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Discussion Papers

No. 4
January 2022

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ISSN 2194-2188

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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 1989, we link the timing of these reforms with evidence of a credit expansion and household responses on many margins. We find evidence that low-income households are more likely to purchase new appliances after the deregulation. These durable goods allowed households to consume less natural gas and spend less time in domestic activities after the reforms.

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

JEL classification: D12, G2, Q41

* This is a completely revised version of *Damar, H. Evren; Lange, Ian; McKennie, Caitlin; Moro, Mirko*: Banking Deregulation and Household Consumption of Durables. IWH Discussion Paper No. 18/2020. Halle (Saale) 2020.

1 Introduction

The important role of credit availability in determining spending decisions of households has been well established in the literature (Jensen and Johannesen, 2016; Agarwal et al., 2015; Damar, Gropp, and Mordel, 2020; Gross and Souleles, 2002; Leth-Petersen, 2010). Links between credit availability and consumption come from households using credit to smooth income shocks or optimally allocating consumption over time (Jappelli and Pistaferri, 2010; Abdallah and Lastrapes, 2012; Telyukova and Wright, 2008). Durable good purchases are a significant part of personal consumption expenditures and a leading indicator of macroeconomic activity. They are sensitive to credit availability and thus considerably impact monetary policy effectiveness (Barsky et. al., 2007; McKay and Weiland, 2020; Sterk and Tenreyro, 2019). The link between credit availability and consumer durable good purchasing is our primary motivation in this analysis.

In this paper, we empirically investigate whether an exogenous increase of available credit allows households to purchase new, often costly, home durable goods (e.g., refrigerators, ovens, and dishwashers).[1] 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

[1] Figure A1 provides some suggestive evidence that credit constraints impact low-income households. The figure shows the growth in durable goods wealth held by the bottom 50% of the income distribution. The two big spikes are towards the end of state level banking deregulation and the mid-2000s, when the Community Reinvestment Act enforcement increased lending to lower income neighborhoods (Saadi, 2020).

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. 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 1989.

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.[2] Using self-reported information on mortgage refinancing and new

[2] Favara and Imbs (2015) establish increases in mortgage credit into the mid-1990s and early 2000s, as different states finalized their deregulatory actions.

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 home durables consumption. Our results show that state-by-state gradual introduction of more lenient bank branching regulations led to an increase in a number of durable good purchases. In order to attenuate potential concerns related to other factors simultaneously influencing household spending, we include a battery of pre- and post-treatment indicators (event study design). The inclusion of pre-trend dummies acts like a 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 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.

Jayarathe 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.[3]

[3] 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

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. [4] Our results suggest that lower-income households benefited the most in our sample from relaxed regulation constraints, as consumption of durables has the most robust effect on this cohort.

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 the bottom three deciles in their state of residence the year before the deregulation as the "low-income" group. We then interact our low-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 lower-earning households were slightly more likely to increase purchases.

As discussed above, we found that banking deregulation enabled low-income 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

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).

[4] 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.

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 low-income households living in recently deregulated states consume statistically significantly less natural gas annually. Complementary to this idea, we also run our model with a binary dependent variable labeled "cold" reflecting a question asked to households on whether or not they recall being "uncomfortably cold last winter for 24 hours or more because the heating equipment broke down." We find evidence suggesting that low-earning households felt less cold in their homes once their state of residence had been treated.

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.

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 supporting evidence that credit

constraints played a role in limiting the spread of energy efficient durable goods. While we find evidence that credit constraints matter for home durable goods, Ankney (2021) shows that the interest rate on auto loans is not correlated with the fuel efficiency of the car being purchased. This result is interpreted as implying that credit constraints do not impact the fuel efficiency of car purchases.

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, the findings of this paper should be useful to policy makers and academics alike by contributing to two heavily debated topics of interest, the first relating to the climate crisis. 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; COP26 Summit, 2021). Climate risks are important for macroeconomic stability thus central banks might encourage lending to projects that reduce emissions. This research shows that predominately low-income 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).

Second, this paper adds value to consumption literature by explaining the observed increases in aggregate durable home good purchases that arise during a form of financial expansion. Assessing the factors driving the rise in durable good spending are important in understanding shifting consumer preferences and the efficacy of increased purchasing ability on an economy. Since the late 1980's, growth trends in durable home good stalk have prominently peaked twice in the U.S. – one in the early 2000's and one about two decades later in 2020. As mentioned previously in this section, the spike in late 2004 was a result of the Community Reinvestment Act enforcement increasing lending to lower income neighborhoods (Saadi, 2020). Durable good consumption rose again to historic levels during the COVID-19 pandemic; driven largely by federal payments fueling personal savings levels (a direct effect) and households substituting spending on services for durables during the lockdown (an indirect effect) (Tauber and Zandwegde, 2021).

These results dovetail with previous literature suggesting that 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.

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 statues 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 mortgage credit grew at a faster rate and interest rates on new mortgages fell in deregulated states, relative to states yet to deregulate.

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 mortgage credit (which includes refinancing activity) 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.[5] Accordingly, we argue that simultaneously observing post-deregulation increases in mortgage 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

[5] 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.

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 home 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 recent purchases of household appliances. Specifically, if a household's newest refrigerator, dishwasher, oven, laundry washer, and/or dryer is less than 5 years old. The date of the purchase is measured with error, which in a difference-in-differences approach complicates the assignment of the outcome (purchase) 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 before and after the banking deregulation.

Our sample consists of households interviewed between 1981 and 1989 (some specifications cover up to 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 exclude households whose total annual income is listed as negative.

The final sample size varies according to the different specifications.[6] However, in Appendix A, Table A1, we present basic descriptive statistics of all variables used in our regressions. Over the period considered, 13% of households in our sample purchased a new dishwasher, 16% purchased a new dryer, and 19% purchased a new oven. A slightly greater proportion of households (20% and 25%) acquired a new laundry washer and a new refrigerator, respectively. All specifications 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 12% graduated from college. The average household size is 2.66 members and their household income is about $35,000 in 2015 dollars. In every regression, we additionally include the annual average coincidence index for each state as a measure of economic activity.[7] 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 low-income group households within each state

[6] 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.

[7] 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.

before the banking deregulation to see whether purchases of goods and equipment differ over the income distribution.[8] 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 lower-earning households consists of those earners in the 1st, 2nd, and 3rd decile groups within the state in the year before the deregulation.

The AHS collects information on energy expenditure, which we use to check whether newer durable goods drive energy use up or down. Lastly, the AHS includes a question that asks households if they recall being uncomfortably cold for at least 24 hours during the previous winter season. We use this as a dependent variable to further analyze how banking deregulation affected energy use across low-income households.

[8] 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.

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 A 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 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.[9]

We include households with positive annual income, while excluding 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.

[9] 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.

4 Empirical Methods

We start our analysis with the purpose of confirming the mechanism linking banking deregulation to credit availability by using aggregated data on mortgage lending from the Home Mortgage Disclosure Act (HDMA) and by using survey responses about interest rates on new mortgages. For this purpose, we run two separate regressions in which the dependent variable is either new mortgage lending at the bank-state-year level or the average interest rate on new mortgages at the state-year level (descriptive statistics for these variables are reported in Table A1 in the Appendix). We include state- and institution-level (when appropriate) fixed effects in these regressions and cluster the standard errors at the state level.

After establishing the link between banking deregulation and credit expansion, we move on to our main empirical analysis. This uses 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:

$$Y_{hst} = B_1 Post\ deregulation_{st} + B_2 X_{st} + B_3 Z_{ht} + \alpha_h + \rho_t + \varepsilon_{hst} \qquad (1),$$

where Y_{hst} represents different outcomes for household h in state s, at year t. We start by defining Y_{hst} as a dichotomous outcome variable, turning on if the household purchased the durable good in question. We then analyze how these durable purchases, through the banking deregulation variables, impacted energy use. In these specifications, Y_{hst} becomes a continuous variable measuring energy usage at the household level.

Each specification accounts for household fixed effects α_h and survey year fixed effects ρ_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. We report standard errors clustered at the state-level (the result are identical when clustering at municipal area level).

Finally, we conclude the empirical analysis by looking at the link between banking deregulation and time spent on household chores. Due to data availability issues discussed in detail below, this part of our analysis relies on a matching approach instead.

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.[10] 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).

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.

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.

[10] 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: 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 (≥4)				4	14	34	46

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 households who did not move, 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 affected high-income households more than low-income households.

There has been much discussion in the literature recently about treatment effect estimation in a difference-in-difference setting. Goodman-Bacon (2021) warns of incorrect signs with heterogeneous timing of treatments and a single treatment variable, de Chaisemartin and D'Haultfœuille (2020) warn of negative weights on some observations when treatment effects increase over time, and Baker et. al. (2021) show that all treatment estimates can be biased if the sample uses data when there are no/few control observations remaining. For this analysis, an event study specification is shown for samples which include a good amount of control observations to remedy the Goodman-Bacon (2021) and Baker et. al. (2021) concerns

respectively. 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. Additionally, Sloczynski (forthcoming) warn of bias due to the weighting of treatment effects by percent of observations in treatment versus control.

5. Results

5.1 Banking deregulation and credit availability

Our research question is based on the idea that banking deregulation affected credit access 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 Home Mortgage Disclosure Act (HDMA) data on mortgage lending and mortgage-related questions included in our survey. We first estimate the impact banking deregulation had on mortgage lending activity, using a specification similar to one in Sun and Yannelis (2016). We use HDMA data for the same period as our main AHS sample (1981-1989) to calculate new mortgage lending at the institution-state-year level.[11] We then estimate a regression where the outcome variable is the log of total mortgage lending by a given institution in a given state in a given year and include institution, state and year fixed effects in the specification.

We also use available data on mortgage and home purchase year in the AHS to identify all new mortgages (home purchase and refinance) to calculate the average interest rate (in basis points) on new mortgages at the state-year level.[12] We then estimate the impact of deregulation on the average mortgage rate at the state level, again in a specification similar to the one used by Sun

[11] In addition to aligning our HMDA sample with our AHS data, stopping at 1989 also has the advantage of avoiding the potentially confounding mortgage-refinancing boom of the early-1990s, driven by low monetary policy rates (Lam and Kaul, 2003). Also, please note that unlike the more recent vintages, public HMDA data from the 1980s only includes originations, but not denied/withdrawn mortgage applications.

[12] Although AHS includes questions on the mortgage rate and the mortgage origination year, it does not ask about the mortgage amount at origination.

and Yannelis (2016).[13] A negative and significant coefficient on interest rate would provide supplementary evidence of an expansion of credit.

Results reported in Table 2 support the view that banking deregulation improved credit access for households. Looking at the regression using the log of mortgage credit as its dependent variable, banking deregulation is associated with faster credit growth during the post-deregulation period. As discussed in Appendix B, converting these coefficients into annualized growth rates involves taking into consideration both compounding over multiple years and the fact that each pre- or post-deregulation indicator variable covers more than one year.[14] Once the coefficients on the indicator variables are converted into approximate annual growth rates in this manner, we conclude that the impact is strongest during two-to-three years after deregulation ($Post\ deregulation(2,3)$) and starts to diminish afterwards.

We also conclude that banking deregulation is associated with lower interest rates on new mortgages. We again observe that impact is strongest during the $Post\ deregulation(2,3)$, where deregulation is associated with, on average, a 73-basis point reduction in the interest rate on new mortgages (relative to a mean of 9.79%). Similar to our results on new mortgage loans, the impact becomes weaker (and statistically insignificant) after the fourth year following deregulation.

[13] 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).

[14] Such a specification for indicator variables is necessary due to the nature of our household sample from the AHS. While we could construct indicator variables for each individual year in our mortgage credit-related samples (such as …-2, -1, 0, +1, +2, …), we maintain the multi-year definition in order to stay consistent with our main empirical analysis discussed below.

Table 2: The Impact of Banking Deregulation on Credit Expansion

	ln(New Mortgage Credit)	Approximate Growth Rate	Mortgage Rate
	Coef. (std. error)	Estimate (ppt)	Coef. (std. error)
Pre-treatment (≤-3)	0.088 (0.058)	0.025	61.17 (62.59)
Post deregulation (0,1)	0.085** (0.036)	0.042	-51.19** (24.68)
Post deregulation (2,3)	0.318*** (0.074)	0.072	-72.70* (43.03)
Post deregulation (≥4)	0.461*** (0.141)	0.069	-100.06 (69.43)
Observations	80,351	80,351	289
R-squared	0.69	0.69	0.26

Notes: This table shows estimate from two separate regressions of the effect of banking deregulation on credit expansion using difference-in-differences methodology. "ln(New Mortgage Credit)" captures the total amount of mortgage originations by institution *i* in state *s* in year *t* , while "Mortgage Rate" is the average interest rate on new mortgages in basis points for each state *s* in year *t*. All indicator variables for before and after deregulation as specified in the manner discussed above. The column "approximate growth rate" for the new mortgage credit regression displays the interpretation of the coefficient as an approximate annualized growth rate (see Appendix B). New mortgage credit regression includes institution, state and year fixed effects, which the mortgage rate regression includes state and year of mortgage fixed effects. Standard errors 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.

Overall, we confirm the findings of many existing studies in the literature and conclude that banking deregulation is likely associated with increased access to mortgage credit. Furthermore, as our "new mortgage" data involves refinancing activity, we assert a link between this expansion of mortgage credit and the type of spending we are interested in.[15] This assertion is based on previous studies that have found evidence of households using home equity extraction for consumption expenditures, especially during our sample period (Manchester and Poterba, 1989).

[15] Unfortunately, neither HMDA nor AHS allow us to separate mortgage refinancing from new home buying activity.

5.2. Banking deregulation and consumption of home durables

Tables 3 below displays 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 and whether this is more pronounced for low-income groups.

The coefficients shown in Rows 1 and 2 test if pre-treatment trends are impacting our results. The inclusion of pre-treatment indicators in this baseline regression act as falsification test. This pre-treatment period dummies 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 are actually driven by pre-trends in purchasing behaviors. Across all columns we found small, insignificant results. Rows 3, 5, 7, and 9 display results from the event-study approach with interactions with low income household dummy. Our strongest results associated with this covariate of interest are found in the post periods down Column 1, suggesting that durable goods consumption grows for all households in our sample by approximately 10 percentage points immediately after the reform, and between roughly 2-4 percentage points during each year following.

Table 3: The impact of banking deregulation on home durable purchases

	(1) Fridge	(2) Clothes Washer	(3) Clothes Dryer	(4) Oven	(5) Dishwasher
Pre-treatment (>-2)	-0.056	-0.077	-0.057	0.008	0.009
	(0.056)	(0.057)	(0.042)	(0.045)	(0.046)
Pre-treatment (>-2)* Low-Income Household	-0.132	-0.039	0.018	0.010	0.013
	(0.098)	(0.051)	(0.017)	(0.052)	(0.051)
Post deregulation (0,1)	0.092*	0.015	-0.008	-0.007	-0.052**
	(0.048)	(0.058)	(0.046)	(0.019)	(0.024)
Post deregulation (0,1)* Low-Income Household	0.231	0.063***	0.065***	0.027	0.076***
	(0.218)	(0.017)	(0.018)	(0.017)	(0.010)
Post deregulation (2,3)	0.180*	0.029	-0.012	-0.023	-0.061
	(0.096)	(0.115)	(0.091)	(0.37)	(0.046)
Post deregulation (2,3)* Low-Income Household	0.023	0.045***	0.052***	0.019	0.056***
	(0.196)	(0.014)	(0.011)	(0.015)	(0.011)
Post deregulation (4,5)	0.279*	0.049	-0.004	-0.042	-0.097
	(0.144)	(0.174)	(0.137)	(0.056)	(0.070)
Post deregulation (4,5)* Low-Income Household	0.024	0.080***	0.079***	0.038	0.80***
	(0.038)	(0.026)	(0.016)	(0.028)	(0.015)
Post deregulation (6,7)	0.415**	0.106	0.034	0.009	-0.63
	(0.192)	(0.231)	(0.182)	(0.075)	(0.094)
Post deregulation (6,7)* Low-Income Household	0.028	0.060***	0.063***	-0.038*	0.074***
	(0.020)	(0.021)	(0.019)	(0.019)	(0.015)
Observations	45,532	45,532	45,532	45,532	45,532

Notes: This table shows estimates of the effect of banking deregulation on the probability of purchasing five durable goods using an event study difference-in-difference specification. Only the samples from surveys in 1985, 1987, and 1989 are utilized. Each regression includes household and year fixed effects and control for annual state coincidence index (GDP). Robust standard errors are in parentheses below each coefficient and were clustered at the county level; *** p<0.01, ** p<0.05, * p<0.1.

To avoid the possible issue of negative weights when treatments occur over time and in different states, which has been highlighted in the recent literature on the difference-in-differences, we focus on those states that deregulated in 1986 or 1987 only and run a standard triple difference-in-difference. This specification confirms that low-income earners tended to purchase more consumer durables after the deregulation, with respect to those who live in states that deregulated much later. Note also that this alternative specification shows an increased likelihood of appliance purchase for low-income households, in line with the results from Table 3.

Table 4: 1987 Deregulation States Impact on Durable Purchases

	(1) Fridge	(2) Clothes Washer	(3) Clothes Dryer	(4) Oven	(5) Dishwasher
Difference in Difference (DiD) Term	0.064	-0.015	-0.022	0.063**	-0.001
	(0.044)	(0.028)	(0.019)	(0.029)	(0.022)
DiD Term* Low-Income Household	0.036**	0.047***	0.054***	0.049***	0.061***
	(0.015)	(0.013)	(0.011)	(0.011)	(0.008)
Observations	49,448	49,448	49,448	49,448	49,448

Notes: This table shows estimates of banking deregulation specification on the consumption of five durable goods using a difference-in-differences estimator where states that deregulated in 1986 or 1987 are considered treated and all those deregulating after 1987 are considered controls for the whole sample. Each regression includes household and year fixed effects, and control for annual state coincidence index (GDP). Robust standard errors are in parentheses below each coefficient and were clustered at the county level; *** p<0.01, ** p<0.05, * p<0.1.

As a robustness check, we reran all five of these main regressions including additional control variables and found no significant changes in the outcomes. These coefficient results are reported in Appendix A, Table A3 of this paper.

5.5 Discussion of Mechanisms

We explore a couple of potential mechanisms leading to this increase in durable good purchases here. The first is to estimate a model where household income is the dependent variable and the treatment variables are independent variables. 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.

Table 5: The Impact of Banking Deregulation on Income

	(1)
Pre-treatment (>-2)	3,306***
	(1,223)
Post deregulation (0,1)	-735
	(893)
Post deregulation (2,3)	-2,717
	(1,684)
Post deregulation (4,5)	-3,933
	(2,553)
Post deregulation (6,7)	-2,234
	(3,419)
Observations	45,432

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.

Another potential mechanism is more on the supply of durable goods. If banking deregulation allowed firms easier access to credit, more retail shops selling durable goods may have come into existence lowering the markup over wholesale prices leading to lower prices for households. The Bureau of Labor Statistics complied durable good price indices for some Metropolitan Statistical Areas from the mid-1980s. Figure A2 reveals the evolution of these price indices with time re-centered around when the state deregulated their banking sector. There is not a clear pattern of price decreases after deregulation relative to before thus it seems unlikely our results are driven by a supply increase leading to a durable good price decrease.

6. Banking deregulation, energy and labor savings

In this section, we investigate potential benefits associated with increased uptake of home durables. Given the time period of banking deregulation coincides with the first energy efficiency standards for appliances, the increase in propensity to purchase new appliances may lead to lower energy bills. Lower energy consumption has also the additional external benefit of lower pollution emissions from the electricity demanded by the household. On the other hand, if there were new appliances that replaced a less energy intensive use (clothes dryer versus air drying) 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.

The first two columns of Table 7 and 8 shows estimates from separate regressions of monthly electricity and gas expenditure (as reported in the AHS survey) on the interaction between the banking deregulation and low-income group. Table 7 uses observations from the 1985, 1987, and 1989 surveys while Table 8 utilizes only the states deregulating in 1986 or 1987 with all other later deregulating states being treated as controls. These results suggests that low-income households in our sample spent less on monthly gas, on average, after credit constraints were relaxed. There seem to be not statistically significant increase in the electricity bill either.

Table 7: The impact of deregulation on monthly energy expenditure on low-income households

	(1) electricity	(2) gas	(3) cold
DiD Term * Low-Income Households	1.975	-2.944**	-0.011
	(1.249)	(1.526)	(0.010)
Observations	64,940	44,348	47,472

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) 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.

Taken together, the results shown in Tables 7 and 8 suggest that the relaxation of credit constraints did not indirectly impact on the extensive margin of the energy use. If anything, there is evidence of more efficient energy consumption following banking deregulation. An additional robustness test was carried out by using an indicator for whether the respondent recalled being uncomfortably cold for at least 24 hours during the previous winter season. The estimated coefficient is negative in both samples but not statistically significant in both samples, providing some evidence that banking deregulation allowed households more ability to pay utility bills during extreme weather conditions.

Table 8: 1987 deregulation impact on monthly energy expenditure on low-income households

	(1) electricity	(2) gas	(3) cold
Did Term * Low Income Households (t=1987)	0.619	-1.549	-0.018*
	(1.302)	(1.174)	(0.010)
Observations	61,991	49,454	50,495

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) 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.

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.

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: 1992-1994 and 1998-2000. Our primarily 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 1992-1994 and 1998-2000, average time spent on domestic activities will decline in later deregulating states relative to early deregulating states.[16] We define the 12 "late-deregulating" states as those that deregulated in 1988 or later. The remaining 36 states form the "deregulated earlier" group.[17]

Given the repeated cross-sectional nature of AHTUS (and the inconsistencies in the availability of individual-level variables between the two survey waves), we are unable to use the same difference-in-differences specification as in our main empirical analysis. Instead, we utilize a nearest neighbor matching approach to calculate individual "difference" estimates for each survey wave. Specifically, we match each household living in a late deregulating state to a household living in an earlier deregulating state, based on seven characteristics: (i) age of survey respondent, (ii) number of adults in the respondent's household, (iii) whether there are any children in the household, (iv) gender, (v) employment status (working vs. not working), (vi) whether the survey was completed on a weekday or weekend and (vii) region of the respondent's state ("Northeast", "Midwest", "South" and "West"). We require the match to be exact for characteristics (iii)-(vii), while looking for the nearest value for (i) and (ii). We also use the survey weights provided by AHTUS in our estimation.

We perform this matching procedure twice, once for each survey wave (1992-1994 and 1998-2000). The average difference between time spent on domestic chores (in minutes during the past 24 hours) by a survey respondent living in a late deregulating states and by the matched respondent in an earlier deregulating state is the "difference" estimate (or, the "average treatment effect on the treated"). Since this procedure involves two different cross-sectional

[16] 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.
[17] 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). We exclude Alaska and Hawaii because they are not in the AHTUS survey.

samples, it is not practical to calculate a difference-in-differences estimate; however, comparing the two difference estimates can provide us with a picture of how differences in time spent on domestic chores evolved between the two survey waves.

The results of our nearest neighbor matching estimation are in Table 9. We find that residents of late deregulating states were spending an average of 16.94 more minutes per day on domestic tasks during the period immediately after deregulation. However, by 1998-2000, which is arguably after the impact of deregulation has been fully felt in these states, this gap is a statistically insignificant 1.29 minutes per day (on average). We interpret the 15.65-minute a day reduction in the gap as an outcome of the banking deregulation and the subsequent relaxation of the borrowing constraint fully taking effect between 1992-1995 and 1998-2000. This corresponds to 3.97 fewer days per year spent on domestic tasks by the residents of the twelve late deregulating states relative to those living in states that deregulated earlier.

Table 9: The impact of banking deregulation on time spent on domestic tasks

	N	Mean Difference	Standard Error
1992-1994	391	16.94***	2.576
1998-2000	160	1.297	2.144

Notes: This table shows nearest neighbor matching estimates for the effect of banking deregulation on time spent on domestic tasks. The two "difference" estimates use two different waves for the AHTUS survey. "Mean Difference" is between the "recently treated" households living in twelve late deregulating states and their matched households living in states that deregulated earlier. "Mean Difference" and its standard error were corrected using the methods outlined in Abadie and Imbens (2006, 2011); *** $p<0.01$.

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 new home durables purchases among the low income households.

Next, we show decreasing average annual natural gas 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 suggestive evidence in

favor of households that recently experienced banking regulation spending less time on domestic chores relative to 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
Durables					
New Dishwasher	45,526	0.13	0.34	0	1
New Laundry Washer	45,526	0.20	0.40	0	1
New Oven	45,526	0.19	0.39	0	1
New Fridge	45,526	0.25	0.43	0	1
New Dryer	45,526	0.16	0.37	0	1
Credit Expansion					
Ln New Mortgage Credit	80,351	8.09	2.01	0	21.19
Mortgage Rate	289	1034	177	500	2100
Household/respondent's					
Age	45,526	37.7	16.72	19	91
Married	45,526	0.56	0.49	0	1
Widowed	45,526	0.14	0.35	0	1
Divorced	45,526	0.12	0.33	0	1
Separated	45,526	0.04	0.19	0	1
Single	45,526	0.14	0.34	0	1
College degree	45,526	0.12	0.31	0	1
Household income	45,526	34,862.52	29,678.42	0	400,000
Low-income Household	45,526	0.12	0.32	0	1
Household Size (Occupants)	45,526	2.66	1.52	1	22
State-level variables					
Coincidence index	45,526	61.18	10.91	30.48	90.74
Price of natural gas	44,348	5.41	0.98	2.87	16.10
Price of electricity	64,940	23.52	4.96	6.73	35.57
Energy expenditure					
Monthly gas expenditure	44,348	46.14	34.70	0	197
Monthly electricity expenditure	64,940	61.80	43.35	1	396
Uncomfortably Cold Prior Winter					
Cold	47,472	0.0849	0.279	0	1

Table A2: States in the sample and banking deregulation year

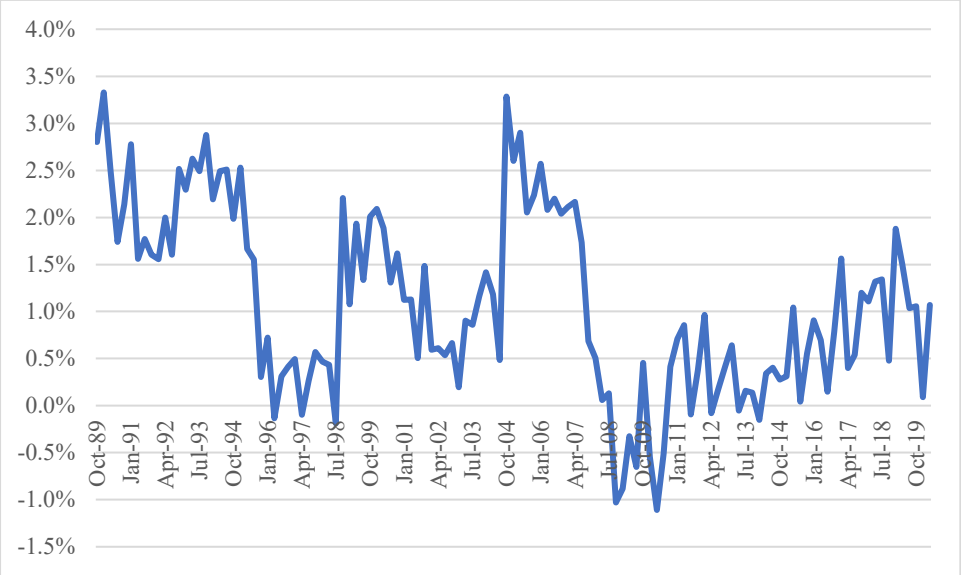
State/Abbreviation	Year Interstate Banking Permitted
Alabama – AL	1987
Arizona – AZ	1986
Arkansas – AR	1986
California – CA	1987
Colorado – CO	1988
Connecticut, CT	1983
Florida – FL	1985
Georgia – GA	1985
Hawaii – HI	1985
Illinois – IL	1986
Indiana – IN	1986
Iowa – IA	1991
Kansas – KS	1992
Kentucky – 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

Table A3: The Impact of Banking Deregulation with Income + Household Controls

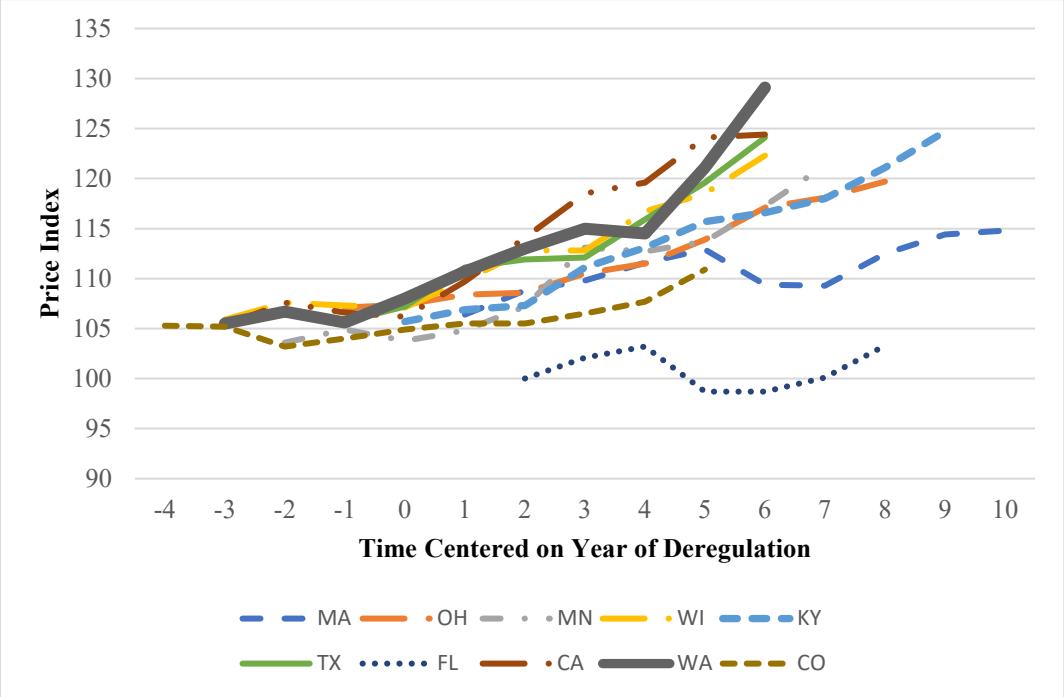
	(1) New Fridge Years < 1991	(2) New Laundry Washer Years < 1991	(3) New Clothes Dryer Years < 1991	(4) New Oven Years < 1991	(5) New Dishwasher Years < 1991
Pre-treatment (>-2)	-0.0623	-0.084	-0.058	0.005	0.008
	(0.0572)	(0.057)	(0.042)	(0.045)	(0.047)
*Pre-treatment (>-2)*Low-Income HH*	-0.128	-0.033	0.0264	0.014	0.019
	(0.102)	(0.0523)	(0.017)	(0.052)	(0.053)
Post deregulation (0,1)	0.0974**	0.021	-0.008	-0.003	-0.052**
	(0.048)	(0.057)	(0.045)	(0.018)	(0.025)
Post deregulation (0,1)*Low-Income HH	0.020	0.062***	0.066***	-0.017	0.070***
	(0.022)	(0.017)	(0.018)	(0.035)	(0.011)
Post deregulation (2,3)	0.188*	0.040	-0.013	-0.006	-0.061
	(0.096)	(0.11)	(0.089)	(0.089)	(0.048)
Post deregulation (2,3)*Low-Income HH	0.020	0.044***	0.055***	0.018	0.056***
	(0.019)	(0.014)	(0.011)	(0.015)	(0.015)
Post deregulation (4,5)	0.291**	0.065	-0.005	0.005	-0.100
	(0.144)	(0.171)	(0.135)	(0.135)	(0.073)
Post deregulation (4,5)*Low-Income HH	0.016	0.074***	0.080***	0.018	0.078***
	(0.039)	(0.026)	(0.015)	(0.015)	(0.015)
Post deregulation (6,7)	0.430**	0.127	0.033	-0.033	-0.061
	(0.192)	(0.227)	(0.179)	(0.053)	(0.100)
Post deregulation (6,7)*Low-Income HH	0.023	0.060***	0.064***	-0.043**	-0.072***
	(0.021)	(0.022)	(0.019)	(0.020)	(0.015)
Observations	45,526	45,526	45,526	45,526	45,526
Household Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects FE	Yes	Yes	Yes	Yes	Yes

Figure A1: Quarterly Growth in Low Income Durable Stock



Source: Bureau of Labor Statistics

Figure A2: Impact of Deregulation on Durable Good Prices



Source: Bureau of Labor Statistics

Appendix B

In this appendix, we present a brief discussion of how the estimated coefficients of the "ln(New Mortgage Credit)" regression (Section 5.1, Table 2) can be interpreted as approximate annualized growth rates.

For each indicator variable ($Pre\ treatment\ (\leq -3), Post\ deregulation(0,1)$, etc.) the coefficient represents the average *cumulative* growth in new mortgage credit at the state-bank level, between the particular period and the reference period (one-to-two years before deregulation). However, while converting this cumulative growth rate into an annualized growth rate, we also need to take into consideration that each indicator captures a multi-year window. The reference period, $Post\ deregulation(0,1)$ and $Post\ deregulation(2,3)$ each cover two years, while the number of years covered in $Pre\ treatment\ (\leq -3)$ and $Post\ deregulation(\geq 4)$ vary based on the year of deregulation in a given state.

Accordingly, we interpret the coefficient of $Post\ deregulation(2,3)$ as the average cumulative growth rate over a *four year* span, relative to the reference period of (-1,-2). We obtain the four year span as the distance between the midpoints of the two windows (-1.5 to 2.5). We can then come up with an approximate annualized growth rate by solving $(1+i)^4 = (1+0.318)$. This yields the annual growth rate of 7.2% given in the second column of Table 2. We follow a similar approach for the coefficient of $Post\ deregulation(0,1)$.

For the remaining two indicator variables, $Pre\ treatment\ (\leq -3)$ and $Post\ deregulation(\geq 4)$, we rely on the average number of years observed for each window in our sample. The average number of years in our sample for the $Pre\ treatment\ (\leq -3)$ window is 3.4, so we interpret the coefficient of this indicator as a cumulative growth rate over a *3.4-year span*. Meanwhile, for $Post\ deregulation(\geq 4)$, we have an average of 1.7 years in the sample, so the coefficient of this indicator variable can be thought of as the cumulative

growth rate over a *5.7-year span* (a window starting in the 4th year after deregulation, with an average width of 1.7 years). Converting the cumulative growth rates implied by the regression coefficients by using these timespans (i.e. $(1+i)^{3.4} = (1+0.088)$ and $(1+i)^{5.7} = (1+0.461)$) yields the approximate annualized growth rates given in the second column of Table 2.

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ISSN 2194-2188