Consumer Credit and the Incidence of Tariffs: Evidence from the Auto Industry*

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Abstract

Captive finance subsidiaries create a channel for trade policy to affect consumer credit. Examining the impact of the Trump administration's metal tariffs on captive automobile lenders, we find that consumers received higher interest rates from captive lenders after the tariffs relative to unaffected non-captive lenders. Further, we document a disparate impact on low-income borrowers and in areas with less lending competition. Our results suggest that tariffs may impact not only the price of goods but also the financing terms of purchases. Thus, focusing solely on directly affected product prices may underestimate tariff pass-through significantly.

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1 Introduction

Understanding how U.S. trade policy filters through corporations to households is a first order economic and foreign policy concern. Several studies – such as Amiti, Redding, and Weinstein 2020 and Fajgelbaum et al. 2020 – document that a significant share of tariffs are passed on to American firms and consumers via higher goods prices. However, many manufacturers facilitate the sale of their products by offering financing through wholly owned captive finance subsidiaries (Murfin and Pratt 2019) or generous trade credit terms (Burkart and Ellingsen 2004). When the sale of a good is bundled with the provision of credit, both channels are available to pass through a tariff cost shock. Thus, focusing exclusively on the price of the good and ignoring the cost of financing may underestimate the measurement of tariff price incidence, much akin to Nakamura and Steinsson 2012.

This paper examines whether tariffs affect consumer credit terms. In the beginning of 2018, the Trump administration announced a 25 percent tariff on over 35 billion dollars of steel imports as well as a 10 percent tariff on aluminum. This created a large cost shock for American manufacturers using these metals, including the automobile industry (Cavallo et al. 2021). We examine the impact of this event on the auto loan market to address three specific questions. First, does a focus on the price of goods fully capture the cost of trade policy? Second, does vertical integration affect tariff cost pass-through? Third, do firms pass along tariff costs more intensely to less sophisticated or higher demand customers?

In some respects, the auto loan market is an ideal setting for examining nuance in the price incidence of tariffs. Auto loans are available through vertically integrated manufacturers (captive auto lenders) as well as non-integrated lenders (non-captives, such as financial institutions). While captive lenders were exposed to the metal tariffs through the manufacturing side of their businesses, non-captive lenders had no direct exposure and hence provide a natural counterfactual. Further, auto purchases often involve two price components, the vehicle purchase price and the financing terms, creating an opportunity for price shrouding. If consumers are less sensitive to increases in loan prices than vehicle prices as found in Busse and Silva-Risso 2010 and Grunewald et al. 2023, it could be optimal for an automobile manufacturer to pass on some or all of a cost shock through its financing terms.[1]

Our empirical evidence indicates that the impact of the metal tariffs was not limited to vehicle prices but, in fact, resulted in higher interest rates for borrowers from captive lenders. Essentially, having a vertically integrated captive lender expands the options for pass-through. While the transmission of monetary policy to household credit is well-documented (Bernanke and Gertler 1995; Di Maggio et al. 2017), we believe our paper provides the first evidence that trade policy affects the cost of consumer credit. As such, the measurement of tariff price incidence should encompass the impact on financing terms. Further, our granular data allows us to document that the increase in interest rates was most pronounced among lower income borrowers with less elastic credit demand and in areas with lower credit market competition, expanding the evidence on the heterogeneous costs of trade policy (Pierce and Schott 2020).

Our empirical analysis uses data on millions of auto loans from Regulation AB II. Under Regulation AB II, issuers of public auto loan asset-backed securities are required to report loan-level information to the Securities and Exchange Commission on a monthly basis. The reported information includes loan, vehicle, and borrower characteristics as of each loan's origination date, as well as loan performance histories over the entire life of each loan. A key feature of the Regulation AB II data is that it contains detailed information about the type of vehicle being financed, which allows us to hold the choice of vehicle fixed when measuring tariff pass-through. As shown later in Section 3.1.1, the Regulation AB II data is representative of both the population of auto loans in the United States as well as the complete auto loan portfolios of our sampled lenders.

Even with loan-level data, obtaining a consistent estimate of the impact of the metal tariffs is difficult because of potential confounding time trends. For example, the tariffs were enacted during tax rebate season when auto loan demand tends to be high, and auto lenders adjust their loan terms to clear the market (Adams, Einav, and Levin 2009). To resolve this and other empirical

1. Consumers might be less sensitive to increases in interest rates than vehicle prices for several reasons, including credit constraints (Argyle, Nadauld, and Palmer 2020) and behavioral factors (Stango and Zinman 2009; Chetty, Looney, and Kroft 2009).

challenges, we use loans from non-captive lenders as a control group for loans from captive lenders in a difference-in-differences design (Benneton, Mayordomo, and Paravisini 2022). While captive lenders were exposed to the tariffs via the manufacturing side of their business, non-captive lenders – i.e., their direct competitors – had no material exposure. Thus, under certain conditions, the response of non-captive lenders should serve as a valid counterfactual for the response of captive lenders in the absence of the tariffs. We verify the validity of non-captive lenders as a control group with numerous robustness checks in Section 4.2, and we explore the potential for spillover effects from the tariffs onto non-captive lenders in Section 4.6.

The granularity of the Regulation AB II data allows us to examine the evolution of loan terms for the same vehicle within groups of similar borrowers across captive and non-captive lenders. For instance, in our baseline empirical specification we compare captive auto loans to otherwise-identical non-captive auto loans originated in the same state, in the same quarter, on the same vehicle make-model-condition, and whose borrowers had similar incomes and credit scores. We document an increase in interest rates from captive lenders following the tariffs. Specifically, relative to non-captive lenders, captive lenders increased their average interest rates by 26 basis points, which represents a 10 percent increase in interest rates when compared to the pre-treatment captive mean of 252 basis points. We also examine whether captive lenders adjusted any other loan terms in response to the tariffs, but we find no economically significant changes in loan amounts, maturities, or loan-to-value ratios. Consistent with our results capturing the causal effect of the tariffs, we find that the increase in captive interest rates is primarily concentrated among more-exposed captive lenders whose manufacturers have multiple domestic production plants, and not less-exposed captive lenders with mostly foreign production. Moreover, we find no evidence of differential pre-trends for all our outcome variables.

What drives the observed increase in captive interest rates? The answer to this question is not immediately evident because our data contains information on originated loans but not loan offers or applications. One possible explanation is that our results do indeed capture tariff pass-through: in response to the metal tariffs, captive lenders charged inframarginal borrowers higher interest rates to offset higher production costs. However, another possible explanation is that our results capture changes in borrower composition. To explore whether the increase in captive interest rates reflects a change along the intensive or extensive lending margin, we examine how the tariffs affected the composition of captive borrowers. Consistent with our results capturing tariff pass-through along the intensive margin, we find no significant deterioration in captive borrowers' average incomes, credit scores, or future default rates following the announcement of the tariffs.[2]

In addition to raising captive interest rates, auto manufacturers might have responded to the tariffs by raising vehicle invoice prices, which are the prices they charge franchised auto dealers for new vehicles.[3] Auto dealers, in turn, might have passed on these higher invoice prices to consumers by raising sales prices. To explore the full impact of the tariffs on vehicle purchases, we examine both invoice and sales prices. The Regulation AB II data provides invoice price information for the vast majority of new vehicle loans and we supplement this with vehicle sales price data from the Texas Department of Motor Vehicles. We find both invoice and sales prices increased for makes and models of vehicles with greater exposure to the tariffs. Thus, the tariffs led to higher vehicle prices for both dealerships and consumers. Although captive lending rates also increased for these same makes and models of vehicles, they did not differentially increase within captive lenders across vehicle-level tariff exposures. Instead, interest rates rose across the board for captive lenders with greater firm-level exposure to the tariffs relative to less-exposed captive lenders, consistent with the idea that firms may spread cost shocks across multiple goods and business segments (Lamont 1997; Giroud and Mueller 2019; Flaaen, Hortacsu, and Tintelnot 2020).

An important metric for evaluating the economic incidence of tariffs is the pass-through rate to consumers (Flaaen, Hortacsu, and Tintelnot 2020). In our setting, this pass-through rate is made up

2. We also find that captive loan origination volumes decreased 6.7 percent in response to the tariffs (Table IA.1), but that this decline in loan origination volumes was not correlated with changes in observable borrower characteristics or future default rates (à la Argyle, Nadauld, and Palmer 2023).

3. Historically, dealerships paid manufacturers the invoice price. With the increased transparency of the internet squeezing dealership margins, some manufacturers started to provide "holdback" so that the amount paid by the dealer for a vehicle is slightly less than the invoice amount. This allows the dealership to make a profit even if selling a vehicle at the listed invoice amount. However, since holdback is consistent within a vehicle make, this is just semantics for our purposes and does not affect our analysis. The dealership pays the manufacturer a preset amount for the vehicle which does not vary whether the dealership sells it at cost or for a large profit.

of two major components: (i) interest rate pass-through and (ii) vehicle price pass-through. Using our estimates from above, we calculate that tariff pass-through via interest rates was almost two-thirds as large as tariff pass-through via vehicle prices. This implies that commonly used methods of measuring tariff incidence that focus solely on directly affected goods' prices would understate the economic impact on consumers in the auto industry by nearly 37 percent (see Section 4.6 for calculations). To the best of our knowledge, our paper is the first to provide evidence that auto manufacturers passed on a significant portion of tariff-related costs to consumers via higher financing costs.

Lastly, we explore cross-sectional heterogeneity in tariff pass-through. As there is substantial evidence of price dispersion in consumer credit (Argyle, Nadauld, and Palmer 2023; Bhutta, Fuster, and Hizmo 2024), we extend that literature to explore the role of borrower demand and market structure in determining tariff pass-through. We start by re-estimating our difference-in-differences model across three proxies for credit demand elasticities. We find that tariff pass-through via interest rates was higher for borrowers with lower incomes, lower credit scores, and smaller loan amounts, which prior studies have found to be associated with lower credit demand elasticities (Attanasio, Goldberg, and Kyriazidou 2008). Next, we show that tariff pass-through via interest rates was significantly higher in states with lower credit market competition, consistent with Weyl and Fabinger 2013. Combined, our results suggest that the metal tariffs had a disparate impact on consumers with less elastic credit demand and in areas with lower credit market competition. These findings are of practical importance because the tariffs were designed in-part to protect such individuals in the labor market (Amiti, Redding, and Weinstein 2020).

While our paper focuses specifically on tariff pass-through to auto loans, bundling a product sale with financing – and the associated potential for mismeasuring tariff incidence – can be found in many industries. For example, large residential homebuilders such as Pulte, Lennar, and D.R. Horton combine new home sales with captive mortgage financing (Gartenberg 2014). Each of these companies also mentioned the 2018 tariffs as a significant cost risk in their annual reports. More generally, many small businesses face credit constraints due to information frictions and their limited ability to make down payments (Eisfeldt and Rampini 2006; Rampini 2019; Graham 2022). These constraints might make some small businesses act as if they are less sensitive to loan prices than goods prices (Brennan, Miksimovic, and Zechner 1988). A large literature also documents that entrepreneurs, managers, and executives exhibit behavioral biases (Malmendier and Tate 2008; Landier and Thesmar 2008), with managers often using heuristics in capital budgeting decisions (Decaire and Sosyura 2023). Thus, it is plausible that behavioral biases or bounded rationality might make some managers more sensitive or attuned to goods prices than loan prices, facilitating tariff pass-through along the financing margin.[4] Consistent with this, numerous business-oriented durable goods firms with captive lending subsidiaries – including Caterpillar and Polaris – reported both higher input costs due to the 2018 tariffs alongside higher captive financing revenues.[5]

Our paper contributes to three strands of literature. First, there is a growing literature on the economic incidence of the 2018 Trump administration import tariffs. While Amiti, Redding, and Weinstein 2019 and Fajgelbaum et al. 2020 document evidence of complete pass-through of these tariffs to import and producer prices, few studies have found evidence of subsequent tariff pass-through to consumer prices (Cavallo et al. 2021). One explanation for this surprising pattern is that domestic firms and capital bore most of the costs. However, an alternative explanation is that measuring tariff incidence is complex, as both firms and consumers can adjust along several margins (Agrawal and Hoyt 2019). By illustrating that automobile manufacturers can use their captive lending subsidiaries to pass on higher costs from the tariffs, we highlight not only the impact of trade policy on consumer credit, but also the potential to understate tariff incidence by focusing solely on goods prices. Thus, our paper complements the recent finding in Flaaen, Hortacsu, and Tintelnot 2020 that tariffs can spill over to bundled and complementary goods, and it reinforces the findings of prior studies on the importance of vertical integration in cost pass-through (Hastings

4. Hortacsu and Puller 2008 document significant cross-sectional variation in financial sophistication across firm size, and there is growing evidence that both small and large firms may act in a boundedly rational manner (DellaVigna and Gentzkow 2019; Hortacsu et al. 2019; Hitsch, Hortacsu, and Lin 2021).

5. For instance, in its 2018 10-K, Caterpillar states, "Material costs were higher primarily due to increases in steel prices. The impact of the recently imposed tariffs on material costs was about $110 million during 2018... Financial Products' segment revenues were $3.729 billion, an increase of $186 million... The increase was primarily due to higher average financing rates."

2004; Hong and Li 2017).

Second, our paper adds to the literature on captive finance (Bodnaruk, O'Brien, and Simonov 2016; Stroebel 2016). To date, most studies in this literature have focused on understanding the reasons behind the existence of captive finance companies (Brennan, Miksimovic, and Zechner 1988). For example, Murfin and Pratt 2019 argue that captive finance units allow durable goods manufacturers to solve the Coase 1972 conjecture, and Barron, Chong, and Staten 2008 argue that they allow manufacturers to consummate sales with profitable but credit-rationed consumers. Showing that captive lenders provide an additional channel through which manufacturers can pass cost shocks on to consumers supports the notion that they serve a unique purpose relative to non-integrated lenders. Moreover, our results suggest that captive lenders allow manufacturers to shroud their price increases along margins where consumers are less price sensitive (Grunewald et al. 2023), consistent with the large literature on shrouded attributes and add-on pricing (Gabaix and Laibson 2006; Brown, Hossain, and Morgan 2010).

Third, our paper contributes to the literature on the transmission of economic shocks from firms to consumer credit. Within this literature, two recent papers examine the effects of market-wide and firm-specific funding shocks on captive auto loan terms. Benmelech, Meisenzahl, and Ramcharan 2017 documents that the collapse of the asset-backed commercial paper market during the Financial Crisis reduced the flow of credit to captive auto lenders and led to lower vehicle sales. Benneton, Mayordomo, and Paravisini 2022 finds that short-term increases in manufacturer credit default swap spreads are associated with worse captive auto loan terms and more relaxed lending standards.[6] In contrast to these papers, we examine how captive auto lenders responded to an input cost shock on the manufacturing side of their business. Our evidence suggests that captive lenders charged inframarginal borrowers higher loan prices in response to higher input costs, and that neither changes in lending standards nor concomitant changes in funding costs drive this result.

6. For several reasons, the "credit fire sale" channel described in Benneton, Mayordomo, and Paravisini 2022's study of the European used car market is not applicable to the U.S. auto market. Foremost, this channel requires that auto manufacturers have used car inventories that they can liquidate with the help of their captive lenders, but auto dealerships – and not auto manufacturers with captive lenders – are the owners of used car inventories in the U.S. Hence, a credit fire sale will be a cash-draining activity in our setting, whereas it is cash-generative in theirs.

Thus, when viewed alongside the above studies, our paper highlights how the strategic responses of integrated manufacturer-lenders may depend on the nature of the cost shock.

The remainder of the paper is organized as follows. Section 2 provides institutional background on the auto loan market and the 2018 metal tariffs. Section 3 describes the Regulation AB II data and presents our main sample. Section 4 documents the impact of tariffs on the auto loan market, and Section 5 examines heterogeneity in tariff incidence across consumers. Section 6 concludes.

2 Institutional Background

Evaluating the impact of trade policy on consumer credit requires an understanding of both the role of captive finance in the auto lending market as well as the impact of the 2018 metal tariffs on American auto manufacturers.

2.1 Captive Lenders and Auto Loans

Most auto manufacturers have their own captive lending subsidiaries whose purpose is to finance the sale of their products. Familiar examples in the United States include Ford Credit, GM Financial, and American Honda Finance Corporation. Captive lenders provide both retail financing to consumers and wholesale financing to franchised – i.e., manufacturer-affiliated – automobile dealerships. Retail financing consists of originating auto loans and leases, whereas wholesale financing consists of providing franchised dealerships with lines of credit to stock their new vehicle inventories or make capital improvements.[7] Retail financing tends to be the dominant form of lending at captive finance companies. For example, American Honda Finance Corporation had $73 billion in finance receivables in 2018, 92 percent of which were retail auto loans and leases.

Captive lenders have a significant presence in the auto loan market. Their 2019 market share of 26 percent was second to just banks at 31 percent and above both credit unions at 20 percent and

7. Various laws in the U.S. prohibit auto manufacturers from selling new cars directly to consumers. Hence, independently owned franchised auto dealers intermediate the new vehicle sales process. Franchised auto dealers have exclusive contracts to purchase new vehicles from their affiliated manufacturer at a uniform price and then sell these vehicles at various retail prices, such as the MSRP.

independent finance companies at 12 percent (Experian 2021). Among the different segments of the auto loan market, captive lenders tend to focus more on new vehicle lending (2019 market share of 54 percent) than used vehicle lending (2019 market share of 7 percent). Captive lenders also frequently provide subsidized financing for their manufacturer's own brands of new vehicles. For example, GM Financial sometimes offers zero percent financing or cash-back incentives to "well-qualified borrowers" for the purchase of certain new GM models. Non-captive lenders, such as banks and credit unions, typically are less willing to provide such forms of subsidized financing.

Captive lenders finance their operations using a combination of internal cash, unsecured debt, and securitizations. Around one-third of captive auto loans are securitized and the remaining two-thirds remain on lenders' balance sheets.[8] Even when auto loans are securitized, captive lenders still retain significant exposure to their performance. Securitized auto loans continue to be reported on captive lenders' balance sheets even after their sale, and captive lenders often hold significant stakes in their own asset-backed securities. Furthermore, in contrast to GSE-backed mortgages, most auto loans are well-seasoned prior to entering the securitization pool. For instance, the average time between the loan origination date and the securitization date is 14 months in our data.

More than 80 percent of auto loans are intermediated by auto dealerships (Cohen 2012 page 22; Romero 2017 page 13; Grunewald et al. 2023 page 8).[9] Figure 1 illustrates a standard auto purchase involving dealer-arranged financing. The auto dealer and consumer first negotiate the sales price for a specific vehicle. Next, the process moves from the dealership sales floor to the financing division. The dealership sends the consumer's credit application to multiple lenders, including their own brand's captive lender if they are a franchised dealer, through an online platform such as DealerTrack. Lenders may submit interest rate bids and other required loan terms to the dealer. The dealer then selects a bid (Jansen et al. 2021) and presents the loan offer to the consumer, oftentimes after adding a slight markup to its interest rate (Cohen 2012). The consumer does not

8. This number is based on the average share of finance receivables that were securitized in 2019 from the captive finance subsidiaries of five of the largest auto manufacturers: Ford, GM, Honda, Nissan, and Toyota.

9. This process – known as indirect, or dealer-arranged, financing – is described in additional detail in Cohen 2012 and Grunewald et al. 2023. The alternative is direct financing which is described in Argyle, Nadauld, and Palmer 2023. Only some non-captive lenders offer direct financing. All captive loans are dealer-intermediated.

see the full set of bids and the loan presented need not be the one with the lowest interest rate. Both captives and non-captives compensate dealers using a combination of fixed payments, markup payments, and other incentives through loyalty programs for selling their product (Grunewald et al. 2023). If the consumer and the dealer agree upon the loan's final terms, then the dealer originates the loan and sells it back to the winning lender. For the most part, this loan sale closes out the dealer's end of the transaction.

It is important to emphasize four aspects of the bidding process described above. First, since the consumer does not directly observe the solicited lenders' bids, they do not actively choose whether to use a captive or a non-captive lender. Instead, the consumer only observes the loan selected by the dealer (potentially with a markup), and they are often unaware of the identity of their lender until after they have agreed upon the final loan terms with the dealer (Consumer Financial Protection Bureau 2023).

Second, through this bidding process, captive lenders directly compete with non-captive lenders. For example, the 2018 10-K of Ally Financial (a non-captive lender) states, "captive automotive finance companies compete vigorously with us". Ford Credit (a wholly owned subsidiary of Ford Motor) lists "other automobile manufacturers' affiliated finance companies" as competitors in its 2018 10-K alongside banks and credit unions. Grunewald et al. 2023 finds that auto dealers solicit bids from 4.35 non-captive lenders, on average, for each auto loan transaction.

Third, there is no direct link between the dealer's choice of lender and the vehicle sales price paid by the consumer, conditional on the consumer already expressing interest in dealer-intermediated financing. The vehicle sales department is typically distinct from the financing department at most dealerships, and the vehicle sales price is usually negotiated between the consumer and the dealer well before the ultimate financing source is determined (Grunewald et al. 2023). Conversely, there is also no obvious incentive for captive lenders to condition their loan offers on the sales price that the consumer pays to the dealer. Captive lenders are the subsidiaries of auto manufacturers, not franchised dealers, and auto manufacturers' revenues come from selling new vehicles to franchised dealerships at (or near) the invoice price, not the sales price.

That said, there may be selection into dealer-intermediated financing which could, in theory, interact with the vehicle sales price. For example, more sophisticated consumers may be more likely to arrive at the dealership with preapproved loans from non-captive lenders, and dealers may be willing to accept a lower vehicle sales price in return for them using dealer-intermediated financing.[10] In Section 4.4.8, we show that our results are robust to limiting the treated and control groups to consumers who select into indirect loans. This is consistent with such selection issues not driving our results.

Fourth, since both captive and non-captive lenders compensate dealers for their business via a variety of incentives, it could be an issue for our analysis if the tariffs led auto manufacturers to differentially adjust their dealer incentives. Unfortunately, we cannot directly observe the incentives that auto manufacturers offer to dealers, and thus we must assume that the tariffs only affected dealers' loan decisions via changes in captive interest rates and not via changes in manufacturer incentives (i.e., an exclusion restriction). Several empirical points support this assumption. First, we find that our results are concentrated within the subset of more-exposed captive lenders whose interest rates increased more following the tariffs (see Section 4.2). We also document that dealers did not differentially change their loan markup policies for captive loans after the tariffs (Section 4.4.3) and that the types of borrowers ultimately receiving captive financing did not materially change either (Section 4.3).

2.2 The 2018 Metal Tariffs

As part of a broad expansion of protectionist trade policy, the Trump administration instructed the Department of Commerce in 2017 to investigate whether the amount of steel and aluminum being imported into the United States posed a threat to national security.[11] Commerce's report,

10. Since dealer profits decrease one-for-one with concessions on vehicle prices but increase less than one-for-one with loan markups (Grunewald et al. 2023), such an exchange is only profitable to the dealer if they can get the consumer to agree to a sufficiently costly loan, potentially from a non-captive lender.

11. The United States imports around 35 percent of the steel it consumes and 90 percent of its aluminum. The top importers of steel into the United States are Canada (20%), the European Union (20%), Brazil (15%), South Korea (10%), Mexico (10%), and Russia (10%) (Department of Commerce 2018b). The top importers of aluminum into the United States are Canada (40%), Russia (10%), the United Arab Emirates (10%), China (10%), and the European

submitted in January of 2018 and made public on February 16 of that year, recommended a range of possible tariff options to boost domestic metal production. On March 1, 2018, President Trump followed Commerce's recommendation and announced a 25 percent tariff on steel imports and a 10 percent tariff on aluminum imports.[12] One week later, he signed the order to take effect in 15 days. While a limited number of major trading partners such as Canada, Mexico, and the European Union were originally excluded from the tariffs, their exemption ended on May 31, 2018.

Domestic markets immediately reacted to the public release of the Department of Commerce report, with aluminum and steel futures prices jumping, respectively, 2 percent and 1 percent. Over the first quarter of 2018, Bureau of Labor Statistics PPI Commodity data reported price increases of more than 7 percent in both the iron and steel and the steel mill products categories, while aluminum prices also rose around 3 percent (Figure 2). Steel and aluminium prices continued to rise throughout the year with the expansion of the metal tariffs to Canada, Mexico, and the European Union (Parkin and Hodari 2018), as well as from strategic price increases by domestic producers (Amiti, Redding, and Weinstein 2019).[13] By December 2018, PPI steel prices (which reflect actual prices paid) had settled at approximately 20 percent higher than they were in January 2018.

2.3 The Impact of the Tariffs on Auto Manufacturers

Auto manufacturers are large consumers of steel and aluminum – through both their purchases of raw materials as well as their auto parts suppliers – and, thus, were exposed to the unexpected increase in metal prices across multiple dimensions of their supply chain. This was apparent in

Union (5%) (Department of Commerce 2018a). Despite China's status as the top producer of steel in the world, the United States imports minimal steel from it because of prior anti-dumping trade laws (Brown 2018).

12. Given that the Trump administration did not seek formal approval from the World Trade Organization before imposing the tariffs, most market participants viewed them as a surprise. The March 1, 2018 edition of the New York Times reads: "In a hastily arranged meeting with industry executives that stunned many inside the West Wing, Mr. Trump said he would formally sign the trade measures next week...against the wishes of Mr. Trump's pro-trade advisers." Amiti, Redding, and Weinstein 2020 and Fajgelbaum et al. 2020 find no evidence that the tariffs were anticipated based on import price patterns from a range of affected industries.

13. Many models of imperfect competition predict that firms will strategically raise their prices when their competitors experience a cost shock. Consistent with this prediction, Fajgelbaum et al. 2020 find that the 2018 tariffs reallocated domestic demand onto U.S.-made goods, such as domestic steel, which insulated domestic producers from foreign competition and allowed them to raise their prices.

both the stock market and their corporate announcements.[14] The share prices of domestic auto manufacturers dropped upon the formal announcement of the tariffs on March 1, 2018 (Carey and Banerjee 2018). By summer 2018, the same auto companies cited the tariffs as they revised their earnings forecasts downward (Carey and Klayman 2018). Many firms, including those that primarily relied upon domestic aluminum and steel, specifically discussed higher commodity prices and the impact of the metal tariffs in their annual reports. Ford's 2018 10-K reported, "Tariffs on steel...had a direct negative impact on costs...The $2 billion year-over-year decline...was primarily explained by higher commodities...driven by metals, primarily steel." Ford CEO James Hackett stated, "From Ford's perspective the metals tariffs took about $1 billion in profit from us." Rick Schostek, executive vice president for Honda North America, testified to the Senate Finance Committee in September 2018, "So, while we're paying relatively little in the way of tariffs on steel, the price of domestic steel has increased as a result of the tariff, saddling us with hundreds of millions of dollars in new, unplanned cost".[15]

Firms have a variety of tactics at their disposal to deal with an unexpected cost shock such as the tariffs. Speaking to analysts, GM CFO Chuck Stevens stated that his firm's options would include negotiating with suppliers, raising prices, and cost cutting (Carey and Klayman 2018). As noted earlier, the existing literature has documented that producer prices increased in response to the 2018 tariffs (Amiti, Redding, and Weinstein 2019) but the evidence is mixed regarding the degree to which consumer prices were affected (Cavallo et al. 2021). Automobile manufacturers, which can

14. On March 2, 2018, the day after the Trump administration announced the metal tariffs, numerous car manufacturers with significant U.S. production issued public rebukes of the new policy. Honda announced that "imprudent tariffs imposed on imported steel and aluminum would raise prices...causing an unnecessary financial burden on our customers". Toyota stated the "steel and aluminum tariffs will...substantially raise costs and therefore prices of cars and trucks sold in America." See Brown 2018, Shepardson 2018, and Zhao 2018.

15. Except for a small number of Chinese-made vehicles such as the Buick Envision, neither imported vehicles nor auto parts were subject to new tariffs during this period (Brown 2018). However, in retaliation for the broad-based import tariffs on Chinese-made goods that the U.S. imposed in mid-2018, China raised their import tariffs on U.S.-made vehicles from 25 percent to 40 percent. We note that these tariffs on U.S.-made vehicles had a negligible impact on U.S. auto companies because most of the vehicles that these companies sell in China are manufactured in China (Roh 2019). (Nevertheless, in Table IA.2, we show that our results are robust to examining the period prior to the retaliatory tariffs from China.) Several other major trading partners including Canada, Mexico, and the European Union also imposed retaliatory tariffs on U.S. exports, but none of these tariffs targeted the auto sector. Although some exclusions and exemptions to the tariffs were granted, this was done slowly and inconsistently. As of December 2018, over 60 percent of the over 50,000 tariff exclusion requests were still pending. Further, metals PPI prices rose during this period and auto companies highlighted significant cost increases well into 2018.

adjust both the invoice prices of their new vehicles as well as the loan terms at which these vehicles are financed, offer an interesting venue to revisit the measurement of tariff price incidence.

Specifically, Figure 3 highlights two of the main ways that auto manufacturers could pass on tariff costs to consumers. First, auto manufacturers could charge franchised dealerships more for new vehicles. Dealerships, in turn, then could pass on higher invoice prices to consumers via higher sales prices.[16] Second, through their captive lending subsidiaries, auto manufacturers could pass on tariff costs to consumers via higher interest rates or worse loan terms. While trade economists have documented tariff pass-through affecting the price of goods in many contexts, empirical evidence of the financing costs channel remains scarce.

Anecdotal evidence suggests that auto manufacturers may have passed on at least some portion of the tariffs through their captive lenders. For example, Figure 4 displays GM's revenues and profits, where revenues and profits are split between GM's vehicle sales segment and its captive financing segment, GM Financial, around the tariffs. GM's vehicle sales segment experienced a significant decline in profits during the year the tariffs were imposed, and this was primarily due to a sharp increase in costs rather than a decline in revenues.[17] In contrast, both revenues and profits rose at GM Financial in 2018. Similarly, Ford also reported a sharp increase in costs from the tariffs alongside higher captive financing revenues.[18]

16. In addition to raising their invoice prices, auto manufacturers could pass on tariff costs by reducing the amount of cash rebates they provide to consumers/dealers (Busse, Silva-Risso, and Zettelmeyer 2006). Although we do not explicitly observe cash rebates in our data, we implicitly capture their effects when we examine sales prices.

17. From GM's 2018 10-K: "We continue to experience higher commodity costs and anticipate higher costs associated with tariffs...The most significant element of our Automotive and other cost of sales is material cost which makes up approximately two-thirds of the total amount...".

18. Ford's 2018 10-K: "Tariffs on steel and aluminum coming into the United States in 2018 had a direct negative impact on costs... Ford Credit generated a full year 2018 EBT of $2.6 billion, $317 million higher than a year ago, and its best EBT in eight years."

3 Data and Sample Selection

3.1 Data

Our auto loan data comes from Regulation AB II. Under Regulation AB II, issuers of public auto loan asset-backed securities are required to report loan-level information to the Securities and Exchange Commission on a monthly basis.[19] The reported information includes loan, vehicle, and borrower characteristics as of each loan's origination date, as well as loan performance histories over the entire life of each loan. Along with variables that are commonly found in most consumer credit datasets such as original loan amounts (i.e., loan principals) and maturities, the Regulation AB II data also contains several unique variables that are crucial to have in our particular setting. For example, the Regulation AB II data contains detailed information on the vehicle being financed, including whether it is a new or used vehicle, its make-model-year, and its assessed value. (The assessed value is generally the invoice price for new vehicles and the Kelley Blue Book value for used vehicles.) Having this information allows us to hold the choice of vehicle fixed when measuring tariff pass-through, which is important to do because (i) consumers might adjust their vehicle choices in response to changes in loan terms (Argyle, Nadauld, and Palmer 2023) and (ii) the choice of vehicle often influences the offered interest rate (Argyle et al. 2021). Another unique feature of the Regulation AB II data is that it identifies loans with subsidized financing, also known as subvented loans. This feature of the data allows us to investigate the impact of the tariffs on both the complete universe of auto loans as well as on those without subventions.[20]

We collect the loan-level data from the Securities and Exchange Commission's website. As of May 2020, there are over 11 million unique loans and 183 million loan-months in the data. The

19. While all public auto loan ABS issued after November 2016 are subject to the Regulation AB II reporting requirements, private placements and public auto loan ABS issued prior to November 2016 are exempt. We note that ABS issuers can include seasoned loans in their offerings, and hence the oldest loan in the Regulation AB II data was originated before 2016 (specifically, 2010). See Sweet 2015 and Neilson et al. 2020 for more details.

20. For several reasons, it is important to demonstrate that our results hold within both the full sample of auto loans and the subsample of auto loans without subventions. First, because subvented loans are more common among captive lenders than non-captive lenders, there could be seasonal variation in subventions that is specific to captive lenders that could compromise our identification (e.g., December sales events). Second, because subventions are often tied to particular models of vehicles, detecting demand-side responses to higher financing costs will be less feasible on the full sample than the non-subvented subsample.

loans come from 181 distinct asset-backed securities and 19 lenders (11 captive lenders and 8 non-captive lenders). All the major captive auto lenders are in the data, along with five of the top ten non-captives.[21] During our sample period of January 2017 to December 2018, the Regulation AB II data contains around 8 percent of all open auto loans in the United States. These loans represent around 30 percent of the overall auto loan portfolios of the 19 included lenders.[22]

As mentioned above, the Regulation AB II data contains the invoice price for new vehicles, which provides us with one way of measuring the relative importance of pass-through along the loan and vehicle price margins. Yet, a drawback of the Regulation AB II data is that it does not include information on the vehicle sales price, which is the more relevant price for determining pass-through to consumers.[23] To measure pass-through to final sales prices, we supplement the Regulation AB II data with vehicle sales price data from the Texas Department of Motor Vehicles (Hoekstra, Puller, and West 2017; Hankins, Liu, and Sosyura 2025). The Texas data reports the sales prices of 1,819,498 new and 2,105,938 used vehicles that were sold in the state of Texas between 2017 and 2018. While the Texas data contains detailed vehicle characteristics such as the make-model-year and vehicle identification number (VIN), it does not contain any borrower or loan characteristics. Thus, the Texas data only allows us to estimate the effect of the tariffs on new vehicles' sales prices as a whole, and not the differential effect across captive-financed and non-captive-financed vehicles. It also should be noted that neither the Regulation AB II data nor the Texas data contain information on the dealerships where consumers purchased their vehicles. Among other things, this prevents us from examining whether the impact of the tariffs varied across dealerships based on

21. Of the top ten non-captive auto lenders in the U.S., the ones that are not in the Regulation AB II data are Chase (3), Wells Fargo (4), Bank of America (5), Credit Acceptance (8), and TD (10). These lenders are not in the data because they hold most of their auto loans directly on their balance sheets instead of issuing public auto loan ABS. (See Footnote 24 for a comparison of these omitted lenders to the ones in the data.) We note that the auto loan market is fragmented and consists of thousands of small banks, credit unions, and independent finance companies that compete against captives and large banks for market share. However, these smaller lenders do not utilize public securitization markets and hence do not appear in the Regulation AB II data.

22. In the U.S., there is around \$600 billion of auto loans and leases originated per annum (Schmidt and Zhang 2020). Around \$100 billion of these originations are packaged into ABS, and around half of ABS issuances are public offerings (Klee and Shin 2020). Hence, in the long-run, we should expect the Regulation AB II data to contain around 8 percent (= \$50 billion / \$600 billion) of open auto loans at any point in time, which is what we find.

23. Unfortunately, the Regulation AB II data does not contain information on down payment amounts, which prevents us from using loan amounts to back out sales prices.

their historical reliance upon captive financing.

3.1.1 Is the Regulation AB II Data Representative?

We briefly compare the Regulation AB II data to population credit bureau data to determine whether the former is representative. Our credit bureau data is mostly similar to the New York Fed's Consumer Credit Panel except for two key differences. First, our credit bureau data covers the entire U.S. population instead of covering a representative random sample of consumers in the U.S. Second, our credit bureau data contains more fields than the Consumer Credit Panel. One particularly important field is lender names, which allows us to compare each lender's securitized auto loan portfolio to their overall auto loan portfolio (the latter of which contains both securitized and non-securitized loans).

Table IA.3 presents lender-level average loan characteristics across the Regulation AB II data and the credit bureau data. Average loan characteristics are similar in both datasets, indicating that selection during the securitization process should not be a major concern in our setting. Later, in Section 4.4.7, we also show that the 19 lenders in the data did not change their securitization practices in response to the tariffs.

In addition to being generally representative of the included lenders' auto loan portfolios, the Regulation AB II data is also representative of the broader population of auto loans in the U.S. Specifically, Momeni and Sovich 2022 show that average loan amounts, balances, maturities, scheduled monthly payments, and default rates are similar in the Regulation AB II data and the population credit bureau data. However, average credit scores and household incomes are slightly higher in the Regulation AB II data than in the population, which is mostly due to the fact that the composition of lenders is different across these data sources. That is, while the Regulation AB II data primarily consists of auto loans from captive lenders and large banks that publicly securitize at least some portion of their loans, the population data also includes thousands of small banks, credit unions, and independent finance companies that do not access public securitization markets and often serve a slightly riskier clientele. Given that our paper focuses on the effect of tariffs on captive lenders (the overwhelming majority of which are in the Regulation AB II data), we do not believe that the absence of small banks, credit unions, and independent finance companies from the Regulation AB II data should materially affect the external validity of our results.[24]

3.2 Sample

We restrict our sample to auto loans that were originated within 12 months of the January 2018 treatment date (i.e., between January 2017 to December 2018).[25] We also require that loans have the following fields populated in the Regulation AB II data: interest rate, loan amount, loan maturity, scheduled monthly payment, vehicle condition (i.e., new or used), make-model-year, assessed vehicle value, borrower credit score, and borrower income. We remove loans with credit scores below 620, incomes above $250,000, vehicle values above $100,000, vehicle model years before 2011, and interest rates above 30 percent (Argyle, Nadauld, and Palmer 2020). In addition, we follow Benneton, Mayordomo, and Paravisini 2022 and restrict our sample to loans with (origination) loan-to-value ratios between 0.10 and 1.20. We winsorize interest rates, loan amounts, loan maturities, and assessed vehicle values at the one percent tails. As shown later in Section 4.4.9, our results are robust to relaxing or tightening these sample filters.

For various reasons, we remove 5 of the 19 Regulation AB II lenders from our sample (23 percent of loans). First, we remove Capital One and California Republic because these lenders do not have public auto loan securitizations during both the pre- and post-treatment periods. Second, we remove Harley Davidson because no other lender in our sample finances new motorcycles. Third, we remove Hyundai because it has its own integrated steel manufacturer.[26] Finally, we remove Nissan because it issued a large vehicle recall in October 2017 right before the tariffs were announced. As shown

24. One potential concern could be that the large non-captive lenders in the Regulation AB II data are different than the large non-captive lenders that are not in the data in important ways (i.e., we are not controlling for the right trend). However, when we compare these lenders across our two data sources in Table IA.4, we find no significant differences in their average loan characteristics. We revisit this issue in Footnote 46.

25. Our choice of treatment date is conservative as it reflects the date of the Department of Commerce's initial recommendation to impose the tariffs. We find similar results if we instead use February 2018 or March 2018 as the treatment date. See Table IA.5.

26. While having its own integrated steel manufacturer may have helped Hyundai hedge against direct cost increases from the tariffs, Hyundai still had indirect exposure to the tariffs through its suppliers' costs. Hence, Hyundai does not serve as an ideal placebo in our setting.

in Section 4.4.9, our results are robust to reincluding these five lenders in the sample.

Our final sample consists of 1,973,639 auto loans from 127 distinct asset-backed securities and 14 lenders. Figure 5 plots the distribution of these loans across lenders. Loans from captive lenders (BMW, Ford, GM Financial-AmeriCredit, Honda, Mercedes-Benz, Toyota, and Volkswagen) make up 61 percent of the sample. Loans from non-captive lenders (Ally Bank, CarMax, Fifth Third, Santander, USAA, and World Omni) make up the remaining 39 percent.[27] Table 1 presents descriptive statistics as of each loan's origination dates. The average loan in our sample has an interest rate of 4.39 percent, a maturity of 66 months, a scheduled monthly payment of $445, and an initial principal of $25,619. Sixty-five percent of loans are used to finance new vehicles, and the average loan-to-value ratio is 0.89. The average borrower in our sample has a credit score of 748 and a household income of $88,341. The unconditional 24-month default rate is 1.20 percent.

The right-most columns in Table 1 compare loans from captive (i.e., treated) lenders to non-captive (i.e., control) lenders. For these comparisons, we restrict the sample to loans originated prior to the treatment date (982,095 loans). There are several noticeable differences between loans from captive and non-captive lenders. Captive loans have higher average initial principals than non-captive loans ($26,914 versus $22,256), as well as lower maturities (66 months versus 68 months), lower interest rates (2.52 percent versus 6.30 percent), and lower loan-to-value ratios (0.89 versus 0.92). Captive lenders also finance a larger share of new vehicles than non-captive lenders (81 percent versus 39 percent), and the average captive borrower has a higher credit score (756 versus 730) and higher household income ($89,979 versus $81,537) than the average non-captive borrower.

Although there are observable time-invariant differences between captive and non-captive loans, our baseline difference-in-differences model in Section 4 removes most of them through the inclusion of various lender, vehicle, and borrower-characteristic fixed effects (e.g., income bin and credit score bin fixed effects). Indeed, as shown later in Figures 6 and 8, we find no evidence of differential

27. In 1981, World Omni Financial created a dedicated subsidiary, Southeast Toyota Finance, to help Toyota establish a foothold in the Southeast United States. We note that Southeast Toyota Finance is distinct from the official captive lender of Toyota, which is called Toyota Motor Credit (see Benmelech, Meisenzahl, and Ramcharan 2017). In its ABS prospectuses, World Omni describes itself as "...a diversified company offering a broad range of products and services to automotive dealers, consumers, and lenders." As shown in Table IA.6, our results are also robust to excluding World Omni Financial from the sample.

pre-trends across captive and non-captive loans after conditioning on our chosen set of fixed effects. Thus, while captive and non-captive loans appear to be different in terms of *levels* prior to treatment, their pre-treatment *changes* are indistinguishable from one another. This is important because the standard falsification test of the parallel trends assumption requires demonstrating similar pre-treatment changes and not levels per se.[28]

One pre-treatment level difference worth emphasizing is the large gap between captive and non-captive interest rates. This gap persists even after conditioning on vehicle and borrower characteristics, and after removing loans with subsidized interest rates from the sample. To highlight this difference, Table 2 reports coefficient estimates from the following regression model:

$$y_{i,l,v,s,w,c,t} = \alpha + \Gamma \cdot \text{Treated}_l + \delta_{v,t} + \delta_{s,t} + \delta_{w,t} + \delta_{c,t} + \varepsilon_{i,l,v,s,w,c,t}, \tag{1}$$

where the outcome variable, $y_{i,l,v,s,w,c,t}$, is the interest rate of loan i originated in quarter t, and the indicator variable Treated_l is equal to one if lender l is a captive lender, and zero otherwise. The model includes separate origination quarter fixed effects for each state (s), $25,000 income bin ($w$), 10-point credit score bin (c), and vehicle make-model-condition combination (v) (e.g., new versus used Honda Accord).[29] The coefficient of interest, Γ, captures the pre-treatment average difference in interest rates between captive and non-captive loans that were originated in the same quarter for the same vehicle to similar borrowers. The estimation period is January 2017 to December 2017 (i.e., the pre-treatment period), and standard errors are clustered at the lender level.

Column 1 in Table 2 reports the results of the estimation.[30] Conditional on vehicle and borrower characteristics, the pre-treatment average interest rate for captive loans is 190 basis points lower

28. Although our fixed effects help control for both level differences between captive and non-captive lenders and their common responses to various shocks, it is still possible that differential responses to common shocks (i.e., differential factor loadings) could threaten our identification. For example, captive lenders might be more sensitive to changes in risk-free interest rates than non-captive lenders due to differences in their funding structures, which in turn might have caused captive lenders to raise their interest rates more than non-captive lenders when risk-free rates rose in 2018. We address this particular concern and several others in Sections 4.2 and 4.4.

29. By including vehicle make-model-condition fixed effects in most of our specifications, we are limiting our identifying variation to vehicles that have both captive and non-captive lending options. Loans on such vehicles account for 98 percent of our sample, and the average characteristics of these loans are similar to those reported in Table 1.

30. See Section A.1 in Appendix A for a discussion of several additional moments of the loan price comparison, as well as additional details about the in-sample market shares of captive and non-captive lenders.

than for non-captive loans. Some of this difference can be attributed to captive lenders providing subsidized financing on specific vehicle models. However, even after we remove subsidized loans in column 2, the pre-treatment average interest rate for captive loans is still 98 basis points lower than for non-captive loans, albeit the difference is marginally significant (t = -1.73). Among other explanations, this persistent gap could be due to institutional differences between captive and non-captive lenders. For example, due to their relationships with their manufacturers, captive lenders might be able to tolerate lower profit margins on financing (Bodnaruk, O'Brien, and Simonov 2016), have higher salvage values in the case of default (Murfin and Pratt 2019), or be able to limit dealerships to smaller interest rate markups (Cohen 2012). It is worth noting that the gap between captive and non-captive loan rates is not simply a feature of the auto loan market. Stroebel 2016 documents a similar pattern in the mortgage market and attributes lower average interest rates on captive-financed mortgages to adverse selection surrounding collateral values.

4 Tariffs and the Provision of Auto Credit

Next, we explore how the metal tariffs impacted the captive auto loan market, both with respect to auto loan terms as well as the composition of borrowers. We find that although captive interest rates increased following the tariffs, there were no significant changes in other captive loan terms or the composition of captive borrowers.

4.1 Loan Terms

4.1.1 Interest Rates

We begin by estimating the effect of the tariffs on the interest rates of captive auto loans. The regression model is:

$$y_{i,l,v,s,w,c,t} = \alpha + \Gamma \cdot \text{Treated}_l \cdot \text{Post}_t + \delta_l + \delta_{v,t} + \delta_{s,t} + \delta_{w,t} + \delta_{c,t} + \varepsilon_{i,l,v,s,w,c,t}, \qquad (2)$$

where the outcome variable is the interest rate of loan i originated in quarter t. As in Equation 1, the indicator variable Treated_l is equal to one if lender l is a captive lender, and zero otherwise. The variable Post_t is equal to one for all quarters t after the treatment date (i.e., January 2018 onward), and zero otherwise. In our baseline specification, we include lender fixed effects (δ_l) and vehicle make-model-condition × origination quarter fixed effects ($\delta_{v,t}$) to ensure that the treatment effect (Γ) is estimated using within-lender variation after netting out common vehicle-level shocks.[31] We also include separate origination quarter fixed effects for each state ($\delta_{s,t}$), \$25,000 income bin ($\delta_{w,t}$), and 10-point credit score bin ($\delta_{c,t}$) to control for common borrower-level shocks across captive and non-captive lenders.[32] The coefficient of interest, Γ, measures the conditional average change in interest rates for captive auto loans relative to non-captive auto loans for the same vehicles and similar borrowers. The estimation sample consists of auto loans originated between January 2017 and December 2018, and standard errors are clustered at the lender level to match the assignment of treatment.

Panel A in Table 3 reports the coefficient estimates from Equation 2. Relative to non-captive loans, captive loans experienced a 26 basis point increase in their average interest rates following the announcement of the tariffs. This estimate represents a 10 percent increase in interest rates when compared to the pre-treatment mean of 252 basis points for captive loans, and it implies an average present value increase in total loan payments of \$179 (0.66 percent of the pre-treatment average captive loan amount).[33] Panel B reports the coefficient estimates from Equation 2 after we

31. These fixed effects allow us to hold the choice of vehicle fixed when measuring the effect of the tariffs, which is important to do because prior studies have shown that: (i) consumers might adjust their vehicle choices in response to changes in loan terms (Argyle, Nadauld, and Palmer 2023), and (ii) failing to control for such demand-side purchasing responses can lead to biased estimates of the pass-through rate (Gulati, McAuslan, and Sallee 2017). In Section A.3 in Appendix A, we relax these fixed effects to examine the scope of demand-side purchasing responses. We note that similar specifications to ours can be found in Argyle et al. 2021, Benneton, Mayordomo, and Paravisini 2022, and Argyle, Nadauld, and Palmer 2023.

32. It is important to include income bin × origination quarter fixed effects and credit score bin × origination quarter fixed effects because captive and non-captive lenders are not balanced along these dimensions prior to treatment. Otherwise, in their absence, our estimates would be susceptible to biases arising from differential time shocks across the income and credit score distributions. We note that we have ample observations for each of our fixed effects. Specifically, on average, there are 311 observations within each vehicle make-model-condition × origination quarter cell, 4,837 observations within each state × origination quarter cell, 24,670 observations within each income bin × origination quarter cell, and 10,279 observations within each credit score bin × origination quarter cell.

33. Discounting at 5 percent, for a pre-treatment average captive loan with a \$26,914 principal and a 66-month maturity, a 26 basis point increase in interest rates from 2.52 percent to 2.78 percent corresponds to a present value

remove subsidized loans from the sample. Similar to Panel A, we find that captive interest rates increased by 29 basis points, on average, following the tariffs.

Table IA.7 re-estimates Equation 2 separately for new and used vehicles and documents similar effects on both types of vehicles. Thus, the increase in captive interest rates does not simply reflect fewer marketing promotions for new vehicles but is pervasive across the auto loan market. Our finding that captive lending rates increased for both directly affected goods (i.e., new cars) and indirectly affected goods (i.e., used cars) buttresses the notion that firms spread the effects of cost shocks across multiple goods.

Given that the tariffs became more binding over time, the pooled coefficient estimate in Table 3 might understate their eventual impact on loan prices. Thus, to examine how the effect of the tariffs evolved during our sample period, we estimate the following regression model:

$$y_{i,l,v,s,w,c,t} = \alpha + \sum_{\tau=-4}^{3} \Gamma_\tau \cdot \text{Treated}_l \cdot \text{D}_{t,\tau} + \delta_l + \delta_{v,t} + \delta_{s,t} + \delta_{w,t} + \delta_{c,t} + \varepsilon_{i,l,v,s,w,c,t}, \tag{3}$$

where $\text{D}_{t,\tau}$ is equal to one whenever quarter t is τ quarters from the treatment date, and zero otherwise. When estimating the model, we exclude the quarter prior to treatment ($\tau = -1$) as the reference quarter. Therefore, the Γ_τ coefficient captures the average difference in interest rates between captive and non-captive loans in quarter τ relative to the average difference in the quarter prior to treatment.

Figure 6 plots the coefficient estimates from Equation 3. Given that there is seasonal variation in subsidized loan offers specific to captive lenders (e.g., December sales events), we focus on the subsample of auto loans without subventions. We find that captive interest rates started to increase within one quarter of the treatment date and continued to rise alongside metal prices throughout the rest of the post-treatment period. The terminal coefficient estimate for the fourth quarter of 2018 is 48 basis points, which is almost double our pooled coefficient estimate of 26 basis points

increase in total loan payments of \$178.62. See Argyle, Nadauld, and Palmer 2023 for a similar calculation. We also arrive at a similar number if we instead estimate Equation 2 with the log monthly loan payment as the outcome variable and then use the resulting difference-in-differences estimate to calculate the present value increase in total loan payments for an average captive loan. For reference, the estimated increase in log monthly loan payments is 1.0 percent ($t = 1.84$) for all captive loans and 1.5 percent ($t = 3.38$) for non-subvented captive loans.

from Table 3. Consistent with the parallel trends assumption being satisfied in our setting, we find no economically significant evidence of differential pre-trends across captive and non-captive loans. Among other concerns, this finding helps rule out that concomitant seasonal demand shocks in the auto loan market – such as higher subprime loan demand during tax rebate season – are driving our results (Adams, Einav, and Levin 2009). In sum, both the original and dynamic specifications suggest captive lenders increased their interest rates in response to the tariffs.

4.1.2 Non-Price Loan Terms

Next, we examine whether the tariffs impacted non-price loan terms. Columns 2 through 4 in Table 3 report coefficient estimates from versions of Equation 2 where the outcome variable is either the log loan amount, log loan maturity, or loan-to-value ratio. We find no consistent evidence that captive lenders adjusted their non-price loan terms in response to the tariffs. For example, while some of the coefficient estimates in Panel A are statistically significant, their economic magnitudes are relatively small compared to the effect of the tariffs on interest rates. Moreover, these estimates either flip signs, lose their statistical significance, or become even smaller once we drop subsidized loans from the sample in Panel B.

Figure 6 also plots the dynamics of these non-price coefficient estimates. We again find no economically meaningful evidence of differential pre-trends. Overall, our results suggest that captive lenders primarily responded to the tariffs by raising their interest rates. This choice is intuitively consistent with profit maximization, as prior studies such as Attanasio, Goldberg, and Kyriazidou 2008 have shown that auto loan demand is less sensitive to interest rates than non-price loan terms like maturities.

4.2 Validity of Non-Captive Loans as Counterfactuals

While non-captive lenders appear to be a natural control group for captive lenders, there are potential issues with using them as a counterfactual. One concern is that the higher interest rates documented might be capturing the effects of a time-varying, captive-specific omitted variable that coincides with the tariffs, and not the effects of the tariffs per se. Alternatively, non-captives may exhibit differential sensitivities to common shocks such as the rising interest rate environment of 2018. Lastly, due to competitive interactions between captive and non-captive lenders, the tariffs may have had spillover effects. This section presents three alternative specifications to validate the use of non-captive lenders as a control group. Later, in Section 4.6, we discuss the potential spillover magnitudes in our setting and adjust our difference-in-differences estimates for the size of the implied attenuation bias.

To alleviate concerns about captive-specific correlated omitted variables, we estimate an alternative difference-in-differences specification that uses variation in tariff exposure across captive lenders. The basic idea behind this specification is that while some of our captive lenders have large domestic manufacturing operations – and hence significant exposure to the tariffs – others produce most of their vehicles outside the United States.[34] Therefore, if our setting captures the causal effect of the tariffs and not an omitted variable, then we should expect to find stronger effects among captive lenders with larger domestic manufacturing operations and greater exposure to the tariffs.

We start by splitting our sample of captive lenders into two exposure groups. Captives whose manufacturers have two-or-more domestic production plants are considered more exposed to the tariffs, whereas captives whose manufacturers have one or zero domestic production plants are considered to be less exposed. The captive lenders in the more-exposed group are Ford, GM-AmeriCredit, Honda, and Toyota, while the captive lenders in the less-exposed group are BMW, Mercedes-Benz, and Volkswagen. We note that we would arrive at the same classification if we instead calculated tariff exposure based on the fraction of each lender's vehicles assembled in North America from the American Automobile Labeling Act, and then split our sample at the median level of tariff exposure.

Given the above classification, we separately estimate Equation 2 for the subsamples of more-exposed and less-exposed captive lenders, where the control group for each subsample consists of

34. Except for some Chinese-made vehicles, imported vehicles were not subject to new tariffs during this period.

all loans from non-captive lenders. Columns 1 and 2 in Table 4 report the coefficient estimates from the models. Consistent with our results capturing the causal effect of the tariffs, we find that the increase in captive interest rates is primarily concentrated among more-exposed captive lenders (Γ =30 basis points; t = 3.37). Less-exposed captive lenders do not experience a significant increase in their average interest rates as a result of the tariffs (Γ = -18 basis points; t = -1.33). Panel B repeats the same estimation for the subsample of loans without subventions and documents a similar pattern. The concentration of our results among more-exposed captive lenders suggests that time-varying, captive-specific omitted variables do not drive the observed increase in captive interest rates.[35]

While the above results are encouraging, many of the non-captive lenders in the control group are financial institutions which may have had differential sensitivities to the rise in interest rates during 2018. To address this concern, we re-estimate Equation 2 after controlling for the interaction of the treatment indicator variable with the change in the Fed Funds rate as well as the change in the one, five, and ten-year Treasuries. In both column 3 in Table 4 as well as in Table IA.9 (which presents each interaction separately), we continue to find that captive interest rates increased relative to non-captive rates following the tariffs. Moreover, we find that the magnitude of the coefficient estimates does not vary much with these additional controls.

Lastly, we compare captive loans to only CarMax loans, omitting all other non-captive lenders from the control group. CarMax offers several advantages for validating our results. First, not only does CarMax not manufacture vehicles and, thus, was not directly affected by the metal tariffs, but it is also not primarily a financial institution like the other non-captives, thereby mitigating concerns of differential exposure to interest rate movements. Second, CarMax does not participate

35. As shown in Table IA.8, we find no differential within-lender changes in captive interest rates across makes and models of vehicles that are primarily domestic-made versus foreign-made. That is, while interest rates rose more sharply for more-exposed captive lenders than less-exposed captive lenders (Table 4), more-exposed lenders broadly spread their interest rate increases across makes and models of vehicles with varying degrees of tariff exposure. This spreading of cost increases is consistent with our prior result that captive interest rates increased for both new and used vehicles, as well as with industry responses to other tariff shocks. For example, a 2019 Wall Street Journal article on retailers adjusting to higher tariffs on Chinese merchandise noted, "[Home Depot] said it plans to manage the cost increases by buying more volume at lower prices from some vendors and by spreading price increases across a wider swath of items to limit the impact on sales" (Kapner and Nassauer 2019).

in DealerTrack or the other auto loan intermediation services and only provides loans to individuals purchasing vehicles at CarMax. That means CarMax loans do not compete with captive loans in the indirect lending market. Third and finally, unlike the other non-captive lenders, CarMax both sells and finances vehicles. If there is a possible interaction between the purchase price and financing terms, then it should also apply to CarMax, as the bundling of buying and financing a vehicle is similar at dealerships and CarMax.[36] Therefore, we believe CarMax is a valid control which allows us to have more confidence in our coefficient estimates. (CarMax, however, is not an ideal baseline specification since these coefficient estimates are driven by identification within only the used car market.) Table 4 documents an increase in interest rates for captive loans relative to CarMax loans after the tariffs, consistent with the validity of our setting.

In sum, across multiple specifications which address various concerns regarding using non-captives as a counterfactual, we document consistent evidence of higher interest rates from captive lenders following the metal tariffs. Figure 7 further confirms that none of these alternative specifications exhibit any economically significant differential pre-trends.[37]

4.3 Composition of Borrowers

So far, we have framed our results in terms of the intensive margin: in response to the tariffs, captive lenders charged inframarginal borrowers higher interest rates. However, changes in the composition

36. As mentioned in Section 2.1, another concern is that the vehicle sales price might interact with the dealer's choice of lender or the choice to obtain indirect financing. Ideally, we would link vehicles' sales prices to their sources of financing, but unfortunately this is not possible with our data. The Regulation AB II data only contains the source of financing but not the sales price, and the Texas DMV data only contains the sales price but not the source of financing. However, in addition to the CarMax test, we re-estimate Equation 2 on the subsample of new vehicle loans with the invoice price as the outcome variable. As shown in Table IA.10, we find no differential change in invoice prices for captive and non-captive financings following the tariffs.

37. Table IA.11 estimates another alternative specification that compares more-exposed captive lenders to less-exposed captive lenders, removing non-captive lenders altogether. While the coefficient magnitudes are similar to our baseline results, the estimate for the all-loan sample is statistically insignificant at the 10 percent level. One reason for this may be a power issue. Captive lenders typically only finance their manufacturer's own brands of vehicles (i.e., Honda Finance only finances Hondas and Acuras), with the main exception being GM-AmeriCredit, which finances some off-brand used (but not new) vehicles in addition to GM's own brands. As such, estimating a regression which includes vehicle make $\times$ model $\times$ condition $\times$ quarter fixed effects using only loans from more-exposed and less-exposed captive lenders will result in our coefficient estimates being identified off a limited number of loans (less than 1 percent of total sample) originated by GM-AmeriCredit on less-exposed captives' brands of vehicles (i.e., BMW/Mini, Mercedes, and Volkswagen/Audi).

of borrowers along the extensive margin could have also produced higher average captive interest rates. For example, captive lenders might have relaxed their underwriting standards and taken on more credit risk in response to lower margins on the manufacturing side of their business (Benneton, Mayordomo, and Paravisini 2022). Demand-side responses to higher anticipated borrowing costs – such as adverse selection or borrowers switching from captive to non-captive lenders – could have also generated an overall riskier pool of captive borrowers and higher average interest rates (Karlan and Zinman 2009; Einav, Finkelstein, and Mahoney 2021). Although our fixed effects help control for changes in the composition of borrowers to some extent, gaining a better understanding of whether our results come from the intensive or extensive margin is important because the former margin is consistent with the existence of tariff pass-through while the latter is not.[38]

To examine the effect of the tariffs on the composition of captive borrowers, we estimate the following regression model:

$$y_{i,l,v,s,t} = \alpha + \Gamma \cdot \text{Treated}_l \cdot \text{Post}_t + \delta_l + \delta_{v,t} + \delta_{s,t} + \varepsilon_{i,l,v,s,t}, \tag{4}$$

where the outcome variable is either the log household income, log credit score, or future default rate of loan i originated in quarter t. The coefficient of interest is Γ, which measures the average change in borrower characteristics for captive loans relative to non-captive loans.

Panel A in Table 5 reports the coefficient estimates from Equation 4. Consistent with our results capturing tariff pass-through along the intensive margin, we find that the characteristics of captive borrowers did not become worse following the announcement of the tariffs. Relative to the pool of non-captive borrowers, the pool of captive borrowers experienced an economically small increase (not a decrease) in average household incomes (Γ = 0.012; t = 3.25) and no significant change in average credit scores (Γ = 0.001; t = 1.13) or future default rates (Γ = -0.000; t = -0.62). Panel B reports the coefficient estimates after we remove subsidized loans from the sample. We continue to find that the pool of captive borrowers experienced a slight increase in average household

38. Concerns about composition effects arise because our data contains information on originated loans but not loan offers or applications. If we had data on loan offers, then we could produce a direct estimate of the effect of the tariffs on offered loan terms holding the pool of borrowers fixed, as well as an estimate of the effect on demand.

incomes and no significant decline in credit scores or default rates. Note that, from a risk-based pricing perspective, the increase in household incomes is not only too small to explain the observed increase in captive interest rates, but it is also of the wrong sign.

Figure 8 plots the dynamics of the coefficient estimates. Like above, we find no significant evidence that captive borrowers' characteristics became worse following the tariffs, as well as no economically meaningful evidence of differential pre-trends. Overall, the results suggest that the increase in captive interest rates in Table 3 reflects tariff pass-through along the intensive margin, not a change in the composition of captive borrowers.[39]

4.4 Alternative Explanations and Robustness

The tariffs may have impacted the auto loan market in many different ways, including changing the borrowing costs of captive lenders or through consumer demand. Below, we examine several potential alternative explanations for our results but find that none are supported in the data.

4.4.1 Differential Increases in Borrowing Costs

The tariff cost shock may have increased the credit risk of auto manufacturers and, in turn, their borrowing costs. To rule out this alternative channel, we re-estimate Equation 2 after controlling for interactions between our treatment indicator and lender-specific measures of borrowing costs. As shown in Table IA.12, we continue to find that captive interest rates increased relative to non-captive interest rates after the tariffs, inconsistent with differential increases in borrowing costs driving our results.

4.4.2 Differential Exposure to Increases in Default Risk

The tariffs might have led to higher auto loan default rates through various mechanisms such as their effect on vehicle prices (see Section 4.5). Captive lenders, which are less diversified than

39. Given that the composition of captive borrowers does not change, what tradeoffs do captive lenders face when deciding whether to raise their loan prices? Section A.3 in Appendix A shows one tradeoff: in response to higher interest rates from the tariffs, captive loan originations declined 6.7 percent.

non-captive lenders, might have been more exposed to this higher default risk and, as a result, might have been forced to raise their interest rates more than non-captive lenders. To explore this potential concern, we conduct two complementary tests. First, in Table 5, we show that the default rates on captive loans did not increase relative to non-captive loans following the tariffs. Second, we re-estimate Equation 2 after controlling for the interaction between our treatment indicator and changes in population auto loan default rates. As shown in Table IA.13, we find that our main results persist even after we add these controls, suggesting that differential exposures to changes in default risk do not drive our results.

4.4.3 Changes in Dealer Interest Rate Markups

As discussed in Section 2.1, auto dealers intermediate the origination of almost all captive auto loans. During the loan origination process, dealers often have the option to charge consumers higher interest rates than what the lender has offered, a practice known as dealer markup (Cohen 2012).[40] One potential concern is that the increase in captive interest rates in Table 3 is not coming from an increase in lenders' offered interest rates, but rather an increase in the size of dealers' markups. If this were true, then we would not be able to interpret our results as evidence of interest rate pass-through from captive lenders.

There are two pieces of evidence that suggest that changes in dealer markups do not drive our results. First, in addition to captive loans, non-captive loans are also subject to dealer markups.[41] Hence, common changes in dealer markups across captive and non-captive loans should be netted out in our difference-in-differences specification. Second, in Table IA.14, we document a significant increase in interest rates for subvented captive loans, which are promotional loans that dealers are

40. The additional revenue from loan markups is split between the dealer and the lender according to a prespecified formula. Examining a sample of non-captive loans, Grunewald et al. 2023 show that the average dealer receives around 75 percent of the present value of the markup via an upfront fee called the dealer reserve. Given an average markup of 113 basis points, the average dealer reserve is around $600, which is much larger than the average loan origination fee of $75. Because of several class-action lawsuits, most lenders cap markups at 200-250 basis points.

41. Most of the non-captive lenders in our sample specialize in indirect auto lending. For example, Santander's 2018 annual report contains the following description of their auto loan business: "The Company's primary business is the indirect origination, securitization, and servicing of retail installment contracts and leases, principally through manufacturer-franchised dealers in connection with their sale of new and used vehicles to retail consumers".

typically not allowed to mark up (Grunewald et al. 2023).[42] This finding helps mitigate the concern that auto dealers increased their markups more for captive loans than non-captive loans.

4.4.4 Temporary Increases in Loan Demand

Upon the announcement of the tariffs, forward-looking consumers might have moved up their vehicle purchases in anticipation of higher future prices. If these consumers also sought captive financing, then the resulting surge in loan demand might have caused captive lenders to increase their interest rates to manage their throughput and clear the market.[43] To rule out this possibility, Figure IA.1 plots vehicle sales volumes for our sample of auto manufacturers around the treatment date. There is no noticeable increase in vehicle sales (and hence loans demanded) following the announcement of the tariffs.[44] Two other pieces of evidence are also inconsistent with this alternative explanation. First, Figure 6 shows that there is no reversal in the increase in captive interest rates during the post-tariff period. Second, Table IA.1 shows that loan origination volumes decreased (not increased) for captive lenders relative to non-captive lenders after the the tariffs.

4.4.5 Unobservable Selection on Consumer Price Inelasticity

Some price-sensitive consumers might have forgone vehicle purchases in response to higher nominal vehicle prices. This could have resulted in the average consumer who purchased a vehicle – and hence the average borrower – being slightly less price-sensitive following the tariffs than before. If consumers that are more inelastic to vehicle prices are also less sensitive to loan prices, then selection on vehicle prices might explain some of the observed increase in interest rates.

While it is difficult to evaluate a shift in unobservable selection, multiple results suggest it does

42. Manufacters do not allow subvented loans to be marked up because the financing rates are designed to sell certain models of vehicles (e.g., "1.99 percent APR for well-qualified borrowers"). Instead of receiving the dealer reserve, auto dealers are compensated with higher origination fees for intermediating these loans (Warshaw 2014).

43. Capacity constraints could arise because of both financial reasons (e.g., no immediate source of funding) and operational reasons (e.g., not enough loan underwriters). A similar phenomenon has been documented in the mortgage market. In particular, Fuster et al. 2013 document evidence of significant capacity constraints in the mortgage market and show that they help explain why mortgage originators make larger profits during refinancing waves.

44. Waugh 2019 finds a slight decline in vehicle demand in areas more exposed to retaliatory tariffs from China. We note that such effects, along with other potential effects such as a reduction in household incomes, should be common across captive and non-captive loans and hence should be absorbed into our various time fixed effects.

not drive our results. First, common forms of selection based on vehicle prices should be netted out in our difference-in-differences specification since both captive and non-captive borrowers are subject to higher vehicle prices. Second, as shown in Table 5, we find no differential changes in captive borrowers' observable characteristics or default rates following the tariffs. Although this does not entirely rule out that captive borrowers are becoming differentially less price sensitive along unobservable dimensions, we note that we find no differential changes in observable borrower-level characteristics that Grunewald et al. 2023 show are correlated with loan price sensitivities, such as household incomes and credit scores. Lastly, Attanasio, Goldberg, and Kyriazidou 2008 find that longer loan maturities correlate with higher financing inelasticities but Table 3 shows that average loan maturities did not significantly increase in response to the tariffs.

4.4.6 Changes in Prepayment Behavior

If higher interest rates caused captive borrowers to prepay their loans more quickly, then the estimated increase in total loan payments in Section 4.1.1 would be overstated because it relies on constant repayment assumptions. To examine whether captive borrowers partially undid the effects of higher interest rates by prepaying their loans at faster rates, we re-estimate Equation 2 with indicators for whether a loan is paid off within 12 or 24 months of its origination date as the outcome variable.[45] As shown in Table IA.15, we find no evidence that captive loans' prepayment rates increased significantly in response to the tariffs.

4.4.7 Changes in Securitization Practices

Another potential concern is whether captive lenders adjusted their securitization practices as input costs rose on the manufacturing side of their business to help their parent companies raise cash or

45. Prepaid loans are identified using two fields in the Regulation AB II data: (i) *Zero Balance Code*, which describes the reason that a loan's balance went to zero; and (ii) *Zero Balance Effective Date*, which records when the loan's balance went to zero. By combining these two fields with information on a loan's current balance and its original maturity, we can construct indicator variables for whether a loan is fully prepaid within 12 or 24 months of its origination date. While most lenders continue to report data on prepaid loans even after they are prepaid, a small set of lenders stop reporting data on prepaid loans a few months after prepayment. We note that this difference in reporting styles does not affect our analysis, as even in the latter case we can measure when a loan is prepaid based on its last reported values for the *Zero Balance Code* and *Zero Balance Effective Date* fields.

smooth earnings. In particular, captive lenders might have securitized a larger fraction of loans with higher interest rates (which command higher prices in secondary markets) despite not raising their overall loan prices. If this were true, then we would not be able to attribute the differential increase in captive interest rates in Table 3 to tariff pass-through.

To test whether changes in captive lenders' securitization practices drive our results, we start by combining the Regulation AB II data with the population credit bureau data from Section 3.1.1. We then use the combined data to estimate the following regression model:

$$y_{l,t} = \alpha + \Gamma \cdot \text{Treated}_l \cdot \text{Post}_t + \delta_l + \delta_t + \varepsilon_{l,t}, \tag{5}$$

where the unit of observation is a lender l in quarter t. We study the impact of the tariffs on four main securitization-related outcome variables: (i) the share of loans that lender l originated in quarter t that were later securitized, (ii) the ratio of the average loan amount for securitized loans to the average loan amount for all loans that lender l originated in quarter t, and (iii) the same ratio but for average loan maturities and (iv) average monthly loan payments.

Table IA.16 reports the coefficient estimates from the model. We find no evidence that captive lenders changed their securitization practices in response to the tariffs. Specifically, for all our securitization-related outcome variables, the coefficient estimates from Equation 5 are economically small and statistically insignificant at the 10 percent level. Figure IA.2 supplements the above test by plotting securitization volumes for captive and non-captive lenders during our sample period. There is no meaningful evidence that captive lenders increased their securitization volumes relative to non-captive lenders.

4.4.8 Selection into Dealer-Intermediated Non-Captive Loans

While our captive treatment group only consists of indirect loans, our non-captive control group consists of both indirect and direct loans. Further, within the non-captive control group, there may be selection into indirect loans, and this selection may interact with the vehicle sales price in ways that potentially bias our estimates. To address this concern, we re-estimate Equation 2 after restricting our non-captive control group to only indirect loans. Specifically, we limit our control sample to auto loans originated by either CarMax, Santander, or World Omni, all of which only originate auto loans indirectly through dealers and do not offer loans directly to consumers. This effectively removes the concern that the control group includes a combination of indirect and direct loans. Table IA.17 reports the coefficient estimates from the model. We continue to find that captive interest rates increased relative to indirect non-captive interest rates, with the magnitude of the interest rate estimate (25 basis points) closely matching the magnitude of our baseline estimate in Table 3 (26 basis points).[46]

4.4.9 Additional Robustness

We conduct several other robustness tests to ensure that our results are not sensitive to our choice of fixed effects, treatment date, sample period, sample filters, and assumptions about our standard errors, among other things. For a more thorough discussion of our robustness tests, see Section A.2 in Appendix A.

4.5 New Vehicle Prices

As illustrated in Figure 3, auto manufacturers can pass through a tariff cost shock by adjusting new vehicle prices to dealerships as well as financing terms to consumers. Franchised dealerships, which purchase new vehicles from auto manufacturers, then may pass higher vehicle costs along to consumers via higher sales prices. To evaluate the full cost of the tariff to consumers, we compare the change in prices for new vehicles that are more and less exposed to the tariffs. We primarily measure exposure at the vehicle make level, but our results are robust to measuring exposure at the make-model level.

46. A related issue is that the non-captive lenders in the Regulation AB II data may have a different mixture of indirect and direct loans than the large non-captive lenders missing from the data. While this issue would affect the generalizability of our estimates, it should not affect their causal interpretation given the results in Table IA.17.

Specifically, we evaluate the change in new car prices by estimating the following model:

$$p_{i,m,v,b,t} = \alpha + \Gamma \cdot \text{US Made}_m \cdot \text{Post}_t + \delta_v + \delta_{b,t} + \varepsilon_{i,m,v,b,t}, \tag{6}$$

where the outcome variable is either the log invoice price or log sales price of vehicle i sold in quarter t, and the indicator variable US Made$_m$ is equal to one if at least 50 percent of new vehicles of make m are assembled in the U.S., and zero otherwise.[47] The model includes vehicle make-model-condition fixed effects (δ_v) and \$25,000 vehicle price bin $\times$ quarter fixed effects ($\delta_{b,t}$) to ensure that the treatment effect (Γ) is estimated using within-model variation after netting out common shocks to vehicles within similar price ranges.[48] The estimation sample consists of new vehicles sold between 2017 and 2018, and standard errors are clustered at the make level to match the assignment of treatment.[49]

The coefficient of interest, Γ, measures the conditional average change in new vehicle prices for makes with a higher proportion of vehicles assembled in the U.S. relative to makes with greater foreign production. If the tariff cost shock caused auto manufacturers and, subsequently, dealers to raise prices more sharply for vehicles with larger cost increases, then Γ should be positive. Columns 1 and 3 in Table 6 report the coefficient estimates from Equation 6. We find that more-exposed makes with a higher proportion of U.S. production experienced larger increases in invoice and sales prices. On average, sales prices rose by 0.7 percent for more-exposed makes following the tariffs, representing a \$225 average increase relative to the pre-treatment average sales price of \$32,206 for this sample. Invoice prices also rose by 1.1 percent for these same makes of vehicles,

47. To measure the fraction of vehicles that are made in the U.S., we use VIN numbers from the Texas DMV data, as the first digit of the VIN number tells us the country where each vehicle was produced and assembled. In particular, a first digit of 1, 4, or 5 corresponds to vehicles produced and assembled in the United States.

48. The vehicle price bin $\times$ quarter fixed effects serve two purposes. First, since the Texas DMV data does not contain borrower-level characteristics, these fixed effects act as replacements for the income bin $\times$ quarter fixed effects and credit score bin $\times$ quarter fixed effects in Equation 2. Second, these fixed effects help control for the fact that makes of vehicles assembled outside the U.S. are more likely to be luxury vehicles, which may have been exposed to different shocks than non-luxury vehicles during our sample period.

49. To be consistent with our auto loan sample in Section 3.2, we impose the following three filters on our vehicle price data. First, we remove vehicles whose prices are either below \$10,000 or above \$100,000. Second, we restrict our attention to make-models that appear in both the Regulation AB II and Texas DMV data, both before and after the treatment date. Third, we winsorize invoice and sales prices at the one percent tails. For invoice prices, we estimate Equation 6 using the Regulation AB II data. For sales prices, we use the Texas DMV data.

consistent with auto dealers first passing through the tariff cost shock to dealers, which then pass it through to consumers. Columns 2 and 4 document similar results when we define US Made$_m$ at the make-model level instead of the make level. In addition, Figure IA.3 shows that sales and invoice increased gradually throughout the post-treatment period and exhibited limited evidence of differential pre-trends.

While the above results suggest that auto manufacturers may have tied vehicle price increases to tariff-related cost increases, the results in Sections 4.1 and 4.2 show that auto manufacturers' captive lenders spread price increases broadly across several different types of loans (e.g., new and used vehicle loans). There are many possible explanations for this interesting empirical pattern. To start, different makes and models of vehicles are (imperfect) substitutes for one another (Berry, Levinsohn, and Pakes 2004) whereas auto loans are primarily complementary goods to vehicle purchases (Einav, Jenkins, and Levin 2012). Moreover, indirect (dealer-intermediated) auto loans often are an add-on or sequential component to the vehicle purchase process and there is a large literature documenting that consumers are less sensitive to the prices of add-on goods (Ellison 2005; Ellison and Ellison 2009; Grunewald et al. 2023). More generally, papers such as Giroud and Mueller 2019, Luco and Marshall 2020, and Armstrong and Vickers 2023 show that cost pass-through for multi-product or multi-division firms is complex even in much simpler settings than the auto market.

4.6 Tariff Incidence on American Consumers

Next, we compare pass-through along the loan price and the vehicle price margins to evaluate the extent to which focusing on sales prices alone would understate the economic impact of the metal tariffs. Below, we show that tariff pass-through via interest rates was almost two-thirds as large as tariff pass-through via vehicle prices, so that ignoring the impact on financing terms would significantly underestimate tariff incidence on consumers.

We start by writing down the formulas for interest rate pass-through and vehicle price pass-through. Conceptually, interest rate pass-through measures how much captive borrowers' financing costs changed relative to auto manufacturers' production costs. This can be written as:

$$\rho(L) = \frac{F \cdot \Delta P}{N \cdot \Delta C}, \tag{7}$$

where F is the number of (new and used) captive auto loans originated, ΔP is the average present value increase in captive financing costs due to the tariffs, N is the number of new vehicles produced and sold, and ΔC is the average increase in production costs per vehicle.

Similarly, vehicle price pass-through measures how much new vehicles' sales prices changed relative to auto manufacturers' production costs. This can be written as:

$$\rho(V) = \frac{N \cdot \Delta V}{N \cdot \Delta C}, \tag{8}$$

where ΔV is the average increase in sales prices for new vehicles following the tariffs.

As stated above, the pass-through rate is equal to the sum of interest rate pass-through and vehicle price pass-through:

$$\rho = \rho(L) + \rho(V). \tag{9}$$

Taking the ratio of Equations 7 and 9 tells us the relative contribution of interest rate pass-through to total pass-through:

$$\frac{\rho(L)}{\rho} = \frac{F \cdot \Delta P}{F \cdot \Delta P + N \cdot \Delta V}. \tag{10}$$

Moreover, dividing Equation 7 by Equation 8 tells us how important interest rate pass-through is relative to vehicle price pass-through:

$$\frac{\rho(L)}{\rho(V)} = M \cdot \frac{\Delta P}{\Delta V}, \tag{11}$$

where $M := F \cdot N^{-1}$ is the captive loan penetration rate.

Given Equations 10 and 11, measuring the importance of interest rate pass-through just involves plugging in values for ΔP, ΔV, and M. From Sections 4.1.1 and 4.5, we have that $\Delta P =$ \$179 and $\Delta V = \$225$. From population data, we have that $M = 0.59$. Therefore, we initially estimate that interest rate pass-through is almost one-half ($= 0.59 \cdot \$179/\$225$) as large as vehicle price pass-through, and that ignoring interest rate pass-through would cause us to underestimate tariff incidence on consumers by 32 percent ($= \$179/(\$179 + \$225/0.59)$).[50]

As mentioned in Section 4.2, one potential concern is that our estimate of interest rate pass-through might be attenuated due to spillover effects on non-captive lenders. Although we cannot precisely measure the size of spillover effects in our data, Appendix C presents two approaches for estimating them. First, we develop a simple model of the auto loan market which predicts that the theoretical magnitude of the spillover on non-captive lenders was 7 basis points. This is consistent with an alternative estimate of 6.26 basis points derived from realized changes in the market interest rates and historical pass-through rates (see Appendix C for a description of this data-driven procedure).[51] Therefore, after accounting for spillover effects, we estimate that captive interest rates increased by 33 (= 26 + 7) basis points, on average, following the tariffs, or by \$227 per loan in present value terms. This spillover-inclusive estimate in turn implies that interest rate pass-through is almost two-thirds ($= 0.59 \cdot \$227/\$225$) as large as vehicle price pass-through, so that focusing on vehicle prices alone would cause us to underestimate tariff incidence on consumers by 37 percent ($= \$227/(\$227 + \$225/0.59)$).[52]

50. Focusing on the relative importance of interest rate and vehicle price pass-through allows us to dispense with estimating the average change in vehicle production costs (i.e., ΔC), as the cost components in the denominators of the pass-through rates drop out when we take the ratio of them. Estimating the average change in production costs is highly difficult because granular data on auto manufacturers' costs is generally unavailable. See Appendix B.

51. While some models of imperfect competition predict that non-captive lenders would have raised their interest rates by as much as captive lenders following the tariffs, other models predict a more muted response. In our model, the main tradeoff that non-captive lenders face when deciding whether to raise their interest rates is between per-loan profits and market share. Competitive interactions between non-captive lenders limit the overall scope for strategic price increases, as the adverse impact of higher prices on market share becomes more severe as competition among non-captives rises. Further, empirical studies find a variety of magnitudes for strategic price increases in imperfectly competitive markets (Muehlegger and Sweeney 2022, Amiti, Itskhoki, and Konings 2019, Flaaen, Hortacsu, and Tintelnot 2020.

52. Given the existing evidence that consumers are more sensitive to vehicle price changes than financing terms (Busse and Silva-Risso 2010) and the limited evidence of interest rate spillovers in our setting, we believe that any vehicle price spillovers should be minimal and thus not materially affect our relative pass-through calculations. Nevertheless, to the extent that vehicle price spillovers are non-zero but still smaller than interest rate spillovers (in a proportional sense relative to their direct effects), our estimates of 0.47 ($= 0.59 \cdot \$179/\$225$) and 0.60 ($= 0.59 \cdot \$227/\$225$) above should act as lower and upper bounds on the relative importance of interest rate pass-through, respectively. Further, these bounds imply that focusing on vehicle prices alone would underestimate tariff incidence on consumers between 32 and 37 percent.

Another way to think about the economic impact of the tariffs is in terms of the average cost increase faced by American consumers in the auto market. Start by considering the subset of consumers that purchase a new vehicle and finance it through a captive lender. On average, these consumers pay ΔV = \$225 in higher vehicle sales prices because of the tariffs, as well as ΔP = \$227 in additional present value loan costs (inclusive of spillover effects). Therefore, the average cost increase faced by this group of consumers is \$452 (= \$225 + \$227), or 1.76 (= 0.88 + 0.88) percent of the \$25,619 average loan amount.

However, tariff incidence is not limited to new vehicles purchased with loans from captive lenders. Used vehicles financed with captive loans also had higher interest rates after the tariffs, resulting in an additional ΔP = \$227 cost for these consumers. We estimate that non-captive interest rates also rose slightly (7 basis points) due to the changing market dynamics, increasing the present value cost of non-captive loans by ΔP = \$48 for both new and used auto purchases. Table IA.18 presents the frequency as well as the change in both average financing costs and vehicle prices for each group of auto purchasers. The population-weighted average increase in financing costs is \$72 while the population-weighted average increase in vehicle prices is \$74. That is, we find that the overall average cost increase faced by American consumers is \$146 per vehicle, or 0.57 percent of the average loan amount, and this cost increase is roughly equally split between higher vehicle prices and financing costs. This further underscores the importance of tariff pass-through along the interest rate margin.

Finally, from a policy standpoint, it is interesting to consider the aggregate dollar cost of the tariffs coming from higher interest rates. From population data, there are around 50 million vehicles sold in the U.S. annually. Combining this value with our average cost estimates from above, we have that the tariffs resulted in around \$3.6 billion (= 50,000,000 · \$72) in additional present value financing costs each year. For reference, Flaaen, Hortacsu, and Tintelnot 2020 estimate that the 2018 import tariffs on washers led to \$1.5 billion in additional costs for American consumers annually.

5 Heterogeneous Incidence of Tariffs

Thus far, we have shown that the tariffs led to higher average interest rates for consumers in the auto loan market. However, given the existing finance literature has documented substantial price dispersion in consumer credit along dimensions such as geography (Argyle, Nadauld, and Palmer 2023) and financial sophistication (Bhutta, Fuster, and Hizmo 2024), we next explore whether there is heterogeneity in the incidence of tariffs. Specifically, we test whether the metal tariffs' impact varied across consumers with different credit demand elasticities and in areas with different degrees of credit market competition.

5.1 Credit Demand

Economic theory predicts that firms will find it easier to pass on cost shocks along margins where consumers are less price sensitive (Chen and Juvenal 2016). To explore the role of credit demand elasticities in determining tariff pass-through, we test whether the increase in captive interest rates was larger for borrowers whom prior studies have found to be less sensitive to credit prices. We measure credit demand elasticities using three proxies. First, we build on Attanasio, Goldberg, and Kyriazidou 2008 and Grunewald et al. 2023, which find that lower income consumers are less sensitive to increases in interest rates than higher income consumers. We start by splitting our sample into two groups based on the median household income. We then estimate the following triple-differences model:

$$
\begin{aligned}
y_{i,l,v,s,w,c,t} = {} & \alpha + \beta \cdot \text{Low Income}_i \cdot \text{Treated}_l \cdot \text{Post}_t + \Gamma \cdot \text{Treated}_l \cdot \text{Post}_t \\
& + \theta \cdot \text{Low Income}_i \cdot \text{Treated}_l + \delta_l + \delta_{v,t} + \delta_{s,t} + \delta_{w,t} + \delta_{c,t} + \delta_{\text{Low Income},t} + \varepsilon_{i,l,v,s,w,c,t},
\end{aligned}
\tag{12}
$$

where Low Income$_i$ is equal to one when loan i has a below-median household income, and zero otherwise. The coefficient of interest, β, measures the differential effect of the tariffs on lower income consumers relative to higher income consumers. Generally, theory predicts that tariff incidence should be higher for lower income consumers who have less elastic credit demand.

Table 7 documents evidence that tariff pass-through via interest rates was indeed higher for lower income consumers. Specifically, we find that higher income consumers experienced an average increase in captive interest rates of 20 basis points, while lower income consumers experienced a much larger average increase of 33 basis points. These estimates, in turn, imply that interest rate pass-through was approximately 72 percent as large as vehicle price pass-through for lower income consumers (inclusive of spillover effects), whereas it was only 49 percent as large as vehicle price pass-through for higher income consumers.[53]

In addition to income-based variation, Argyle, Nadauld, and Palmer 2020 find that consumers with lower credit scores are less sensitive to increases in interest rates. Therefore, we repeat the above exercise using credit scores as an alternative measure of credit demand elasticities. Table 7 reports the coefficient estimates from Equation 12 after replacing the Low Income$_i$ variable with Low Credit Score$_i$, which is equal to one when loan i has a below-median credit score, and zero otherwise. Again, we find that interest rate pass-through is differentially higher for consumers with lower credit scores who likely have less elastic credit demand. While consumers with higher credit scores experienced an average increase in captive interest rates of 15 basis points due to the tariffs, consumers with lower credit scores experienced an average increase of 36 basis points. This difference implies that the ratio of interest rate pass-through to vehicle price pass-through was 39 percentage points higher for lower credit score consumers than higher credit score consumers (79 percent versus 40 percent, respectively).

Our third proxy is loan amount, as smaller loan amounts have been shown to be associated with tighter credit constraints and lower credit demand elasticities (Adams, Einav, and Levin 2009). Table 7 reports the coefficient estimates from Equation 12 after replacing the Low Income$_i$ variable with Low Loan Amount$_i$, which is equal to one when loan i has a below-median loan amount, and zero otherwise. Again, consistent with differences in credit demand contributing to heterogeneity in

53. Specifically, after incorporating the average spillover effect of 7 basis points from Section 4.6, we estimate that lower (higher) income consumers experienced a \$275 (\$185) increase in present value loan costs as a result of the tariffs. Plugging these values in for ΔP in Equation 11 and using our prior estimates of $\Delta V = \$225$ and $M = 0.59$ then yields the numbers quoted above. We note that one challenge that arises when computing heterogeneity in the relative importance of interest rate pass-through is that our vehicle sales price data does not contain consumer-level characteristics. This forces us to use the same value of ΔV for the entire population, which may be unwarranted.

the pass-through rate, we find that the increase in captive interest rates was higher for consumers with smaller loan amounts (36 basis points) than larger loan amounts (12 basis points).

In the right-most columns of Table 7, we examine whether changes in the composition of borrowers are driving our cross-sectional results. To do so, we re-estimate Equation 12 with the default rate as our outcome variable. As shown in columns 4 through 6, we find no significant changes in default rates for any of our specifications. This suggests that composition effects do not explain the heterogeneity in interest rate pass-through across our credit demand proxies.

Finally, to better understand how interest rate pass-through varies based on credit demand, Figure 9 plots the coefficient estimates from Equation 2 for each income, credit score, and loan amount quartile. As shown in Panel A, we find that interest rate pass-through is monotonically decreasing across the income quartiles. Specifically, while consumers in the lowest income quartile experienced an average increase in captive interest rates of 37 basis points, consumers in the highest income quartile experienced an average increase in interest rates of just 17 basis points. Panel B plots the coefficient estimates for each credit score quartile. Although the pattern is non-monotonic, we continue to find that pass-through via interest rates is higher for borrowers that have lower credit scores. Further, we note that the non-monotonic pattern partly stems from differences in the composition of lenders across the credit score quartiles. For example, AmeriCredit has an outsized presence in the first quartile, and this lender has one of the lower pass-through rates in our sample. As shown in Panel C, removing AmeriCredit from the sample produces a pattern that is closer to monotonic. Panel D further shows that interest rate pass-through is monotonically decreasing in loan size.

Combined, our results suggest that consumers with lower incomes, lower credit scores, and smaller loan amounts shouldered a disproportionate share of the tariffs. This finding has important implications for assessing the effectiveness of recent tariff policies, as the 2018 tariffs were designed in-part to protect such individuals in the labor market (Amiti, Redding, and Weinstein 2020). In the next section, we examine whether other economic forces – such as the degree of lending market competition – also contributed to the heterogeneous incidence of the tariffs.

5.2 Credit Market Competition

Theory also predicts that the pass-through rate will depend on market structure and competition. In particular, Weyl and Fabinger 2013 show that the theoretical relation between pass-through and competition is ambiguous, and that it depends on several factors such as the nature of the cost shock and the shapes of the demand and supply curves.[54] With respect to the auto loan market, one of the most important factors to consider is that the tariffs affected the marginal costs of captive lenders but not non-captive lenders. For such a firm-specific cost shock, a wide range of models (including the one in Appendix C) predict that pass-through will be increasing as the level of competition declines.

One challenge that arises when examining heterogeneity in the pass-through rate across the level of competition is that most auto lenders face similar competitive environments. In general, competition in the auto loan market tends to be national in scope. As described in Section 2.1, the vast majority of auto loans are originated through auto dealerships, which access thousands of lenders across the U.S. via online platforms such as DealerTrack and RouteOne. However, the alternative to dealer financing is to borrow directly from a lender, and this market is largely local. Argyle, Nadauld, and Palmer 2023 find that the median direct auto loan is originated from a branch within 15 minutes of the borrower's home. Thus, differences in the number of credit unions and regional lenders serving each state may create meaningful geographic variation in credit market competition.

We follow the banking literature (e.g., Drechsler, Savov, and Schnabl 2017) and use the Herfindahl–Hirschman index (HHI) as our inverse measure of credit market competition. Like Yannelis and Zhang 2023, we construct our HHIs at the state level (which is our most granular measure of location) based on pre-treatment lender market shares in each state.[55] We then split our sample

54. Empirical studies find both positive and negative relations between pass-through and competition. For instance, while Genakos and Pagliero 2022 finds that pass-through increases as competition rises in the gasoline market, Doyle and Samphantharak 2008 finds the opposite.

55. We use the population credit bureau data from Section 3.1.1 to construct our HHIs. The average state-level HHI is around 0.025 with an interquartile range of 0.022 to 0.028. These numbers are consistent with Yannelis and Zhang 2023, and they suggest that there is some local component of competition in addition to the national component.

into two groups based on the median state-level HHI. Finally, we re-estimate Equation 12 after replacing the Low Income$_i$ variable with Low Competition$_s$, which is equal to one when state s has an above-median HHI (i.e., below-median competition), and zero otherwise.

Table 8 reports the coefficient estimates from the model. Consistent with the predictions above, we find that tariff pass-through via interest rates was higher in states with lower credit market competition. While consumers in states with more competitive credit markets experienced an average increase in captive interest rates of 23 basis points, consumers in states with less competitive credit markets experienced an average increase of 28 basis points. Albeit economically small, this 5 basis point difference is statistically significant at the 10 percent level.

It is possible that the above results are attenuated because there is not much variation in our measure of competition around the median (Roberts and Whited 2013). Therefore, to better understand the role of credit market competition in our setting, we focus our attention on the tails of the competition distribution. We start by restricting our sample to loans that are either in the lowest or highest quartile of the competition distribution and then re-estimate Equation 12 after setting Low Competition$_s$ equal to one when state s is in the lowest quartile of the competition distribution, and zero otherwise. Afterwards, we further restrict our sample to loans that are either in the top or bottom decile of the competition distribution and re-estimate Equation 12 after setting Low Competition$_s$ to be equal to one when state s is in the lowest competition decile, and zero otherwise. Columns 2 and 3 in Table 8 report the coefficient estimates. Consistent with our prior results being attenuated, we find significantly more heterogeneity in the pass-through rate as we move further out into the tails of the competition distribution. For example, while consumers in the highest competition decile experienced an average increase in captive interest rates of 24 basis points, consumers in the lowest decile experienced an average increase of 41 basis points. This 17 basis point difference is more than three times as large as our above- versus below-median estimate in column 1, and it implies that interest rate pass-through was around 88 percent as large as vehicle price pass-through in less competitive credit markets, whereas it was only 57 percent as large as vehicle price pass-through in more competitive credit markets.

6 Conclusion

We examine tariff pass-through to consumer credit using the unique laboratory of auto lending during the 2018 metal tariffs. To do so, we compare loans from auto manufacturers' captive lenders to loans from non-captive lenders that were originated in the same quarter, in the same state, for the same vehicle make-model-condition, to borrowers with similar incomes and credit scores. The empirical evidence indicates that auto manufacturers passed on a significant portion of tariff-related costs to consumers via higher captive interest rates. Interest rate pass-through was almost two-thirds as large as vehicle price pass-through in our setting. Moreover, pass-through via the financing channel was significantly more pronounced among consumers with less elastic credit demand and in areas with lower credit market competition. Our results suggest that tariffs can have a material impact on both the price of goods as well as the associated financing terms to consumers.

While our paper focuses specifically on tariff pass-through to vehicle financing costs, our results have broad implications. Many firms support product sales with the provision of credit. Thus, the potential for mismeasuring tariff incidence is not limited to the auto industry. Caterpillar and Pulte Homes are just two examples of firms which reported higher input costs due to the 2018 tariffs and higher captive financing revenues. More generally, many types of firms sell bundled and complementary goods or goods with add-on or less salient features. This provides firms with the option to spread tariff costs across multiple price dimensions. Focusing solely on directly affected goods or just one component of total prices thus may lead researchers to significantly underestimate cost pass-through to consumers.

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Table 1: Descriptive Statistics

Variable	Mean (1)	SD (2)	P10 (3)	P25 (4)	P50 (5)	P75 (6)	P90 (7)	Captive (8)	Non-Captive (9)	*t*-diff (10)
Loan Amount	25,619	10,737	13,189	17,675	23,896	31,805	40,514	26,914	22,256	2.14
Interest Rate	4.39	3.56	0.00	1.90	3.89	6.29	8.95	2.52	6.30	-3.62
Monthly Payment	445	180	245	315	411	546	686	450	397	1.84
Loan Maturity	66	9	60	61	68	73	74	66	68	-1.61
Loan-to-Value	0.89	0.21	0.58	0.76	0.93	1.06	1.14	0.89	0.92	-0.89
Vehicle Value	29,742	12,245	15,725	20,746	27,200	36,998	46,656	30,862	25,044	1.90
New Vehicle?	0.65	0.48	0.00	0.00	1.00	1.00	1.00	0.81	0.39	2.02
Credit Score	748	63	659	698	751	803	831	756	730	2.68
Income	88,341	49,258	36,000	50,391	76,476	115,000	160,000	89,979	81,537	3.15
Co-Signed?	0.32	0.47	0.00	0.00	0.00	1.00	1.00	0.31	0.36	-2.51
Subvented?	0.60	0.49	0.00	0.00	0.00	1.00	1.00	0.81	0.22	4.30
12-Month Default	0.00	0.06	0.00	0.00	0.00	0.00	0.00	0.00	0.01	-1.43
24-Month Default	0.01	0.11	0.00	0.00	0.00	0.00	0.00	0.01	0.02	-1.59
12-Month Paidoff	0.07	0.25	0.00	0.00	0.00	0.00	0.00	0.03	0.09	-4.46
24-Month Paidoff	0.17	0.37	0.00	0.00	0.00	0.00	1.00	0.11	0.22	-5.12

NOTE.—This table describes our sample of 1,973,639 auto loans that were originated between January 2017 and December 2018. Descriptive statistics are as of each loan's origination date. In columns 8 through 10, we compare auto loans originated by captive lenders to auto loans originated by non-captive lenders. For these comparisons, we restrict our attention to the subsample of auto loans that were originated prior to the treatment date (982,095 loans). Columns 8 through 10 are defined as follows: *Captive* is the pre-treatment mean for captive loans, *Non-captive* is the pre-treatment mean for non-captive loans, and *t-diff* is the t-statistic for the difference in pre-treatment means between captive and non-captive loans. Standard errors, used to calculate the t-statistics in column 10, are clustered at the lender level.

Table 2: Pre-Treatment Conditional Comparison: Interest Rates

	Interest Rate	
	All Loans	No Subventions
	(1)	(2)
Treated	-1.903***	-0.980*
	(-3.61)	(-1.73)
Vehicle × Quarter FE	Y	Y
State × Quarter FE	Y	Y
Income × Quarter FE	Y	Y
Credit Score × Quarter FE	Y	Y
N	982,095	403,856
R^2	0.64	0.58

NOTE.—This table reports coefficient estimates from Equation 1. The dependent variable is the interest rate. The sample is restricted to auto loans originated during the pre-treatment period of January 2017 to December 2017. In column 2, we further restrict the sample to auto loans without subsidized financing. Vehicle fixed effects refer to vehicle make-model-condition combinations. t-statistics, presented below the coefficient estimates, are calculated by clustering at the lender level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Table 3: Auto Loan Terms

Panel A: All Loans

	Interest rate	Loan Amount	Loan Maturity	Loan-to-Value
	(1)	(2)	(3)	(4)
Treated × Post	0.255***	-0.008	-0.011***	-0.008**
	(2.75)	(-1.29)	(-4.19)	(-2.32)
Lender FE	Y	Y	Y	Y
Vehicle × Quarter FE	Y	Y	Y	Y
State × Quarter FE	Y	Y	Y	Y
Income × Quarter FE	Y	Y	Y	Y
Credit Score × Quarter FE	Y	Y	Y	Y
N	1,973,067	1,973,067	1,973,067	1,973,067
R^2	0.70	0.55	0.21	0.21

Panel B: Excluding Subvented Loans

	Interest rate	Loan Amount	Loan Maturity	Loan-to-Value
	(1)	(2)	(3)	(4)
Treated × Post	0.288***	0.008*	0.000	0.002
	(2.85)	(1.66)	(0.16)	(0.70)
Lender FE	Y	Y	Y	Y
Vehicle × Quarter FE	Y	Y	Y	Y
State × Quarter FE	Y	Y	Y	Y
Income × Quarter FE	Y	Y	Y	Y
Credit Score × Quarter FE	Y	Y	Y	Y
N	791,300	791,300	791,300	791,300
R^2	0.67	0.57	0.16	0.18

NOTE.—This table reports coefficient estimates from Equation 2. The dependent variable is either the interest rate, log loan amount, log loan maturity, or loan-to-value ratio. The sample is restricted to auto loans originated between January 2017 and December 2018. In Panel A, we report coefficient estimates for the full sample of auto loans. In Panel B, we restrict the sample to loans without subsidized financing. t-statistics, presented below the coefficient estimates, are calculated by clustering at the lender level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Table 4: Alternative Specifications: Interest Rates

Panel A: All Loans

	Interest Rate			
	(1)	(2)	(3)	(4)
Treated × Post	0.295***	-0.178	0.268***	0.204**
	(3.37)	(-1.33)	(2.78)	(2.25)
Treated Group	More Exposed Captives	Less Exposed Captives	All Captives	All Captives
Control Group	All Non-Captives	All Non-Captives	All Non-Captives	CarMax
Lender FE	Y	Y	Y	Y
Vehicle × Quarter FE	Y	Y	Y	Y
State × Quarter FE	Y	Y	Y	Y
Income × Quarter FE	Y	Y	Y	Y
Credit Score × Quarter FE	Y	Y	Y	Y
Treated ×Δ Fed Funds			Y	
Treated ×Δ 1Y Treasury			Y	
Treated ×Δ 5Y Treasury			Y	
Treated ×Δ 10Y Treasury			Y	
N	1,815,793	869,959	1,973,067	1,516,426
R^2	0.70	0.68	0.70	0.68

Panel B: Excluding Subvented Loans

	Interest Rate			
	(1)	(2)	(3)	(4)
Treated × Post	0.299***	0.129	0.295**	0.227***
	(2.99)	(1.08)	(2.75)	(2.82)
Treated Group	More Exposed Captives	Less Exposed Captives	All Captives	All Captives
Control Group	All Non-Captives	All Non-Captives	All Non-Captives	CarMax
Lender FE	Y	Y	Y	Y
Vehicle × Quarter FE	Y	Y	Y	Y
State × Quarter FE	Y	Y	Y	Y
Income × Quarter FE	Y	Y	Y	Y
Credit Score × Quarter FE	Y	Y	Y	Y
Treated ×Δ Fed Funds			Y	
Treated ×Δ 1Y Treasury			Y	
Treated ×Δ 5Y Treasury			Y	
Treated ×Δ 10Y Treasury			Y	
N	753,877	581,944	791,300	509,803
R^2	0.67	0.65	0.67	0.62

NOTE.—This table reports coefficient estimates from Equation 2. The dependent variable is the interest rate. The sample is restricted to auto loans originated between January 2017 and December 2018. Column 1 uses more-exposed captives as the treated sample and non-captives as the control sample. Column 2 uses less-exposed captives as the treated sample and non-captives as the control sample. We restrict these samples to vehicle models with at least 100 loans. Column 3 uses captives as the treated sample and non-captives as the control sample. Column 4 uses captives as the treated sample and CarMax as the control sample. In Panel A, we estimate the model on the full sample of subvented and non-subvented loans. In Panel B, we restrict the sample to loans without subsidized financing. *t*-statistics, presented below the coefficient estimates, are calculated by clustering at the lender level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Table 5: Borrower Characteristics

Panel A: All Loans

	Income	Credit Score	12-Month Default	24-Month Default
	(1)	(2)	(3)	(4)
Treated × Post	0.012***	0.001	-0.000	-0.011
	(3.25)	(1.13)	(-0.62)	(-1.64)
Lender FE	Y	Y	Y	Y
Vehicle × Quarter FE	Y	Y	Y	Y
State × Quarter FE	Y	Y	Y	Y
N	1,973,067	1,973,067	1,973,067	1,361,478
R^2	0.15	0.22	0.03	0.04

Panel B: Excluding Subvented Loans

	Income	Credit Score	12-Month Default	24-Month Default
	(1)	(2)	(3)	(4)
Treated × Post	0.013***	-0.002	-0.000	-0.007
	(3.01)	(-0.70)	(-0.18)	(-1.15)
Lender FE	Y	Y	Y	Y
Vehicle × Quarter FE	Y	Y	Y	Y
State × Quarter FE	Y	Y	Y	Y
N	791,300	791,300	791,300	557,380
R^2	0.13	0.20	0.03	0.05

NOTE.—This table reports coefficient estimates from Equation 4. The dependent variable is either the log household income, the log credit score, an indicator for 12-month default, or an indicator for 24-month default. A loan is considered to be in default if it is 90 or more days past due on its payments (including charge-offs and repossessions). The sample is restricted to auto loans originated between January 2017 and December 2018. In Panel A, we report coefficient estimates for the full sample of auto loans. In Panel B, we restrict the sample to loans without subsidized financing. Vehicle fixed effects refer to vehicle make-model-condition combinations. t-statistics, presented below the coefficient estimates, are calculated by clustering at the lender level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Table 6: Vehicle Invoice and Sales Prices

	Invoice Price (1)	Invoice Price (2)	Sales Price (3)	Sales Price (4)
US Made × Post	0.011***	0.008*	0.007**	0.013**
	(2.76)	(1.67)	(2.25)	(2.18)
Data Source	Reg AB	Reg AB	Texas	Texas
Definition of US Made	Make	Make-Model	Make	Make-Model
Make × Model FE	Y	Y	Y	Y
Price Bin × Quarter FE	Y	Y	Y	Y
N	1,288,551	1,288,551	1,900,745	1,900,745
R^2	0.92	0.92	0.91	0.91

NOTE.—This table reports coefficient estimates from Equation 6. The dependent variable is either the log invoice price or log sales price. The sample is restricted to new vehicles purchased between 2017 and 2018 that meet the following conditions: (i) their price is between \$10,000 and \$100,000, (ii) their make-model is in both the Regulation AB II data and the Texas DMV data, both before and after the treatment date. In columns 1 and 2, the model is estimated using the Regulation AB II data. In columns 3 and 4, the model is estimated using the Texas DMV data. In columns 1 and 3, the indicator variable *US Made* is assigned at the vehicle make level, and it is equal to one if at least 50 percent of make m's vehicles in our sample are manufactured in the U.S., and zero otherwise. In columns 2 and 4, the indicator variable *US Made* is assigned at the vehicle make-model level, and it is equal to one if at least 50 percent of make-model $\tilde{m}$'s vehicles in our sample are manufactured in the U.S., and zero otherwise. Price bin fixed effects refer to \$25,000 price bins. t-statistics, presented below the coefficient estimates, are calculated by clustering at the make level (columns 1 and 3) or make-model level (columns 2 and 4). Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Table 7: Triple-Differences Regression: Incomes, Credit Scores, and Loan Amounts

	Interest Rate			12-Month Default		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	0.197**	0.153**	0.115	0.000	0.000	-0.001
	(2.41)	(2.34)	(1.08)	(-0.30)	(-0.14)	(-0.71)
Treated × Post × Low income	0.130**			0.000		
	(2.42)			(-0.24)		
Treated × Post × Low credit score		0.209*			0.000	
		(1.89)			(-0.82)	
Treated × Post × Low loan amount			0.237*			0.000
			(1.77)			(0.69)
Lender FE	Y	Y	Y	Y	Y	Y
Vehicle × Quarter FE	Y	Y	Y	Y	Y	Y
State × Quarter FE	Y	Y	Y	Y	Y	Y
Income × Quarter FE	Y	Y	Y	Y	Y	Y
Credit Score × Quarter FE	Y	Y	Y	Y	Y	Y
Cross-Sectional Cut × Quarter FE	Y	Y	Y	Y	Y	Y
N	1,973,067	1,973,067	1,973,067	1,973,067	1,973,067	1,973,067
R^2	0.70	0.71	0.71	0.03	0.03	0.03

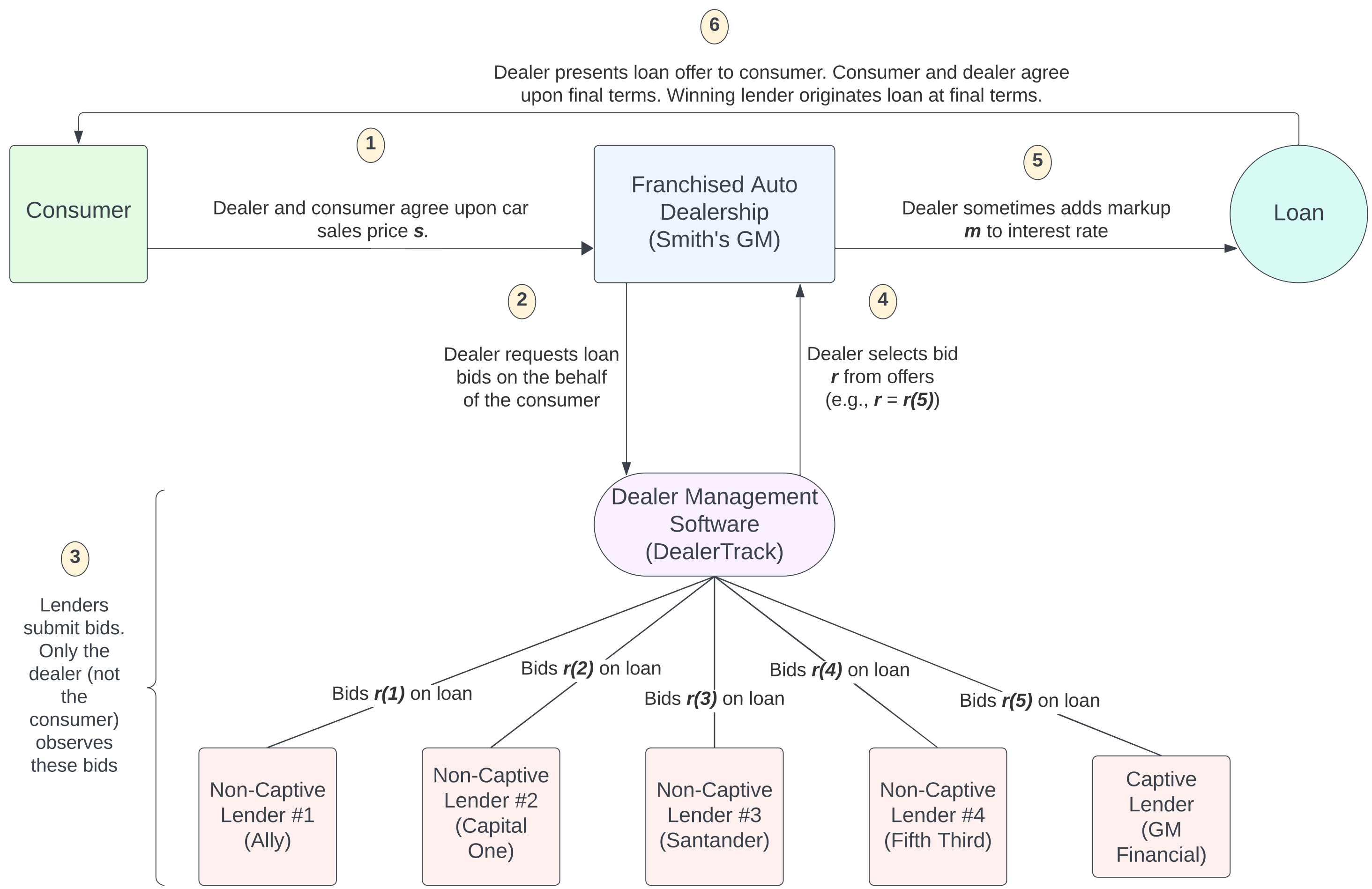
NOTE.—This table reports coefficient estimates from Equation 12. The dependent variable is either the interest rate or an indicator for 12-month default. A loan is considered to be in default if it is 90 or more days past due on its payments (including charge-offs and repossessions). The sample is restricted to auto loans originated between January 2017 and December 2018. Vehicle fixed effects refer to vehicle make-model-condition combinations. Cross-sectional cut fixed effects refer to either above- versus below-median income cuts (columns 1 and 4), above- versus below-median credit score cuts (columns 2 and 5), or above- versus below-median loan amount cuts (columns 3 and 6). t-statistics, presented below the coefficient estimates, are calculated by clustering at the lender level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Table 8: Triple-Differences Regression: Competition

	Interest Rate			12-Month Default		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	0.231**	0.213**	0.241**	-0.001	-0.001	0.001
	(2.40)	(1.99)	(2.15)	(-0.95)	(-1.16)	(0.92)
Treated × Post	0.054*			0.001		
× Low competition (median)	(1.89)			(1.24)		
Treated × Post		0.086**			0.001	
× Low competition (25th, 75th)		(2.08)			(1.15)	
Treated × Post			0.168**			0.001
× Low competition (10th, 90th)			(2.29)			(0.79)
Lender FE	Y	Y	Y	Y	Y	Y
Vehicle × Quarter FE	Y	Y	Y	Y	Y	Y
State × Quarter FE	Y	Y	Y	Y	Y	Y
Income × Quarter FE	Y	Y	Y	Y	Y	Y
Credit Score × Quarter FE	Y	Y	Y	Y	Y	Y
Competition × Quarter FE	Y	Y	Y	Y	Y	Y
N	1,973,067	1,024,049	369,238	1,973,067	1,024,049	369,238
R^2	0.70	0.70	0.69	0.03	0.04	0.04

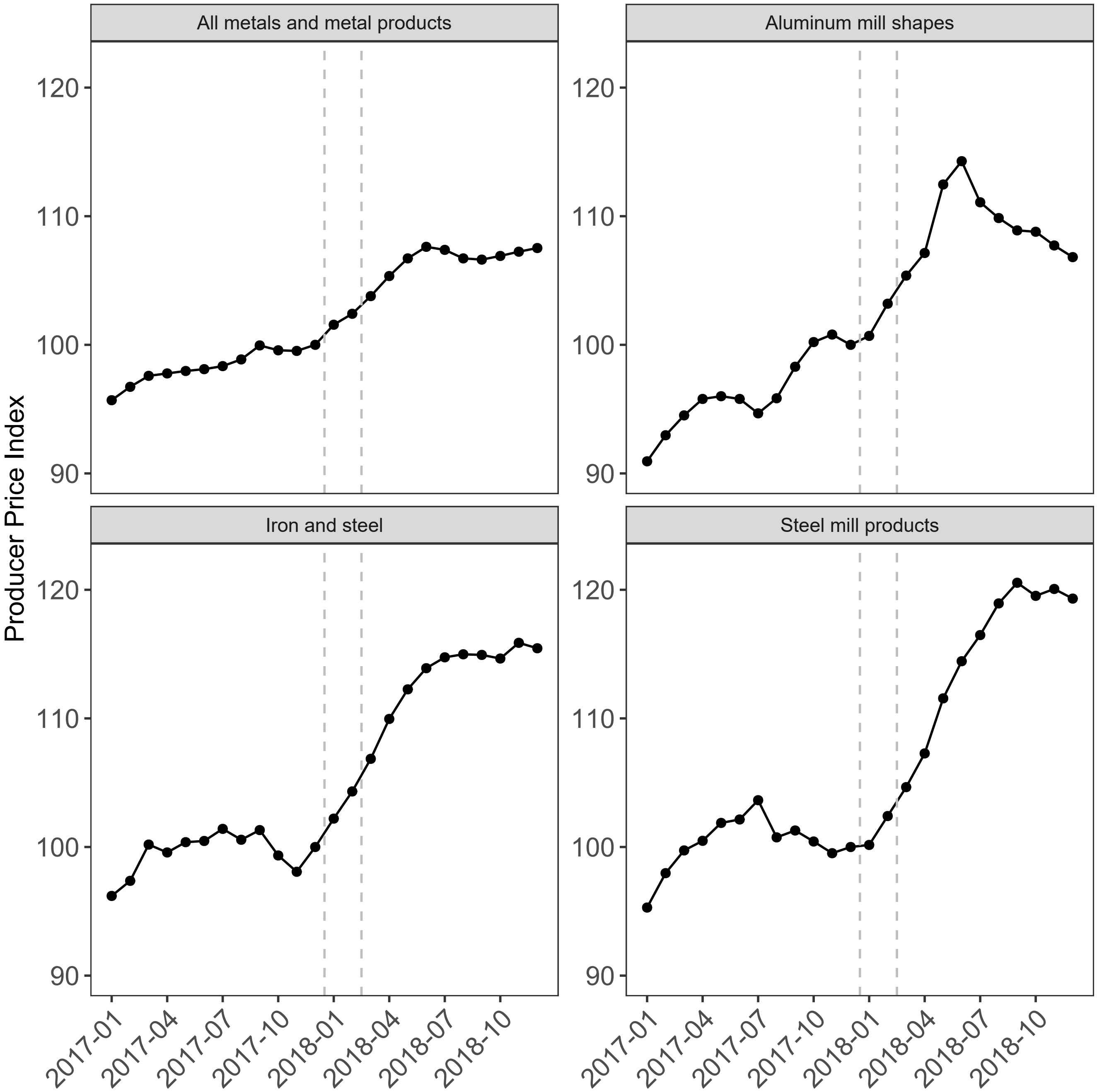
NOTE.—This table reports coefficient estimates from Equation 12. The dependent variable is either the interest rate or an indicator for 12-month default. A loan is considered to be in default if it is 90 or more days past due on its payments (including charge-offs and repossessions). The cross-sectional variable Low competition is calculated using pre-treatment lender market shares at the state level. The sample is restricted to auto loans originated between January 2017 and December 2018. In columns 2 and 4, we restrict the sample to loans in either the first or fourth quartile of competition. In columns 3 and 6, we restrict the sample to loans in either the first or tenth decile of competition. Vehicle fixed effects refer to vehicle make-model-condition combinations. Competition fixed effects refer to above- versus below-median (columns 1 and 4), first versus fourth quartile (columns 2 and 4), or first versus tenth decile (columns 3 and 6). t-statistics, presented below the coefficient estimates, are calculated by clustering at the lender level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Figure 1: Typical Indirect Auto Financing Process at a Franchised Auto Dealership



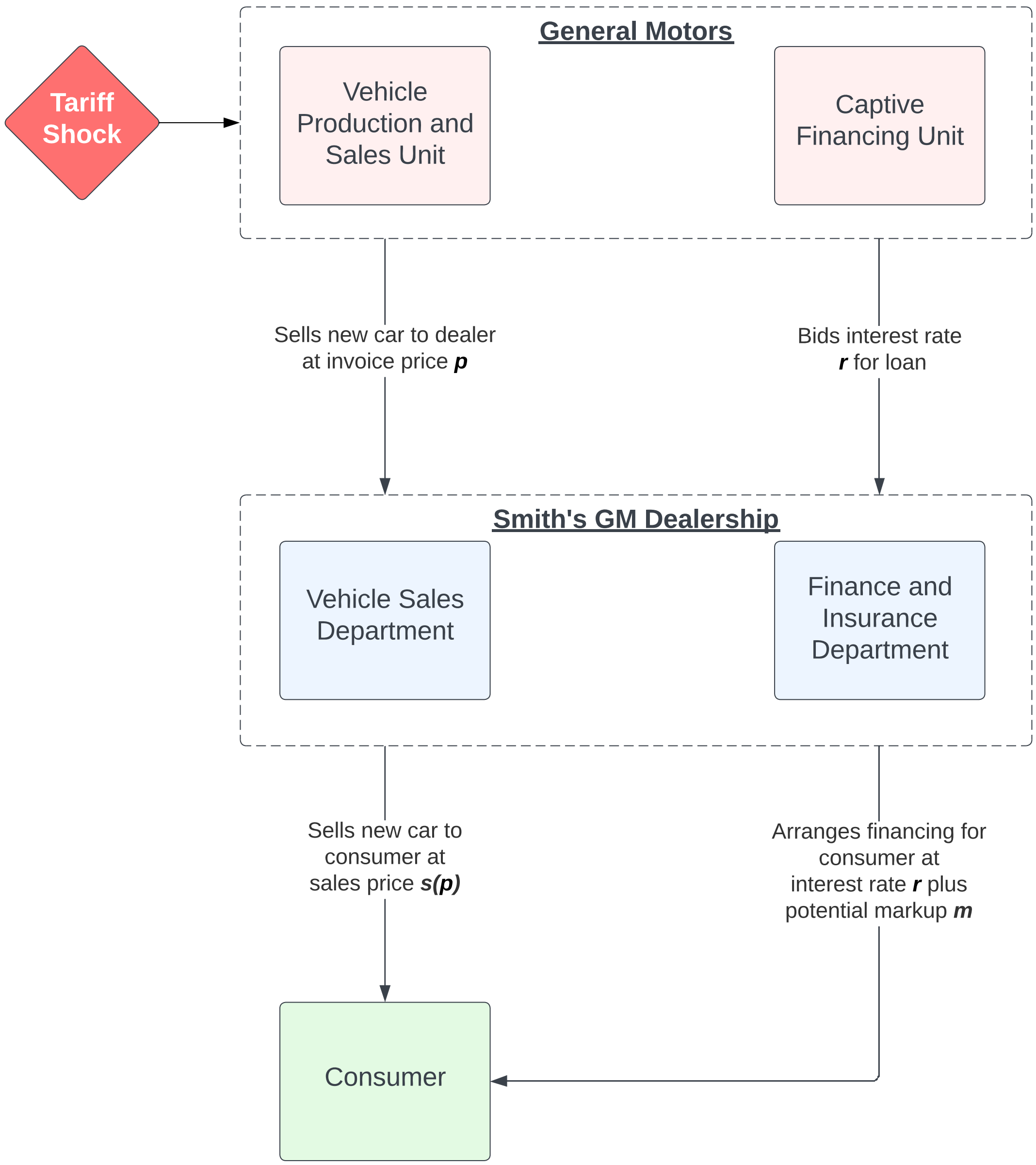
NOTE.——This figure contains a flowchart of the typical indirect auto financing process at a franchised auto dealership. The process begins in the top-left corner with the consumer selecting a car and agreeing upon a sales price and other terms with the dealer. Next, if the consumer requires financing for the transaction, then the dealer solicits loan offers from both the manufacturer's captive lender and several non-captive lenders. Lenders interested in financing the transaction then submit bids, and the dealer then selects a bid from the set of offers. Afterwards, the dealer presents the loan offer to the consumer, oftentimes adding a markup to the loan's interest rate. If the consumer and the dealer agree upon the final loan terms, then the dealer originates the loan and sells it back to the winning lender.

Figure 2: Metals Prices Around the 2018 Tariffs



NOTE.——This figure plots scaled metals prices around the date of the metals tariffs. Prices are sourced from Bureau of Labor Statistics Commodity PPI data. For each series, prices are scaled to 100 as of December 2017. The vertical dashed lines correspond to January 2018 and March 2018.

Figure 3: Relationship Between Auto Manufacturer, Dealership, and Consumer



NOTE.——This figure describes the relationship between an auto manufacturer (General Motors), one of its franchised dealerships (Smith's GM Dealership), and the consumer. The auto manufacturer sells new cars to the dealer at invoice prices p, which the dealer in turn sells to consumers at negotiated sales prices $s(p)$. For consumers that require financing for their transactions, the auto manufacturer's captive lending subsidiary bids interest rates of r to finance the purchases, and it originates the loans if the dealer selects its offers (see also Figure 1). The tariff shock, which is shown on the upper left-hand corner, increases the manufacturer's vehicle production costs. The manufacturer has the option of passing on these costs by raising its invoice prices p (which may lead to a higher sales price for consumers) or by raising its captive financing rates r.

Figure 4: Financial Statement Data From General Motors

Year Ending December 2017 (Pre-Tariff):

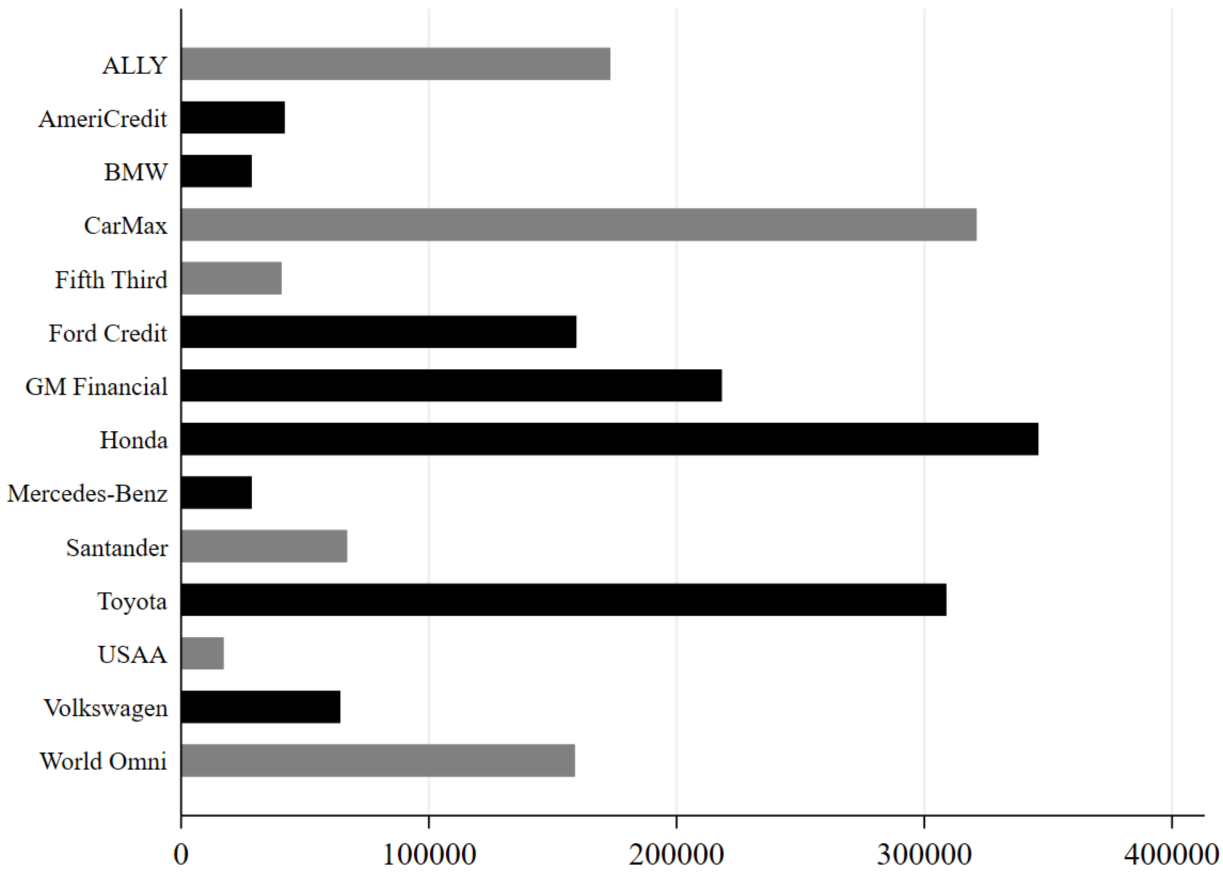
	Total Automotive (1)	GM Financial (2)
Net sales and revenues	$133,607	$12,151
Earnings (loss) before interest and taxes	$12,268	$1,196

Year Ending December 2018 (Post-Tariff):

	Total Automotive (1)	GM Financial (2)
Net sales and revenues	$133,143	$14,016
Earnings (loss) before interest and taxes	$10,622	$1,893

NOTE.——This figure displays GM's revenues and earnings in the year before the tariffs (2017) and the year of the tariffs (2018). Revenues and earnings are split between GM's vehicle sales segment (Total Automotive) and GM's captive financing segment (GM Financial).

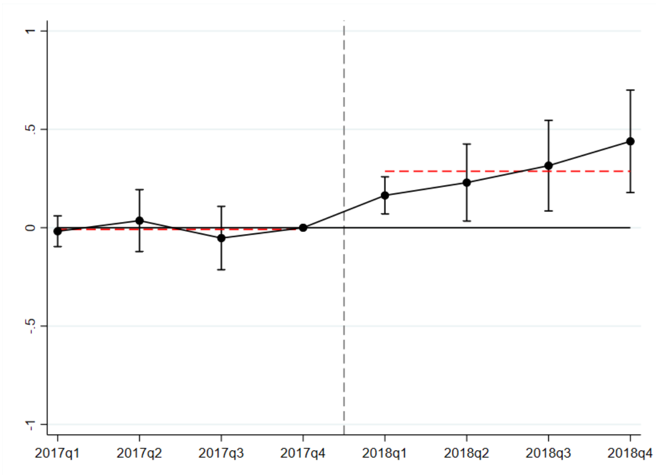
Figure 5: Distribution of Loans Across Lenders



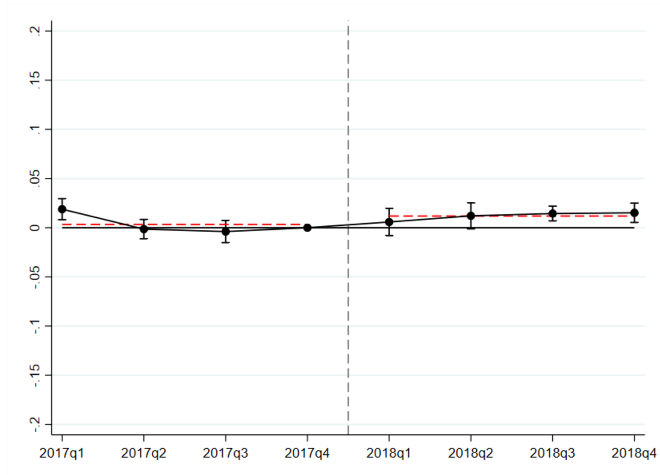
NOTE.——This figure plots the distribution of loans across lenders for our sample. The black bars correspond to captive lenders, and the gray bars correspond to non-captive lenders. The x-axis corresponds to the number of loans in our sample for each lender.

Figure 6: Auto Loan Terms

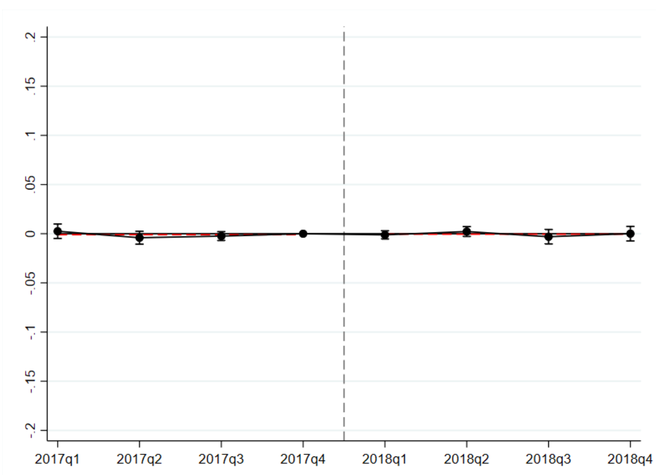
(a) Interest Rate



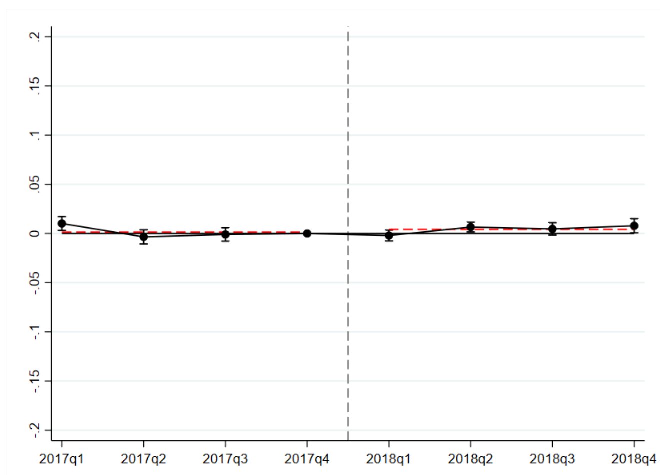
(b) Loan Amount



(c) Loan Maturity



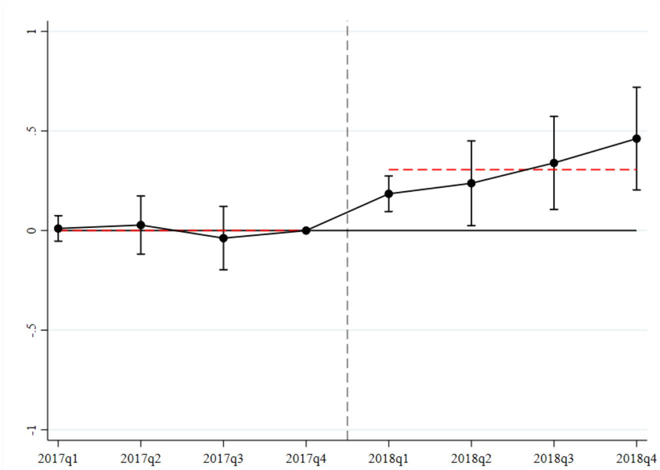
(d) Loan-to-value



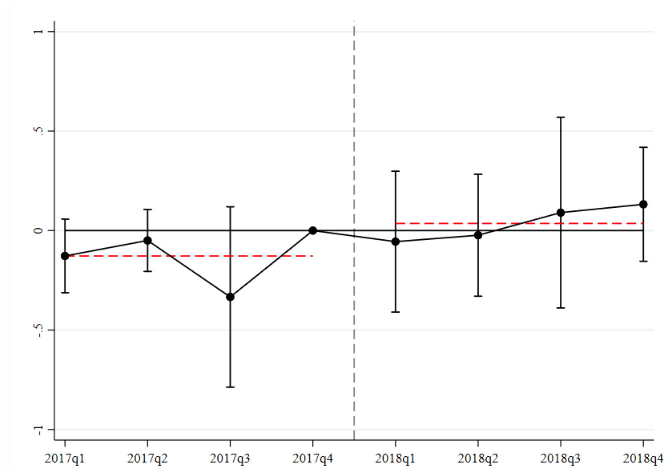
NOTE.——This figure plots coefficient estimates from Equation 3. The dependent variable is either the interest rate, log loan amount, log loan maturity, or loan-to-value ratio. The x-axis corresponds to the number of quarters from the treatment date. The quarter $\tau = -1$ is the reference quarter. The circles correspond to the coefficient estimates, and the vertical bars correspond to 95 percent confidence intervals. The dashed red lines correspond to the pre-treatment and post-treatment averages of the coefficient estimates. The sample is restricted to auto loans originated between January 2017 and December 2018 that do not have subsidized financing. Standard errors are clustered at the lender level.

Figure 7: Alternative Specifications: Interest Rates

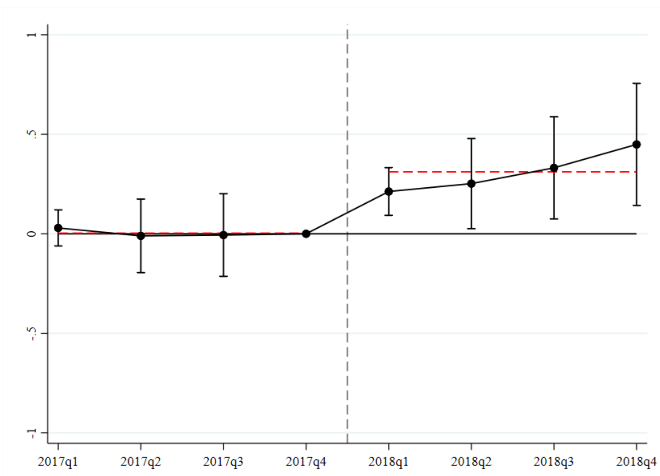
(a) More-Exposed Captives vs. Non-Captives



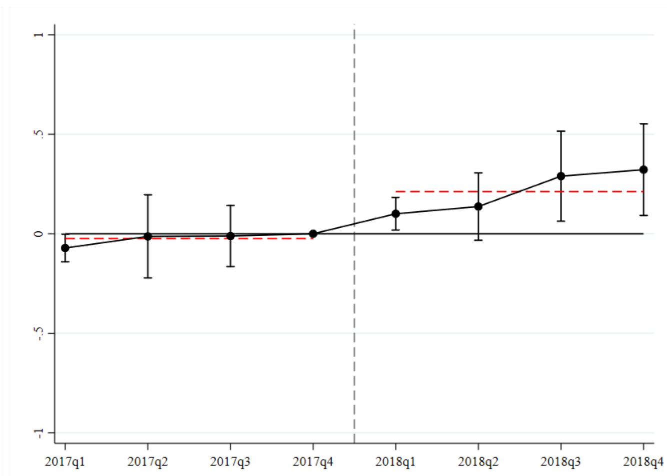
(b) Less-Exposed Captives vs.. Non-Captives



(c) Captives vs. Non-Captives with Risk-Free Controls



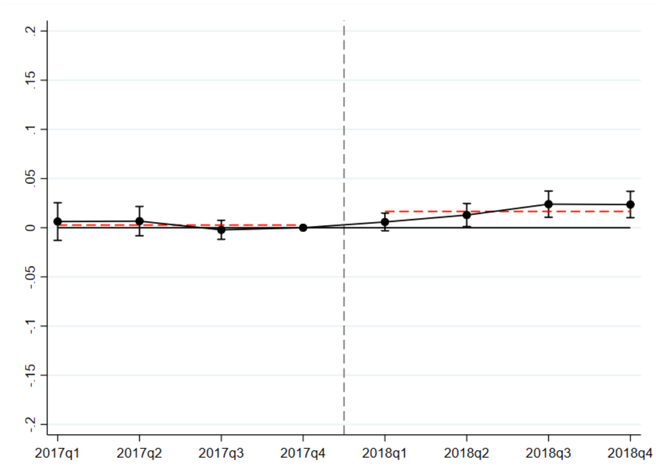
(d) Captives vs. CarMax



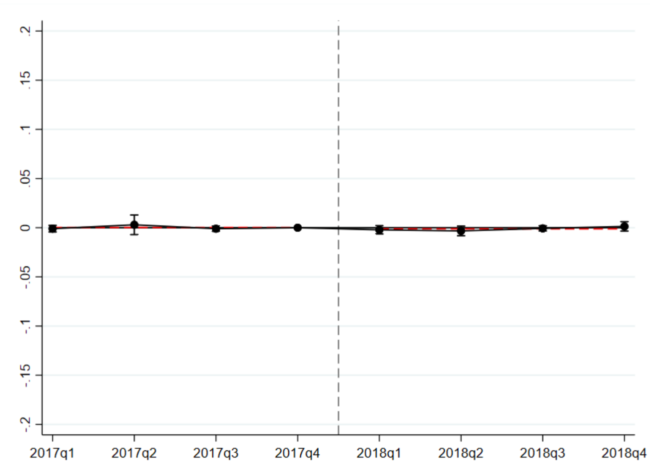
NOTE.——This figure plots coefficient estimates from Equation 3. The dependent variable is the interest rate. The x-axis corresponds to the number of quarters from the treatment date. The quarter $\tau = -1$ is the reference quarter. The circles correspond to the coefficient estimates, and the vertical bars correspond to 95 percent confidence intervals. The dashed red lines correspond to the pre-treatment and post-treatment averages of the coefficient estimates. The sample is restricted to auto loans originated between January 2017 and December 2018 that do not have subsidized financing. Standard errors are clustered at the lender level. Panel A uses more-exposed captive lenders as the treated sample and non-captives as the control sample. Panel B uses less-exposed captive lenders as the treated sample and non-captives as the control sample. Panel C uses captives as the treated sample and non-captives as the control sample, and includes captive-specific controls for changes in risk-free interest rates. Panel D uses captives as the treated sample and just CarMax as the control sample.

Figure 8: Borrower Characteristics

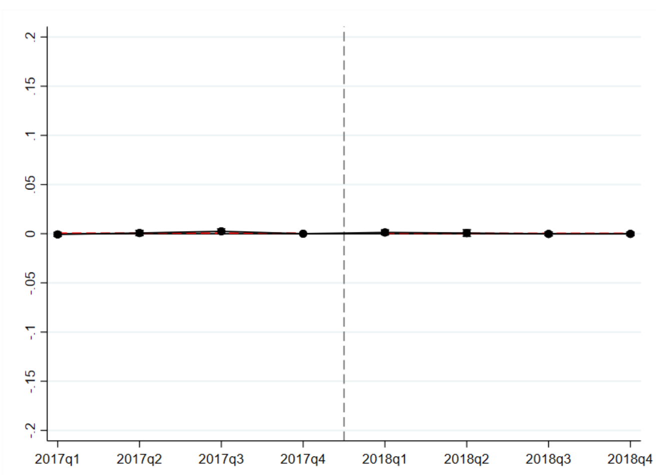
(a) Income



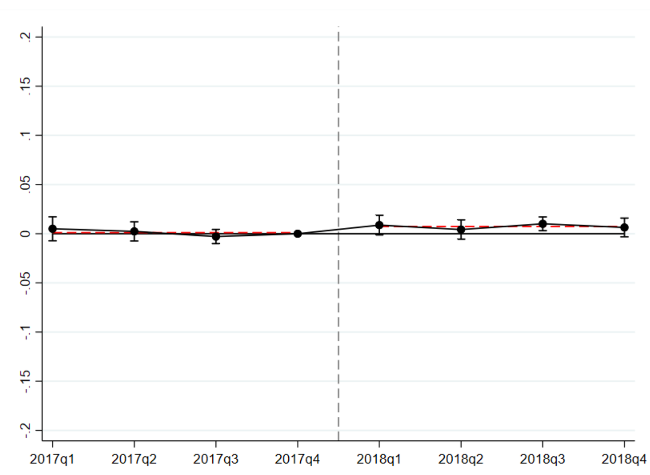
(b) Credit Score



(c) 12-Month Default



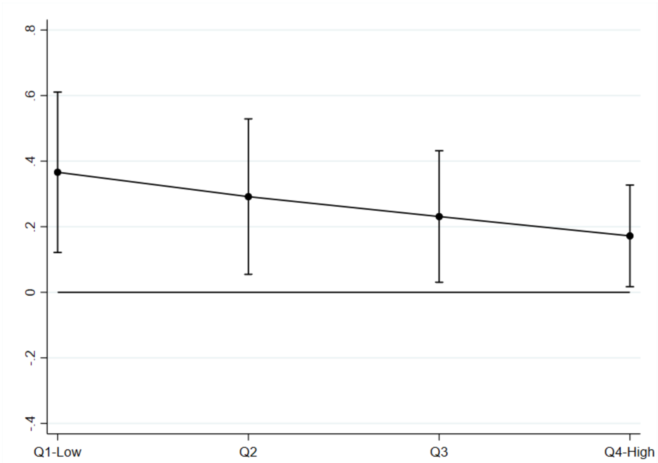
(d) Vehicle Value



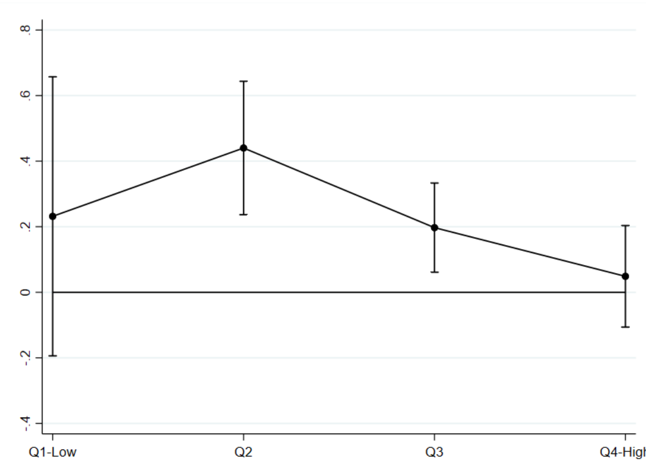
NOTE.——This figure plots coefficient estimates from Equation 3. The dependent variable is either the log household income, the log credit score, an indicator for 12-month default, or the log vehicle value. A loan is considered to be in default when is 90 or more days past due on its payments (including charge-offs and repossessions). The x-axis corresponds to the number of quarters from the treatment date. The quarter $\tau = -1$ is the reference quarter. The circles correspond to the coefficient estimates, and the vertical bars correspond to 95 percent confidence intervals. The dashed red lines correspond to the pre-treatment and post-treatment averages of the coefficient estimates. The sample is restricted to auto loans originated between January 2017 and December 2018 that do not have subsidized financing. Standard errors are clustered at the lender level.

Figure 9: Heterogeneous Effects Across Incomes, Credit Scores, and Loan Amounts

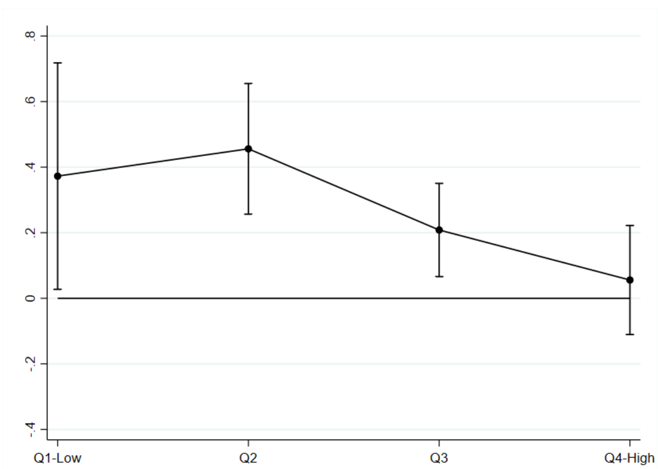
(a) Income



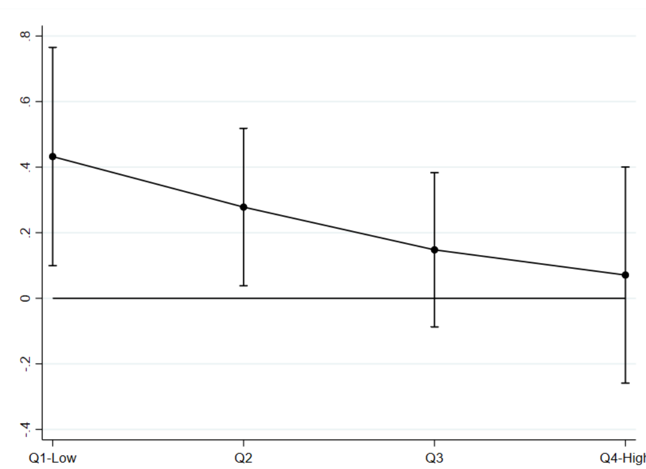
(b) Credit score



(c) Credit score (exlcluding AmeriCredit)



(d) Loan size



NOTE.——This figure plots coefficient estimates from the following regression model:

$$r_{i,l,v,s,w,c,t} = \alpha + \sum_{q=1}^{4} \left(\beta_q^b \cdot \text{Quartile}_{q,i}^b \cdot \text{Treated}_l \cdot \text{Post}_t + \theta_q^b \cdot \text{Quartile}_{q,i}^b \cdot \text{Treated}_l \right)$$
$$+\Gamma \cdot \text{Treated}_l \cdot \text{Post}_t + \delta_l + \delta_{v,t} + \delta_{s,t} + \delta_{c,t} + \delta_{\text{Quartile}^b,t} + \varepsilon_{i,l,v,s,w,c,t}$$

where the dependent variable, $r_{i,l,v,s,w,c,t}$, is the interest rate on loan i originated in quarter t. The indicator variable $\text{Quartile}_{q,i}^b$ is equal to one if loan i belongs to quartile q for borrower characteristic b, and zero otherwise. We examine three borrower characteristics: incomes (Panel A), credit scores (Panels B and C), and loan amounts (Panel D). The x-axis corresponds to quartiles $q = 1$ to $q = 4$. The circles correspond to the coefficient estimates for the β_q^b's, and the vertical bars correspond to 95 percent confidence intervals. The sample is restricted to auto loans originated between January 2017 and December 2018. In Panel C, we remove AmeriCredit loans from the sample. Standard errors are clustered at the lender level.

Internet Appendix

A Additional Sample Details and Supplemental Analyses

A.1 Additional Sample Details

A.1.1 Market Shares of Captive and Non-Captive Lenders

Our final sample consists of 1,973,639 auto loans from 8 captive lenders and 6 non-captive lenders. Loans from captive lenders make up around 60 percent of the sample, and loans from non-captive lenders making up the remaining 40 percent. Consistent with the population patterns described in Section 2, we find that captive lenders have greater market share for new vehicles (76 percent) than used vehicles (32 percent), and vice versa for non-captive lenders.[56]

Although captive lenders specialize in financing their manufacturer's brands of vehicles (e.g., Hondas and Honda Finance), non-captive lenders also manage to acquire significant market share in these brands. Specifically, for the 87 percent of vehicle brands that have an in-house captive lender in our sample, non-captive lenders have a 30 percent overall market share, which rises (falls) to 56 percent (20 percent) for used (new) versions of these vehicles. Non-captive lenders tend to dominate the market for brands of vehicles that do not have an in-house captive lender. Indeed, for the 13 percent of vehicle brands that do not have an in-house captive lender in our sample, non-captive lenders have a 96 percent overall market share, which is persistent across both new and used versions of these vehicles. Table IA.19 provides a full list of the vehicle brands in our sample. This table also reports in-sample captive market shares for each brand and whether each brand has an in-house captive lender in our sample.

Defining a vehicle at the make-model-condition level, we find that around 98 percent of the loans in our sample are for vehicles that have both captive and non-captive lending options.[57] These are the loans from which our main source of identifying variation comes from, as most of our regressions include various forms of vehicle make-model-condition fixed effects. Given the large degree of overlap between this subsample and our main sample, it is not surprising that the market shares of captive and non-captive lenders are similar between them (e.g., 39 percent versus 40 percent for non-captive lenders). Moreover, as shown in Table IA.20, average pre-treatment lending conditions are almost identical across these samples.

A.1.2 Loan Price Determinants

As shown in Table 2, captive interest rates tend to be lower than non-captive interest rates even after controlling for vehicle and borrower characteristics. One reason for this gap is that a much larger fraction of captive loans are subvented than non-captive loans. Indeed, if we remove subvented loans from the sample, then the conditional distributions of captive and non-interest rates are much

56. Note that these in-sample market shares are different than the population market shares reported in Section 2. This is because captive lenders are over-represented in the Regulation AB II data, as the data excludes numerous smaller non-captive lenders that do not access public securitization markets. See Section 3.1.1.

57. These are vehicles for which there are both captive and non-captive loans in our sample, regardless of whether the captive loan is from the vehicle's in-house captive lender or another captive. The difference between the 98 percent of vehicles that have both captive and non-captive lending options and the 87 percent of vehicles that have their own in-house captive lenders comes from the fact that some captive lenders (in particular, GM-AmeriCredit) sometimes finance vehicles from other manufacturers. We note that this phenomenon is much more pronounced in the used car market than the new car market, as franchised dealers sometimes acquire and resell off-brand used vehicles and solicit captive financing for them.

closer to one another. See Figure IA.4 and IA.5, which plot the distributions of non-subvented captive and non-captive interest rates for used and new vehicles across borrower characteristics.[58]

A.2 Robustness Tests

A.2.1 Assumptions About Standard Errors

Table IA.21 examines whether our results are robust to different assumptions about our standard errors. We find that our main results are unchanged if we calculate our standard errors using other forms of clustering – such as state clustering, vehicle make-model-condition clustering, and ABS clustering – or using a wild bootstrap procedure with lender clustering (Cameron, Gelbach, and Miller 2008).

A.2.2 Choice of Fixed Effects

Table IA.22 examines whether our results are robust to including more granular versions of our baseline fixed effects. The purpose of this test is to rule out more nuanced concerns about our identification, such as whether our results capture the heterogeneous impact of other contemporaneous tariffs across states with different manufacturer market shares (i.e,. a manufacturer × state × time omitted variable). Inconsistent with the presence of various correlated omitted variables driving our results, we find that the magnitudes of our estimates do not change much when we include more granular versions of our baseline fixed effects (Oster 2019).

A.2.3 Controlling for Other Loan Terms

Table IA.23 re-estimates our baseline interest rate model after controlling for other co-determined loan terms such as loan amounts, maturities, and loan-to-value ratios. We continue to find that captive interest rates increased in response to the tariffs. Among other things, this result helps reinforce that our baseline estimates capture tariff pass-through and not borrower-level adjustments to worse loan terms (Argyle, Nadauld, and Palmer 2020).

A.2.4 Choice of Treatment Date

As mentioned in Section 3.2, our choice of January 2018 as the treatment date is conservative as it reflects the date of the Department of Commerce's initial recommendation to impose the metal tariffs. We find similar results if we instead use February 2018 or March 2018 as the treatment date, as shown in Table IA.5. The fact that our results are robust to small changes in the treatment date is not surprising given that Figure 6 shows that interest rates rose more during the later parts of the sample period when the tariffs were more binding and metals prices had risen more.

58. This is especially the case for the prime segment of the market, which is the segment of the market that captive lenders typically focus on. In fact, while captive lenders do tend to offer significantly lower (non-subvented) interest rates than non-captive lenders in the subprime segment of the market, they are much less willing to lend to these types of borrowers in the first place.

A.2.5 Choice of Sample Period

Figure IA.6 plots the coefficient estimates from Equation 3 after extending the the sample period to 2019. There are two main takeaways from the figure. First, there was a temporary decline in the effect on interest rates in Q2-Q3 2019, which is when the U.S. temporarily exempted some countries from the steel and aluminum tariffs. This effect then reverted to its prior level in Q4 2019 after the President announced plans to reinstate the tariffs on some of these countries and increase them on others. Second, the terminal coefficient estimate for the fourth quarter of 2019 is 50 basis points, which is almost double our pooled coefficient estimate of 26 basis points in Table 3. Thus, although our 26 basis point estimate might be representative of the average effect of the tariffs during the sample period, it might significantly understate the long-run effects of the tariffs going forward.[59]

A.2.6 Choice of Sample Filters

Table IA.2 re-estimates our baseline interest rate model after adjusting several of the sample filters listed in Section 3.2. Specifically, columns 1 and 2 adjust the credit score filters, columns 3 and 4 adjust the level of winsorization, column 5 extends the sample period to 2019, column 6 restricts the sample period to before the retaliatory tariffs from China, and column 7 removes the loan-to-value ratio filters. For all these cases, we continue to find that captive interest rates increased relative to non-captive interest rates following the tariffs.[60]

Table IA.24 re-estimates our baseline interest model after including the five lenders that we previously excluded in Section 3.2. Similarly, Table IA.6 re-estimates the same model but after removing World Omni from the sample (see Footnote 27). In both cases, we find that our main results persist.

A.2.7 Placebo Analyses

To strengthen our claim that the metal tariffs primarily drove the differential increase in captive interest rates in 2018, we conduct two placebo analyses using only auto loans originated between 2015 and 2017.[61] Table IA.25 reports the coefficient estimates from Equation 2 for this sample after we redefine Post_t to be equal to one for loans originated in 2017, and zero otherwise. Consistent with our main results capturing the causal effects of the 2018 tariffs, we find no differential changes in captive lending rates during the placebo periods. Moreover, while our baseline estimates in Table 3 are positive and economically significant, our placebo estimates in Table IA.25 are mostly negative and economically small.

59. For reference, we find that captive interest rates increased by 29 basis points, on average, when we re-estimate Equation 2 on the extended sample period. This is similar to our baseline estimate of 26 basis points in Table 3.

60. The fact that captive interest rates remained elevated in 2019 is inconsistent with an alternative explanation that centers on wholesale vehicle prices being difficult to adjust in the short-run due to purchase contracts with dealers / MSRP price stickiness (and hence incapable of offsetting higher input costs).

61. Our data contains significantly fewer loans originated in 2015 than 2016. This is because the Regulation AB II reporting requirements only apply to public auto loan ABS issued after November 2016 and it is uncommon for ABS issuers to include very seasoned loans (e.g., older than 18 months) in their ABS offerings.

A.2.8 Negative Sample Weights

Given that treatment occurs all at once in our setting (i.e., it is not rolled out in a staggered manner over time), there is no particular reason to be concerned about potential biases arising from time-heterogeneous treatment effects (de Chaisemartin and D'Haultfoeuille 2020). Nevertheless, to further assuage this concern, we follow de Chaisemartin and D'Haultfoeuille 2020 and calculate the group-time weights used to construct our baseline difference-in-differences estimates. As shown in Figure IA.7, we find that over 95 percent of the group-time weights are positive, and that the sum of the negative group-time weights is only -0.007. This small number of (and size of) negative group-time weights helps rule out the principal concern raised in de Chaisemartin and D'Haultfoeuille 2020, which centers around the interaction of large negative weights and heterogeneous treatment effects.

A.3 Loan Originations and Vehicle Choices

To document some of the costs that captive lenders face when raising their interest rates, we start by examining how the tariffs impacted captive loan origination volumes. The model is:

$$y_{f,s,v,t} = \alpha + \Gamma \cdot \text{Treated}_f \cdot \text{Post}_t + \delta_f + \delta_{s,t} + \delta_{v,t} + \varepsilon_{f,s,v,t}, \tag{13}$$

where the outcome variable is the logged number of loans that captive lenders ($f = 1$) or non-captive lenders ($f = 0$) originated in quarter t in state s for vehicle make-model-condition v.[62] Table IA.1 reports the coefficient estimates from the model. Relative to non-captive lenders, captive lenders experienced a 6.7 percent decline in loan originations following the tariffs. Given that captive interest rates rose 10 percent in response to the tariffs (= 26 basis points / 252 basis points), the implied interest rate elasticity of extensive margin loan demand is -0.67 (-6.7 / 10.0). This estimate of the interest rate elasticity is consistent with other estimates in the auto loan literature, which range from -0.00 in Attanasio, Goldberg, and Kyriazidou 2008 to -0.10 in Argyle, Nadauld, and Palmer 2020 and -0.94 in Argyle, Nadauld, and Palmer 2023.

Before we proceed, we highlight three important aspects of the above results. First, while our level of aggregation in Equation 13 follows Benneton, Mayordomo, and Paravisini 2022, Table IA.1 shows that our results are robust to other levels of aggregation, such as at the captive × state × income bin × credit score bin × quarter level. Second, although data limitations prevent us from discerning the extent to which the decline in captive loan originations comes from fewer vehicle sales versus lower loan penetration rates, the findings in Gavazza and Lanteri 2021 and Argyle, Nadauld, and Palmer 2023 suggest that both margins are likely active. Third, the decline in captive loan originations does not contradict the absence of borrower composition effects in Table 5. Indeed, both Argyle, Nadauld, and Palmer 2020 and Argyle, Nadauld, and Palmer 2023 find that loan originations decline in response to higher offered interest rates, and that the decline in originations is not correlated with observable borrower characteristics or future default rates.

62. To better account for the count-data structure of the number of loan originations, column 2 in Table IA.1 re-estimates Equation 13 using a Poisson model (Cohn, Liu, and Wardlaw 2022). For both our linear and Poisson models, we use hetroskedasticity-robust standard errors to conduct statistical inference. We do so because we cannot cluster our standard errors at the captive level, as there are just two clusters along this dimension. Our results are robust to alternative methods of computing the standard errors, including clustering at the captive × state × vehicle level ($t = -15.17$) and using a bootstrap procedure ($t = -10.45$).

Another potential cost that captive lenders face when raising their interest rates is that borrowers might substitute towards less profitable vehicles (Gulati, McAuslan, and Sallee 2017; Argyle et al. 2021; Argyle, Nadauld, and Palmer 2023). To examine the effect of the tariffs on vehicle choices, we re-estimate Equation 2 after making two changes. First, we use vehicle values as our outcome variable instead of interest rates or other loan terms. Second, we relax our vehicle fixed effects so that we no longer control for demand-side purchasing responses to the tariffs. If substitution is present in our setting, then we should expect that average vehicle values will decline for captive loans relative to non-captive loans. However, as shown in Table IA.26, we find no differential changes in average vehicle values for captive loans following the tariffs. Although this test is imperfect because we do not observe the sales price, it suggests that captive borrowers did not fully offset the effects of the tariffs through their vehicle choices.

B Calculations for Tariff Pass-Through

This appendix provides more details about our pass-through calculations in Section 4.6. First, we elaborate on how we estimate ΔP, M, N, and ΔV. Afterwards, we present a range of estimates for ΔC.

B.1 Financing Costs

To estimate ΔP, we follow the approach used in Argyle, Nadauld, and Palmer 2023. Discounting at 5 percent, for a pre-treatment average captive loan with a principal of \$26,914 and a maturity of 66 months, a 26 basis point increase in captive interest rates from 2.52 percent to 2.78 percent corresponds to a present value increase in total loan payments of \$179. If we also incorporate a 7 basis point spillover effect, then this estimate rises to \$227 per captive loan

B.2 Captive Loan Penetration Rate

To estimate M, we first rewrite it as follows:

$$M = \frac{F_n + F_u}{N_n \cdot 0.9^{-1}} = 0.90 \cdot \left(\frac{F_n}{N_n} + \frac{F_u}{N_n} \right),$$

where F_n is the number of captive loan originations for new cars, F_u is the number of captive loan originations for used cars, N_n is the number of new cars that are financed, and 0.90 is the fraction of new cars that are financed relative to the number of new cars sold in the population (Butler, Mayer, and Weston 2023). Next, we rewrite the ratio of F_u to N_n as follows:

$$\frac{F_u}{N_n} = \frac{F_u}{N_u} \cdot \frac{N_u}{N_n},$$

where N_u is the number of used cars that are financed. From Experian 2021, we know that $F_n/N_n = 0.55$, $F_u/N_u = 0.07$, and $N_u/N_n = 1.50$. Therefore, we have that the captive loan penetration rate is $M = 0.90 \cdot (0.55 + 0.07 \cdot 1.50) = 0.59$.

B.3 Number of Vehicles Sold

From the U.S. Department of Transportation 2021, there are around $N = 17$ million new vehicles sold in the U.S. each year. For reference, there are around 50 million new and used vehicles sold per year.

B.4 Vehicle Prices

As shown in Table 6, we estimate that new vehicle sales prices rose 0.7 percent in response to the tariffs. Multiplying this by the pre-treatment mean sales price of \$32,206 for the sample, we estimate that $\Delta V = \$225$.

B.5 Production Costs

Estimating ΔC is highly difficult because granular data on auto manufacturers' costs is generally not available. Dawson and Colias 2018 illustrate the challenges involved with estimating ΔC by writing, "Tariff-related costs are raising expenses and squeezing profits for big and small auto-industry players, and driving some companies to fight their partners over who pays...A typical vehicle is made up of roughly 30,000 individual parts, and car companies on average work with hundreds of suppliers at once for each model line, either buying components directly or contracting them out further down the chain...Sorting out the cost of tariffs is difficult because some parts cross the U.S. border multiple times before being installed in a car, blurring the lines of what is 'domestic' content. And although much of the steel used in car manufacturing is American-made, the auto industry is still paying more because a new 25% tariff imposed in June on imports prompted domestic steelmakers to increase prices by an equivalent amount."

Given the difficulty of this problem, estimating ΔC requires us to make several assumptions that cannot be easily verified in the data, such as that the entire increase in steel prices (and, subsequently, manufacturers' costs) was due to the tariffs. Below, we present three methods for estimating ΔC which suggest that average production costs per new vehicle rose between $200 and $700 following the tariffs.[63] However, we caution that these estimates are fairly speculative, which is one reason why we primarily focus on comparing the relative importance of interest rate and vehicle price pass-through in Section 4.6.

B.5.1 Ford Method

Ford's 2018 10-K cites $750 million in additional tariff-related costs in North America. Given that Ford sold 2,540,000 new vehicles at wholesale to North American dealerships in 2018, this implies an average cost increase of $295 per vehicle.

B.5.2 Media Mentions Method

1. Lobosco 2019 states, "Automakers, for example, have said the tariffs have driven up the cost of production in the United States by $400 per vehicle. "

2. Center for Automotive Research 2019 states, "The price of the average vehicle sold in the United States could rise...by slightly more than USD 350, depending on which policies are enacted."

3. Panzino 2019 states, "Mike Manley, CEO of Fiat Chrysler Automobiles NV, said on Jan. 14 that U.S. metal tariffs are projected to raise the company's 2019 costs by $300 million to $350 million, Reuters reported. The automaker confirmed the numbers to S&P Global Market Intelligence, which translate to a price increase of about $135 or $160 per vehicle."

4. Tax Foundation 2019 states, "Ford and General Motors estimated that the tariffs cost them about $1 billion each the first year they were in effect—roughly $700 per vehicle produced."

63. We note that there are some estimates in the popular press of potential tariff costs to vehicle manufacturing which are much larger than ours (e.g., Higgins 2018). However, these larger estimates refer to a hypothetical vehicle import tariff that was never enacted, and not the steel and aluminum tariffs that we examine.

B.5.3 Weight-Based Method

Another method of estimating the average cost increase from steel and aluminum inputs per vehicle is to look at their contributions to vehicle weight. This is similar to the method used in Flaaen, Hortacsu, and Tintelnot 2020 to select ranges as their control group for washing machines.

The first step in this process is to figure how much steel and aluminum (in tons) goes into the average vehicle. According to Experian 2021, around 40 percent of vehicles are sedans and the rest are non-sedans, such as trucks and SUVs. The average weight of a sedan is around 1.5 tons, and the average weight of a non-sedan is around 2.5 tons. Thus, the average vehicle weighs around 2.1 tons. Steel accounts for around 55 percent of the average vehicle's weight and aluminum accounts for around 15 percent. Therefore, the average vehicle is comprised of around 1.16 tons of steel and 0.32 tons of aluminum.

The second step in this process is figuring out the cost of 1.16 tons of steel and 0.32 tons of aluminum in 2017 (i.e., prior to the tariffs). According to the Department of Commerce 2018, the average cost of steel was \$684 per ton in 2017, and the average cost of aluminum was \$2,200. This implies that the average cost of steel per vehicle was around \$790 in 2017, and the average cost of aluminum per vehicle was \$693 (for a total combined cost of \$1,483).

The third and final step is to then calculate how much these input costs change in response to the tariffs. Suppose that steel prices rose 20 percent in response to the tariffs and aluminum prices rose 10 percent. Then the increase in steel costs per vehicle would have been \$158, and the increase in aluminum costs per vehicle would have been \$69. Thus, our estimate of ΔC using this weight-based method is \$227 per vehicle. We note this is likely an underestimate given that it does not account for a variety of inputs in the manufacturing process that also use steel and aluminum, such as outsourced auto parts.

C Spillover Effects

This first part of this appendix introduces an imperfect competition model of the auto loan market in the spirit of Salop 1979 and Berg et al. 2021. There are two main insights from the model:

1. In response to a cost shock to captive lenders, both captive and non-captive lenders raise their loan prices. This effect arises due to competitive interactions between captive and non-captive lenders and the particular form of consumer demand assumed in the model. The main implication of this finding is that researchers must take into account the responses of both captive and non-captive lenders when measuring the aggregate effects of the cost shock.

2. The total effect of a cost shock on captive loan prices can be deconstructed into a direct effect p^d that is specific to captive lenders and a spillover effect p^s that is common to both captive and non-captive lenders. While the direct effect can be estimated using a difference-in-differences model that compares the loan prices of captive and non-captive lenders before-and-after the cost shock, the spillover effect cannot as it is absorbed into the common time trend. The model predicts that the spillover effect will be equal $p^s = p^d \cdot \bar{d}$, where $\bar{d}$ is the market share of captive lenders. The total effect on captive loan prices is $p^t = p^d + p^s = p^d \cdot (1 + \bar{d})$.

The second part of this appendix uses both the above model and a separate data-driven procedure to estimate the average spillover effect on non-captive lenders. We estimate an average spillover effect of 6.26 basis points using our data-driven procedure, which is almost identical to our model-based estimate of 7 basis points.

C.1 Model Setup

There are $i = 1, \ldots, n$ lenders located equidistant around a unit circle offering auto loans at prices p_i. There is also a unit mass of consumers uniformly distributed around the circle. The location of the lenders represents various non-price aspects of their loan offers – e.g., the convenience of doing business with the lender, the willingness of the lender to underwrite high LTV loans, etc. The location of the consumers represents their preferences for these non-price loan characteristics.[64]

C.1.1 Consumers

If a consumer is located at z and selects a loan from lender i located at z_i, then their net utility is $v - p_i - t \cdot |z - z_i|$, where v is the private value of the loan to the consumer and t is a cost of deviating from the ideal non-price loan features. We assume that v is large so that all consumers select an auto loan instead of purchasing the vehicle using cash.

C.1.2 Lenders

There are n_1 captive lenders with marginal costs of loan production $c > 0$. There are also n_2 non-captive lenders with marginal costs of loan production $c + \alpha$, where $n_1 + n_2 = n$ and $\alpha > 0$. Let $\bar{d} = n_1 \cdot n^{-1}$ denote the fraction of captive lenders. Lenders choose their prices to maximize profits

64. The model can also be re-framed as one where dealers represent consumers and have preferences over the amount of incentives offered from different lenders.

$(p_i - c_i) \cdot q_i$, where q_i is the demand for lender i. Following Raith 2003 and Aghion and Shankerman 2004, we assume that lenders do not know the marginal costs of their neighboring lenders on the circle, and thus base their pricing decisions on the expected costs of their neighbors.

C.1.3 Cost Shock

We consider a cost shock to captive lenders that increases their marginal cost of loan production from c to $c + \gamma$. Our goal is to understand how the cost shock affects equilibrium prices.

C.1.4 Equilibrium Notation

Let $p(1)$ denote the equilibrium loan price for captive lenders prior to the cost shock, and let $\tilde{p}(1)$ denote the price after. Let $p(0)$ and $\tilde{p}(0)$ denote the same quantities but for non-captive lenders.

C.2 Equilibrium Prior to the Cost Shock

The solution to the model in the absence of the cost shock is well-known and is derived for a similar setting in Berg et al. 2021. The equilibrium loan price for captive lenders is:

$$p(1) = c + \frac{t}{n} + \alpha \left(\frac{n(1 - \bar{d})}{2n - 1} \right),$$

and their market share per firm is:

$$m(1) = \frac{1}{n} + \alpha \left(\frac{n(1 - \bar{d})}{t(2n - 1)} \right).$$

Similarly, the equilibrium loan price for non-captive lenders is:

$$p(0) = p(1) + \alpha \left(\frac{n - 1}{2n - 1} \right).$$

and their market share per firm is:

$$m(0) = \frac{1}{n} - \alpha \left(\frac{n\bar{d}}{t(2n - 1)} \right).$$

Consistent with the data, the model predicts that non-captives charge higher loan prices than captives prior to the cost shock. Despite this gap, captives do not raise their loan prices because it will result in a loss of market share and total profits.

C.3 Equilibrium After the Cost Shock

The model with the cost shock is equivalent to the model without the cost shock but with the difference in marginal costs reversed. The equilibrium loan price for non-captive lenders after the cost shock is:

$$\tilde{p}(0) = (c + \alpha) + \frac{t}{n} + (\gamma - \alpha) \left(\frac{n\bar{d}}{2n - 1} \right),$$

and the equilibrium loan price for captive lenders after the cost shock is:

$$\tilde{p}(1) = \tilde{p}(0) + (\gamma - \alpha)\left(\frac{n-1}{2n-1}\right).$$

There are two main findings from the model. First, non-captive lenders find it optimal to raise their loan prices in response to a cost shock to captive lenders:[65]

$$\tilde{p}(0) - p(0) = \underbrace{\gamma\left(\frac{n\bar{d}}{2n-1}\right)}_{:=p^s}.$$

We call the term p^s the spillover effect of the cost shock on non-captive lenders. Second, the total effect p^t of the cost shock on captive loan prices is equal to the spillover effect p^s plus an additional direct effect p^d that is specific to captive lenders:

$$\underbrace{\tilde{p}(1) - p(1)}_{:=p^t} = \underbrace{\gamma\left(\frac{n\bar{d}}{2n-1}\right)}_{:=p^s} + \underbrace{\gamma\left(\frac{n-1}{2n-1}\right)}_{:=p^d}.$$

C.3.1 The Size of the Spillover Effect in Relation to the Direct Dffect

As discussed further in Section C.4, the spillover effect p^s cannot be empirically identified in a difference-in-differences setting. This is problematic because it implies that our difference-in-differences estimates will only capture the direct effect of the cost shock p^d on captive lenders, which is an underestimate of the total effect p^t (which also includes the common spillover p^s).

An alternative approach for estimating the total effect is to leverage the implied relationship between p^d and p^s from the model. Notice that the ratio of the spillover effect to the direct effect is equal to:

$$\frac{p^s}{p^d} = \bar{d}\left(\frac{n}{n-1}\right).$$

If we hold $\bar{d}$ fixed, then the above ratio converges to $\bar{d}$ as the number of lenders n grows large. That is, the model predicts that the ratio of the spillover effect to the direct effect will be equal to the market share of captive lenders. Therefore, given an estimate of p^d from our difference-in-differences model and an estimate of the market share of captive lenders from population data, we can then estimate the spillover effect as:

$$p^s = p^d \cdot \bar{d},$$

and the total effect as:

$$p^t = p^d \cdot (1 + \bar{d}).$$

65. Given that captives will raise their loan prices in response to the cost shock, non-captives can raise prices a little to increase their profits per loan without sacrificing market share. This model is not well-suited to examining effects on total quantities (as opposed to market shares) because no consumers exit the market (i.e., purchase the vehicle using cash) in response to higher loan prices.

C.4 What Do We Recover From Difference-in-Differences?

We now demonstrate that empirical identification of the spillover effect is infeasible in a difference-in-differences setting without imposing strict assumptions on the data-generating process.

C.4.1 Setup

Suppose there are two periods, one before the cost shock ($t = 0$) and the other after ($t = 1$). Let P_t be a post-period indicator that is equal to one if $t = 1$, and zero otherwise. Suppose there are $i = 1, \ldots, N$ lenders in the sample. Let T_i be a treatment indicator equal to one if lender i is a captive lender, and zero otherwise.

C.4.2 Model

Suppose we estimate the following simplified difference-in-differences model:

$$p_{i,t} = \alpha + \beta_1 \cdot T_i \cdot P_t + \beta_2 \cdot T_i + \beta_3 \cdot P_t + \varepsilon_{i,t},$$

where $p_{i,t}$ is the loan price of lender i in period t. If the parallel trends assumption holds, then β_1 identifies the direct effect of the cost shock on captive loan prices:

$$\beta_1 = [\tilde{p}(1) - p(1)] - [\tilde{p}(0) - p(0)] = p^s + p^d - p^s = p^d.$$

Note that the above identification holds regardless of auto loan prices would have changed in the absence of treatment. For example, if we added a market-wide cost shock that affected both captive and non-captive lenders to our theoretical model, then we would still recover the direct effect of the original captive-specific cost shock from our difference-in-differences model.

C.4.3 Spillover Effect

If we assume that auto loan prices would not have changed in the absence of treatment, then β_3 identifies the spillover effect of the cost shock and $\beta_1 + \beta_3$ identifies the total effect. However, because changes in funding rates, loan demand, and other macroeconomic factors can cause auto loan prices to change over time, there is little reason to believe this assumption will be satisfied. Given this, we use both our model and an alternative data-driven procedure to estimate the average spillover effect, as discussed further below.

C.5 Estimating the Average Spillover Effect

As shown in Figure IA.8, the time-series of average captive and non-captive interest rates is consistent with the existence of spillover effects on non-captive lenders. However, extracting a reliable estimate of the average spillover effect from these time-series averages is difficult because other time-varying factors may have also affected non-captive interest rates during our sample period. Below, we use both our theoretical model and a data-driven procedure to estimate the average spillover effect on non-captive lenders.

C.5.1 Model-Based Estimate

Our model predicts that the average spillover effect on non-captive lenders should be equal to the product of our baseline difference-in-differences coefficient and the pre-treatment market share of captive lenders: $p^s = p^d \cdot \bar{d}$. From Table 3, we have that $p^d = 26$ basis points. From population data, we have that $\bar{d} = 26$ percent. Therefore, our model-based estimate of the spillover effect is 7 basis points (= 26 basis points $\times$ 0.26), and the spillover-inclusive increase in captive interest rates is 33 (= 26 + 7) basis points, or $227 per loan in present value terms.

C.5.2 Data-Driven Estimate

Our data-driven procedure for estimating the spillover effect proceeds in two main steps. In the first step, we predict how non-captive lenders' interest rates would have changed in the absence of the tariffs based on realized changes in market interest rates and historical non-captive interest rate pass-through rates. Specifically, we start by estimating the following model during the pre-treatment to estimate non-captive lenders' historical pass-through rates:

$$\Delta \text{Rate}_t = \alpha + \beta \cdot \Delta R^f_t + \varepsilon_t, \tag{14}$$

where $\Delta \text{Rate}_t = \text{Rate}_t - \text{Rate}_{t-1}$ is the month-over-month change in the average non-captive interest rate, $\Delta R^f_t = R^f_t - R^f_{t-1}$ is the month-over-month change in the 1-year Treasury yield, and β is the pass-through rate.[66] Then, we combine the estimated model parameters with realized month-over-month changes in 1-year Treasury yields during the post-treatment period to construct a sequence of predicted changes in non-captive interest rates: $\{\widehat{\Delta \text{Rate}}_t = \widehat{\alpha} + \widehat{\beta} \cdot \Delta R^f_t\}$.

In the second step of our data-driven process, we take the difference between our predicted changes in non-captive interest rates from above and their actual changes during the post-treatment period. This gives us a sequence of interest rate residuals following the tariffs, which we then sum up to arrive at our estimate of the spillover effect:

$$\widehat{\text{Spillover}} = \sum_{2018-01}^{2018-12} \Delta \text{Rate}_t - \widehat{\Delta \text{Rate}}_t. \tag{15}$$

Using the above procedure, we estimate an average spillover effect of 6.26 basis points, which is almost identical to our model-based estimate of 7 basis points. Although the consistency of our estimates is reassuring, it is important to acknowledge that neither our data-driven estimate nor our model-based estimate is perfect, as they both rely on various sets of assumptions that are difficult to verify in the data. For instance, our data-driven procedure implicitly assumes that no other time-varying factors besides the rise in Treasury yields would have systematically affected non-captive interest rates during the post-treatment period.

66. Our estimate of the spillover effect is robust to using other risk-free interest rates besides the 1-year Treasury yield, as well as an alternative data-driven method based on loan-level data.

Table IA.1: Loan Originations

Panel A: Captive-Level Aggregations

	Number of Loans Originated			
	Linear Model	Poisson Model	Linear Model	Poisson Model
	(1)	(2)	(3)	(4)
Treated × Post	-0.067***	-0.117***	-0.048***	-0.125***
	(-9.44)	(-3.25)	(-8.40)	(-10.54)
Level of Aggregation	$f \times s \times v \times t$	$f \times s \times v \times t$	$f \times s \times w \times c \times t$	$f \times s \times w \times c \times t$
Captive FE	Y	Y	Y	Y
State × Quarter FE	Y	Y	Y	Y
Vehicle × Quarter FE	Y	Y		
Income × Quarter FE			Y	Y
Credit Score × Quarter FE			Y	Y
N	321,016	312,757	183,824	183,824
R^2	0.49	0.70	0.76	0.76

Panel B: Lender-Level Aggregations

	Number of Loans Originated			
	Linear Model	Poisson Model	Linear Model	Poisson Model
	(1)	(2)	(3)	(4)
Treated × Post	-0.031***	-0.05	-0.047***	-0.121***
	(-6.59)	(-1.30)	(-15.37)	(-12.24)
Level of Aggregation	$l \times s \times v \times t$	$l \times s \times v \times t$	$l \times s \times w \times c \times t$	$l \times s \times w \times c \times t$
Lender FE	Y	Y	Y	Y
State × Quarter FE	Y	Y	Y	Y
Vehicle × Quarter FE	Y	Y		
Income × Quarter FE			Y	Y
Credit Score × Quarter FE			Y	Y
N	596,568	587,512	795,360	795,360
R^2	0.42	0.73	0.43	0.53

NOTE.—This table reports coefficient estimates from Equation 13. The dependent variable in columns 1 and 3 is the log of one plus the number of loans originated. The dependent variable in columns 2 and 4 is the raw number of loan originations. We estimate a linear regression model in columns 1 and 3 and a Poisson regression model in columns 2 and 4. In Panel A, we calculate the number of loan originations at either (i) the captive (f) × state (s) × vehicle make-model-condition (v) × origination quarter (t) level in columns 1 and 2; or (ii) captive × state × vehicle make-model-condition × income bucket (ω) × credit score (c) × origination quarter level in columns 3 and 4. In Panel B, we perform the same aggregations but at the lender (l) level instead of the captive level. The sample is restricted to auto loans originated between January 2017 and December 2018. t-statistics, presented below the coefficient estimates, are calculated using hetroskedasticity-robust standard errors. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Table IA.2: Adjusted Sample Filters

	Interest rate						
	Credit score		Winsorizing		Sample period		Loan-to-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated × Post	0.260***	0.236***	0.245***	0.257***	0.286***	0.282*	0.265***
	(3.06)	(3.26)	(2.76)	(3.38)	(3.58)	(1.94)	(2.65)
Sample filter	660+	500+	Winsor 2%	No winsor	2017-2019	Only Q1 & Q2	No filter
Lender FE	Y	Y	Y	Y	Y	Y	Y
Vehicle × Quarter FE	Y	Y	Y	Y	Y	Y	Y
State × Quarter FE	Y	Y	Y	Y	Y	Y	Y
Income × Quarter FE	Y	Y	Y	Y	Y	Y	Y
Credit Score × Quarter FE	Y	Y	Y	Y	Y	Y	Y
N	1,772,625	2,498,681	1,881,893	2,086,697	2,255,198	960,415	2,431,877
R^2	0.65	0.85	0.68	0.73	0.70	0.71	0.73

NOTE.—This table reports coefficient estimates from Equation 2. The dependent variable is the interest rate. Across the columns, we adjust our sample filters from Section 3. In Columns 1 and 2, we adjust our credit score filter. In Columns 3 and 4, we adjust our level of winsorization. In Column 5, we extend our sample period to 2019. In Column 6, we restrict our sample period to prior to the retaliatory tariffs from China. In Column 7, we remove our loan-to-value ratio filter. The row *Sample filter* lists the sample adjustment being applied. t-statistics, presented below the coefficient estimates, are calculated by clustering at the lender level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Table IA.3: Comparison of Loan Terms Across Data Sources

Panel A: All Lenders

	Mean (1)	SD (2)	P25 (3)	P50 (4)	P75 (5)
Originations	0.32	0.28	0.14	0.25	0.43
Loan Amount	1.01	0.07	0.98	1.02	1.04
Loan Maturity	1.00	0.03	0.99	1.00	1.02
Monthly Payment	0.99	0.05	0.96	0.99	1.02

Panel B: Restricted Sample of Lenders

	Mean (1)	SD (2)	P25 (3)	P50 (4)	P75 (5)
Originations	0.37	0.31	0.15	0.27	0.45
Loan Amount	1.00	0.08	0.98	1.00	1.04
Loan Maturity	1.01	0.03	1.00	1.01	1.03
Monthly Payment	1.00	0.05	0.98	1.01	1.03

NOTE.—This table compares the average loan terms in the Regulation AB II data to the average loan terms in the population credit bureau data. The comparisons are conducted at the lender level for loans that were originated between 2017 and 2018. Panel A reports descriptive statistics for the entire set of 19 lenders in the Regulation AB II data. Panel B reports descriptive statistics for the restricted sample of 14 lenders that we use to estimate our regression models throughout the paper. The rows in the table are defined as follows. *Originations* is the ratio of the number of loan originations in the Regulation AB II data (calculated at the lender level) to the number of loan originations in the credit bureau data. *Loan amount* is the ratio of the average loan amount for originated loans in the Regulation AB II data (calculated at the lender level) to the average loan amount of originated loans in the credit bureau data. *Loan maturity* and *Monthly payment* are the same ratios but for average loan maturities and monthly payments, respectively.

Table IA.4: Comparison of Large Non-Captive Lenders Across Data Sources

Panel A: Unweighted Models

	Interest Rate (1)	Loan Amount (2)	Maturity (3)	Interest rate (4)	Loan Amount (5)	Maturity (6)
Regulation AB Lender	0.1927	0.017	0.031	0.1128	0.0091	0.0246
	(0.28)	(0.27)	(1.13)	(0.14)	(0.12)	(0.78)
Month FE	Y	Y	Y	Y	Y	Y
Credit Score FE	Y	Y	Y	Y	Y	Y
Credit Score × Month FE				Y	Y	Y
N	624	624	624	624	624	624
R^2	0.94	0.77	0.64	0.95	0.8	0.71

Panel B: Weighted Models

	Interest Rate (1)	Loan Amount (2)	Maturity (3)	Interest rate (4)	Loan Amount (5)	Maturity (6)
Regulation AB Lender	0.0444	0.016	0.0287	0.0623	0.0035	0.0207
	(0.07)	(0.27)	(1.07)	(0.08)	(0.05)	(0.67)
Month FE	Y	Y	Y	Y	Y	Y
Credit Score FE	Y	Y	Y	Y	Y	Y
Credit Score × Month FE				Y	Y	Y
N	624	624	624	624	624	624
R^2	0.95	0.76	0.64	0.96	0.78	0.71

NOTE.—This table reports coefficient estimates from the following model: $y_{l,t} = \alpha + \beta \cdot \text{Regulation AB Lender}_l + \delta_t + \delta_c + \varepsilon_{l,t}$, where the outcome variable, $y_{l,t}$, is either the average interest rate, log loan amount, or log loan maturity for loans originated by non-captive lender l in month t. The indicator variable Regulation AB Lender$_l$ is equal to one if non-captive lender l is in the Regulation AB II data, and zero otherwise, δ_t are month fixed effects, and δ_c are 25-point average credit score bin fixed effects that are constructed at the lender level. The model is estimated using our population credit bureau data (see Section 3.1.1), and the sample is restricted to large non-captive lenders with at least 10,000 auto loan originations per quarter. The sample period runs from January 2017 to December 2018. Panel A reports coefficient estimates for unweighted models. Panel B reports coefficient estimates for weighted models that use lender-level loan origination volumes as population weights. t-statistics, presented below the coefficient estimates, are calculated by clustering at the lender level. *, **, and *** denote statistical significant at the 10%, 5%, and 1% levels, respectively.

Table IA.5: Alternative Choices of Treatment Date

	Interest Rate					
	All Loans			Excluding Subvented Loans		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	0.245***	0.222**	0.229**	0.294***	0.298***	0.302***
	(2.69)	(2.53)	(2.50)	(2.96)	(2.97)	(3.04)
Treatment date	Jan-18	Feb-18	Mar-18	Jan-18	Feb-18	Mar-18
Lender FE	Y	Y	Y	Y	Y	Y
Vehicle × Month FE	Y	Y	Y	Y	Y	Y
State × Month FE	Y	Y	Y	Y	Y	Y
Income × Month FE	Y	Y	Y	Y	Y	Y
Credit Score × Month FE	Y	Y	Y	Y	Y	Y
N	1,971,643	1,971,643	1,971,643	789,583	789,583	789,583
R^2	0.71	0.71	0.71	0.68	0.68	0.68

NOTE.—This table reports coefficient estimates from Equation 2 when using either January 2018, February 2018, or March 2018 as the treatment date. The dependent variable is the interest rate. The sample is restricted to auto loans originated between January 2017 and December 2018. t-statistics, presented below the coefficient estimates, are calculated by clustering at the lender level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Table IA.6: Excluding World Omni

	Interest Rate (1)	Loan Amount (2)	Loan Maturity (3)	Loan-to-Value (4)
Treated × Post	0.315**	-0.008	-0.008***	-0.008*
	(2.49)	(-0.88)	(-2.75)	(-1.69)
Lender FE	Y	Y	Y	Y
Vehicle × Quarter FE	Y	Y	Y	Y
State × Quarter FE	Y	Y	Y	Y
Income × Quarter FE	Y	Y	Y	Y
Credit Score × Quarter FE	Y	Y	Y	Y
N	1,814,144	1,814,144	1,814,144	1,814,144
R^2	0.72	0.56	0.21	0.22

NOTE.—This table reports coefficient estimates from Equation 2 after excluding loans from World Omni from the sample. The dependent variable is either the interest rate, log loan amount, log loan maturity, or loan-to-value ratio. The sample is restricted to auto loans originated between January 2017 and December 2018. t-statistics, presented below the coefficient estimates, are calculated by clustering at the lender level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Table IA.7: Auto Loan Terms for New and Used Vehicles

Panel A: New Vehicles

	Interest rate (1)	Loan Amount (2)	Loan Maturity (3)	Loan-to-Value (4)
Treated × Post	0.243***	-0.029***	-0.023***	-0.020***
	(3.20)	(-3.55)	(-5.74)	(-4.22)
Lender FE	Y	Y	Y	Y
Vehicle × Quarter FE	Y	Y	Y	Y
State × Quarter FE	Y	Y	Y	Y
Income × Quarter FE	Y	Y	Y	Y
Credit Score × Quarter FE	Y	Y	Y	Y
N	1,289,837	1,289,837	1,289,837	1,289,837
R^2	0.67	0.42	0.23	0.21

Panel B: Used Vehicles

	Interest Rate (1)	Loan Amount (2)	Loan Maturity (3)	Loan-to-Value (4)
Treated × Post	0.297**	0.010	0.003	0.004
	(2.35)	(1.04)	(0.51)	(0.83)
Lender FE	Y	Y	Y	Y
Vehicle × Quarter FE	Y	Y	Y	Y
State × Quarter FE	Y	Y	Y	Y
Income × Quarter FE	Y	Y	Y	Y
Credit Score × Quarter FE	Y	Y	Y	Y
N	683,230	683,230	683,230	683,230
R^2	0.66	0.55	0.15	0.14

NOTE.—This table reports coefficient estimates from Equation 2. The dependent variable is either the interest rate, log loan amount, log loan maturity, or loan-to-value ratio. The sample is restricted to auto loans originated between January 2017 and December 2018. In Panel A, we restrict the sample to loans for new vehicles. In Panel B, we restrict the sample to loans for used vehicles. t-statistics, presented below the coefficient estimates, are calculated by clustering at the lender level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Table IA.8: Captive Auto Loan Terms for U.S. Made and Foreign Made Makes and Models

	Interest Rate (1)	Interest Rate (2)	Interest Rate (3)	Interest Rate (4)
US Made × Post	0.119	0.002	-0.035	-0.040
	(0.99)	(0.03)	(-0.35)	(-0.75)
Definition of US Made	Make	Make	Make-Model	Make-Model
Excluding Subvented Loans?		Y		Y
Lender FE	Y	Y	Y	Y
Vehicle FE	Y	Y	Y	Y
State × Quarter FE	Y	Y	Y	Y
Income × Quarter FE	Y	Y	Y	Y
Credit Score × Quarter FE	Y	Y	Y	Y
N	1,195,740	189,255	1,195,740	189,255
R^2	0.60	0.74	0.60	0.74

NOTE.—This table reports coefficient estimates from Equation 2 after we (i) restrict the sample to captive auto loans, (ii) replace *Treated* with *US Made*, and (iii) replace the vehicle × quarter fixed effects ($\delta_{v,t}$) with vehicle fixed effects (δ_v). The dependent variable is the interest rate. The sample consists of captive auto loans originated between January 2017 and December 2018. In columns 1 and 2, *US Made* is assigned at the vehicle make level, and it is equal to one if at least 50 percent of make m's vehicles are manufactured in the U.S, and zero otherwise (see Section 4.5). In columns 3 and 4, *US Made* is assigned at the vehicle make-model level, and it is equal to one if at least 50 percent of make-model $\tilde{m}$'s vehicles are manufactured in the U.S, and zero otherwise (see Section 4.5). t-statistics, presented below the coefficient estimates, are calculated by clustering at the lender level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Table IA.9: Controlling for Differential Pass-Through of Risk-Free Interest Rates

Panel A: All Loans

	Interest Rate					
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	0.255***	0.252***	0.256***	0.259***	0.264***	0.268***
	(2.75)	(2.72)	(2.78)	(2.80)	(2.82)	(2.78)
Treated ×Δ Fed Funds		Y				Y
Treated ×Δ 1Y Treasury			Y			Y
Treated ×Δ 5Y Treasury				Y		Y
Treated ×Δ 10Y Treasury					Y	Y
Lender FE	Y	Y	Y	Y	Y	Y
Vehicle × Time FE	Y	Y	Y	Y	Y	Y
State × Time FE	Y	Y	Y	Y	Y	Y
Income × Time FE	Y	Y	Y	Y	Y	Y
Credit Score × Time FE	Y	Y	Y	Y	Y	Y
N	1,973,067	1,973,067	1,973,067	1,973,067	1,973,067	1,973,067
R^2	0.70	0.70	0.70	0.70	0.70	0.70

Panel B: Non-Subvented Loans

	Interest Rate					
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	0.288***	0.292***	0.288***	0.298***	0.306***	0.295***
	(2.85)	(2.87)	(2.84)	(2.92)	(2.94)	(2.75)
Treated ×Δ Fed Funds		Y				Y
Treated ×Δ 1Y Treasury			Y			Y
Treated ×Δ 5Y Treasury				Y		Y
Treated ×Δ 10Y Treasury					Y	Y
Lender FE	Y	Y	Y	Y	Y	Y
Vehicle × Time FE	Y	Y	Y	Y	Y	Y
State × Time FE	Y	Y	Y	Y	Y	Y
Income × Time FE	Y	Y	Y	Y	Y	Y
Credit Score × Time FE	Y	Y	Y	Y	Y	Y
N	791,300	791,300	791,300	791,300	791,300	791,300
R^2	0.67	0.67	0.67	0.67	0.67	0.67

NOTE.—This table reports coefficient estimates from Equation 2 after including an extensive set of controls for changes in risk-free interest rates. Specifically, we include interactions between our treatment indicator variable and monthly changes in the Fed Funds rate, 1-year Treasury rate, 5-year Treasury rate, and 10-year Treasury rate. The dependent variable is the interest rate. The sample is restricted to auto loans originated between January 2017 and December 2018. In Panel A, we report coefficient estimates for the full sample of auto loans. In Panel B, we restrict the sample to loans without subsidized financing. t-statistics, presented below the coefficient estimates, are calculated by clustering at the lender level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Table IA.10: Invoice Prices for Captive-Financed and Non-Captive-Financed Vehicles

	Invoice Price (1)	log Invoice Price (2)
Treated × Post	11.689	0.001
	(0.40)	(1.35)
Lender FE	Y	Y
Vehicle × Time FE	Y	Y
State × Time FE	Y	Y
Income × Time FE	Y	Y
Credit Score × Time FE	Y	Y
N	1,289,837	1,289,837
R^2	0.87	0.89

NOTE.—This table reports coefficient estimates from Equation 2 for the subsample of new vehicles. The dependent variable is either the invoice price in column 1 or the log invoice price in column 2. The sample is restricted to auto loans originated between January 2017 and December 2018. t-statistics, presented below the coefficient estimates, are calculated by clustering at the lender level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Table IA.11: More-Exposed Captive Lenders Versus Less-Exposed Captives

	Interest Rate	
	(1)	(2)
More Exposed × Post	0.271	0.239**
	(1.27)	(2.19)
Exclude Subvented Loans?		Y
Lender FE	Y	Y
Make × Model × Condition FE	Y	Y
State × Quarter FE	Y	Y
Income × Quarter FE	Y	Y
Credit Score × Quarter FE	Y	Y
N	1,185,241	181,267
R^2	0.58	0.73

NOTE.—This table reports coefficient estimates from Equation 2 after we replace the *Treated* variable with *More Exposed*, which is equal to one for more-exposed captive lenders (defined in Section 4.2, and zero otherwise. The sample is restricted to captive auto loans originated between January 2017 and December 2018. The dependent variable is the interest rate. In column 1, the model is estimated using all captive auto loans. In column 2, the model is estimated on the subsample of non-subvented captive auto loans. t-statistics, presented below the coefficient estimates, are calculated by clustering at the lender level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Table IA.12: Controlling for Differential Changes in Borrowing Costs

Panel A: All loans

	Interest Rate			
	(1)	(2)	(3)	(4)
Treated × Post	0.502**	0.423*	0.395**	0.305***
	(2.48)	(1.91)	(2.37)	(3.54)
Financing Cost Proxy	Cost of debt	Note rate	Bond rate	Credit rating
Lender FE	Y	Y	Y	Y
Vehicle × Quarter FE	Y	Y	Y	Y
State × Quarter FE	Y	Y	Y	Y
Income × Quarter FE	Y	Y	Y	Y
Credit score × Quarter FE	Y	Y	Y	Y
N	1,755,262	1,755,262	1,755,262	1,610,090
R^2	0.71	0.71	0.71	0.70

Panel B: Excluding subvented loans

	Interest Rate			
	(1)	(2)	(3)	(4)
Treated × Post	0.450***	0.479***	0.374***	0.299***
	(3.35)	(2.65)	(2.59)	(2.94)
Financing Cost Proxy	Cost of debt	Note rate	Bond rate	Credit rating
Lender FE	Y	Y	Y	Y
Vehicle × Quarter FE	Y	Y	Y	Y
State × Quarter FE	Y	Y	Y	Y
Income × Quarter FE	Y	Y	Y	Y
Credit score × Quarter FE	Y	Y	Y	Y
N	686,092	686,092	686,092	454,308
R^2	0.67	0.67	0.67	0.75

NOTE.—This table reports coefficient estimates from Equation 2 after including two additional control variables: (i) a linear financing cost proxy and (ii) the interaction between the linear financing cost proxy and the treatment indicator. The dependent variable is the interest rate. The sample is restricted to auto loans originated between January 2017 and December 2018. In Panel B, we remove subvented loans from the sample. The row *Financing Cost Proxy* lists the proxy variable for firm financing costs used in each model. These variables are sourced from Bloomberg and are available for most (but not all) of our lenders. Our financing cost proxies include estimates of the cost of debt, the short-term note (par) coupon rate, the long-term bond (par) coupon rate, and the credit rating. Vehicle fixed effects refer to vehicle make-model-condition combinations. t-statistics, presented below the coefficient estimates, are calculated by clustering at the lender level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Table IA.13: Controlling for Differential Exposures to Auto Loan Default Risk

	Interest Rate	
	All Loans	Non-Subvented Loans
	(1)	(2)
Treated × Post	0.226**	0.283***
	(2.48)	(2.77)
Treated ×Δ Default Rate	Y	Y
Lender FE	Y	Y
Vehicle × Quarter FE	Y	Y
State × Quarter FE	Y	Y
Income × Quarter FE	Y	Y
Credit Score × Quarter FE	Y	Y
N	1,973,067	791,300
R^2	0.70	0.67

NOTE.—This table reports coefficient estimates from Equation 2 after controlling for the interaction between the treatment indicator variable and quarterly changes in aggregate auto loan default rates reported by the New York Fed. The dependent variable is the interest rate. The sample is restricted to auto loans originated between January 2017 and December 2018. In column 1, we report coefficient estimates for the full sample of auto loans. In column 2, we restrict the sample to loans without subsidized financing. t-statistics, presented below the coefficient estimates, are calculated by clustering at the lender level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Table IA.14: Ruling Out Changes in Dealer Loan Markups

	Interest Rate (1)	Loan Amount (2)	Loan Maturity (3)	Loan-to-Value (4)
Treated × Post	0.273***	-0.017*	-0.018***	-0.015***
	(2.70)	(-1.88)	(-5.50)	(-3.93)
Lender FE	Y	Y	Y	Y
Vehicle × Quarter FE	Y	Y	Y	Y
State × Quarter FE	Y	Y	Y	Y
Income × Quarter FE	Y	Y	Y	Y
Credit Score × Quarter FE	Y	Y	Y	Y
N	1,783,813	1,783,813	1,783,813	1,783,813
R^2	0.72	0.56	0.22	0.22

NOTE.—This table reports coefficient estimates from Equation 2. The dependent variable is either the interest rate, log loan amount, log loan maturity, or loan-to-value ratio. The sample is restricted to captive auto loans with subsidized financing and non-captive auto loans with-or-without subsidized financing that are originated between January 2017 and December 2018. Vehicle fixed effects refer to vehicle make-model-condition combinations. t-statistics, presented below the coefficient estimates, are calculated by clustering at the lender level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Table IA.15: Prepayment Speed

	12-month paid-off		24-month paid-off	
	All loans	No subventions	All loans	No subventions
	(1)	(2)	(3)	(4)
Treated × Post	0.002	-0.004	0.007	0.002
	(0.26)	(-1.31)	(0.73)	(0.25)
Lender FE	Y	Y	Y	Y
Vehicle × Quarter FE	Y	Y	Y	Y
State × Quarter FE	Y	Y	Y	Y
Income × Quarter FE	Y	Y	Y	Y
Credit score × Quarter FE	Y	Y	Y	Y
N	1,973,067	791,300	1,361,478	557,380
R^2	0.05	0.04	0.06	0.04

NOTE.—This table reports coefficient estimates from Equation 2. The dependent variable is either an indicator for whether a loan is paid off within 12 months of its origination date or an indicator for whether a loan is paid off within 24 months of its origination date. The sample is restricted to auto loans originated between January 2017 and December 2018. In Columns (2) and (4), we further restrict the sample to loans without subsidized financing. Vehicle fixed effects refer to vehicle make-model-condition combinations. t-statistics, presented below the coefficient estimates, are calculated by clustering at the lender level.Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Table IA.16: Ruling Out Changes in Securitization Practices

Panel A: All Lenders

	Originations (1)	Loan Amount (2)	Loan Maturity (3)	Monthly Payment (4)
Treated × Post	0.04	0.02	0.00	0.00
	(0.33)	(0.52)	(0.01)	(0.09)
Lender FE	Y	Y	Y	Y
Quarter FE	Y	Y	Y	Y
N	142	142	142	142
R^2	0.72	0.72	0.80	0.77

Panel B: Restricted Sample of Lenders

	Originations (1)	Loan Amount (2)	Loan Maturity (3)	Monthly Payment (4)
Treated × Post	0.01	0.02	-0.01	0.00
	(0.06)	(0.44)	(-0.64)	(-0.21)
Lender FE	Y	Y	Y	Y
Quarter FE	Y	Y	Y	Y
N	112	112	112	112
R^2	0.71	0.72	0.82	0.75

NOTE.—This reports coefficient estimates from regressions of the form:

$$y_{l,t} = \alpha + \Gamma \times \text{Treated}_l \times \text{Post}_t + \delta_l + \delta_t + \varepsilon_{l,t},$$

where the unit of observation is at the lender-origination quarter level and the sample period runs from 2017 to 2018. Panel A reports coefficient estimates for all 19 lenders in the Regulation AB II data. Panel B reports coefficient estimates for the restricted sample of 14 lenders that we use to estimate our regression models throughout the paper. The outcome variables are defined as follows. *Originations* is the ratio of the number of loan originations in the Regulation AB II data (calculated at the lender-origination quarter level) to the number of loan originations in the credit bureau data. *Loan Amount* is the ratio of the average loan amount for originated loans in the Regulation AB II data (calculated at the lender-origination quarter level) to the average loan amount of originated loans in the credit bureau data. *Loan Maturity* and *Monthly Payment* are the same ratios but for average loan maturities, and monthly payments respectively. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Table IA.17: Excluding Direct Non-Captive Loans

	Interest Rate (1)	Loan Amount (2)	Loan Maturity (3)	Loan-to-Value (4)
Treated × Post	0.250**	-0.002	-0.010**	-0.003
	(2.20)	(-0.04)	(-2.33)	(-0.76)
Lender FE	Y	Y	Y	Y
Vehicle × Quarter FE	Y	Y	Y	Y
State × Quarter FE	Y	Y	Y	Y
Income × Quarter FE	Y	Y	Y	Y
Credit Score × Quarter FE	Y	Y	Y	Y
Original Estimate	0.255***	-0.008	-0.011***	-0.008**
N	1,742,749	1,742,749	1,742,749	1,742,749
R^2	0.71	0.54	0.22	0.21

NOTE.—This table reports coefficient estimates from Equation 2 after restricting the control sample to auto loans originated by either CarMax, Santander, or World Omni. The dependent variable is either the interest rate, log loan amount, log loan maturity, or loan-to-value ratio. The sample consists of auto loans originated between January 2017 and December 2018. t-statistics, presented below the coefficient estimates, are calculated by clustering at the lender level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Table IA.18: Average Cost Increase Calculations

Vehicle Type (1)	Financing Source (2)	Percent of Population (3)	Δ Average Financing Cost (4)	Δ Average Vehicle Price (5)	Δ Average Total Cost (6)
New	Captive	17.70%	227	225	452
New	Non-Captive	12.30%	48	225	273
New	Cash	3.33%	0	225	225
Used	Captive	3.03%	227	0	227
Used	Non-Captive	40.30%	48	0	48
Used	Cash	23.33%	0	0	0
Weighted Average:			72	74	146
Percent of Average Loan Amount:			0.28%	0.29%	0.57%

NOTE.—This table calculates the average change in costs faced by consumers that purchase a vehicle in the U.S. The definitions of the columns are as follows. *Vehicle Type* describes whether the consumer purchases a new or used vehicle. *Financing Source* describes whether the vehicle was financed by a captive, a non-captive lender, or in cash. *Percent of Population* is the percent of consumers in the population that purchase a particular vehicle type and finance it in a particular way. Δ *Average Financing Cost* is the change in the average present value financing cost (inclusive of spillovers) for consumers in each group. Δ *Average Vehicle Price* is the change in the average vehicle price for consumers in each group. Δ *Average Total Cost* is the sum of Δ *Average Financing Cost* and Δ *Average Vehicle Price*. The weighted average increase in costs is calculated by summing the product of the average cost increase for each group and their population weights in column 3. Population data is from Experian 2021.

Table IA.19: List of Vehicle Brands and Captive Market Shares

Make (1)	Number of Loans (2)	In-House Captive (3)	In-House Captive in Sample? (4)	Captive Market Share (%) (5)
Acura	29,612	Honda	Yes	79
Alfa Romeo	16		No	0
Audi	21,090	Volkswagen	Yes	75
BMW	40,270	BMW	Yes	71
Buick	29,452	GM Financial	Yes	56
Cadillac	22,623	GM Financial	Yes	63
Chevrolet	262,025	GM Financial	Yes	64
Chrysler	11,337		No	4
Dodge	42,306		No	5
Fiat	1,713		No	1
Ford	217,340	Ford Credit	Yes	69
GMC	68,196	GM Financial	Yes	66
Honda	363,491	Honda	Yes	89
Hyundai	35,749	Hyundai	No	3
Infiniti	8,480	Nissan	No	2
Jaguar	872		No	2
Jeep	42,119		No	3
Kia	29,588	Hyundai	No	4
Land Rover	2,002		No	4
Lexus	35,870	Toyota	Yes	71
Lincoln	16,435	Ford Credit	Yes	84
Mazda	11,305		No	2
Mercedes	40,114	Mercedes	Yes	72
Mercury	14		No	0
Mini	1,702	BMW	Yes	14
Mitsubishi	7,974		No	6
Nissan	60,387	Nissan	No	4
Porsche	877		No	3
Ram	13		No	0
Scion	802	Toyota	Yes	100
Sprinter	123		No	2
Subaru	11,057		No	3
Suzuki	13		No	8
Tesla	27	Tesla	No	7
Toyota	497,447	Toyota	Yes	57
Volkswagen	59,079	Volkswagen	Yes	83
Volvo	2,102	Volvo	No	2
All Makes	1,973,639	–	–	61

NOTE.—This table reports the complete list of vehicle makes (i.e., brands) in our sample. Columns 3 through 5 are defined as follows. *In-House Captive* is the name of the make's in-house captive lender, regardless of whether it is in the sample. (External lending partnerships are not considered in-house.) *In-House Captive in Sample?* is "Yes" if the make has an in-house captive lender and it is in our sample, and "No" otherwise. (Recall that Hyundai and Nissan are in the Regulation AB II data but we exclude them from our sample.) *Captive Market Share* is the percent of captive-financed loans in our sample for each make, regardless of whether the captive is the make's in-house captive or a different captive. The rows highlighted in light grey correspond to makes without an in-house captive lender. The rows highlighted in dark grey correspond to makes that have an in-house lender but it is not in our sample.

Table IA.20: Average Lending Conditions for Main Sample and Overlap Subsample

	Non-Captive Loans		Captive Loans	
	Main Sample	Overlap Subsample	Main Sample	Overlap Subsample
Variable	(1)	(2)	(3)	(4)
Loan Amount	22,256	22,196	26,914	26,612
Interest Rate	6.30	6.26	2.52	2.51
Monthly Payment	397	395	450	446
Loan Maturity	68	68	66	66
Loan-to-Value	0.92	0.92	0.89	0.90
Vehicle Value	25,044	24,979	30,862	30,361
New Vehicle?	0.39	0.39	0.81	0.81
Credit Score	730	730	756	756
Income	81,537	81,253	89,979	89,160
Co-Signed?	0.36	0.36	0.31	0.31
Subvented?	0.22	0.22	0.81	0.81
12-Month Default	0.01	0.01	0.00	0.00
24-Month Default	0.02	0.02	0.01	0.01
12-Month Paidoff	0.09	0.09	0.03	0.03
24-Month Paidoff	0.22	0.22	0.11	0.11

NOTE.—This table reports pre-treatment means for our main sample of 1,973,067 auto loans (called the *Main Sample*) and our 98 percent subsample of these loans for vehicles that have both a captive and a non-captive lending option (called the *Overlap Subsample*). For these comparisons, we restrict our attention to the subsample of auto loans that were originated prior to the treatment date. Columns 1 and 2 report pre-treatment means for non-captive loans. Columns 3 and 4 report pre-treatment means for captive loans.

Table IA.21: Alternative Forms of Clustering

	Interest Rate				
	(1)	(2)	(3)	(4)	(5)
Treated × Post	0.255***	0.255***	0.255***	0.255***	0.255***
	(2.75)	(5.67)	(3.90)	(2.68)	(2.76)
Lender FE	Y	Y	Y	Y	Y
Vehicle × Quarter FE	Y	Y	Y	Y	Y
State × Quarter FE	Y	Y	Y	Y	Y
Income × Quarter FE	Y	Y	Y	Y	Y
Credit Score × Quarter FE	Y	Y	Y	Y	Y
Lender Clustering	Y				
State Clustering		Y			
Vehicle Clustering			Y		
ABS Clustering				Y	
Lender Wild Cluster Bootstrap					Y
N	1,973,067	1,973,067	1,973,067	1,973,067	1,973,067
R^2	0.70	0.70	0.70	0.70	0.70

NOTE.—This table reports coefficient estimates from Equation 2 using different methods for computing the standard errors. The dependent variable is the interest rate. In Column (1), we cluster the standard errors at the lender level as we do throughout the paper. In Column (2), we cluster the standard errors at the state level. In Column (3), we cluster the standard errors at the vehicle make-model-condition level. In Column (4), we cluster the standard errors at the asset-backed security level. In Column (5), we compute the standard errors using the wild cluster robust bootstrap with lender clustering. The sample is restricted to auto loans originated between January 2017 and December 2018. Vehicle fixed effects refer to vehicle make-model-condition combinations. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Table IA.22: More Granular Fixed Effects

	Interest Rate			
	(1)	(2)	(3)	(4)
Treated × Post	0.255**	0.324**	0.334***	0.347***
	(2.75)	(3.01)	(3.25)	(4.64)
Lender FE	Y	Y	Y	Y
State × Quarter FE	Y			
Vehicle × Quarter FE	Y			
Income × Quarter FE	Y	Y		
Credit Score × Quarter FE	Y	Y		
Vehicle × State × Quarter FE		Y	Y	
Income × Credit Score × State × Quarter FE			Y	
Vehicle × Income × Credit Score × State × Quarter FE				Y
N	1,973,067	1,935,616	1,924,144	1,031,917
R^2	0.70	0.73	0.75	0.85

NOTE.—This table reports coefficient estimates from Equation 2 after including more granular versions of our baseline fixed effects. The dependent variable is the interest rate. The sample is restricted to auto loans originated between January 2017 and December 2018. In Column (1), we re-estimate our baseline model used throughout the paper. In Column (2), we include separate origination quarter fixed effects for each vehicle and state combination. In Column (3), we include separate origination quarter fixed effects for each income and credit score bucket combination. In Column (4), we include separate origination quarter fixed effects for each vehicle-state-income bucket-credit score bucket combination. Vehicle fixed effects refer to vehicle make-model-condition combinations. t-statistics, presented below the coefficient estimates, are calculated by clustering at the lender level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Table IA.23: Fixed Effects for Other Loan Terms

	Interest Rate			
	(1)	(2)	(3)	(4)
Treated × Post	0.255***	0.249***	0.329***	0.322***
	(2.75)	(2.70)	(3.63)	(3.50)
Lender FE	Y	Y	Y	Y
State × Quarter FE	Y	Y	Y	Y
Vehicle × Quarter FE	Y	Y	Y	Y
Income × Quarter FE	Y	Y	Y	Y
Credit Score × Quarter FE	Y	Y	Y	Y
Loan Amount × Quarter FE		Y	Y	Y
Maturity × Quarter FE			Y	Y
LTV × Quarter FE				Y
N	1,973,067	1,973,067	1,973,067	1,973,067
R^2	0.70	0.71	0.72	0.73

NOTE.—This table reports coefficient estimates from Equation 2. The dependent variable is the interest rate. The sample is restricted to auto loans originated between January 2017 and December 2018. In Column (1), we re-estimate our baseline model used throughout the paper. In Column (2), we include separate origination quarter fixed effects for loan amount buckets. In Column (3), we include separate origination quarter fixed effects for loan maturity buckets. In Column (4), we include separate origination quarter fixed effects for LTV buckets. Vehicle fixed effects refer to vehicle make-model-condition combinations. Loan amount fixed effects refer to loan amount deciles. Maturity fixed effects refer to maturity deciles. LTV fixed effects refer to LTV deciles. t-statistics, presented below the coefficient estimates, are calculated by clustering at the lender level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Table IA.24: Reincluding Removed Lenders

Panel A: Reincluding all Removed Lenders Except for Hyundai

	Interest Rate (1)	Loan Amount (2)	Loan Maturity (3)	Loan-to-Value (4)
Treated × Post	0.267**	-0.005	-0.011***	-0.007*
	(2.31)	(-0.88)	(-5.12)	(-1.86)
Lender FE	Y	Y	Y	Y
Vehicle × Quarter FE	Y	Y	Y	Y
State × Quarter FE	Y	Y	Y	Y
Income × Quarter FE	Y	Y	Y	Y
Credit Score × Quarter FE	Y	Y	Y	Y
N	2,351,905	2,351,905	2,351,905	2,351,905
R^2	0.68	0.54	0.19	0.20

Panel B: Reincluding all Removed Lenders Including Hyundai

	Interest Rate (1)	Loan Amount (2)	Loan Maturity (3)	Loan-to-Value (4)
Treated × Post	0.206*	0.002	-0.009***	-0.002
	(1.81)	(0.63)	(-3.48)	(-0.59)
Lender FE	Y	Y	Y	Y
Vehicle × Quarter FE	Y	Y	Y	Y
State × Quarter FE	Y	Y	Y	Y
Income × Quarter FE	Y	Y	Y	Y
Credit Score × Quarter FE	Y	Y	Y	Y
N	2,459,111	2,459,111	2,459,111	2,459,111
R^2	0.68	0.54	0.19	0.20

NOTE.—This table reports coefficient estimates from Equation 2 after adjusting the sample of lenders. The dependent variable is either the interest rate, log loan amount, log loan maturity, or loan-to-value ratio. The sample is restricted to auto loans originated between January 2017 and December 2018. In Panel A, we reinclude all removed lenders from Section 3.2 except for Hyundai, which has its own integrated steel manufacturer. In Panel B, we also reinclude Hyundai in the sample. Among the five reincluded lenders, Harley Davidson, Hyundai, Nissan are classified as treated lenders. Capital One and California Republic are classified as control lenders. t-statistics, presented below the coefficient estimates, are calculated by clustering at the lender level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Table IA.25: Placebo Analyses Between 2015 and 2017

	Interest Rate (1)	Interest Rate (2)	Interest Rate (3)	Interest Rate (4)
Treated × Post 2017	-0.076	0.053	-0.201	-0.074
	(-0.57)	(0.70)	(-1.31)	(-0.86)
Placebo Period	2016-2017	2016-2017	2015-2017	2015-2017
Excluding Subvented Loans?		Y		Y
Lender FE	Y	Y	Y	Y
Vehicle × Quarter FE	Y	Y	Y	Y
State × Quarter FE	Y	Y	Y	Y
Income × Quarter FE	Y	Y	Y	Y
Credit Score × Quarter FE	Y	Y	Y	Y
N	1,690,511	664,315	2,054,876	806,682
R^2	0.71	0.68	0.7	0.68

NOTE.—This table reports coefficient estimates from Equation 2 for placebo samples of loans originated between 2016-2017 (columns 1 and 2) and 2015-2017 (columns 3 and 4). The Post 2017_t variable is equal to one for all quarters t after January 2017, and zero otherwise. Columns 1 and 3 report coefficient estimates for all loans. Columns 2 and 4 report coefficient estimates after excluding subvented loans from the sample. Vehicle fixed effects refer to vehicle make-model-condition combinations. t-statistics, presented below the coefficient estimates, are calculated by clustering at the lender level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Table IA.26: Vehicle Choices

Panel A: Dollar Vehicle Value

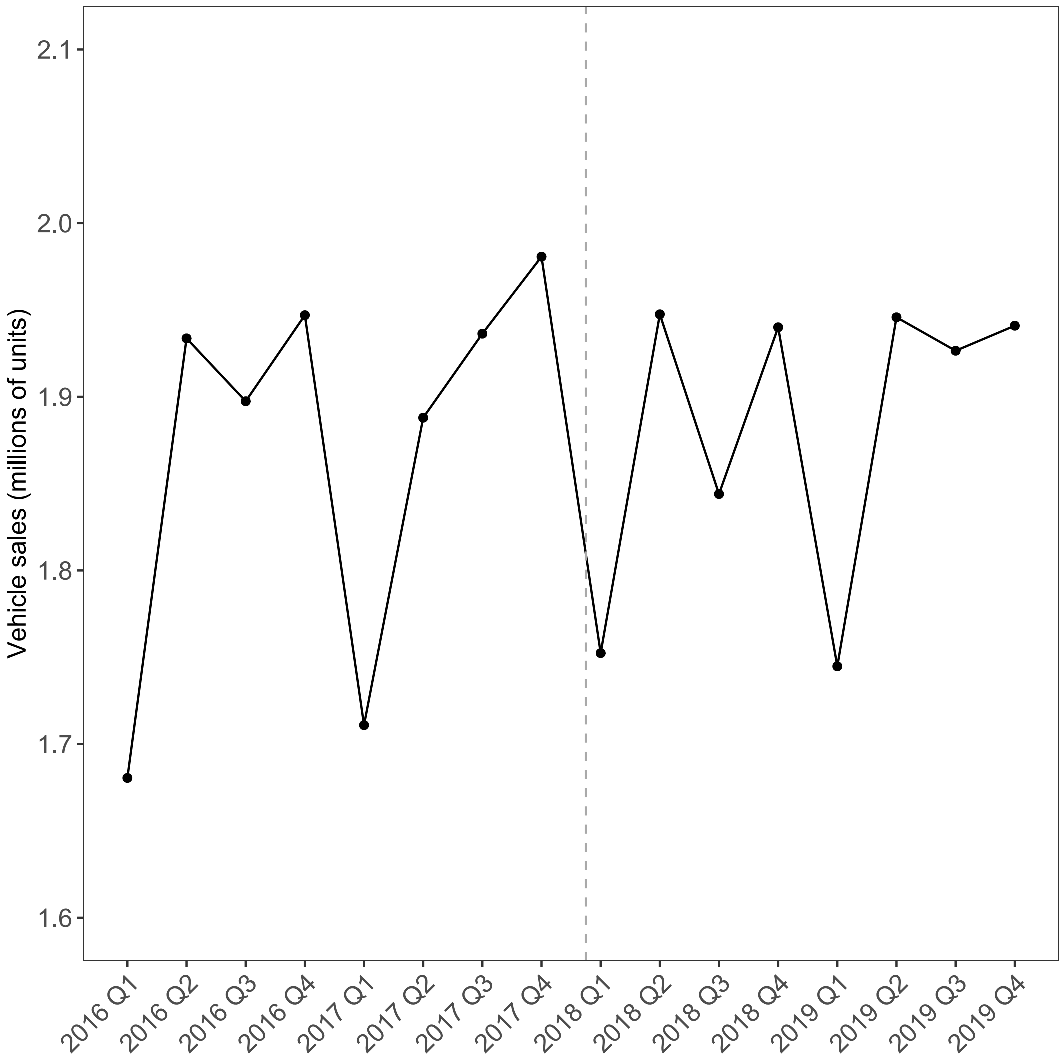
	All Vehicles		New Vehicles		Used Vehicles	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	72.300	-359.562	549.868	-174.072	-98.380	-252.723
	(0.13)	(-1.13)	(0.73)	(-0.34)	(-0.27)	(-0.78)
Lender FE	Y	Y	Y	Y	Y	Y
Condition × Quarter FE	Y					
Condition × Type × Quarter FE		Y				
Type × Quarter FE				Y		Y
State × Quarter FE	Y	Y	Y	Y	Y	Y
Income × Quarter FE	Y	Y	Y	Y	Y	Y
Credit Score × Quarter FE	Y	Y	Y	Y	Y	Y
N	1,973,639	1,973,634	1,290,119	1,290,116	683,520	683,518
R^2	0.47	0.59	0.35	0.52	0.26	0.39

Panel B: Log Vehicle Value

	All Vehicles		New Vehicles		Used Vehicles	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	0.005	-0.012	0.017	-0.007	-0.003	-0.011
	(0.26)	(-1.15)	(0.69)	(-0.41)	(-0.19)	(-0.75)
Lender FE	Y	Y	Y	Y	Y	Y
Condition × Quarter FE	Y					
Condition× Type × Quarter FE		Y				
Type × Quarter FE				Y		Y
State × Quarter FE	Y	Y	Y	Y	Y	Y
Income × Quarter FE	Y	Y	Y	Y	Y	Y
Credit Score × Quarter FE	Y	Y	Y	Y	Y	Y
N	1,973,639	1,973,634	1,290,119	1,290,116	683,520	683,518
R^2	0.49	0.63	0.34	0.54	0.24	0.41

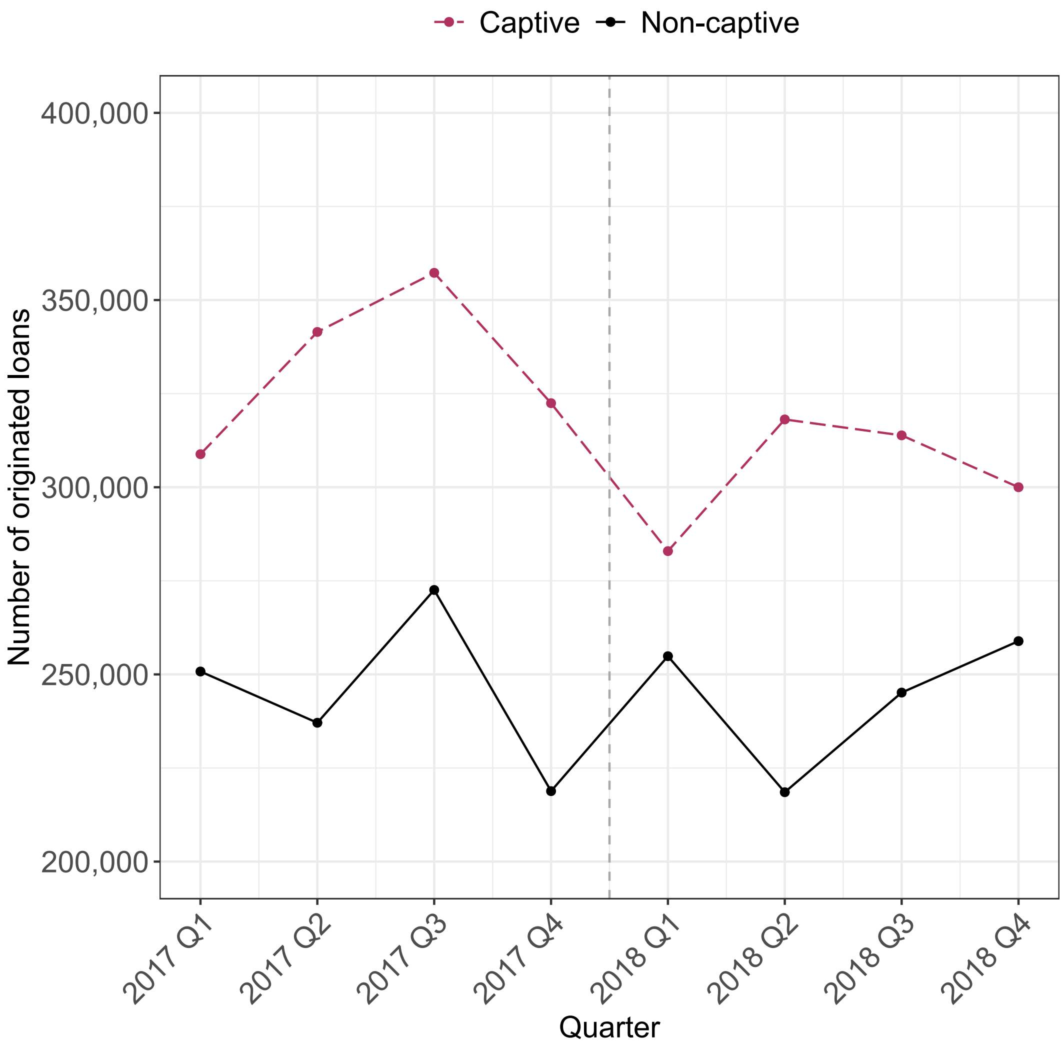
NOTE.—This table reports coefficient estimates from Equation 2 after removing the vehicle make-model-condition × origination quarter fixed effects. The dependent variable is either the assessed vehicle value in Panel A or the natural log of the assessed vehicle in Panel B. The sample is restricted to auto loans originated between January 2017 and December 2018. In columns 3 and 4, the sample is restricted to loans for new vehicles. In columns 5 and 6, the sample is restricted to loans for used vehicles. Column 1 includes vehicle condition × origination quarter fixed effects to examine substitution within new and used vehicles. Column 2 includes vehicle condition × type (i.e., truck, SUV, or sedan) × origination quarter fixed effects to examine substitution within new and used vehicles for a particular type. Column 4 and 6 includes type fixed effects to examine substitution within new vehicles and types and used vehicles and types, respectively. t-statistics, presented below the coefficient estimates, are calculated by clustering at the lender level. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Figure IA.1: Time Series of Vehicle Sales



NOTE.——This figure plots the number of vehicles sold in the U.S. between January 2017 and December 2018 for BMW, Ford, General Motors, Honda, Mercedes-Benz, and Volkswagen. For each manufacturer, we include all its affiliated brands in its sales total (e.g., we include both Acura and Honda sales for Honda).

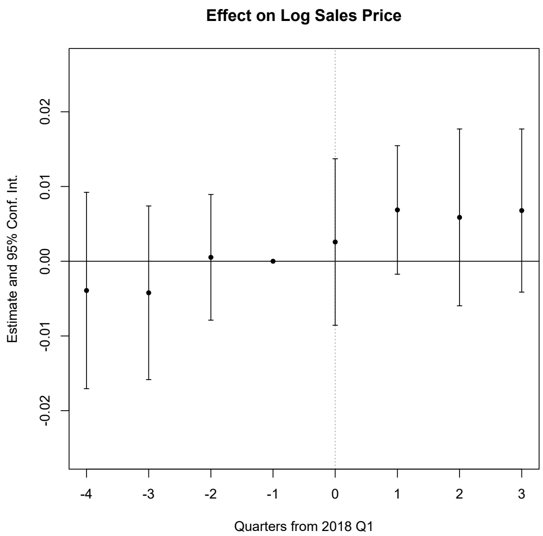
Figure IA.2: Securitization Volumes for Captive and Non-Captive Lenders



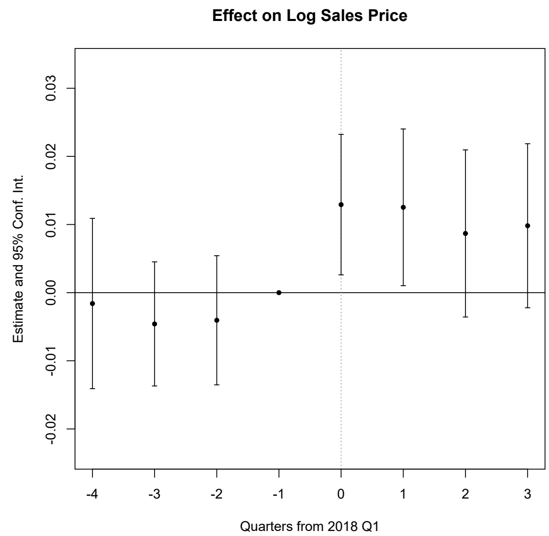
NOTE.——This figure plots securitization volumes, measured in terms of the number of loans originated each quarter that were later securitized, for captive lenders (red) and non-captive lenders (black).

Figure IA.3: Vehicle Invoice and Sales Prices

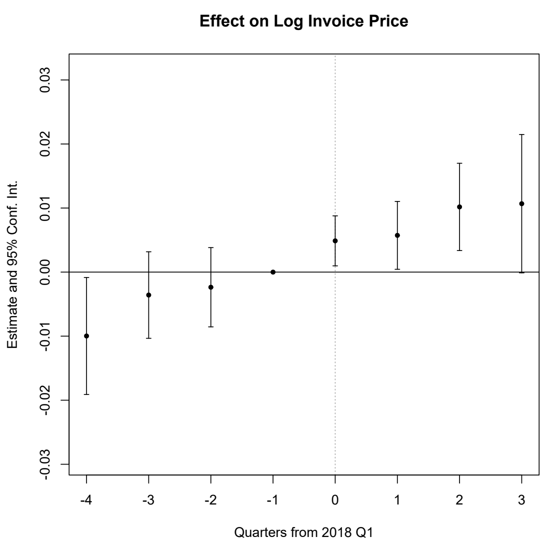
(a) Sales Price with Make Treatment



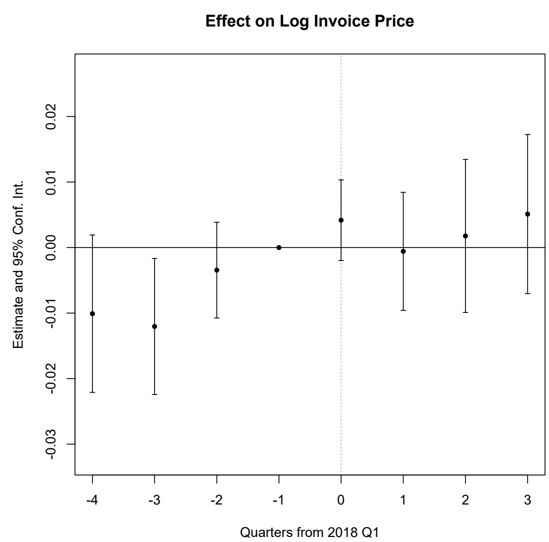
(b) Sales Price with Make-Model Treatment



(c) Invoice Price with Make Treatment

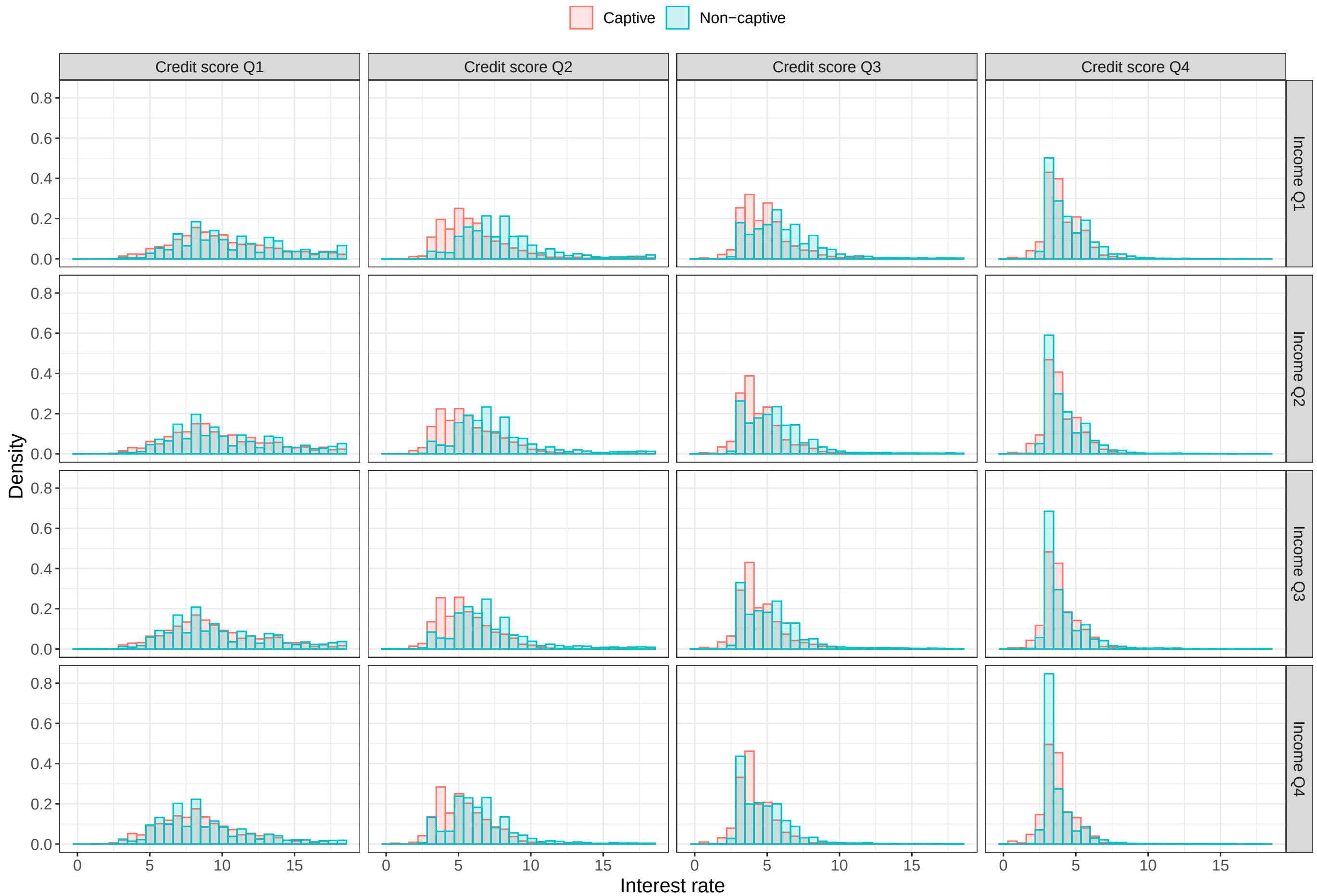


(d) Invoice Price with Make-Model Treatment



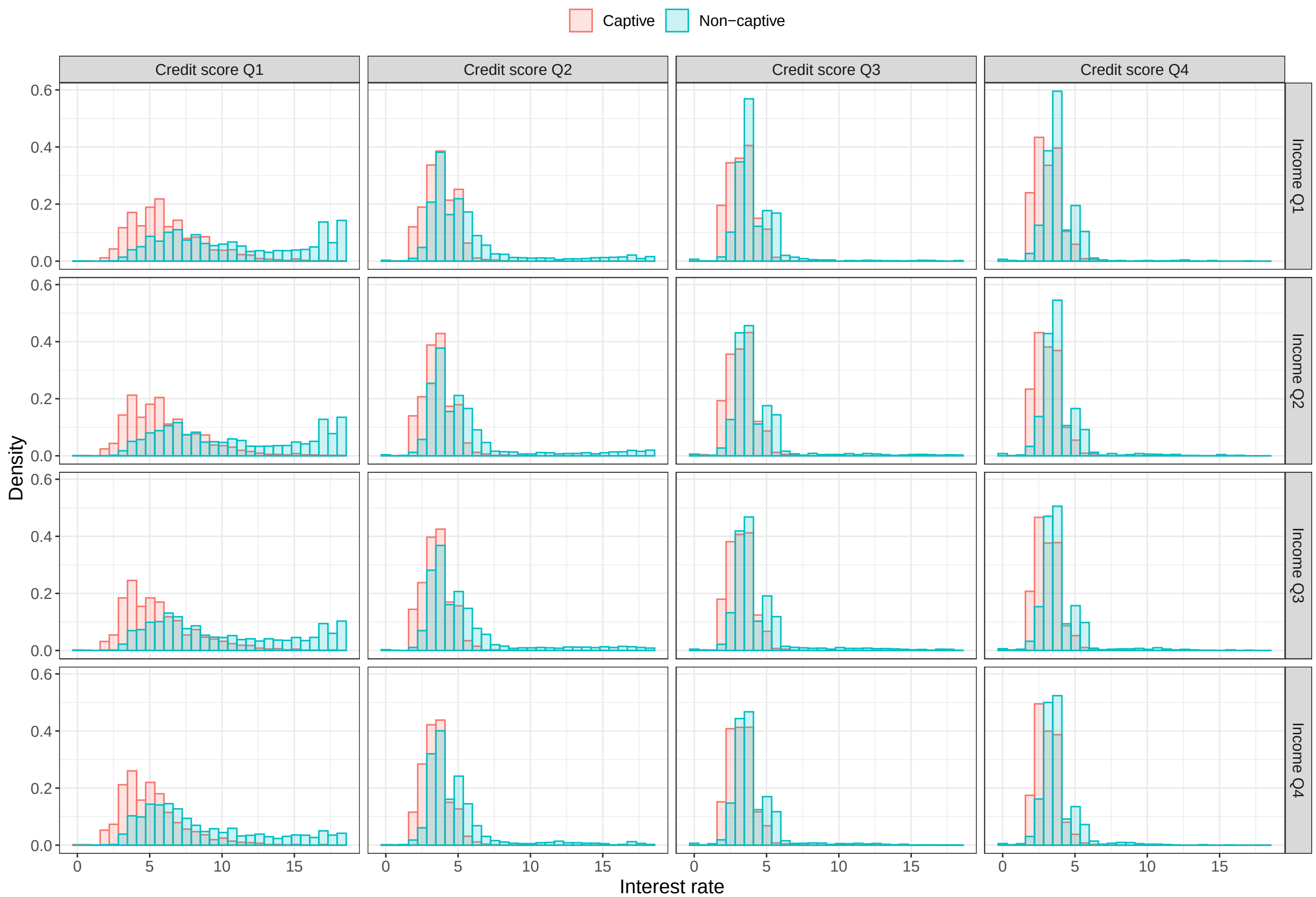
NOTE.——This figure plots coefficient estimates from Equation 6. The dependent variable is either the log sales price or log invoice price. The x-axis corresponds to the number of quarters from the treatment date. The quarter $\tau = -1$ is the reference quarter. The circles correspond to the coefficient estimates, and the vertical bars correspond to 95 percent confidence intervals. The sample and variable definitions are the same as in Table 6. Standard errors are clustered by either vehicle makes (Panels A and C) or make-models (Panels B and D).

Figure IA.4: Distribution of Non-Subvented Interest Rates for Used Vehicles



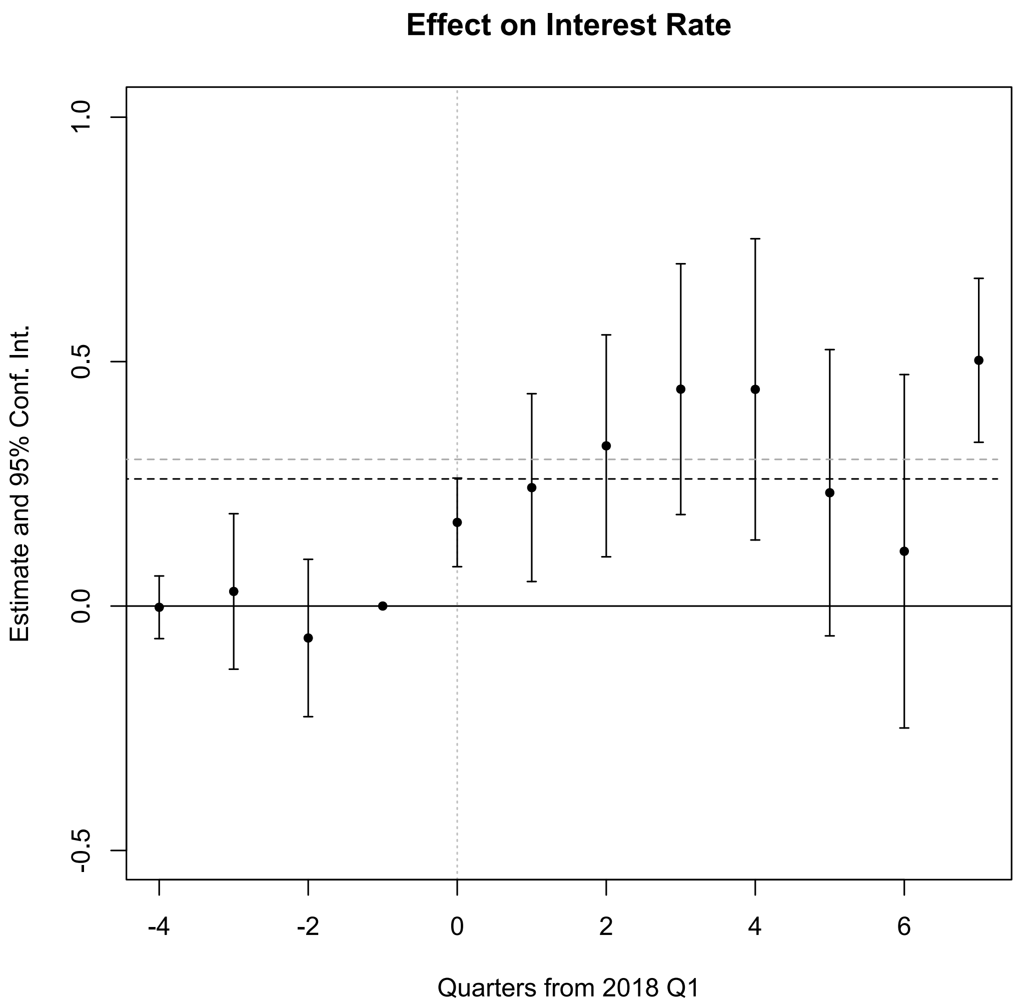
NOTE.——This figure plots the distribution of non-subvented captive interest rates (red) and non-captive interest rates (blue) for used vehicles. The sample is restricted to non-subvented used vehicle loans that were originated during the pre-treatment period of January 2017 to December 2017. The columns correspond to quartiles of the credit score distribution, and the rows correspond to quartiles of the income distribution. Each panel depicts the interest rate distribution for a particular credit score quartile × income quartile combination.

Figure IA.5: Distribution of Non-Subvented Interest Rates for New Vehicles



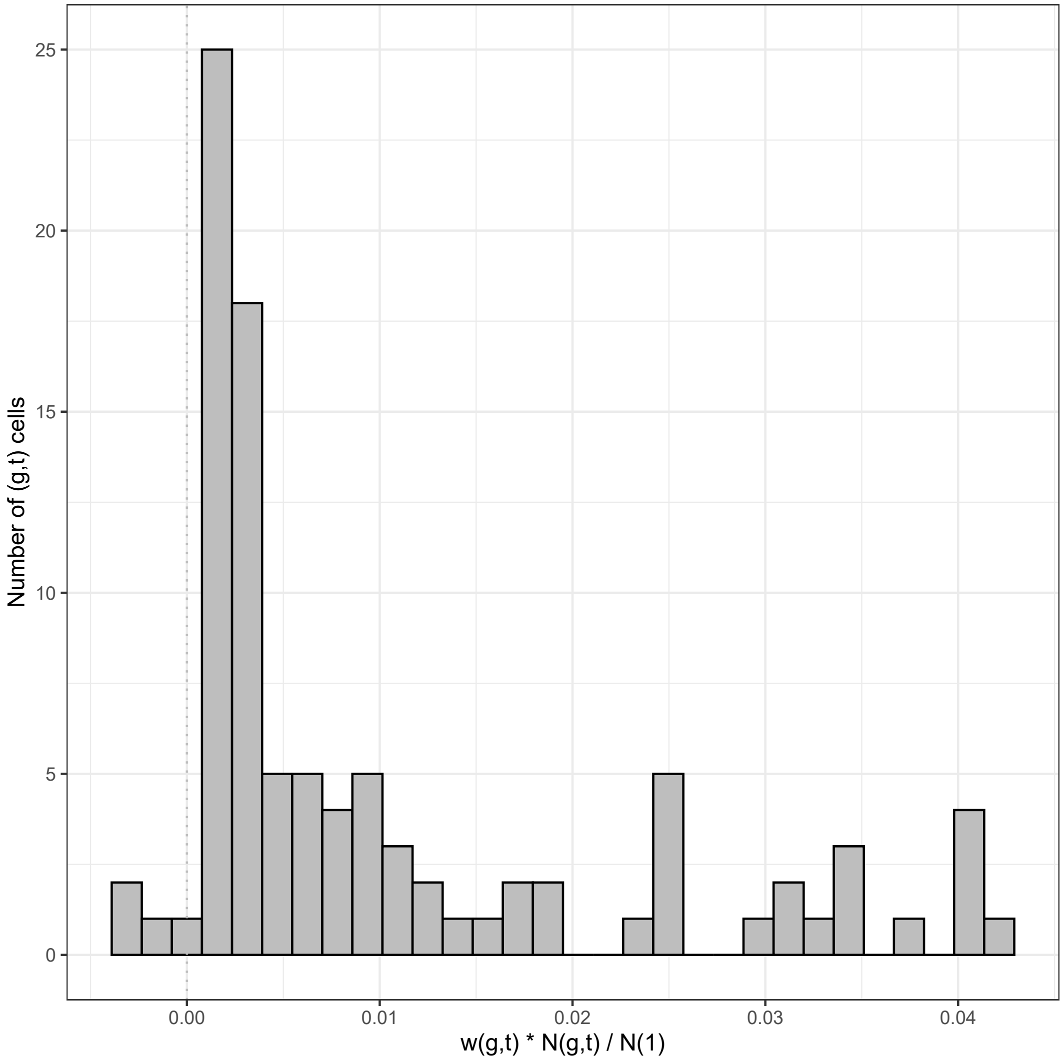
NOTE.——This figure plots the distribution of non-subvented captive interest rates (red) and non-captive interest rates (blue) for new vehicles. The sample is restricted to non-subvented new vehicle loans that were originated during the pre-treatment period of January 2017 to December 2017. The columns correspond to quartiles of the credit score distribution, and the rows correspond to quartiles of the income distribution. Each panel depicts the interest rate distribution for a particular credit score quartile × income quartile combination.

Figure IA.6: Long-Run Effect on Interest Rates



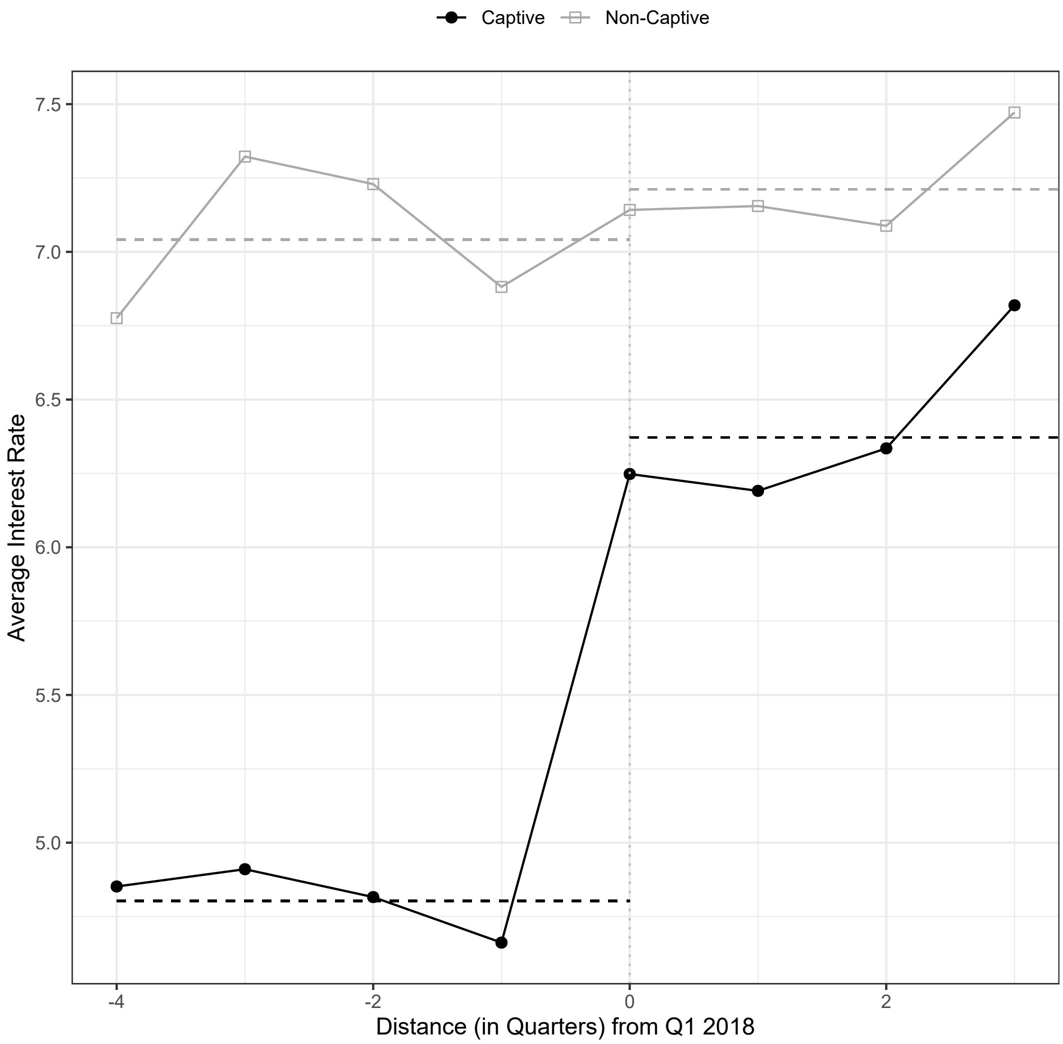
NOTE.——This figure plots coefficient estimates from Equation 3 after extending the sample period to Q4 2019. The dependent variable is the interest rate. The x-axis corresponds to the number of quarters from the treatment date. The quarter $\tau = -1$ is the reference quarter. The circles correspond to the coefficient estimates, and the vertical bars correspond to 95 percent confidence intervals. The dashed black line corresponds to our baseline difference-in-differences estimate of 26 basis points from Table 3. The gray dashed line corresponds to 30 basis points, which is the estimate we arrive at when we re-estimate our baseline difference-in-differences model on the extended sample period. The sample is restricted to auto loans originated between January 2017 and December 2019 that do not have subsidized financing. Standard errors are clustered at the lender level.

Figure IA.7: Weights Used to Construct Difference-in-Differences Estimate



NOTE.——This figure plots the histogram of group-time weights used to construct our baseline difference-in-differences estimates. Groups are defined in terms of lenders, and time is defined in terms of origination months. For more details on this procedure, see de Chaisemartin and D'Haultfoeuille 2020.

Figure IA.8: Average Captive and Non-Captive Interest Rates



NOTE.——This figure plots the average captive and non-captive interest rates during the sample period. The sample is restricted to non-subvented loans that were originated between January 2017 to December 2018. The dashed horizontal lines to the left of zero correspond to average captive and non-captive interest rates during 2017. The dashed horizontal lines to the right of zero correspond to average captive and non-captive interest rates during 2018.