Less is More: The Impact of Auto Lender Risk on Household Auto Purchases

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Credit risk can be an impediment to new auto purchases, especially for electric vehicles. This paper looks at the elimination of auto loan cramdowns for Chapter 13 bankruptcy proceedings, where the loan value is made equal to the auto value, on three outcomes: auto value, likelihood of new auto, and loan-to-value ratio of new autos. Using a difference-in-difference approach based on a state's historical use of Chapter 13 bankruptcy, we show that household's secure better loan-to-value ratios and acquire higher valued autos due to lower credit risk following the reform. Black households are more likely to have a new car while low-asset households have worse loan-to-value ratios then the average. Together, these results indicate that lower risk to auto lenders can facilitate new electric vehicles purchases.

I. Introduction

The literature investigating the impact of credit constraints on durable good consumption has been expanding rapidly. Lender risk is an important component of credit availability as shown in Gross et al (2021), Muller (2022), and Mitman (2016). These analyses reveal improvements to macroeconomic performance from reduced lender risk utilizing the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005 (BAPCPA) as an exogenous shock. 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 (Strahan, 2003; Saadi, 2020). Our paper focuses on a large lender market, new automobiles, to better understand the link between lender risk, through changes brought forward in BAPCPA and household car purchases. To identify the effect, we exploit the elimination of auto loan cramdowns for bankruptcy proceedings, which lowered auto loan rates. We study the effect of reduced lender risk on the following outcomes at household level: (1) the value of the vehicles owned; (2) likelihood of new autos in the household; and (3) the loan-to-value (LTV) ratio. We estimate the last outcome to measure asset equity, conditional on having a new auto.

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 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).

BAPCPA 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 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 household auto outcomes 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. (2021) 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 a difference-in-differences methodology on Survey of Income and Program Participation data in 2002/3, 2004/5, and 2009/10, we find treatment effects on auto asset value per household and loan-to-value ratio 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 Black. When doing this, we find a positive and statistically significant effect on the probability of a new car in the household for Black households. We also find car value to be statistically larger after eliminating cramdowns for Black households, nearly doubling in magnitude relative to our full sample estimates. These findings suggest that Chapter 13 bankruptcy reform helped increase auto credit availability to these subgroups.

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; Sun and Yannelis, 2016), impact income distributions (Beck et al, 2010), debt levels (Brown et al, 2019), 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).[1] In general, these papers provide evidence that improving credit access to young adults is an important determinate of savings accumulation and 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 increase in 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

[1] 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.

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 Black households. These heterogeneous treatment effects allow us to measure differential changes of 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 US. Potentially escalating this effect, at a national level, is 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 auto lending conditions for these households.

Finally, credit risk in the auto market is important for advancing environmental goals. Jacobsen et al, (2022) show that the emissions rate for local air pollutants are much smaller rate for newer cars and thus there are welfare benefits to getting more new cars on the road. For climate and local air pollution reasons, many countries are trying to advance electric vehicle sales with an increasing concern for getting these vehicles into low-income and minority communities. Our work points to mechanisms previously unexplored about the way risk to auto lender impacts the ability of household to acquire newer, more expensive cars (such as electric vehicles).

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The remainder of the paper is organized as follows. Section II details our dataset and defines our main variables. Section III provides an overview of our empirical strategy and model. Section IV presents the results, and Section V concludes.

II. 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. 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 2001 to January 2004, those included in the '2004 Panel' were conducted between October 2004 to January 2006 and finally those included in the study named '2008 Panel' were effectively administered in September 2009 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[2]. Next, we measure the effect of Chapter 13 on the likelihood of a new car in the household. A new car is defined as the model year being two years or fewer from the interview year. Our final outcome, the loan-to-value ratio of a new car purchase is the amount the household's stated as being owed on the vehicle divided by the Census Bureau's calculation of the value of the vehicle. For these regressions, we restrict our sample to be conditional on having a new car. Loan-to-value ratios are common metrics used to understand the level of risk in the loan.

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. There are some outliers in the household net worth variable, however removing those do not change the economic significance of the results.

The literature has 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 interact a low-wealth dummy variable or an indicator for those respondents identifying as Black or Black 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 they are Black and zero otherwise. The indicator variable associated with households having low

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[2] The arcsinh transformation is a useful tool for estimating this variable, as it its range includes large positive values, it can be treated as a natural logarithm (Bellemare and Wichman, 2020).

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.[3]

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Table 2. – Descriptive Statistics

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Variable	Obs	Mean	Std. Dev.	Min	Max
Outcomes					
Arcsinh (Car Value)	577,535	8.13	3.22	0	11.24
Likelihood of New Car	577,535	0.15	0.35	0	1
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	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	-2,465,627	2.1e+08
Low-asset Household	577,535	0.27	0.44	0	1

To analyze the impact of BAPCPA 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 bankruptcy 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 arguethe elimination of

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[3] 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.

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 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.

III. 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.[4] 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:

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

Where Y_{it} is either: presence of a new car, 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

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[4] 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.

indicator variables associated to the three survey waves in our sample. Note that the interaction between $WaveDummy * Ch13$ equates to a (placebo) test of parallel trends across households in treated vs control states before the treatment (we will elaborate more on this at the end of this section). $Ch13_i$ (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 captures the impact of the eliminating cramdowns 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.

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

$$Y_{it} = \alpha + \beta_1\, WaveDummy_t + \beta_2 Ch13_i + \beta_3 (WaveDummy * Ch13)_{it} + \beta_4 (WaveDummy * Ch13 * Subgroup\ Dummy)_{it} + \beta_5 X_{it} + \delta_j + \tau_t + \epsilon_{it}\ (2)$$

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 interacting our treatment variable, associated with β_4, with racial or wealth categories. Specifically, we separately interact the difference-in-differences estimator with two binary variables that turn on if: (1) the head of household respondent identifies as "Black, or African American;" or (2) 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 differential effects encumbered by these demographics through an auto loan channel.

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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).[5] 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 not be correlated with historical Chapter 13 usage. The BAPCPA restrictions on use of the auto

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[5] 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.

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loan cramdowns in Chapter 13 bankruptcies took effect in October 2005, which is when the 2004 Panel was 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.

IV. 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 Black, 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 different 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, indicating

households in treated states (e.g., states where Chapter 13 is historically common) are more likely to gain value in auto assets after BAPCPA. 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 Black 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 Black 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) *Black Head of Household*
Pre-trend *((Panel Year = 2001) x Chapter 13 Reform)*	-0.016 (0.099)	-0.024 (0.060)	-0.031 (0.129)
Pre-trend *((Panel Year = 2001) x Chapter 13 Reform x Subgroup)*	- -	0.059 (0.165)	0.022 (0.216)
Post-reform *((Panel Year = 2008) x Chapter 13 Reform)*	0.234** (0.110)	0.165* (0.094)	0.169* (0.099)
Post-reform *((Panel Year = 2008) x Chapter 13 Reform x Subgroup)*	- -	0.161 (0.127)	0.422* (0.249)
Observations	577,535	577,535	577,535
State FE	YES	YES	YES
Panel 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. 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 probability of having a new car 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, column 3 of Table 4) show positive treatment effects in the post-period for Black respondents. This coefficient is statistically significant at the 5 percent level and suggest that the likelihood of getting a new car increased as a result of the bankruptcy reform. This is consistent with the hypothesis that racial minorities households were granted more access to credit (causing a surge in purchasing power) 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 Probability of New Car Purchase

Subgroup	(1) *None*	(2) *Low-asset Household*	(3) *Black Head of Household*
Pre-trend *((Panel Year = 2001) x Chapter 13 Reform)*	0.007 (0.006)	0.007 (0.008)	0.003 (0.008)
Pre-trend *((Panel Year = 2001) x Chapter 13 Reform x Subgroup)*	-- --	0.001 (0.010)	0.020 (0.014)
Post-reform *((Panel Year = 2008) x Chapter 13 Reform)*	0.011 (0.007)	0.012 (0.009)	0.002 (0.008)
Post-reform *((Panel Year = 2008) x Chapter 13 Reform x Subgroup)*	-- --	-0.014 (0.014)	0.033** (0.013)
Observations	577,535	577,535	577,535
State FE	YES	YES	YES
Panel 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. 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 anti-cramdown legislative impact on the auto's loan-to-value (LTV) ratio at the time the survey question was asked. 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.[6]

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) *Black Head of Household*
Pre-trend *((Panel Year = 2001) x Chapter 13 Reform)*	-0.004 (0.019)	-0.014 (0.022)	0.002 (0.023)
Pre-trend *((Panel Year = 2001) x Chapter 13 Reform x Subgroup)*	-- --	-0.034 (0.030)	-0.048 (0.035)
Post-reform *((Panel Year = 2008) x Chapter 13 Reform)*	-0.047** (0.020)	-0.043** (0.019)	-0.051*** (0.015)
Post-reform *((Panel Year = 2008) x Chapter 13 Reform x Subgroup)*	-- --	0.057* (0.029)	0.102 (0.101)
Observations	88,263	88,263	88,263

---

[6] 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.

State FE	YES	YES	YES
Panel 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. 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 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.

V. Conclusion

This analysis investigates the impact of the 2005 BAPCPA, which eliminated auto loan cramdowns in Chapter 13 bankruptcies, 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 auto assets. In this way, new auto assets and auto 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

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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 probability of having a new car in the household for Black respondents relative to other demographic groups. We also find a modest, positive effect suggesting Black 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 increase the level of credit low-asset (e.g., high-risk) households and Blacks faced in terms of borrowing.

To conclude, our results argue that this policy reform was a driver increasing lending levels and car ownership across households, shocking accessibility to credit. Our findings should be important an important tool for policy makers looking to increase electric vehicle adoptions, particularly among black and low-asset households, in the U.S. through lowering credit 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.

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