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## Banking Deregulation and Household Consumption of Durables

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# Banking Deregulation and Household Consumption of Durables\*

## Abstract

We exploit the spatial and temporal variation of the staggered introduction of interstate banking deregulation across the U.S. to study the relationship between credit constraints and consumption of durables. Using the American Housing Survey from 1981 to 1993, we link the timing of these reforms with evidence of a credit expansion and household responses on many margins. We find robust evidence that households are more likely to purchase new appliances and invest in home renovations and modifications after the deregulation. These durable goods allowed households to consume less electricity and spend less time in domestic activities after the reforms.

*Keywords: banking deregulation, credit constraints, energy consumption, durable goods*

*JEL classification: D12, G2, Q41*

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# 1 Introduction

The important role of credit availability in determining spending decisions of households has been well established in the literature. A number of studies have shown that reductions in the supply of credit lead to lower consumption expenditures (Jensen and Johannesen, 2016; Agarwal et al., 2015; Damar, Gropp, and Mordel, 2020), while an increase in the availability of credit raises consumption spending (Gross and Souleles, 2002; Leth-Petersen, 2010; Abdallah and Lastrapes, 2012). One link between credit availability and consumption spending is households using credit to smooth income shocks (see Jappelli and Pistaferri, 2010 for a review). As such, credit constraints can create welfare losses by preventing households from optimally allocating consumption over time (Abdallah and Lastrapes, 2012). Households can also rely on credit for spending, while simultaneously saving part of their income for unexpected spending shocks where credit use is not possible (Telyukova and Wright, 2008).

Furthermore, households could also use credit to undertake activities that will yield future benefits. Sun and Yannelis (2016) find that following an increase credit availability, more individuals take out student loans in pursuit of higher education, which allows for the accumulation of human capital and can lead to higher future income. Their finding aligns with Banerjee and Newman's (1993) argument that improved credit markets provide low-income populations with never-before-feasible opportunities to invest in themselves through education, training, or business entrepreneurship. This link between credit availability and household spending on activities with future benefits is our primary motivation.

In this paper, we empirically investigate whether an exogenous increase of available credit allows households to purchase new, often costly, durable goods (e.g., refrigerators, ovens, and dishwashers) or to make home modifications (e.g., installation of a new roof, storm windows, insulation). Consumer spending on such durables, which provide a stream of services over time, is more discretionary compared to nondurables (e.g., food) and is closely linked to (and to some extent can determine) the business cycle. Purchases of durable goods by households could yield several future benefits. First, replacing older household goods with newer, more energy efficient/conserving versions can decrease a household's energy costs, causing an intensive margin effect on energy consumption. In addition to potentially relaxing households' future budget constraints by reducing energy use and thus lowering utility bills, such a switch to energy-saving products would also have obvious environmental benefits through lower

emissions. On the other hand, increased credit availability could also raise energy consumption through an extensive margin effect, if households opt to purchase new durable goods to achieve tasks that did not previously require energy use (clothes dryer vs. hanging clothes on a line). In addition, any purchase of a new durable good can reduce time spent on chores, i.e. replacing household labor, and provide benefits to the household from a labor-leisure tradeoff standpoint.

Our empirical analysis follows the broad consensus reached by previous studies and exploits the removal of interstate bank branching restrictions in the U.S. during the 1980s and early 1990s to identify exogenous changes to the availability of consumer credit at the household level. Studies such as Dick and Lehnert (2010), Bui and Ume (2020), and Sun and Yannelis (2016) provide empirical evidence of higher consumer credit after deregulation, while Livshits et al. (2016) provide a theoretical motivation for this credit expansion.

We combine data on interstate banking deregulation in each state-year with data from the American Housing Survey (AHS), which is a biennial panel survey providing details on household consumption including recent durable goods purchases and home modifications. In order to establish that banking deregulation is associated with an expansion of credit and to estimate the effect of this expansion on durable good purchases, we employ a difference-in-differences specification that exploits cross-state, cross-year variation in the timing of policy enactment. This allows us to determine if variations in the availability of consumer credit can explain differences in durable good purchases by households for the period 1981 to 1993.

Given that our primary goal is to examine the effect brought about by credit relaxation on the consumption of durables, we first document that banking deregulation did indeed increase the availability of credit.<sup>1</sup> Using self-reported information on mortgage refinancing and new mortgage interest rates available in the AHS, we establish that households living in recently deregulated states were more likely to refinance their mortgages and that the average interest rate on new mortgages declined in recently deregulated states. Combining this evidence with findings from earlier studies, we conclude that banking deregulation increased the availability of consumer credit.

We then move on to our main analysis by investigating the impact of credit availability on durables consumption. Our results show that state-by-state gradual introduction of more lenient

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<sup>1</sup> Favara and Imbs (2015) establish increases in mortgage credit into the mid-1990s and early 2000s, as different states finalized their deregulatory actions.

bank branching regulations led to an increase in a number of durable good purchases and home modifications and renovations. In order to attenuate potential concerns related to other factors simultaneously influencing household spending, we run placebo tests that assign banking deregulation (i.e., the treatment) to years preceding a state relaxing their credit constraints. We find no evidence of prior trends in consumption of home appliances or modifications accounting for these findings.

In order to examine any income-driven heterogeneity in the impact of relaxed credit constraints on households, we also divide our sample into income subgroups and estimate our model including triple differences. Jayarathne and Strahan (1998) argue that the removal of intrastate branching regulations forced significant competition in the market, incentivizing bank employees to create customer loyalty and establish relationships with their clients. Such an overall increase in service proficiency could have reduced the degree of rationing, especially by smaller banks. However, it is possible that higher income households benefitted more from an increase in bank competition, if new entrants were more likely to lend to these households due to their lower perceived risk.<sup>2</sup>

On the other hand, previous research also suggests that U.S. banking deregulation has indirectly benefited low-income groups by providing a relative income boost. For example, these reforms may have lowered interest rates, to which firms responded by hiring unskilled workers (Beck et al, 2010). This would suggest lower income households might drive an increase in durable good purchases as their incomes rise with banking deregulation.<sup>3</sup>

Given the substantial geographical variation in the distribution of income in the U.S. and the earlier findings regarding the effect of banking deregulation on income distribution, we construct income groups at the state level, using annual income from the year before a household's residential state initiated deregulation. We classify households whose annual incomes are within

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<sup>2</sup> As discussed in Ergungor (2010), banks often need to use “soft information” to evaluate low-income borrowers, given their inadequate credit histories. A bank's ability to collect and utilize such information relies on relationships it has in a local market, often through an established branch presence. Therefore, it is reasonable to expect that new entrants, without existing relationships in their new markets, will initially lend to less-informationally opaque borrowers, such as those with higher incomes. There are many other explanations of why lower income households suffer the greatest from credit market imperfections. For example, inefficient banking services may hinder low-income earners from investing in their own human capital (Galor and Zeria, 1993), or disincentivize individuals from borrowing because a loan's fixed cost may be too high relative to their income (Banerjee and Newman, 1993).

<sup>3</sup> Of course, the increase in credit availability might also change how retailers price the durable goods. Bertola et al. (2005) argue that durable good sellers have an incentive to discriminate against groups that rely on credit by charging higher present-value prices.

the top three deciles in their state of residence the year before the deregulation as the “high-income” group. We then interact our high-income group and state-year banking deregulation indicators to estimate a heterogeneous treatment effect across household types. Our results show that all households, regardless of income, have benefited from the deregulation, but the highest-earning households were slightly more likely to increase purchases.

The easing of credit constraints is expected to disproportionately help younger households purchase durable goods as they have a smaller asset base to use as collateral (Alessie et. al., 1997). We test this hypothesis by estimating whether households under 40 increased their propensity to purchase durable goods when banking deregulation occurs. Results show that younger households were more likely to purchase durable goods compared to older households when credit constraints are relaxed, however the differential effect is of smaller magnitude than the main effect.

As discussed above, we assume that banking deregulation enabled households to purchase home goods with high upfront costs (by making credit available to those with low collateral and high fixed costs of borrowing). Thus, a typical household could now purchase new, potentially energy-saving household goods. To understand the impact of purchasing new durable goods on energy use, we estimate the effect of deregulation on energy consumption by focusing on marginal changes in annual electricity and natural gas utility bills. The results suggest that households living in recently deregulated states consume statistically significantly less electricity annually.<sup>4</sup>

Finally, our analysis investigates the possible impact banking deregulation had on household “labor-leisure” decisions caused by the purchase of potentially labor-saving technologies. Bui and Ume (2020) show that banking deregulation lowered hours worked which might suggest an increase in leisure or non-market hours worked. We examine the possibility of banking deregulation having a labor-savings effect by using data from the AHTUS-X database (Fisher, et al. 2018) and looking at patterns of time spent on “unpaid domestic work.” We find evidence of average time spent on non-market hours worked falling faster in states that recently deregulated relative to those that deregulated earlier. These findings support the argument that relaxation of credit constraints, brought on by banking deregulation, led to labor- and energy-savings across households.

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<sup>4</sup> There is evidence of a reduction of natural gas consumption across our sample as well, however these findings are not statistically significant.

The implications of this research include improved understanding of the “Energy Efficiency Gap.” Allcott and Greenstone (2012) define this as when entities do not undertake investments whose discounted lifetime sum of expense is the smallest in favor of goods with lower upfront but higher per period energy costs. One potential explanation for this “gap” is that households are liquidity constrained and unable to pay the higher upfront costs of energy efficient appliances or home renovations. This paper provides the only test we are aware of for the hypothesis that liquidity constraints impact the ability of households to undertake energy efficiency investments. Our results imply that liquidity constraints played a role in limiting the spread of energy efficient durable goods.

A different line of inquiry that this research contributes to is household durable good consumption. Despite the existence of a substantial literature on durables consumption throughout the life cycle, household spending on non-housing/non-automobile durables have received little attention. The few studies look at these goods, such as Browning et al. (2016), do not consider the role of credit constraints in their consumption. Meanwhile, most of the studies that incorporate credit constraints into their analysis of durables spending have primarily focused on housing (Fernandez-Villaverde and Krueger, 2005) or automobiles (Alessie et al., 1997). Our results complement these studies by establishing a link between credit constraints and the consumption of “other durable goods”, such as home appliances. Furthermore, our main findings align with Alessie et al. (1997), who conclude that financial liberalization in the United Kingdom during the early-1980s made it easier for younger households to purchase cars by relaxing their credit constraints.

Finally, there have been recent calls for more green finance or green central banking as way to encourage reductions in risks from climate change (United Nations, 2017; Bank of England, 2017). Climate risks are important for macroeconomic stability thus central banks might encourage lending to projects that reduce emissions. This research shows that households respond to more general increases in lending by undertaking investments that reduce their energy use. Further, this research shows that investments that allow household to adapt to a changing climate may have additional macroeconomic impacts through changes in labor supply (Rudebush, 2019).

The remainder of the paper is organized as follows. Section II provides institutional background on banking deregulation and rationalizes the validity of using this policy reform’s spatial and temporal variation as an exogenous assignment. This section also provides further



evidence on how deregulation influences consumer behaviour and income channels in the market. Section III describes the data and identification strategy. Section IV details our econometric methodology. Section V provides our core results, and Section VI concludes.

## 2. U.S. Banking deregulation, access to credit, and consumption

Beginning in 1927, the U.S. federal government supported the right to prevent interstate banking – the expansion of banking branches across state lines – and regulate intrastate banking – the branching of banks within states. States routinely generated significant revenues by regulating the banking sector through purchasing bank shares or taxing banks. In order for a banking company to enter the market with full compliance, the company had to be granted a bank charter from the specific state it would be conducting business in. States had incentives to provide charters, as they charged fees for each charter. States made no profit from out-of-state branches, and thus had no incentive to allocate business licenses for them to operate in their territory. The Douglas Amendment to the 1956 Bank Holding Company (BHC) Act ended cross-state ownership of banks and branches unless a target bank’s state permitted such acquisitions. Not surprisingly, as no state gained financial benefits for allowing them, all states chose to bar these transactions. In the 1960’s, banks began lobbying Congress to loosen fiscal restrictions put in place after the Great Depression.

In 1975, Maine started the banking deregulation process with legislation permitting out-of-state bank holding companies to buy up existing companies within the state. After this, deregulation of the banking sector began to trend and throughout the 1980s and early 1990s, states relaxed their once strict regulations through legislative acts. With new statutes permitting small bank holding companies to consolidate, the market experienced significant entry and subsidiaries converted into branches (Beck et al., 2010). Passage of the 1994 Riegle-Neal Interstate Banking and Branching Efficiency Act eliminated previous federal restrictions on interstate banking and branching, giving banks the ability to aggregate on a national level.

The unique and individual timing of states eradicating restrictions on bank acquisitions and cross state branching allows us to assess the relationship between interstate deregulation and household durable purchases. As previously mentioned, due to the nature of their design, the staggered introduction of banking deregulation across states acted as a natural experiment –

providing plausibly exogenous variation to credit supply. Increases in the supply of credit following banking deregulation have been well documented in the literature. For example, Sun and Yannelis (2016) show that relaxation of financial constraints gave rise to the average household's access to credit through increased total private loan volume, overall lower banking fees, and decreased mortgage loan interest rates. Dick and Lehnert (2010) also argue that previously excluded households acquired the ability to enter new consumer markets due to branching deregulation. As further confirmation of these findings, we document in the next section that banking deregulation increased credit availability in our sample as well, since households in deregulated states were more likely to refinance their mortgages and new mortgages were given at a lower interest rates.

Once the link between banking deregulation and increased availability of credit is established, the logical next step is to analyze the real effects of such an expansion in the credit supply. Sun and Yannelis (2016) find that a larger percentage of high school graduates were able to access to higher education through greater availability of student loans, while Black and Strahan (2002) argue that relaxed credit constraints led to a more efficient economy through increased entrepreneurship. Similarly, Banerjee and Newman (1993) find evidence of credit market conditions improving after deregulation, with lower barriers for entry for entrepreneurs, which led to increasing capital accumulation. Meanwhile, Beck et al. (2010) reach a more general conclusion that the deregulation led to a tighter distribution of income by boosting income levels of households whose annual earnings were below the national median bracket.

In our case, the link between banking deregulation and real economic activity runs through an increase in home equity-related credit (such as refinancing) and households using these loans for home renovations, including the purchase of new appliances. The AHS does not ask households what they did the proceeds of a new loan (refinance or otherwise); however, other studies utilizing data sources containing such information have found a clear link between home equity-related borrowing and spending on home improvements. One such study is Greenspan and Kennedy (2007), who observe that during the early-1990s (which overlaps with our sample period) households spent approximately 30 cents of each dollar of home equity extraction on home improvement projects. Looking at a period extending from late-1990s to mid-2000s, Cooper (2009) notes that a one-dollar increase in home equity extraction is

associated with 21 cents worth of additional spending on home improvement spending.<sup>5</sup> Accordingly, we argue that simultaneously observing post-deregulation increases in home equity-related credit and home renovation spending points to a “credit constraint channel” even in the absence of an AHS question explicitly linking these two phenomena. Similar to Sun and Yannelis (2016), the credit constraint channel in our study would suggest that as interest rates fall and/or banks become more willing to lend to previously excluded households, borrowers extract more home equity and use this equity to invest in home improvement projects.

### 3. Data

To uncover the link between banking deregulation and consumer durable purchases, we utilize data from two sources: panel data from the American Household Survey (AHS) in collaboration with the date individual states enforced legislative changes that relaxed their credit constraints. This allows us to capture precise variation of state-represented deregulation decisions by year. Our core household-level data for our analysis is longitudinal, permitting us to observe many of the same households over time before and after the policy change.

#### *3.1 American Housing Survey and consumption of durables*

The AHS dataset is a biennial panel housing survey launched in 1973 by the U.S. Census Bureau with funding from the U.S. Department of Housing and Urban Development (HUD). This survey provides information on nationally representative stock of housing, their characteristics, and it is accompanied by a rich set of household- or respondent-level information. The panel nature of the data will allow us to identify the effects of banking deregulation on purchase of durables and equipment while controlling for time-invariant household-level characteristics.

The AHS is well-suited to our analysis in that respondents provide rich micro data information on a variety of purchases of durables and home investments: (i) recent purchases of household appliances (specifically, if a household’s newest refrigerator, dishwasher, oven, laundry

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<sup>5</sup> In case of Cooper (2009), “home improvement” could include projects that we are not interested in such as landscaping. At the same time, however, spending on new kitchen appliances outside of a full kitchen remodeling is not a part of the 21 cents mentioned above. Therefore, it is unclear whether the estimate from Cooper (2009) is above and below the applicable figure for the set of projects in our study.

washer, and/or dryer is less than 5 years old), (ii) recently added energy conservation features (such as whether new storm windows or doors, wall insulation, and/or a new roof were added/installed in the last two years,), (iii) recent installation of major equipment (i.e. if heating equipment, central air conditioning, and/or water heaters were added/replaced in the last two years). The date of the purchase is measured with error, which in a difference-in-differences approach complicates the assignment of the outcome (purchase or installation) to the treatment (i.e., timing of the banking deregulation). We will deal with this issue by adopting an *event-study* approach, including different indicators for the time periods after the banking deregulation.

Our sample consists of households interviewed between 1981 and 1993. As the survey is administered every two years, we observe seven survey years in total. We chose this time frame because the U.S. experienced the largest movement of individual states relaxing their banking regulations. Our final sample excludes any household reporting a move to a new residence between pre- and post-periods. Dropping these households reduces concerns over identification by ensuring we are in fact observing households that experienced the reform in the state where they are interviewed and exclude households who moved in or out the state. We also excluded units whose total household annual income is listed as negative.

The final sample size varies according to the different specifications.<sup>6</sup> However, in Appendix A, Table A1, we present basic descriptive statistics of all variables used in our regressions. Over the period considered, 8% of households in our sample purchased a new dishwasher, a new dryer or a new oven. A slightly greater proportion of households (10% and 11%) acquired a new laundry washer and a new refrigerator, respectively. Our dataset shows that 3% of households insulated their homes with foam, weather stripping and caulking; while a similar proportion of households (4%) installed major equipment such as heating, central AC, dehumidifiers, portable dishwashers, water heaters, and heat pumps. A greater proportion of households replaced all or part of their roofs (8%) or completely exchanged their exterior glass doors and windows with new storm doors and windows made of triple glass for weather protection (6%). A much smaller percentage of household (1.4%) renovated their home.

In some specifications we include some characteristics of the respondent or household. The average age of the respondent is 37.7. About 56% of these individuals are married, and 10%

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<sup>6</sup> For example, when analyzing gas consumption, we restrict further the sample by excluding households that do not utilize natural gas for their main heating source.

graduated from college. The average household size is 2.67 members and their household income is about \$58,500 in 2015 dollars. In every regression, we include the annual average coincidence index for each state as a measure of economic activity.<sup>7</sup> Expenditure patterns may also be driven by changes in electricity and natural gas prices, which were annually set by state public utility commissions during our sample period. We deal with this by including the retail prices of electricity and natural gas at the state-year level, which we obtain from the Energy Information Administration (EIA).

Our analysis measures the potential differential effects of deregulation across household income groups. We construct an indicator for high-income group households within each state before the banking deregulation to see whether purchase of goods and equipment differ over the income distribution.<sup>8</sup> These income categories were generated by observing a household's annual income levels reported in the survey year *prior* to banking deregulation. Research has shown that banking deregulation boosted particular incomes at certain distribution levels (Beck et al., 2010). Our classification of top-earning households consists of those earners in the 8<sup>th</sup>, 9<sup>th</sup>, and 10<sup>th</sup> decile groups within the state in the year before the deregulation.

Finally, the AHS collects information on energy expenditure, which we will use to check whether new durables, instalments and equipment drive energy use up or down.

### *3.2 Linking banking deregulation to household responses*

Consistent with Amel (1993), Kroszner and Strahan (1999), and Demyanyk, Ostergaard, and Sorensen (2006), we chose the date of banking deregulation for each state as the date on which restrictions were lifted on interstate banking by allowing bank holding companies to expand across state borders. Table A2 in the Appendix presents deregulation dates associated with the states included in our sample. The AHS excludes twelve states from its nationally representative dataset. Due to this lack of data, we are unable to include any observations for

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<sup>7</sup> This index is calculated monthly by the Federal Reserve Bank of Philadelphia. Four state-level economic indicators are used to generate this statistic each month: nonfarm payroll employment, the average hours worked by manufacturing employees, the unemployment rate, and wages/salary disbursements that have been deflated by the consumer price index (CPI), benchmarked by U.S. city averages during that month.

<sup>8</sup> Household annual earnings depend on that area's general cost of living and preexisting state-specific conditions. According to the National Center for Education Statistics, the median annual household income across the entirety of the U.S. was \$50,200 in the year 1990. When narrowing our observations to state-by-state comparisons, however, we see significant variation in the distribution of median income across geographical areas. For example, in 1990, residents of the state of New Jersey had a much higher median income than that of the U.S. (\$68,256 vs. \$50,200). In retrospect, households in Alabama only had a median income of \$39,412. It is because of this wide dispersion of median incomes across individual states that we construct our income variable as state-specific.

these states. Fortunately, most of these states are predominantly rural, contain low populations, and, for the majority, relaxed credit constraints very late relative to other states. Thus, these states would have been assigned into treatment late in our data’s timeline, making it difficult, if not impossible, to observe variation in household appliance purchases in the post-treatment period. After accounting for these omitted areas, our sample is left with 38 states in total.<sup>9</sup>

We include households with positive annual income, while excluding those with missing information on key variables (such as respondents who consistently refused to answer questions pertaining durable good purchases, fuel usage, etc.), and households that moved to a new residence (as discussed above). By excluding households that moved within the previous 12 months, we eliminate the confounding effect of new home purchases. This ensures that we are in fact observing banking deregulation’s effect on household purchases of durable goods and energy conservation features rather than new home purchases.

## 4 Empirical Methods

We adopt a difference-in-differences approach with household fixed effects to estimate the consumption effect of relaxed credit constraints. We run several models of the following form:

$$Y_{hst} = B_1 \text{Post deregulation}_{st} + B_2 X_{st} + B_3 Z_{ht} + \alpha_h + \rho_t + \varepsilon_{hst} \quad (1),$$

where  $Y_{hst}$  represents different outcomes for household  $h$  in state  $s$ , at year  $t$ . Every specification accounts for household fixed effects  $\alpha_h$  and survey year fixed effects  $\rho_t$ . *Post deregulation* is a dummy variable that equals one in the years after state  $s$  deregulates and equals zero otherwise. The coefficient attached to this variable identifies the impact of banking deregulation on our outcomes. Tables of results report standard errors clustered at the state-level. Results are identical when clustering at municipal area level.

Our first analysis seeks to confirm the related literature and test whether banking deregulation indeed affected credit availability. We run two separate regressions in which  $Y_{hst}$  is whether

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<sup>9</sup> The excluded states are: Alaska, Delaware, Idaho, Maine, Montana, Nebraska, New Hampshire, North Dakota, South Dakota, and Vermont. Households from all these states, except Alaska and Maine, would have been assigned into treatment very late in our timeline in the first place. Meanwhile, Maine deregulated prior to the first ever AHS (1978), so it would have been dropped from our sample even if it was included in later waves of AHS. This leaves Alaska as the outlier among these states, being assigned into treatment in 1982 and thus having one year in the control group.

the household has refinanced their mortgage in year  $t$ , or the interest rate on new mortgages. We can assign treatment (i.e., the reform) to the household with precision because the survey reports the year of the mortgage refinance, while the interest rates documented in the survey has the year the mortgage was issued.<sup>10</sup> In our main analysis,  $Y_{hst}$ , is a dichotomous outcome variable, turning on if the household purchased the durable good in question. Finally, we will analyze how these durable purchases, through the banking deregulation variables, impacted energy use and time spent on household chores.

Given the biennial nature of the survey and that the date of the purchase is not precisely known, our main analysis adopts a more flexible specification than the standard linear treatment effect model to accommodate for measurement errors. We include three indicator variables for each reform. The first indicator, *Post deregulation*(0,1), is for the year of the reform or the year following the reform when the biennial survey is not administered in the year of the deregulation.<sup>11</sup> The second indicator is for the second or third year after the reform (*Post deregulation*(2,3)). The final indicator variable, *Post deregulation*( $\geq 4$ ), is for the fourth year after the reform and later. We run five main regressions with different household durable good purchases as the outcome variable (dryers, fridge, laundry washer, dishwasher and oven). We further run five regressions with energy conservation features and installments as our outcome variables (major installments, renovations, insulation added, new storm windows/doors and new roof).

To check the validity of the estimation strategy, we run a battery of robustness checks employing models that include a further indicator, *Pre – treatment*( $\leq 3$ ), for the third, fourth, etc., year before the deregulation, i.e., accounting for pre-trends. Before the durable goods purchase and energy conservation regressions however, we first run two mortgage credit-related regressions that establish the expansion of credit supply following deregulation.

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<sup>10</sup> Measuring housing finance activity is not a primary goal of the AHS, which can raise questions about the accuracy and coverage of the mortgage-related information contained within it. According to the replication analysis performed by Lam and Kaul (2003), AHS is indeed prone to undercounting refinancing activity. However, there is no evidence of geographical variation in this coverage ratio (volume of refinanced mortgages in AHS/actual refinancing volume) in a given survey year. Therefore, we have no reason to believe that such an undercount will affect our results. More importantly, Lam and Kaul (2003) show that the earlier survey years (which we use in our sample) do a better job of accurately capturing refinancing activity.

<sup>11</sup> To be precise, because the survey is administered every odd year, the indicator takes the value of 1 in the year of the reform for those states that enact the deregulation in an odd year. However, the reform comes into effect in an even year for 15 out of 38 states. For these 15 states, the indicator turns on for the year immediately after the reform. For instance, the reform comes into effect in 1988 in Colorado, when there is no survey, so *Post deregulation* takes the value of 1 for those households interviewed in 1989.

Table 1 reports the spread of observations by years relative to deregulation for these different regressions. There is a wide spread of survey years in each treatment status, providing confidence that our results are not influenced by pure time trends or outliers.

It is worth noting that our use of household fixed effects will account for any unobserved heterogeneity that is time-invariant at the household-level and, because we restrict to household who did not move state, state-level. To alleviate concerns arising from omission of relevant state-specific factors, we add a vector  $X_{st}$  that represent state-specific variables, such as energy prices and average coincidence index statistic, which control for time-varying state-level conditions. Other general factors common to our sample of households are also captured by our survey year dummies. Furthermore, we include a vector  $Z_{ht}$  of household characteristics (household income and household size) and respondent-level controls (respondent's age, marital status and whether she gained a college degree), which help us account for any time-variant source of heterogeneity at the household-level. Our empirical strategy allows for treatment effect heterogeneity over the household income distribution. We test for the idea that the reform may have impacted high-income households more than low-income households.

**Table 1: Distribution of Observations by Treatment Status**

Panel A: Percent of Observations in Each Treatment Category by Survey Year for Durable Good Purchases and Energy Features							
Survey Year	1981	1983	1985	1987	1989	1991	1993
Pre-treatment (-1, -2)	68	30	0.7	0.2			
Post deregulation (0,1)		38	16	43	1	0.1	0.4
Post deregulation (2,3)			12	17	67	1.5	0.2
Post deregulation ( $\geq 4$ )				4	14	34	46
Panel B: Percent of Observations in Each Treatment Category by Survey Year for Mortgage Refinance Activity							
Survey Year	1981	1983	1985	1987	1989	1991	1993
Pre-treatment (-1, -2)			85	15			
Post deregulation (0,1)			32	65	1	0.3	0.3
Post deregulation (2,3)			25	18	55	1	0.2
Post deregulation ( $\geq 4$ )				7	14	38	39

Notes: Mortgage-related outcome variables are only available from 1985 and onwards.

There has been much discussion in the literature recently about treatment effect estimation in a difference-in-difference setting. Goodman-Bacon (2018) warns of incorrect signs with heterogeneous timing of treatments, de Chaisemartin and D'Haultfœuille (2020) warn of negative weights on some observations when treatment effects increase over time, and Sloczynski (forthcoming) warn of bias due to the weighting of treatment effects by percent of observations in treatment versus control. For this analysis, an event study specification is shown to remedy the Goodman-Bacon (2018) concern. Further tests of the data reveal that only 0.01% negative weights in our main specification to remedy the de Chaisemartin and



D'Haultfœuille (2020) concern. Furthermore, applying the Sloczynski (forthcoming) weighting is not altering our estimated treatment effects as the weights are 47/53 for treated vs control observations.

## 5. Results

### *5.1 Banking deregulation and credit availability*

Our research question is based on the idea that banking deregulation affected credit availability of households. In previous sections, we surveyed the evidence and concluded that the current empirical literature agrees with this statement. In this section, we provide further evidence using two questions that are included in our survey. We first estimate the impact banking deregulation had on the propensity and the ability of a household to refinance their mortgage. The outcome variable for the mortgage refinance, which is only available after 1985, takes the value of one when a homeowner stated they took out a new mortgage on their existing home.

We also use the available data on mortgage and home purchase year to identify all new mortgages (home purchase and refinance) and calculate the average interest rate (in basis points) on new mortgages at the state-year level. We then estimate the impact of deregulation on the average mortgage rate at the state level, in a specification similar to the one used by Sun and Yannelis (2016).<sup>12</sup> A negative and significant coefficient on interest rate would provide supplementary evidence of an expansion of credit.

When looking at mortgage market-related activity during our sample period, we have to take into account refinancing boom starting in 1992, driven by low monetary policy rates (Lam and Kaul, 2003). Accordingly, Panel B of Table 1 provides the distribution of treatment observations in years relative to deregulation timelines. If most of our treatment observations are from 1993, then it will not be possible to separate out the effect of deregulation from that of the Federal Reserve's monetary policy actions. However, as seen in Panel B of Table 1, 1993 is not overrepresented in our treatment sample, which alleviates any concerns related to monetary policy acting as a confounding factor.

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<sup>12</sup> The average mortgage rate in our sample of new mortgages identified in the AHS is 9.79% (for 1980-1993). Although, this is slightly higher than the average mortgage rate of 8.33% in the Federal Housing Finance Agency data used by Sun and Yannelis (2016), this study also covers a longer time period (early-1970s to early-1990s).

Results reported in Table 2 support the view that banking deregulation improved credit access for households. Banking deregulation is associated with, on average, a 1.7% percentage point increase in the likelihood of refinancing a mortgage and a 53 basis point reduction in the interest rate on new mortgages (relative to a mean of 6% and 9.79%, respectively). When we estimate event study treatment effects, we find that increased probability of refinancing increases with years after deregulation and each year after deregulation is statistically significant. For interest rates, the same pattern follows with a smaller interest rate for each year after deregulation. These findings are relevant, as previous studies have found evidence of households using home equity for consumption expenditures, especially during our sample period (Manchester and Poterba, 1989). Therefore, we conclude that banking deregulation is likely associated with households having increased access to the types of credit that can be used to finance the type of spending we are interested in.

**Table 2: The Impact of Banking Deregulation on Credit Expansion**

	(1)	(2)	(1)	(2)
	Mortgage Refinance		Mortgage Rate	
Pre-treatment ( $\leq 3$ )	-0.013 (0.015)		55.36 (48.79)	
Post deregulation (0,1)	0.025** (0.011)		-47.61** (18.55)	
Post deregulation (2,3)	0.040** (0.016)		-69.36** (26.31)	
Post deregulation ( $\geq 4$ )	0.061*** (0.019)		-87.63** (33.48)	
Post deregulation (all)		0.017*** (0.009)		-53.47*** (18.10)
Observations	58,247	58,247	456	456
R-squared	0.04	0.05	0.44	0.45
Time Variant Controls	Yes	Yes	No	No

**Notes:** This table shows estimate from four separate regressions of the effect of banking deregulation on credit expansion using difference-in-differences methodology. “Mortgage Refinance” is an indicator variable on whether the household refinanced their mortgage in the survey wave, while “Mortgage Rate” is the average interest rate on new mortgages in basis points for each state-year. Since the mortgage rate regression is at the state-level and the year of new mortgage is known precisely, we run regression where the reference year is the year before the deregulation and the pre-treatment period is two, three, etc. years before the deregulation. Mortgage refinance regressions include household and survey year fixed effects, control for annual coincidence index, and household income. Mortgage rate regressions include state and year of mortgage fixed effects. Robust standard errors are in parentheses below each coefficient and were clustered at the metropolitan statistical area for the refinance regressions and at the state level for the interest rate regressions; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

## *5.2. Banking deregulation and consumption of durables*

Tables 3 display regression results from our main analysis and investigates whether the exogenous increase in the availability of credit allows households the purchasing power to buy new durable household appliances in Panel A (“durables”), and home modifications and energy conservation features in Panel B (“modifications”). Results show that banking deregulation is followed by an increase of consumption and renovations by households, on average.

The Panel A in Table 3 shows that durable goods consumption increases by approximately 2-5 percentage points immediately after the reform, and by some 26-40 percentage points four or more years after. Panel B provides evidence that households are also more likely to invest in major home modification or renovations. The results suggest that relaxed credit constraints increased the likelihood of household undertaking large energy conservation upgrades by 2 to 10 percentage points but only two or more years following the reform.

## *5.3 Falsification test and robustness checks*

Although our model controls for a variety of factors at household and state-level, including time-invariant unobserved characteristics at household levels and state-level time variant characteristics, the estimation may still be affected by selection bias if say the time of the reform is correlated with the error term. To address some of these remaining concerns we estimate flexible models that include a further indicator variable that captures years leading to the reform. This pre-treatment period dummy includes three years or earlier before the reform. Effects measured with this approach are relative to the survey year preceding the reform in each state, i.e., one or two years before the deregulation. We expect the estimates to be small and not statistically different from zero to rule out the possibility that the changes that we observe in Tables 3 are actually driven by pre-trends in purchasing behaviors.

Table 4 illustrates two key points: that household purchases of durable goods were not changing before deregulation, and the impact of access to credit is likely a main factor responsible for the materialization of consumption in this sector to increase. As shown in Table 4, the coefficient associated with our placebo pre-treatment does not enter statistically significantly into our regression and its magnitude is considerably small in comparison to our treatment effect in Table 3. These results allow us to argue that the timing of bank deregulation does not vary with the degree of pre-existing consumption trends of large household appliances.

**Table 3: The impact of banking deregulation on durables consumption and home modifications**

PANEL A. DURABLES		New Dishwasher		New Laundry Washer		New Oven		New Fridge		New Dryer	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	
Post deregulation (0,1)	0.026** (0.010)	0.029*** (0.011)	0.013 (0.008)	0.016* (0.009)	0.014 (0.009)	0.015 (0.009)	0.014* (0.008)	0.015* (0.009)	0.006 (0.007)	0.008 (0.007)	
Post deregulation (2,3)	0.053** (0.020)	0.057** (0.021)	0.037** (0.016)	0.039** (0.017)	0.014 (0.019)	0.016 (0.019)	0.026 (0.019)	0.027 (0.020)	0.024 (0.015)	0.024 (0.016)	
Post deregulation (≥4)	0.408*** (0.021)	0.413*** (0.022)	0.404*** (0.017)	0.406*** (0.018)	0.263*** (0.026)	0.265*** (0.027)	0.368*** (0.028)	0.369*** (0.028)	0.340*** (0.020)	0.341*** (0.021)	
Observations	88,379	77,779	106,548	95,948	126,803	116,194	126,972	116,363	99,613	89,014	
R-squared	0.193	0.194	0.227	0.231	0.153	0.155	0.225	0.227	0.187	0.190	
PANEL B. MODIFICATIONS		Insulation added		New roof		Storm Windows and Doors Protection		Major Equipment Installed		Renovations	
	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	
Post deregulation (0,1)	-0.002 (0.003)	-0.002 (0.003)	-0.005 (0.008)	-0.005 (0.009)	-0.000 (0.007)	-0.001 (0.008)	0.001 (0.006)	0.002 (0.007)	-0.001 (0.002)	-0.001 (0.002)	
Post deregulation (2,3)	0.025*** (0.004)	0.025*** (0.004)	0.061*** (0.010)	0.061*** (0.011)	0.051*** (0.012)	0.050*** (0.012)	0.098*** (0.010)	0.098*** (0.010)	0.018*** (0.005)	0.017*** (0.005)	
Post deregulation (≥4)	0.029*** (0.005)	0.029*** (0.005)	0.067*** (0.016)	0.068*** (0.016)	0.057*** (0.016)	0.055*** (0.016)	0.099*** (0.015)	0.099*** (0.016)	0.018*** (0.005)	0.017*** (0.005)	
Observations	128,269	117,660	99,887	89,289	99,882	89,284	99,855	89,257	104,975	94,374	
R-squared	0.002	0.004	0.008	0.009	0.008	0.010	0.030	0.031	0.011	0.012	
Time Variant Household Controls		No	Yes	No	Yes	No	Yes	No	Yes	No	

**Notes:** This table shows estimates of the effect of banking deregulation on the probability of purchasing five durable goods (Panel A) and adding five major modifications in homes (Panel B) using difference-in-differences. Each regression includes household and year fixed effects and control for annual coincidence index, energy prices (price of electricity and natural gas at the state-level). Some specifications include respondent's characteristics such as age, marital status, whether the respondent graduated from college, household size and household income. The variation in observations main derive from respondents not answering the question analyzed. Robust standard errors are in parentheses below each coefficient and were clustered at the state level; \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

The estimated impact of deregulation on the consumption of durables is very similar across the two specifications in Table 3 and 4, which provide further confidence in our finding. Our results are also robust to different flexible specifications of the indicator variables for the period after the deregulation. Table 5 shows a less parsimonious specification where the post-reform period is broken down into five indicators (from zero, 1 year after the reform to the last dummy that capture 8 or more years after the deregulation).

We further explore the mechanism leading to this increase in durable good purchases, by estimating a model where household income is the dependent variable and the treatment variables are independent. Given the heterogeneity in incomes by state, this model uses a state fixed effect. Results given in Table 6 show that household income becomes statistically significant eight years after deregulation. This is similar to the finding in Beck et.al. (2010) for wages of unskilled workers, which become statistically significant six years after deregulation. Given that we observe increased credit access and more durables spending soon after deregulation, with income responding much later, it is unlikely that deregulation increased credit access by increasing incomes. In other words, as opposed to making more households “creditworthy” by increasing their income and hence satisfying pre-deregulation credit standards, deregulation increased competition and led to lenders granting credit to those households previously excluded from the market.

#### *5.4 Durables consumption across the income and age distribution*

In this section, we investigate whether the effect of deregulation on durable good purchases is heterogeneous across the income and age distribution of households. We interact the banking deregulation indicators with a binary variable indicating whether the household was in the top 30% of incomes within the state during the year before the deregulation .

**Table 4: Testing for pre-trends on consumption of durables and home modifications**

	New Dishwasher		New Laundry Washer		New Oven		New Fridge		New Dryer	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
<b>PANEL A. DURABLES</b>										
Pre-treatment ( $\leq 3$ )	-0.007 (0.006)	-0.008 (0.007)	-0.004 (0.005)	-0.005 (0.005)	-0.001 (0.004)	-0.000 (0.004)	-0.002 (0.005)	-0.001 (0.005)	-0.004 (0.004)	-0.004 (0.004)
Post deregulation (0,1)	0.027*** (0.009)	0.031*** (0.010)	0.014* (0.008)	0.017** (0.008)	0.014 (0.009)	0.015 (0.009)	0.014* (0.008)	0.016* (0.009)	0.007 (0.006)	0.009 (0.007)
Post deregulation (2,3)	0.055*** (0.019)	0.058*** (0.020)	0.037** (0.016)	0.039** (0.017)	0.014 (0.019)	0.016 (0.019)	0.026 (0.019)	0.027 (0.020)	0.025 (0.015)	0.025 (0.015)
Post deregulation ( $\geq 4$ )	0.411*** (0.019)	0.415*** (0.021)	0.405*** (0.017)	0.407*** (0.018)	0.263*** (0.026)	0.265*** (0.027)	0.368*** (0.028)	0.369*** (0.028)	0.341*** (0.020)	0.342*** (0.020)
Observations	88,379	77,779	106,548	95,948	126,803	116,194	126,972	116,363	99,613	89,014
R-squared	0.193	0.194	0.227	0.231	0.152	0.155	0.224	0.227	0.187	0.190
<b>PANEL B. MODIFICATIONS</b>										
	Insulation added		New roof		Storm Windows and Doors Protection		Major Equipment Installed		Renovations	
	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
Pre-treatment ( $\leq 3$ )	0.001 (0.002)	0.001 (0.002)	-0.003 (0.004)	-0.005 (0.005)	0.002 (0.003)	0.003 (0.004)	0.000 (0.003)	0.000 (0.004)	0.001 (0.001)	0.001 (0.002)
Post deregulation (0,1)	-0.002 (0.003)	-0.002 (0.003)	-0.005 (0.008)	-0.004 (0.009)	-0.001 (0.007)	-0.002 (0.008)	0.001 (0.007)	0.002 (0.007)	-0.001 (0.002)	-0.001 (0.002)
Post deregulation (2,3)	0.025*** (0.004)	0.025*** (0.004)	0.062*** (0.010)	0.062*** (0.010)	0.051*** (0.012)	0.050*** (0.012)	0.098*** (0.010)	0.098*** (0.010)	0.018*** (0.005)	0.017*** (0.005)
Post deregulation ( $\geq 4$ )	0.029*** (0.005)	0.029*** (0.005)	0.068*** (0.015)	0.069*** (0.015)	0.056*** (0.016)	0.054*** (0.016)	0.099*** (0.015)	0.099*** (0.016)	0.018*** (0.005)	0.016*** (0.005)
Observations	128,269	117,660	99,887	89,289	99,882	89,284	99,855	89,257	104,975	94,374
R-squared	0.002	0.004	0.008	0.009	0.008	0.010	0.030	0.031	0.011	0.012
<b>Time Variant Household Controls</b>										
	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

**Notes:** This table shows estimates of banking deregulation specification on the consumption of five durable goods (Panel A) and on major home modifications (Panel B) using difference-in-differences to which pre deregulation indicators (pre-treatments) are added. Each regression includes household and year fixed effects and control for annual coincidence index, energy prices (price of electricity and natural gas at the state-level). Some specifications include respondent's characteristics such as age, marital status, whether the respondent graduated from college, household size and household income. The variation in observations main derive from respondents not answering the question analyzed. Robust standard errors are in parentheses below each coefficient and were clustered at the state level; \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Table 5: Robustness checks**

PANEL A. DURABLES	New Dishwasher		New Laundry Washer		New Oven		New Fridge		New Dryer	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Post deregulation (0,1)	0.017*** (0.005)	0.020*** (0.006)	0.005 (0.006)	0.007 (0.007)	0.013 (0.008)	0.014 (0.009)	0.013** (0.006)	0.015** (0.006)	0.001 (0.006)	0.003 (0.007)
Post deregulation (2,3)	0.026* (0.013)	0.031** (0.014)	0.016 (0.014)	0.019 (0.015)	0.011 (0.019)	0.013 (0.021)	0.025 (0.016)	0.028* (0.017)	0.012 (0.015)	0.014 (0.015)
Post deregulation (4,5)	0.362*** (0.019)	0.370*** (0.020)	0.365*** (0.014)	0.369*** (0.015)	0.257*** (0.028)	0.261*** (0.030)	0.364*** (0.020)	0.369*** (0.021)	0.316*** (0.020)	0.319*** (0.021)
Post deregulation (6,7)	0.353*** (0.033)	0.363*** (0.033)	0.370*** (0.018)	0.374*** (0.019)	0.256*** (0.036)	0.260*** (0.038)	0.369*** (0.020)	0.375*** (0.022)	0.325*** (0.026)	0.330*** (0.027)
Post deregulation (≥8)	0.317*** (0.047)	0.331*** (0.047)	0.335*** (0.028)	0.340*** (0.029)	0.252*** (0.039)	0.257*** (0.042)	0.364*** (0.029)	0.372*** (0.031)	0.299*** (0.034)	0.306*** (0.034)
Observations	88,379	77,779	106,548	95,948	126,803	116,194	126,972	116,363	99,613	89,014
R-squared	0.193	0.195	0.228	0.231	0.153	0.155	0.225	0.227	0.187	0.191
PANEL B. MODIFICATIONS	Insulation added		New roof		Storm Windows and Doors Protection		Major Equipment Installed		Renovations	
	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
Post deregulation (0,1)	-0.003 (0.003)	-0.003 (0.003)	-0.007 (0.008)	-0.007 (0.009)	0.005 (0.005)	0.004 (0.006)	-0.000 (0.005)	-0.001 (0.005)	0.004*** (0.001)	0.004** (0.002)
Post deregulation (2,3)	0.024*** (0.005)	0.025*** (0.005)	0.057* (0.033)	0.057 (0.035)	0.066*** (0.012)	0.065*** (0.013)	0.093*** (0.013)	0.092*** (0.013)	0.031*** (0.005)	0.031*** (0.005)
Post deregulation (4,5)	0.025*** (0.008)	0.025*** (0.008)	0.060 (0.055)	0.060 (0.058)	0.079*** (0.018)	0.078*** (0.018)	0.092*** (0.014)	0.090*** (0.015)	0.039*** (0.007)	0.039*** (0.007)
Post deregulation (6,7)	0.031** (0.011)	0.032*** (0.012)	0.061 (0.076)	0.060 (0.079)	0.090*** (0.025)	0.088*** (0.025)	0.088*** (0.019)	0.085*** (0.019)	0.046*** (0.010)	0.046*** (0.010)
Post deregulation (≥8)	0.024 (0.015)	0.025 (0.015)	0.054 (0.105)	0.054 (0.109)	0.103*** (0.035)	0.101*** (0.036)	0.084*** (0.027)	0.080*** (0.027)	0.060*** (0.014)	0.061*** (0.014)
Observations	128,269	117,660	99,887	89,289	99,882	89,284	99,855	89,257	104,975	94,374
R-squared	0.002	0.004	0.008	0.009	0.008	0.010	0.030	0.031	0.011	0.012
Time Variant Household Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

**Notes:** This table shows estimates of banking deregulation on the consumption of five durable goods (Panel A) and home modifications (Panel B) Each regression includes household and year fixed effects and control for annual coincidence index, energy prices (price of electricity and natural gas at the state-level). Some specifications include respondent's characteristics such as age, marital status, whether the respondent graduated from college, household size and household income. The variation in observations main derive from respondents not answering the question analyzed. Robust standard errors are in parentheses below each coefficient and were clustered at the state level; \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Table 6: The impact of banking deregulation on income**

	(1) Income
Post deregulation (0,1)	-263.96 (351.15)
Post deregulation (2,3)	-259.38 (522.73)
Post deregulation (4,5)	372.73 (691.91)
Post deregulation (6,7)	1180.10 (848.61)
Post deregulation ( $\geq 8$ )	1782.44* (1024.33)
Observations	117,673
R-squared	0.076

**Notes:** This table shows estimates of the effect of banking deregulation on household income. Each regression controls for state and year fixed effects. Robust standard errors are in parentheses below each coefficient; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Our results presented in Table 7 Panel A reveals that households that fall into this category typically purchase more durable goods, on average, than other income groups, starting in four years after the enactment of banking deregulation. The finding suggests of a drop in consumption of most of the durables until three years after the reform. We repeat the same exercise to investigate whether there is a heterogeneous effect across income groups with respect to home modifications and renovations. The results in Table 7 Panel B imply that top-earners invested more in home modifications and renovations, on average, relative to other income groups after deregulation. That being said, however, the economic magnitude of these coefficients is small in contrast to the sizeable percentage increase across the rest of the sample.

What is the most essential to take away from our analysis is that all households within our sample were considerably more likely, on average, to purchase durable goods once granted access to credit. These findings provide further evidence in favor of our earlier conclusion that, independent of its potential impact on income, banking deregulation led to a spike in consumption by enhancing availability to credit for all income brackets. This supports the argument made by Beck et al. (2010) that unregulated expansion of large banks do not disproportionately help wealthy and/or higher income individuals, but rather benefit an entire population by increasing their access to durable goods and home improvements.



**Table 7: The impact of banking deregulation on durables consumption across income groups**

PANEL A. DURABLES	New Dishwasher		New Laundry Washer		New Oven		New Fridge		New Dryer	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Post deregulation (0,1) × High income	-0.002 (0.005)	0.002 (0.005)	-0.000 (0.003)	0.003 (0.003)	0.005 (0.004)	0.007 (0.004)	0.001 (0.004)	0.004 (0.005)	-0.003 (0.005)	0.000 (0.004)
Post deregulation (2,3) × High income	-0.024*** (0.007)	-0.018*** (0.007)	-0.005 (0.007)	-0.001 (0.007)	-0.018*** (0.006)	-0.016** (0.007)	-0.011* (0.006)	-0.008 (0.006)	-0.001 (0.007)	0.005 (0.006)
Post deregulation (4,5) × High income	0.033*** (0.011)	0.039*** (0.011)	0.046*** (0.006)	0.050*** (0.006)	0.033*** (0.007)	0.035*** (0.008)	0.035*** (0.008)	0.039*** (0.008)	0.056*** (0.009)	0.062*** (0.009)
Post deregulation (6,7) × High income	0.025** (0.011)	0.031*** (0.011)	0.031*** (0.011)	0.035*** (0.011)	0.018* (0.010)	0.019* (0.010)	0.004 (0.009)	0.008 (0.009)	0.034*** (0.012)	0.039*** (0.012)
Post deregulation (≥8) × High income	0.024 (0.019)	0.029 (0.019)	0.010 (0.010)	0.013 (0.010)	0.004 (0.008)	0.005 (0.008)	0.003 (0.009)	0.005 (0.010)	0.008 (0.011)	0.013 (0.011)
Observations	88,379	77,779	106,548	95,948	126,803	116,194	126,972	116,363	99,613	89,014
R-squared	0.195	0.196	0.228	0.232	0.153	0.156	0.225	0.228	0.189	0.192
PANEL B. MODIFICATIONS	Insulation added		New roof		Storm Windows and Doors Protection		Major Equipment Installed		Renovations	
	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
Post deregulation (0,1) × High income	-0.002 (0.005)	-0.004 (0.005)	-0.008 (0.008)	-0.004 (0.008)	-0.007 (0.006)	-0.007 (0.006)	-0.010* (0.006)	-0.005 (0.006)	0.001 (0.003)	0.002 (0.004)
Post deregulation (2,3) × High income	0.017*** (0.005)	0.015*** (0.005)	0.005 (0.009)	0.011 (0.009)	0.020*** (0.008)	0.021** (0.008)	0.010 (0.009)	0.016 (0.010)	0.001 (0.004)	0.002 (0.004)
Post deregulation (4,5) × High income	0.001 (0.004)	-0.001 (0.004)	0.009 (0.010)	0.014 (0.011)	0.003 (0.006)	0.004 (0.006)	0.008 (0.011)	0.014 (0.012)	-0.005 (0.003)	-0.004 (0.004)
Post deregulation (6,7) × High income	0.005 (0.004)	0.003 (0.005)	0.026** (0.010)	0.031*** (0.011)	0.013 (0.008)	0.014 (0.008)	0.015* (0.008)	0.021** (0.009)	-0.005 (0.004)	-0.004 (0.004)
Post deregulation (≥8) × High income	-0.001 (0.005)	-0.002 (0.006)	0.028 (0.022)	0.033 (0.022)	-0.012 (0.009)	-0.012 (0.009)	0.015 (0.010)	0.020* (0.011)	0.011 (0.008)	0.012 (0.008)
Observations	128,269	117,660	99,887	89,289	99,882	89,284	99,855	89,257	104,975	94,374
R-squared	0.003	0.005	0.009	0.009	0.008	0.010	0.030	0.031	0.011	0.013
Time Variant Household Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

**Notes:** This table shows estimates of banking deregulation across income groups. Each regression includes household and year fixed effects and control for annual coincidence index, energy prices (price of electricity and natural gas at the state-level). Some specifications include respondent's characteristics such as age, marital status, whether the respondent graduated from college, household size and household income. The variation in observations main derive from respondents not answering the question analyzed. Robust standard errors are in parentheses below each coefficient and were clustered at the state level; \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

Next, we interact the banking deregulation indicators with a dummy variable that denotes whether the head of the household is 40 or younger. Those results, given in Table 8, reveal that younger households did disproportionately increase their durable good purchases as credit constraints eased. However, the result is not consistent across all treatment years and products.

## 6. Banking deregulation, energy and labor savings

In this section, we investigate potential benefits associated with increased uptake of durables and home investments. It could be argued that the new appliances are more energy efficient than the ones replaced. Further, the same can be argued for all of the renovations that household invested in, such as insulation, new doors and windows, new roofs as they lessen the need to heat or cool. More energy efficient appliances have the direct effect of lowering home energy bills, *ceteris paribus*. Lower energy consumption has also the additional external benefit of lower pollution emissions. On the other hand, if there were new appliances that replaced a less energy intensive use it may result in higher energy use.

Our survey contains information on energy use so we can empirically investigate the link between the timing of the reforms and average energy expenditures. AHS controls for seasonality (as utility costs are more likely to be higher or lower in particular times of the year) by taking the total average of all 12 months. In this way, we utilize monthly expenditures as a proxy for household energy consumption to examine if banking deregulation affected household energy use in a meaningful way.

Results from Table 9 suggests that the typical household in our sample spent less on monthly electricity, on average, after credit constraints were relaxed. The impact is about \$5 on an average \$65 in expenditure, so a 7.5% decline. Our treatment-effect is also negative in both natural gas regressions presented in columns 3 and 4, however, they are not statistically different from zero.

**Table 8: The impact of banking deregulation on durables consumption across income groups**

PANEL A. DURABLES	New Dishwasher		New Laundry Washer		New Oven		New Fridge		New Dryer	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Post deregulation (0,1) × Young	-0.010** (0.005)	-0.021*** (0.008)	-0.009** (0.004)	-0.009 (0.005)	-0.001 (0.003)	-0.005 (0.004)	-0.004 (0.004)	-0.008 (0.005)	-0.011** (0.004)	-0.007 (0.007)
Post deregulation (2,3) × Young	-0.019*** (0.007)	-0.028*** (0.008)	-0.003 (0.005)	-0.003 (0.006)	-0.003 (0.004)	-0.009* (0.005)	0.004 (0.004)	-0.002 (0.005)	-0.001 (0.006)	0.006 (0.007)
Post deregulation (4,5) × Young	0.061*** (0.014)	0.049*** (0.014)	0.124*** (0.014)	0.120*** (0.015)	0.045*** (0.009)	0.036*** (0.009)	0.073*** (0.010)	0.064*** (0.010)	0.137*** (0.014)	0.140*** (0.015)
Post deregulation (6,7) × Young	0.020* (0.012)	0.005 (0.012)	0.098*** (0.014)	0.089*** (0.015)	0.048*** (0.010)	0.035*** (0.010)	0.058*** (0.010)	0.046*** (0.010)	0.096*** (0.013)	0.095*** (0.015)
Post deregulation (≥8) × Young	0.016 (0.022)	-0.001 (0.021)	0.076*** (0.022)	0.064*** (0.023)	0.036** (0.014)	0.020 (0.015)	0.059*** (0.017)	0.042** (0.017)	0.092*** (0.016)	0.090*** (0.017)
Observations	88,379	77,779	106,548	95,948	126,803	116,194	126,972	116,363	99,613	89,014
R-squared	0.195	0.196	0.233	0.235	0.154	0.156	0.227	0.229	0.195	0.197
PANEL B. MODIFICATIONS	Insulation added		New roof		Storm Windows and Doors Protection		Major Equipment Installed		Renovations	
	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
Post deregulation (0,1) × Young	0.007** (0.003)	0.001 (0.005)	-0.006 (0.009)	0.002 (0.015)	0.003 (0.007)	-0.003 (0.010)	0.003 (0.004)	0.001 (0.006)	0.002 (0.004)	-0.003 (0.005)
Post deregulation (2,3) × Young	0.008* (0.005)	-0.001 (0.006)	0.015 (0.013)	0.028* (0.017)	0.002 (0.008)	-0.011 (0.010)	0.017* (0.009)	0.012 (0.010)	0.006 (0.006)	-0.002 (0.008)
Post deregulation (4,5) × Young	0.005 (0.005)	-0.006 (0.007)	0.032*** (0.011)	0.046*** (0.014)	0.020** (0.010)	0.004 (0.012)	-0.000 (0.009)	-0.007 (0.011)	-0.011** (0.005)	-0.021*** (0.007)
Post deregulation (6,7) × Young	0.008* (0.005)	-0.005 (0.006)	0.046*** (0.013)	0.062*** (0.017)	0.010 (0.010)	-0.011 (0.012)	0.008 (0.010)	-0.002 (0.012)	-0.002 (0.007)	-0.014 (0.009)
Post deregulation (≥8) × Young	0.005 (0.006)	-0.009 (0.008)	0.056*** (0.020)	0.073*** (0.023)	0.009 (0.018)	-0.016 (0.020)	0.019 (0.012)	0.008 (0.014)	0.008 (0.012)	-0.006 (0.014)
Observations	128,269	117,660	99,887	89,289	99,882	89,284	99,855	89,257	104,975	94,374
R-squared	0.003	0.004	0.009	0.009	0.008	0.010	0.030	0.031	0.012	0.013
Time Variant Household Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

**Notes:** This table shows estimates of banking deregulation on five types of home modifications and renovations age groups. Each regression includes household and year fixed effects and control for annual coincidence index, energy prices (price of electricity and natural gas at the state-level). Some specifications include respondent's characteristics such as age, marital status, whether the respondent graduated from college, household size and household income. The variation in observations main derive from respondents not answering the question analyzed. Robust standard errors are in parentheses below each coefficient and were clustered at the state level; \*\* p<0.05, \* p<0.1.

We then move towards investigating the possibility of banking deregulation allowing household to purchase labor-saving technologies using survey data on time use. Specifically, we use the American Heritage Time Use Survey (AHTUS) from the AHTUS-X database (Fisher, Gershuny, Flood, Roman and Hofferth, 2018) to look at patterns on time spent on “unpaid domestic work” (as specified in the AHTUS), which includes tasks such as cleaning, cooking and washing dishes. If it is indeed the case that banking deregulation relaxes borrowing constraints and allows households to purchase new labor-saving appliances, then we should expect a decline in time spent on such domestic activities. Alternatively, Bui and Ume (2020) find that banking deregulation led to fewer hours worked outside the home, which may lead to more hours worked on domestic activities.

**Table 9: The impact of banking deregulation on monthly energy expenses**

	(1) Electricity	(2) Electricity	(3) Natural Gas	(4) Natural Gas
Post deregulation ( $\geq 0$ )	-4.90** (2.18)	-4.879** (2.193)	-0.537 (1.709)	-0.594 (1.689)
Observations	96523	96513	19308	19308
R-squared	0.043	0.054	0.137	0.138
Time Varying Household Controls	No	Yes	No	Yes

**Notes:** This table shows estimates of the effect of banking deregulation on monthly energy expenditure (OLS). Each regression controls for annual average coincidence index, energy prices (price of electricity and natural gas at the state-level), the size of the house measured in square feet, the age of the respondent/head of the house, the number of children under the age of 18 currently living in the house, and year fixed effects. Robust standard errors are in parentheses below each coefficient and were clustered at the metropolitan statistical area; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

The primary challenge associated with this approach is data availability. AHTUS data from the 1980s, the period during which most banking deregulation took place, does not include geographical information for survey respondents. Therefore, we are unable to perform an analysis that looks at time spent on domestic activities before vs. after banking deregulation. Instead, we use data from the 1990s and exploit variation in the “time elapsed since deregulation” across different states.

Specifically, we use AHTUS data from two waves in the 1990s: 1992-1995 and 1998-2000. Our primary assertion is that by 1992, the impact of deregulation has been almost completely felt by households living in states that deregulated early. Consider a household living in a state that deregulated in 1985. It is highly likely that the relaxation of the borrowing constraint, the purchase of labor-saving appliances and the reduction in time spent on domestic activities

already took place during the years between deregulation and the start of our AHTUS sample. On the other hand, a household living in a state that only deregulated in 1990 may not have fully felt the impact of the deregulation by the start of our AHTUS sample. Therefore, we can reasonably expect that between 1992-1995 and 1998-2000, average time spent on domestic activities will decline in later deregulating states relative to early deregulating states.<sup>13</sup>

Given the repeated cross-sectional nature of AHTUS, inconsistencies in the availability of individual-level variables and the variation in sample sizes between the two survey waves, we estimate the following specification *separately* for 1992-1995 and 1998-2000:

$$Y_{hst} = \alpha_s + B_1 X_h + \rho_t + \varepsilon_{hst} \quad (2)$$

where the dependent variable  $Y_{hst}$  is the time spent on domestic chores by the survey respondent (in minutes during the past 24 hours),  $\alpha_s$  is a state-level fixed effect,  $X_h$  is a vector of survey respondent characteristics (gender, household size and the presence of children in the household) and  $\rho_t$  is a fixed effect for the year in which the survey was completed. We also use the survey weights (accounting for population/sample distribution and the day of the week in which the survey was completed) provided by AHTUS while estimating this specification.

Our main coefficient of interest is  $\alpha_s$ , which can be thought of as the baseline average time spent on domestic activities in each state. We would like to see whether  $\alpha_s$  for states that deregulated later declined (or declined at a faster rate) between the two survey waves. After obtaining  $\alpha_s$ s for each state and for each survey wave, we calculate an average  $\alpha_s$  for 12 “late-deregulating” states, defined as states that deregulated in 1988 or later. The remaining 36 states form the “deregulated earlier” group.<sup>14</sup> When calculating the group averages for each wave, we weigh each state by the number of AHTUS respondents to account for variations in population.

We find that during the 1992-1995 period, the average baseline time spent on domestic chores was 176.6 minutes in late deregulating states and 171.4 minutes in states that deregulated early (a difference that is statistically significant at the 1% level). During the second AHTUS wave

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<sup>13</sup> According to our results in Tables 3, we establish that the effect on durable goods consumption is most consistently present starting in 4 years after deregulation.

<sup>14</sup> The “late-deregulating” states are: Colorado (1988), Delaware (1988), Iowa (1991), Kansas (1992), Mississippi (1988), Montana (1993), Nebraska (1990), New Mexico (1989), North Dakota (1991), South Dakota (1988), Vermont (1988) and West Virginia (1988). Alaska and Hawaii are excluded because they are not covered by the AHTUS survey.

(1998-2000), these averages were 163.1 minutes vs. 168.2 minutes (a difference that is significant at the 1% level). Therefore, as predicted, average time spent on domestic chores fell at a faster rate in late deregulating states (by 10.4 minutes). This difference of 10.4 minutes a day corresponds to 2.64 fewer days per year spent on domestic chores by the residents of the twelve late deregulating states. We interpret this difference as an outcome of the banking deregulation and the subsequent relaxation of the borrowing constraint fully taking effect between 1992-1995 and 1998-2000.

## 7 Conclusion

A large literature has shown that households respond to an increase in credit availability. We test this explanation using the staggered introduction of banking deregulation in the U.S. on durable good purchases.

Our results suggest that banking deregulation relaxed credit constraints, which led to an increase in durable good purchases and home renovations. Mechanism tests conducted provide evidence of this by separating the difference in time periods, where before the banking deregulation treatment there was no statistically significant difference in propensity to purchase durable goods, and afterwards, there is a robust, statistically significant increase in durable good purchases.

Our identification strategy is an event-study approach that allows us to examine the trend in average household durable good purchases and energy conservation additions made after treatment to otherwise similar households whose state legislature has not yet enacted banking deregulation policy. Combining household-level data from the American Household Survey on the date in which states relaxed their credit constraints, we find positive and statistically significant treatment effects for all durable goods available within our data. We also find small differences across income groups, with high income groups slightly more likely to purchase durables or installing energy conservation equipment or engaging with renovations and home modifications.

Next, we show decreasing average annual electricity consumption over time from households who were subject to banking deregulation. Finally, this increased propensity to purchase

durable goods led households to decrease the quantity of time previously spent on household chores. Due to the form of the American Heritage Time Use Survey (AHTUS) around the sample period we can test for changes in time spent on household chores only for recent versus early experience with banking deregulation. Nevertheless, we find some suggestive evidence in favor of time spent on domestic chores falling faster for households that recently experienced banking deregulation than those households living in states that deregulated much earlier.

We view this labor-saving component of the analysis as a preliminary exploration, however. Empirically, this explanation warrants further investigation with longitudinal datasets providing information regarding how household members spend their time before and after purchasing or adding more durable goods to their homes in the post period. Research on the channels linking access to credit and labor-leisure decisions is a promising area.

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## Appendix A

**Table A1: Descriptive Statistics**

Variable	Obs	Mean	Std. Dev.	Min	Max
<i>Durables</i>					
New Dishwasher	88,379	0.08	0.27	0	1
New Laundry Washer	106,548	0.10	0.29	0	1
New Oven	126,803	0.08	0.28	0	1
New Fridge	126,972	0.11	0.31	0	1
New Dryer	99,613	0.08	0.27	0	1
<i>Modifications</i>					
Insulation added	128,269	0.03	0.16	0	1
New roof	99,887	0.08	0.28	0	1
New storm windows and doors protections	99,882	0.06	0.23	0	1
Major equipment installed	99,855	0.04	0.20	0	1
Renovations	104,975	0.01	0.11	0	1
<i>Household/respondent's</i>					
Age	117,660	37.7	25.1	1	91
Married	117,673	0.56	0.50	0	1
Widowed	117,673	0.14	0.35	0	1
Divorced	117,673	0.12	0.33	0	1
Separated	117,673	0.04	0.19	0	1
Single	117,673	0.14	0.34	0	1
Household income	117,673	2.67	1.53	1	20
College degree	128,269	0.11	0.31	0	1
Household income	117,673	32,424	28,102	0	400,000
<i>State-level variables</i>					
Coincidence index	128,269	58.76	10.91	30.48	90.74
Price of natural gas	128,269	5.87	1.46	2.87	21.23
Price of electricity	128,269	24.58	5.75	6.73	38.61
<i>Energy expenditure</i>					
Monthly gas expenditure	19,308	44.80	30.51	0	197
Monthly electricity expenditure	29,969	67.58	45.80	1	396

**Table A2: States in the sample and banking deregulation year**

<b>State/Abbreviation</b>	<b>Year Interstate Banking Permitted</b>
Alabama – AL	1987
Arizona – AZ	1986
Arkansas – AR	1986
California – CA	1987
Colorado – CO	1988
Connecticut, CT	1983
Florida – FL	1985
Georgia – GA	1985
Hawaii – HI	1985
Illinois – IL	1986
Indiana – IN	1986
Iowa – IA	1991
Kansas – KS	1992
Kentucky – KY	1984
Louisiana – LA	1987
Maryland – MD	1985
Massachusetts – MA	1983
Michigan – MI	1986
Minnesota – MN	1986
Mississippi – MS	1988
Missouri – MO	1986
Nevada – NV	1985
New Jersey – NJ	1986
New Mexico – NM	1989
New York – NY	1982
North Carolina – NC	1985
Ohio – OH	1985
Oklahoma – OK	1987
Oregon – OR	1986
Pennsylvania – PA	1986
Rhode Island – RI	1984
South Carolina – SC	1986
Tennessee – TN	1985
Texas – TX	1987
Utah – UT	1984
Virginia – VA	1985
Washington – WA	1987
Wisconsin – WI	1987

**Source:** Amel (1993), Kroszner and Strahan (1999), and Demyanyk, Ostergaard, and Sorensen (2006). Only States in our sample are reported



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