Bank-Branch Supply, Financial Inclusion, and Wealth Accumulation

Claire Célerier
Rotman School of Management, University of Toronto

Adrien Matray
Princeton University

This paper studies how financial inclusion affects wealth accumulation. Exploiting the U.S. interstate branching deregulation between 1994 and 2005, we find that an exogenous expansion of bank branches increases low-income household financial inclusion. We then show that financial inclusion fosters household wealth accumulation. Relative to their unbanked counterparts, banked households accumulate assets in interest-bearing accounts, invest more in durable assets, such as vehicles, have a better access to debt, and have a lower probability of facing financial strain. The results suggest that promoting financial inclusion for low-income populations can improve household wealth accumulation and financial security. (*JEL* G21, I3, I30, R20, R23)

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Close to 40% of the world's population are unbanked; that is, they possess neither a checking nor a savings account. Policy makers and regulators around the world make the expansion of financial inclusion a priority, arguing that access to financial services largely benefits households. The unbanked phenomenon is not specific to developing countries: in a financially developed economy, the United States, 30% of the low-income population do not participate in the financial mainstream and at the same time have very little wealth. If households benefit from financial inclusion, why would some remain

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unbanked in an economy where the supply of financial services is relatively high? Does financial inclusion promote wealth accumulation? If yes, how?

The objective of this paper is to understand whether and how financial inclusion can promote wealth accumulation in an economy with a well-developed financial system like that in the United States. We provide evidence that holding a bank account has a positive effect on wealth accumulation and that this effect occurs through better access to saving devices and debt.

Understanding the effect of participating in the financial mainstream is critical. If low-income households rely on informal or alternative financial services that are as efficient as standard financial services to accumulate wealth, promoting financial inclusion might be ineffective. This will happen for instance if formal financial services charge fees that are so high that they dampen or even cancel out any positive effects of financial inclusion on wealth accumulation, or if credit history and collateral are so indispensable to get access to credit that having a bank account makes little difference. Oppositely, in economies with well-developed financial systems, being unbanked might be particularly costly, as people extensively rely on financial services for any economic transaction—making the use of informal finance less pervasive than in developing countries—and alternative financial services are costly. If unbanked households 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.

Estimating how financial inclusion affects 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 sociodemographic conditions and usage of standard financial services. We address these challenges by using 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 IBBEA on financial inclusion and household wealth accumulation using micro data from the Survey of Income and Program Participation (SIPP). The SIPP's large sample size, which allows us to focus on the subpopulation of low-income households, details information both on households balance sheets and financial well-being and their yearly frequency, making this data set particularly well suited for our analysis. We complement the SIPP data with data from the Bureau of Labor Statistics' Consumer Expenditure Survey (CEX) to study household consumption and investment flows.

Our study is articulated around three parts. We start by establishing that interstate branching deregulation triggered an exogenous increase in both the density of bank branches in low-income counties and in financial inclusion. Adopted in 1994, the IBBEA made bank branching across states legal, but

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gave states the right to erect barriers to the entry of interstate branches. States lifted these barriers 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. Using this index and data from the FDIC to identify the location of bank branches, we first find that the density of bank branches increases by around 20% in poor counties after a state fully deregulates. Turning to the SIPP from 1993 to 2005 to identify low-income households with or without a bank account, we then show in a repeated cross-sectional research design that the interstate branching deregulation increased the likelihood for a low-income household to be financially included by 4 percentage points after a state fully deregulates, which corresponds to a 15% increase in relative terms. This first result suggests that even in a well-developed financial market, low-income households are partly rationed by the supply of banking services.

In the second part of this study, we show that financial inclusion—instrumented by the interstate branching deregulation—has a positive effect on household wealth accumulation and net worth. Households with a bank account have wealth, excluding housing, around $6,900 higher than do unbanked households, which is economically meaningful compared to the average wealth of $1,245 for unbanked households in our sample. This result holds when controlling for a large set of household sociodemographics, including income decile fixed effects interacted with year and state fixed effects, for time-varying state controls, and for time-varying unobserved heterogeneity across regions by including region $\times$ year fixed effects. We also find a similar effect on household net worth, that is, after subtracting both secured and unsecured debt to household assets, suggesting that financial inclusion spurs asset accumulation beyond its possible effect on access to debt.

In the third part of this study, we investigate the channels through which financial inclusion fosters wealth accumulation. First, we find that financial inclusion leads to the accumulation of both liquid assets in households savings accounts, allowing them to earn interest, and durable assets, namely vehicles. Banked households have a 56% higher probability of owning a vehicle, and around $5,900 more dollars in their vehicle than their unbanked counterparts.

Second, we exploit data from the Consumer Expenditure Survey and show that this accumulation of assets partially results from a higher investment in durable goods. Households with a bank account invest more per year in durable assets and the share of total spending in durable assets is significantly higher. This result suggests that a key channel through which financial inclusion affects wealth accumulation is by improving low-income household ability to make investments that require large upfront lump-sum payments. Hence, the composition of goods that households purchase changes, as low-income households now purchase more durable goods relative to nondurable ones, in particular in the form of vehicles rather than houses. In this sense, saving benefits for low-income households happens through a different form than for

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middle or high-income households for which homeownership is the principal asset their wealth is invested in (e.g., Cocco 2005). It also relieves the concern that the positive effect we observe on wealth accumulation only results from the crowding out from informal savings, such as cash in hand and at home.

Third, we show that this higher investment in durable goods partially stems from a better access to both nonhousing debt, in the form of vehicle and credit card debt and home equity withdrawal. While low-income households are not more likely to become homeowners, they are more likely to report positive home equity loans, consistent with recent evidence that households extract equity from their houses (Leth-Petersen 2010; Mian, Rao, and Sufi 2013; Sodini, Van Nieuwerburgh, Vestman, and von Lilienfeld-Toal 2017). Collateralized housing, however, is not the sole explanation for the accumulation of wealth we observe, as low-income renters also accumulate more wealth when they become financially included.

Fourth, and finally, we find that households with a bank account are less likely to face financial strain when they face a negative income shock due to the layoff of one of the household members. While the probability of financial strain—as measured by the failure to pay important bills, such as 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. We also find that households with a bank account are less likely to default on their rent, which implies that financial inclusion might have positive effects beyond the directly affected households. These results suggest that financial inclusion offers households useful tools to manage their personal finance and to have a financial cushion that absorbs negative income shocks.

Overall, we find that marginal returns to financial inclusion among low-income populations affected by supply-side changes are high. These results suggest that unbanked households are constrained by the supply of banking services and can benefit from them.

We develop multiple tests to confirm the validity of our instrument. We first show that the interstate deregulation has no effect on 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 suggests that branching deregulation constitutes a supply shock that does not reflect or induce contemporaneous or expected changes in the demand for banking services. Second, we find that this interstate deregulation has no measurable impact on income and employment for both the whole population and low-income populations at various levels of aggregation, including states, metropolitan statistical areas (MSAs), counties, and households.[1] Third, we show that our

[1] Although this absence of an 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 completely preceded the interstate branching deregulation we consider. 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.

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results prevail when restricting the sample to MSAs that cover different states and controlling for MSA × Year fixed effects. By definition, MSAs regroup adjacent territories with a high degree of social and economic integration. While counties are not identical in a typical MSA, they are likely to face similar trends. Therefore, it is reassuring that households within the same MSA and that face the same economic trends but different bank regulation also react differently to the shock. Finally, in all our specifications, we extensively control for economic conditions with several measures of unemployment, income, and income volatility, both at the household and state levels. That introducing these controls does not affect the coefficient of the deregulation index suggests that deregulation affects financial inclusion only through the increase in supply of banking services and not through a higher income or through better employment conditions.

Our paper contributes to the literature that investigates the effect of financial inclusion on household wealth accumulation and financial well-being. So far, this literature has largely focused on developing economies using randomized control experiments. Ashraf, Karlan, and Yin (2006) and Schaner (2018) 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, Karlan, Robinson, and Ubfal (2018) find a limited effect on wealth accumulation when very poor households are directly *offered* a bank account. In a recent paper, Agarwal, Alok, Ghosh, Ghosh, Piskorski, and Seru (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. We address this question using a natural experiment in an economy with a well-developed financial sector, the United States, where, first, households may decide whether or not to open a bank account, second, the use of informal finance is less pervasive possibly leading to a high private cost of financial exclusion, and third, holding a bank account may also reduce household usage of costly alternative financial services (see Melzer 2011, Melzer 2018 and Carrell and Zinman 2014 on the costs of access to payday loans and Morse 2011 for the opposite view). Our results show that benefits from financial inclusion can be high in countries with developed financial markets and that the main durable assets low-income households invest in is their vehicle.

Our paper also adds to the literature on the determinants of being unbanked. This literature faces the challenge of disentangling demand from supply-side factors (see Barr and Blank 2008 for a broad survey of the literature) and has mostly identified socioeconomic characteristics as the most influential determinants of holding a bank account (Barr 2005; Barr, Dokko, and Feit 2011; Hogarth and O'Donnell 1999; Rhine, Greene, and Toussaint-Comeau 2006). On the demand side, Kearney, Tufano, Guryan, and Hurst (2010) and Cole, Iverson, and Tufano (2018) show that by offering a savings account with

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lottery-like features, banks can motivate the opening of savings accounts. We show that the local presence of bank branches also matters.

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, Cookson, and Heimer forthcoming; Herpfer, Mjos, and Cornelius 2018; Nguyen 2019). There is, however, little evidence on how local finance affects households. Brown, Cookson, and Heimer (forthcoming) find a sizable effect of local financial markets on individuals' financial health when they become adults, Argyle (2017) on the cost of auto loans and Burgess, Pande, and Wong (2005) on borrowing from poor households in India. At the macro level, Burgess and Pande (2005) find that a branch expansion reduced local poverty in India, and Bruhn and Love (2014) that it increased labor market activity in Mexico.[2] Our paper exploits household-level data to show how an increase in branch density fosters the take-up of bank accounts by households, which results in higher household asset accumulation.

Finally, our paper also contributes to the broad literature studying the real effects of banking deregulation with one important difference. So far, the literature has essentially studied the first wave of deregulations that happened in the 70s and 80s and that ended in 1994 (see Kroszner and Strahan 2014 for a detailed survey). The intrastate bank and branching deregulation and the interstate banking deregulations lead to a dramatic increase in financial integration (Landier, Sraer, and Thesmar 2017) and have been found to affect aggregate outcomes, such as growth (Jayaratne and Strahan 1996; Morgan, Rime, and Strahan 2004), income distribution (Beck, Levine, and Levkov 2010; Black and Strahan 2001), firms entry (Black and Strahan 2002; Kerr and Nanda 2009) and innovation and the mobility of skilled workers (Hombert and Matray 2017). The interstate branching deregulation we exploit took place in 1994 and, in contrast, affected the density of bank branches but did not affect income, unemployment and economic growth, or the volume of corporate loans (Favara and Imbs 2015; Rice and Strahan 2010), likely because the financial integration of the market for loans was completed.

1. Branching Deregulation, Branch Density, and Financial Inclusion

In this section, we first describe the nature of the changes to bank branching regulations experienced in the United States since 1994. We then show that the interstate branching deregulations led to an increase in the supply of bank branches in low-income counties and fostered financial inclusion.

1.1 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

[2] Suri and Jack (2016) investigate the effect of mobile banking on local poverty.

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banks (Johnson and Rice 2008). Interstate banking refers to the control by bank holding companies of banks across state lines, whereas interstate branching means that a single bank may operate branches in more than one state without requiring separate capital and corporate structures for each state.

The United States adopted several laws until the late 1950s to make and keep interstate banking and branching prohibited. U.S. states had indeed incentives to restrict competition from out-of-state banks as they were collecting revenues from local banks through taxes and fees to newly chartered banks. The adoption of the McFadden Act in 1927, when some national banks were trying to open branches across states, implicitly prohibited interstate branching by commercial banks. Then, 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.

While interstate banking started expanding on a reciprocal basis from 1978, interstate branching was still not allowed until 1994.[3] In 1994, the adoption of 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, the IBBEA also allowed states to erect barriers to out-of-state bank entry with regard to four dimensions: (1) the minimum age of the targeted bank (5 years, 3 years, or fewer), (2) de novo branching without an explicit agreement by state authorities, (3) the acquisition of individual branches without acquiring the entire bank, and (4) a state-wide deposit cap, that is, the total amount of state-wide 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 between 1994 and 2005. By 2005, we do not observe additional deregulation.

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 multistate banking. As Figure 1 shows, not only has the total number of branches increased since 1994 but also has each local market experienced strong penetration by "out-of-state" branches, which have challenged local incumbents.

[3] 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.

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

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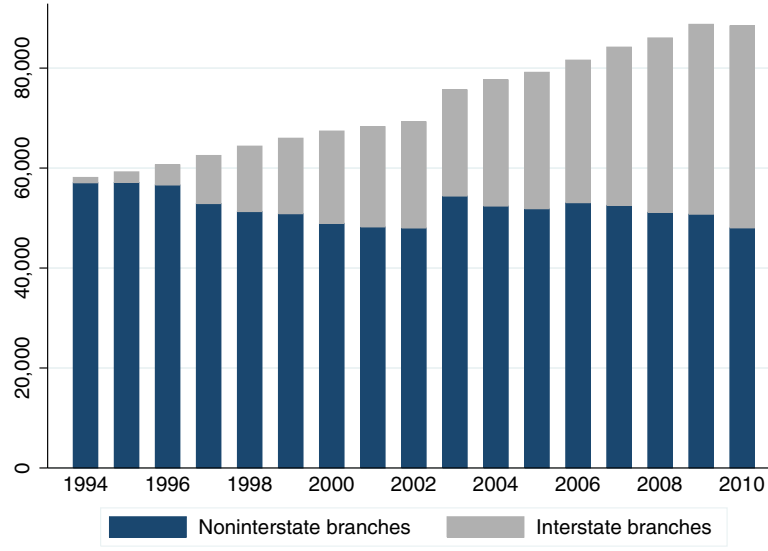

Figure 1
Number of branches operated by FDIC-insured commercial banks
This figure shows the number of interstate and noninterstate branches operating in the United States over the years. Data are from the FDIC.

1.2 Effect on bank branch density

1.2.1 Data. Detailed information on bank branches comes from the Sum of Deposits (SOD) maintained by the Federal Deposit Insurance Fund (FDIC). The FDIC provides annual branch-level data for the universe of bank branches starting from June 1994. For each bank branch, the data set provides information on the location at the zip code level, the amount of deposits, and the identifier of the bank the branch belongs to.

We also collect county-level information on population and poverty rate from the Census Bureau, unemployment from the Bureau of Labor and Statistics, and average personal income from the Bureau of Economic Analysis.

Finally, we split counties into quartiles of poverty rate *within each state* to identify "low-income" counties. We then define as "low-income," counties in the top quartile of the distribution of poverty rate. This methodology ensures that our sample of low-income counties is evenly distributed across states.

1.2.2 Results. Figure 2 first shows that in 1994, at the beginning of our sample period, bank branch density correlates negatively with county poverty. We measure branch density as the number of bank branches scaled by population. In terms 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 rates, therefore, face a lower supply of bank branches at the beginning of our sample period.

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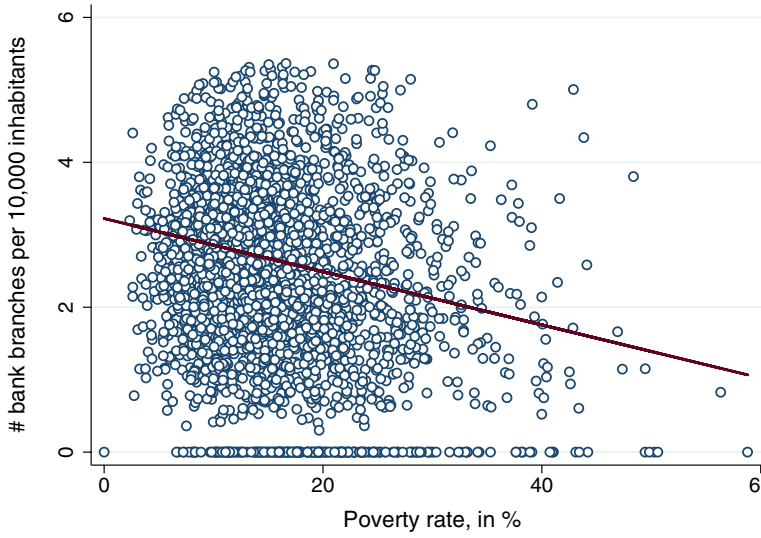

Figure 2
Bank branch density and county characteristics, 1994
This figure shows the correlation between bank branch density at the county level, calculated as the number of branches per 10,000 inhabitants over county poverty rate. The red line is the fitted linear regression. Data are from the FDIC and BEA.

To assess whether the interstate branching deregulation leads to a positive supply shock on branch density, we estimate 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 $BankBranchDensity_{c,t}$ 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, 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 density, we run regressions both on the full sample and on the sample of "low-income" counties over the period 1994–2005, which corresponds to the last year after we observe the last deregulation.

Panel A of Table 2 presents the results: bank branch density significantly increases following deregulation, and the effect is slightly higher in low-income counties (Columns 2 and 4). The coefficient of the $Deregulation$ variable implies that states where all branching restrictions were lifted experienced approximately a ($4 \times 5\% =$) 20% increase in the density of bank branches.

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1.3 Effect on financial inclusion

1.3.1 Measuring financial inclusion: SIPP data. We investigate whether the effect of the interstate branching deregulation on bank branch density translates into a higher household access to bank accounts using data from the SIPP covering the 1993–2005 period.[5] The SIPP is a running panel that collects detailed information about income and demographics for 20,000 to 30,000 households over several waves—8 to 12 waves—during 2 to 3 years. Each wave includes a "core" survey that collects detailed information on household sociodemographics and income, and one or several topical modules that collect information on various topics. One of these topical modules, the Assets and Liabilities topical module, collects information on all assets and liabilities held by each household and their value. Because the Asset and Liabilities topical modules are available in most cases for only one wave, we use the SIPP in a cross-sectional analysis.

We exploit the data from the SIPP Assets and Liabilities topical modules to create a dummy variable *Bank account* 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 then exploit the data from the core survey to build a large set of control variables. These controls include household sociodemographic variables: household size, the number of dependent children, and marital status; household head characteristics: age, gender, education, and employment status; and household economic characteristics: monthly income, social security, and transfer income.

We build our final dataset in the following way. We work at the household level, rather than at the individual level, because households often pool resources, and a bank account in one member's name can provide access to banking services to other members of the same household. We therefore collapse observations at the household and year levels. We then drop households whose head is younger than 20 years old and older than 80 years old. These exceptions account for 5.51% of the initial sample.

The large sample size of the SIPP allows us to focus our main analysis on low-income households. We use the SIPP poverty threshold, which varies with both the number of adults and children in the household and the age of the household head, to restrict the sample to low-income households, that is, households with income below twice the poverty line. This leaves us with a total sample of 107,386 low-income households living in 50 states, plus the District of Columbia over the 1993–2005 period.[6]

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[5] Data are available online: http://www.nber.org/data/survey-of-income-and-program-participation-sipp-data.html. We restrict the sample to 1993–2005, because we do not have information on further deregulation after 2005. However, all our results are robust to including the period 2007–2010.

[6] To ensure the confidentiality of the data, the SIPP aggregates five states in two groups over the 1993–2003 period. The first group of states comprises Maine and Vermont, and the second group comprises North Dakota, South Dakota, and Wyoming.

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Table 1 shows that 31% of low-income households are unbanked in our sample.

Unbanked households are often black, single, unemployed, and transfer income recipients. They are also more likely to be less educated than are their banked counterparts.

1.3.2 Specification. We study the effect of the interstate branching deregulation on the probability of holding a bank account by estimating the following linear probability regression:[7]

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

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 time-varying state characteristics, and $\delta_{t,r}$ and η_s are region $\times$ year and state fixed effects, respectively.[8]

Household controls include sociodemographic variables: marital status, sex, an urban area dummy, and a household head level of education (elementary, high school, college). Household controls also include economic variables: household head employment status and dummies indicating whether the household received any social security income or social transfer income. To control in a nonparametric way for household income, age, size, and number of children, we include a set of dummies for income deciles and categories of household size (1, 2, 3, 4, and 5 or more), the number of children (1, 2, 3, 4, and 5 or more), and eight age categories. We interact income deciles fixed effects with year and state fixed effects. This nonparametric estimation allows us to better control for household economic conditions that could drive households' demand for a bank account and wealth accumulation.

Time-varying state controls include state-level gross domestic product (GDP) growth, log of GDP per capita, total and low-income population unemployment, low-income average wage and a log of the total population. Finally, standard errors are clustered at the state level to account for serial correlation within states.[9] Table A.1 in the Online Appendix reports the coefficients of the control variables.

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[7] Although our dependent variable is binary, the use of a nonlinear model, such as a probit or a logit model, is not suitable given the numerous fixed effects we are using. In addition, Angrist and Pischke (2009) argue that once raw coefficients from nonlinear estimators are converted to marginal effects, they offer only small or no efficiency or precision gains over linear specifications. The other main advantage of linear probability models is that the coefficient can be directly interpreted in terms of percentage points. Therefore, following Angrist and Pischke (2009), we use a linear probability model. However, our results still hold in logit regressions.

[8] We are constrained to use state-level controls and not county-level controls, because the SIPP only provides households' location at the state level, not at the county level.

[9] Clustering standard errors by state and by year makes little difference, presumably because the specification in Equation (2) includes year fixed effects.

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Table 1
Summary statistics

	Unbanked households				Banked households				Total sample
	Mean	*p10*	*p50*	*p90*	*Mean*	*p10*	*p50*	*p90*	*Mean*
SIPP Core Survey (1993–2005)									
Sociodemographics									
Age (year)	46	26	43	71	50	27	48	75	48
High school education (%)	39	0	0	100	38	0	0	100	38
College education (%)	24	0	0	100	42	0	0	100	35
Elementary education (%)	37	0	0	100	21	0	0	100	27
Black (%)	31	0	0	100	13	0	0	100	20
Married couple (%)	32	0	0	100	44	0	0	100	39
Single female-headed (%)	61	0	100	100	55	0	100	100	57
Household size	2.7	1	2	5	2.6	1	2	5	2.6
Number of children	1.0	0	1	3	1.0	0	0	3	1.0
Economic conditions									
Monthly household income (in $)	1,094	250	948	2,121	1,379	489	1,241	2,530	1,267
Recipients of social security (%)	38	0	0	100	41	0	0	100	40
Recipients of transfer income (%)	49	0	0	100	24	0	0	100	34
Recipients food stamps (%)	36	0	0	100	13	0	0	100	22
Unemployed head of household (%)	9	0	0	0	5	0	0	0	7
Homeowner (%)	18	0	0	100	43	0	0	100	32
Observations	*42,057*				*65,329*				*107,386*
	(39%)				*(61%)*				
SIPP Assets and Liabilities Topical Module (1993–2005)									
Wealth									
Wealth exc. housing, amount ($)	1,245	0	0	4,135	5,894	33	3,219	16,570	3,886
Wealth exc. housing >0 (%)	46	0	0	100	93	100	100	100	73
Net worth exc. housing, amount ($)	637	−1,430	0	3,598	4,284	−2,892	1,913	15,599	2,709
Net worth >0 (%)	40	0	0	100	75	0	100	100	60
Assets									
Total assets exc. housing ($)	1,830	0	441	5,804	7,314	249	4,593	18,242	4,945
Vehicle assets ($)	1,818	0	441	5,786	4,460	0	2,682	12,700	3,319
Vehicle assets >0 (%)	50	0	100	100	79	0	100	100	66
Savings account assets ($)	0	0	0	0	2,548	0	215	11,775	1,448
Checking account assets ($)	0	0	0	0	171	0	0	753	97
Liabilities									
Total debt ($)	1,230	0	0	4,768	3,037	0	486	9,831	2,256
Vehicle loans, amount ($)	622	0	0	1,276	1,426	0	0	7,489	1,079
Vehicle debt >0 (%)	12	0	0	100	23	0	0	100	19
Credit card debt and bills, amount ($)	334	0	0	678	1,033	0	0	4,558	731
Credit card debt >0 (%)	15	0	0	100	42	0	0	100	30
Observations	*27,533*				*36,219*				*63,752*
	(43%)				*(57%)*				
SIPP Adult Well-Being Topical Module (1992–2005)									
Any financial hardship prior 12 months (%)	45	0	0	100	33	0	0	100	38
Any rent delinquency prior 12 months (%)	15	0	0	100	9	0	0	0	12
Layoff within HH in prior 12 months (%)	15	0	0	100	10	0	0	0	12
Observations	*12,657*				*18,052*				*30,709*
	(31%)				*(69%)*				
Consumer Expenditure Survey (1996–2006)									
Yearly investment in durable goods									
Amount ($)	2,205	0	296	6,804	4,085	10	1,547	12,850	3,509
Fraction with spending >0 (%)	75	0	100	100	91	100	100	100	86
Share of total consumption (%)	11	0	2	34	15	0	7	42	14
Yearly Investment in vehicles									
Amount ($)	1,742	0	7	5,946	3,034	0	369	10,591	2,638
Fraction with spending >0 (%)	54	0	100	100	75	0	100	100	68
Yearly inv. home improvement and repair									
Amount ($)	400	0	44	1,270	832	0	340	2,757	699
Fraction with spending >0 (%)	59	0	100	100	81	0	100	100	74
Observations	*6,584*				*14,894*				*21,468*
	(31%)				*(69%)*				

This table reports summary statistics on all the dependent and independent variables used in each household-level specification. Banked households hold a checking or a savings account. All nominal variables are deflated using the CPI in 2000.

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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 η_s capture time-invariant determinants of access to banking services in each U.S. state, such as the population density, the initial structure of the local banking market and the level of education, as well as ex ante heterogeneity in access to financial services. Region × Year fixed effects remove every unobserved time-varying heterogeneity across Census regions, such as differences in regional business cycles, different regional trends in access to financial services[10] or aggregate shocks that could stem from changes in federal regulations of the banking sector. 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 *within the same region*, and hence do not exploit differences across regions.

1.3.3 Results. Table 3 reports four versions of our baseline regression that all indicate a large, positive and robust impact of interstate branching deregulation on the probability to be financially included.

The first column only includes State and Region × Year fixed effects. The inclusion of Region × Year fixed effects ensures that we compare households across states in the *same* Census region. The coefficient on *Deregulation index* is equal to 0.010, significant at the 1% level, implying that when a state fully deregulates, the probability for a household to hold a bank account increases by 4 percentage points.

In Columns 2 and 3, we introduce progressively household and state level controls that may directly affect the demand for banking services and find that the effect of the interstate branching deregulation on household financial inclusion remains stable, which suggests that the deregulation is not correlated with other household- or state-level characteristics that may affect the decision to open a bank account. In Column 2, we report the effect of the interstate branching deregulation when we add household sociodemographic indicators for marital status, sex, urban area, educational attainment, household size, number of children, and age. Column 3 includes our large set of controls for household income and economic condition as well as state time-varying controls. In particular, we control in a nonparametric and time-varying way for household income by interacting household deciles of income with state fixed effects and year fixed effects. The inclusion of this large set of fixed effects implies that we compare households across states but in the same income decile-year. In this case, each step of deregulation increases household likelihood to hold a bank account by 0.012 percentage points relative to a household with

[10] For instance, FDIC (2009, 2011) document regional heterogeneity in access to banking services.

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the same sociodemographic characteristics and in the same income decile but living in a state that has not deregulated.[11]

Column 4 restricts the sample to households for which information on their MSA is available. MSAs are integrated economic entities often considered as representing "local labor markets." We include MSA × year fixed effects to control for any time-varying unobserved heterogeneity across MSAs. In this case the effect of the deregulation is identified on households living in the same MSA (i.e., the same local labor market), but on different sides of a state border. We find that within the same MSA, households in a state that has deregulated are more likely to hold a bank account relative to households in the same MSA but on the other side of the border. This specification therefore suggests that controlling for a fine level of local economic activity does not affect our results.

Columns 5 and 6 reproduce the main results on the sample of households with nonimpute information on asset holdings that we will use in our analysis on wealth accumulation. Restricting to this "*IV Analysis*" sample produces virtually the same point estimates.

By combining the results in Tables 2 and 3, we compute an overall back-of-the-envelope elasticity of bank accounts to bank branch density in a county of roughly 40% over the sample period.[12] In other words, when the density of bank branches increase by 10%, the share of low-income households that are financially included increases by around 4%.

Finally, Figure 3 shows the dynamics of the share of banked households around interstate branching deregulation by plotting the coefficient of dummy variables for each year around *each* possible deregulation estimated in model 2. The probability of holding a bank account is higher after deregulation and does not a display discernible pattern before the deregulation date, suggesting the parallel pre-trend assumption is verified.

1.3.4 Discussion. Our result that the probability to be unbanked decreases when the supply of banking services increases suggests that low-income households are unbanked partly because they face barriers to financial services and not solely because they choose to remain outside the financial mainstream irrespective of the supply of banking services.

Demand might drive the unbanked phenomenon if households do not perceive any benefit from holding a bank account because of distrust of financial institutions (Rhine, Greene, and Toussaint-Comeau 2006), social or religious norms (Karlan, Savonitto, Thuysbaert, and Udry 2017), or because of perceived

[11] Column 1 of Table A.21 of the Online Appendix also reports that the point estimate is exactly similar when we control for household income volatility, because income volatility is particularly high among low-income populations (Hannagan and Morduch 2015). We measure income volatility within each survey across all SIPP waves for all households that are included in several waves.

[12] Branch density increases by 20% (0.05×4) after a state fully deregulates, whereas bank accounts increase by 8% (0.048×0.61), with 0.61 being the initial share of bank accounts among low-income households and hence leading to a total elasticity of 8/20 = 0.40.

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Table 2
Interstate branching deregulation and bank branch coverage

	log(branch density per inhabitants)		log(branch density per square miles)	
Counties:	All	Low income	All	Low income
Mean bank branch density:	0.26	0.24	0.04	0.05
	(1)	(2)	(3)	(4)
	A. Commercial banks			
Deregulation index	0.052***	0.055***	0.053***	0.061***
	(0.016)	(0.015)	(0.016)	(0.016)
County-year controls	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes
Observations	34,441	8,167	34,441	8,167
	B. Credit unions			
Deregulation index	0.004	0.002	0.002	0.000
	(0.005)	(0.005)	(0.004)	(0.005)
County-year controls	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes
Observations	18,162	3,703	18,162	3,703

Panels A and B report OLS regressions of the Interstate Deregulation index on banks and credit unions' density, 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 mile in Columns 3 and 4. The deregulation index ranges from 0 to 4, where 0 is the least and 4 the 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 within each state at the beginning of our sample period (in 1994). All regressions include county and year fixed effects. Time-varying county controls include controls for the log and the delta log of per capita income, the log of population, the poverty rate, and the unemployment rate. Standard errors are clustered by state. ** p <.05; *** p <.01.

limited returns from financial inclusion. For instance, according to FDIC (2011, 2013) around 30% of unbanked households do not have an account because they do not trust banks, and more than half because they do not believe they have enough money to benefit from it.

While in economies with well-developed financial markets, demand can be an important determinant of why households would forgo participation in the financial mainstream, our result shows that households are also partly constrained by supply. Households might mostly face two barriers to financial inclusion that the interstate deregulation might relieve: affordability and physical access (Beck, Demirgüç-Kunt, and Peria 2008). First, standard financial products might embed features that are too costly for low-income households, such as minimum account balances and high maintaining and service fees (Barr and Blank 2008; Hogarth, Anguelov, and Lee 2005; Washington 2006). The change in the degree of competition among banks resulting from the deregulation might improve efficiency, subsequently leading banks to lower fees and/or increased interest rates on bank accounts.[13] Second, household distance to bank branches is higher in low-income neighborhoods (Caskey 1994; Goodstein and Rhine 2017), leading to high search and

[13] Drechsler, Savov, and Schnabl (2017) find that bank interest expenses decrease with bank market power.

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Table 3
Interstate branching deregulation and financial inclusion

	=1 if the household holds a bank account					
Sample	*All*				*IV analysis*	
	(1)	(2)	(3)	(4)	(5)	(6)
Deregulation index	0.010*** (0.004)	0.011*** (0.003)	0.012*** (0.003)	0.010** (0.005)	0.014*** (0.004)	0.016*** (0.004)
Household controls						
Income decile ×State FEs	–	–	Yes	Yes	Yes	Yes
Income decile ×Year FEs	–	–	Yes	Yes	Yes	Yes
Income decile ×MSA FEs	–	–	–	Yes	–	Yes
Sociodemographics	–	Yes	Yes	Yes	Yes	Yes
Time-varying state controls	–	–	Yes	Yes	Yes	Yes
Region × Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	–	–	–	–	–
MSA × Year fixed effects	–	–	–	Yes	–	Yes
Observations	107,386	107,386	107,383	40,384	63,745	24,796
R^2	0.034	0.125	0.193	0.258	0.223	0.310
F-statistic	6.0	9.9	11.5	4.4	11.6	16.0

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 1993–2005). The deregulation index ranges from 0 to 4, where 0 is the least and the 4 most deregulated. Column 1 does not include any control, and Columns 2 to 6 include household controls, plus time-varying state controls. All regressions include state fixed effects and year × census region fixed effects, except Columns 4 and 6 that contain MSA × Year fixed effects. Columns 3 to 6 also include household income deciles × state and household income deciles × year fixed effects. Household sociodemographic controls include indicators for marital status, household size and the number of children, the sex of the household head, educational attainment, age, recipient of social security or transfer income, and employment status. Section 1.3.2 describes the variables in detail. 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. In Columns 5 and 6, the sample is restricted to households with nonimputed information on their balance sheet. Standard errors are clustered by state. ** p <.05; *** p <.01.

transaction costs (Argyle 2017), as well as larger information asymmetries that are central in credit allocation (Akerlof 1970; Stiglitz and Weiss 1981). The deregulation, resulting in an increase in bank branch density as we show in Table 2 reduces the physical distance to the bank.

1.4 Endogeneity concerns

Our results so far imply that the interstate branching deregulation has been a positive shock on low-income household propensity to hold a bank account, suggesting a possible instrument to investigate the effect of financial inclusion on household wealth accumulation.

The validity of this empirical strategy relies on deregulation being an exogenous trigger of financial inclusion (the relevance condition) with no direct effect on household wealth (the exclusion restriction condition), which potentially raises two concerns.

First, the timing of the deregulations might be driven by factors that could also determine, or be correlated with, financial inclusion. This would happen, for instance, if states adopt deregulation as a response to low-income households' low access to banking services. Second, interstate branching deregulations might directly improve low-income household economic conditions, which

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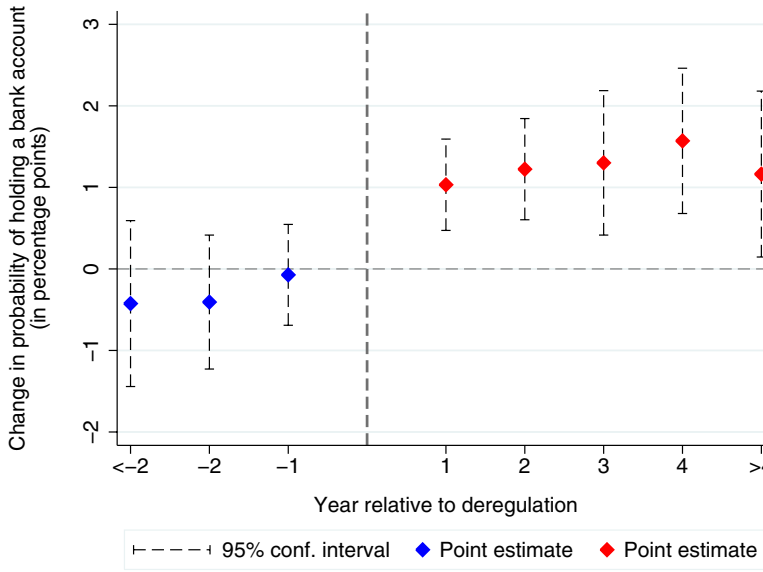

Figure 3
Impact of banking deregulation on the share of banked households
This figure shows the relative change in the odd ratios of holding a bank account around deregulation dates among low-income households over the period 1993–2005. The specification is the same as that in Equation (2), except that 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 1 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.

subsequently would simultaneously affect their demand for financial services and their propensity to accumulate wealth. We extensively address each of these concerns to assess the empirical validity of our strategy.

1.4.1 Motives for deregulation. We start by investigating the timing of the interstate branching deregulation following the method of Kroszner and Strahan (1999). One might be concerned that states have more incentives to deregulate *when the share of banked households is low*, leading to a mechanical increase in financial inclusion following deregulation. Alternatively, states might 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 also translate into a subsequent increase in both the share of banked households after deregulation and household wealth.

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. Both the share of unbanked households before the deregulation and the log of personal income of low-income households have no effect on the timing of the deregulation (Column 1).

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Table 4
Interstate branching deregulation and economic conditions

	Do economic conditions drive deregulation?			Does deregulation affect economic conditions?			
				State level (CPS - low-income)		Household level (SIPP)	
	Time to deregulation (Weibull Model)			Income	Unemployment	Income	Unemployment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Unbanked share (%)	0.422	0.509	0.740				
	(1.465)	(1.700)	(1.899)				
Low-income personal income	−0.556	−0.554	0.712				
	(1.788)	(1.788)	(1.823)				
Low-income unemployment (%)		0.009	0.056				
		(0.083)	(0.088)				
Unemployment (%)			−0.002				
			(0.002)				
GDP per capita			−1.986				
			(1.350)				
Deregulation index				0.003	−0.000	0.004	−0.002
				(0.003)	(0.002)	(0.003)	(0.001)
Household controls							
Income decile×State FEs	–	–	–	–	–	–	–
Sociodemographics	–	–	–	–	–	Yes	Yes
log(state population)	–	–	–	Yes	Yes	–	–
Region × Year fixed effects	–	–	–	–	–	Yes	Yes
Observations	401	401	401	714	714	107,386	107,386

Columns 1–3 test whether different state-level variables can predict the timing of the adoption of the deregulation using a hazard rate model. Columns 4 and 5 report the OLS regression of the effect of the Interstate Branching Deregulation index (*Deregulation index*) on log income and unemployment rate of low-income populations at the state-year level over the period 1993–2005. 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 1 if the household head is unemployed (Column 6). Household controls include household size fixed effects, family type, household head education dummies (elementary, high school, and college), and age dummies. Standard errors are clustered by state.

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The same is true for the state level unemployment rate of low-income households (Column 2) and state GDP per capita and total unemployment rate (Column 3).[14]

1.4.2 Ruling out the income channel. Next, we investigate whether interstate branching deregulation has a positive effect on economic activity, and hence low-income households' economic conditions, in two separate analyses. First, we look at whether the deregulation is associated with a change in the density of a group of institutions that were not directly concerned by the change in regulation. Second, we test directly whether interstate branching deregulation has any real effects on the whole economy at the state, MSA and county levels, and on low-income population economic conditions.

1.4.2.1 Legally unaffected lenders. Credit unions were not concerned by the adoption of the IBBEA and therefore form a "placebo" sample of financial institutions. This placebo group allows us to test whether changes in economic conditions—rather than deregulation only—drive the expansion in bank branch density. If this were the case, we would expect credit unions to expand as well following the deregulation. Data on credit union location and deposit holdings come from the National Credit Union Administration. The data provide 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. Therefore, the differential response between bank branches and credit unions branches suggests that the observed expansion in bank branches is not due to better economic conditions, expected or not.

1.4.2.2 Aggregate effects. To further rule out that the interstate deregulation affected economic conditions, we look at the effect of the deregulation on different measures of prosperity at the state, MSA, county and individual levels using data from the Current Population Survey (CPS), the Bureau of Economic Analysis (BEA) and the SIPP in a difference-in-differences setting similar to Equation (1).

---

[14] We test whether the share of banked households or the macroeconomic 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:

$$h(t, X_t, \beta) = h_0(t)\exp[X_t'\beta],$$

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. We use the first year of deregulation across all the states present at the beginning of the sample and keep state deregulation step pairs even when the state has still not deregulated in 2005 (the last year we can observe a deregulation), in which case the duration is right censored. For each state, that we have one observation for each year, up to and including the year of deregulation, gives us a total of 401 observations. We then progressively introduce different time-varying characteristics for each state to see whether they can predict the timing of the deregulation.

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

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Columns 4 and 5 of Table 4 show that the deregulation has an insignificant effect on income per capita (Column 4) and unemployment rate (Column 5) at the state level. The Online Appendix provides an additional battery of tests: Table A.2 shows a similar absence of effect when we focus on poor or low-income in the state, and Tables A.3 and A.4 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 effects and effects on income risk, Table A.5 in the Online Appendix documents the absence of effect on income 3 years after, as well as on income volatility.

The absence of aggregate effects might still mask distributional effects, whereby poor and low-income households are still positively affected. Table A.6 in the Online Appendix estimates the Beck, Levine, and Levkov (2010) main equation and shows that the interstate deregulation essentially has no effect on the Gini index and the Theil index in treated states and on the income of households at each point of the bottom income distribution.[16]

1.4.2.3 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 household level—rather than at the aggregate level—and specifically for the subsample of low-income households we are interested in. Column 6 of Table 4 shows that interstate branching deregulation does not affect the log of household income, and Column 7 shows no effect on the likelihood for the household to be unemployed.

1.4.2.4 MSA × year fixed effects Columns 4 and 6 of Table 3 find that the effect of deregulation still holds when we include MSA × year fixed effects. This specification tightly controls for local economic conditions as the effect of the deregulation is identified on households living in the same MSA (i.e., same local labor market), but on different sides of a state border. Within the same local labor market, we therefore show that households living in a state that deregulates are more likely to hold a bank account than households in the same MSA but that live on the other side of the border. This specification alleviates the concern that the positive effect of the interstate deregulation on financial inclusion reflects the effect of the deregulation on household economic conditions that would then simultaneously drive their financial wealth and their demand for 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.

[16] The coefficient of the deregulation index on the Gini index is significant at the 10% level but positive and very close to 0.

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It has more trouble still explaining a differential response between households within the same local labor market (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 local: 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.

Overall, these findings may seem surprising in light of the literature showing that *intrastate* branching and interstate banking deregulations affected directly the real economy (Cetorelli and Strahan 2006; Jayaratne and Strahan 1996; Kerr and Nanda 2009; Morgan, Rime, and Strahan 2004) and the bottom part of the income distribution (Beck, Levine, and Levkov 2010). However, the deregulation episodes we consider in this paper have little connection with those that were documented to have real effects. The index of restrictions used here starts after 1994, once both the intrastate branching and interstate banking deregulations documented as having direct real effects were completed. This lack of macroeconomic effect of the interstate branching deregulation on macroeconomic conditions is consistent with Favara and Imbs (2015) or Rice and Strahan (2010), who show using the same deregulation that while the increase in banking competition leads to a decrease in interest rates for small firms, the amount that small firms borrow did not change, consistent with the absence of macroeconomic effect.

1.4.3 Ruling out the house-price appreciation channel. One last concern would be that deregulation affects low-income households and their demand for financial services through the appreciation in house prices documented in Favara and Imbs (2015).[17] To address this issue, we first show that the deregulation has no effect on house prices in our SIPP sample in Column 1 of Table A.25 in the Online Appendix. Second, we show that the effect of deregulation on financial inclusion is stronger in MSAs where housing elasticity is relatively high and in rural areas (Columns 2 and 8 of Table A.25 in the Online Appendix), while the effect of deregulation on house prices documented in Favara and Imbs (2015) is mostly concentrated in MSAs where the supply of housing is inelastic. To obtain this result, we use the topography-based measure of housing elasticity at the MSA level introduced by Saiz (2010). We then split households into two groups: households living in areas with a house supply elasticity below 2, and households living in areas with a house supply elasticity above 2, or in rural areas. This result is consistent with the effect of deregulation being stronger on households that are ex ante more likely to be constrained by the low supply of bank branches, that is, households living in areas where density is relatively low.

[17] In our SIPP sample—like in the American Community Survey—30% of low-income households are homeowners.

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1.5 The Community Reinvestment Act and financial inclusion

The increase in financial inclusion we observe following the interstate branching deregulation also might be partially driven by the Community Reinvestment Act (CRA), as banks must comply with certain levels of services to low-income populations and minorities for the regulator to approve an expansion.

This potential confounding effect, however, would not invalidate our strategy to exploit the interstate deregulation as an exogenous trigger of financial inclusion. Indeed, the main assumption for our strategy to be valid is that the branch expansion is not driven by *demand* factors but by a *supply* shock. Whether the supply shock comes solely from the deregulation or from the combination of the deregulation and the CRA, therefore, does not invalidate our identification strategy. However, given the policy implications, assessing the role of the CRA is important.

Enacted in 1977 and amended several times, the CRA aims at fighting "redlining," namely the existence of discriminatory practices against low-income and minority borrowers. Under the CRA, banks are evaluated on specific performances, resulting in the attribution of a grade that ranges from 1 (outstanding) to 4 (Substantial Noncompliance). These ratings might affect the supply of financial services by banks since federal regulators can reject applications for both branch expansions and mergers and acquisitions if the bank ratings are considered too low (Section 804).[18]

While the Congress has amended the CRA several times during our sample period, in 1994, 1995, and 1999, because these amendments took place at the federal level, it affected all states. Therefore, the inclusion of year fixed effects in our difference-in-differences setting should net out the aggregate effect of the CRA and relieve the concern that these CRA amendments are the only cause of our results.[19]

If the CRA *alone* cannot explain our result, its interaction with the Riegle-Neal Act, however, might have affected banks' behavior in low-income counties, as banks have more incentives to comply with CRA requirements in case of expansion or merger and acquisitions (M&As). This could affect our results in both directions. The CRA might attenuate the effect of the deregulation, as out-of-state banks wanting to open a branch or acquire a bank in a different state that has deregulated have to satisfy CRA tests in their assessment areas, where the banks are located. In this case, banks may increase their supply of banking services to low-income households in their home state (the control state) before entering the newly deregulated state (the treated state). On the other hand, banks in the treated state might also have higher incentives to comply, as both targets and acquirers involved in an M&A operation need

[18] When assessing the CRA ratings in the case of a merger, the regulator evaluates the ratings of both parties, that is, the buyer and the target.

[19] Our results are also robust if we restrict the analysis to the years after 2000.

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to have satisfactory CRA rating for the M&A to be approved, or because they might react to the competitive shock by also expanding more.

Does it imply that without the existence of the CRA, the branching deregulation would not have any effects? There are four reasons to think the branching deregulation had an effect on its own. First, banks can improve their CRA ratings when needed without changing their actual business just by purchasing *existing* loans that qualify for the CRA. Indeed, Avery and Brevoort (2015) show that this is likely to be the main margin of adjustments banks use.

Second, we test formally whether banks adjust their behavior following the deregulation by using data on all CRA ratings since 1990. Table A.7 in the Online Appendix reports the results of a diff-in-diff specification where the dependent variable is the probability for a bank to get an outstanding evaluation (Column 1), a "good" evaluation (rating equal to 1 or 2, Column 2) or a rating increase between two evaluations (Column 3). In all cases, we fail to find an effect of the deregulation. This lack of effect suggests that the interstate branching deregulation did not lead banks to change their behaviors to comply more with CRA ratings.

Third, we test whether our results hold when we focus on a subset of the deregulations that only affects the distribution of branches (the right to open de novo branches and the acquisition of individual branches) and not the acquisition of local banks (minimum age of the target institution and statewide deposit cap). Indeed, one of the main reason a bank may have an incentive to provide services to low-income households to comply with the CRA is because they are planning to be involved in a M&A operation that might be denied in case of nonconforming ratings. Table A.10 reproduces our main results on (1) the increase of the likelihood to have a bank account (Column 1) and (2) on the increase in wealth accumulation (Columns 2–7) and find similar results.

Finally, regulators have assessed banks on three performance tests since 1997–1998, a lending test, an investment test, and a service test. However, the lending test is most important for banks' overall CRA rating for at least five reasons: (1) the lending test has the highest weight of the three tests; (2) banks have to score outstanding on their lending test before getting the highest score on the other two tests; (3) banks need to receive a "low satisfactory" on their lending test to receive a satisfactory score overall; (4) small banks (less than $250 million of assets) do not have to comply with the service test; and (5) public attention has focused particularly on residential mortgage lending, in part because information on mortgage lending became public pursuant to the Home Mortgage Disclosure Act (HMDA) (Agarwal, Benmelech, Bergman, and Seru 2012; Dahl 1999).[20] All these reasons explain why studies have found a substantial effect of the CRA mostly on *mortgage lending* to low and moderate income households (Agarwal, Benmelech, Bergman, and Seru 2012;

[20] The service test assesses the distribution and opening of branches in low-income areas.

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Ding and Nakamura 2017; Ringo 2017; Saadi 2016). However, we find a positive effect of the deregulation on the probability to get a bank account and a positive effect on wealth accumulation for households without any debt (see Table A.8 in the Online Appendix). These findings suggest that our results are not solely driven by banks expanding mortgages to low-income households to improve their CRA ratings. Of course, it is still possible that banks adjust their behavior solely on the service tests, but given that services and lending tests are so intricate, we would expect both dimensions to adjust at the same time if banks were just catering to CRA evaluations following the interstate branching deregulation.

Overall, these results suggest that while the CRA might have amplified the supply shock due to the deregulation, which does not invalidate our conclusion, it is unlikely that it can explain most of the effect.

2. Financial Inclusion and Household Wealth

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 present the results.

2.1 Data

The SIPP collects detailed information on all components of household balance sheets—both assets and liabilities—in the wealth inventory realized in the Asset and Liabilities Topical Modules.

We focus on two measures to study household wealth accumulation: total wealth and net worth *excluding housing*. We exclude home equity when investigating wealth accumulation as less than 10% of households in our sample have bought a house during the 1993–2005 period. As such, housing in our sample mostly affects household wealth accumulation through changes in market prices rather than through the decision to buy, and therefore should not be included when investigating active household wealth accumulation. Household wealth includes household total assets, which is the sum of the vehicle assets, liquid assets, and other assets, minus secured debt, that is, vehicle debt in our setting. Liquid assets include assets held in checking and savings accounts, stocks, bonds, mutual funds and money market funds. Other assets include money owed to the interviewee and mortgage held. Household net worth is household wealth, minus unsecured debt, which includes credit cards, bank loans, and store or medical bills.

To build our data set, we follow Gruber and Yelowitz (1999) and drop observations with imputed wealth information because the SIPP imputation methodology has been largely criticized. This leads us to drop roughly 30% of our sample.[21] We also winsorize all asset and liability values at the 5% level.

[21] Results still hold when including the imputed values.

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We think that winsorizing at the 5% level allows to estimate a more accurate average effect of financial inclusion because of the high skewness to the right of the wealth and asset distributions. For instance, the standard deviation is twice larger than the mean when looking at the asset distribution winsorized at the 1% level, with a strong "jump" between the 95th and 99th percentile ($33,415 at the 95th percentile and $87,198 at the 99th percentile). By contrast, the standard deviation is only 33% larger than the sample mean when we winsorize at the 5% and the gap between the 95th and the 99th percentile is only $20,000. Results, which are reported in Section 11 of the Online Appendix, still hold if we winsorize all asset and liability values at the 1% level.

Table 1 shows the summary statistics for the different components of wealth for banked and unbanked households in our sample. On average, low-income households' wealth and net worth excluding housing amount to $3,886 and $2,709, respectively. The first component of low-income household assets is the vehicle, which accounts for 70% of the household assets, and then the savings account, which stands for 25% of household total wealth. While we make no causal statement here on the role of financial inclusion in asset accumulation, a clear correlation emerges: "banked" household wealth and net worth are more than 4 and 6.5 times higher than unbanked households' ones, respectively, despite having a monthly household income that is on average only 1.3 times higher.

2.2 Empirical strategy: Instrumental variable approach

Studying how financial inclusion affects household wealth accumulation raises the issue of endogeneity. Households more prone to investing and saving and that have more wealth are also more likely to be both rich and to own a bank account. To address this issue, we build on the analysis made in Section 1 and use the interstate branching deregulation to instrument our bank account dummy. Columns 5 and 6 in Table 3 indicate that the interstate branching deregulation has a high predictive power for the probability to hold a bank account, with a F-statistic above 10, the threshold for strong instruments (Stock and Yogo 2005).[22]

To analyze how financial inclusion affects households' balance sheets, we estimate the following two-stage least squares regression:

$$Y_{ist} = \alpha + \beta \widehat{BankAccount}_{ist} + \theta X_{ist} + \lambda StateControl_{st} + \delta_{r,t} + \eta_s + \epsilon_{ist}, \quad (3)$$

where Y_{ist} is a household-level outcome variable that measures household wealth, net worth or later debt or investment in durable goods. $\widehat{BankAccount}_{ist}$ is the dummy variable $BankAccount_{ist}$ equal to one if household i in state s

[22] By definition, the exclusion restriction of an instrument cannot be tested. Thus, the different tests performed in Section 1.4 suggest that interstate branching deregulation did not directly affect household economic conditions, which would then affect household wealth accumulation, and hence support the validity of the instrument.

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at time t holds a bank account that we instrument with the deregulation index. X_{ist} is a vector of household characteristics, $StateControl_{st}$ are state time-varying characteristics and $\delta_{r,t}$ and η_s are region $\times$ year and state fixed effects, respectively. Unless otherwise specified in a table, we include exactly the same set of household controls as the one of Equation (2).[23] The large set of state time-varying and household controls holds constant the standard drivers of wealth accumulation and alleviates the concern that our identification strategy might capture a direct effect of the interstate branching deregulation on household economic prosperity. We cluster standard errors at the state level.[24]

The parameter of interest is β, which measures the permanent difference in the outcome variable Y_{ist} between banked and unbanked households. The identification of β relies on comparing Y_{ist} before and after households living in state s have become banked thanks to the deregulation, relative to a control group of households not experiencing a change in regulation. We also test in the Online Appendix the direct effect of deregulation on wealth accumulation in reduced form specifications (Tables A.16 to A.19). Because we include Region $\times$ year fixed effects, the comparison is only made within the group of states belonging to the same census region.

2.3 Results

We start by estimating how financial inclusion affects wealth and net worth accumulation of low-income households. By subtracting secured and total debt from total assets, respectively, these two measures allow us to test whether differences in asset accumulation between banked and unbanked households might be entirely driven by differences in better access to debt, raising the question of possible excess indebtedness, or whether financial inclusion also has an effect over and beyond a better access to debt.

Table 5 reports the effect of financial inclusion on wealth and net worth holdings estimated from Equation (3). For both variables, we first look at the overall effect on the level amount in dollars (Columns 1 and 4). We then investigate the effect at the "extensive margin" as in our sample, low-income households can have zero wealth and net worth, by using a dummy variable for having any positive wealth and net worth (Columns 2 and 5). Finally, we investigate the magnitude in relative terms in specifications with log wealth and net worth (Columns 3 and 6).

Overall, we find that being financially included has sizable causal effects on the wealth and net worth of low-income households. Relative to unbanked households, households that exogenously obtained a bank account because of the deregulation have on average a wealth higher by \$6,914 (Column 1) and are more likely to have any positive amount of wealth by approximately 40%

[23] Household controls are the same as before.

[24] Results are robust when clustering at the state and year levels.

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Table 5
Financial inclusion, wealth accumulation, and net worth

	Wealth			Net worth		
	$ amount	Any	log	$ amount	Any	log
	(1)	(2)	(3)	(4)	(5)	(6)
$\widehat{Bankaccount}$	6,914***	0.43**	5.34***	5,752***	0.15	5.90***
	(1,931)	(0.17)	(1.08)	(2,087)	(0.21)	(1.34)
Household controls						
Income decile × Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Income decile × State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Sociodemographics	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying state controls	Yes	Yes	Yes	Yes	Yes	Yes
Region × Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	63,745	63,745	61,343	63,745	63,745	51,016

This table reports the effect of financial inclusion on wealth and net worth accumulation using the SIPP (1993–2005). $\widehat{BankAccount}$ is the dummy *Bank Account* instrumented by the Interstate Branching Deregulation index. Wealth is defined as total assets minus secured debt. Net worth is defined as total assets minus secured and unsecured debt. All regressions include household income deciles × state and household income deciles × year fixed effects, as well as census region × year fixed effects. Household sociodemographic controls include indicators for marital status, household size and the number of children[CG2], the sex of the household head, educational attainment, age, recipient of social security or transfer income, and employment status. Section 1.3 .2 describes the variables in detail. 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. ** p <.05; *** p <.01.

(Column 2). Turning to the log specification, we find that being financially included translates into wealth holdings that is 5 times larger (Column 3). These effects are significant at the 1%, 5%, and 1% levels. While the effect might seem large, this causal difference in wealth, estimated controlling for households' socioeconomic characteristics, is close to the unconditional mean difference we can observe in the summary statistics.

We then explore the effect of financial inclusion on net worth accumulation. We find similarly that financial inclusion translates into higher net worth holdings (Columns 4–6). Having access to a bank account leads to a higher net worth of $5,752 (Column 4), which represents a relative increase of almost seven times (Column 6). However, banked households are not more likely to have any positive net worth. Overall, these results implies that higher unsecured debt does not account for the whole positive effect of financial inclusion on wealth accumulation that we identified and that low-income households are able to accumulate higher wealth on top of the additional debts they might contract.

We also run the same specification with MSA × year fixed effects in Table A.12 in the Online Appendix and again find that the deregulation has a significant effect on wealth accumulation. One important limitation of this strategy, however, is that it divides the sample size by a factor of 3.[25]

[25] This is coming from the fact that (1) MSAs do not perfectly map the U.S. territory (less than half counties in the United States belong to a MSA), (2) the SIPP does not report the MSA when there are confidentiality issues, and (3) only a subset of MSAs cover several states.

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While the large magnitude of the effect of financial inclusion on household wealth might seem surprising in light of the absence of macroeffects, this might be due to the skewed nature of asset holdings: changes in the wealth of low-income households is unlikely to affect changes in aggregate wealth. In addition, while the IV strategy allows us to causally assess the effect of financial inclusion on wealth accumulation, the magnitude of the IV estimates might be higher than the average effect on the entire population given that the IV assesses a "local treatment effect."[26] In our setting, the instrument estimates the causal effect of being financially included on the sample of households that do respond to the deregulation (i.e., the "compliers" in econometric terms) (Angrist and Pischke 2009). These compliers are likely to be the households that have the more to gain from easier access to mainstream financial services, implying that the effect of owning a bank account on their wealth might be higher than for the average population.

3. Channels of Wealth Accumulation

3.1 Theoretical background

Financial inclusion can promote wealth accumulation in four important ways: (1) by giving households access to saving devices that earn interest; (2) by facilitating the mental allocation of income into savings, leading households to accumulate liquid and permanent assets; (3) by improving access to credit, which might foster household investment in durable goods; and (4) by increasing households ability to absorb negative income shocks, with both some financial cushion and some financial devices, without excessively relying on costly alternative financial services.

First, the accumulation of liquid assets on the savings account will allow households to benefit from compound interests, increasing over time the value of assets saved. By offering higher returns to savings relative to informal means of savings, such as cash savings at home, having access to interest-bearing accounts might in addition foster household incentives to save.

Second, financial inclusion can foster savings if households are less prone to spend money in bank accounts than cash-in-hand (Bertrand, Mullainathan, and Shafir 2006; Dupas and Robinson 2013b). This higher propensity to save liquid assets held in a bank account would result from what Thaler (1990) names "mental accounting," that is, households' propensity to compartmentalize their money for different uses depending on how it is mentally labeled. The access to an account clearly labeled "saving," in addition to the checking account, can hence be particularly valuable as households would tend to mentally label the money on these accounts as money that should not be used immediately for consumption. The progressive accumulation of savings

[26] Section 4 of the Online Appendix reports the ordinary least squares (OLS) results. Estimated effects are indeed lower than with the IV specification.

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can then allow households to finance investments that require large lump-sum payments. This benefit is likely to be higher for homeowners who can extract equity from their house when facing liquidity shocks (Chen, Michaux, and Roussanov forthcoming; Leth-Petersen 2010; Mian, Rao, and Sufi 2013; Sodini, Van Nieuwerburgh, Vestman, and von Lilienfeld-Toal 2017).

Third, financial inclusion might improve household's access to debt by decreasing information frictions between the bank and the banked households. Such a decrease can come from the bank's ability to progressively collect and process information about banked households through the management of their bank accounts. These lower information frictions will result in a higher ability for the bank to both screen loan applicants by establishing their ex ante creditworthiness and to monitor borrowers conditional on the loan being granted (Mester, Nakamura, and Renault 2007; Norden and Weber 2010). This benefit is likely to be higher for homeowners who can extract equity from their house when they face liquidity shocks (Chen, Michaux, and Roussanov forthcoming; Leth-Petersen 2010; Mian, Rao, and Sufi 2013; Sodini, Van Nieuwerburgh, Vestman, and von Lilienfeld-Toal 2017).

Fourth, financial inclusion might allow households to use their savings or to have access to affordable credit to smooth consumption over the life cycle and better absorb systemic or idiosyncratic shocks to which low-income households are particularly exposed to. Hardy and Ziliak (2013) show that income volatility is higher for the 10% poorest than for the richest, and Morduch and Schneider (2017) that a large number of low-income households face episodic poverty within a year, implying that low-income households are particularly at risk of financial hardships. Financial inclusion might also allow households not to rely excessively on costly alternative financial services, such as payday loans or check cashing. FDIC (2009) documents that 54% of unbanked households rely on nonbank money orders, 38% on check cashing and 14% on pawn shops.[27]

3.2 Data

3.2.1 Consumer Expenditure Survey (CEX). We complement information on household assets and liabilities from the SIPP data with data on household expenses from the Interview Survey of the CEX. The CEX is a rotating survey conducted by the Bureau of Labor and Statistics, where households are observed for four quarters.[28] The survey contains around 7,500 distinct households and detailed information about the flows of consumption across several categories.

[27] While having access to payday loans might be better than no access to credit (e.g., Karlan and Zinman 2009; Morse 2011), such loans have typical biweekly interest charge of 15% per $100 borrowed, implying annualized percentage rates of 400% (e.g., Bertrand and Morse 2011; Melzer 2018). The high costs of debt from payday loans may "snowball" over time if low-income households have to refinance this debt. These high costs might directly prevent low-income households from accumulating wealth as they have to reimburse the high interest rate.

[28] 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 subsequent surveys.

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Similar to Aguiar and Hurst (2013), we drop households that report 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.[29] To be consistent with the analysis so far, we restrict the sample to low-income households.

We build three variables to measures the flow of income that is saved in the form of investment: total investment, corresponding to the amount invested in durable goods, amount spent in automobile, and, finally, total amount spent in home equipment and repair. We average these variables over all the interviews for which the household is present. For each outcome variable, we look at the dollar level of spending, a dummy variable equal to one if the household reports any positive investment spending and the log of total amount of investment spending.

Finally, we use the CEX detailed information about sociodemographic characteristics to build the same set of controls as in the rest of the paper, the information on household usage of financial services to identify unbanked households, and the interview unit on vehicles to better track investment in vehicles. The similarity in the richness of the CEX and the SIPP data and the representativeness of the samples make it relevant to use them jointly to study different aspects of household wealth accumulation and consumption (e.g., Gruber and Yelowitz 1999).

3.2.2 SIPP topical module on well-being. To measure the ability for households to smooth consumption, we exploit the Adult Well-being Topical Module from the SIPP, which contains detailed questions about financial hardship and ability for the households to pay for important expenditures (Melzer 2018). The module is available for almost all the waves used so far but covers fewer years relative to the modules on assets and liabilities, that is, only 1995, 1998, 2003, 2005, and 2010.

We create two different dummies to proxy for financial hardship. The first one indicates whether the household has failed to meet any basic essential expenses over the past 12 months, which includes, for instance, food expenses, medical expenses, utility bills, and rents or mortgage payments.

In addition, the SIPP also allows us to measure specifically whether households fail to pay the full amount of the rent or mortgage over the previous 12 months (e.g., Hsu, Matsa, and Melzer 2018). This specific dimension is interesting for two reasons. First, improving household ability to pay their rent or repay their mortgage debt is an important determinant of better access to credit (Hsu, Matsa, and Melzer 2018), which can, in turn, foster asset accumulation. Second, rent delinquency generates spillover effects, as it also affects landlords.

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

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3.3 Asset accumulation

We start by studying the effect of financial inclusion on asset accumulation and its different components using the same specification as Equation (3) to understand what types of assets low-income households accumulate when they become financially included.

Columns 1–3 of Table 6 find that being financially included has a positive and significant effect on asset holdings. Given that assets corresponds to wealth plus secured debt, this result confirms the effect of financial inclusion uncovered in Section 2.3. Economically, the effect is large. Having access to mainstream financial products allows households to hold an additional amount of asset of $9,963 (Column 1), representing an amount around 5 times larger relative to unbanked households (Column 3). Banked households also experience an increase in the odds of having positive asset holdings by 51%.

We then decompose the effect across class of assets to understand better what drives the process of asset accumulation. We decompose total asset into its two main components: "permanent assets," namely vehicles, and "liquid assets," which are split across interest-bearing assets and checking accounts.

Columns 4–6 find that holding a bank account has a consistent positive effect on the value of the car banked households own (+ $5,943, Column 4), the probability to have a car (+56%, Column 5) or the log of the car's value (Column 6). In all cases, the effect is significant at the 1% level.[30]

When turning to the different components of liquid assets, we restrict the estimation to the dollar amount as by definition, unbanked households cannot have wealth on a checking or saving account. While somehow mechanical, we still view these results as informative about the magnitude of the additional amount of dollars banked households can accumulate. They also relieve the concern that newly banked households open a bank account but do not use it and inform us about whether they accumulate savings or simply use a checking account for their day-to-day transactions.

We find that low-income households use their bank account not only as a means of payment when money accumulates in their checking account (+ $321, Column 8) but also as a means to accumulate wealth in interest-bearing accounts, to a larger extent than the amount of money kept in their checking account (+ $3,739, Column 7). This result partially explains how financial inclusion can foster wealth accumulation among low-income households, by allowing them to benefit from the effect of compound interest. The larger effect on assets held in savings accounts than on checking accounts can be explained by the larger incentives to save through this device to benefit from compound interests, as well as by a possible phenomenon of mental accounting. The small effect on assets held in checking accounts is also consistent with Morduch and Schneider (2017), who find that while households save frequently

[30] The SIPP infers the market value of the car using the car model and the purchase year.

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Table 6
Financial inclusion and asset accumulation

	Total assets (excl. house)			Permanent assets (vehicle)			Liquid assets	
							Interest account	Checking account
	$ amount (1)	Any - indicator dummy (2)	log (3)	$ amount (4)	Any - indicator dummy (5)	log (6)	$ amount (7)	$ amount (8)
$\widehat{Bankaccount}$	9,963***	0.51***	5.30***	5,943***	0.56***	5.21***	3,769**	321*
	(2,557)	(0.16)	(0.99)	(2,028)	(0.19)	(1.34)	(1,411)	(167)
Household controls								
Income decile × Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income decile × State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sociodemographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying state controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region × Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	63,745	63,745	63,745	63,745	63,745	63,745	63,745	63,745

This table reports the effect of financial inclusion on asset accumulation using the SIPP (1993–2005). $\widehat{BankAccount}$ is the dummy *Bank Account* instrumented by the Interstate Branching Deregulation index. Nonbank 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 amounts in savings accounts, plus amounts in interest-earning checking account, plus amounts in money market deposit accounts, plus amounts in CDs. All regressions include household income deciles × state and household income deciles × year fixed effects, as well as census region × year fixed effects. Household sociodemographic controls include indicators for household living in urban areas, marital status, household size and the number of children categories, the sex of the household head, educational attainment, age, recipient of social security or transfer income, and employment status of every adult in the household. Section 1.3.2 describes the variables in detail. 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. $^{**}p<.05$; $^{***}p<.01$.

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small amounts on their checking accounts, they largely spend their checking account balance within 6 months, a phenomenon Deaton calls "high-frequency" savings. "High-frequency" savings is crucial for low-income households that face a large income volatility within a year.

3.4 Investment in durable goods

We estimate the effect of financial inclusion on household investment in durable goods by using a specification similar to Equation (3), where we control also for yearly expenditures on nondurable goods and services, and the CEX data.

Table A.20 in the Online Appendix first confirms that the first stage of our IV analysis holds using the CEX data. Remarkably, the effect is of the same magnitude as with the SIPP data, with one step of deregulation leading to a 1.1-percentage-point increase in the probability to get a bank account. The F-statistics is above 11, which is also beyond the threshold for strong instruments from Stock and Yogo (2005).

Table 7 then reports the effect of holding a bank account on total investment in durable goods. The results show that banked households invest more in durable goods than their counterparts without a bank account, both at the "extensive" and "intensive" margins. Column 1 indicates that holding a bank account translates into a higher level of total investment spending in durable goods of $10,400 per year at the household level, significant at the 1% level. This finding is confirmed when we look at the odds of making any positive expenditure. The linear probability estimate shows that banked households are 0.50 percentage points more likely to spend money on investment. In a specification with log expenditures, Column 3 shows also a positive and highly significant effect of holding a bank account on the log of amount invested. Finally, Column 4 reports a higher amount of investment spending as a proportion of total household spending.

We then look at the different components of household investment in durable goods. Column 5 looks at the amount households invest in their vehicle and find that having a bank account translates into a higher level of investment by $8,600 per year, significant at the 1% level. Like for 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 a car.

Finally, Columns 8–10 report the effect on investment in housing. Having a bank account leads to a positive effect on the total amount invested (Column 8) and a high increase in the likelihood to make any positive investment in housing (Column 9).

This finding of a positive effect of financial inclusion on household *flow* of investment in durable goods helps to explain our results on the *stock* of assets. It also alleviates the concern that the observed increase in household wealth resulting from financial inclusion results only from households reallocating money from informal savings to their bank account as such a behavior would

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Table 7
Financial inclusion and household investment expenses: CEX

	Total investment spending				Auto financing and repair			Home equipment and repair		
	\$ amount (1)	Any - indicator dummy (2)	Share (3)	log (4)	\$ amount (5)	Any - indicator dummy (6)	log (7)	\$ amount (8)	Any - indicator dummy (9)	log (10)
$\widehat{Bankaccount}$	10,397***	0.51***	0.37***	7.39***	8,620***	0.94***	8.64***	1,091**	0.95***	6.51***
	(2,323)	(0.17)	(0.08)	(1.40)	(2,099)	(0.24)	(1.82)	(526)	(0.23)	(1.54)
Household ccontrols										
Income decile FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sociodemographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-vary. state controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region × Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	21,478	21,478	21,478	21,478	21,478	21,478	21,478	21,478	21,478	21,478

This table reports the effect of holding a bank account on investment spending using data from the CEX (1996–2006). $\widehat{Bankaccount}$ is the dummy *Bank Account* instrumented with our deregulation index. The measure of spending varies across specifications as indicated in the Panel heading. Household sociodemographic controls include yearly expenditures on nondurable goods and services and indicators for marital status, household size and number of children categories, the sex of the household head, educational attainment, age, recipient of social security or transfer income, employment status and income deciles. Section 1.3.2 describes the variables in detail. 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. ** p <.05; *** p <.01.

Table 8
Financial inclusion and nonhousing debt

	Total nonhousing debt			Vehicle loans			Credit card debt		
	$ amount (1)	Any (2)	log (3)	$ amount (4)	Any (5)	log (6)	$ amount (7)	Any (8)	log (9)
$\widehat{Bankaccount}$	3,751*	0.65***	5.31***	2,588**	0.19	1.89	1,077	0.92***	6.03***
	(1,906)	(0.21)	(1.82)	(1,199)	(0.20)	(1.55)	(765)	(0.27)	(1.84)
Household controls									
Income decile × Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income decile × State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sociodemographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying state controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region × Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	63,745	63,745	63,745	63,745	63,745	63,745	63,745	63,745	63,745

This table reports the effect of financial inclusion on debt accumulation using the SIPP (1993–2005). $\widehat{Bankaccount}$ is the dummy *Bank account* instrumented by the Interstate Branching Deregulation index. All regressions include household income deciles × state and household income deciles × year fixed effects, as well as census region × year fixed effects. Household sociodemographic controls include indicators for household marital status, household size and the number of children, the sex of the household head, educational attainment, age, recipient of social security or transfer income, and employment status. Section 1.3.2 describes the variables in detail. 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 calculated with observations clustered by state and are reported in parentheses. $^{**}p < .05$; $^{***}p < .01$.

imply no change in the allocation of consumption between durable and nondurable goods. [31]

3.5 Access to credit

We now investigate whether improved access to debt and home equity loans can partially explain why financial inclusion leads households to accumulate more wealth.

To do so, we use the various component of household liability available in the SIPP and a specification similar to Equation (3). Table 8 reports results.

Similar to the previous analyses, we report in Columns 1–3 the level of total debt in dollar amount, the probability of holding any debt and the log of debt amount. Relative to unbanked households, we find that banked households have a debt higher by $3,751, are more likely to have any positive debt by 65% and have a debt that is 6 times larger (Columns 1, 2, and 3, respectively). The effect in dollars is of smaller magnitude than the effect of holding a bank account on asset accumulation, consistent with the positive effect we observe of holding a bank account on both household wealth and net worth. This result suggests that, in our sample, while financial inclusion comes with a higher access to debt, it did not lead to household overindebtness and allowed households to accumulate more assets.

[31] This situation might occur in developing countries. For instance, Dupas, Karlan, Robinson, and Ubfal (2018) show that half of the increase in formal savings they observe after promoting bank accounts for rural populations in Chile, Malawi, and Uganda is explained by crowding out from informal savings. We thank an anonymous referee for pointing out this possibility.

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We then study the effect of having a bank account on access to the main components of secured debt, vehicle loans, and unsecured debt, credit cards. We find support for the hypothesis that having access to debt allows households to invest in durable goods and make large upfront payments, as we find that banked households have higher amount of vehicle debts (Column 4). This can partially explain how financial inclusion allows households to accumulate permanent wealth in the form of cars, as we find in Table 6.

We also find that banked households are more likely to hold unsecured debt by 92% (Column 8). Even though the effect on debt level is not significant we also observe a positive point estimate (Column 7) and a significant effect on the log value of debt (Column 9). This positive effect of financial inclusion on credit card debt might have ambiguous effects on asset accumulation. Indeed, poor and less educated households might be more likely to make borrowing mistakes (Campbell 2006), which might lead these households to borrow too much in credit card and making them more likely to fill for bankruptcy, thereby affecting negatively their asset holding (Dick and Lehnert 2010). On the other hand, better access to unsecured credit might also improve household ability to smooth consumption when exposed to income and expense uncertainty.

Another way financial inclusion might improve wealth accumulation is by allowing homeowners to extract equity out of their house through home equity borrowing.[32] Table 9 explores this possibility. While financial inclusion does not increase homeownership (Column 1), we find some evidence that being financially included increases one's probability to report positive home equity loans and the amount of home equity loans (Columns 2 to 4), consistent with the idea that financial inclusion makes it easier for households to use their house as a collateral asset to borrow against.[33]

Columns 5–10 of Table 9 reproduce the effect of financial inclusion on asset accumulation for homeowners and renters separately. Two keys results emerge. First, financial inclusion positively affects asset accumulation for both home owners and renters, implying that equity extraction is not the sole mechanism through which financial inclusion affects asset accumulation.[34] Second, while the point estimates indicate that low-income homeowners accumulate more assets than renters when they become financially included, the difference between the two coefficients is not statistically significant and largely explained by a larger amount of vehicle assets for homeowners.

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[32] For a model of household liquidity management with home equity borrowing, see Chen, Michaux, and Roussanov (forthcoming), and, for household-level evidence of the effects of home equity borrowing on household consumption and savings, see Leth-Petersen (2010), Berger, Guerrieri, Lorenzoni, and Vavra (2017), or Sodini, Van Nieuwerburgh, Vestman, and von Lilienfeld-Toal (2017).

[33] Higher home equity loans also may be driven by an increase in house prices. Table A.25 of the Online Appendix shows that, for our sample of low-income homeowners, deregulation did not increase house value.

[34] This, therefore, relieves the concern that the effects we observe simply results from the crowding out of savings from housing to bank accounts, that is, households borrowing against the value of their house, while their net worth remains unchanged.

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Table 9
Financial inclusion and home equity borrowing

					Asset accumulation					
	Home Ownership	Home equity loans			*Homeowners*			*Renters*		
	Dummy (1)	$ amount (2)	Any - indicator dummy (3)	log (4)	Total assets $ amount (5)	Any vehicle Dummy (6)	Vehicle asset log (7)	Total assets $ amount (8)	Any vehicle Dummy (9)	Vehicle asset log (10)
Deregulation index	0.000 (0.003)									
$\widehat{Bankaccount}$		11,166 (12,248)	0.44*** (0.16)	4.50** (1.81)	6,594** (3,242)	0.55** (0.24)	6.34*** (2.34)	3,014** (1,347)	0.47** (0.23)	3.65** (1.71)
Household controls										
Income decile × Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income decile × State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sociodemographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying state controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region × Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	63,745	63,745	63,745	63,745	25,158	25,158	25,158	38,554	38,554	38,554

This table reports the effects of deregulation on home ownership (Column 1), financial inclusion on home equity borrowing (Columns 2–4) and financial inclusion on asset accumulation for home owners (Columns 5–7), respectively, and renters (Columns 8–10). $\widehat{BankAccount}$ is the dummy *Bank Account* instrumented with our deregulation index. All regressions include household income deciles × state and household income deciles × year fixed effects, as well as census region × year fixed effects. Household sociodemographic controls include indicators for marital status, household size and the number of children categories, the sex of the household head, educational attainment, age, recipient of social security or transfer income, employment status and income deciles. Section 1.3.2 describes the variables in detail. 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. $^{**}p < .05$; $^{***}p < .01$[CG3].

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3.6 Financial inclusion and financial strain

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

3.6.1 Empirical strategy. We address the question of financial inclusion and financial strain focusing on situations where households face negative income shocks because owning a bank account might directly affect household financial strain for some endogenous reasons. We have indeed shown that financial inclusion is associated with both a higher level of consumption of durable goods and higher level of debt, which might mechanically increase household probability to miss payments of debt or bills.

We follow Hsu, Matsa, and Melzer (2018) and proxy negative income shocks by identifying households facing unemployment. Using respondents' employment history, we build a dummy variable *Layoff* that indicates whether anyone in the household has been without a job and looking for work in the year-long period for which the hardship variable is measured. Around 15% 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.

To study whether financial inclusion can mitigate the negative effect of experiencing a negative income shock, we estimate a linear probability model similar to Equation (3) with the variables *Bank Account* and *Bank Account*$\times$*Layoff* instrumented simultaneously with $Deregulation$ and *Deregulation*$\times$*Layoff*.

3.6.2 Results. Table 10 presents the results. We begin by estimating the effect of being laid off on financial hardship excluding interaction terms. As reported in Column 1, we find that households that experienced unemployment during the year have a 13.6 percentage points higher probability of facing financial strain, confirming that layoffs constitute a negative income shock for households.

We then turn to the question of the role of financial inclusion as a mitigating factor. We find that access to a bank account significantly reduces the risk of financial hardship for households facing negative income shock.[35] The interaction between *Layoff* and $\widehat{Bank\,Account}$ instrumented is negative and highly significant (Column 2) implying that among households experiencing a period of unemployment, those who are financially included are relatively less likely to experience financial hardship. Both delinquencies and unemployment

[35] However, as expected, owning a bank account increases one's probability of facing difficulties in paying bills or repaying debt as it increases the level of debt and consumption.

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Table 10
Financial inclusion and financial strain: SIPP

	=1 if the household fails to pay for...							
	Any basic expenses				Rent			
			Including crisis years				Including crisis years	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Layoff	0.136***	0.779**	0.625***	0.417***	0.077***	0.557**	0.524***	0.274**
	(0.009)	(0.312)	(0.183)	(0.141)	(0.010)	(0.259)	(0.167)	(0.117)
$\widehat{Bankaccount}$		0.674**	0.409			0.614*	0.548**	
		(0.334)	(0.320)			(0.312)	(0.258)	
$\widehat{Bankaccount} \times Layoff$		−1.297**	−0.982***	−0.606**		−0.958*	−0.869***	−0.412*
		(0.570)	(0.330)	(0.251)		(0.477)	(0.296)	(0.207)
Household controls								
Income decile × Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income decile × State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sociodemographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying state controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FEs	–	–	–	Yes	–	–	–	Yes
Observations	30,693	30,693	40,239	40,239	30,693	30,693	40,239	40,239

This table reports the effect of financial inclusion on financial distress. Data come from the SIPP Adult Well-Being Supplement (1995–2005). $\widehat{BankAccount}$ 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 1–4) or did not pay in full his rent or mortgages (Columns 5–8). Columns 3 and 4 and 7 and 8 expand the sample to the 2009 and 2010. Household sociodemographic controls include indicators for household living in urban areas, marital status, household size and number of children categories, the sex of the household head, educational attainment, age, recipient of social security or transfer income, and employment status of every adult in the household. Section 1.3.2 describes the variables in detail. 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.

increase during the financial crisis. Therefore, we include the crisis years in Column 3 and find that our result still holds. In a final specification, we control even more flexibly for state economic conditions by including a full set of state-by-year fixed effects (Column 4), thereby removing any time-varying unobserved heterogeneity at the state level. While laid-off households are 41.7% 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 then turn to a more specific measure of financial hardship that the SIPP allows us to measure: the difficulty to pay his rent or reimburse his mortgage as used in Hsu, Matsa, and Melzer (2018). Similar to the "omnibus" measure of financial hardship used previously, we find that financial inclusion reduced the rent and mortgage delinquency of displaced workers (Columns 6–8) and that this effect is also robust to the inclusion of state ×year fixed effects (Column 8).

These results point to one social benefit of financial inclusion, namely the reduction in rent delinquency for landlords, implying that the benefits of financial inclusion are not exclusively private but can also spread beyond the direct beneficiaries.

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4. Conclusion

In this paper, we investigate whether financial inclusion affects wealth accumulation of low-income households.

To do so, we exploit the interstate branching deregulation in the United States after 1994 as an exogenous trigger on the supply of mainstream financial products. We find that following the deregulation, the supply of bank branches increases, and financial inclusion increases. This result is consistent with the hypothesis that supply-side factors contribute to the unbanked phenomenon.

We then instrument financial inclusion with the interstate deregulation and show that financial inclusion positively affects low-income household wealth and net worth accumulation. Turning to the channels behind this increase, we find that having a bank account allows households to accumulate not only liquid assets on their bank accounts, but also permanent assets. This increase in assets can be accounted for by a higher investment in durable goods and a better access to debt.

We finally show that financial inclusion can help low-income households to smooth consumption by reducing the likelihood that they have to cut on essential spending when facing a negative income shock. By reducing household rent delinquency, we show that the benefits of financial inclusion might spread beyond the unbanked households.

Overall, our results suggest that households are partly constrained by the supply of bank services and that improving access to a bank account increases wealth accumulation for low-income households.

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