Essays on Household Finances, Consumption & Individual Mental Health

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Abstracts

Chapter I: 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.

Chapter II: We explore the effects of hydraulic fracking booms on individual mental health by exploiting geological variation across U.S. shale plays. Utilizing a difference-in-differences specification to infer mental health outcomes, we find evidence that fracking leads to general declines in mental wellbeing. Coefficient results associated with depression, bad mental days, and above average bad days are positive and robust, indicating a negative impact of the boom-and-bust cyclical natural on mental health for an entire population. We also find some evidence suggesting individuals are more prone to alcohol abuse following a fracking boom. Estimating the heterogeneous treatment effects of subgroups within our sample pool, we shed some light on which demographics are more susceptible to experienced worsened mental health conditions spurred by the implementation of intense local fracking. Effects are largest and most consistent for those who self-reported as: (i) married, widowed, or single (associated with marital status); (ii) unemployed, a homemaker, or a student (associated with employment status), and (iii) female (relative to male counterparts).

Chapter III: Literature investigating the impact of credit constraints on durable good consumption has been expanding rapidly. This paper looks at the elimination of auto loan cramdowns for Chapter 13 bankruptcy proceedings on three outcomes related to automobiles: asset value over time, number of autos in the household, and loan-to-value (LTV) ratio of new autos. Using a difference-in-differences framework based on a state's historical use of Chapter 13 bankruptcy, we show that moneylenders increase lending to households as a result of lower credit risk following the reform. Further, as a proxy measurement for discrimination, we add wealth and race interactions to our main model and find positive, robust treatment effects of cramdown elimination on autos for low-asset and Black or African American households. Together, these results may indicate that lower risk to lenders leads to more lending across society in terms of equity, inclusion, and diversity.

Declaration

I wish to submit these essays detailed above in accordance with the University of Stirling research degree regulations. I declare that the work shown here represents my own research and was authored by me.

A modified version of Chapter I was made publicly available as a Halle Institute for Economic Research (IWH) discussion paper, which was updated in April 2022. The paper is co-authored by Evren Damar, Ian Lange, and Mirko Moro, all of whom mentored me throughout my PhD process. I confirm that I have contributed over 80% of the research and output for all three of the studies presented in this thesis.

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Dedication

To my husband, Michael Hanley, whose steadfast belief and support has carried me to where I am today. To my father, who never doubted I'd be unable to accomplish anything I set my mind to. To my mother – my confident and best friend.

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Introduction

Credit availability plays an important role in household decision-making. Along this research thread, we've learned that credit expansion provokes upticks in spending levels (Gross and Soulels, 2002), is crucially linked with credit demand (Robertus et al., 2005), and has a robust, rippling effect on the national economy (Leth-Petersen, 2010). At the same time, the big energy transition is on the global horizon, and consumer credit will be a key component in the speed and ease of this major shift towards cleaner usage of natural resources. While this comprehensive change hasn't yet arrived for us – at an aggregate level – these chapters are meant to be informative for future credit and energy policy.

In this thesis, we study the effects of credit supply on U.S. household consumption and hydraulic fracking boom impacts on individual mental health. While the primary focus of this research is to investigate the effects after a credit supply shock across households, the study of the psychological effects caused by a fracking boom is by itself a very relevant topic, especially, if linked to the broader impacts of energy booms and transitions in the local areas affected. The latter subject represents the second chapter's motivation. By linking mental health and shale extraction, we connect two major points of interests that decision-makers are currently focusing their efforts on. While mental health has become a vital talking point across legislators just as of late, we can assume both matters will continue to be hotly debated topics in politics for the foreseeable future. We find systematic decline of mental health for those individuals living within a county where a fracking boom has erected – relative to individuals residing in fracking counties of less production.

The chapters of this thesis are organized as self-contained papers and attempt to estimate causal effects by exploiting naturally occurring experiments which assign credit supply shocks or local-area booms to individuals. In other words, we take care of measuring the effects of treatment on the treated given arguably exogenous assignments. Chapter II assigns individuals into treatment based on concrete geological features located beneath the earth's crust where their residency's lie above. Chapters I and III carry treatment definitions driven by policy enactments and act as bookends of this thesis. Findings in the first and last of these studies speak to the impacts of credit expansion by providing some empirical evidence on spending decisions of household members once those are lifted. Credit availability is investigated through two channels associated with: (i) interstate banking deregulation; and (ii) the Chapter 13 bankruptcy reform on auto lending. These legislative acts increased the purchasing power of households through inclusionary expansion to credit (Chapter I) and more neutral loan attainment for auto assets (Chapter III), respectively, as our results indicate households acquired newer (likely more energy efficient) durable goods after these policies were implemented.

Chapter I: Banking Deregulation and Consumption of Home Durables

1.1 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). As a component of our research methods, we measure the impact across the sample's income distribution by differentiating between low-income households relative to all other earners. 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 also 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 to energy related benefits, 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.[1]

Our empirical analysis follows the broad consensus reached by previous studies and exploits the removal of bank branching restrictions in the U.S. during the 1970s and 1980s to identify exogenous changes to the availability of consumer credit at the household level. Previous research motivated by banking deregulation and its associated effects is vast, as the staggard timing of state-level changes provides ideal conditions to empirically explore. At a high-level, most studies have concluded implementation of this regulatory change solved market failures and spurred overall better economic performance (see, for example, Jayarante and Strahan, 1998; Strahan, 2003; Stiroh and Strahan, 2003; Demyanyk, 2008; and Huang, 2008). More closely aligned with our research motivation, 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.

[1] Bui and Ume (2020) show that banking deregulation lowered hours worked which might suggest an increase in leisure or non-market hours worked.

We combine data on interstate banking deregulation 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 this paper, we focus on interstate banking deregulations because durable consumption is available in the AHS from the mid-80s during which period interstate deregulation were more common. We also argue that interstate banking – in the context of durables consumption – is supported by previous literature findings showing that while intrastate branching and interstate banking deregulation increased the rate of total loan growth and credit card loan growth, tighter competition from out-of-state banks led banks to extend credit to new/previously unserved households (Dick and Lehnert, 2010).

To establish the causal effect between deregulation and consumption of durable, we employ a difference-in-differences approach. Given that information on consumption of durables is available only every two years and for a limited period, we run a difference-in-difference regression by focusing on those households surveyed in the states that deregulated in 1986 and 1987, the treatment group, while those living in the remaining 5 states form the control group. We complement this difference-in-differences analysis by exploiting the staggered introduction of reforms in different states during the period of interest and by running event-study regressions. This strategy allows us to test for pre-trends, but given the limited dataset available, may suffer from biases arising from heterogenous timing of treatments highlighted in, e.g., among others Goodman-Bacon (2021), de Chaisemartin and D'Haultfœuille (2020) and Baker et al. (2022). We use both designs to confirm our results.

As a data validation exercise and 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 in our sample. 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.[2] Combining this evidence with findings from earlier studies which explicitly finds links between home-equity related borrowing and home durables and appliances, we conclude that banking deregulation increased the availability of consumer credit (see for example, Greenspan and Kennedy, 2007; and Cooper, 2009).

We then move on to our main analysis by investigating the impact of credit availability on home durables consumption and estimate the impact of credit supply across the income distributions through carefully constructed state-dependent income deciles. Aggregate data on durable good consumption over time provides some suggestive evidence that credit constraints impact low-income households. Figure A1 in the appendix 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).

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 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

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

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] 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

[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 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.

slightly more likely to increase purchases.

Further, our paper investigates potential consequences associated with the increase in purchase of home durables. First, we ask whether the availability of durables affected energy consumption, given that new appliances are typically more energy efficient. Second, we use time use surveys to explore whether the availability of durables decreased time spent on home chores.

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 reported in the AHS. 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 of banking deregulation on household “labor-leisure” decisions caused by the purchase of potentially labor-saving technologies. We examine the possibility of banking deregulation having a labor-savings effect by using time use 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 deregulated late relative to those that deregulated earlier.

These findings taken together support the argument that relaxation of credit constraints, brought on by banking deregulation, led to labor- and energy-savings across households, which are suggestive of broader net benefits linked with credit availability. As such, our paper contributes to several strands of the literature. First, we improve our understanding of home durables 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. 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 papers by establishing a link between credit constraints and the consumption of "other home 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.

This paper also 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 is 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).

The findings from this research support the idea that credit consumption of durables may affect energy- and labor-savings. These results dovetail with previous literature suggesting that households may 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 align 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. Bui and Ume (2020) investigated time-use effects of the banking deregulation but did not specifically analyze the impact of durables.

To the best of our knowledge, this paper is the first one to link banking deregulation and home energy consumption. This research improves our understanding of the so-called "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 may play a role in limiting the spread of energy efficient durable goods.

Finally, climate risks are important for macroeconomic stability thus central banks might encourage lending to projects that reduce emissions. 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). This paper shows that predominately low-income households may 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 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.

Due to the nature of their design, the introduction of banking deregulation across states acted as a natural experiment – providing plausibly exogenous variation to credit supply.[5] Increases in the supply of credit following banking deregulation have been well documented in the literature. For instance, Jayaratne and Strahan (1996) found some evidence of an increase in credit, though they attribute the increase in economic growth to improvements in bank lending quality. 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.

The literature along this thread distinguishes between *intrastate* and *interstate* banking deregulation. The former refers to branch expansion of bank branches within states, while the latter – interstate banking (the one that was completely barred by legislation) – refers to banks having

[5] As we will describe later, most of the existing papers exploited the staggered nature of reforms across states, while we will employ a difference-in-differences based on states that adopted interstate deregulation in 1987. This is because survey data on durables consumption is available only after 1985 and most of the states in the survey enacted banking deregulations in 1987. We do not see the use of the single treatment period in a difference-in-differences setting given the recent literature highlighting the bias arising from using multiple treatment periods in a two-way fixed effects difference-in-differences (see e.g., our discussion later based on Goodman-Bacon, 2021; Baker et al., 2022; Chaisemartin and D'Haultfœuille 2020).

the ability to consolidate and franchise across state lines, and if successful enough, eventually having the ability to become a national franchise – or national bank – if their business model and customer service is good enough to do so. While some literature on banking deregulation attempt to utilize the implementation of both interstate and intrastate deregulation to estimate impacts on credit markets (see, for example, Beck et al., 2010; and Dick and Lehnert, 2010), we only employ *interstate* banking deregulations to define our treatment because of consumption data is mostly available for those states over a period of time characterized by a larger number of interstate deregulations. Interstate deregulations increased competition from larger out-of-state banks – which contrast with intrastate banks which favored merger and acquisitions within the state. This increase competition represents a lower entry barrier and an expanded array of new lenders from which household could borrow, presumably to purchase durable goods (Chava et al., 2013). Dick and Lehnert (2010) explores the consumer credit channel more explicitly and show that while both deregulations seem to have a similar impact on total loan and credit card loan growth, interstate deregulation resulted in a certain type of competition that led banks to extend credit on the extensive margin -- as opposed to the intensive margin -- by lending to previously excluded households. What is important here is to highlight that previous literature has established quite firmly that interstate banking deregulation relaxed credit constraints.[6]

As a validation exercise, we document in the next section that banking deregulation increased credit availability using our sample, since mortgage credit grew at a faster rate and interest rates on new mortgages fell in deregulated states, relative to states yet to deregulate (we elaborate more

[6] For instance, 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.

about the link between new mortgages and durables below).

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. There is a rich literature looking at a variety of important outcomes. 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.[7] 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 AHS and the date in which individual states enforced legislative changes reforming interstate banking. 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

[7] 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.

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.

Our durable consumption sample consists of households interviewed between 1985 and 1993. As the survey is administered every two years, we observe five survey years in total. We chose this time frame because the U.S. experienced the largest movement of individual states relaxing their interstate 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. [8] 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

[8] 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.

every regression, we additionally include the annual average coincidence index for each state as a measure of economic activity.[9] 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 before the banking deregulation to see whether purchases of goods and equipment differ over the income distribution.[10] 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.

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 the banking deregulation for each state as the date on which

[9] 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.

[10] 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.

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. After accounting for these omitted areas, our sample is left with 38 states in total. Our main analysis focuses on 18 states that deregulated in 1986 and 1987 with 5 states used as control group. Given that this survey is biennial, we link these deregulation dates to interviews in 1987 and will refer to this variable as 'Post 1987 deregulation' for simplicity. The event-study use the full sample of 38 states, with most states being treated by the end of the period available. [11]

Finally, we exclude from the sample 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 start our analysis with the purpose of confirming the mechanism linking banking deregulation

[11] 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.

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.

Our main analysis is based on a difference-in-difference regression focusing on states that deregulated in 1986 and 1987 and linked to surveys occurred in 1987. We run several models of the following form:

$$Y_{hst} = \alpha + B_1 Treat_{hs} + B_2 Post_{\,t} + B_3(Post * Treat)_{hst} + B_4 X_{st} + B_5 Z_{ht} + \alpha_h + \rho_t + \varepsilon_{hst} \quad (1),$$

where Y_{hst} represents different outcomes for household *h* in state *s*, at year *t*. We start by defining Y_{hst} as a dichotomous outcome variable, turning on if the household purchased a 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. $Treat_{hs}$ represents out treatment group and is defined as any state that experienced the policy change in 1986 or 1987. $Post_{\,t}$ is a dummy variable that equals one in the years after state s deregulates and equals zero otherwise. The coefficient attached to B_3 identifies the impact of banking deregulation on our outcomes.

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

To test for the idea that the reform may have affected low-income households differently than low-income households, we run triple difference-in-differences of the following form:

$$Y_{hst} = \alpha + B_1 Treat_{hs} + B_2 Post_{\ t} + \ B_3(Post\ * Treat)_{hst} + B_4(Post * Treat * Low\_income)_{hst} + B_4 X_{st} + B_5 Z_{ht} + \alpha_h + \rho_t + \varepsilon_{hst} \qquad (2),$$

where all variable specifications remain consistent with equation (1) outside of the coefficient associated with B_4. This triple interaction includes a third indicator variable represented as Low_income_{hs} that turns on if household h in state s is classified into the 1st, 2nd, or 3rd lowest earning deciles, and remains zero otherwise.

The difference-in-difference design in (1) and (2) do not allow us to test for pre-trends because the first available year for durable consumption is 1985 and most of our sample include states that deregulate in 1986 and 1987. Taking that into account, however, these mechanisms offer a cleaner design and offsets the potential biases arising from adopting staggered difference-in-differences (Baker et al., 2022). Recent literature shows that difference-in-differences – including event-study designs – which exploits the staggered introduction of banking deregulation, may provide biased estimates of the average treatment effect (see, e.g., Sun and Abraham, 2020, Goodman-Bacon, 2021, Baker et al. 2022). With this limitation in mind, we also estimate a more flexible event-study specification as a robustness test. The advantage of this strategy rests on the inclusion of more states (that deregulated between 1985 and 1989) and lead indicators, which estimated parameters allow us to test for pre-trends. The simple difference-in-difference and the event-study designs will then complement each other, and comparing their results add confidence in our findings.

To run the event-study, we augment equations (1) and (2) by including three indicator variables (rather than one) for the reforms. 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.[12] 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

[12] 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.

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.

The last part of the paper focuses on potential benefits linked with home energy use and reduction of domestic tasks arising from the purchase of more home durables. To this end, we run our main regressions in which Y_{hst} captures energy use.

We conclude the empirical analysis by looking at the link between banking deregulation and time spent on household chores. Due to data availability issues, this part of our analysis relies on a matching approach instead, which we describe more specifically later in section 6.

5. Results

5.1 Banking deregulation and credit availability

The central motivation behind our analysis is focused on the idea that banking deregulation affected credit access of households. As previously mentioned, current empirical research addressing this theory fundamentally aligns with this statement. In this section, we provide further evidence affirming this effect utilizing 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 Sun and Yannelis (2016). The HDMA dataset is restricted to observe the same range of years as our main

AHS sample (1981-1989) to calculate new mortgage lending at the institution-state-year level.[13] 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 includes institution, state and year fixed effects in the specification.

Sourcing available data from the AHS, we additionally exploit variation in mortgages and home purchase years to identify all new mortgages (home purchase and refinance). This allows us to assess average interest rates (in basis points) on new mortgages at the state-year level.[14] 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 and Yannelis (2016).[15] A negative and significant coefficient on interest rate would provide supplementary evidence of an expansion of credit.

Coefficient estimates measuring the effects discussed above are shown in Table 1 of this section. Overall, our findings support the view that banking deregulation improved credit access for households. Columns 1 and 4 report estimates from our main difference-in-difference design and columns 2 and 5 display results utilizing the event-study approach. Column 3 provides calculated average interest rates (in basis points) on new mortgages at the state-year level corresponding to the estimates displayed in column 2. Moving across columns 1, 2, 4, and 5, coefficients linked to post-deregulation are positive and statistically significant if associated with mortgage refinement or mortgage credit outcomes and conversely, negative and mostly significant when tied with

[13] 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.
[14] Although AHS includes questions on the mortgage rate and the mortgage origination year, it does not ask about the mortgage amount at origination.
[15] 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).

mortgage rate outcomes.

Table 1: The impact of banking deregulation on credit expansion

	Probability of Mortgage Refinance Coef. (std error)	ln(New Mortgage Credit) Coef. (std. error)	Approximate Growth Rate Estimate (ppt)	Mortgage Rate Coef. (std. error)	Mortgage Rate Coef. (std. error)
Post 1987 deregulation	0.21** (0.09)	--	--	-82.76** (38.24)	--
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)
Dataset	AHS	HMDA		AHS	AHS
Observations	42,187	80,351		182	289
R-squared	0.14	0.69		0.21	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.

Table 1’s robust findings display inverse relationships across dependent variables and confirm both mechanism approaches bolster a similar argument. Two main explanations provide our rational in support of these estimates representing causal effects. First, banking deregulation is linked with a higher likelihood of mortgage refinancing and faster credit growth. On average, the probability of mortgage refinance increased by approximately 21 percentage points (row 1, column 1) for those states that deregulated in 1987. 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.[16] Once the coefficients on the indicator variables are converted into approximate annual growth rates in this manner, we conclude that the impact is strongest two-to-three years after deregulation (row 4, column 3) and starts to diminish afterwards.

Second, the estimates reported in this section provide validation that banking deregulation is associated with lower interest rates on new mortgages. We find that on average, the interest rate decline by nearly 83-basis points (row 1, column 4) following the 1987 deregulation. When looking at the event-study, we again observe that impact is most robust during the $Post\ deregulation(2{,}3)$, where deregulation leads to an average of nearly 73-basis point reduction in the interest rate on new mortgages (relative to a mean of 9.79%) (row 4, column 5). Similar to our results on new mortgage loans, the impact becomes weaker (and statistically insignificant) after the fourth year following deregulation.

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.[17] 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).

5.2. Banking deregulation and consumption of home durables

[16] 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.

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

Section 5.2 considers Tables 2 below displays regression results from our main analysis – focusing on states that deregulated in 1986 and 1987 – 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. Our estimates indicate that the probability of consuming home durable increased by approximately 3.6-6.1 percentage points for low-income groups. We detect no difference in the purchasing habits of other income groups.

Table 2: Impact of banking deregulation on home durable purchases on low-income households

	(1) Fridge	(2) Clothes Washer	(3) Clothes Dryer	(4) Oven	(5) Dishwasher
Post 1987 deregulation	0.064 (0.044)	-0.015 (0.028)	-0.022 (0.019)	0.063** (0.029)	-0.001 (0.022)
(Post 1987 deregulation)* (Low-Income Household)	0.036** (0.015)	0.047*** (0.013)	0.054*** (0.011)	0.049*** (0.011)	0.061*** (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.

5.3. Robustness and pre-trends

We run robustness tests using the event-study approach by exploiting the staggered introduction of reforms in different states over the period for which we have consumption data available (1985 to 1993). As mentioned, this method has the advantage of using a larger sample of states, and the drawback to potentially suffer from biases arising from the different treatment timing and the fact that early control states are treated later. This approach allows us to test for pre-trends, i.e., whether consumption of durables is statistically different in control vs treated states before deregulation.

Table 3: The impact of banking deregulation on home durable purchases – the event-study approach

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

The coefficients shown in Rows 1 and 2 in Table 3 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, statistically insignificant results. Rows 3, 5, 7, and 9 display results from the event-study approach with interactions with low income household dummy. Taken all together, these specifications show results in line with Table 2. For three out of five home durables, these estimates are statistically significant and show that low-income earners tended to purchase more consumer durables each wave after the deregulation. Results for oven follow a similar pattern but their standard errors are larger – perhaps because of loss of power.

5.4 Discussion of mechanisms

We explore a couple of potential mechanisms leading to this increase in durable good purchases here. First, we ask whether the increase in consumption can be explained by an increase in income (following the deregulation) rather than a direct effect of more consumer credit available. To investigate this, we estimate a model where household income is the dependent variable. Given the heterogeneity in incomes by state, this model uses a state fixed effect. Although not reported in Table 4 it is worth mentioning that results for household income become 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 4: The Impact of banking deregulation on income

	(1)	(2)
Pre-treatment (>-2)		532
		(2596)
Post deregulation (0,1)		1710
		(1661)
Post deregulation (2,3)		2478
		(3264)
Post deregulation (4,5)		3568
		(4873)
Post deregulation (6,7)		7059
		(6481)
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. To explore this possibility, we use data from the Bureau of Labor Statistics, which compiled durable good price indices for some Metropolitan Statistical Areas from the mid-1980s. Figure A2 in the Appendix 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

6.1 Energy efficiency

In this section, we investigate potential benefits associated with increased uptake of home durables, namely, potential energy and housework labor savings following from the purchase of 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.

Table 5: The impact of deregulation on monthly energy expenditure on low-income households

	(1) electricity	(2) gas	(3) cold
Post 1987 deregulation * Low Income Households	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) 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 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.

Our survey contains information on energy use so we can empirically investigate the link between the timing of the reforms and average energy expenditures. The AHS collects information on energy expenditure. The AHS takes into account 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 AHS also 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 further outcome variable in separate regressions to further analyze how banking deregulation affected energy use across low-income households.

Table 6: The impact of deregulation on monthly energy expenditure on low-income households

	(1) Electricity	(2) Gas	(3) Cold
Pre-treatment (>-2)	-3.17**	-0.93	0.05*
	(1.58)	(1.07)	(0.03)
Pre-treatment (>-2)*Low-Income Household	2.57*	-0.05	-0.04
	(1.32)	(1.19)	(0.03)
Post deregulation (0,1)	-0.17	2.29	-0.04**
	(2.52)	(2.32)	(0.02)
Post deregulation (0,1)* Low-Income	0.97	-1.18	-0.01
Household	(1.46)	(1.42)	(0.01)
Post deregulation (2,3)	3.34	4.92	-0.08**
	(5.46)	(4.40)	(0.04)
Post deregulation (2,3)* Low-Income	2.04	-4.07**	-0.01
Household	(1.47)	(1.95)	(0.01)
Post deregulation (4,5)	3.78	7.96	-0.14**
	(8.78)	(7.47)	(0.06)
Post deregulation (4,5)* Low-Income	4.47	-4.21	-0.01
Household	(2.91)	(2.99)	(0.02)
Post deregulation (6,7)	7.74	9.02	-0.18**
	(11.32)	(11.38)	(0.08)
Post deregulation (6,7)* Low-Income	5.51	-6.99	-0.08***
Household	(3.67)	(7.15)	(0.02)
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.

The first two columns of Table 5 and 6 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 5 utilizes only the states deregulating in 1986 or 1987 with all other later deregulating states being treated as controls as in equation (1) while Table 6 uses observations from the 1985, 1987, and 1989 surveys. 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.

Taken together, the results shown in Tables 5 and 6 suggest that the relaxation of credit constraints did not have an indirect 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 and results are reported in the third column in each Table 5 and 6. The estimated coefficient is negative in both samples and statistically significant for low-income households when using the simple difference-in-difference approach in Table 5. We conclude that these results provide some evidence that banking deregulation allowed households with some ability to pay utility bills during extreme weather conditions.

6.2 Labor-savings

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 et al., 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.

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.[18] We define the 12 "late-deregulating" states as those that deregulated in 1988 or later. The remaining 36 states form the "deregulated earlier" group.[19]

[18] 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.

[19] 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.

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 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 7. 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 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 and aggregate 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.

Data from the American Time Use Survey has consistently shown that women aged 15 and older spend drastically more time on housework than men of the same age – regardless of the associated levels of paid working hours – since their records began to be published in 2003. The estimated impact of time gained back likely helped offset household pressures during a period in the U.S. where women's participation in the labor force was increasing while women's time spent on housework (e.g., unskilled work) was decreasing (Coverman and Sheley, 1996).

Table 7: 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 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, especially for low-income groups. Information on home durable consumption for households is available from 1985 to 1991 only. Given this, our identification strategy adopts a simple difference-in-difference design by focusing on the majority of states that deregulated in 1986 and 1987. This strategy is robust to biases stemming from heterogenous treatment timing. However, we also run staggered difference-in-differences (event-study) approach as a robustness to confirm our main results. The event-study design allows us also 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 with the date in which states relaxed their credit constraints, we find positive and statistically significant treatment effects for new home durables purchases among low-income households. These results are consistent with the explanation that banking deregulation eased credit constraints, bolstered purchasing power capabilities across income distributions, and induced lower-earning households to increase their investment in household durable good assets, potentially for the first time. Our findings thereby support the argument that banking deregulation reduced economic inequalities across households by narrowing the gap in housing wealth.

Next, we show decreasing average annual natural gas consumption over time from low-income households who were subject to banking deregulation. The impact provides some evidence that relaxation of credit constraints led to more energy efficient households in the U.S. and can be largely explained by the timing of the enactment aligning with the implementation of the first federally regulated energy efficiency standards for durable goods. While our model found insignificant coefficients associated with household electricity payments, it allows us to assume no effect on increased electricity consumption after the policy change.

Finally, as shown in Table 7, the amplified 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 interstate banking deregulation year

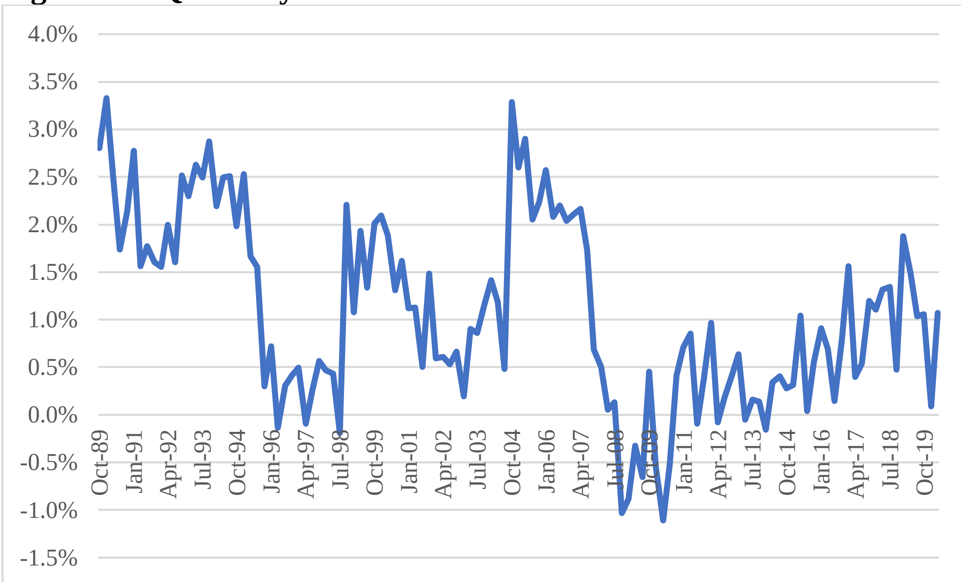
State/Abbreviation	Year Interstate Banking Permitted
Alabama – AL	1987
Arizona – AZ	1986
Arkansas – AR	1986
California – CA	1987
Colorado – CO	1988
Connecticut, CT	1983
Florida – FL	1985
Georgia – GA	1985
Hawaii – HI	1985
Illinois – IL	1986
Indiana – IN	1986
Iowa – IA	1991
Kansas – KS	1992
Kentucky –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

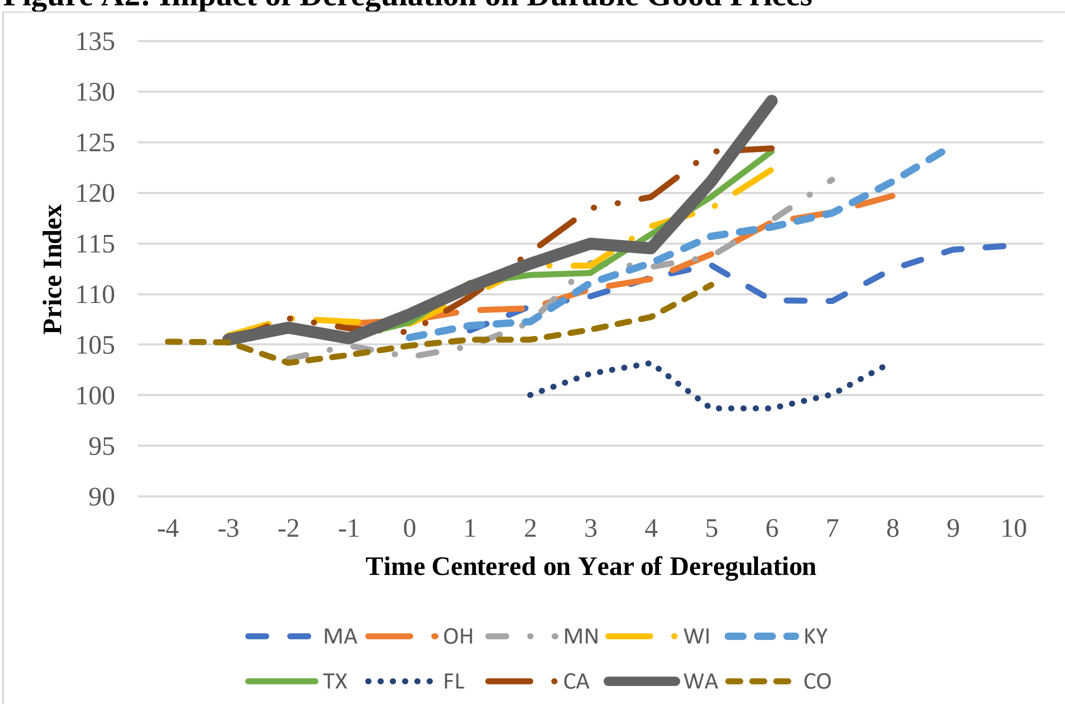
	(1) New Fridge Years < 1991	(2) New Laundry Washer Years < 1991	(3) New Clothes Dryer Years < 1991	(4) New Oven Years < 1991	(5) New Dishwasher Years < 1991
Pre-treatment (>-2)	-0.0623	-0.084	-0.058	0.005	0.008
	(0.0572)	(0.057)	(0.042)	(0.045)	(0.047)
*Pre-treatment (>-2)*Low-Income HH*	-0.128	-0.033	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.

Chapter II: The Effects of Shale Booms on Individual Mental Health

2.1 Introduction

At the beginning of the 21st century, technological breakthroughs and high returns in the oil and gas industry have led to a surge of exploration and production of unconventional gas and oil in the United States. Just over a few decades ago, conventional drilling techniques left opportunities to extract from shale deposits technically infeasible. Starting in 2000, however, substantial advancements in horizontal drilling and hydraulic fracturing (i.e., fracking) proved both feasible and profitable to extraction firms, initiating an exponential spike in drilling extraction activities from shale rock formations (commonly referred to as shale plays) located miles beneath the Earth's surface. As a response to these technological innovations, U.S. tight oil production increased from 0.35 to 2.5 million barrels per day while shale gas production ramped up by roughly 93.3% to reach 30 billion cubic feet per day between 2000 to 2012, causing an almost immediate economic expansion in resource booming localities and their associated populations (James and Smith, 2017).

Empirical research has long examined the impacts natural resource booms have on local economies, labor markets, and inequalities. At the state-level, this line of research tends to conclude that resource booms attract labor, lower unemployment rates, and bolster state revenues (i.e., Black et al., 2005; Weber, 2012; Allcott and Keniston, 2014). At a more localized level, in contrast, previous literature generally associates economic booms with higher wages, higher wages, spillover effects into other economic sectors, and increased infrastructure development (Gylfason et al., 2003; Komarek, 2017). Our research contributes to this literature thread by investigating the effect of resource booms on individual mental health through the widespread adoption of fracking techniques in specific localized areas. Using a difference-in-differences framework, we exploit the plausible exogeneous within-state, cross-county variation of resource booms, and find that individuals living in shale booming areas are more likely to suffer from regressed mental health conditions and partake in increased risky behavior surrounding alcohol intake.

In recent decades, research motivation surrounding resource booms has pivoted its main focus from investigating economic impacts to new controversies surrounding potential-public health risks that extraction may induce. Common concerns addressed in literature include drinking water quality, harmful environmental effects, and-consequential negative property value impact (Boxall et al., 2005; Spiller and Timmins, 2013; Gopalakrishnan and Klaiber, 2014; Jacoby et al., 2012; James and James, 2014; Mason et al., 2015; Muehlenback et al., 2015). In general, these papers' findings align with the argument that economic booms lead to worsened local amenities and general health of a population in the long run. Recent studies have expanded this line of literature by investigating other potential impacts on non-labor market effects such as elevated birthrates, high school graduation levels, sexually transmitted infections (STIs), and crime. In short, these research topics have provided evidence that positively associates booming communities with all four outcomes, supporting the argument that individual benefits and a population's standard of living are reduced on average after a booming cycle begins (see, for example, papers by James and Smith, 2017; Buck et al., 2019; Cunningham et al., 2020; Andrews and Deza, 2018; Komarek, 2018; Street, 2020; Wilson, 2020; and Cascio and Narayan, 2022). The general consensus across these papers is that local long-term and short-term impacts that change prior community demographics are largely explained by a major influx of population through increased employment demand.

While one could assume that the status of an individual's mental health could be impacted by outcomes such as dropping out of high school, becoming infected with a STI, or being a victim of a violent crime, existing relevant literature has generally overlooked the potential mental health impacts that may be linked with dramatic economic changes associated with oil and gas extraction. The purpose of this paper is to investigate this relationship. For example, it may be likely that entire communities undergo a type of collective trauma when such a massive economic expansion impinges on community life. Sudden economic booms may impact mental health via a variety of channels. Some of the communities that experience "boom and bust" cycles are also often located in more isolated rural areas characterized by a high percentage of low-income earners or largely indigenous populations (Abrahamson et al., 2018) and more likely to feel "threaten" by the dramatic in-migration of workers within their local areas, and consequent demographic and cultural changes (Hirsch, et al., 2014).

The public's perception of mental illness in the United States has been evolving incrementally since economist John Nash was diagnosed with paranoid schizophrenia in the late 1950's. While it's been over 70 years after Nash's prognosis, until recent decades, general efforts made to reform the healthcare sector in the U.S. have often excluded mental health and substance abuse (Frank and McGuire, 2000). Following a similar timeline to the national spike in oil and gas extraction via fracking activity, mental health awareness began to be more commonly addressed with the turn of the 21st Century. Alongside trending usage of fracking techniques, mental health economics began developing and research associated with the topic continues to grow – largely as a tool for providing evidence-based recommendations for the strategic development of mental health policy (Knapp and Wong, 2020). As resource scarcity has become a critical factor impacting many outcomes – and demand for better evaluation methods still remains a critical factor for mental healthcare – our analysis provides an important literature branch linking the two.

Mental illness typically impacts the economy through two distinct channels – the detrimental costs suffered internally at an individual level and the aggregate societal consequences that largely fall outside of the healthcare sector (Golberstein et al., 2021). National suicide rates and their cascading effects provide a fundamental example of this. The average suicide in the U.S. is tied to a societal cost of $1.3 million dollars, in which, 97% is due to lost productivity (Gurewich et al., 2016).

At a global scale, it is expected that deficiencies linked to mental health conditions will account for over half of the global economic burden attributed to non-communicable diseases by 2030 (amounting to roughly $6 trillion dollars) (Geneva*: World Economic Forum*, 2011). According to the WHO, the effects caused by depression and anxiety (two of the most common mental disorders) costs the worldwide economy just over $1 trillion dollars per year based in lost productivity. To put this in perspective, the estimated impacts of anxiety and depression equate to a loss greater than 50 million years of work (Chisholm et al., 2016). With such a direct connection between economics and mental wellbeing, this paper could be beneficial to decision-makers attempting to identify the most efficient deployment of available resources to fracking communities with the expectation of individual need.

Our research investigates whether the sudden economic booms following shale gas extraction impact individual mental health outcomes. To estimate this effect, we utilize an a difference-in-differences type of model specification to estimate mental health outcomes over time across household residents. We identify a treatment and control group by adopting an identification strategy akin to Bartik et al. (2019), with our treatment group defined as the head of household respondents who are located in counties geologically positioned above a shale play, that qualify into the top-quartile of the Rystad Energy "prospectivity" index for fracking suitability.[2] The remaining counties in the lower three quartiles, located within the same state and shale play to a treatment county make up our counterfactual group. Thus, our identification strategy is based on within state and within shale deposit variation. We argue that this allows us to estimate the true causal effect of a fracking boom on mental health, as counterfactual counties likely share more characteristics with the treatment group since, (i) counties within a state vary differently from counties in separate states; and (ii) fracking counties are likely to be different that counties without fracking for reasons that may affect key outcomes (Bartik et al, 2019). To the best of our knowledge, this is the first empirical study that estimates the phycological impacts of shale boom-and-bust economies by exploiting within state variation across a sample of fracking communities.

We study a variety of outcomes that affect mental health (including one's quantity and frequency of alcoholic consumption as a measurement of risk behavior) across head-of-household respondents surveyed through the Behavioral Risk Factor Surveillance System (BRFSS) over the period 2002-2012. We compare mental health outcomes of individuals living in treatment

[2] This classification is based on the international oil and gas consulting firm, Rystad Energy's, confidential fracking suitability estimates pertaining to different portions of each North American shale play. Inputs that determine this function include concrete characteristics specific to the geological feature that affect the quantity and quality of the natural resource as well as the ease and cost of production. These geographic measurements were later aggregated to the county level by Bartik et al. by computing the average and maximum score given by Rystad's 2014 NASMap data and then dividing the counties in each shale play into uniquely ranked quartiles (2019).

counties before and after a fracking boom relative to respondents living in less productive fracking counties located within a shale play (or shale formation). This approach tackles the concern surrounding the validity of the counterfactual by acknowledging that places with fracking likely differ systematically from places without these activities, biasing results (Bartik et al., 2019). Specifically, our assignment allows us to compare the local change in mental health outcomes and risky behaviors in areas with high geological potential for extraction activity via fracking relative to those with lower potential for fracking within one of the eight shale formations within our dataset. Based on our data timeline and the methodology BRFSS exploits (addressed in Section 2.3), we assume the bulk of our observations reflect long-term residents, mitigating biases driven by migration effects.

The results section of this paper provides evidence that the changes associated with the rapid extraction of oil and gas lead to poor mental health for individuals located in counties that are abundantly endowed in valuable, accessible shale deposits. We also run regressions to analyze the likelihood of individuals partaking in increased alcohol consumption, a classified risky behavior found to be negatively associated with mental health (World Health Organization, 2014). Our findings indicate that the treated sample is more likely to participate in binge drinking activities[3] relative to those living in counties within a shale play formation that has less extraction activity.[4] This aligns with the Center for Disease Control's (CDC's) behavioral health principle indicating that individuals who experience sadness often or are classified as depressed are more likely to engage in risky behavior such as drinking and/or smoking (Linardakis et al., 2015). Estimates are robust when we apply a different, less restrictive, identification strategy based on the spatial and temporal variation of play formations and energy production as in James and Smith (2017). In this case, the comparison is between counties whose geographic center lies directly above a booming shale play relative to other, non-fracking counties in the U.S.

Later in Section 2.5, we test whether these mental health effects varying across subgroups within the population. In doing so, this research contributes to the body of mental health care literature

[3] Binge drinking is defined as a pattern of alcohol consumption causing an individual's blood alcohol concentration (BAC) to be at 0.08 g/dl or above; an amount that typically equates to men consuming five or more drinks and women consuming four or more (National Institute on Alcohol Abuse and Alcoholism and the Centers for Disease Control (CDC), 2015).
[4] It is worth noting that none of the counties observed in this study were affected by policies constricting public consumption of alcohol during the period associated with this analysis.

addressing need/risk gaps across subpopulations (Beck et al., 2018). We find evidence indicating that high school graduates and those who have attended some college courses are significantly more likely to suffer from depression and experience above average bad mental days. Results associated with clinical depression are intensified further with additional educational attainment, specifically associated with those who have attained a two- or four-year postsecondary degree. In regressions affiliated with relationship status, we show all heterogenous interactions are positive and largely statistically significant from zero, making the argument that mental health decreases for treated individuals across these subgroups during the post period in response to a fracking boom. Effects are largest and most consistent for those who self-reported as married, widowed, or single.

Our estimates also indicate that homemakers, unemployed, and retirees are significantly more likely to partake in binge drinking following a boom. Through a mental health channel, our paper also adds depth to recent important literature linking economic conditions and substance abuse. At a high-level, this line of research has found that a local area's economy is often inversely related to an individual's level of drug and alcohol consumption (Carpenter et al., 2017;), emergency room visits due to drug overdose (Hollingsworth el al., 2017; Ruhm, 2019), and hospitalizations from alcohol-related conditions (Eliason and Storrie, 2009; Browning and Heinesen, 2012).

This paper should be important to researchers, mental health practitioners and policymakers alike, as fracking operations continue to be disputed within legislation but a temporary dependency on extraction remains. In particular, communities with jurisdictions debating over whether to adopt fracking techniques for resource extraction will likely find our estimates to be relevant for their decision-making. There are still extensive deposits located all over the world that would significantly impact the supply of fossil fuels, incentivizing localities to adopt fracking in their communities for the short-term benefits fueled by lower energy costs and increased economic activity. This paper offers policymakers and local municipalities a new factor to consider by providing evidence that links local fracking to worsened individual mental health conditions and, more generally, a person's overall quality of life. Just as importantly, this research is intended to expand the body of mental health literature. Despite recent years of progress, significant evidence in mental health economics remain lackluster. Empirical research affiliated with this topic stand in need of valid, quality measurements (Beck et al., 2018), is unevenly distributed across countries and regions, and is often transferred lethargically across health care, social care, and other public systems (Knapp and Wong, 2020).

The remainder of the paper is organized as follows. Section 2.1 discusses previous literature relating to our research motivation. Section 2.3 details our dataset and defines our main variables. Section 2.4 provides an overview of our empirical strategy and model. Section 2.5 presents the results, Section 2.6 discusses a robustness check, and Section 2.7 concludes.

2.2 Previous Literature

A significant literature exists looking into the direct economic effects of natural resource booms, with a number of more recent studies specifically investigating the positive shocks on market effects initiated by fracking specifically.[4] A common consensus across this area of literature is that resource booms temporarily tend to strengthen a local labor market by attracting workforce migrants, increasing local employment, and inflating local wages (Feyrer et al., 2017; Maniloff and Mastronnmaraca, 2017; Wilson, 2020). A few papers explored the link between fracking and property valuation and found some evidence suggesting modest reductions in property values for house within one mile of a shale well (Gopalakrishnan and Klaiber, 2014) or that are groundwater-dependent and in close proximality to wells (Muehlenbacks, Spiller, and Timmins, 2015).

A smaller set of papers have examined fracking booms on non-labor market effects. A study by Kearney and Wilson (2018) classifies children as a normal good and find shale booms have positive effects on local birthrates across both married and nonmarried individuals. Cascio and Narayan (2022) show spiked levels of high school dropout rates among males in fracking areas and argue this effect is driven by high returns on unskilled labor linked to fracking employment. Street (2020) and Wilson (2020) both compliment this argument with findings advocating that

[4] Some particularly strong papers in alignment with this motivation include: Weber, 2012; Allcott and Keniston, 2014; Paul and Timmins, 2014; Thiemo, 2014; Maniloff and Mastromonaco, 2014; Feyrer et al., 2015; and Wilson, 2016.

this workforce is relatively low-skilled, and labor driven. Cunningham et al. (2020) show that, when they restrict their sample to remote high production areas, fracking is positively associated with higher STI rates.

Along this thread of literature, an emerging trend of studies have focused on the transformation fracking has on regions of the U.S. through a legal lens. Several papers have empirically shown a spike in local crime rates resulting in the period after fracking activities have been introduced (James and Smith, 2017; Andrews and Deza, 2018; Komarek, 2017; Bartik et al., 2019; Street, 2020). This aggregate effect could be attributed, in part, to changes in the population caused by an influx of workers moving to an area or driven by individuals responding to new economic conditions (Street, 2020). Papers by Street (2020) and James and Smith (2017) find evidence suggesting both mechanism channels contribute to a rise in local cases filed.

More generally, this study extends the growing body of literature exploring the lasting impacts economic upturns and downturns have on general health. A few papers have found evidence suggesting that increased economic growth stimulates improvements in health conditions of a population over time (Fogel, 1994; Costa, 2015). Some researchers have also concluded that economic recessions are positively associated with risky lifestyle behaviors (Ruhn, 2000; Nuemayer, 2004; Gerdtham and Ruhm, 2006; Buchmueller et al., 2007; Cunningham et al., 2020). When narrowed to research motivated by the oil and gas industry specifically, McKenzie et al. (2019) link oil and natural gas activity to cardiovascular disease by means of pollutants associated with extraction. Hill and Ma (2017) found that drilling an additional well pad within 1 kilometer to groundwater intake locations causes strikingly severe contamination effects to drinking water. Another study by Hill (2013) generated robust results arguing that the introduction to drilling in an area increased low birth weights across infants and decreased term birth weight associated with mothers. Complimentary to this study, Currie et al. (2017) measured changes in infant birth weights in Pennsylvania from 2004 to 2013 to determine potential health impacts channeled through fracking technologies. By reducing a location's previous standards of air and water quality, their empirical results indicate a robust surge of low-birth weight babies as well as a significant drop in average birth weight across infants born within fracking communities, supporting the argument that health outcomes are worsened by fracking activity. These outcomes become exponentially exasperated the closer in proximity a birth is in relation to the site.

More similar to the motivation of this research, a paper by Hays et al. (2016) investigated the relationship between health risks and noise produced by oil and gas operations. The authors found evidence suggesting that outcomes such as sleep disturbance, hypertension, and cardiovascular disease are positively correlated with environmental hazards and noise exposure at a regional scale. Each of these outcomes could also be argued to be correlated with the mental health outcomes of individuals, as mental illness often carries significant internal costs associated with a worse health-related quality of life and longevity impacts (Golberstein et al., 2021). Even the less-severe health outcomes in this case (such as sleep deprivation) could potentially be channels that also impact one's long-term mental health and lead to higher societal costs due to lost workdays and decreased productivity. A goal of this paper is to link the effects of oil and gas extraction activity, specifically from fracking techniques to the larger costs likely associated with a decline in individual mental health conditions.

Some research has investigated the relationship between community-level exposure to changes in economic conditions and *mental* wellbeing. Avdic et al. (2020) show results that reveal economic declines often lead to a long-term decrease to one's mental health condition and life satisfaction. Gibson et al. (2011) looked at changes over time across twenty-nine British Columbia mining communities and found evidence suggesting a general uptick in mental health disorders and cardiovascular disease in periods of economic decline. Maguire and Winters (2016) studied a sample of Texas residents over time and concluded that horizontal drilling for gas extraction was associated with poorer subjective well-being and a higher number of days taken off work due to mental health reasons. More broadly, Catalano et al. (2010) conclude that adverse economic transitions project higher cases of depression, suicide, and substance abuse. We expand on this literature by investigating the cyclical nature of boom-and-bust industries and its effect on individuals by utilizing a unique identification strategy that incorporates within state, cross-county variation between local fracking areas, mitigating the endogeneity bias that is likely to have occurred if we based our treatment definition solely on observed drilling activity.

Empirical papers pertaining specifically to fracking's impact on local mental health trends, however, are surprisingly scarce. To the best of our knowledge, existing studies in this domain have largely been based on qualitative methods yet generally agree that localities exposed to fracking have more negative spillover effects associated with mental wellbeing overtime (Perry, 2012; Brasier et al., 2014; Perry et al., 2015). The central intention of this paper is to fill this quantitative gap in boom-and-bus literature and mental health outcomes at the individual-level, which to date remains uncertain. As briefly mentioned in the previous section, this type of research should prove valuable to localities in making decisions about allowing fracking, as policymakers and their respective communities have not had systematic evidence of its benefits or costs on the mental health of residents.

Lastly, our paper is linked to a larger literature and ongoing debate questioning if natural resource abundance stimulates economic growth across industries. Summarizing this literature is outside the scope of this paper, given that our interest is in estimating reduced-form regressions on the effects of mental health. However, after much in-depth assessment of this deliberated topic, a number of papers conclude that natural resource endowments tend to crowd out other market sectors (particularly the manufacturing industry), increase local wages, and reduce both economic growth and general welfare (see for example Sachs and Warner, 2001; Ismail, 2010; Harding and Venables, 2013). A more recent study by Allcott and Keniston (2017), however, provides evidence arguing against a Natural Resource Curse in the United States. The authors' findings suggest that resource-abundant counties have higher on average real wages relative to other areas and furthermore, manufacturing sectors tend to experience overall growth during an oil and gas boom due to upstream and locally traded subsectors.

This paper makes several contributions to previous literature. First, the focus on individual mental health provides a broad picture of fracking's lasting impacts on residential wellbeing and quality of life. Previous work pertaining to fracking specifically has generally focused on the local labor market benefits of fracking or estimating the social and environmental costs impacted by the extraction technique's spillover effects. Second, we use individual-level mental health-related outcomes (such as stress) in relation to boom-and-bust cycles have largely been at the community-level (state or regional) and theoretical in nature, providing either a general consensus relating to a certain population or little to no empirical evidence to support the claimed arguments. This enables us to provide a subgroup analysis on our sample to measure mental health outcomes across demographics. This allows us to understand what kind of individual is impacted the greatest by fracking activity, and to the best of our knowledge, we are the first to do so. Third, and perhaps most importantly, we improve this strand of the literature by adopting a careful identification strategy that compare mental health of statistically similar individuals living in the same state, but exposed to different level of extraction activities, before and after the boom.

Similar to the Bartik et al. (2019) paper, we examine counties across eight different shale plays in the U.S. This new research design model builds upon important previous work that has focused largely on single shale plays. By estimating our results across multiple shale formations we have a more comprehensive measure of fracking's impacts on mental health outcomes in the U.S. Lastly, and in relation to the Bartik et al. identification method, this paper attempts to solve the typical identification problem many empirical researchers have run into in the past when investigating the effects of fracking.[5] Our identification strategy minimizes the endogeneity likely present across locations by recognizing that areas endowed with fracking potential likely differ from the rest of the country in substantial ways (such as through socioeconomic and demographic channels). This approach offers a credible solution based on the geological characteristics of shale deposits and the timing of when new technologies become available to a location.

2.3 Data

This paper employs three main sources of data: (i) Rystad Energy's *Use of GIS in Valuation of North America Shale Plays* 2014 dataset; (2) the initiation date of fracking across shale plays; and (3) the Behavioral Risk Factor Surveillance System (BRFSS) survey dataset that contains individual's mental health responses together with socio-demographic characteristics. In this section we briefly describe all three sources exploited in our model and conclude with tables detailing summary statistics. This section also provides further explanatory support towards the

[5] The most commonly used method from fracking literature compares areas located directly over shale formations to areas without shale deposits below them (see, for example, Weber, 2012; Fetzer, 2014l; Maniloff and Mastromonaco, 2014; Weinstein, 2014; Cascio and Narayan, 2022).

validity of the research design. Table A in the Appendix section of this paper reports summary statistics and illustrates in greater detail each of our key variables described in this section below.

1) Fracking Data. – Below we describe both sources of fracking data in relation to our model and how they were previously collected.

a. Prospectivity Estimates. – The Rystad Energy oil and gas consulting firm has created a fracking suitability index identifying locations within North American shale plays based on estimates surrounding productivity potential. This index was developed by the company through exploiting a geographic information system (GIS) shapefiles to compare geological characteristics across shale play acreage and set a value to specific portions of shale plays. Their method ranks top prospectivity locations by observing geological features within a shale formation and identifies if a particular area contains a combination of key optimal factors that affect the decision to implement fracking operations at that location. Key geologic parameters such as the depth and thermal maturity of the play can significantly impact project incentives (Rystad Energy, 2015). By evaluating each formation's geology, Rystad is able to provide producers with economic outcomes based on the scoring of their valuation model. Outcome results at specific coordinates include the total quantity of hydrocarbons contained under the surface, the level of drilling ease expected from fracking techniques, and the cost of completing the intended extraction at a particular well. The company utilized their model's findings to then create a "prospectivity" index, given a location's geological inputs within a portion of a shale play that indicates the opportunity value of extraction. These estimates are unique to each formation and are based on a nonlinear function of geological inputs. Each explanatory variable within this model measure factors contributing to the expected ease of extraction via fracking techniques; the quality of the resource, and projected level of production costs. We apply the Bartik et al. (2019) prospectivity measurement which has been aggregated to the county level and divided into Rystad score quartiles by the authors.

b. First Fracking Year– This data indicates the year a county's fracking potential becomes public knowledge. Sourced from the Bartik et al (2019) paper, these dates were determined in large part by tracing investor calls, production announcement releases, or the first date when fracking techniques were adopted and implemented within a county. By utilizing this county-specific dates as a map to understanding our post period, we are able to exploit the temporal variation resulting from the heterogeneous geology across shale plays, the location's associated recovery levels, and the micro-/macro-economic factors influencing oil and gas development at the time (Bartik et al., 2019). Based on the year when our treatment counties initiated fracking, our post period is defined as 2010 or greater.

2) Mental Health Data.. – The BRFSS is a telephone survey that collects health-related data from a nationally representative cross section of about 400,000 individuals per year in order to identify health-related risky behaviors. It has attained a wide array of sponsors through the Center of Disease Control (CDC) and federal agencies including Health Resources and Services Administration, Administration on Aging, Department of Veterans Affairs, and Substance Abuse and Mental Health Services Administration. First established in 1984 with just fifteen states included in the survey, it now carries an ongoing presence in all fifty states and successfully completes more than 400,000 adult (≥ 18 years) interviews each year, causing it to be considered the largest continuously conducted survey relating to health conditions and health-related behaviors in the world. As of 2020, eleven countries (including Australia, Canada, Italy, and South Korea) have requested the BRFSS staff provide them with the technical assistance required to implement similar health-related surveillance systems for their residents. For the purpose of our study, we use the BRFSS's "SMART" subsection which identifies household respondents at the county level. This division of the survey provides physical and mental health data from the heads of a household at the city- and county- level for selected areas. By additionally offering information on an individual's county of residence as well as personal questions pertaining to risky and behavior, this dataset has been a useful analytical tool for the CDC, state health departments, and other federal agencies to identify emerging health trends and potential problems at a more localized level.

The BRFSS SMART dataset also aligns well with our research motivation. Studying the effects of fracking on mental health outcomes at the individual level requires very detailed information associated with each respondent. All BRFSS data is intended to be as thorough and exhaustive as possible. The reason for this is to aid government departments in properly designing, implementing, evaluating, and modifying public health programs while supporting data-driven evidence for state health objective progress. As a result, the meticulous nature of the survey provides us with an advantage for estimating incremental changes of individuals overtime within our sample.

To provide evidence that cyclical boom-and-bust industries affect mental wellbeing we utilize a handful of the questions continuously asked by surveyors in the SMART waves. We first estimate the impact that fracking has on an individual's mental health based on a question asking respondents to provide the number of days during the previous month in which they experienced an uncommon level of stress, depression, or significant emotional issues. This gave us the structure for our first dependent variable, which we label "Bad Mental Days" in the analysis. We then built upon this question by generating two additional mental health outcome variables, "Above Average Bad Mental Days" and "Depression" – following the guidelines from the American Psychiatric Association and CDC, respectively. Based on the American Psychiatric Association guidelines, the variable 'depression' is an indicator variable that turns on if an induvial reports experiencing sadness, anxiety, and/or loss of interest in personal pleasure for at least 14 days out of a month (2013). The second variable captures the idea on individuals grappling with an "above average" level of bad mental health days in the past thirty days. This definition varies across gender and is 3.5 days for females and 2.4 for males according to the CDC (2021).[6]

Additionally, because medical research has shown that alcohol abuse is positive correlated with mental health problems and suicide (CDC, 2021; WHO, 2011), we also estimate the effect of fracking on alcohol assumption at an individual level. We generate two variables (the average intake of alcoholic drinks and the number of times an individual binge drank) from one AHS question that asked respondents to recall the number of drinks they had on each day they drank in the past thirty days. Based on CDC and other organization and guidelines, the variable representing the impact of binge drinking is defined as having five or more drinks if you are a male per drinking session and four or more if you are a female. Although our results suggest binge drinking is strongly correlated with post fracking initiatives, it is also likely that underreporting will prevent us from observing this full effect, causing our findings to be biased downwards. According to the CDC, data collected from respondents regarding personal drinking

[6] According to the CDC, the average American adult feels sad or depressed 3.5 days a month if that individual is a woman and 2.4 days a month if male (2021).

habits account for only about 22 to 32% of actual alcohol consumption when compared to state alcohol sales data (CDC, 2014).

Previous research surrounding the impacts of shale booms has often debated whether – mainly – economic/labor market effects are caused by extracting the natural resources or by an in-migration influx during the boom. This is a valid concern, as people tend to leave as local labor market conditions regress and pursue new locations experiencing economic expansions. When this is unable to be controlled for, it tends to bias results because the true residential effect is convoluted with temporary residential workers. This obstacle introduces our first data drawback we experienced. For the purpose of our paper, there are two leading disadvantages to collecting data from the BRFSS – the first being that we have no ability to track migration of individuals and consequently, no way to control for movement of people.

Unobserved migration patterns can lead to biased results, as it reduces our ability to understand the extent to which our results are driven by worsening mental health conditions of long-term residents of the local area affected by the boom, or perhaps, by movers with preexisting mental health conditions. What does work in our favor, however, is the way in which the BRFSS conducts its survey. Up until 2011, with help from the CDC, this survey was implemented via in-house interviews or landline calls from state health department representatives. The BRFSS updated its methodology for the June 2012 release to include the use of cells phones. This amendment allowed the database to reach more segments of the population previously unobserved (e.g., those with a cell phone but no landline or those without a permanent residence). Being that our data's timeline ends in 2012, we can assume much of our sample pool is comprised of individuals who are – and have been – long-term residents in a boom-and-bust economy. It is also worth mentioning that the BRFSS administers its survey utilizing Random Digit Dialing (RDD) techniques to households in residential directories, and we know households in residential directories are more likely to have stable residences that the general population (Street, 2020). To that end, it seems likely that the bulk of temporary residents are weeded out simply by the survey process, supporting the argument that there is inconsequential selection bias driving our results.

Another potential data limitation is spurred by the inconsistency of geographic areas observed from each BRFSS SMART collection wave. Survey area coverage varies year-to-year depending on the outcome level from respondents and other criteria delegated by the BRFSS. If a county's completed interviews are too low to statistically represent the area (defined as less than 500 respondents), it is dropped from the larger dataset for that observation year. This limitation poses as a concern if we do not have enough coverage of rural places that are actually affected by the boom in the survey, given that many fracking communities are largely rural with lower residential populations. After creating our balanced sample we were left with 60,274 observations.

3) Merged Data. – All three data sources were aggregated by merging the survey and fracking information to identify households in counties that have experienced a shale gas boom driven by fracking technology. Thus, our dataset consists of BRFSS SMART respondents residing in nineteen U.S. counties across eight shale plays displayed in Table 1. This estimation sample has been restricted to ensure each 'treatment' county, i.e., a county in which the treatment group of individual respondents reside, is observed consistently across all ten years in our dataset – to this end, we work with a balanced sample. Of the nineteen counties, eleven have been ranked into the top prospectivity quartile by Rystad Energy and 39,302 individual respondents living in these countries are classified as our treatment group. The 20,972 individuals residing across the remaining eight counties located within the shale formations but lower Rystad score, are considered the control group. Respondents surveyed by the BRFSS that are located in counties within a shale formation but not ranked as a top candidate by Rystad in terms of fracking potential form a plausibly valid counterfactual group which we take as a good indication of what would have happened to individuals residing in fracking communities in the absence of fracking. In total, there are 60,274 adult respondents observed between 2002 and 2012, averaging to just over 6,027 respondents per year.

Table 1: Summary Statistic of Treatment and Control Observations and Counties

(1) County Name and State	(2) Shale Play	(3) First Fracking Year by Play	(4) Respondents (N)
Treatment Group: Top-quartile Counties			
Adams County, CO	Niobrara-Denver	2010	5,968
Allegheny County, PA	Marcellus	2008	8,459
Arapahoe County, CO	Niobrara-Denver	2010	7,720
Armstrong County, PA	Marcellus	2008	1,167
Bradford County, PA	Marcellus	2008	1,574
Campbell County, WY	Niobrara-Powder River	2005	469
Carter County, OK	Woodford-Ardmore	2007	463
Laramie County, WY	Niobrara-Denver	2010	8,252
Webb County, TX	Eagle Ford	2009	1,186
Weld County, CO	Niobrara-Denver	2010	2,333
Westmoreland County, VA	Marcellus	2008	1,711
Treated units			39,302
Control Group: Outside Top-quartile Counties			
Caddo Parish, LA	Haynesville	2008	2,020
Canadian Count, OK	Bakken	2007	469
Eastland County, TX	Barnett	2001	516
Fayette County, PA	Marcellus	2008	6,745
Kanawha County, WV	Permian All Plays	2005	4,628
Midland County, TX	Permian All Plays	2005	885
Tarrant County, TX	Barnett	2001	4,507
Ward County, ND	Bakken	2007	1,202
Control units			20,972
Total units			60,274

Notes: This table reports the number of individual respondents by residential county and associated shale play. The first portion of the table indicates that the individuals observed are part of our treatment group based on their county's maximum fracking prospectivity score estimated by Rystad. Labeled as a "top-quartile" county, this binary variable equals 1 if the county is in the top-quartile of the Rystad measurement and 0 otherwise. Column 3 displays the first-year fracking operations became public knowledge within a shale play.

Table 1 above lists the treated and control counties that make up our balanced sample of counties. Column 3 summarizes the temporal variation driven by the unique year fracking techniques were initiated within that county. Column 4 displays the distribution of survey household respondents observed within top-quartile counties as well as in the counties categorized as control. The Barnett formation was the first play to effectively implement modern fracking methods and go public with their successful production. Fracking was initiated in six of the eleven top-quartile counties in our sample by 2008, the same year mining employment numbers began to reflect the surge in U.S. oil and gas production (James and Smith, 2017).

2.4 Empirical Methods

To measure the aggregate impact booming activity has on individual mental health outcomes, we utilize a differences-in-differences specification with fixed effects. The main empirical challenge we faced when attempting to identify this impact is defining a plausible counterfactual to the group of respondents most exposed to shale booms, given that people living across locations may have different (unobservable) characteristics. This issue is prevalent in research designs comparing areas over shale formations to areas without shale formations beneath them (see, for example, Cascio and Narayan 2022; Fetzer 2014; Maniloff and Mastromonaco 2014; Weber 2012; Weinstein 2014). As previously highlighted in this paper, assignment hinging on strictly play-based strategies may lead to biased results for multiple reasons. For example, strategies consistent with this line of thought often fail to exploit the substantial within-play variation that causes some counties to be more amenable to fracking relative to other counties located above the same deposit formation. Equally as important to mention, the methodology fails to acknowledge that areas endowed with fracking ability likely differ systematically in ways that can impact outcomes as well as other input variables (Bartik et al, 2019).

The root of our empirical design lessens much of the concern for biases by simply acknowledging that areas of the country heavily endowed with fracking opportunities differ significantly from areas of the country that aren't. To better support the argument that the common trends assumption holds true across treatment and control groups, our central identification strategy builds on the framework laid out in the 2019 paper by Bartik et al. and is contingent upon: (i) the spatial geology variation within a shale play; (ii) the temporal variation in national mining activity; and, perhaps most importantly, (iii) an arguably valid counterfactual group. We restrict our sample to 20 counties across 1 of the 9 shale plays listed in Table 1 and compare the change in individual mental health outcomes in areas with high geological potential for fracking relative to areas with lower geological potential within the same shale formation and within the same U.S. state. U.S. shale plays are not all identical, nor are the areas that surround them. Accounting for differences in geological features within plays acknowledges that these areas vary in both levels and trends (Zagorski, Wrightstone, and Bowman, 2012; Budzik, 2013; McCarthy et al., 2011; Covert, 2015). Additionally, by observing the date in which fracking methods were publicly adopted within a play, we also recognize the substantial fluctuations across local area's general willingness to accept fracking practices take place in their residential community.

For validity purposes, we modify the Bartik et al. (2019) strategy slightly to exclude a staggered treatment specification, which has been shown to confound results derived by difference-in-differences type of framework (Baker et al., 2022). Instead, we specify our post treatment period to be all the years in our sample greater than 2009, as the first fracking year associated with our treatment counties are less than or equal to 2010 (see Table 1). By specifying our pre- and post-periods this way, we are assuring our comparison group does not change overtime. Lastly, this paper takes the Bartik et al. (2019) specification one step further by fixing our unit of observation at the individual level, bridging a gap in boom-and-bust literature. In this way, our framework utilizes cross county variation within a state, exploits within-play variation, and attempts to control for heterogeneity across play formations, with outcomes referring to people (not places).

Several models of the following form are estimated employing OLS:

$$y_{ict} = \alpha_{ict} + \delta_i(1[PostFracking]_{ct} * 1[RystadTopQuartile]_c) + (state_i * t) + \rho_t + BX_i + \gamma_c + \epsilon_{ict} \quad (1)$$

The outcome variable in equation (1), y_{ict}, measures a variety of self-reported mental health outcomes or recent alcohol consumption for individual (i) living in county (c) during year (t) where $t = 2002, \dots, 2012$. This specification includes year fixed effects (ρ_t) county fixed effects (γ_c), a vector of demographic controls observed at the individual (X_i),[7] and state-by-year fixed effects to control for state-specific linear time trends, represented by $(state_i * t)$. The latter interaction term supports our identification centered on cross-county variation within a state, given that all counties in our dataset intersect at least 1 of the 9 shale plays listed in Table 1. Including a state-linear trend variable in all our regressions further strengthens our main specification by aiding to detect if any pre-existing trends were present prior to the post period.

The effect of the shale boom on mental health is captured by the interaction between two key covariates, (i) $1[PostFracking]_{ct}$, which is a binary variable equal to 1 after 2009 (and remains

[7] According to the CDC, the average American adult feels sad or depressed 3.5 days a month if that individual is a woman and 2.4 days a month if male (2021).

“on” for all remaining subsequent years) each county’s associated play publicly adopts fracking and 0 otherwise, and (ii) $1[RystadTopQuartile]_c$, which is an indicator variable equal to 1 if a respondent’s county of residence c is one of the top quartile category for counties in a shale play with a maximum prospectivity value for extraction.[8] The interaction between $PostFracking_{ct}$ and $RystadTopQuartile_c$ in equation (1) is our variable of interest, the difference-in-differences estimator. Empirically, it measures the change in the difference in y_{ict} between individuals residing in counties with high and low Rystad prospectivity values before and after fracking technology was initiated in an area’s linked shale formation. The coefficient attached to this interaction, δ_i, indicates the quantified impact of the boom-and-bust cyclical nature on mental health for the entire population. Analogous to all difference-in-differences methodology, this model hinges on the assumption that in the absence of fracking activity, treated individual’s mental health conditions would have changed similarly over time with that of residents in non-treatment counties. To further support our validity discussion, we argue that the local culture differences across these counties do not vary substantially, as our identification strategy compares changes between counties within a state.

Equation (1)’s design is intended to provide statistical estimates of the average treatment effect of shale booms on mental health outcomes. Stopping our quantitative analysis here, however, would be a disservice to mental health literature, as more recent empirical evidence suggests that the impact of fracking on outcomes related to general welfare effects are likely to vary substantially across individuals/households (Bartik et al., 2019; Street, 2020). For this reason, we also consider the heterogeneous effects of fracking activity by population subgroups within a county, shown quantitatively through equation (2) below.

$$y_{ict} = \alpha_{ict} + \delta_i(1[PostFracking]_{ct} * 1[RystadTopQuartile]_c * Subgroup\ Dummy) + (state_i * t) + \rho_t + BX_i + \gamma_c + \epsilon_{ict} \quad (2)$$

Here, we estimate the treatment effects across sociodemographic and gender groups through the triple difference-in-differences specification. In this context, our estimates allow us to assess what specific characteristics predispose respondents to varying magnitudes of mental wellbeing

[8] The model is fit on a sample of individuals residing in counties that intersect at least one of the nine shale plays listed in Table 1.

impacts brought on by booming cycles. Valuable to this paper's motivation, these triple-interactions shed some light on groups with high-risk of psychological impacts spurred by fracking booms.

2.5 Results

Tables in this section report treatment effect estimates of fracking booms on mental health and risky alcoholic consumption outcomes. Fundamentally, each coefficient measures the difference in outcomes between individuals grouped into the top-quartile fracking counties relative to less productive fracking counties before and after extraction activity started. Table 2 below displays our results after running the main specification from equation (1). Tables 3.1 through 3.4 report heterogeneous effects derived from equation (2).

2.5.a Main Results

Table 2: The Effect of Shale Booms on Individual Mental Health & Drinking Habits

	(1) Depressed	(2) Bad Mental Days	(3) Above Average Bad Days	(4) Average Alcoholic Drinks	(5) Binge Drinking
(Fracking Exposure) *x*	2.07***	0.56***	1.78***	0.01	2.60***
(Post Fracking Activity)	(0.13)	(0.04)	(0.48)	(0.01)	(0.30)
Observations	60,498	60,498	60,498	60,498	60,498
County FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
State-Level Linear Trends	YES	YES	YES	YES	YES

Notes: This table reports estimates of individual mental health and alcohol consumption on a measure of fracking exposure. The fracking exposure indicator is derived from the Bartik et al. (2019) measurement – e.g., a county is geographically positioned in the first (or highest) quartile of the Rystad max prospectivity score within a shale play. The control group consists of other fracking counties located above a shale play but ranked in the lower three quartiles by Rystad in terms of production potential. Household controls include family size, income level, employment, marital status, education attainment, race, and age. The sample consists of all local households surveyed by the BRFSS SMART waves in a particular year. Coefficients and standard errors reported in columns 1, 3, and 5 have been multiplied by 100 to interpret them as percentage point increases or decreases. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Standard errors are in parentheses and clustered at the county level.

Results in Table 2 report the average treatment effect for our sample's aggregate population. Starting with the most severe psychological outcome in column 1, the coefficient of interest indicates a positive uptick in the average likelihood of individuals in top fracking counties being clinically depressed in the post period relative to those in less productive extracting counties. Significant at all levels, this translates to a 2.1 percentage point increase in the probability (we interpret proportions as probabilities for this analysis) of respondents reporting they are experiencing a considerable level of sadness, anxiety, and/or loss of interest in personal pleasure for a minimum of 14 days during a typical month.

In column 2 we measure the amount of "bad mental health days" an individual reports through a continuous variable contingent on self-reported days a resident experienced an uncommon level of stress, depression, or significant emotional issues. The coefficient associated with this outcome shows the marginal effect of fracking booms on the amount of time one experiences this type of phycological trauma during a typical month. We can interpret the result in Table 2, column 2 as an average increase of roughly 13 hours per month across the treatment group in the post period for our full sample and is significant at the 1% level. Following the CDC definition guidelines discussed in previous sections, column 3 allows for variation across male and female measurement thresholds, where the outcome indicator classifies females into this category if they self-report having 3.5 poor mental health days a month relative to 2.4 for males. Similarly, this result holds a significance at the 1% level and translates into a 1.8 percentage point increase for males and females collectively.

Columns 4 and 5 estimate the impact of a fracking boom on individual alcoholic consumption, with the latter more narrowly focusing on risky behavior engagement. We find no evidence that, on average, individuals tend to drink more days per month after a fracking boom, with the coefficient on the lead indicator being close to zero, 0.009, and statistically insignificant. Notably, however, results in column 5 are highly significant and can be interpreted as a 2.6 percentage point increase in the probably of treated respondents to partake in binge drinking activity after the shock of a fracking boom. Together, the estimates in rows 4 and 5 indicate that although treated individuals did not drink more days during an average month in the post period, on the days they did decide to drink, they were more likely to consume larger quantities of alcohol. This is an important connection to our findings in the first three columns, as frequent binge drinking has been linked to poor mental health – channeled largely through a decline in life satisfaction and psychological distress (Makela et al., 2014).

2.5.b Heterogeneous Treatment Effects

It is unsurprising that mental health is particularly impacted by some groups more than others. Research stemming from phycology has shed some light on identifying portions of the population at higher risk for developing symptoms that often lead to a decline in one's overall mental wellbeing (Huang and Zhao, 2021). In this section, we attempt to identify who are the more susceptible subgroups within our sample whose mental health conditions are more vulnerable to the impacts of fracking.

Tables 3.1 through 3.4 report our findings from running various forms of equation (2) (e.g., our heterogeneous treatment effect specification). Outcomes depend on which demographic subgroup in our sample was interacted with our variable of interest. This has the advantage of revealing any interesting patterns in effects that may exist across individuals belonging to specific sociodemographic and gender categories. It is worth mentioning that some observation levels differ slightly across tables in this section, as we don't observe individuals who refused to answer demographic questions of interest. Lastly, while we are unable to directly test for the mechanisms underlying the increase or decrease in mental health and risky behavior outcomes across subgroups, we suggest some potential pathways for these results. The remainder of this section's discussion highlights our key findings.

Table 3.1: The Heterogeneous Effect of Booms on Individual Mental Health & Drinking Habits, by Educational Attainment

	(1) Depressed	(2) Bad Mental Days	(3) Above Average Bad Days	(4) Average Alcoholic Drinks	(5) Binge Drinking
Reference Category:					
(Fracking Exposure) *x* (Post) *x* (HS Graduate / Some College)	4.40*	0.16	5.46**	-0.52*	-2.77***
	(1.17)	(0.48)	(1.51)	(0.16)	(0.42)
(Fracking Exposure) *x* (Post) *x* (2- or 4- Year Degree Graduate)	7.89**	1.00	5.41	-0.26	-3.09***
	(1.99)	(0.65)	(2.30)	(0.16)	(0.08)
Observations	26,635	26,635	26,635	26,635	26,635
County FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
State-Level Linear Trends	YES	YES	YES	YES	YES

Notes: This table reports estimates of individual mental health and alcohol consumption on a measure of fracking exposure. The fracking exposure indicator is derived from the Bartik et al. (2019) measurement – e.g., a county is geographically positioned in the first (or highest) quartile of the Rystad max prospectivity score within a shale play. The control group consists of other fracking counties located above a shale play but ranked in the lower three quartiles by Rystad in terms of production potential. Household controls include family size, income level, employment, marital status, education attainment, race, and age. The sample consists of all local households surveyed by the BRFSS SMART waves in a particular year. Coefficients and standard errors reported in columns 1, 3, and 5 have been multiplied by 100 to interpret them as percentage point increases or decreases. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Standard errors are in parentheses and clustered at the county level.

Table 3.1 above reports the demographic effects of a fracking boom by educational attainment, with the excluded category being high school dropouts. Results in row 1 measure fracking's exposure effect on mental health and risky alcohol consumption for high school graduates and individuals who have attended some college, relative to the rest of the sample. Results across row 2 report outcomes for individuals who have attained a two- or four- year degree from a recognized postsecondary institution. Positive and significant estimates shown in column one suggest that as educational attainment increases, so does the likelihood for individuals to experience severe mental distress in the post period. While both treated subgroups are positively correlated with depression effects, those with advanced degrees are nearly twice as likely to suffer from this condition after a fracking boom. These findings further support our assumption that our observed respondents are mostly long-term residents rather than migrant workers, as we know boom and bust migration culture tends to incentivize a lower-skilled (Street, 2020; Wilson, 2020), less educated (Cascio and Narayan, 2022) labor force through high returns. Declining mental health across the most educated individuals in our sample may be a type of peer effect if, at a local level, fracking spurs a temporary reduction in the returns to education within these communities through improved earnings prospects and opportunity costs.

We also find some evidence indicating that these two subgroups are less likely to partake in risky alcohol consumption, as coefficient estimates down columns 4 and 5 are negatively associated with both behavioral outcomes and mostly significant. We know that increased disposable income tends to be complimentary to alcohol intake, as evidence has shown that surges in discretionary spending levels often increase the prevalence of alcohol consumption (Ettner, 1996). These findings seem probable if we assume a sizable percent of the groups reported in Table 3.1 have forgone swift high profit returns in the energy sector to invest in their education, thus essentially having less money in their pockets to burn.

Table 3.2: The Heterogeneous Effect of Booms on Individual Mental Health & Drinking Habits, by Relationship Status

	(1) Depressed	(2) Bad Mental Days	(3) Above Average Bad Days	(4) Average Alcoholic Drinks	(5) Binge Drinking
Reference Category:					
(Fracking Exposure) *x*	8.19*	2.49***	11.15**	-0.25	1.11
(Post) *x* (Married)	(2.74)	(0.33)	(2.81)	(0.14)	(2.21)
(Fracking Exposure) *x*	4.20	1.88**	10.53***	-0.15	0.97
(Post) *x* (Divorced)	(2.50)	(0.53)	(1.51)	(0.13)	(2.17)
(Fracking Exposure) *x*	16.46**	4.45***	17.78**	-0.27	-0.85
(Post) *x* (Widowed)	(3.87)	(0.65)	(4.06)	(0.17)	(2.30)
(Fracking Exposure) *x*	6.80	3.67**	13.90**	0.69*	4.36
(Post) *x* (Separated)	(5.45)	(1.08)	(3.17)	(0.22)	(4.65
(Fracking Exposure) *x*	13.00**	3.22***	11.55**	0.35	1.13
(Post) *x* (Never Married/Single)	(2.72)	(0.38)	(3.02)	(0.16)	(3.47)
Observations	26,568	26,568	26,568	26,568	26,568
County FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
State-Level Linear Trends	YES	YES	YES	YES	YES

Notes: This table reports estimates of individual mental health and alcohol consumption on a measure of fracking exposure. The fracking exposure indicator is derived from the Bartik et al. (2019) measurement – e.g., a county is geographically positioned in the first (or highest) quartile of the Rystad max prospectivity score within a shale play. The control group consists of other fracking counties located above a shale play but ranked in the lower three quartiles by Rystad in terms of production potential. Household controls include family size, income level, employment, marital status, education attainment, race, and age. The sample consists of all local households surveyed by the BRFSS SMART waves in a particular year. Coefficients and standard errors reported in columns 1, 3, and 5 have been multiplied by 100 to interpret them as percentage point increases or decreases. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Standard errors are in parentheses and clustered at the county level.

Table 3.2 considers the heterogenous effects of fracking activity on individual's mental health separated by self-reported marital status.208 Positive, consistent estimates reported down columns 1 through 3 suggest that, in general, treated individuals across all relationship subgroups are more

8 It should be noted that the category excluded in this case consist of individuals identifying as a couple.

likely to experience dwindling mental health in the post period. All mental health outcomes shown are statistically significant from zero across the listed subgroups excluding depression associated with separated respondents. In terms of both magnitude and significance, estimates associated with widows, single individuals (e.g., those reporting they have never been married and are not currently in a relationship), and married couples are the most vulnerable groups to suffering deteriorated mental health conditions after a fracking boom. These results are somewhat in contrast to previous threads of mental health literature suggesting individuals susceptible to common mental disorders at baseline levels are less likely to have long-term partners or enter marriages (Eaton et al., 2017).

Table 3.3 below showcases the treatment effect of booms broken down by employment status across our sample pool. The categories at this observation level include employed for wages, long-term unemployed, homemaker, student, and the retirement pool. Excluding student respondents, we find positive, significant coefficient results across columns 1 through 3. These findings may be interpreted as signals of mental distress resulting from a local boom for all reported employment categories shown in Table 3.3. Perhaps most notably is the magnitude of effects for some groups. Individuals who responded as being long-term unemployed (e.g., unemployed for one year or longer) are the most likely, on average, to become clinically depressed in the post period (column 1, row 2). The estimate associated with the student subgroup came in at a close second (column 1, row 4), indicating that these individuals are 17% more susceptible to this outcome after a boom relative to other groups. This effect compliments the Cascio and Narayan (2022) argument if perceptions regarding returns to education are lowered across students after a boom occurs. Homemakers, wage earners, and those in retirement (column 1, rows 5, 1, and 3) followed in terms of magnitude, respectively, and are significant at the 1% level. Interestingly, we find a mix bag across behavior changes linked to alcohol consumption.

Table 3.3: The Heterogeneous Effect of Booms on Individual Mental Health & Drinking Habits, by Employment Status

	(1) Depressed	(2) Bad Mental Days	(3) Above Average Bad Days	(4) Average Alcoholic Drinks	(5) Binge Drinking
Reference Category:					
(Fracking Exposure) *x* (Post) *x* (Employed for Wages)	7.75***	1.94***	6.71**	0.15	1.52
	(1.11)	(8.97)	(1.81)	(0.08)	(0.70)
(Fracking Exposure) *x* (Post) *x* (Unemployed > 1 Year)	18.80**	4.53***	20.00***	-0.61**	3.52**
	(3.70)	(0.42)	(2.19)	(0.19)	(0.81)
(Fracking Exposure) *x* (Post) *x* (Homemaker)	12.90***	2.78***	13.3***	0.20	4.46**
	(1.96)	(0.17)	(1.34)	(0.16)	(0.97)
(Fracking Exposure) *x* (Post) *x* (Student)	17.0**	1.66	-9.74**	-0.45**	-6.85**
	(3.17)	(0.95)	(2.91)	(0.14)	(1.52)
(Fracking Exposure) *x* (Post) *x* (Retired)	7.55***	2.27***	10.20**	0.21**	2.41***
	(8.18)	(12.80)	(1.96)	(0.06)	(0.28)
Observations	26,635	26,635	26,635	26,635	26,635
County FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
State-Level Linear Trends	YES	YES	YES	YES	YES

Notes: This table reports estimates of individual mental health and alcohol consumption on a measure of fracking exposure. The fracking exposure indicator is derived from the Bartik et al. (2019) measurement – e.g., a county is geographically positioned in the first (or highest) quartile of the Rystad max prospectivity score within a shale play. The control group consists of other fracking counties located above a shale play but ranked in the lower three quartiles by Rystad in terms of production potential. Household controls include family size, income level, employment, marital status, education attainment, race, and age. The sample consists of all local households surveyed by the BRFSS SMART waves in a particular year. Coefficients and standard errors reported in columns 1, 3, and 5 have been multiplied by 100 to interpret them as percentage point increases or decreases. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Standard errors are in parentheses and clustered at the county level.

Self-employed residents make up the excluded category excluded from regressions reported in Table 3.3. We assume part of the strong negative effects across coefficients associated with mental distress is driven by the argument that the reference demographic is less vulnerable to adverse phycological outcomes spurred by a fracking boom, relative to the other employment groups in our sample. These findings are consistent with Andersson (2008) which finds a strong positive correlation between self-employment and life satisfaction and predicts that this cohort is less likely to view their job as mentally straining. Given that wealth is a major determinant for one's ability to be self-employed (Abdurazzakova and Sherzodbek, 2021), these results seem plausible, as financially, this subgroup has the freedom to choose a career of their liking. Moreover, the boom likely stimulated the local economy in the post period, possibly depreciating economic stressors for these earners.

Results shown across row 3 in Table 3.3 suggest that the homemaker subgroup is more likely to experience a higher level of bad metal days and be clinically diagnosed with depressive disorders after a fracking boom. Coefficient estimates in column 5 indicate that this demographic is also positively correlated with binge drinking behavior. For the portion of homemakers in our sample whose partner's employment is linked to the resource boom, we can assume poor mental health may be heightened, in part, by the new-found solitude they may feel due to their partner's uncommon work schedules that often exist in the energy sector (e.g., 14 days on and 7 days off) (Carrington et al., 2011; Smith and James, 2017). In alignment with this argument, risky drinking habits for this subgroup could be aggravated by sudden household income gains interacting with the possible boredom and/or hardships experienced while living and running their household alone during their partners "on" days.

Results associated with those identifying as being unemployed for more than one year are found across row 2 in Table 3.3. Estimates indicate that long-term unemployment exerts a positive, and quantitatively large influence on binge drinking and depreciating mental health. The positive effect for depression and bad mental days (shown in columns 1 and 2) is the largest in magnitude relative to other types of employment groups and is significant at the 1% and 5% levels. The coefficients associated with these outcomes imply that individuals in this category experience an average of 4.53 more days where they have an uncommon level of stress, depression or significant emotional issues and are 18.8% more likely to be diagnosed as clinically depressed compared to the rest of the population. This is consistent with work by Katja et al. (2000) and Gordo (2006) who show an increased level of phycological distress linked to long-term unemployment in adulthood.

Our last table in this section, Table 3.4, shows the treatment effect of fracking booms for all female respondents relative to males in our sample. Compared to males, women are less likely to binge drink during the post period of a fracking boom (column 5) yet are more likely to be clinically depressed (column 1) and experience a higher amount of bad mental days (column 2) relative to their gender counterparts.

Table 3.4: The Heterogeneous Effect of Booms on Individual Mental Health & Drinking Habits for Females

	(1) Depressed	(2) Bad Mental Days	(3) Above Average Bad Days	(4) Average Alcoholic Drinks	(5) Binge Drinking
Reference Category:					
(Fracking Exposure) *x*	1.69**	0.07**	1.48	-0.03	-1.18**
(Post) *x* (Female)	(0.51)	(0.02)	(0.06)	(0. 04)	(0.27)
Observations	26,568	26,568	26,568	26,568	26,568
County FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
State-Level Linear Trends	YES	YES	YES	YES	YES

Notes: This table reports estimates of individual mental health and alcohol consumption on a measure of fracking exposure. The fracking exposure indicator is derived from the Bartik et al. (2019) measurement – e.g., a county is geographically positioned in the first (or highest) quartile of the Rystad max prospectivity score within a shale play. The control group consists of other fracking counties located above a shale play but ranked in the lower three quartiles by Rystad in terms of production potential. Household controls include family size, income level, employment, marital status, education attainment, race, and age. The sample consists of all local households surveyed by the BRFSS SMART waves in a particular year. Coefficients and standard errors reported in columns 1, 3, and 5 have been multiplied by 100 to interpret them as percentage point increases or decreases. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Standard errors are in parentheses and clustered at the county level.

Estimates in Table 3.4, row 1, columns 1 and 2 provide some evidence congruent with health care research supporting the theory that women are a high-risk group for mental health burden (Afifi, 2007). While the risk of depression has been shown to increase as women age (U.S. Department of Health and Human Services, 1999) the risk of experiencing a depressive disorder by gender begins to deviate from one another early in life. By the time individuals reach adolescence, the female to male ratio of depression rises from 1:1 to 2:1 (Nakamura, 2005), indicating that women are twice as likely than men to endure serious mental health disorders going forward.

2.6 Potential mechanisms

2.6.a Theories behind economic expansion and severe societal mental health outcomes

Having found that the cyclical nature of booms is negatively correlated with mental health outcomes of treated residents, this section explores some potential channels underlying these findings and how this work and previous literature support specific coefficient estimates reported in this paper. Measurements attributing to severe mental health outcomes could be explained through three key mechanisms: (1) the internal migration effect, (2) the direct health effect, and (3) the indirect health effect. Taken together, these channels shed some like on the tendency fracking booms have to exasperate mounting pressures relating to cultural society. The remainder of this section provides a brief explanation of each effect and supportive evidence as to why each is likely a contributing cause of poor mental health outcomes in the post period across our treatment group.

2.6.b Internal migration effect

The first potential mechanism links surges in an influx of workers and increased demand stress on a previously smaller economy and the local population. This effect is likely associated with local residents feeling threatened and resisting cultural changes spurred by the in-migration trend.[21] Typically, the vast majority of in-migrants are a migrating to a specific type of job opportunity and often cause fundamental demographic shifts in a community.

Likely due to the physically demanding nature of oil and gas jobs, the majority of workforce migrants in this field are young men in their early twenties who are unmarried at the time of their move for work (James and Smith, 2017; Street, 2020; Wilson, 2020). While this trend is typical for most migrant groups all over the world moving for work opportunities, it has been estimated that three-fourths of mental disorders are accrued by individuals between the years of 18 and 25 (Kessler et al., 2007). Thus, this workforce is arguably more vulnerable to experience poor

[21] This is similar to cultural anxiety impacts highlighted in analyses on political extremism (Hirsch et al, 2014).

mental health conditions brought on by the effects of fracking given their age.

2.6.c Direct health effect for young adults

Medical research shows individuals with observed suicidal tendencies likely suffer from numerous mental disorders[221] (Gasmsner et. Al, 1959), and those illnesses are often directly affected by societal factors. This is especially true in younger cohorts, who share the highest risk of developing severe, long-lasting mental disorders that often were escalated by social determinants (World Health Organization and Calouste Gulbenkian Foundation, 2014). Evidence from the World Health Organization (WHO) find young adults to be persistently more likely to develop mental disorders if their self-perception of themselves and professional accomplishments fall short when comparing themselves to their peers (2011). This particular link between the labor market and mental health outcomes of young people is relevant to our study, as the majority of the energy sector workforce affiliated with shale extraction tend to be young men in their early twenties (Street, 2020).

Table 3.4 in this paper reports the heterogeneous effect of fracking booms on mental health outcomes disaggregated by gender. Estimates show some evidence that men are more likely than women to binge drink in the post period. These findings are harmonious with sociology literature maintaining women often exceed men in *internalizing* disorders – such as depression and anxiety – while men tend to exhibit more *externalizing* disorders – such as substance abuse (Mouzon and Rosenfield, 2013).

2.6.d Indirect Health Effect

These channels are identified by interactions across economic and social landscapes that are associated with local amenities and the adoption of fracking within localities. For example, Bartik et al. (2019) found that while fracking activity is a driver of oil and gas recovery and

[1] According to the World Health Organization, mental disorders include the following: depression, bipolar disorder, schizophrenia and other psychoses, dementia, and developmental disorders including autism (2020).

vastly improves typical economic indicators, it also can deteriorate local amenities (including increased crime rates, traffic, and declines in local health conditions). Complimentary to this argument, a growing subset of literature has argued that noise and light pollution caused by fracking operations have led to increased feelings of irritation, unease, and fatigue associated with stress and sleep disruption (Korfmacher et al., 2013; Remond and Faulkner, 2013; Coram et al., 2014; Werner et al., 2015). These adverse impacts on wellbeing could also be a viable channel driving adverse outcomes across communal mental health.

2.7. Robustness Check

To account for potential spillover effects initiated by booming production in top-quartile counties, in this section we apply an alternative identification strategy as a mechanism check. Fracking opportunities might have spillovers on other counties for multiple reasons. First, counties in close physical proximity to top-quartile counties may experience spillover effects if, within the boundaries of their own communities, residents experience an increase in fossil fuel recovery (Bartik et al., 2019). Second, it is rational to assume that a spikes in economic activity from top-quartile counties can directly impact nearby counties through externalities associated with resource booms. In the case that nearby counties' economies are benefiting from the resource extraction of a top-quartile county, our estimates would likely be under-estimating the impacts on the treatment group.

Following the James and Smith (2015) paper, we apply an alternative treatment definition based on areas located above booming shale formations. This, more relaxed, identification strategy exploits the spatial variation of underground oil and gas shale deposits coupled with the U.S. temporal variation across regions in shale energy production. Counties are assigned into treatment if their geographic center resides above what is defined as a "booming shale play".[9] The author's baseline specification then defines control group counties as all other U.S. counties. Appling the same strategy to our survey dataset, we observe twelve counties across five states

[9] We apply the same meaning for "booming shale play" utilized by James and Smith (2015) and is defined as plays which attributed to at least 1% of the ramped-up increase in U.S. oil and gas production between 2002 and 2012. Growth estimates were derived from data collated from the Energy Information Administration (2012).

whose center coordinates are located within a booming production area. Once restricting this sample size to include only those counties who are consecutively observed through BRFSS interviews for our entire range of data (2002-2012), we are left with a balanced sample of six counties in which 50,446 individuals are identified as treated across four states.[10] The remaining 1,967,745 within our sample pool reside in the ninety-two remaining counties and form the control group.

Although we assume these results are less robust to our main empirical model (based on counterfactual arguments discussed in prior sections), this, more flexible, specification relaxes our advanced model's constraints. By assigning treatment and control counties in this way, before and after observations are no longer restricted to be from respondents residing within shale play counties. Through the inclusion of counties outside shale plays, results aligning with our central findings support the likelihood that our estimates contain little to no spillovers.

The effects of shale booms over time are estimated with the following equation:

$$Y_{hct} = \alpha_h + \emptyset_h(\,Boom\,Play_c * Post_t) + B_3X_h + state_h * t + \rho_t + \partial_c + \varepsilon_{hct}, \qquad (3)$$

where the dependent variable, Y_{hct}, measure different proxies for mental health outcomes for head-of-household h, residing in county c, during time t where t = 2002, ..., 2012. $Boom\,Play_c$ is a binary variable equal to one if the center county c lies above a booming play, $Post_t$ turns on during all years from 2005 onward, $state_i * t$ control for state-level linear trends, and B_3X_h is a vector of demographic controls variables. Year fixed effects, denoted by ρ_t, control for factors that affect the mental health of all households equally in a given year, such as the Great Recession. County fixed effects, ∂_c, all also included as a vector of dummy variables and account for any cross-county differences in the propensity to experience poor mental health outcomes.

Much like our main regression specification, our variable of interest, specified as the interaction between $Boom\,Play_c$ and $Post_t$ in equation (1), is the difference-in-differences estimator. Essentially, this variable measures the difference in mental health outcomes of head-of-household respondents residing in booming shale play counties relative to otherwise similar

[10] The 6 counties in the treatment group are: Adams, Colorado, Arapahoe, Colorado, Denver County Colorado, Allegheny Pennsylvania, Kanawha, West Virginia, and Laramie, Wyoming.

head-or-household respondents in nontreatment counties. Analogous to all difference-in-differences methodology, this model relies on the assumption that in the absence of a booming shale play, residents' mental health in treatment counties would have changed similarly over time with that of residents in non-treatment counties. Table 4 below reports these regression findings.

Table 4: The Impact of Booming Plays on Mental Health

	(1) Depressed	(2) Bad Mental Days	(3) Above Average Bad Days	(4) Average Alcoholic Drinks	(5) Binge Drinking
(Post 2005 x Booming County)	1.00**	0.20**	1.51**	0.08	1.09***
	(0.00)	(0.09)	(0.01)	(0.05)	(0.00)
Observations	1,694,495	1,694,495	1,694,495	1,694,495	1,694,495
R-squared	0.086	0.106	0.085	0.084	0.048
County FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
State-Year Linear Trends	YES	YES	YES	YES	YES

Notes: This table reports the impacts of a fracking boom on mental health and alcoholic consumption outcomes using an alternative model specification. All regressions include county and year fixed effects and state-linear trends. Columns 1-3 are measured monthly. Outcomes variable formulas were generated using the Center for Disease Control's (CDC) definitions of mental health conditions. The six treatment counties include: Adams, Colorado, Arapahoe, Colorado, Denver County Colorado, Allegheny Pennsylvania, Kanawha, West Virginia, and Laramie, Wyoming, Standard errors are clustered at the county level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1 represent significance level, respectively.

The coefficient estimates in Table 4 indicate that, on average, resource booms lead to mental health issues and risky alcohol consumption. These findings relate closely with the principal findings of our paper, providing: (i) confidence that our identification strategy was not confounded by spillover effects; and (ii) further evidence on the robustness of our results in Section 2.5.

2.8 Conclusion

This paper links two current topics of interest – mental health and industrial fracking booms. Both can be assumed will continue to be heavily debated policy matters for the foreseeable future. In summary, we find that the treatment group's mental health conditions are worsened during the post period in response to a fracking boom. Effects vary in intensity once we interact our variable of interest with demographic subgroups, making the argument that certain individual's mental health conditions are more vulnerable to the impacts of fracking.

Our sample pool consists only of counties where fracking takes place and whose geographic location is above a shale play. Criteria separating the treatment versus the counterfactual groups is based on the geological feature's concrete characteristics relating to the ease of fracking within a county and the potential profits projected. By exploiting a treatment definition primarily determined by geography, we argue our identification strategy is convincingly exogenous. Our main advantage of exploiting within state, within shale play, across county variation is that all our counties we observe actually experienced a fracking boom, allowing us to make the argument that we are estimating the true treatment-on-treated effect.

While it is unclear how long the reliance on crude oil production will last until we see a significant transition towards clean energy usage, attested forecasters continue to project that shale gas will account for 70% of total U.S. natural gas production by 2040 (*U.S. Energy Information Administration,* 2021). With geopolitical tensions on the rise in pursuit of lowering harmful green gases, it's likely that many fracking states are currently faced with a long-term balancing act, as government officials sign carbon-reduction roadmaps to clean energy transition while simultaneously continuing to rely on strong extraction levels for state and local revenues as well as the jobs the oil and gas industry provides.

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Appendix A

Table A.1: Summary Statistics (N=60,498)

	(1) Mean	(2) Std. Dev.	(4) Min	(5) Max
Mental health variables				
Depressed	0.100	0.299	0	1
Bad Mental Days	3.127	7.528	0	30
Above Average Bad Days	0.207	0.405	0	1
Average Alcoholic Drinks	1.136	1.963	0	76
Binge Drinking (>= 5)	0.044	0.205	0	1
Control variables				
Female	0.601	0.489	0	1
Age brackets	53.57	17.20	18	99
18-23	20.68	1.72	18	23
24-29	26.71	1.69	24	29
30-36	33.16	2.01	30	36
37-42	39.58	1.71	37	42
43-47	45.05	1.40	43	47
48-53	50.55	1.70	48	53
54-59	56.47	1.70	54	59
60-65	62.41	1.74	60	65
66-71	68.43	1.68	66	71
72+	78.93	5.05	72	90
No of children under 18				
No child	0.679	0.467	0	1
One	0.129	0.335	0	1
Two	0.121	0.326	0	1
Three	0.049	0.215	0	1
Four	0.015	0.122	0	1
Five and more	0.006	0.081	0	1
Race				
Refused/Not asked	0.987	0.114	0	1
White	0.008	0.093	0	1
Black	0.002	0.046	0	1
Asian	0.002	0.016	0	1
Pacific Islander	0.000	0.011	0	1
American Indian	0.001	0.036	0	1
Other race	0.001	0.028	0	1
Marital status				
Married	0.556	0.497	0	1

Divorced	0.150	0.357	0	1
Widowed	0.240	0.329	0	1
Separated	0.022	0.147	0	1
Never Married	0.123	0.329	0	1
Couple	0.026	0.156	0	1
Education				
High School Drop-Out	0.081	0.274	0	1
HS Grad/Some College	0.584	0.493	0	1
College graduate	0.335	0.472	0	1
Employment status				
Employed for wages	0.485	0.499	0	1
Self-employed	0.078	0.268	0	1
Unemployed > 1 Year	0.021	0.141	0	1
Unemployed < 1 Year	0.270	0.161	0	1
Homemaker	0.830	0.276	0	1
Student	0.018	0.131	0	1
Retired	0.229	0.420	0	1
Unable to work	0.060	0.238	0	1
Household income				
$0k-$14k	0.113	0.316	0	1
$15k-$24k	0.177	0.382	0	1
$25k-$49k	0.285	0.451	0	1
>$50k	0.425	0.494	0	1

Chapter III: Auto Lender Risk Impacts on Household Vehicle Purchases: Less is More

3.1 Introduction

The literature investigating the impact of credit constraints on durable good consumption has been expanding rapidly. The link between relaxed borrowing constraints on household car purchases is our paper's primary focus. Typically, the identification strategy adopted by this line of empirical research exploits plausibly exogenous policy changes such as the staggered state-level banking deregulation and enhanced enforcement of the Community Reinvestment Act to estimate the impact of credit supply on consumption or investment.[231] Our paper exploits the elimination of auto loan cramdowns for bankruptcy proceedings, which expanded auto loans, to study the effect of increased credit supply on the following outcomes at household level: (i) the value of the vehicles owned; (ii) the number of cars owned; and (iii) the loan-to-value (LTV) ratio. We estimate the last outcome to measure asset equity, conditional on having purchased a new car.

In the United States, bankruptcy filings fall under one of several chapters of the Bankruptcy Code. Chapter 13 refers to the proceeding in which debtors undertake a reorganization of their finances under the supervision and approval of the courts. Prior to 2005, in the Chapter 13 plan, an auto debtor could propose to pay only the replacement value of their car to the auto lender instead of the entire loan balance. Thus, what is known as a "cramdown" divided the loan into two claims: a secured claim equal to the market value of the car and an undersecured claim equal

[1] Banking deregulation accelerated during the 1980s and 1990s and were characterized by relaxation of limits on bank entry and expansion across states. Previous literature has shown that these banking reforms have increased credit supply and contributed to economic growth and innovation (see e.g., Strahan, 2003). The Community Reinvestment Act, enacted in 1977, required federal banking regulators to incentivize financial institutions to meet the local credit needs of the community in which they lived.

to the remaining balance owed. It is a common understanding that car value depreciates at a faster pace relative to other household assets, causing a considerable number of borrowers owing more on a loan than their car is worth (Chakrabarti and Pattison, 2019). Cramdown bankruptcy largely benefited the filer by reducing the baseline amount on an undersecured auto loan to match the asset's current market value. Due to vehicle's general rapid depreciation rates, to avoid negative equity, borrowers were more inclined to file for bankruptcy, resulting in market deficiencies on the lender side amounting to hundreds of millions of dollars per year (Hamburger, 2001).

Post 2005, passed legislation changed these conditions, mandating that filers must repay the full original loan amount in order to keep their vehicle asset, despite a vehicle's associated market value or type of auto loan. As a result, auto lenders were less likely to take losses on the undersecured portion of the loan, implying that marginal consumers, deterred by provisions within the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005 (BAPCPA) were more likely to repay their unsecured debts (Gross et al., 2021). The elimination of the cramdown was included in the 2005 BAPCPA – and mainly affected the first two and a half years of an auto loan. The anticramdown provision gives more protection to auto lenders – who are more likely to receive larger payments from debtors filing for bankruptcy and, consequently, lower losses after the Chapter 13 reform.

Our motivation for this paper supports the idea that the bankruptcy reform spearheaded significant changes in lending conditions and was a major driver incentivizing auto lenders to increase their levels of lending to the public. We investigate changes in lending pre- and post-reform for household durable goods through a vehicle asset channel. Our results provide some empirical evidence indicating that the treatment of Chapter 13 reform matters for auto lending. This theory aligns with a study by Gross et al. (2019) whose results imply the enactment of BAPCPA allowed for a significant portion (between 60 and 75 percent) of the cost savings to creditors led by reduced bankruptcy filings to be passed on to consumers.

Utilizing difference-in-differences on data from surveys administered in 2002/3, 2004/5, and 2009/10, we find significant and positive treatment effects on auto asset value per household, the number of cars a household owns, and loan-to-value equity on new car purchases. To investigate if the reform affected demographic groups differently within our sample, we also estimate heterogeneous treatment effects by interacting our variable of interest (e.g., our difference-in-differences estimator) with low-asset households and respondents who identify as African American. When doing this, we find a positive and significant effect on the number of cars per household for both demographic groups. We also find car value to be substantially larger after eliminating cramdowns for African American households, nearly doubling in magnitude relative to our full sample estimates. These findings suggest that Chapter 13 bankruptcy reform helped to decrease the level of discrimination these subgroups had faced previously in terms of borrowing attributed to auto assets.

The role of credit availability in household decision making is important. This line of research has shown that credit expansion influences people to spend more (Gross and Soulels, 2002), is correlated with credit demand (Robertus et al., 2005), and through these channels, has a direct impact on the national economy (Leth-Petersen, 2010). Inversely, credit constraints on households have been shown to deter investment decisions surrounding human capital (Lang, 1994; Card, 1995; Lochner and Monge-Naranjo, 2012), impact income distributions, and put limitations on economic growth and development (Becker, 1975). Recent studies have expanded this line of literature by investigating other potential impacts of credit supply effects on decision making. Leth-Petersen (2010) provided evidence that relaxing credit constraints impacts younger cohorts' borrowing decisions the greatest. This aligns with several other papers' arguments on decisions to attend college after relaxing constraints (Carnero et al., 2002; Kean and Wolpin, 2001; Lochner and Monge-Naranjo, 2012).[242] In general, these papers provide evidence that improving credit access to young adults is an important determinate of savings accumulation and

[2] It should be noted that Cameron and Taber (2004) argue against this theory and find little to no effect on decisions to attend college after relaxing borrowing constraints.

working during school (Kean and Wolpin, 2001) and may delay enrollment and affect the quality and completion of a degree (Carnero et al., 2002; Lochner and Monge-Naranjo, 2012).

Our paper builds on Chakrabarti and Pattison (2019), which uses the elimination of the ability to cramdown auto loans under Chapter 13 bankruptcy proceeding to show that interest rates on auto loans fell in states that disproportionately used Chapter 13 bankruptcy. We draw our empirical strategy from this paper by defining our treatment group to consist of the top seven states with highest Chapter 13 filing percentage (forty percent or greater) between 2001 and 2004. The central arguments showcased in the Chakrabarti and Pattison (2019) study are consistent with Muller's (2022) findings that provide empirical evidence showing bankruptcy reform in the U.S. benefits individual lenders in the same way an exogenous should credit supply would. Hart and Moore's (1994) theoretical research, pioneering the way for this type of paper, also supports Muller's (2022) results, arguing that a drop in bankruptcy caseloads tends to even out the distribution between risky and safe borrowers. Ankney (2022) investigates the impact of auto loan rates on the fuel efficiency of the car, finding little relationship between the two. Our analysis contributes to this body of literature by exploring vehicle investment decisions by households given an arguably exogenous shock in credit supply as a result of auto loan reform.

Finally, our paper adds to the stream of research motivated by wealth gaps across population distributions. The income inequality gap has increased substantially in past decades (Pfeffer and Schoeni, 2016). To investigate if bankruptcy reform affected subgroups of our sample pool differentially, we also break out our baseline effect by low-asset and African American households. These heterogeneous treatment effects allow us to measure differential changes caused by the policy reform by wealth and race-ethnicity categories, relative to the full sample of households. Recent literature has argued this unequal growth in wealth can partially be explained through investment decisions made by different demographic channels, including households with children dependents (those who would likely benefit most from acquiring additional autos) (Gibson-Davis and Percheski, 2018) and individuals identifying as minority racial-ethnicities (Bandeji and Grigoryeva, 2021).

More broadly, Raphael and Stoll (2001) provide strong evidence linking the effect of car ownership on the probability of being employed for minorities residing in spatially isolated populations across the U.S. (Raphael and Stoll, 2001). Potentially escalating this effect, at a national level, recent data trends show significant disparities in the distribution of vehicles across households of different racial groups, with households headed by individuals identifying as "Black and/or People of Color" to be the least likely to have access to a vehicle (National Equity Atlas, 2019). Accordingly, having the purchasing power (e.g., access to available credit) to own a car may be an important instrument in helping to close racial gaps in employment and asset wealth. Our results provide some evidence that Chapter 13 bankruptcy reform increased favorable lending conditions for this subgroup and, perhaps, decreased the level of unfair discrimination relating to auto lending that African American households previously faced.

The remainder of the paper is organized as follows. Section 3.2 details our dataset and defines our main variables. Section 3.3 provides an overview of our empirical strategy and model. Section 3.4 presents the results, and Section 3.5 concludes.

3.2 Data

To understand how changes to bankruptcy rules impact the availability of credits and the resulting household auto decisions, we utilize three waves from the Survey of Income and Program Participation (SIPP). This dataset is ideal as it has consistent information on automobile numbers and value by households together with a rich set of covariates just before and after the period of the 2005 bankruptcy reform. During those years, the SIPP selected close to 50,000 households to participate in short-term panel survey studies in which a core set of questions was asked every four months to the same households over a period of three to four years. Alongside this core set of questions, a rotating set of topical modules was included. At the end of this three-four-year period, the survey is administered to a new randomly selected sample of households – a new Panel – for the next three-four years, and so on. In other words, the survey is organized in panels, within which the same households are followed for a period of three to four years before a new panel of households is selected for another three to four years.

The main outcomes of interest to us are included in a specific rotating topical module labelled 'Real estate, dependent car and vehicles' which is included in a few waves within each panel's timeframe, then the questionnaire is administered to a set of different households in 2003/4, 2004/5 and 2009/10. In this paper we are extracting information related to automobiles at household level. In this way, we are exploiting three separate household panel groups, which enables us to construct a large sample of cross-sectional data. Rather than observing each calendar year, we aggregate the years to the Panel year to keep the sample pools separate and boost the power of observations across our models.

Table 1: Summary of the Structure of the SIPP Surveys from 2000 Onwards

Panel	Date of first and last interview	Number of households in each panel in our sample	Number of waves	Waves that include information on automobile ownership (Real estate modules)	Time period for topical module on real estate
2001	Feb 01- Jan 04	206,324	9	Wave 3, Wave 6 Wave 9	Oct 01 - Jan 02 Oct 02 to Jan 03 Oct 03 to Jan 04
2004	Feb 04- Jan 08	194,595	12	Wave 3 Wave 6	Oct 04 to Jan 05 Oct 05 to Jan 06
2008	Sept 08-Dec 12	176,616	13	Wave 4 Wave 7	Sep 09 to Dec 09 Sep 10 to Dec 10

Table 1 above provides a breakdown of each panel and summarizes the SIPP structure distinguishing between number of waves of the longitudinal core element and cross-sectional topical modules and detailing the interview periods. Interviews covering the real estate module in the so-called '2001 Panel' were effectively administered between October 2002 to January 2003, those included in the '2004 Panel' were conducted between October 2005 to January 2006 and finally those included in the study named '2008 Panel' were effectively administered in September 2010 to December 2010. Given that the questions around auto ownership are asked multiple times per panel, this analysis is essentially using a separated cross-section dataset of 577,535 households, of which 87 percent own at least one vehicle.

There are three outcome variables utilized in this analysis relating to vehicle assets. The SIPP dataset administers information on a car's value that is determined by the Census Bureau and not a reflection of the household's belief in the asset's value. The Census Bureau assigns vehicle trade-in values by applying the National Automobile Dealers Association (NADA) methodology that uses inputs such as the reported year, make and model to determine the asset's worth. We exploit this information to define each of our three dependent variables. Our first outcome of interest was constructed by applying the inverse hyperbolic sine (or arcsinh) transformation and measures a car's value over time.[253] The arcsinh transformation is a useful tool for estimating this variable, as it allows retaining zero-valued observations and because its range includes large positive values, it can be treated as a natural logarithm (Bellemare and Wichman, 2020). Next, we measure the effect of Chapter 13 on the number of cars in the household. The survey question associated with this outcome observes zero up to three cars per household member, so this variable takes the value of 0,1, 2… up to 20. We then apply the arcsinh technique again, for the same arguments just discussed, to construct our final outcome – the loan-to-value ratio of a new car purchase. For these regressions, we restrict our sample to be conditional on having a new car, where a new car is defined as one whose year of make was two years or less from the year of the interview.[264] Loan-to-value ratios are common metrics used to understand the level of risk in the loan. The loan amount is defined as the household's stated level of money owed on the vehicle.

[3] The arcsinh method is akin to taking the natural log of a value with some additional advantages explained later in the paper. For simplicity purposes, we refer this specification as a natural log in subsequent sections of this paper.
[4] We exploit the arcsinh method to multiple outcomes in this study, as it allows us to interpret the coefficients as elasticities and for observations to equal zero without assigning it as undefined. We are also able to transform negative values by constructing variables this way, contrary to standard log transformations.

Table 2 below displays summary statistics for all dependent and explanatory variables in our regressions. The average household in our sample owns two cars with roughly 15 percent owning at least one new car.

Table 2: Descriptive Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Outcomes					
Arcsinh (Car Value)	577,535	8.13	3.22	0	11.24
Number of Cars per Household	503,048	2.07	1.06	0	20
Arcsinh (Loan-to-Value Ratio)	88,263	0.72	0.51	0	3.69
Household/respondent's					
Age	577,535	36.6	22.7	18	87
Birth Year	577,535	1968.3	22.8	1916	1992
Female	577,535	0.52	0.50	0	1
White	577,535	0.80	0.39	0	1
Black	577,535	0.13	0.33	0	1
Hispanic	577,535	0.12	0.32	0	1
Asian	577,535	0.03	0.17	0	1
Married	577,535	0.56	0.49	0	1
Spouse Absent	577,535	0.14	0.35	0	1
Widowed	577,535	0.12	0.33	0	1
Divorced	577,535	0.04	0.19	0	1
Separated	577,535	0.14	0.34	0	1
Single	577,535	0.12	0.31	0	1
Household Net Worth	577,535	197,436	893,730.7	-2,465,627	2.21e+08
Low-asset Household	577,535	0.27	0.44	0	1

Bankruptcy literature has long debated who are the "winners" and "losers" of the BAPCPA law and whether its legislative impacts are equally inclusive across all socioeconomic and demographic subgroups (see, for example, Deckerson, 2006; Rodriguez, 2007; Cappiello, 2013; and Marzen, 2016). To understand how this policy affected particular demographic groups in our sample pool, we also run heterogeneous treatment effect regressions with a specific focus on race and household assets. These are constructed by interacting a low-wealth dummy variable or an indicator for those respondents identifying as Black of African American with our pre- and post-treatment years with our Chapter 13 treatment variable. Black is a binary variable that takes the value of one if the head of the household respondent confirmed he was African American and zero otherwise. The indicator variable associated with households having low total net worth is a binary indicator that turns on if one's total net worth falls into the first (e.g., the lowest) quartile.

We define this as having a value of aggregated assets totaling to less than $45,000, but greater than zero. This is approximately 34 percentage points lower than the sample's median amount at $68,405 but accounts for approximately 25 percent of our total sample, allowing us to observe the lowest quartile in respect to household assets.[275] We utilize these estimates as a proxy to measure potential discrimination effects.

To analyze the impact of pre-reform and post-reform on household consumption of automobile assets, we assign our treatment and control groups based on the Chakrabarti and Pattison (2019) strategy, which exploits the recorded variation in states' usage of Chapter 13 generated by disparities across local legal cultures. Previous literature defines local legal culture as the way in which legal representatives, judges, and trustees implement/handle universal federal bankruptcy code (e.g., differences in legal incentives and lawyer fees received for filings that impact a state's magnitude of Chapter 13 usage) (Braucher, 1993; Sullivan, Warren, and Westbrook, 1994; Lefgren and McIntyre, 2009; Chakrabarti and Pattison, 2019). Subsequently, we can assume the elimination of cramdowns have larger effects in states where Chapter 13 usage has consistently been more common.

Based on the discussion above, our treatment states consist of those where Chapter 13 consumer bankruptcy filings were historically the most consistent during our pre-reform period. Specifically, we define this group as the top seven states whose fraction of bankruptcies files of this classification were forty percent or greater between 2001 and 2004. These include Alabama, Arkansas, Georgia, North Carolina, South Carolina, Tennessee, and Texas. The remaining forty-three states make up our control group. The breakout of states by filing percentage thresholds is shown in Appendix A, Table A.1 of this paper. Additionally, we recognize that many of our treated states are largely located in the Southeastern region on the U.S. We address the concern

[5] We utilize total net worth as our main wealth measurement (rather than monthly income) to define this variable, as total net worth aggregates household assets and includes debt levels of households – the latter provides an additional advantage, as it includes one's future earnings assigned to debt repayment.

for biased results driven by region-specific shocks that affect our outcomes in the following section.

3.3 Empirical Methods

In this analysis we utilize an event study difference-in-difference (DiD) methodology. We have information on household automobile choices for three separate waves (2001, 2004, and 2008) and use the exogenous elimination of auto loan cramdowns in Chapter 13 bankruptcy proceedings in 2005 as the beginning of the treatment period. This will be the first difference in the DiD methodology. The second difference comes from the highly uneven use of Chapter 13 bankruptcy across the US. As discussed thoroughly in Chakrabarti and Pattison (2019), a subset of U.S. states used Chapter 13 bankruptcy extensively while the other states rarely used Chapter 13 bankruptcy.[286] The difference between the Chapter 13 high-usage states and the "non"-Chapter 13 usage states is the second difference in the DiD methodology. It allows us to estimate how changes in auto asset gains upon the reform's enactment vary with states' Chapter 13 usage.

The general form of the model, estimating the effects of Chapter 13 reform on auto assets, is shown by the equation below:

$$Y_{it} = \alpha + \beta_1\, WaveDummy_t + \beta_2 Ch13_i + \beta_3 (WaveDummy * Ch13)_{it} + \beta_4 X_{it} + \delta_j + \tau_t + \epsilon_{it} \quad (1)$$

Where Y_{it} is either: presence of a new car, the number of cars in the household, car value, or the loan to value ratio of a new car purchase and corresponds to household i in wave year t. $WaveDummy_t$ is a vector of year indicator variables associated to the three survey waves in our sample. $Ch13_i$ (e.g., our treatment indicator) represents the average fraction of bankruptcies filed under Chapter 13 in a state corresponding to household i from years 2001-2004. The coefficient β_3

[6] Refer to Appendix A, Table A.1 for the full list of treated and controlled states and their associated average fraction of bankruptcies filed under Chapter 13 from 2001-2004.

captures the impact of the eliminating cramdowns reform on auto asset purchases at the household level. X_{it} is a vector of control variables including: gender, race, age, marital status, total net worth aggregated at the household level, and birth year. All regressions include state and year fixed effects, indicated in equation (1) by δ_j and τ_t, respectively. State-level fixed effects control for time-invariant factors that generate state variation in Chapter 13 usage which could affect auto loans (e.g., local legal culture, population characteristics, etc.). Year fixed affects controls for elements linked to the economy and the reform that affected all states at a macro-level. To account for a plausible state-over-time correlation component present in the error term (ϵ_{it}), we cluster the standard errors at the state-level. When estimating number of cars in a household, a Poisson model is used.

To measure heterogenous treatment effects, we then estimate multiple forms of equation (2) below:

$$Y_{it} = \alpha + \beta_1\, WaveDummy_t + \beta_2 Ch13_i + \beta_3 (WaveDummy * Ch13)_{it} + \beta_4 (WaveDummy * Ch13 * Subgroup\ Dummy)_{it} + \beta_5 X_{it} + \delta_{\mathrm{j}} + \tau_{\mathrm{t}} + \epsilon_{it} \quad (2)$$

Where all terms are as defined in equation (1) apart from the interacted $Subgroup\ Dummy$ variable. We run this slightly modified version of our main model to understand if Chapter 13 reform impacted some subgroups of our sample differently by triple-interacting our variable of interest, associated with β_4, with racial and wealth categories. Specifically, we separately interact the difference-in-differences estimator with two binary variables that turn on if: (i) the head of household respondent identifies as "Black, or African American;" or (ii) if that household's aggregate net worth categorizes them into a low-asset group (defined as totaling $45,000 or less). This specification can be viewed as a form of a triple-difference-in-differences model, comparing changes around the legislative reform on car-related assets between separate subgroups relative to the full sample. In this way, our results allow us to identify potential discrimination effects encumbered by these demographics through an auto loan channel.

There are a couple of threats to the identification of the impact of this bankruptcy reform on household car decisions, (e.g., the coefficient associated with β_3 in equation (1) and β_4 in equation (2)). First, the states which predominantly used Chapter 13 bankruptcy (treated states) could be changing their norms differently than those that did not commonly use Chapter 13 bankruptcy (control states).[297] Although Chapter 13 usage is potentially endogenous, our main specification holds constant time-invariant factors that generate differences in overall behavior of Chapter 13 states (β_2) by including state-level fixed effects. Second, Chapter 13 bankruptcy states are mostly in the Southeast of the United States. This causes validity issues if regional shocks that affected auto lending were driving our results. While the close geographic concentration of treated states is a concern, we test for differential pre-existing trends that may confound our estimates by interacting our treatment variable with the survey period prior to the reform change. We report these findings along with our coefficients of interest in the results section of this paper and conclude that there is little evidence of pre-trend effects. On top of addressing the first two concerns, all of our regressions include a rich set of control variables to bolster confidence are not driven by household specific factors.

Further supporting our credit mechanism argument, Chakrabarti and Pattison (2019) discuss that the biggest winner of the 2005 BAPCPA was the auto lending industry, implying that few other industries would have been impacted by the policy and alter their credit behavior to invalidate the SUTVA assumption. Complimentary to this point, the Car Allowance Rebate System, otherwise known as Cash for Clunkers, was in effect July and August of 2009. This was a few months before the 2008 Panel was asked about their car ownership. The defense against this threat is to rely on the differential change in outcomes for the Chapter 13 bankruptcy states relative to the other states given that Cash for Clunkers was a nationwide program.

Conditioning on state and year fixed effects, our identification relies on the parallel trends assumption (e.g., if cramdowns were not eliminated through Chapter 13 reform, auto lender volume in treatment and control states would have followed similar trends). In other words, if cramdowns were not eliminated, changes in auto durable good assets across households would

[7] Table A.1 in Appendix A displays the geographic variation between treated and control states in our sample, as well as the portion of bankruptcies that were filed during the years leading up to Chapter 13 reform.

not be correlated with historical Chapter 13 usage. The BAPCPA restrictions on use of the auto loan cramdowns in Chapter 13 bankruptcies took effect in October 2005, which is when the 2004 Wave asked its questions surrounding auto assets to respondents. Given this timing, we construct the model such that the 2004 Wave is the reference category and the coefficients on other years are relative to this year. With this set-up, the coefficient on the 2001 Wave Chapter 13 state interaction acts as a pre-trends test to reveal whether the Chapter 13 states were acting differently than the non-Chapter 13 states before the BAPCPA eliminated the auto loan cramdown possibility for Chapter 13 bankruptcy.

3.4 Results

Tables 3 through 4 report estimates of the effect of Chapter 13 bankruptcy reform on our variables of interest in the pre- and post-periods. We first assess our simple difference-in-differences model from the baseline specification in equation (1). These coefficient estimates are given in column 1 of the next three tables and reflect our entire sample pool. Results in columns 2 and 3 of this section report heterogeneous treatment effect measurements for our two selected demographic variables. Estimates from these columns are derived by applying our triple-difference-in-differences specification shown in equation (2). We exploit this methodology to observe how the reform impacted certain subgroups differently. Specifically, column 2 represents the interaction of our first difference-in-difference estimate (e.g., our treatment variable interacted with pre- and post- reform periods) with a low-asset household dummy, allowing us to compare and contrast the households that fall into this category relative to the rest of the sample. We then exchange explanatory variables to estimate the effect of cramdown elimination on respondents identifying as African American, relative to all other races in our sample. Estimated heterogenous treatment effects for this subgroup are shown down column 3 in the tables below.

To test for changes in asset value relating to vehicles, we begin our analysis by estimating the main model at the household level with the natural log of car value as the outcome of interest. These results are given in Table 3. The coefficients shown in rows 1 and 2 are not statistically significant from zero, suggesting no sign of a pre-trend across the treatment effects in columns 1, 2 or 3. This supports our identifying assumption, suggesting there is no evidence of widespread financial gains relating to household autos prior to the policy reform in 2004. The estimates shown in columns 1 through 3 across row 3 are positive and statistically significant from zero, indicating households in treated states (e.g., states where Chapter 13 is historically common) are more likely to gain value in auto assets after the anticramdown reform. These findings align with the argument suggesting the elimination of cramdowns led to more favorable lending terms through lower auto loan interest rates and increased access to credit (Chakrabarti and Pattison, 2019).

While the triple-interaction estimate in row 4 column 2 – measuring the impact of the reform on low-asset households relative to the rest of the sample – is not statistically significant from zero, we do find a positive and significant heterogeneous treatment effect on the coefficient associated with the African American subgroup in row 4 column 3. The treatment effect for this demographic is pronounced, nearly doubling in magnitude relative to the rest of the sample. This result provides empirical evidence in favor of the idea that African American households were relatively more likely to gain financial worth in terms of durable good assets after the policy reform. This lends some support to the assumption that this group faced higher levels of discrimination by lenders prior to the legislative change.

Table 3: The Impact of Chapter 13 Bankruptcy Reform on Car Value

Subgroup	(1) ***None***	(2) ***Low-asset Household***	(3) ***African American Head of Household***
Pre-trend *((Panel Year = 2001) x Chapter 13 Reform)*	-0.016 (0.099)	-0.007 (0.083)	-0.031 (0.129)
Pre-trend *((Panel Year = 2001) x Chapter 13 Reform x Subgroup)*	- -	-0.132 (0.129)	0.022 (0.216)
Post-reform *((Panel Year = 2008) x Chapter 13 Reform)*	0.234** (0.110)	0.265** (0.026)	0.169* (0.099)
Post-reform *((Panel Year = 2008) x Chapter 13 Reform x Subgroup)*	- -	-0.092 (0.102)	0.422* (0.249)
Observations	577,535	577,535	577,535
State FE	YES	YES	YES
Year FE	YES	YES	YES

Notes: *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Robust standard errors are reported in parentheses and are clustered at the state level. Depending on the outcome being measured, individual controls include gender, race, age, marital status, total net worth aggregated at the household level, and birth year.

We repeat this process again to measure the effect of eliminating cramdowns on the quantity of automobiles owned at the household level. Table 4 below reports the total effect and demographic effects of Chapter 13 bankruptcy reform on this outcome. Similar to the pre-reform estimates displayed in Table 3, the results in the first two rows of Table 4, across columns 1 through 3, show no pre-trend effect, further suggesting the parallel trends assumption holds valid in our model and components in the pre-period are unlikely to be driving findings in the post-period.

Although we find no conventional effect for this outcome variable in relation to our full sample, the coefficients associated with our triple-difference-in-differences specifications (shown in row 4, columns 1 and 2 of Table 4) show positive treatment effects in the post-period for both low-asset households and African American respondents. These coefficients are significant at the 10 percent level and suggest that the number of vehicles owned per household increased as a result of the financial reform. This is consistent with the hypothesis that racial minorities and less-wealthy (e.g., high-risk) households were granted more access to credit (causing a surge in purchasing power to those previously discriminated against) and/or better borrowing terms after the reform generated by less risk for auto lenders.

Table 4: The Impact of Chapter 13 Bankruptcy Reform on the Number of Cars per Household

Subgroup	(1) *None*	(2) *Low-asset Household*	(3) *African American Head of Household*
Pre-trend *((Panel Year = 2001) x Chapter 13 Reform)*	0.005 (0.013)	0.001 (0.014)	0.001 (0.011)
Pre-trend *((Panel Year = 2001) x Chapter 13 Reform x Subgroup)*	-- --	-0.007 (0.025)	0.022 (0.029)
Post-reform *((Panel Year = 2008) x Chapter 13 Reform)*	0.011 (0.015)	-0.003 (0.013)	0.002 (0.014)
Post-reform *((Panel Year = 2008) x Chapter 13 Reform x Subgroup)*	-- --	0.040* (0.022)	0.033* (0.019)
Observations	577,535	577,535	577,535
State FE	YES	YES	YES
Year FE	YES	YES	YES

Notes: *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Robust standard errors are reported in parentheses and are clustered at the state level. Depending on the outcome being measured, individual controls include gender, race, age, marital status, total net worth aggregated at the household level, and birth year.

Lastly, we apply the same methodology to estimate the anticramdown legislative impact on a household's loan-to-value (LTV) ratio – defined as the amount a household borrowed from a lender divided by the value of the car bought, expressed as a percentage. Examining the reform's influence through this channel plays an important role in understanding the true treatment effect of lower credit risk on household consumers in the post-period. This outcome is often utilized by lenders to measure the risk associated with approving a particular loan. Negative and significant results in the post-period would suggest that eliminating cramdowns resulted in lower interest rates and/or more loan approvals (thus more access to credit), aligning with the argument that vehicle assets are increased after the Chapter 13 policy reform was enacted. Positive and significant results would imply the opposite, showing negative spillover effects onto borrowers in states where Chapter 13 is historically common, thus potentially indicating negative equity outcomes faced by consumers.[307]

We measure the LTV ratio outcome by restricting the sample to include only those households who confirmed at least one new car purchase in the last two years. Approximately 15 percent of households in our study recently consumed unused vehicles – defined as a vehicle whose year of make was two years or less from the year of the interview – between 2001 and 2010. Regression results are reported in Table 5 below.

Table 5: The Impact of Chapter 13 Bankruptcy Reform on Loan-to-Value Ratios of New Car Purchases

Subgroup	(1) ***None***	(2) ***Low-asset Household***	(3) ***African American Head of Household***
Pre-trend *((Panel Year = 2001) x Chapter 13 Reform)*	-0.004 (0.019)	-0.020 (0.022)	0.002 (0.023)
Pre-trend *((Panel Year = 2001) x Chapter 13 Reform x Subgroup)*	-- --	0.053*** (0.017)	-0.048 (0.035)
Post-reform *((Panel Year = 2008) x Chapter 13 Reform)*	-0.047** (0.020)	-0.061*** (0.021)	-0.051*** (0.015)
Post-reform *((Panel Year = 2008) x Chapter 13 Reform x Subgroup)*	-- --	0.047*** (0.011)	0.102 (0.101)
Observations	88,263	88,263	88,263
State FE	YES	YES	YES
Year FE	YES	YES	YES

Notes: *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Robust standard errors are reported in parentheses and are clustered at the state level. Depending on the outcome being measured, individual controls include gender, race, age, marital status, total net worth aggregated at the household level, and birth year.

We find that full sample effects in row 3, columns 1 through 3, to be negative and highly significant at either the 5 or 10 percent levels in the post period. These results further suggest that treated households did benefit from the Chapter 13 reform through increasing the value of their

[7] The latter is often an end result caused by low-risk lenders charging high-risk households amounts that exceed a new car's retail value.

vehicle assets. Inversely, we find the opposite effect for low-net worth households, indicating their amounts owed on vehicle purchases is higher than the value associated with those assets. It is important to note the pre-treatment trends assumption likely does not hold for low-asset households, however, as the coefficient associated to the interaction effect during the pre-trend is positive and significant. This causes us to assume post-reform estimates in column 2 are not capturing the true effect of eliminating cramdowns at conventional levels.

3.5 Conclusion

This analysis investigates the impact of the 2005 Chapter 13 reform (eliminating auto loan cramdowns) on three outcomes relating to auto assets at the household level. Using detailed administrative data on household assets, we apply a difference-in-differences framework to estimate the effect of Chapter 13 reform on vehicle assets. In this way, vehicle asset quantity and equity outcomes serve our analysis as a proxy measurement to understand if the elimination of auto loan cramdowns spurred price reductions of auto loans and expanded access to credit. Following the Chakrabarti and Pattison (2019) identification strategy we explore the treatment effects of eliminating cramdowns on auto credit markets for households associated with the highest historical use percentage of Chapter 13.

Results indicate that the anti-cramdown provision bolstered vehicle asset value and increased a household's quantity of cars in states with a historically higher share of bankruptcies under Chapter 13. Our central findings support the argument that, after the reform, auto lenders faced less financial risk in the market, which provided positive spillover effects on household consumers through expanding assess to credit and delegating more favorable lending conditions. This is consistent with the arguments led by Chakrabarti and Pattison (2019), who provide evidence that eliminating cramdowns decreased loan interest rates and increased the size and quantity of auto loans, particularly amongst high-risk, subprime borrowers. We test for potential biases by interacting both pre- and post-reform periods and find little corroborating evidence of pre-trend components driving our effects.

We additionally explore a triple-difference-in-differences specification that allows us to examine heterogeneous treatment effects by race and wealth subgroups. Estimates document evidence that cramdown elimination elevated the number of cars per household for both African American respondents and low-asset households relative to other demographic groups. We also find a modest, positive effect suggesting African American households gained asset value in terms of vehicle consumption after the Chapter 13 reform. Together, these results postulate the idea that Chapter 13 bankruptcy reform helped decrease the level of discrimination low-asset (e.g., high-risk) households and African Americans faced in terms of borrowing.

To conclude, our results argue that this policy reform was a major driver increasing lending levels and car ownership across all households, shocking accessibility to credit. Our findings should be important an important tool for policy makers looking to decrease the wealth gap in the U.S. through lowering asset disparities across different socioeconomic and demographic channels.

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Appendix A

Table A.1: Fraction of Bankruptcies File Under Chapter 13 from 2001-2004 Across U.S. States

(1) State	(2) Fraction Percent	(3) Treated State
Alabama	40+%	Y
Arkansas	40+%	Y
Georgia	40+%	Y
North Carolina	40+%	Y
South Carolina	40+%	Y
Tennessee	40+%	Y
Texas	40+%	Y
Delaware	30-40%	N
Louisiana	30-40%	N
Maryland	30-40%	N
Mississippi	30-40%	N
New Jersey	30-40%	N
Pennsylvania	30-40%	N
Utah	30-40%	N
Florida	20-30%	N
Illinois	20-30%	N
Michigan	20-30%	N
Missouri	20-30%	N
Nebraska	20-30%	N
Nevada	20-30%	N
Ohio	20-30%	N
Virginia	20-30%	N
Arizona	10-20%	N
California	10-20%	N
Colorado	10-20%	N
Connecticut	10-20%	N
Idaho	10-20%	N
Indiana	10-20%	N
Kansas	10-20%	N
Kentucky	10-20%	N
Massachusetts	10-20%	N
Minnesota	10-20%	N
Montana	10-20%	N
Oklahoma	10-20%	N
Oregon	10-20%	N
Vermont	10-20%	N
Washington	10-20%	N
Wisconsin	10-20%	N

Iowa	0-10%	N
Maine	0-10%	N
New Hampshire	0-10%	N
New Mexico	0-10%	N
North Dakota	0-10%	N
Rhode Island	0-10%	N
South Dakota	0-10%	N
West Virginia	0-10%	N
Wyoming	0-10%	N

Notes: This table displays the state-by-state variation in the average fraction of bankruptcies files under the monetary policy reform between 2001 and 2004 in the U.S. This data was collected from the Administrative Office of the U.S. Court.