The Capitalization of Consumer Financing into Durable Goods Prices*

August 2018

Abstract

We investigate the transmission of cross-sectional credit-supply shocks to durable goods prices. Using loan-level data on millions of used-car transactions, we examine individual price effects when only some borrowers are exposed to an exogenous shock to loan maturity. We document that maturity is capitalized into the price treated borrowers pay for a car of the same year-make-model-trim sold in the same month, affecting incidence and attenuating the benefit of cheaper financing. Our estimates suggest that one additional year of maturity is worth 2.8% of the car's purchase price, implying a price elasticity with respect to monthly payments of –0.23.

Keywords: credit supply, durable goods, loan maturity, incidence of credit shocks
JEL Codes: E31, E43, E51, G21, H22, L11, L62

*We thank our discussants Tom Chang and Paul Goldsmith-Pinkham, and workshop and conference participants at BYU, Federal Reserve Board, Minnesota, MIT, Stanford SITE, the University of Washington Summer Finance Conference, and Effi Benmelech, Shai Bernstein, Natalie Cox, Giovanni Favara, Vincent Golde, Brad Larsen, Greg Leiserson, Brigitte Madrian, John Mondragon, Jonathan Parker, Antoinette Schoar, David Sraer, Jeremy Stein, Stijn Van Nieuwerburgh, and Emil Verner for helpful comments. We appreciate the research assistance of Lei Ma and Alex Tuft. The data were provided by an anonymous information-technology firm.

1 Introduction

We investigate the incidence of credit-supply shocks by studying how financing terms affect the prices of durable goods in the cross-section of borrowers. While an increase in the supply of credit may increase demand for durable goods and thereby their prices, any anticipated increase in collateral prices may also drive an expansion in credit. Despite this identification challenge, recent work has made important progress towards establishing the existence of a causal link running from aggregate credit shocks to average prices.[1] In this paper, we exploit disaggregated data to document the transmission of credit conditions to prices in the cross-section of borrowers, documenting significant heterogeneity across borrowers in the intensity of credit-supply shocks and their effect on individual-level prices.

When credit-supply shocks treat some borrowers but not others, are the effects in the final product market shared across all borrowers or concentrated among affected borrowers? A credit-supply shock, even when affecting only a subset of borrowers, may influence aggregate demand and, therefore, the market-clearing price in the product market. Alternatively, when segmented borrowers face individualized pricing (as is common in many durables markets), credit-supply shocks have the potential to drive cross-sectional dispersion in prices. Understanding how credit-supply shocks impact prices is a necessary step toward welfare analysis, as credit-supply incidence determines the identity of the winners and losers in response to, e.g., monetary policy. Whereas existing estimates evaluate average price effects, one of our key contributions is to evaluate whether credit shocks have heterogenous price effects across affected and unaffected borrowers.

Methodologically, we isolate plausibly exogenous changes in the monthly cost of debt service arising from lender maturity policies in the auto-loan market.[2] While maturity decreases smoothly with car age in aggregate, the policies in place at any given lender often feature discontinuous drops in offered maturity at particular car ages. For example, a given lender may offer a 72-month loan to a borrower to purchase a car up to three years old but may only be willing to offer a 60-month loan to the same borrower to purchase a four-year-old car. Importantly, we find step-function maturity schedules with breaks at different car ages for different lenders in the data. We combine this fact

[1] We discuss the literature that establishes this link, and other related literature, in Section 2.2.

[2] As we discuss below, a large body of evidence documents the importance borrowers attach to payment size, for which maturity has first-order importance.

with the observation that manufacture year is typically considered a sufficient statistic for car age, such that cars are effectively treated as if they fully age by one year on January 1. Thus, on January 1 cars of a given manufacture year may age across a discontinuity in a particular lender's maturity schedule while experiencing constant maturity at another lender. For empirical design purposes, maturity policies that feature discontinuities in allowable maturity around the new year map neatly into "pre" and "post" event dates. Similarly, variation across lenders' maturity policies—even for cars of the same year, make, model, and trim—defines treatment and control samples. Taken together, the pre- and post-event dates and treatment and control samples allow for causal inference in a difference-in-differences framework. We emphasize that the variation we exploit in offered maturity arises from interacting the passage of time with predetermined lender maturity policies, as opposed to any potentially endogenous decision taken by a lender to change its existing maturity schedule.

Given that credit-supply shocks could induce borrowers to substitute towards goods of a different quality, controlling for product quality is important when comparing the prices paid by treated and untreated borrowers. To account for the role of substitution in explaining our results, we analyze car prices holding quality fixed with a battery of manufacture year by make (e.g., Honda) by model (e.g., Accord) by trim (e.g., LX) (YMMT) by month fixed effects. To the extent that our fixed effects hold collateral quality fixed, our results suggest that treated and untreated borrowers pay materially different prices for observationally identical cars purchased at the same point in time, controlling for any time-invariant differences across geographic region. Employing our maturity variation in an instrumental variables framework, we find that one month of exogenously lower maturity is associated with treated borrowers paying 0.3% lower prices. As offered maturity most frequently changes by 12 months, our estimates imply that the modal reduction in offered maturity reduces car prices in our sample by 3.6%, or roughly $720 on a $20,000 car.[3]

While we also estimate the effect of interest rate variation on prices, our focus on maturity as a driver of prices differs from much of the previous literature, despite borrowers being more sensitive to the latter in practice (see, for example, Argyle et al., 2017a). Of course, lenders may change interest rates at the same time they change maturity policies, and we find this is responsible for some of our estimated impact of credit on prices. Using a two-stage least-squares procedure in

[3]To assess the magnitude of this estimate, note that estimates of gross margins on used cars are 5–20% (Gavazza et al., 2014; Huang et al., 2015; Larsen, 2018).

which we instrument separately for both maturity and interest rates, we find that maturity accounts for roughly 80% of the price impact of our credit-supply shocks. Accounting for contemporaneous shifts in interest rates, a borrower offered 12 months shorter maturity pays about 2.8% less when purchasing a car of a given YMMT in a given month. These estimates imply a price elasticity with respect to monthly payments of –0.23.

We conduct several robustness exercises to address the most plausible alternative explanations for the pricing results, including the possibility that YMMT by month fixed effects do not adequately capture vehicle quality. A finance-induced shock to demand could lead treated borrowers to shift toward *unobservably* lower quality, less expensive cars. For example, a borrower could shift demand to a car with the same YMMT but that is in worse condition or has higher mileage and thus a lower price. We address this possibility several ways in section 5.2. For example, we examine a subset of our data comprised of repeat sales—transactions involving the exact same vehicle. If treated-borrower purchases occur at lower values because of differences in unobserved quality, these differences should persist in the second transaction. In a sub-sample of roughly 8,700 vehicles for which we observe a subsequent transaction, we find no difference in the *second* transaction price of cars originally treated with low maturity in the *first* transaction (relative to other cars of the same YMMT sold in the same month).[4] Instead, treated cars' prices appear to rebound when sold at a later time, inconsistent with an interpretation in which treated borrowers bought lower quality cars. We further address concerns about unobserved heterogeneity in car quality through an analysis of pricing effects within samples of older vs. newer cars. Unobserved variation in car quality likely increases with car age, yet our estimates are similar in a sample of cars aged 1–5 years as compared to cars aged over five years. We also show that borrower characteristics do not change in economically meaningful ways with our treatment and that our results are robust to the Oster (2017) adjustment for potential unobserved correlated heterogeneity.

Our results provide insight into the transmission of credit-supply shocks to the prices of durable goods. The object of interest in most studies of credit and asset prices is an aggregate price, e.g., as characterized by a price index. In these settings, it is not clear whether observed price effects are driven by a shift of all borrowers to a new market-clearing price (as may be the case if affected buyers simply substituted to a different good) or if prices change differentially for the affected group

[4]We also test for endogenous selection into observable resale and find none.

of buyers. Our estimates speak to this question directly—we find that prices change differentially for treated buyers. A priori, though, it is not immediately obvious why one consumer with different financing terms would pay a different price than another consumer purchasing the same car at the same point in time. Our inclusion of commuting zone fixed effects rules out the effect being driven by variation in average prices of cars across geographic markets, while lender fixed effects cast doubt on clientele selection effects across lenders. Instead, we provide suggestive evidence consistent with individual demand shocks (caused by individual credit-supply shocks) driving differences in prices by influencing the search or bargaining intensity of affected borrowers.

The auto market, like many markets for big-ticket items in which consumers transact infrequently (real estate, machines, furniture, higher education, labor, etc.), is not characterized by a single market-clearing price. Instead, buyers search for suitable cars and negotiate over price with potential sellers. The ultimate transaction price divides the surplus created by the wedge between a buyer's and seller's private valuations. Constrained borrowers given shorter maturity loans will have higher monthly payments, pushing them closer to binding debt-service budget constraints and lowering their private valuation for a given vehicle. This may result in a lower equilibrium price in a particular negotiation, or the borrower may be forced to search longer for a better price on an alternative vehicle.[5] In an effort to provide some evidence consistent with this mechanism, we examine the fraction of offered loans subsequently accepted by the borrower (the loan take-up rate) using a subset of lenders for which we have loan application data. Difference-in-differences estimates indicate that take-up rates at the lender level drop at institutions after a reduction in allowable loan maturities. Though evidence of more frequently rejected loan offers does not uniquely support a search or bargaining intensity mechanism, it is consistent with low-maturity borrowers being less likely to come to mutually agreeable terms with sellers.

What do our results suggest regarding winners and losers in credit expansions? In a world in which the market-clearing price changes for everyone, affected borrowers would benefit at the expense of any unaffected borrowers forced to pay higher prices without increased access to credit. Meanwhile, the benefits of higher prices would spread evenly across all sellers. Relative to this counterfactual, our results indicate that treated and untreated borrowers share the benefits of a credit expansion more equally, as price increases are concentrated among those benefitting from

[5] In section 2.2, we also discuss related literature on the strategic use of debt in bargaining games.

increased access to credit. At the same time, the benefits of higher prices on the sell side are concentrated among sellers who sell to affected borrowers. Thus, increased access to credit at the intensive margin may be less helpful for potential buyers of durable goods than previously thought, instead doing more to separate sellers into winners and losers. We characterize the net impact on the average treated borrower by estimating the internal rate of return of the maturity shock—the annual discount rate at which a borrower would be indifferent between a higher price with longer maturity and a lower price with shorter maturity. Using our estimated maturity, interest rate, and price effects, we find a break-even discount rate near 9 percent. To the extent that the average borrower has a discount rate above this range, our estimates indicate that affected buyers share the cost of a negative credit-supply shock with the seller, as we would expect given bargaining.We detail this calibration exercise in greater detail in the conclusion.

2 Literature and Conceptual Motivation

2.1 The Transmission of Financing Terms to Durables Prices

The central economic question we explore in this paper is how variation in the terms of credit affects individual prices paid in the cross-section of borrowers. In this section, we discuss the economics that link changes in buyer financing to prices paid in durable goods markets in the presence of disaggregated credit shocks.

In a world with credit-constrained borrowers, we would expect a decrease in the supply of credit to result in a negative shock to demand for the final good. In a textbook model of supply and demand, this negative shock to demand would lower both the good's unit price and quantity purchased. This conceptually corresponds to the standard interpretation of papers like Favara and Imbs (2015) and Di Maggio and Kermani (2017), who quantify the effect of aggregate credit-supply shocks on average house prices using price indices. In the credit-supply-shock settings they study, the nature of transmission is difficult to ascertain. In contrast, we study *how* the aggregate price level declines in response to the credit shock. Does the credit-supply shock simply affect aggregate demand and thereby move prices for everyone, or is the decrease in the aggregate price level, e.g., as measured by a price index, driven disproportionately by lower prices paid among affected borrowers? Answering this question is necessary to understand how the costs and benefits of credit supply are spread across market participants.

Absent frictions in the durable goods market, buyers facing differential costs of credit should still pay the same price for the same good. In a classical supply and demand framework in which demand curves are continuous in transportation services, affected borrowers would substitute toward lower quality cars (ones with lower levels of transportation services). Decreasing aggregate demand would drive down prices for everyone, but two people buying the same car would pay the same price whether they had selected that car because of a payment shock or not.

Alternatively, consumers may not optimize over a continuum of potential transportation services. If demand for transportation services is sticky with respect to cars of a given type, disaggregated shocks could lead to heterogeneous price effects. For example, consumers may simplify their search by choosing a vehicle type first, they may choose a dealer first, or they may have strong brand preferences. In such a choice environment, a treated buyer's demanded quantity of car services could be fixed such that the credit-supply shock primarily impacts her private valuation for the desired vehicle. With lower private valuation, the treated buyer may search or bargain more intensively, resulting in lower prices on realized transactions. Thus, in a market characterized by search and bargaining, like the auto market (or a variety of other durable goods markets), it might be natural to expect equilibrium transaction prices to be influenced by individual borrowers' credit terms. In this vein, debt's influence on a variety of bargaining outcomes has precedent in the corporate finance literature—see discussion in section 2.2 below.

2.2 Contribution to the Literature

The empirical literature studying the causal link between credit and prices has been concentrated in the housing market. See, for example, Mian and Sufi (2009), Glaeser, Gottlieb, and Gyourko (2012), Adelino, Schoar, and Severino (2012), Favara and Imbs (2015), Landvoigt, Piazzesi, and Schneider (2015), Zevelev (2016), Di Maggio and Kermani (2017), Verner and Gyöngyösi (2017), and Davis et al. (2017), none of which has studied loan maturity.[6] Favara and Imbs (2015) exploit state-level exposure to bank branching deregulation as an instrument for credit-supply shocks to

[6]A recent empirical macro literature also studies the causes and consequences of credit shocks for house prices (Jorda, Schularick, and Taylor, 2015), price-discrimination markups (Cornia, Gerardi, and Shapiro, 2011), business cycles (Borio and Lowe, 2002; Mian, Sufi, and Verner, 2017; Krishnamurthy and Muir, 2017), and stock markets (Hansman et al., 2018). See Mian and Sufi (2018) for a survey of recent work on credit-driven business cycles.

demonstrate a causal link between credit expansion and house prices. Di Maggio and Kermani (2017) use state-level variation in anti-predatory lending laws' impact to trace a boom and bust in house prices resulting from credit-supply shocks. These papers feature geographic variation in credit supply shocks that may affect local credit markets in complex ways as opposed to quantifiable, individual-level payment shocks. In addition, they do not examine the cross-sectional implications of credit capitalization on individual borrowers. Closer in spirit to our work is Adelino, Schoar, and Severino's (2012) analysis of conforming loan limits (CLL) and housing prices. While an increase in the CLL impacts house prices in the cross-section (with prices near the CLL more affected), the differentiated nature of real estate does not permit disentangling whether two borrowers with different access to financing terms would pay different prices for the same house.

We differ from previous work along a second dimension. The set of frictions that are the source of credit-supply shocks in the literature are often macro in nature. Aside from examples cited above, these include credit shocks driven by regulation (Rice and Strahan, 2010), financial innovation (Mian and Sufi, 2009; Nadauld and Sherlund, 2013), government credit subsidies (Lucca, Nadauld, and Shen, 2016), and funding market failures (Benmelech, Meisenzahl, and Ramcharan, 2017). In each of these papers, macroeconomic fluctuations influence the aggregate supply of credit. In contrast, our setting demonstrates the existence of a different class of relevant credit-market frictions. Our results underscore that firm-level institutional idiosyncrasies play an important role in determining the borrower-level supply of credit and that such policies have material effects on consumer outcomes. Moreover, we are the first paper to study the effect of loan maturity on prices.

Our results are related to the corporate finance literature highlighting the strategic role that debt plays in determining bargaining outcomes.[7] Israel (1991) and Muller and Panunzi (2004) argue that debt can be used to influence bargaining outcomes in the market for corporate control. Spiegel and Spulber (1994) show that debt burdens influence the prices charged by regulated firms such as utilities. Hennessy and Livdan (2009) demonstrate the strategic role of debt in the allocation of surplus between firms and their suppliers, while Matsa (2010) documents the influence of debt on the outcomes of negotiations between firms and organized labor. In each case, debt limits the financial flexibility of firms, which strengthens a firm's bargaining position. We show a similar dynamic in a retail setting. Borrowers who are offered shorter maximum maturity have limited

[7]See also related work in psychology, e.g. Lee and Ames (2017).

financial flexibility and appear to be able to use this to influence the outcome of the bargaining game with car sellers.

Within the vast public-finance literature on economic incidence, several papers have looked specifically at the market for new cars and the incidence of taxes and manufacturer subsidies. Although these papers do not examine the incidence of financing shocks per se or the distributional implications of individual-level changes in access to credit, they document capitalization effects of cost shocks into vehicle prices. For example, Busse, Silva-Risso, and Zettelmeyer (2006) examine the effects of manufacturer cash rebates for new cars, documenting that incidence depends on statutory incidence, i.e. whether the rebate is issued to buyers or sellers. Consistent with our findings that prices capitalize changes in credit terms, they find that prices rise by 10-30% of the amount of a customer rebate. Sallee (2011) finds that new Toyota Prius prices did not capitalize hybrid vehicle tax incentives at all, attributing the lack of pass-through to Toyota's concerns about future demand given the dynamics of buyer price beliefs. Busse, Knittel, and Zettelmeyer (2012) find that resale prices capitalize exposure to gasoline taxes.[8] We complement this literature by studying the transmission of credit-supply shocks with cross-sectional identification, further emphasizing that disaggregate credit shocks can have disaggregate price effects.

Finally, we note the contribution of this paper relative to Argyle et al. (2017a), which uses similar data but a different empirical strategy to document that consumers make debt decisions with monthly payment amounts as their primary consideration. In this paper, we explore the goods-pricing implications of monthly payment shocks, documenting that monthly payment shocks are capitalized into asset purchase prices in a way that, depending on borrower discount rates, offsets much of the value of easier credit. The role of maturity has been understudied relative to interest rates in this literature, a sentiment shared by Hertzberg et al. (2017).

3 Data

Our data on auto loan originations come from a technology firm that provides data warehousing and analytics services to retail-oriented lending institutions nationwide. We begin with a dataset consisting of over four million auto loans originated by 372 unique lenders covering all 50 states.

[8]Other relevant incidence papers include Goolsbee (1998), who shows that investment tax credits increase capital goods prices, especially for low-inventory goods.

The data include only loans originated directly through the lending institution, as opposed to so-called indirect loan applications processed through auto dealerships. Direct loan applications occur primarily in one of two ways. First, borrowers may identify the exact car they would like to purchase and then apply for a loan. In this circumstance, lenders evaluate the collateral and offer loan terms specific to the collateral. A second scenario occurs when borrowers apply for an auto loan more generally, without having identified a specific car they would like to buy. In this circumstance, lenders evaluate a potential borrower based on their credit characteristics. An approved application then specifies a maturity range and principal amount, conditional on a bundle of collateral characteristics, within which borrowers can shop. In either case, loan terms are finalized after borrowers select a specific car.

Our sample includes loans originated between 2005 and 2017, though over 80% were originated between 2011 and 2017. The growth in originations over time is mostly driven by growth in our data provider's client base, though it also partly reflects increased reporting of loan originations over time within lender, as our data provider's products have become more integral to the lenders' businesses. Moreover, aggregate auto loan originations have increased substantially over our sample period, with outstanding auto debt in the U.S. increasing 56% between 2010 and 2017. Similar data are used in Argyle et al. (2017a, 2017b).

The dataset, anonymized of any personally identifiable information, includes loan contract features such as the purchase price, loan amount, maturity, interest rate, and origination date. We also have information on the underlying collateral, including the VIN number in most cases, which allows us to extract the manufacture year, make, model, and trim (YMMT) of the vehicle. Borrower information includes FICO scores and self-reported debt-to-income (DTI) ratios.

We use the full sample of 4,192,502 loans to detect maturity policies in each lender $\times$ car age $\times$ month cell, as described in detail in the following section. After inferring lender maturity policies, we drop loans that were not originated during a stable policy regime, eliminating roughly two thirds of the observations (mostly consisting of those lender $\times$ car age $\times$ month cells with the fewest observations). We lose an additional quarter of the remaining observations that are missing vehicle trim information that we use in YMMT fixed effects to hold observable vehicle quality fixed.

These two restrictions leave us with a final dataset of 972,621 loans originated by 112 unique lenders. Table 1 reports summary statistics broken out by treatment and control groups. The average borrower in our sample has a credit score at loan origination of 714, slightly above the national average of 700—the population of borrowers served by credit unions in our data is not particularly weighted towards subprime borrowers. Average back-end DTI ratios, which measure the monthly fraction of total debt-service payments to income, are around 35%. Examining collateral and loan characteristics, most of the car purchases we study are used cars; the average car in our sample is 3.9 years old and sold for $20,341. The average loan-to-value was 90.7%, with average maturity of 61.3 months and interest rate of 4.1%. We discuss the comparison of treatment and control groups after defining them in section 4.1.

4 Empirical Strategy

We are interested in whether receiving longer maturity loans causes buyers to pay higher prices for the same durable goods relative to buyers who received shorter maturity loans at the same time. In answering this question, we face immediate identification challenges, as the relationship between credit and prices may be driven by a variety of economic mechanisms, including simple reverse causality. For example, lenders—willing to offer longer maturities for higher quality collateral—may use price as a proxy for unobservable collateral quality. In this case, buyers who pay higher prices, perhaps because they have higher private value for the good, would also receive longer maturities. Alternatively, any aspects of quality that are observable to the lender but not to the econometrician may jointly drive both higher prices and longer maturities.

To overcome these empirical challenges, the ideal experiment would feature randomly assigned loan maturities. We do our best to approximate this by exploiting maturity rules used by lenders based on the age of cars. Based on conversations with lenders, the maximum maturity borrowers are offered on an auto loan is frequently determined as a function of car age, a practice that is motivated by lenders' risk management concerns. Longer maturity loans increase the likelihood that the loan balance exceeds the collateral value during the life of a loan, exposing lenders to losses in the case of default. For older cars with shorter remaining expected life, lenders offer shorter maturity. However, instead of smoothly mapping car age into offered maturity, many lender policies feature discrete drops in maximum offered maturity at particular car ages, as illustrated in Figure 1. This leads to a discontinuous drop in maturity offered for a given car as it ages across a break in a given institution's maturity schedule. To the extent that all cars of a given manufacture year are considered the same age, these discontinuities should occur as the calendar moves from December to January, when all cars turn one year older. There do not appear to be industry standard rules mapping car age to maturity; indeed, we find that discrete January maturity drops show up for cars of different ages at different lenders. At any point in time, we observe treated buyers (those borrowing from an institution with a discrete drop in maturities in January for a car of the age being purchased) and untreated buyers (those borrowing from an institution without such a discrete drop for the car age being purchased) even for cars of the same YMMT. We use this feature of the data to construct a difference-in-differences quasi-experiment, comparing the change in prices paid before and after January 1 for borrowers treated with an exogenous maturity shock to the corresponding price change for untreated borrowers. To be clear, our variation does *not* arise from some lenders changing their policies in January; rather, predetermined maturity schedules interact with the passage of time to treat individual cars with persistent maturity shocks beginning in January.

In addition to randomly assigned maturities, the ideal experiment would also hold fixed the quality of the goods purchased by treated and untreated borrowers. Absent this, any result suggesting that borrowers given exogenously lower maturity pay lower prices might be driven by a shift of treated borrowers toward lower quality goods. We do our best to hold car quality fixed by controlling for YMMT fixed effects interacted with the month of sale. Thus, the spirit of our tests is to compare the prices of two cars of the same YMMT being purchased in the same month, where one buyer receives exogenously different maturity than the other. While these fixed effects soak up a large majority of the variation in car quality, as we discuss the interpretation of our tests, we will be careful to address the possibility that our results are affected by any residual variation in quality within YMMT in a given month.

4.1 Identifying Loan Maturity Policies by Car Age

The first step in our analysis is to empirically identify maturity policies for the 372 lenders in our sample to assign vehicles experiencing a discrete change in maturity on January 1 to a treatment group and compare their prices to a control group of vehicles experiencing no change. In considering the tradeoff between false positives and false negatives in our detection of lender rules, note the asymmetry of the loss associated with each. Suppose that, in an effort to detect more true positives, we choose an algorithm that admits more false positives—that is, instances in which maturity dropped for reasons unrelated to age-based maturity policies. Such false positives would be more accurately characterized as occurrences of *abnormally* low maturity, rather than the *exogenously* low maturity we seek. To the extent our classification is contaminated with too many occurrences of merely abnormally low maturity, the interpretation of our tests would be plagued by the reverse causality and omitted variable problems discussed above. In the limit, if there were no actual maturity policies in the data and our sample consisted entirely of false positives, a negative coefficient of treatment on prices would tell us only that cars with abnormally low maturity have lower prices, similar to what we would learn from a naïve regression of prices on maturities.

On the other hand, an increase in the number of false negatives assigns more occurrences of actual exogenous drops in maturity to the control group. In this case, the treatment group would consist of borrowers who experienced exogenously low maturity, while the control group would consist of some borrowers who *might* have experienced exogenously low maturity. The more we assign actual maturity discontinuities to the control group, the more the gap between treatment and control groups narrows, making it more difficult for us to detect any affect of maturity on prices. In other words, while false positives bias our tests towards the naïve regression, false negatives bias us towards finding no results. Consequently, in designing our algorithm to detect maturity policies, we take care to avoid false positives even at the cost of potentially missing actual discontinuities.

Since each lender is likely to have its own maturity policies applying to cars of various ages, we look for rules at the lender $\times$ car-age level, where car age is defined as the calendar year of loan origination minus the year of manufacture. For lenders with maximum maturity policies, we would expect to see the top end of the maturity distribution of originated loans to be very stable over relatively long periods of time. While, in principle, we are interested in identifying the maximum available maturity, there is often a very small number of borrowers who receive the absolute maximum observed maturity in any given month.[9] These may be manifestations of very sharp lender policies that apply to only a small subset of borrowers, or they may represent

[9]We round each maturity to the nearest three months such that, for example, maturities of 60 months (the most prevalent maturity at 27% of the data) are grouped with maturities of 61 months (2% of the data) and 59 months (0.5%). While a significant majority (83%) of the loans in our data already have maturities that are multiples of three months, some borrowers receive abnormal terms, perhaps motivated by demand-side factors such as a desired monthly payment level (Argyle et al., 2017a).

exceptions made to more broadly applicable policies.[10] To find maximum maturity rules that affect a meaningful share of borrowers, we test for lender policies at the 80th percentile of maturity for each lender × car age × month.[11]

To illustrate our method of categorizing lender policies, consider Figure 2 which plots the 80th percentile of maturity for three-year-old cars (upper panel) and four-year-old cars (lower panel) in each month for the largest lender in our sample. The x's represent the individual monthly observations, and we categorize long periods of identical (or nearly identical) maturities as lender policies, as shown in the boxed areas. For each month, we examine the six months before and after; if at least five of the six months both before and after have the same maturity as the month in question, we consider the entire 13-month period to be part of a lender maximum maturity policy.[12] For three-year-old cars shown in the figure, we identify two separate lender policies: a 66-month policy lasting from December 2007 through December 2012, followed by a 72-month policy lasting from January 2013 through July 2017 (the end of our sample). For four-year-old cars, we identify four separate lender policies over time, each shown in dashed red boxes.[13]

Again, we stress that our identification strategy does not use potentially endogenous variation in lender policies over time. Instead, we estimate maturity policies at a given point in time and examine what happens to the supply of maturity on January 1, as each car ages by one year. Figure 3 combines the policies for three-year-old and four-year-old cars from Figure 2 into one plot. The policies for three-year-old cars are shown as dashed bars, while the policies for four-year-old cars are shown as solid bars. Note that a three-year-old car in any given December becomes a four-year-old car the following January, and therefore becomes subject to (potentially) different offered maturity. The vertical dotted lines correspond to the set of year ends for which cars turning four years old would have experienced a discrete drop in maturities at this particular lender. Consider the example of a 2006 Honda Civic LX illustrated in the figure. In December 2009, as a three-year-old-car, this car would be subject to a 66-month maximum maturity policy. Yet the same car sold in January

[10]One lender's policy to loan officers stated: "Recommended guideline: Auto Loans – 84 months (exception if approved by level 3 with justification)," indicating that longer terms may be available on a case-by-case basis.

[11]Our results do not depend on our choice of the 80th percentile, as we discuss in more detail below.

[12]We require that the endpoints of the 13-month window do not deviate from the prevailing maturity policy. This prevents us from including the first month of a new policy in the time window of an old policy.

[13]The 80th percentile of loan maturities moves around more in the early part of our sample because there are fewer loans during that time period. The coverage of our data provider improves over the early part of our sample, even within lender.

2010 would be subject to the 60-month maturity policy in effect for four-year-old cars. We group cars experiencing this kind of discrete maturity shock in January into the set of treated observations. In contrast, consider the 2012 Honda Civic LX example shown in the upper right of the figure. By late 2015, this lender's policy allows 72-month loans for both three- and four-year-old cars. Thus, a four-year-old car sold in January 2016 would be subject to the same maturity policy as the three-year-old car sold in December 2015. We group all occurrences in which a given car experiences the same maturity policy from December to January into the control group.

Conservatively, we require maturity policies to be fairly long-lasting (at least 13 months), which results in a significant number of lender $\times$ car age $\times$ months that do not fall into any maturity policy. We provide evidence below that our approach seems to avoid false positives.[14] Throughout our data, maturity policies that are stable across adjacent car ages (control observations) significantly outnumber maturity policies that drop (treated observations), such that treatment observations would constitute only 3.3% of our sample. To increase the variation in "treated," we repeat the above analysis at both the 70th and 90th percentiles of the maturity distribution. Thus, the treated subset of our final sample consists of any cars that experienced a discrete drop in maturity policy at any of the 70th, 80th, or 90th percentiles upon turning one year older, while control observations are those that experienced a continuous maturity policy at any of these quantiles.[15] There are, of course, overlaps in those observations considered treated at each quantile, but inclusion of all three quantiles results in a final sample with 5.6% treated observations.[16]

To assess how effective our approach is at capturing actual exogenous variation in the supply of maturity, we examine the characteristics of the set of shocks we identify. In particular, we apply our algorithm to look for discrete changes in available maturity that would apply to a given car being financed by a given lender in any two consecutive months (not just December-January). Our intuition is that false positives (periods of high maturity followed by periods of low maturity that are

[14]The cost of this conservatism is, of course, that we are likely to miss some actual maturity shocks. For example, it seems likely that a car turning four years old in January 2014 in Figure 3 would have experienced a discrete drop in maturity; however, because the lender's long 60-month policy was briefly interrupted, January 2014 does not belong to any maturity policy.

[15]Of course, it is possible for a lender to have a discrete drop in maturity policy at one quantile (say, the 70th percentile) but to have a continuous policy at another quantile (say, the 90th percentile). We consider any such cases as treated since they display a maturity shock.

[16]Inclusion of all three quantiles does not materially change the magnitudes of our coefficient estimates relative to focusing on any individual quantile, though the increased number of treated observations does result in predictably smaller standard errors.

unrelated to changes in maturity policies) should be distributed roughly uniformly across months of the year. Similarly, if we are mistakenly identifying a discrete drop in a maturity policy that is actually based on some underlying car characteristic that moves smoothly over time (say, mileage), there is no reason to expect those mistakes to show up disproportionately in January.

Figure 4 shows the timing of maturity discontinuities, conditional on the sign of the discontinuities. We detect 71 lender $\times$ car age $\times$ month combinations for which there is a discrete increase in maturities from one month to the next, as shown in the top panel of the figure. This is not surprising, given that the bulk of the loans in our data were originated during a period of lengthening maturities. These 71 occurrences are distributed roughly evenly across months, with no single month accounting for more than 13 of the 71 total shocks. In the bottom panel we plot the 118 instances of discrete drops in maturity from one month to the next. Of these, 106 (90%) occur in January, with no other month having more than three. While we cannot know how many of the 12 non-January negative maturity shocks we identify represent actual policy changes vs. false positives, in the worst case Figure 4 suggests that no more than two or three of the 106 January maturity discontinuities that define our treatment are false positives. Moreover, if lenders were inclined to proactively update maturity policies at the new year, we would expect both positive and negative treatments to occur disproportionately in January.

Because maturity policies are very persistent, we include all months July through December in the pre-period and all months January through June in the post period, although monthly event studies will allow us to focus on the months around the end of the year. This leaves us with a total sample of 972,621 cars, of which 54,757 (5.6%) are treated observations. Table 1 tabulates summary statistics for the treatment and control samples. The groups are very similar on observables, including FICO scores at origination, debt-to-income ratios (DTI), and loan-to-value (LTV) ratios. Although the differences in means are mostly statistically significant owing to the precision afforded by our large sample size, the typical difference is a tenth of a standard deviation, suggesting that treatment- and control-group borrowers are balanced for practical purposes. Consistent with being slightly older (4.29 vs. 3.86 years), treated cars have slightly lower prices ($20,432 vs. $18,821), shorter maturities (61.4 months vs. 59.3 months), and higher interest rates (4.09% vs. 4.31%). While much of this price difference will be absorbed by our rich controls for vehicle heterogeneity, our empirical results below show that some of this price differential is a causal effect of treatment-group borrowers being offered shorter maximum maturities in the post period.

4.2 First-Stage Results

We now turn to estimating the reduced-form impact of our detected maturity shocks on the average borrower's maturity. Recall that a maturity "shock" in our data does not arise from lenders changing policies but from borrowers buying a car that has recently crossed a discontinuity in a lender's maturity policy. In Table 2, we estimate

$$Maturity_{iglt} = \beta_1 Post_t + \beta_2 Treatment_i + \beta_3 Post_t \times Treatment_i + X_{it}'\gamma + \varphi_g + \psi_l + \varepsilon_{iglt} \quad (1)$$

where $Maturity_{iglt}$ is the loan maturity of transaction i, in geography g, financed by lender l in month t. Event time runs from July through the following June with *Post* equal to zero for transactions occurring July through December and one for January through June. Treatment is a dummy equaling one for observations within a *Lender* $\times$ *Rollage* $\times$ *Rollyear* group with an identified shock to offered maturity occurring in January and zero for any observations in groups for which maximum allowable maturity is not changing. We use *Rollage* to refer to the age that cars turn during January of the event year and define *Rollyear* as the calendar year of that January. Controls X_{it} consist of borrower characteristics (DTI and FICO score) and various fixed effects that control for the quality of collateral, such as YMMT by month fixed effects. In some specifications, we also control for commuting zone fixed effects φ_g and lender fixed effects ψ_l. We double cluster our standard errors by month and commuting zone.

Table 2 reports the results. Column 1, without any fixed effects, shows a first-stage effect on average maturity of –2.4 months, meaning that the maturity for treatment-group borrowers decreased by an average of 2.4 months after their cars aged across a maturity discontinuity on January 1 relative to any change in maturity for control-group borrowers. As shown in Table 1, cars in the treated group are slightly older and have slightly shorter maturities than cars in the control group. In column 2 we add car-age fixed effects, which predictably narrow the gap between treatment- and control-group maturities but leave the $Treatment \times Post$ coefficient of interest unchanged, suggesting that our difference-in-differences specification accounts for heterogeneity across car age. In column 3 we add finer collateral fixed effects, controlling for the car age interacted with make (e.g.

Honda), model (e.g. Accord), and trim (e.g. LX). Column 4 adds a time dimension, interacting YMMT fixed effects with month fixed effects. In this case, the coefficient measures the difference in maturity offered to buyers of the same YMMT during the same month but with different lender maturity policies. In column 5 we add commuting zone fixed effects to account for potential differences in maturity norms across geography. Anecdotally, prices of cars differ by geography, and column 5 allows for the same to be true of maturities. Finally, column 6 adds lender fixed effects. The estimated magnitude of our detected maturity shock is robust across all specifications, showing a stable effect on originated maturities of slightly more than two months.

As indicated above, auto loan maturities cluster significantly on multiples of three, six, or twelve months. While the most common change in *maximum* allowable maturity for treated borrowers is –12 months (Figure 5), not everyone receives the maximum maturity. Table 2 shows that average originated maturity decreases by around two months, meaning that many borrowers either do not qualify for the maximum maturity or endogenously choose shorter maturity than the maximum allowable. Borrowers that demand loan maturities lower than the maximum allowable could be unaffected by any changes in maturity policy. Our instrumental-variables strategy below is designed precisely to deal with any such sorting behavior. The key takeaway from Table 2 is that the members of the treated group are consistently more likely to be treated with shorter maturities than members of the control group.

To test for whether the difference-in-differences coefficients in Table 2 are affected by pretrends, Figure 6 plots the conditional average maturity for each month from July through June for the treatment and control groups.[17] The figure shows stable maturities for the control group throughout the entire event year. The treatment group, in contrast, has stable maturities that are slightly higher than the control group from July through December, followed by a sharp drop in January, continuing to February. Maturities in February through June are stable and significantly lower than those in the control group. It is difficult to say exactly why the drop in maturities spans January and February, though it is possible that lenders and borrowers agree to terms in a pre-approval process that occurs before the car is actually purchased in some cases. This event-study approach supports our difference-in-differences parallel trends identifying assumption and bolsters our interpretation

[17] Specifically, we control for the expected decrease in maturity as a car ages and any differences across geography by regressing maturity on car age × month fixed effects and commuting zone fixed effects. We then plot the average residuals within each month for treatment and control groups.

of the $Treatment \times Post$ coefficients in Table 2 as causal effects of year-end discontinuous maturity policies.

5 Results

Having identified plausibly exogenous variation in the supply of maturity, we now estimate the effect of maturity on prices in the cross-section of borrowers. We run the same specification as in equation (1), replacing the dependent variable with the log of the car price. For consistency, we include the same borrower controls (DTI and FICO). Similarly, we include the same sets of fixed effects in each column as we did in Table 2.

We report these reduced-form results in Table 3. In column 1, where we don't include any fixed effects, we find a statistically insignificant effect of -2.6%. Of course, one way in which borrowers are likely to respond to a lower maturity is by shifting toward cheaper cars, either older cars or lower end models. Controlling for car age fixed effects (column 2) sharpens our estimation significantly (as evident in smaller standard errors and the increase in R^2 from 0.06 to 0.37) with little effect on the coefficient magnitude. Holding fixed the age of the car, affected borrowers spend 2.7% less on their car purchase, significant at the 1% level. In column 3 we interact the car age fixed effects with make-model-trim fixed effects. The coefficient drops to 0.9%, indicating that a significant portion of the effect on prices from column 2 is being driven by a shift of affected borrowers toward lower-quality vehicles. This highlights the importance of holding the quality of the good fixed when measuring the impact of credit terms on durable goods prices, one of the virtues of our setting and dataset.

In column 4 we interact YMMT fixed effects with month fixed effects such that the coefficient tells us the difference in price paid by an affected borrower for the same YMMT in the same month. These YMMT-month fixed effects absorb any time-varying shocks to YMMT values, e.g., because of the introduction of a new model. Because prices may differ systematically across different geographic regions, we supplement these fixed effects with commuting zone fixed effects in column 5. Finally, we add lender fixed effects in column 6 to rule out our results being driven by any lender-specific clientele-selection effects. Across all of the more stringent specifications, the estimated effect of a shock to maturity on the price paid for the same YMMT in the same month is significant and stable around 0.6–0.7%. Recall that the magnitude of our estimate for the first-stage effect on average maturities was about 2.3 months. The estimates in Table 3, then, indicate that a borrower who is shocked with 12 months shorter maturity would pay about 3.6% less for an observationally identical car. Directly estimating the value of an extra month of offered maturity by two-stage least squares, Appendix Table A1 (with columns corresponding to those in Tables 2 and 3) shows a price effect around 0.3% per month of maturity.[18]

The estimates in Table 3 compare average prices during the post-period (January through June) to average prices throughout the pre-period (July through December). As shown in Figure 6, the trends in maturity moved roughly in parallel across treatment and control groups around year end. To the extent that the difference in prices are being driven by shifts in lender maturity policies, we would expect the time series pattern of prices to match that of maturities. In Figure 7, we plot the average residuals from a regression of log price on car age $\times$ month fixed effects and commuting zone fixed effects, as we did in Figure 6 for maturities. Control vehicles show a flat pattern over the entire event year, while treated vehicles' prices are largely flat but for a large drop in January and February, matching the pattern of maturities shown in Figure 6.

One potential concern with our empirical approach is that our results could be driven by that fact that we use the same sample to determine discontinuities in offered maturity—i.e., the assignment of treatment and control—as we do to estimate potential pricing effects. Though we have tried to provide evidence that we are capturing actual shocks to the supply of maturity, we attempt to further mitigate these concerns by performing the following exercise. Within a given lender $\times$ car age $\times$ month cell, we randomly assign half the loans to a training sample and the other half to a hold-out sample. We use the training sample to identify lender maturity policies in order to define treatment and control observations, following the procedure outlined in Section 4.1. We then use the hold-out sample to estimate our reduced form pricing regressions. In this way, we estimate the effect of maturity on prices out of sample relative to the data we use to detect offered maturity discontinuities. The results, shown in Appendix Table A2, closely mirror those in Table 3 in terms of magnitudes and significance.

[18]See De Chaisemartin and D'Haultfœuille (2017) and Hudson, Hull, and Liebersohn (2017) for a precise discussion of the identification conditions needed for the consistency of difference-in-differences instrumental-variables estimators. In particular, the necessary assumptions around parallel trends, treatment exogeneity, monotonicity, and stability of treatment effects across time and subgroups are each quite plausible in our setting.

5.1 Isolating Maturity Effects from Interest Rate Effects

Our results presented thus far have focused on the maturity dimension of the financing contract, motivated by evidence in Argyle et al. (2017a) that constrained consumers have stronger preferences over maturity than over interest rates. However, given that maturities and interest rates frequently move together in a contract bundle, the empirical strategy discussed in Section 4 is subject to the concern that identified breaks in maturity policies may be coincident with breaks in lenders' interest rate policies. While any interest-rate contagion in our estimates would not invalidate a claim that we have estimated a causal effect of credit on prices generally, it would compromise the interpretation that estimated price effects are driven by changes in maturity. In this section, we turn our attention to disentangling the effects due to changes in maturity from those due to changes in interest rates.

We begin by examining whether the maturity shocks we detect coincide with changes in interest rates by re-estimating the difference-in-differences specifications from Table 2, with interest rates for each loan replacing maturity as the dependent variable. We report the results in Table 4. With no fixed effects, the coefficient of interest on $Treatment \times Post$ is six basis points and statistically insignificant. As we control for increasingly fine collateral fixed effects in columns 2–4, the estimate remains insignificant, ranging in magnitude from four to nine basis points. In column 5 we add commuting zone fixed effects to the YMMT $\times$ month fixed effects of column 4, which increases the coefficient to 12 basis points, marginally significant at the 10% level. Finally, with the addition of lender fixed effects in column 6, the coefficient is 16 basis points, significant at the 1% level. Figure 8 plots the time series pattern of interest rates during the event year for the treatment and control groups. Consistent with the estimates in the table, rates appear to be somewhat higher in the post-period months for treated cars, though the pattern appears much less stark than the corresponding pattern for maturities in Figure 6. This is not surprising given that our empirical design is built to detect maturity breaks rather than interest rate breaks. Still, though the relationship is not strong, the consistent message from Table 4 and Figure 8 is that interest rates appear to increase somewhat for treated cars in the post-period, which could potentially be driving some of the lower prices that we observe for treated borrowers in the post period.[19]

[19]Note that the evidence that any interest-rate movements coincident with our shifts in maturity are positive further supports the claim that we have identified true shifts in the supply of maturity. If the changes were driven by demand for maturity, we would expect to see lower interest rates associated with the lower maturities, as borrowers often have a menu of maturity-interest rate bundles from which to choose, with an upward-sloping term structure.

In an effort to pin down a more precise causal estimate of maturity, accounting for any interest-rate impact, we estimate a two-stage least squares regression, instrumenting for both maturity and interest rates. We estimate separate first stages for maturity and rates as follows:

$$Maturity_{iglt} = \sum_k \pi_k^{mat} \mathbb{I}_{k,ilt} \times Post_t + \sum_k \alpha_k^{mat} \mathbb{I}_{k,ilt} + X'_{iglt}\gamma^{mat} + v_{iglt}^{mat} \tag{2}$$

$$Rate_{iglt} = \sum_k \pi_k^{rate} \mathbb{I}_{k,ilt} \times Post_t + \sum_k \alpha_k^{rate} \mathbb{I}_{k,ilt} + X'_{iglt}\gamma^{rate} + v_{iglt}^{rate}. \tag{3}$$

As before, X_{iglt} contains borrower controls, lender fixed effects, commuting zone fixed effects, and rich collateral fixed effects. The key innovation with respect to the reduced-form formulations is the instrument set, which is a full set of treatment cell indicators interacted with $Post$. In our notation, k indexes the individual *Lender* × *Rollage* × *Rollyear* cells that make up our treatment group, with $k = 0$ corresponding to the control group. The $\mathbb{I}_{k,ilt}$ indicator variables identify whether a given borrower i financing their purchase with lender l at time t was in treatment cell k, as defined in section 4.1. The key feature of this specification is to allow unique magnitudes of the difference-in-differences coefficient π_k for each treated cell. Identifying the unique impact of maturity policy breaks separately from interest rate breaks relies on the magnitudes of estimates of π_k^{mat} and π_k^{rate} not being perfectly correlated across each of the 106 identified policy breaks. The exclusion restriction is satisfied if variation in π_k^{mat} and π_k^{rate} is exogenous to pricing decisions, only affecting prices through loan maturities and interest rates.

Consider the following illustrative example. A particular institution has a policy in place that decreases allowable maturity by six months as a vehicle manufactured in 2006 rolls from three years old in December 2009 to four years old in January 2010. The same institution's policy calls for a 20 basis point increase in interest rates in this scenario. Meanwhile, a different lender's policy results in a 12-month decrease in maturity and a 10 basis-point interest-rate increase as three-year-old vehicles age by a year. The variation in the magnitudes of the maturity and interest-rate breaks across *Lender* × *Rollage* × *Rollyear* combinations allows us to simultaneously identify the causal impact of maturity and interest rate changes on prices.

Instruments for maturity and interest rates allow for a second-stage specification that utilizes equations (2) and (3) and is given by

$$\log Price_{iglt} = \sum_k \alpha_k \mathbb{I}_{k,ilt} + \eta^{mat} Maturity_i + \eta^{rate} Rate_i + X'_{iglt}\mu + \varepsilon_{iglt} \tag{4}$$

such that the η coefficients are semi-elasticities of price with respect to maturity and interest rates and represent the local average treatment effects of maturity and interest rates on prices for complier borrowers affected by the instruments. We report estimates in Table 5, where each column corresponds to the same column of Table 3. The estimate in column 6, for example, indicates that for a car of the same YMMT bought in the same month, holding fixed average differences in prices across commuting zones and lenders, an additional month of supplied maturity translates into 23 basis points higher price, significant at the 1% level. This compares to an estimate of 29 basis points per month of maturity in the two-stage least squares specification without interest rates in Appendix Table A1, indicating that roughly 80 percent of the effect on prices previously estimated is coming through the maturity channel. The coefficient on *Rate* indicates that for a one percentage-point increase in interest rates, prices fall by 90 basis points. Given our first-stage estimate of a change in interest rates of 16 basis points, these estimates imply that roughly 14 basis points of price impact is driven by changes in interest rates, compared to the total price impact of 70 basis points reported in Table 3.

The takeaway from Table 5 is not that interest rates don't matter much for prices. Had we designed our approach around finding shocks to interest rates, we may well have found large price impacts due to changes in lender-offered interest rates. Instead, Table 5 provides evidence that there is a significant impact of maturities on prices independent of interest-rate effects—the maturity effects on price reported in Tables 3 and A1 are not interest rate effects in disguise. Given the relative importance of maturity for monthly payments and the salience of monthly payment size (Argyle et al., 2017a), we interpret these results as evidence that the maturity dimension of financing is capitalized into durable goods prices.

5.2 Unobserved Heterogeneity

One novel aspect of our empirical strategy is that our ability to control for YMMT $\times$ month fixed effects substantially reduces the scope for our estimates of price effects to be driven by substitution toward lower quality goods. Indeed, the R^2 of our pricing model in Table 3 is over 0.9. Still, the fixed effects cannot conclusively rule out the possibility that unobserved vehicle or borrower heterogeneity plays some role in our results. Vehicles of a given YMMT in a given month may still exhibit meaningful differences in vehicle condition, including mileage, accident history, and whether it was owned by a smoker or driven by an aggressive, pizza-delivering teenager. Similarly, borrowers who take up loans with lower maximum maturities may also be different in some way correlated with their demand for cars. We address these concerns in several ways: by analyzing repeat-sales prices, testing for heterogenous effects in subsamples with relatively less scope for unobserved heterogeneity, and checking for changes in borrower composition in our difference-in-differences framework.

We first attempt to address unobserved quality concerns by evaluating prices for cars that sold multiple times in our sample. If our pricing results are driven by consumers that shift demand to cars with unobservably lower quality in response to being offered lower maturity loans, the lower quality would presumably be manifest in a lower relative price when the car is sold again. Relaxing our sample selection criteria for power considerations, our entire dataset features 8,697 cars with at least two transactions. We require the two transactions to occur at least 18 months apart to avoid contamination resulting from aggressive purchasers looking to quickly flip cars. Our repeat-sales pricing analysis begins with the calculation of pricing residuals for every transaction in our data, conditioning on YMMT $\times$ month fixed effects $\delta_{YMMT(i),t}$, lender fixed effects φ_l, and commuting zone fixed effects α_g in the following regression

$$\log Price_{iglt} = \alpha_g + \varphi_l + \delta_{YMMT(i),t} + u_{iglt}. \tag{5}$$

We then evaluate whether the fitted pricing residuals for second sale transactions are unusually low if the *first sale* for that car was a transaction with $Treatment \times Post = 1$ by running a difference-in-differences regression, with $Treatment$ and $Post$ taking their values as of the first sale at t_0:

$$\hat{u}_{iglt} = \beta_1 Treatment_{it_0} + \beta_2 Treatment_{it_0} \times Post_{t_0} + \varepsilon_{iglt}. \tag{6}$$

If our results are being driven by unobserved differences in quality, these differences would likely be persistent, resulting in lower prices for those same cars when sold a second time.

Before estimating equation (6), we assess the scope for our sample of repeat sales to be selected in important ways. Specifically, a concern for this exercise is the possibility for cars to only be observed selling twice in our data if they have not decreased in value significantly. Such endogenous resale behavior would bias our estimates of price effects at the second sale upwards if correlated with $Treatment \times Post$. In Appendix Table A3, we estimate a linear-probability model to see whether cars that faced financing with exogenously lower maturity due to a lender's maturity discontinuity ($Treatment \times Post = 1$) are less likely to be sold again. We find no evidence of differential selection into resale.

Table 6 presents results estimating equation (6). Column 1 reports that cars previously treated with low maturity sell for a statistically insignificant higher price (60 basis points) when sold a second time. Of course, this sample is different from our main sample so in column 2 we estimate the difference-in-differences regression for the first sale of the same 8,697 cars, essentially the same specification as in column 6 of Table 3.[20] The estimate shows that our main result—a pricing discount for cars treated with low maturity relative to otherwise comparable cars—holds in this subsample, though the statistical significance is muted due to a substantially smaller sample size. The difference between the first- and second-sale estimates is significant at the 10% level, indicating that financing-related discounts appear to rebound when the same car is sold subsequently. While we acknowledge that we only observe a small subsample of cars with repeat transactions, this price rebound at second sale is inconsistent with many forms of unobserved vehicle quality (accident history, high mileage, etc.) driving our results.

A complementary approach to testing whether our results are driven by unobserved vehicle heterogeneity is to examine subsamples of our data where the scope for unobserved heterogeneity is reduced. Younger cars, for example, have had less time to accumulate quality differences such as the beneficial effects of fastidious maintenance or the negative impacts of heavy use or accidents. As one measure of this, we show using National Household Travel Survey microdata that the standard deviation of vehicle mileage is strongly increasing in vehicle age (see Appendix Figure A1). To the extent that dealers may specialize in older and younger cars, this analysis also helps us understand whether dealer heterogeneity could be driving some of our results. Dividing our sample by the

[20]The only difference is that the fixed effects are accounted for in the creation of the pricing residuals rather than being estimated directly in the regression.

sample median age (five years), we re-estimate our 2SLS specification in equation (4) for young (average age three years old) and old cars (average 8.5 years old) and report these results in Table 7. Though the R^2 indicates greater scope for substitution to lower priced cars of a given YMMT among older vehicles, we find very similar effects of a month of offered maturity on prices in both samples, and a formal test fails to reject the equality of the maturity coefficients in the two samples.

Next, we follow the exercise of Oster (2017) and adjust our estimates for omitted variables bias. In Table 8 we adjust the $Treatment \times Post$ coefficients in the six reduced form price regressions from Table 3. Unsurprisingly, the bias is meaningfully large without adequate fixed effects (columns 1–2), though it does not change the sign of our estimates. With the addition of Age × MMT fixed effects, however, the adjusted estimate is within our original 95% confidence interval (column 3). This is true of the other more aggressive fixed effect structures as well (columns 4–6), where we see estimates very similar to the unadjusted coefficients reported earlier. Taken together, the results of Table 8 suggest that the scope for our results to be driven by unobserved product or borrower heterogeneity correlated with maturity policies is quite limited thanks to the richness of our controls.

Finally, we examine difference-in-differences estimates using the borrower controls (FICO and DTI) as dependent variables to explore potential changes in borrower composition. As expected given implied changes in monthly payments, we find a slight increase in reported DTIs in some specifications (point estimates range from $\frac{1}{25}$ to $\frac{1}{30}$ of a standard deviation) but no significant result when lender fixed effects are included. We find no significant change in FICO scores, regardless of the fixed effect structure. Appendix Figure A2 plots event studies of FICO and DTI by month of the year for treatment and control groups separately. The magnitudes of any differences are economically small with no consistent or statistically significant pattern that would suggest the composition of treated borrowers changing in January relative to control-group borrowers.

Taken together, these results suggest that unobservable heterogeneity in borrowers or car quality is not a likely source of bias in our estimates of the causal impact of maturity on prices.

5.3 Discussion

What is the underlying mechanism that would result in two buyers of observationally identical goods paying different prices? The auto market, like many durable goods markets in which consumers transact infrequently, is not characterized by a single market-clearing price. Instead, buyers and sellers typically bargain over the surplus defined by the difference between their private valuations (i.e., marginal willingness to pay and marginal willingness to accept). Shorter maturity impacts a borrower's budget constraint, which in turn may impact her private valuation, and ultimately, equilibrium prices. While our results establish that credit terms influence the cross-section of equilibrium prices, we do not directly observe the bargaining to be able to ascertain the precise process through which credit shocks generate heterogeneity in prices paid. Demonstrating bargaining as the exact mechanism would require truly unique data on the set of offers and counteroffers that lead to equilibrium prices. While our data do not record offers and counteroffers from the bargaining process (see Larsen, 2018 for an example of such a study), we appeal to patterns in borrower applications to provide suggestive evidence.

The Appendix describes a test and provides results using a sub-sample of lenders in our data that provide details on loan applications, from which we can evaluate the take-up rate of approved loans. Borrowers may reject offered loans in favor of a different loan with better terms. However, important for our purposes, a borrower could alternatively reject an offered loan because she entered into a negotiation for a car and could not come to terms with the seller on price. (Unfortunately, the specific reason for the rejection of approved loans is not provided in the data.) Appendix Table A4 shows that lenders that instantiate a reduction in offered maturity experience a decline in take-up rates of 7–8 percentage points. Though indirect and only suggestive, the evidence is consistent with decreases in offered maturity reducing the bargaining surplus, resulting in fewer consummated car purchases.

6 Conclusion

We investigate the impact that cross-sectional variation in credit terms has on the prices paid for durable goods. We find that borrowers treated with 12 months shorter maturity pay roughly 2.8% less for cars of the same manufacture year-make-model-trim (YMMT) at the same point in time compared to unaffected borrowers. These results are not driven by changes in the interest rates of the accompanying loans. Moreover, the prices of cars bought by affected borrowers, if anything, rebound when sold in later transactions, indicating that initial price differentials were unlikely to be driven by unobservable quality differences within YMMT. Our interpretation is that constrained buyers, pinched by lower maturity and the associated higher monthly payments, have lower private valuation for cars in their choice set. This lower private valuation affects their incentives in the search and bargaining processes inherent in the auto market, resulting in lower realized prices for observationally equivalent vehicles. Frictions in the auto market likely play a significant role in facilitating the pass-through of finance terms to prices at the individual level—including search and bargaining or sticky demand driven by consumer preferences for a certain car type, brand, or dealership. While it is an open question whether our results would generalize to other markets, many markets for big-ticket items are characterized by similar frictions (real estate, machines, furniture, higher education, labor, etc.).

Our focus on the cross-section of prices sheds light on the incidence of credit-supply shocks. Our results suggest that the price impact of changes in the cost of credit is concentrated among affected borrowers, rather than being spread across all borrowers through an aggregate demand channel. This serves to decrease any wedge in surplus between treated and untreated borrowers caused by differential access to credit. Meanwhile, sellers are sorted into winners and losers based on the financing of their buyers.[21]

To assess the net impact on a typical treated borrower, we estimate the break-even discount rate—the rate at which a borrower would be indifferent between a higher price with longer maturity and a lower price with shorter maturity—using our estimated maturity, interest rate, and price effects. Consider a borrower buying an average car priced at \$20,000 financed by a 72-month loan at an interest rate of 4.2% and an LTV of 90%. Under these parameters, the borrower would put \$2,000 down and have a monthly payment of \$283.26 for 72 months. Compare this to a typical treated borrower receiving a 60-month loan (the modal maturity shock in our data is 12 months). This borrower would pay 3.6% less for the car, or \$19,280. We estimated treatment effects on interest rates as large as 16 basis points for borrowers affected by a maturity shock of 2.3 months, which we gross up to 84 basis points ($16 \times (12/2.3)$) to be consistent with a 12-month maturity shock. This results in a 60-month loan for \$17,352 at an interest rate of 5.04%, a downpayment of \$1,928, and a \$327.77 monthly payment. Compared to the untreated borrower, the treated borrower has a lower initial down payment by \$72, then makes higher monthly payments by \$44.51 for 60

[21]Note that our findings of the impact of consumer financing disruptions on sellers provides a positive economic rationale for commonplace vertical integration between lenders and dealers.

months, and finally benefits from not having to make any payments in months 61–72. The internal rate of return on the marginal cash flows is about 8.9%—the annual discount rate that would make borrowers indifferent between a lower purchase price and higher monthly payments.

Our analysis speaks to the transmission of policy actions through to final-goods prices. For example, one goal of monetary policy is to influence consumer demand through the interest-rate channel. Our results demonstrate how capitalization effects can blunt monetary policy's ability to affect demand by changing monthly payments. Moreover, demand, and ultimately prices, can be influenced through dimensions of the credit surface besides rates, such as maturity. Given the importance of monthly debt service capacity to consumer demand and equilibrium prices, a parameter of interest is an estimate of the sensitivity of durable goods prices to changes in monthly debt service payments. Our estimates can be used to recover a price-to-monthly payment elasticity. Using the same baseline calibration exercise just described, modal treatment effects would move monthly payments by 15.7%. Also recall our second-stage price estimates of a 3.6% decline in prices. The elasticity of price to changes in monthly payments can be calculated by dividing estimated price changes by estimated changes in monthly payment amounts. This calculation implies an elasticity estimate of –0.23, suggesting that policy actions that increase monthly payment amounts by 10% would be associated with price declines of 2.3%.

We view our results as a novel contribution to the literature investigating the link between credit and prices. While most studies that link credit and prices evaluate credit shocks in the time series and examine their impact on aggregate price levels, our focus on the cross-section of borrowers potentially provides insight into how aggregate prices move in the presence of credit shocks. Additional evidence suggests that financing terms may affect the dynamics of the bargaining game between sellers and retail buyers, consistent with the literature showing the effect of corporate debt on various forms of negotiations. Finally, our results also have implications for the optimal design of macroprudential policy. Given the tight link between payment size, asset prices, and demand, maturity is an important if presently overlooked lever in affecting prices and consumption. Overall, our results call for further examination of the attributes of loan contracts that consumers most value with potential implications for credit product design.

References

Adelino, M., A. Schoar, and F. Severino (2013). "Credit Supply and House Prices: Evidence from Mortgage Market Segmentation." NBER Working Paper No. 17832.

Argyle, B., T. Nadauld, and C. Palmer (2017a). "Monthly Payment Targeting and the Demand for Maturity." Working Paper.

— (2017b). "Real Effects of Search Frictions in Consumer Credit Markets." MIT Sloan Working Paper 5242-17.

Benmelech, E., R. Meisenzahl, and R. Ramcharan (2017). "The real effects of liquidity during the financial crisis: Evidence from automobiles." *Quarterly Journal of Economics* 132.1, pp. 317–365.

Bhutta, N. and D. Ringo (2017). "The Effect of Interest Rates on Home Buying: Evidence from a Discontinuity in Mortgage Insurance Premiums." SSRN Working Paper No. 3085008.

Borio, C. E. and P. W. Lowe (2002). "Asset prices, financial and monetary stability: exploring the nexus." BIS Working Paper No. 114.

Busse, M., C. Knittel, and F. Zettelmeyer (2012). "Stranded Vehicles: How Gasoline Taxes Change the Value of Households' Vehicle Assets." Working Paper.

Busse, M., J. Silva-Risso, and F. Zettelmeyer (2006). "$1,000 cash back: The pass-through of auto manufacturer promotions." *American Economic Review* 96.4, pp. 1253–1270.

Cornia, M., K. Gerardi, and A. Shapiro (2011). "Price Discrimination and Business-Cycle Risk." Federal Reserve Bank of Atlanta Working Paper 2011-3.

Correia, S. (2016). *Linear Models with High-Dimensional Fixed Effects: An Efficient and Feasible Estimator*. Tech. rep. Working Paper.

Davis, M. et al. (2017). "The Impact of Interest Rates on House Prices and Housing Demand: Evidence from a Quasi-Experiment." Working Paper.

De Chaisemartin, C. and X. D'HaultfŒuille (2017). "Fuzzy differences-in-differences." *The Review of Economic Studies* 85.2, pp. 999–1028.

Di Maggio, M. and A. Kermani (2017). "Credit-induced boom and bust." *The Review of Financial Studies* 30.11, pp. 3711–3758.

Englmaier, F., A. Schmöller, and T. Stowasser (2017). "Price Discontinuities in an Online Market for Used Cars." *Management Science*.

Favara, G. and J. Imbs (2015). "Credit Supply and the Price of Housing." *American Economic Review* 105.3, pp. 958–992.

Gavazza, A., A. Lizzeri, and N. Roketskiy (2014). "A quantitative analysis of the used-car market." *American Economic Review* 104.11, pp. 3668–3700.

Glaeser, E. L., J. D. Gottlieb, and J. Gyourko (2012). “Can Cheap Credit Explain the Housing Boom?” *Housing and the Financial Crisis*. University of Chicago Press, pp. 301–359.

Goolsbee, A. (1998). “Investment tax incentives, prices, and the supply of capital goods.” *The Quarterly Journal of Economics* 113.1, pp. 121–148.

Hansman, C. et al. (2018). “Riding the Credit Boom.” NBER Working Paper No. 24586.

Hennessy, C. A. and D. Livdan (2009). “Debt, bargaining, and credibility in firm–supplier relationships.” *Journal of Financial Economics* 93.3, pp. 382–399.

Hertzberg, A., A. Liberman, D. Paravisini, et al. (2018). “Screening on loan terms: evidence from maturity choice in consumer credit.” *The Review of Financial Studies*, hhy024.

Huang, G., H. Luo, and J. Xia (2015). “Invest in information or wing it? A model of dynamic pricing with seller learning.” SSRN Working Paper No. 2668838.

Hudson, S., P. Hull, and J. Liebersohn (2017). “Interpreting Instrumented Difference-in-Differences.” MIT Mimeo.

Israel, R. (1991). “Capital structure and the market for corporate control: The defensive role of debt financing.” *The Journal of Finance* 46.4, pp. 1391–1409.

Jordà, Ò., M. Schularick, and A. M. Taylor (2015). “Betting the house.” *Journal of International Economics* 96, S2–S18.

Krishnamurthy, A. and T. Muir (2017). “How credit cycles across a financial crisis.” NBER Working Paper No. 23850.

Landvoigt, T., M. Piazzesi, and M. Schneider (2015). “The Housing Market(s) of San Diego.” *American Economic Review* 105.4, pp. 1371–1407.

Larsen, B. (2018). “The Efficiency of Real-World Bargaining: Evidence from Wholesale Used-Auto Auctions.” NBER Working paper No. 20431.

Lee, A. J. and D. R. Ames (2017). ““I can’t pay more” versus “It’s not worth more”: Divergent effects of constraint and disparagement rationales in negotiations.” *Organizational Behavior and Human Decision Processes* 141, pp. 16–28.

Lucca, D. O., T. Nadauld, and K. Chen (2016). “Credit supply and the rise in college tuition: Evidence from the expansion in federal student aid programs.” FRB of NY Staff Report No. 733.

Matsa, D. A. (2010). “Capital structure as a strategic variable: Evidence from collective bargaining.” *The Journal of Finance* 65.3, pp. 1197–1232.

Mian, A. R. and A. Sufi (2018). “Finance and business cycles: the credit-driven household demand channel.” NBER Working Paper No. 24322.

Mian, A. and A. Sufi (2009). “The consequences of mortgage credit expansion: Evidence from the US mortgage default crisis.” *The Quarterly Journal of Economics* 124.4, pp. 1449–1496.

Mian, A. and A. Sufi (2011). "House prices, home equity-based borrowing, and the US household leverage crisis." *American Economic Review* 101.5, pp. 2132–56.

Mian, A., A. Sufi, and E. Verner (2017). "Household debt and business cycles worldwide." *The Quarterly Journal of Economics.*

Müller, H. M. and F. Panunzi (2004). "Tender offers and leverage." *The Quarterly Journal of Economics* 119.4, pp. 1217–1248.

Nadauld, T. D. and S. M. Sherlund (2013). "The impact of securitization on the expansion of subprime credit." *Journal of Financial Economics* 107.2, pp. 454–476.

Oster, E. (2017). "Unobservable Selection and Coefficient Stability: Theory and Evidence." *Journal of Business & Economic Statistics*, pp. 1–18.

Rice, T. and P. Strahan (2010). "Does Credit Competition Affect Small-Firm Finance?" *The Journal of Finance* 65.3, pp. 861–889.

Sallee, J. M. (2011). "The surprising incidence of tax credits for the Toyota Prius." *American Economic Journal: Economic Policy* 3.2, pp. 189–219.

Spiegel, Y. and D. F. Spulber (1994). "The capital structure of a regulated firm." *The RAND Journal of Economics*, pp. 424–440.

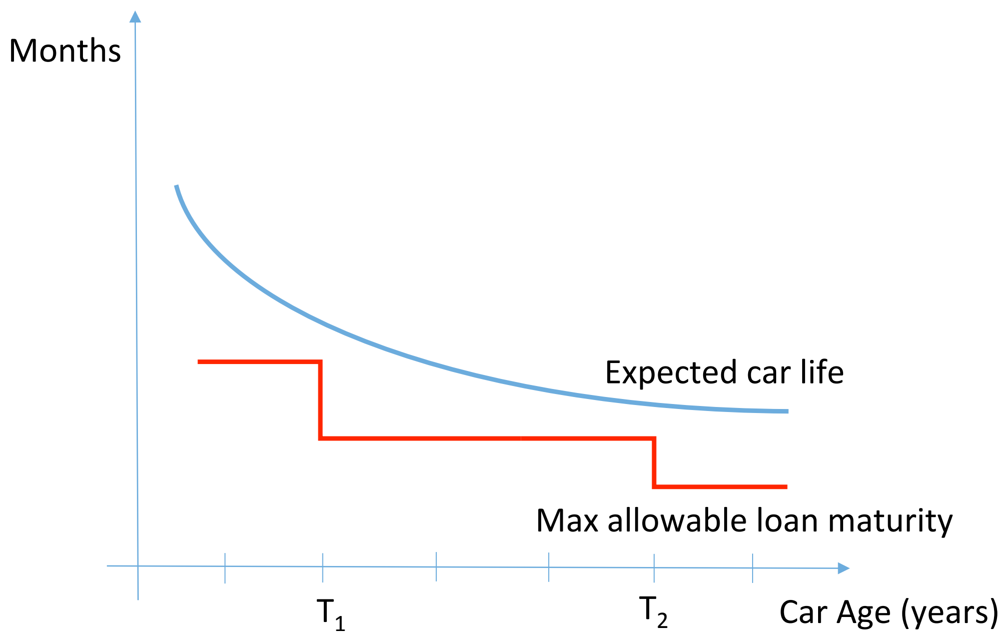
Stanton, R., C. Strickland, and N. Wallace (2015). "A New Dynamic House-Price Index for Mortgage Valuation and Stress Testing." Working Paper.

Verner, E. and G. Gyöngyösi (2018). "Household Debt Revaluation and the Real Economy: Evidence from a Foreign Currency Debt Crisis." Working Paper.

Zevelev, A. A. (2016). "Does Collateral Value Affect Asset Prices? Evidence from a Natural Experiment in Texas." SSRN Working Paper No. 2815609.

Tables and Figures

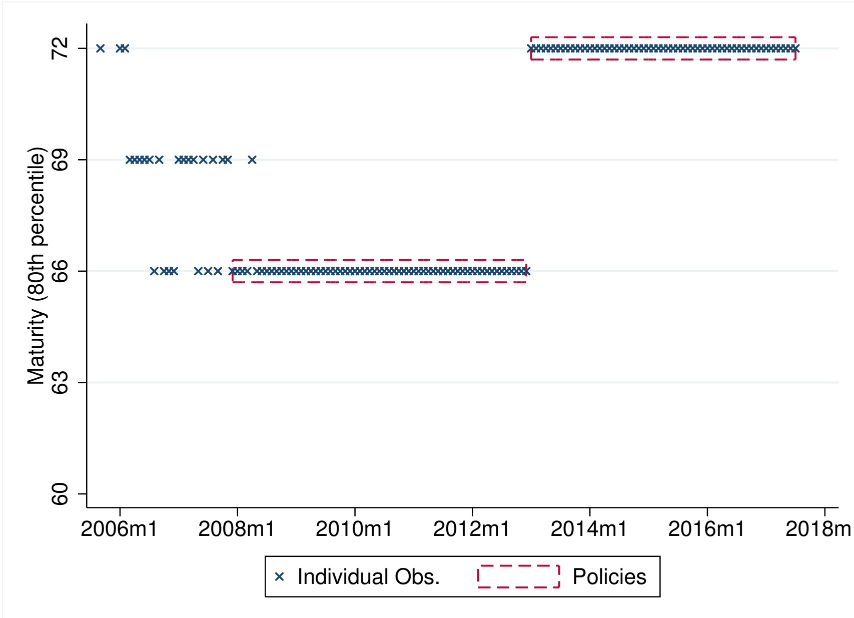
Figure 1: Hypothetical Lender Maximum Maturity Rule



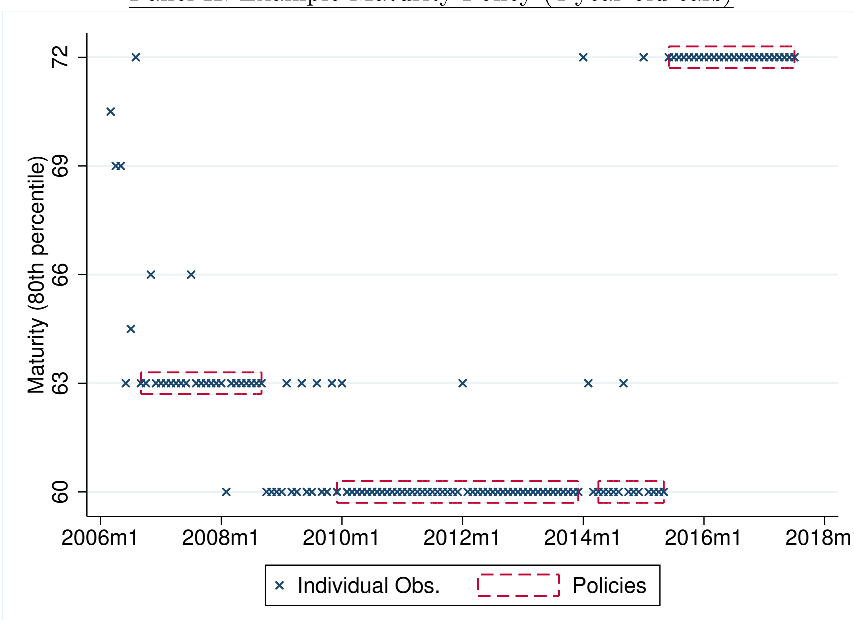
Notes: Figure depicts a hypothetical discontinuous lender rule. While the conditional expected remaining life of a car decreases smoothly with car age, many lenders have discrete maximum allowable loan maturity rules that discontinuously decrease maximum allowable maturity once a car age reaches certain cutoffs.

Figure 2: Example-Lender Maturity Policies

Panel I. Example Maturity Policy (3-year-old cars)

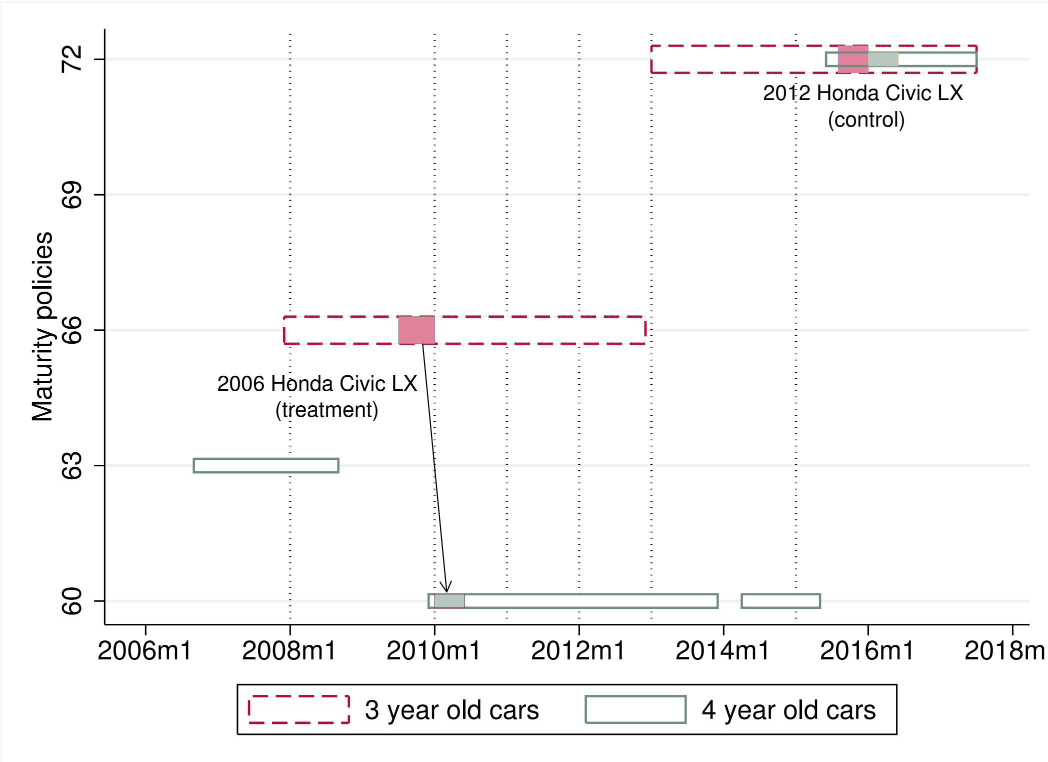


Panel II. Example Maturity Policy (4-year-old cars)



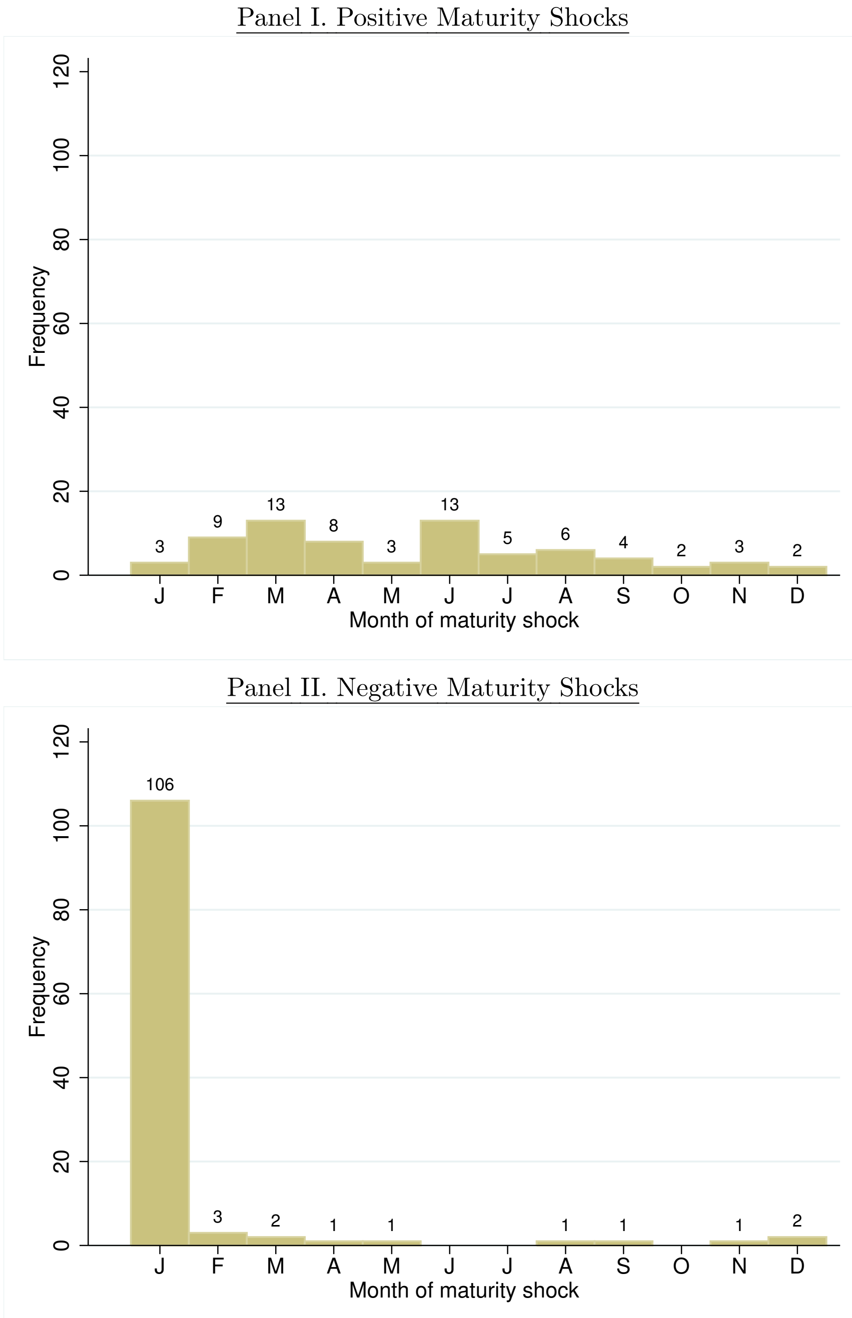
Notes: Figure plots the maturity policies for the largest lender in our data. Individual observations x capture the 80th percentile of maturity for a given car age within a given month at this lender. Boxed areas correspond to the maturity policies identified by our algorithm (see section 4.1 for details). The top panel and bottom panels show the policies for three- and four-year-old cars, respectively.

Figure 3: Example-Lender Maturity Shocks By Car Age



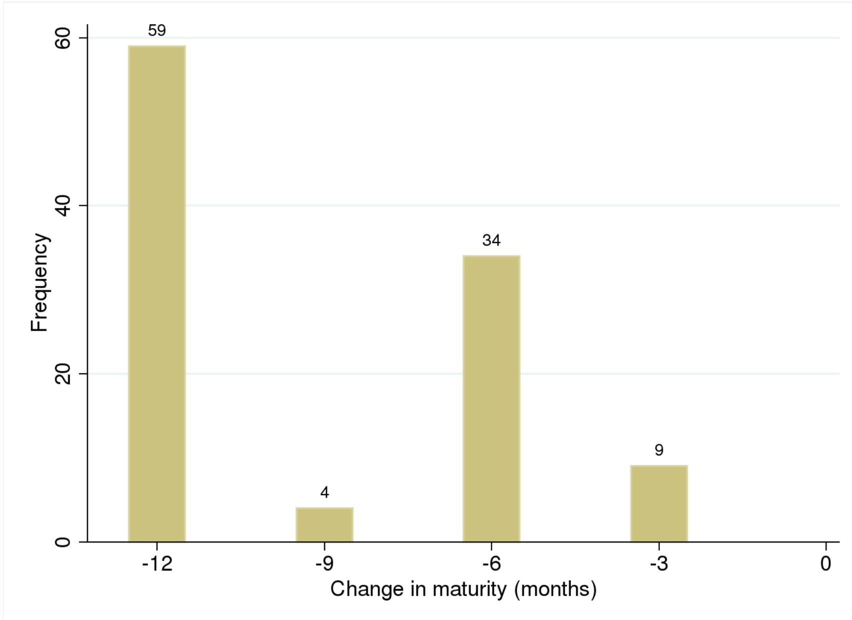
Notes: Figure illustrates our empirical design by plotting treatment- and control-group observations against identified maturity policies for the example lender used in Figure 2. Dashed rectangles correspond to the lender's maturity policy for three-year-old cars, as identified in Figure 2. Similarly, the solid rectangles represent the maturity policy for four-year-old cars for the same lender. Dotted vertical lines represent instances of maturity shocks in which a given vehicle would receive discontinuously lower maturity in January relative to December. As an example of a treated transaction in our sample, a 2006 Honda Civic LX bought in December 2009 would have had a maturity policy of 66 months, whereas the same vehicle purchased in January 2010 would have a 60 month maximum allowable maturity. In contrast, by 2016 the policies for three and four year old cars are both 72 months. Thus, a 2012 Honda Civic LX bought in December 2015 or January 2016 has the same maturity policy and is assigned to the control group.

Figure 4: Distribution of Maturity Shock Timing



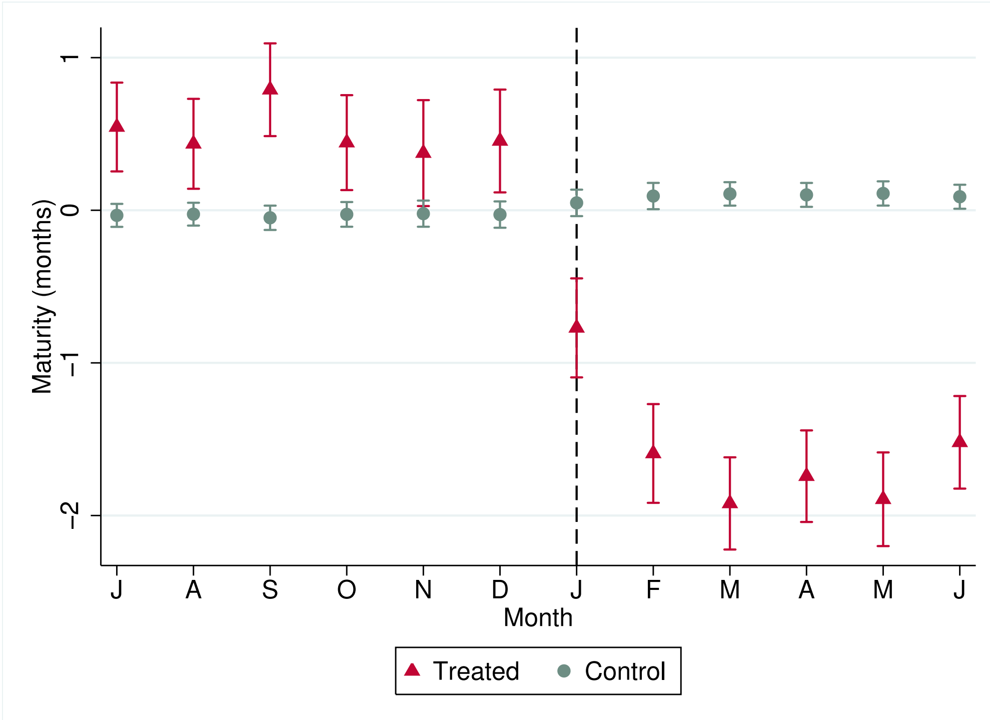
Notes: Figure plots the number of discrete changes in maturity identified in our data that occur in each month for both positive (upper panel) and negative (lower panel) shocks to maturity.

Figure 5: Distribution of Maximum Maturity Shocks by Size in Months



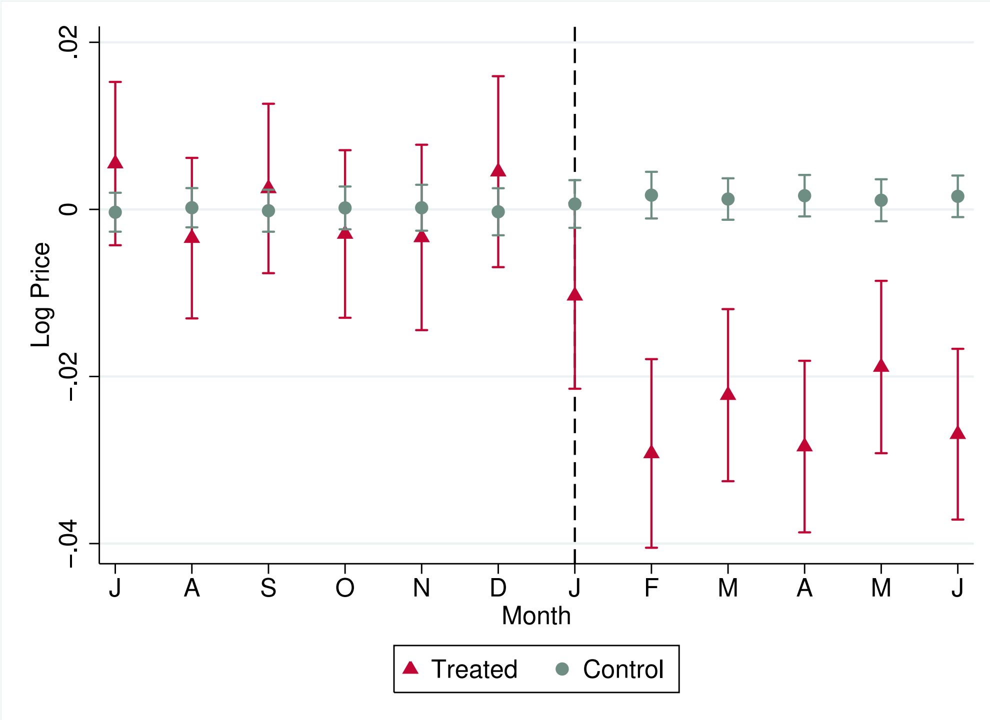
Notes: Figure plots number of occurrences of detected discontinuous maturity drops based on the size of the drop in maximum offered maturity.

Figure 6: Average Maturities Around Year End



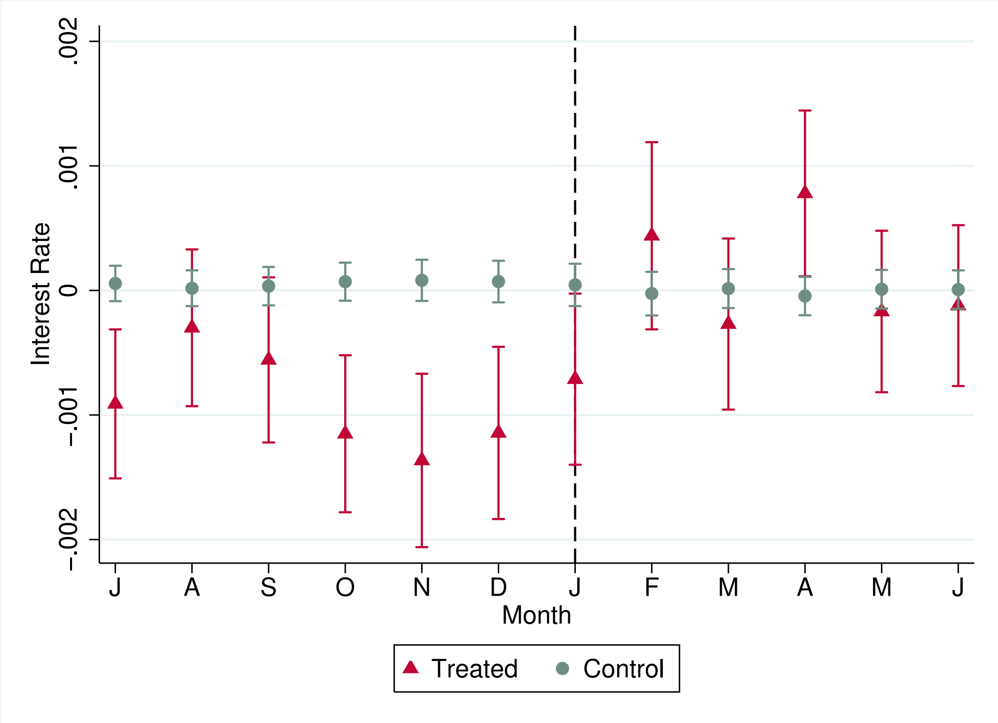
Notes: Figure plots the average conditional maturity by month around the year end for both treatment and control groups. We first regress maturity on car age × month-of-sale fixed effects and commuting zone fixed effects and then plot average residuals within each month.

Figure 7: Average Price Around Year End



Notes: Figure shows average conditional log price around year end for both treatment and control groups. We first regress the log of car price on car age × month-of-sale fixed effects and commuting zone fixed effects and then plot the average residuals within each month.

Figure 8: Average Interest Rate Around Year End



Notes: Figure shows the average conditional interest rate around the new year for both treatment and control groups. We first regress interest rate on car age × month-of-sale fixed effects and commuting zone fixed effects and then plot the average residuals within each month.

Table 1: Summary Statistics

	Overall Estimation Sample (1)	Control Sample (2)	Treatment Sample (3)	Difference (2) - (3)
FICO	714.1	714.5	706.9	7.6
	(69.0)	(69.0)	(68.2)	[0.3]
DTI	0.346	0.347	0.331	0.016
	(0.256)	(0.259)	(0.199)	[0.001]
LTV	0.907	0.907	0.907	0.000
	(0.222)	(0.222)	(0.217)	[0.001]
Car Age	3.88	3.86	4.29	-0.43
	(2.95)	(2.94)	(3.09)	[0.01]
Car Price	20,341	20,432	18,821	1,611
	(9,432)	(9,460)	(8,951)	[39.5]
Maturity	61.3	61.4	59.3	2.1
	(12.8)	(12.8)	(12.4)	[0.05]
Interest Rate	0.0410	0.0409	0.0431	-0.0022
	(0.0244)	(0.0244)	(0.0246)	[0.0001]
Observations	972,621	917,864	54,757	

Notes: The table shows means and standard deviations in parentheses for the overall estimation sample (1), alongside statistics for the control (2) and treatment (3) samples. The differences in means (2) - (3) are reported in the final column with standard errors in brackets. FICO is the credit score of the borrower as of the origination date of the loan; DTI is the debt-to-income ratio of the borrower; LTV is the loan-to-value ratio for the vehicle being financed.

Table 2: First-Stage Difference-in-Differences Results on Maturity

Maturity	(1)	(2)	(3)	(4)	(5)	(6)
Treatment × Post	-2.404***	-2.390***	-2.021***	-2.157***	-2.284***	-2.290***
	(0.664)	(0.303)	(0.277)	(0.304)	(0.271)	(0.265)
Treatment	-0.932	-0.395	-0.325	-0.371	0.561**	0.368
	(0.955)	(0.406)	(0.403)	(0.365)	(0.282)	(0.263)
Post	-0.872***	0.913***	0.754***			
	(0.197)	(0.188)	(0.102)			
Borrower Controls	Yes	Yes	Yes	Yes	Yes	Yes
Car Age FE		Yes				
Age × MMT FE			Yes			
YMMT × Month FE				Yes	Yes	Yes
Commuting Zone FE					Yes	Yes
Lender FE						Yes
Observations	972,621	972,621	972,621	972,621	972,621	972,621
R-squared	0.004	0.132	0.207	0.350	0.407	0.447

Notes: Table reports difference-in-differences regressions of loan maturities measured in months over an event year from July to June. Post is a dummy equal to one for observations after January 1st. Treatment is a dummy equal to one for loans that originate from a lender whose maximum maturity policy changed discontinuously for the transacted car at January 1, as discussed in section 4. Borrower controls include FICO (the credit score of the borrower at loan origination) and DTI (the back-end debt-to-income ratio of the borrower at origination). YMMT signifies combinations of manufacture year-make-model-trim. Robust standard errors (in parentheses) are double clustered by month and commuting zone. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3: Reduced-Form Difference-in-Differences Results on log(Price)

log(Price)	(1)	(2)	(3)	(4)	(5)	(6)
Treatment × Post	-0.026	-0.027***	-0.009***	-0.006**	-0.007***	-0.007***
	(0.033)	(0.006)	(0.003)	(0.003)	(0.003)	(0.002)
Treatment	-0.059	-0.025***	-0.009*	-0.007	0.006	0.006
	(0.050)	(0.008)	(0.005)	(0.006)	(0.005)	(0.005)
Post	-0.052***	0.061***	0.060***			
	(0.007)	(0.006)	(0.006)			
Borrower Controls	Yes	Yes	Yes	Yes	Yes	Yes
Car Age FE		Yes				
Age × MMT FE			Yes			
YMMT × Month FE				Yes	Yes	Yes
Commuting Zone FE					Yes	Yes
Lender FE						Yes
Observations	972,621	972,621	972,621	972,621	972,621	972,621
R-squared	0.060	0.369	0.872	0.909	0.911	0.914

Notes: Table reports difference-in-differences regressions results of log(prices) over an event year from July to June. Post is a dummy equal to one for observations after January 1st. Treatment is a dummy equal to one for loans that originate from a lender whose maximum maturity policy changed discontinuously for the transacted car at January 1, as discussed in section 4. Borrower controls include FICO (the credit score of the borrower at loan origination) and DTI (the back-end debt-to-income ratio of the borrower at origination). YMMT signifies combinations of manufacture year-make-model-trim. Robust standard errors (in parentheses) are double clustered by month and commuting zone. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4: Reduced-Form Difference-in-Differences Results on Interest Rates

Interest Rate	(1)	(2)	(3)	(4)	(5)	(6)
Treatment × Post	0.0006	0.0004	0.0007	0.0009	0.0012*	0.0016***
	(0.0010)	(0.0011)	(0.0008)	(0.0007)	(0.0007)	(0.0005)
Treatment	0.0001	-0.0002	-0.0020*	-0.0030***	-0.0009	-0.0005
	(0.0014)	(0.0017)	(0.0011)	(0.0008)	(0.0005)	(0.0004)
Post	0.0002	-0.0006	-0.0001			
	(0.0007)	(0.0006)	(0.0002)			
Borrower Controls	Yes	Yes	Yes	Yes	Yes	Yes
Car Age FE		Yes				
Age × MMT FE			Yes			
YMMT × Month FE				Yes	Yes	Yes
Commuting Zone FE					Yes	Yes
Lender FE						Yes
Observations	972,621	972,621	972,621	972,621	972,621	972,621
R-squared	0.426	0.443	0.499	0.604	0.640	0.664

Notes: Table reports difference-in-differences regressions of loan interest rates over an event year from July to June. Post is a dummy equal to one for observations after January 1st. Treatment is a dummy equal to one for loans that originate from a lender whose maximum maturity policy changed discontinuously for the transacted car at January 1, as discussed in section 4. Borrower controls include FICO (the credit score of the borrower at loan origination) and DTI (the back-end debt-to-income ratio of the borrower at origination). YMMT signifies combinations of manufacture year-make-model-trim. Robust standard errors (in parentheses) are double clustered by month and commuting zone. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 5: Two-Stage Least Squares Effects of Maturity and Rate on log(Price)

log(Price)	(1)	(2)	(3)	(4)	(5)	(6)
Maturity	0.0044*	0.0074***	0.0034***	0.0022***	0.0024***	0.0023***
	(0.0025)	(0.0015)	(0.0007)	(0.0004)	(0.0004)	(0.0004)
Interest Rate	-0.3650	-1.9512***	-1.4669***	-0.9201***	-0.8630***	-0.9049***
	(0.8457)	(0.6278)	(0.4177)	(0.3234)	(0.3283)	(0.3327)
Borrower Controls	Yes	Yes	Yes	Yes	Yes	Yes
Car Age FE		Yes				
Age × MMT FE			Yes			
YMMT × Month FE				Yes	Yes	Yes
Commuting Zone FE					Yes	Yes
Lender FE						Yes
Observations	972,621	972,621	972,621	972,621	972,621	972,621
R-squared	0.164	0.421	0.875	0.911	0.914	0.916

Notes: Table reports two-stage least-squares regressions of log transaction prices on loan maturity and interest rate. Excluded instruments are the interactions of Post with Treatment dummies that identify treated lender x rollage x rollyear combinations (as discussed in section 4). Post is collinear with fixed effects and is omitted. Borrower controls include FICO scores, DTI, and Treatment dummies. Robust standard errors (in parentheses) are double clustered by month and commuting zone. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 6: Repeat Sales Reduced-form Effects of Treatment on log(Price)

	Second-sale log(Price) (1)	Initial-sale log(Price) (2)	Difference (1) - (2)
Treatment × Post	0.006	-0.012	0.018*
	(0.007)	(0.010)	(0.011)
Treatment	-0.005	0.009	
	(0.004)	(0.010)	
YMMT × Month FE	Yes	Yes	
CZ FE	Yes	Yes	
Lender FE	Yes	Yes	
Observations	8,697	8,697	
R-squared	0.001	0.001	

Notes: Table reports difference-in-differences regressions results of log price residuals for a subsequent second sale (1) and the initial sale (2) for those cars that we observe transacting twice in the entire dataset, at least 18 months apart. These pricing residuals $\hat{u}$ are calculated from equation (5) by controlling for manufacture year-make-model-trim by month fixed effects, commuting zone, and lender fixed effects. Post is a dummy equal to one for observations for which the first sale occurred after January 1st. Treatment is a dummy equal to one for observations for which the first loan was originated by a lender whose maximum maturity policy changed discontinuously for the transacted car at January 1, as discussed in section 4. Robust standard errors (in parentheses) are double clustered by month and commuting zone. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 7: Two-Stage Least Squares Effects of Maturity and Rate on log(Price) by Vehicle Age

	1–5 year old cars		6+ year old cars	
log(Price)	(1)	(2)	(3)	(4)
Maturity	0.0023**	0.0025**	0.0022***	0.0019***
	(0.0011)	(0.0011)	(0.0006)	(0.0006)
Interest Rate	0.3552	0.5287	-1.1577***	-1.2614***
	(1.0143)	(1.0523)	(0.4177)	(0.4129)
YMMT × Month FE	Yes	Yes	Yes	Yes
Commuting Zone FE	Yes	Yes	Yes	Yes
Lender FE		Yes		Yes
Observations	523,648	523,648	448,943	448,943
R-squared	0.907	0.909	0.870	0.876

Notes: Table reports 2SLS regressions of log transaction prices on maturity and rates for used cars less than five years old (column 1) and five or more years old (column 2). Excluded instruments are the interactions of Post with Treatment dummies that identify treated lender x rollage x rollyear combinations (as discussed in section 4). Borrower controls include FICO scores, DTI, and Treatment dummies. Robust standard errors (in parentheses) are double clustered by month and commuting zone. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 8: Price Effects Adjusted for Potential Omitted Variable Bias

log(Price)	(1)	(2)	(3)	(4)	(5)	(6)
Estimated Coefficient	-0.026	-0.027	-0.009	-0.006	-0.007	-0.007
Omitted Variables Bias-Adjusted	-0.068	-0.071	-0.029	-0.010	-0.009	-0.007
Includes Zero?	No	No	No	No	No	No
Adjusted Coefficient within Original Confidence Interval?	No	No	Yes	Yes	Yes	Yes
Car Age FE		Yes				
Age $\times$ MMT FE			Yes			
YMMT $\times$ Month FE				Yes	Yes	Yes
Commuting Zone FE					Yes	Yes
Lender FE						Yes

Notes: Table reports coefficients adjusted for omitted variable bias for the reduced-form price regressions performed in 4. The first row reports the uncorrected coefficients found in Table 4. The second row reports coefficients adjusted for potential omitted variable bias following Oster (2017). The third row reports whether the interval between the unadjusted and adjusted coefficients includes zero. Finally, the fourth row reports whether the adjusted coefficient is included in the 95% confidence interval of the observed regression.

Appendix

Loan acceptance rates in application data

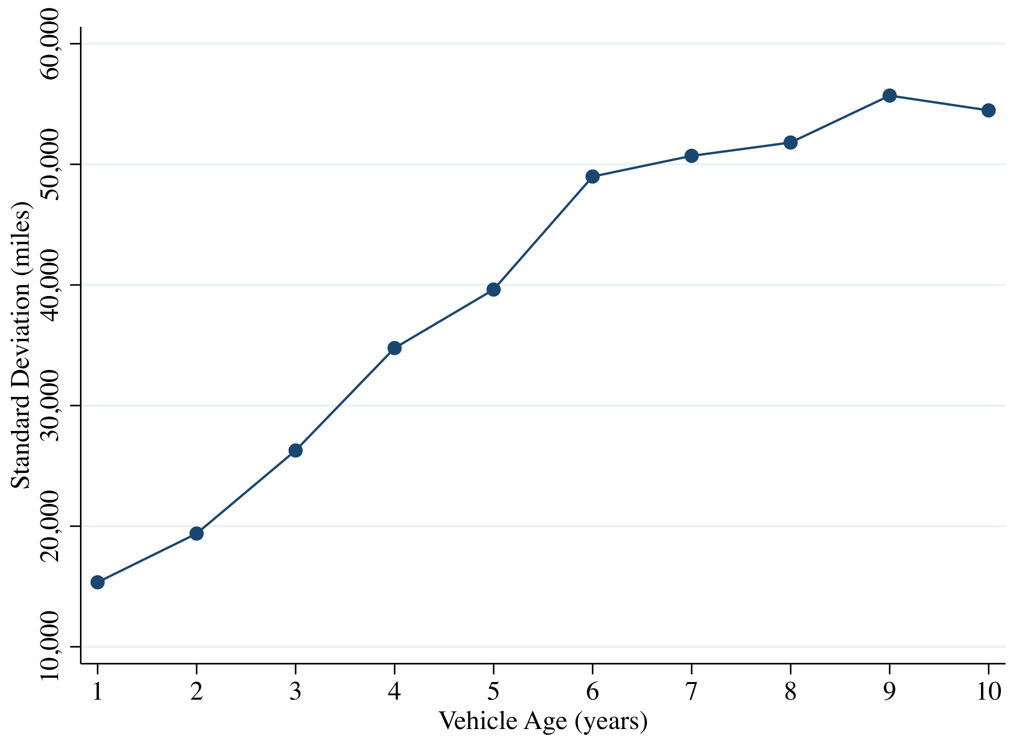
A subset of lenders in our data provide details on loan applications. Loan applications have three potential outcomes: approved and subsequently funded, denied, or approved and then rejected by the borrower. Application data thus allow us to observe borrowers that were approved for loans but chose not to originate the loan, an outcome we quantify in a variable *Take Up,* calculated as the fraction of all approved loans that are subsequently originated. Borrowers could reject offered loans in favor of a different offered loan with better terms. Alternatively, borrowers could reject offered loans because they entered into a negotiation for a car and could not come to terms with the seller on price and so they choose not to buy a given car. This is more likely to happen if a borrower is originating a loan directly from a lender to purchase a car in a private transaction, rather than from a dealer. Unfortunately, the specific reason for the rejection of approved loans is not provided in the data. Borrowers rejecting offered loans in favor of searching for a different loan or borrowers rejecting offered loans because of a failed negotiation are not mutually exclusive. A failed bargaining outcome could result in borrowers searching elsewhere or giving up all together.

We evaluate whether loan take-up rates at the institution level decrease for cars that have aged across a maturity discontinuity. We calculate take-up rates at the lender $\times$ commuting zone $\times$ month level and create a variable $Treatment$ equal to one for institutions with discontinuous maturity policies and a variable $Post$ equal to one in periods after a maturity shock. Calculating take-up rates at the lender-commuting zone-month level results in 13,572 observations. We then estimate the following regression

$$Take\,Up_{lgt} = \theta_1 Treatment_l \cdot Post_t + \pi_2 Treatment_l + \pi_3 Post_t + \rho_t + \lambda_{gl} + \varepsilon_{lgt}$$

Subscripts l, g, and t refer to lender l in commuting zone g at time t. Estimates of θ_1, reported in Appendix Table A4, indicate declines in take up rates for treated borrowers on the order of 7–8% that are largely robust to any combination of fixed effects. Though only suggestive, this evidence is consistent with decreases in offered maturity inducing more intensive search or bargaining, resulting in fewer originated loans supporting car purchases.

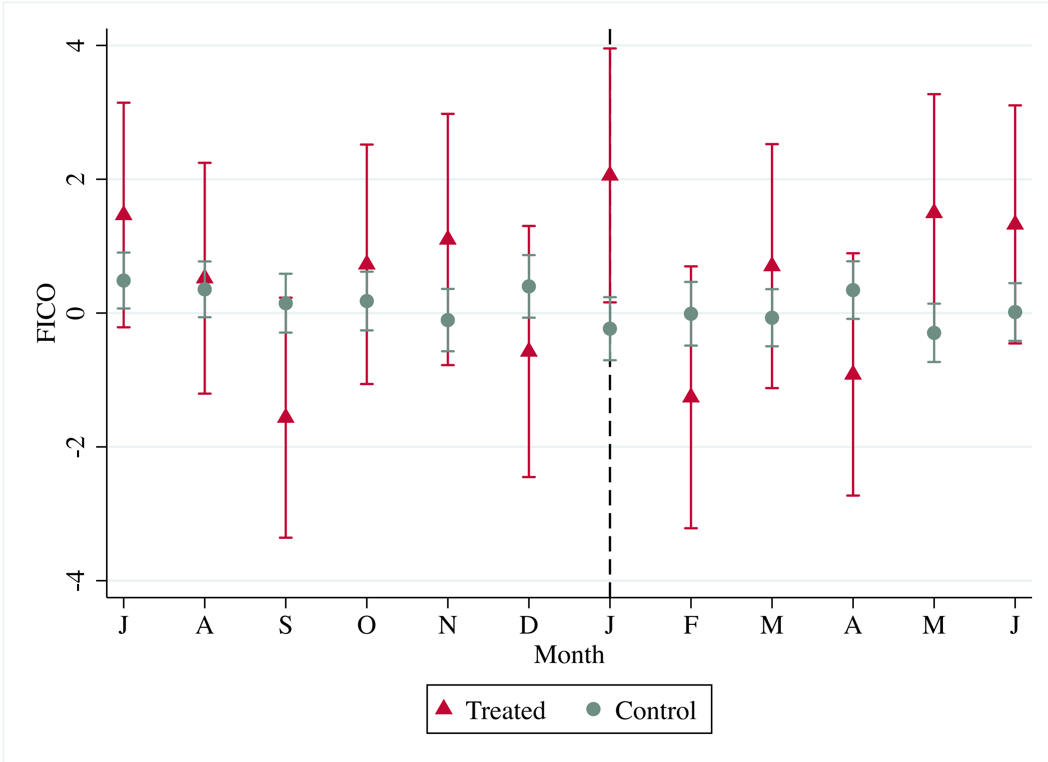
Figure A1: Mileage Variation by Car Age



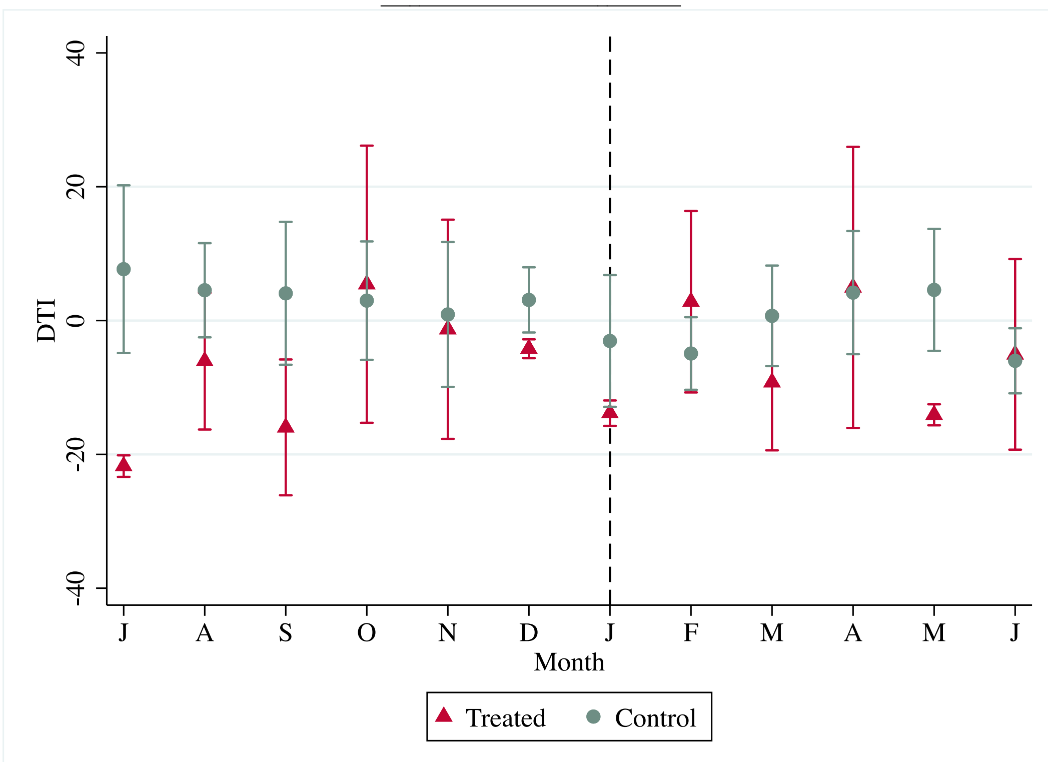
Notes: Figure plots standard deviation of vehicle odometer readings in miles by vehicle age for used cars 1–10 years old observed in the 2017 National Household Travel Survey.

Figure A2: Average Borrower Characteristics Around Year End

I. FICO Scores



II. Debt-to-Income Ratios



Notes: Figure plots an event study of average borrower characteristics by treatment and control group and month of year. Panel I plots average FICO scores, and panel II plots average debt-to-income ratios. We first regress interest rate on car age × month-of-sale fixed effects and commuting zone fixed effects and then plot the average residuals within each month.

Table A1: Two Stage Least Squares Effects of Maturity on Log Price

log(Price)	(1)	(2)	(3)	(4)	(5)	(6)
Maturity	0.0109	0.0112***	0.0042***	0.0027**	0.0032***	0.0029***
	(0.0112)	(0.0025)	(0.0014)	(0.0012)	(0.0012)	(0.0010)
Post	-0.0423***	0.0503***	0.0566***			
	(0.0134)	(0.0055)	(0.0054)			
Treatment	-0.0486	-0.0209**	-0.0079	-0.0061	0.0044	0.0048
	(0.0469)	(0.0085)	(0.0049)	(0.0051)	(0.0038)	(0.0041)
Borrower Controls	Yes	Yes	Yes	Yes	Yes	Yes
Car Age FE		Yes				
Age × MMT FE			Yes			
YMMT × Month FE				Yes	Yes	Yes
Commuting Zone FE					Yes	Yes
Lender FE						Yes
Observations	972,621	972,621	972,621	972,621	972,621	972,621
R-squared	0.210	0.411	0.874	0.911	0.913	0.916

Notes: Table reports difference-in-differences regressions of log prices for an event year running from July to June using two-stage least squares. The excluded instrument is Treatment × Post, where Post is a dummy equal to one for observations after January 1st. Treatment is a dummy equal to one for loans that originate from a lender that experienced a discontinuous policy change, as discussed in section 4. Post is collinear with fixed effects and is omitted. Borrower controls include FICO (the credit score of the borrower at loan origination) and DTI (the back-end debt-to-income ratio of the borrower at origination). Robust standard errors (in parentheses) are double clustered by month and commuting zone. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A2: Reduced-Form Difference-in-Differences Results for log(Price) Using Hold Out Sample

log(Price)	(1)	(2)	(3)	(4)	(5)	(6)
Treatment × Post	-0.0003	-0.031**	-0.012***	-0.009*	-0.009**	-0.009***
	(0.025)	(0.012)	(0.004)	(0.005)	(0.004)	(0.003)
Treatment	-0.101***	-0.026**	-0.007	-0.008	0.006**	0.008***
	(0.037)	(0.012)	(0.009)	(0.009)	(0.003)	(0.002)
Post	-0.062***	0.055***	0.063***			
	(0.008)	(0.005)	(0.006)			
Borrower Controls	Yes	Yes	Yes	Yes	Yes	Yes
Car Age FE		Yes				
Age × MMT FE			Yes			
YMMT × Month FE				Yes	Yes	Yes
Commuting Zone FE					Yes	Yes
Lender FE						Yes
Observations	232,984	232,984	232,984	232,984	232,984	232,984
R-squared	0.070	0.325	0.885	0.923	0.925	0.926

Notes: Table reports difference-in-differences regressions results of log prices for an event year running from July to June using a different sample to define treatment and control than the one used in estimation. We randomly divide observations into two groups at the lender × car age × month-of-sale level. Using the first group to define treatment and control (see section 4), we repeat the reduced-form analysis of prices from Table 3 using only the hold-out sample. Borrower controls include FICO (the credit score of the borrower at loan origination) and DTI (the back-end debt-to-income ratio of the borrower at origination). Robust standard errors (in parentheses) are double clustered by month and commuting zone. See notes to Table 3 for more details. *** p<0.01, ** p<0.05, * p<0.1

Table A3: Linear-Probability Model of Observed Resale Selection

$\mathbb{I}$(Second Sale Observed)	(1)	(2)	(3)	(4)	(5)	(6)
Treatment × Post	0.0009	0.0007	0.0015	0.0014	0.0016	0.0017
	(0.0013)	(0.0018)	(0.0017)	(0.0019)	(0.0017)	(0.0017)
Treatment	-0.0006	-0.0012	-0.0012	-0.0014	-0.0021	-0.0026**
	(0.0014)	(0.0019)	(0.0019)	(0.0019)	(0.0013)	(0.0013)
Post	0.0025***	-0.0004	-0.0014***			
	(0.0006)	(0.0006)	(0.0003)			
Car Age FE		Yes				
Age × MMT FE			Yes			
YMMT × Month FE				Yes	Yes	Yes
Commuting Zone FE					Yes	Yes
Lender FE						Yes
Observations	963,612	963,612	963,612	963,612	963,612	963,612
R-squared	0.001	0.006	0.0256	0.190	0.198	0.199

Notes: Table reports linear-probability model difference-in-differences regressions of a dummy for whether a given vehicle was observed being resold in our sample. Borrower controls include FICO (the credit score of the borrower at loan origination) and DTI (the back-end debt-to-income ratio of the borrower at origination). Sample includes the first sale per VIN from the main estimation sample subject to observing at least two sales per YMMT category. Robust standard errors (in parentheses) are double clustered by month and commuting zone. See notes to Table 6 for more details. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A4: Effect of Maturity Shock on Lender-level Take-up

Loan Take-up	(1)	(2)	(3)	(4)
Treatment × Post	-0.073***	-0.078***	-0.078***	-0.074***
	(0.025)	(0.025)	(0.025)	(0.0232)
Treatment	0.044**	0.044**	0.051**	0.046**
	(0.022)	(0.022)	(0.020)	(0.020)
Post	-0.007	-0.006		
	(0.009)	(0.009)		
Lender FEs	Yes	Yes	Yes	
CZ FEs		Yes	Yes	
Month FEs			Yes	Yes
Lender × CZ FEs				Yes
Observations	13,572	13,572	13,572	13,572
R-squared	0.427	0.458	0.509	0.585

Notes: Table reports difference-in-differences regressions results of take-up rates on treatment indicators. Take-up is defined as the fraction of observations of offered loans that are accepted at the lender-commuting zone-month level. Post is a dummy equal to one for observations after January 1st. Treat is a dummy equal to one for lenders with a discontinuous maximum allowable maturity at year end, as discussed in section 4, for any car age, in a given July to June period. Robust standard errors (in parentheses) are double clustered by month and commuting zone. *** $p<0.01$, ** $p<0.05$, * $p<0.1$