable Y_{ist}.

3.3 Endogeneity Concerns

There are three main concerns that might affect the validity of our empirical strategy. The first one is that ex-ante, interstate branching deregulation may have been adopted in reaction to low-income households economic conditions or lack of financial participation. Second, following its adoption, the interstate branching deregulation may have improved economic conditions for low-income households - and hence the demand for bank accounts - and these improved economic conditions are imperfectly controlled for in our regressions. Third, our effect may partially be confounded with the effect of the Community Reinvestment Act (CRA). While the exclusion restriction of an instrument cannot be tested by definition, we address each of these concern extensively to comfort the empirical validity of our strategy.

Motives for Deregulation

We start by investigating the timing of deregulation following the method of Kroszner and Strahan (1999). We predict the timing of deregulation in a Weibull proportional hazards model using different variables that might correlate with financial inclusion in the future in columns (1) to (3) of Table 4. We find that our interstate branching deregulation is not predicted by the share of unbanked households before the deregulation or the log of personal income of low-income household (column (1)). The same is true when we include the state level unemployment rate of low income households (column (2)) and

state GDP and total unemployment rate (column (3)). None of these variables have a significant effect on the timing of deregulation.[14]

Ruling-out Income Channel

Despite the fact that economic conditions are not able to predict the adoption of the law, it might still be the case that *following* the adoption, the deregulation has a positive effect on economic activity which might affect as a result both the demand for banking services and household wealth. We reject this hypothesis by looking first as whether deregulation has affected the density of a control group of institutions that were not affected by deregulation. Second, we test directly whether interstate branching deregulation has any real effects on the whole economy and on low-income population economic conditions.

Legally Unaffected Lenders. Credit union were not affected by the adoption of the IBBEA. Therefore, we can test if the interstate branching deregulation affected this placebo sample of financial institutions. 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.[15]. Panel B of Table 2 shows that branching deregulation has no effect on the density of credit union branches.

Aggregate Effects. To further rule out that deregulation affected economic conditions, we look at the effect of the interstate branching deregulation on different measures of state prosperity using Current Population Survey (CPS) data in a difference-in-differences set-

[14]We detailed the methodology of the Weibull proportional hazards model in the appendix

[15]Data can be downloaded here: http://www.ncua.gov/DataApps/QCallRptData/Pages/CallRptData.aspx

ting similar to Equation 1. In columns (4) and (5) of the Table 4 we show that the deregulation has a insignificant effect on income per capita (column (4)) and unemployment rate (column (5)). In the Online Appendix we provide an additional battery of tests: Table 4 show a similar absence of result when we focus on poor or low-income in the state; Tables 5 and 6 show that this absence of effect on aggregate income and on the unemployment rate is also observed at the MSA and county levels (both when focusing on all counties or restricting ourselves to low-income counties). Finally, to rule out the possibility of any long run effect and effect on income risk, Table 7 in the online appendix documents the absence of effect on income three years after, as well as on income volatility.

The absence of aggregate effect might still mask the distributional effects, whereby poor and low-income households are still positively affected. In Table 8 in the Online Appendix, we show using a similar method as Beck et al. (2010) that the deregulation has no effect on the Gini index and the Theil index in treated states.

Effect on Household: SIPP. Another way to test for potential effects of deregulation on low-income household unemployment and income is to use directly our SIPP sample. This test offers another source of validation as we are now estimating the effect of deregulation at the households level (rather than for the aggregate) and specifically for the subsample of low-income households we are interested in. Column (6) of Table 4 shows that interstate branching deregulation has no impact on the log of household income and column (7) shows no effect on the likelihood for the household to be unemployed.

Overall, these findings may seem surprising in light of the literature showing that intrastate branching and interstate banking deregulation affected the real economy di-

rectly.[16] However, the deregulation episodes we consider in this paper have little connection with those that were documented to have real effects (Favara and Imbs, 2015). The index of restrictions used here starts after 1994, once all the deregulation waves documented as having direct real effects were completed. In addition, Rice and Strahan (2010), using the same deregulation, show that while the increase in banking competition leads to a decrease in interest rates for small firms, there is no effect on the amount that small firms borrow, which is consistent with the absence of macroeconomic effect.

INSERT TABLE 4 AROUND HERE

MSA* × *Year Fixed Effects Column (4) in Table 3 focuses on households living in the same MSA but in different states. This specification includes MSA times year fixed effects. We find that within the same MSA, households who live in a state that has deregulated are more likely to hold a bank account than households in the same MSA but that live on the other side of the border. The argument would have to be that the demand boom that motivates commercial banks to lobby for deregulation is extremely localized: the boom would have to prevail in counties on one side of the state border, but not in others across the border, even though they are contiguous and actually part of the same MSA.

The focus on this reduced sample is important because it helps alleviate concerns of an omitted variable bias, and the simultaneity and reverse causality issues that come with it. In principle, the positive estimates of β in Table 3 could reflect unobserved variables driving both deregulation and financial inclusion at state level, or the effect of deregulation on household economic conditions that would then drive the demand for

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

bank accounts. This argument already has trouble explaining why credit unions do not seem to be taking advantage of such a hypothetical boom, and why we do not observe any effect of deregulation on household income level, distribution and volatility at the state, MSA, county and individual levels. It has more trouble still explaining a differential response between households within the same MSA and only separated by a state border. The argument would have to be that the demand boom that leads to financial inclusion is extremely localized: the boom would have to prevail in counties on one side of the state border, but not in others across the border, even though they are actually part of the same MSA.

The Community Reinvestment Act

One final concern with our identification strategy is that the expansion of bank branches we show in Table 2 might be partially driven by 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.[17]

This might be an issue given that to obtain satisfactory CRA ratings, banks not only need to comply with the lending requirements to underserved households, but also need to provide banking services to low- and moderate-income groups, as part of their CRA service test. CRA ratings can be important when banks want to merge, as a ratings consider too low by the regulator can be a motive for refusing the operation. When assessing the CRA ratings, the regulator evaluates the ratings of both parties - the buyer

[17]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).

and the target-.[18] Because the CRA is a federal law, the year fixed effects in our regression should partly help controlling for its effect. However, our state level results could still be biased if the deregulation we are exploiting changes the incentives of banks to provide services to low-income households because they are planning to be involved in a M&A operation.

We test formally this concern in two ways. First, because the effect of the deregulation might partially reflect the effect of the CRA if banks that are in deregulated states are more likely to be involved in M&A operations - whether as an acquiring bank or as a target -, we show that the volume of bank M&A does not change when a state deregulates. We look at the number of targeted banks in treated states, the number of acquisitions made by banks headquartered in treated states, and the total number of M&A operations involving banks - whether as target or buyer - in a treated state. Second, we show that the effect of the deregulation on the expansion of bank branch density is robust to removing from the sample banks involved in some way in a M&A deal. Finally, it should be noted that banks can partially improve their CRA ratings when needed without changing their actual business just by *buying* existing loans that qualify for the CRA, as shown in Avery and Brevoort (2015).

4 Results

4.1 Investment in Durable Goods

We estimate the effect of financial inclusion on household investment in durable goods using the CEX data. We build three different investment variables using the CEX: "total

[18] We would like to thanks an anonymous referee for pointing this out.

investment", corresponding to the amount invested in durable goods, amount spent in automobile - both the financing of the acquisition of a new car and repairs - and finally the total amount spent in home equipment and repair. For each outcome variable, we look at the dollar level of spending (Panel A), a dummy variable equal to one if the household reports any positive investment spending (Panel B) and the log of total amount of investment spending (Panel C).

Table 9 in the Online Appendix first confirms that the first stage of our IV analysis holds using the CEX data. The effect is of the same magnitude as with the SIPP data. One step in deregulation leads to a 0.9 percentage point increase in the probability to get a bank account.

Column (1) of Table 5 then reports the effect of holding a bank account on total investment in durable goods. In Panel A, we find that holding a bank account translates into a higher total investment spending in durable goods of \$5,306 per year at the household level, significant at the 1% level. Panel B shows a positive but not significant effect on the likelihood for the household to have make any investment spending.

In Panel A of Column (2), we look at the amount invested in car and find that having a bank account translates into a higher investment of \$4,359 per year, significant at the 5% level. Similarly to total investment, we find that holding a bank account is associated with a positive but not significant effect on the probability to have *any* investment in car.

Finally, column (3) reports the effect on investment in housing. While having a bank account leads to a positive but insignificant effect on the total amount invested (Panel A), we find that it leads to a high increase in the likelihood to make any positive investment in housing (Panel B).

Panel C essentially confirms results of Panel A when we use the log instead of the

level of investment. While the results might seem high - for instance in Column (1) the effect of having a bank account multiplies by six the amount of total investment made -, it should be reminded that the baseline amount of investment for the households in our sample is low.

INSERT TABLE 5 AROUND HERE

4.2 Household Wealth

To estimate better the role of having access to a bank account on wealth accumulation for low-income households, we now turn to the SIPP which contains detailed data on household balance sheet (both assets and liabilities). We estimate the effect of holding a bank account on different types of assets in Table 6, using the specification described in equation 3, where $\widehat{BankAccount}$ is *Bank Account* instrumented by the branching deregulation index. For every measure of household wealth, we use different version of the dependent variable: the level amount in dollar (Panel A), a dummy equal to one if the household reports any positive amount (Panel B) and the log of the amount (Panel C).

In Column (1) of Table 6, we look at total assets excluding housing that we estimate separately in Column (3). Having a bank account has a clear positive effect on our measure of total asset, whether we look at the level (+$7,359, Panel A), the probability to report any positive amount (+70%, Panel B) or the log amount (Panel C). In all cases, the effect is significant at the 1% level.

We then look at two different broad categories of assets: "permanent assets" (that we can split across vehicle and house value) and "liquid assets" (split across non bank asset, interest-bearing account and checking account).

In Column (2), similar to the result in Column (1), we find that holding a bank

account has a consistent positive effect on the value of the car banked households have (Panel A), the probability to have a car (Panel B) or the log of car value (Panel C), always at least significant at the 5% level. Holding a bank account however, does not seem to allow low-income households to hold a house with a higher value (Column (3)).

When turning to the different components of liquid assets, we find that having a bank account also leads low-income households to accumulate liquid wealth on their bank account on top of accumulating permanent assets or borrowing. We find in particular that having a bank account allows low-income households to accumulate wealth on interest-bearing account (Column (5)), rather than just on checking account (Column (6)). While somehow mechanic since unbanked households cannot have wealth on bank accounts,[19] these two results are interesting for two reasons. First, accumulating wealth on interest-earning account can partially explain how financial inclusion can foster wealth accumulation among low-income, as it allows these low-income households to benefit from the effect of compound interest. Second, these results relieve the concern that unbanked households open a bank account but do not use it and do not accumulate savings.

INSERT TABLE 6 AROUND HERE

In addition to promoting savings and a progressive wealth accumulation, another reason financial inclusion could foster wealth among low-income household is by helping them access credit outside the very high cost options offered by Alternative Financial Services (AFS). We study this explanation in Table 7.

Column (1) of Table 7 shows that both the amount of total debt (Panel A and C) and the probability to hold any debt (Panel B) increase for households holding a bank

[19]This is why we only report the dollar level in Panel A and not the dummy or the log value in Panel B and C respectively.

account. In term of economic magnitude, we find that holding a bank account translates into an increase by 6,987 dollars in the total amount of debt.

One open question is whether this higher access to debt explains the entirety of asset accumulation we have found in Table 6. To study whether it is the case or not, we look at net worth that we define as total wealth minus secured and unsecured debt. We find in particular that financial inclusion has almost the same effect on the log value of net worth (Column (2)) than it has on total asset in Table 6. This result suggests that the effect of holding a bank account on household wealth is not driven only by an easier access to credit and debt accumulation.

When then study the effect of having a bank account on access to unsecured and secured debt separately across their different components. The two main conclusions that emerge is that having a bank account improves access to credit card debt (Column (5)) and vehicle loans (Column (3)). Having a higher likelihood to have a credit card (Column (5)–Panel B) and the log amount of it (Column (5))–Panel C), can have important welfare consequences by allowing households to smooth better their consumption (something we explore in the next section). The higher access and higher amount of vehicle loans (Column (3)) can partially explain how financial inclusion allows households to accumulate permanent wealth in the form of cars, as we find in Table 6.

INSERT TABLE 7 AROUND HERE

5 Financial Inclusion and Household Financial Security

We finally study how financial inclusion may reduce low-income household financial distress, in particular when they are exposed to a negative income shock.

5.1 Empirical Strategy

We exploit a different module of the SIPP, the Adult Well-being Topical Module, which contains detailed questions about financial hardship and ability for the households to pay for important expenditures (e.g. Melzer (2017)). The module is available for almost all the waves used so far and we have observations for 1995, 1998, 2003, 2005, 2010.

We create four different dummies of financial hardship. The first one indicates whether the household has failed to meet any basic essential expenses over the past twelve months - which includes food expenses, medical expenses, utility bills and rents or mortgage payments. We then split this variable into its main components. The second dummy indicates whether the household has not had enough food to eat over the past four month, the third one whether the household failed to pay utility bills, and the fourth one whether the household did not pay his rent or mortgage in full.

To identify households experiencing a negative income shock, we follow Hsu et al. (2018). Using respondents' employment history, we code $Layoff$, an indicator for whether anyone in the household has been without a job and looking for work in the year-long period for which mortgage economic hardship is assessed. Around 15 percent of our sample of households experience such an unemployment spell. This figure is higher than the unemployment rate, because it refers to households rather than individuals and, it is

measured over a year rather than at a single point in time.

5.2 Results

Table 8 presents the results. In Column (1), we find that $Layoff$ is associated with a higher probability to fail to pay for basic expenses, consistent with the fact it represents a negative income shock. When adding the interaction with *Bank Account* instrumented (Column (3)), we find that financial inclusion completely undo the negative effect of layoff. While unbanked households are 60% more likely to fail to pay for basic expenses (the coefficient on $Layoff$ not interacted), households with a bank account do not experience such difficulties. We find similar effects when looking at paying for utilities (Column (4)) or the ability to pay in full his rent or mortgage (Column (5)).

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.

We rule out the alternative interpretation of our result that bank competition decreases the share of unbanked households by fostering demand for bank accounts. First, deregulation has no impact either on the sample of non-deregulated institutions or on county prosperity or individual income and employment status. Second, in all of our

specifications, we control for a large set of covariates that capture demand effects at both the household and state levels.

This result allows us to use the interstate branching deregulation as an instrument for holding a bank account and to test how financial inclusion affect low-income household wealth accumulation.

We find that having a bank account allow households to invest in durable goods. This translates into higher stock of asset, both in the form of permanent assets (cars) and liquid assets (amounts on interest-bearing accounts). We also find that part of this increase in asset can be accounted for by better access to debt. This higher access to debt however do not prevent households from accumulating net worth.

We finally find that financial inclusion can help low-income households smoothing consumption shocks, by reducing the likelihood they have to cut on essential spending when facing a negative income shock coming from being laid-off.

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 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

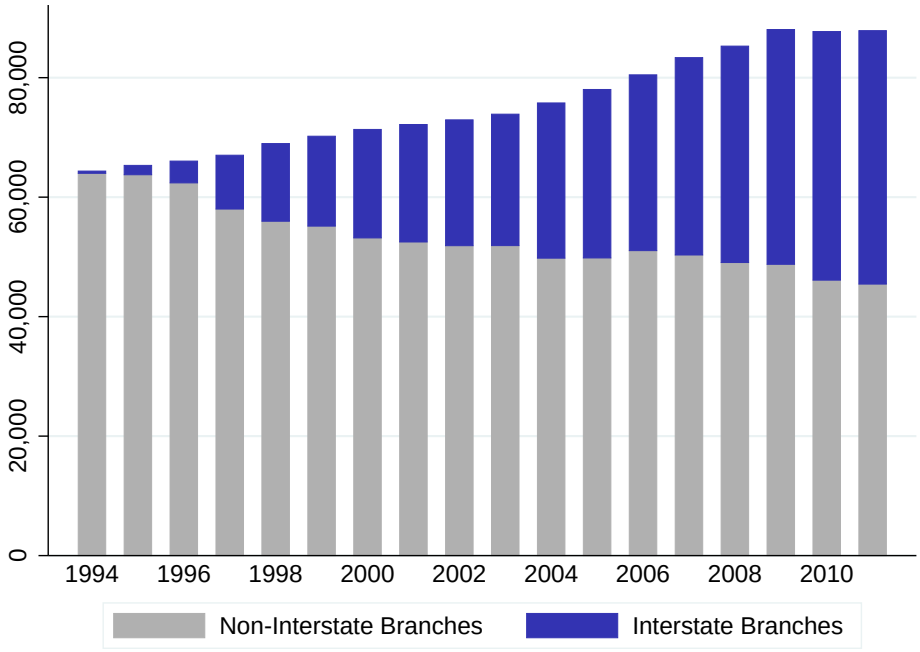

Figure 1. Number of Branches Operated by FDIC-insured Commercial Banks

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

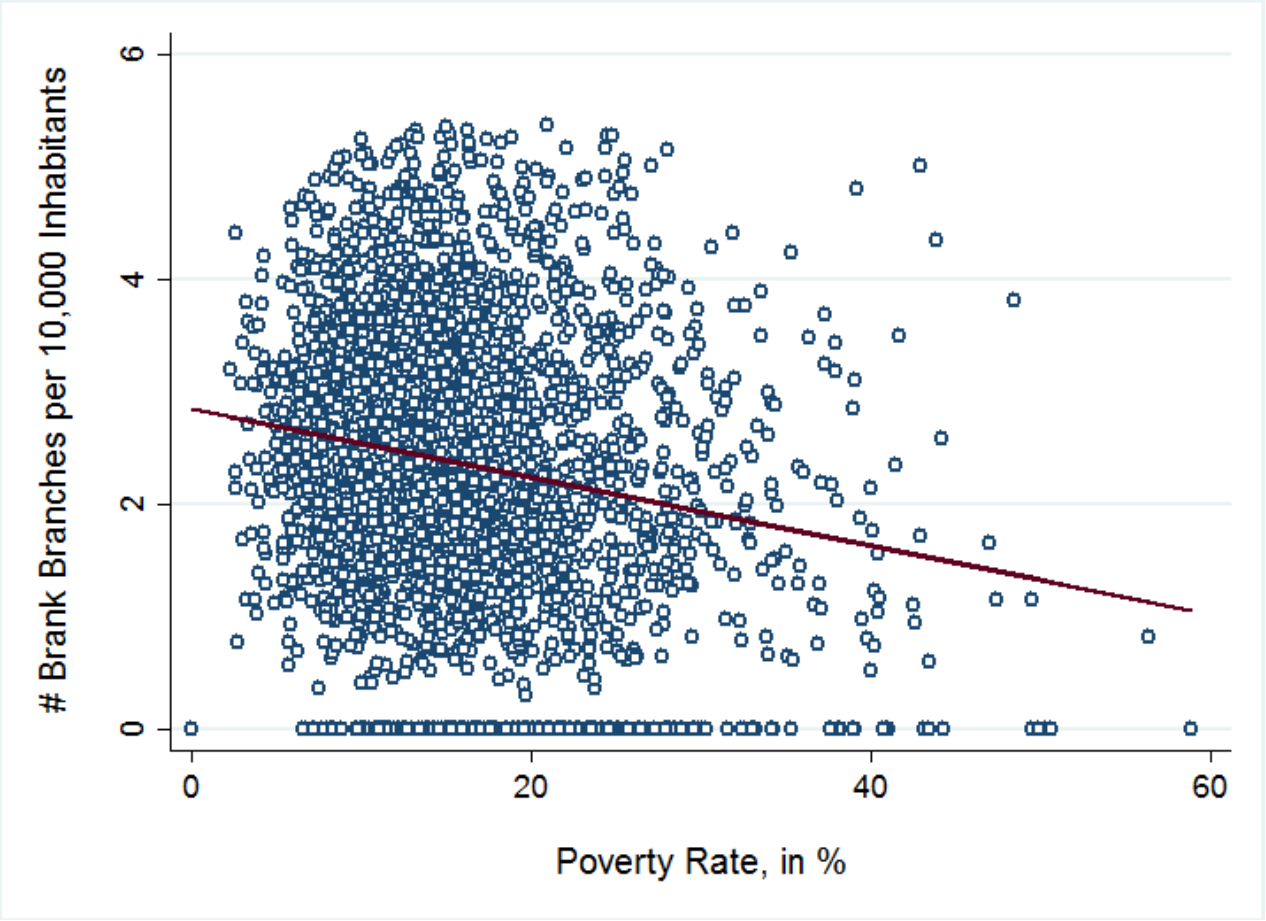

Figure 2. Bank Branch Density and County Characteristics, 1994

This figure shows the correlation between bank branch density at the county level, calculated as the number of branches per 10,000 inhabitants over county poverty rate. The red line is the fitted linear regression. Data are form the FDIC and BEA.

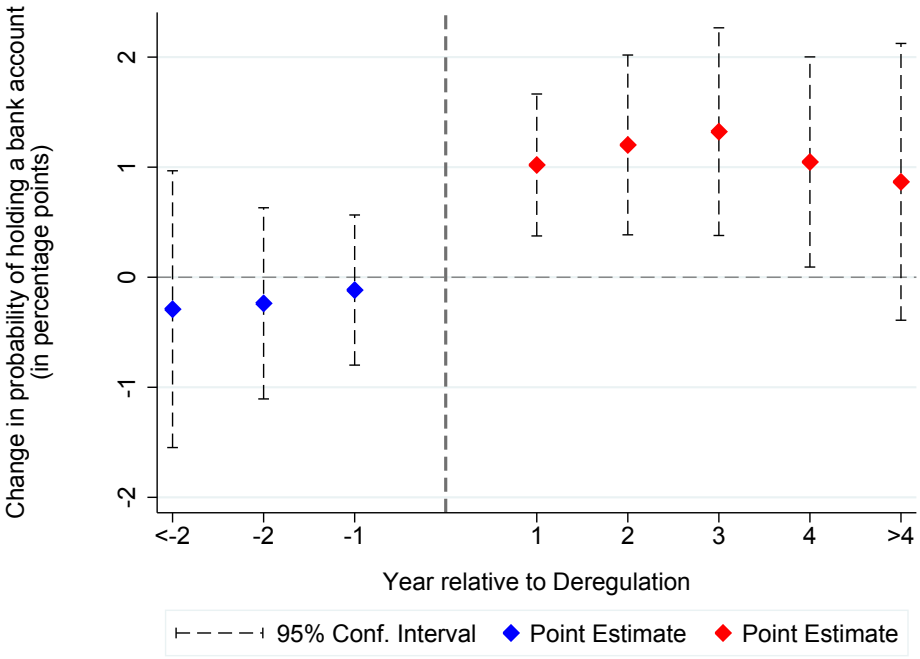

Figure 3. The Impact of Banking Deregulation on the Share of Banked Households

This figure shows the relative change in odd ratios of holding a bank account around deregulation dates among low-income households. The specification is the same as Equation (2), except that the deregulation index is replaced by a collection of variables $\sum_{s=1}^{4} I^s(k)$, where $I^s(k)$ is a dummy equal to one exactly k years after (or before if k is negative) the state implements a given step of deregulation $s \in \{1, 2, 3, 4\}$. We plot the point estimates for $k = -3, \ldots, 4$, using the deregulation year $k = 0$ as the reference year, as well the 95% confidence interval using standard errors clustered at the state level.

B Tables

Table 1. Summary Statistics

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

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

Table 2. Interstate Branching Deregulation and Bank Branch Coverage

	Branch Density Per Inhabitants		**Branch Density Per Square Miles**	
Counties	All	Low Income	All	Low Income
	(1)	(2)	(3)	(4)
	Panel A. Commercial Banks			
Deregulation	0.069***	0.072***	0.069***	0.078***
	(0.019)	(0.020)	(0.019)	(0.023)
County-Year Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Observations	46,380	11,035	46,358	11,035
	Panel B. Credit Unions			
Deregulation	0.005	0.005	-0.001	-0.004
	(0.009)	(0.006)	(0.008)	(0.005)
County-Year Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Observations	25,186	5,065	25,186	5,065

This table reports OLS regressions of the Interstate Deregulation Index on bank and credit union density (Panels A and B respectively). The dependent variable is the log of the number of branches in each county per 1,000 inhabitants in columns (1) and (2) and per square miles in columns (3) and (4). The deregulation index ranges from 0 to 4, 0 is least, 4 is most deregulated. In columns (2) and (4) the sample is restricted to counties in the top quartile of the distribution in terms of poverty rate of each state. All regressions include county and year fixed effects. Time-varying county controls include controls for the log and the delta log of per capital income and population, the poverty rate and the unemployment rate. Standard errors are clustered by state.

Table 3. Interstate Branching Deregulation and Financial Inclusion: SIPP 1993 - 2010

	=1 if the household holds a bank account							
Sample	All					1993-2006	1997-2010	Large States
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Deregulation Index	0.011**	0.011***	0.010**	0.02*		0.012***	0.016***	0.014**
	(0.004)	(0.004)	(0.005)	(.012)		(0.004)	(0.005)	(0.005)
Deregulation [≤ t-4]					-0.002			
					(0.019)			
Deregulation [t-3,t-1]					-0.010			
					(0.017)			
Deregulation [t+1,t+2]					0.033**			
					(0.014)			
Deregulation [≥ t+3]					0.027*			
					(0.020)			
Household Controls								
Income Decile FE	–	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Houshold Size FE	–	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sociodemographics	–	Yes	Yes	Yes	Yes	Yes	Yes	Yes
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	–
State × Year Trend	–	–	Yes	–	–	–	–	–
MSA × Year FE	–	–	–	Yes	–	–	–	–
Observations	119,732	119,732	119,732	40,900	119,732	98,319	89,401	64,297

This table reports linear probability regressions of the Interstate Branching Deregulation Index on access to bank accounts. The dependent variable equals 1 if the household holds a checking or a savings account (SIPP 1993 – 2010). The deregulation index ranges from 0 to 4, 0 is least, 4 is most deregulated. Column (1) does not include any control, while columns (2) to (8) include household controls, plus time-varying state controls. All regressions include state and year fixed effects except column (4) which contains MSA × Year fixed effects. Household controls include monthly income decile fixed effects, household size fixed effects, family type, dummies for whether the household receives Social Security income or transfer income, household head education dummies (elementary, high school, college), unemployment dummy, race dummy for household head is black, and age as a cubic polynomial. State time varying controls include total unemployment rate, low-income population unemployment rate, population (log), GDP growth, GDP per capita (log) and a republican dummy. Column (3) include a state × time trend, Column (4) includes MSA × Year fixed effects. In Column (5), 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. Column (6) restricts to the sample period 1993–2006, while Column (7) restricts to the period 1997–2010. Column (8) restricts to the largest 11 states (California, Florida, Georgia, Illinois, Michigan, Missouri, New York, North Carolina, Ohio, Pennsylvania and Texas). Standard errors are clustered by state.

Table 4. Interstate Branching Deregulation and Economic Conditions

	Do Economic Conditions Drive Deregulation?			**Does Deregulation Affect Economic Conditions?**			
				State Level (CPS)		Household Level (SIPP)	
	Time to Deregulation (Weibull Model)			Income	Unemployment	Income	Unemployment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Unbanked Share (%)	-1.358	-0.308	0.480				
	(2.242)	(2.206)	(2.629)				
Low-Income Unemployment Rate (%)		10.566	10.241				
		(7.261)	(9.299)				
Low-Income Personnal Income	1.436	0.513	-0.504				
	(1.563)	(1.999)	(2.110)				
GDP per capita			1.228				
			(2.050)				
Unemployment Rate (%)			-0.055				
			(0.238)				
Deregulation Index				-0.002	-0.001	0.003	-0.001
				(0.002)	(0.004)	(0.004)	(0.001)
Household Controls							
Income Decile FE						-	-
Houshold Size FE						Yes	Yes
Sociodemographics						Yes	Yes
Time-Varying State Controls						Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	333	333	330	918	918	115,800	119,732

In Columns (1) to (3), we test if different state-level variables can predict the timing of the adoption of the different deregulation using a hazard rate model. Columns (4) to (5) reports the OLS regression of the effect of the Interstate Branching Deregulation Index (*Deregulation Index*) on log income and unemployment rate at the state-year level over the period 1993–2010. The deregulation index ranges from 0 to 4, 0 is least, 4 is most deregulated. We include state and year fixed effects. Columns (5) and (6) use our SIPP sample over the same period. The dependent variable is the log of household income (Column (5)) or a dummy equal to one if the household head is unemployed. Household controls include monthly income decile fixed effects (except when in Column (6) where we look directly at income as the dependent variable), household size fixed effects, family type, dummies for whether the household receives Social Security income or transfer income, household head education dummies (elementary, high school, college), unemployment dummy, race dummy, and age as a cubic polynomial. State time varying controls include total unemployment rate, low-income population unemployment rate, population (log), GDP growth, GDP per capita (log) and a republican dummy. Standard errors are clustered by state.

Table 5. Financial Inclusion and Household Investment Expenses: CEX 1996 - 2010

	Total Investment Spending	**Auto Financing and Repair**	**Home Equipment and Repair**
	(1)	(2)	(3)
	Panel A: Amount in $		
$\widehat{BankAccount}$	5,306***	4,359**	997
	(1,747)	(1,838)	(1,168)
	Panel B: Any - Indicator Dummy		
$\widehat{BankAccount}$	0.410	0.424	0.807**
	(0.341)	(0.373)	(0.380)
	Panel C: Log		
$\widehat{BankAccount}$	5.845**	4.619*	5.512**
	(2.855)	(2.397)	(2.666)
Household Controls			
Income Decile FE	Yes	Yes	Yes
Houshold Size FE	Yes	Yes	Yes
Age FE	Yes	Yes	Yes
Number of Children FE	Yes	Yes	Yes
Total Expenditures Excl Durables	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Observations	54,918	54,918	54,918

This table reports the effect of holding a bank account on investment spending using data from the CEX (1996–2010). $\widehat{BankAccount}$ is the dummy *Bank Account* instrumented with our deregulation index. The measure of spending varies across specifications as indicated in the Panel heading. Each model includes state and year fixed effects; additional control variables are included as specified in the table. The control variables include the following household characteristics are: income deciles, quarterly expenditures on non investment goods, number of household members fixed effects, household age fixed effects, and number of children fixed effects. Standard errors are clustered by state and are reported in parentheses. ** and *** indicate statistical significance at 5% and 1% levels, respectively.

Table 6. Financial Inclusion and Asset Accumulation: SIPP (1993 - 2010)

	Total Assets	Permanent Assets		Liquid Assets		
	Excl. House	Vehicle	Home	Non bank Assets	Interest Account	Checking Account
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Amount in $					
$\widehat{BankAccount}$	7,359***	2,640**	-29,446	580	3,316**	336*
	(2,935)	(1,261)	(24,161)	(436)	(1,717)	(177)
	Panel B: Any - Indicator Dummy					
$\widehat{BankAccount}$	0.70***	0.70**	-0.08	0.07		
	(0.16)	(0.28)	(0.14)	(0.14)		
	Panel C: Log					
$\widehat{BankAccount}$	6.676***	5.905***	-0.570	0.832		
	(1.328)	(2.027)	(1.545)	(1.032)		
Household Controls						
Income Decile FE	Yes	Yes	Yes	Yes	Yes	Yes
Houshold Size FE	Yes	Yes	Yes	Yes	Yes	Yes
Sociodemographics	Yes	Yes	Yes	Yes	Yes	Yes
Time-Varying State 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	61,802	61,802	61,802	61,802	61,802	61,802

This table reports the effect of financial inclusion on asset accumulation using the SIPP (1993–2010). $\widehat{BankAccount}$ is the dummy *Bank Account* instrumented by the Interstate Branching Deregulation Index. Non-bank assets include stocks or mutual fund shares and other liquid assets (i.e. savings bonds and life insurance policies). Interest earning assets held in banking institutions include amount in Savings Account plus amount in Interest-Earning Checking Account plus Amount in Money Market Deposit Accounts plus Amount in CD's. Interest earning assets held in other institutions include Amount in Money Market Funds, U.S. Government Securities, Municipal or Corporate Bonds and Other Interest-Earning Assets. The measure of assets varies across specifications as indicated in the Panel heading. All regressions include state and year fixed effects. Household controls include monthly income decile fixed effects, household size fixed effects, family type, dummies for whether the household receives Social Security income or transfer income, household head education dummies (elementary, high school, college), unemployment dummy, race, and age as a cubic polynomial. State time varying controls include total unemployment rate, low-income population unemployment rate, population (log), GDP growth, GDP per capita (log) and a republican dummy. Standard errors are clustered by state and are reported in parentheses. ** and *** indicate statistical significance at 5% and 1% levels, respectively.

Table 7. Financial Inclusion and Household Debt: SIPP (1993 - 2010)

	Total Debt	Net Worth	Secured Debt		Unsecured Debt		
	Excl. House	Excl. House	Vehicle Loan	Home Equity Loan	Credit Card	Non bank Debt	Bank Loans
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A: Amount in $						
$\widehat{BankAccount}$	6,987**	1,347	5,926***	4,506	774	383	-96
	(3,040)	(4,281)	(2,115)	(12,939)	(1,317)	(658)	(241)
	Panel B: Any						
$\widehat{BankAccount}$	0.954**	-0.094	0.702**	0.279	0.633*	0.125	
	(0.365)	(0.392)	(0.276)	(0.215)	(0.361)	(0.340)	
	Panel C: Log						
$\widehat{BankAccount}$	8.997***	7.194***	5.969**	2.857	4.288**	2.415	
	(2.982)	(1.909)	(2.274)	(2.124)	(1.892)	(1.886)	
Household Controls							
Income Decile FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Houshold Size FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sociodemographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-Varying State Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	61,803	51,058	51,912	52,151	52,016	61,803	61,803

This table reports the effect of financial inclusion on debt accumulation using the SIPP (1993–2010). $\widehat{BankAccount}$ is the dummy *Bank Account* instrumented by the Interstate Branching Deregulation Index. Net worth is the sum of all household assets minus total debt. All regressions include state and year fixed effects. Household controls include monthly income decile fixed effects, household size fixed effects, family type, dummies for whether the household receives Social Security income or transfer income, household head education dummies (elementary, high school, college), unemployment dummy, race dummy for household head is black, and age as a cubic polynomial. State time varying controls include total unemployment rate, low-income population unemployment rate, population (log), GDP growth, GDP per capita (log) and a republican dummy. The size of the sample is 20% lower than in our main analysis because there is no information on unsecured debt and mortgage on own house form panel 1992 wave 7 and panel 19934 wave 4. Standard errors are calculated with observations clustered by state and are reported in parentheses. ** and *** indicate statistical significance at 5% and 1% levels, respectively.

Table 8. Financial Inclusion and Financial Strain

*D*ependent variable	**=1 if the household fails to pay for...**					
	Any Basic Expenses			**Food**	**Utilities**	**Rent**
	(1)	(2)	(3)	(4)	(5)	(6)
Bank Account	-0.020***	-0.020***				
	(0.006)	(0.007)				
Bank Account × Layoff		0.000				
		(0.014)				
$\widehat{BankAccount}$			0.682	0.076	0.314	0.470*
			(0.374)	(0.138)	(0.302)	(0.273)
$\widehat{BankAccount}$ × Layoff		0.000	-1.303***	-0.226*	-0.758**	-0.966***
		(0.014)	(0.375)	(0.139)	(0.324)	(0.333)
Layoff	0.129***	0.129***	0.830***	0.152**	0.499***	0.598***
	(0.008)	(0.011)	(0.206)	(0.075)	(0.176)	(0.179)
Household Controls						
Income Decile FE	Yes	Yes	Yes	Yes	Yes	Yes
Houshold Size FE	Yes	Yes	Yes	Yes	Yes	Yes
Sociodemographics	Yes	Yes	Yes	Yes	Yes	Yes
Time-Varying State 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	37,120	37,120	37,120	36,348	37,044	37,120

This table reports the effect of financial inclusion on financial distress. Data come from the SIPP Adult Well-Being supplement (1995–2010). $\widehat{BankAccount}$ is the dummy *Bank Account* instrumented by the Interstate Branching Deregulation Index. $Layoff$ is an indicator for whether anyone in the household has been without a job and looking for work in the year-long period for which the dependent variable is measured. Dependent variables are indicators for whether the household has failed to pay for basic expenses over the last year (Columns (1)–(3)), to pay for utilities (Column (4)) or did not pay in full his rent or mortgages (Column (5)). All regressions include state and year fixed effects. Household controls include monthly income decile fixed effects, household size fixed effects, family type, dummies for whether the household receives Social Security income or transfer income, household head education dummies (elementary, high school, college), unemployment dummy, race, and age as a cubic polynomial. State time varying controls include total unemployment rate, low-income population unemployment rate, population (log), GDP growth, GDP per capita (log) and a republican dummy. Standard errors are clustered by state.