Does Trade Policy Affect Consumer Credit? The Role of Captive Finance*

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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 consumers received higher interest rates and worse loan terms from captive lenders after the tariffs relative to unaffected non-captive lenders. The worse loan terms represent a tightening of credit along the intensive margin, not a shift in the composition of borrowers. Further, we document a disparate impact on low-income borrowers and in areas with less lending competition. Our results suggest that focusing solely on directly affected product prices may underestimate tariff pass-through.

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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 durable goods manufacturers both produce goods as well as provide financing through their wholly owned captive finance subsidiaries (Murfin and Pratt 2019). Thus, vertically integrated lending units provide these firms with an alternative channel for transferring the cost of a tariff to consumers.

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 (Roberts 2018; 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 to less sophisticated or higher demand customers? We present novel evidence that the impact of tariffs is not limited to direct producer and consumer prices but also affects consumer credit terms. Further, our granular data allows us to document both the role of vertical integration in heterogeneous firm-level responses to tariffs as well as the disparate impact on low-income borrowers. While the transmission of monetary policy to household credit is well-documented (Bernanke and Gertler 1995; Di Maggio et al. 2017; Di Maggio, Kermani, and Palmer 2020), we believe our paper provides the first evidence on how trade policy affects consumer credit terms.

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 firms (captive auto lenders) as well as non-integrated financial institutions (non-captive lenders). While captive lenders were exposed to the metal tariffs through the manufacturing side of their businesses, non-captive lenders had no direct exposure, providing us with a natural counterfactual. Further, auto purchases often bundle the vehicle purchase price with financing terms, creating an opportunity for price shrouding (Gabaix and Laibson 2006). If consumers are less sensitive to increases in loan prices than vehicle prices, it could be optimal for an automobile manufacturer to pass on some or all of a cost shock through its financing terms. The main question we ask is whether tariffs affected the provision of consumer credit in the auto loan market. Using loan-level data, we find that the tariffs resulted in higher interest rates and worse loan terms for borrowers from captive lenders, and that the impact of the tariffs was most pronounced among lower-income borrowers with less elastic loan demand and in areas with lower lending competition. Overall, our results suggest that integrated manufacturer-lenders passed on a non-trivial component of tariff-related costs via higher financing costs. Thus, ignoring this channel would understate the degree of cost pass-through, much akin to Nakamura and Steinsson 2012.

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 (Sweet 2015). 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. 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 overall 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 lenders adjust their terms to clear the market (Adams, Einav, and Levin 2009). To resolve this and other empirical 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.[1] The granular nature of the Regulation AB II data allows us to examine the evolution of loan terms 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 estimate our difference-in-differences models on a sample of auto loans originated between January 2017 and December 2018. This sample period reflects a 24-month window around the Department of Commerce's January 2018 recommendation to impose the metal tariffs. Our main result is that captive auto lenders charged higher interest rates following the recommendation of the tariffs. Relative to loans from non-captive lenders, auto loans from captive lenders experienced a 26 basis point increase in their average interest rates. This represents a 10 percent increase in interest rates when compared to the pre-treatment captive average of 252 basis points, and it implies that auto manufacturers passed on 36 percent of tariff-related costs to consumers through their captive lenders. (See Section 4.5 for calculations.) We also examine how captive lenders adjusted their loan amounts, maturities, and loan-to-value ratios in response to the tariffs. Although interest rates seem to be the main margin of adjustment, we find that other loan terms also became somewhat less accommodating following the announcement of the tariffs. Furthermore, in support of our empirical setting, we find no evidence of differential pre-trends for all our outcome variables.

What drives the observed worsening of captive auto loan terms? The answer to this question is not immediate 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 and provided them with worse loan terms. However, another possible explanation is that our results capture changes in the composition of captive borrowers along the extensive margin. For example, captive lenders might have relaxed their underwriting standards and expanded their

1. One potential issue with using loans from non-captive lenders as our control group is that there could be spillover effects from the tariffs on these lenders. In Section 4.1.1, we construct a simple theoretical model to gauge the magnitude of such spillover effects and find that spillovers should exert a small attenuation bias on our estimates.

pool of borrowers to compensate for lower vehicle sales margins. Demand-side responses to higher borrowing or vehicle costs – such as adverse selection or good borrowers switching from captive to non-captive lenders – could have also produced an overall riskier pool of captive borrowers as well as tighter lending conditions (Stiglitz and Weiss 1981).

To better understand whether our results reflect 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 average household incomes, credit scores, or future default rates for the pool of captive borrowers following the announcement of the tariffs. We also find that captive loan origination volumes decreased in response to the tariffs (which represents a cost to captive lenders), but this decline in loan origination volumes is not correlated with observable borrower characteristics or future default rates (à la Argyle, Nadauld, and Palmer 2023). To the best of our knowledge, our paper is the first to document that auto manufacturers used their captive lenders to pass on higher input costs from the tariffs. We focus the rest of our tests on understanding the economic mechanisms behind this pass-through decision.

Theories of cost pass-through predict that firms will find it easier to pass on costs along margins where consumers are less price sensitive (Chen and Juvenal 2016). Given that Grunewald et al. 2023 find that consumers are less sensitive to increases in loan prices than vehicle prices, auto manufacturers might have chosen to pass on some portion of the tariffs through their financing terms to limit the overall impact on demand. To explore the role of borrower demand in determining tariff pass-through, we re-estimate our difference-in-differences model across three proxies for loan price sensitivities. For our first and second proxies, we build on Attanasio, Goldberg, and Kyriazidou 2008 and Argyle, Nadauld, and Palmer 2020, which find that low-income and low-credit score borrowers are less sensitive to increases in loan prices than high-income and high-credit score borrowers. Third, we examine how pass-through varies across loan amounts, as smaller loan amounts could be indicative of more binding credit constraints and lower loan price sensitivities (Adams, Einav, and Levin 2009). Consistent with the above predictions, we find that pass-through to loan prices is higher when borrowers have lower incomes, lower credit scores, and smaller loan amounts.

Theories also predict that the degree of cost pass-through will depend on market structure and competition. In particular, Weyl and Fabinger 2013 show that for a cost shock such as the tariffs that affected the marginal costs of captive lenders but not non-captive lenders, the pass-through rate should be increasing as the level of competition declines. To measure lending market competition, we calculate state-level Herfindahl–Hirschman indexes based on pre-treatment lender market shares (Yannelis and Zhang 2021). Consistent with the above theories, we find that pass-through is higher in states with lower lending market competition. Combined, our results suggest that the metal tariffs had a disparate impact on low-income borrowers and borrowers in areas with lower lending 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).

Finally, in addition to financing terms, an auto manufacturer could also pass through a cost shock to its new vehicle prices. Supplementing the Regulation AB II data with vehicle sales price data from the Texas Department of Motor Vehicles, we find that auto manufacturers passed on 42 percent of tariff-related costs through higher new vehicle prices. Given that 36 percent of these costs were also passed on via higher loan prices, our results suggest that commonly used methods of measuring tariff incidence that focus solely on directly affected goods' sticker prices would understate the economic impact on American consumers by almost one-half.

Our paper contributes to three distinct strands of literature. First, there is a growing literature on the economic incidence of the 2018 Trump administration import tariffs. Several studies – such as Amiti, Redding, and Weinstein 2019 and Fajgelbaum et al. 2020 – have documented evidence of complete pass-through of these tariffs to domestic import and producer prices. Yet, 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 the cost of the tariffs. 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 documenting that automobile manufacturers used their captive finance subsidiaries to pass on higher costs from the tariffs, we highlight not only the impact of the metal tariffs on consumer credit, but also how focusing on the direct impact on specific goods' prices may understate tariff incidence. 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 2004; Hong and Li 2017).[2]

Second, our paper adds to the literature on captive finance (Banner 1958; 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 companies allow durable goods manufacturers to solve the Coase 1972 conjecture, and Barron, Chong, and Staten 2008 argue that captive lenders allow manufacturers to consummate sales with profitable but credit-rationed consumers. Consistent with captive finance companies serving a unique purpose relative to non-integrated financial institutions, we show that captive lenders provide an additional outlet through which manufacturers can pass on cost shocks to consumers. 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 literature on shrouded attributes and add-on pricing (Ellison 2005; Gabaix and Laibson 2006; Brown, Hossain, and Morgan 2010). We note that the role of captive lenders is not limited to auto manufacturers. Numerous other durable goods firms – including Boeing, Caterpillar, and Polaris – also reported higher input costs due to the metal tariffs as well as higher captive financing revenues during this period.[3]

Third, our paper contributes to the broader literature on the transmission of economic shocks from firms to consumer credit. Within this literature, two recent papers examine the effects of

2. Of course, our paper is also related to the broader fields of tax incidence, cost pass-through, and exchange rate pass-through. These literatures span several decades and include the work of Hotelling 1932, Spengler 1950, Harberger 1962, Bulow and Pfleiderer 1983, Poterba 1989, Campa and Goldberg 2005, Chetty, Looney, and Kroft 2009, Nakamura and Zerom 2010, Weyl and Fabinger 2013, Irwin 2019, and Genakos and Pagliero 2022. In Section 4.5, we provide a more thorough comparison of our pass-through estimates to estimates from these literatures.

3. 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."

market-wide and firm-specific funding shocks on captive auto loan terms. Benmelech, Meisenzahl, and Ramcharan 2017 find 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 find that short-term increases in manufacturer credit default swap spreads are associated with worse captive auto loan terms and more relaxed captive lending standards.[4] In contrast to these papers, we examine how captive auto lenders responded to an input cost shock on the manufacturing side of their business. We show that captive lenders charged inframarginal borrowers higher loan prices in response to higher input costs, and that neither changes in lending standards along the extensive margin nor concomitant changes in funding costs drive this result. 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 shock.

The remainder of the paper is organized as follows. Section 2 provides institutional background on the auto loan market and the 2018 Trump import tariffs. Section 3 describes the Regulation AB II data and presents our main sample. Section 4 documents the effect of the tariffs on captive auto loans, and Section 5 examines the channels driving our results. Section 6 concludes.

2 Institutional background

Evaluating the impact of trade policy on consumer credit requires understanding 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.

4. For several reasons, the used car "credit fire sale" channel described in Benneton, Mayordomo, and Paravisini 2022 is not applicable in 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 United States. Hence, a credit fire sale will be a cash-draining activity in our setting, whereas it is cash-generative in theirs. We note that Benneton, Mayordomo, and Paravisini 2022 study the European used car market where some manufacturers own their own dealerships and hold used car inventories. In Table A.2, we show our results hold for both used and new vehicles.

2.1 Captive lenders

Most auto manufacturers have their own captive lenders 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.[5] 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 independent finance companies at 12 percent (Experian 2021). Within the different segments of the auto loan market, captives tend to perform best in terms of new vehicle lending (2019 market share of 54 percent). This is because captive lenders often provide subsidized financing for their own brands of new vehicles. For example, GM Financial sometimes offers zero percent financing or cash-back incentives to "well-qualified borrowers" to entice them into purchasing certain new GM models. Non-captive lenders are often less willing to provide such 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 the balance sheet.[6] 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

5. Various laws in the United States prohibit automobile manufacturers from selling new vehicles directly to consumers. Hence, independently-owned franchised automobile dealerships intermediate the new vehicle sales process. Franchised dealerships have exclusive contracts to purchase new vehicles from their affiliated manufacturer at the invoice price and then to sell these vehicles to consumers at various retail prices, such as the MSRP.

6. 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.

in their own asset-backed securities. Furthermore, in contrast to GSE-eligible 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.

Franchised auto dealerships intermediate the origination of most captive auto loans (Romero 2017). In a standard vehicle sale that involves financing, a franchised auto dealership sends the consumer's credit application to both their captive lender and other non-captive lenders. Lenders wishing to finance the transaction submit bids to the dealer, and the dealer then accepts the most profitable bid with terms acceptable to the consumer (Jansen et al. 2021). Afterwards, the auto loan is originated and sold to the winning lender.[7] Through this bidding process, captive lenders compete with non-captive lenders for desirable auto loans. For example, the 2018 10-K of Ally Financial (a non-captive lender) states, "captive automotive finance companies compete vigorously with us". Ford Credit's (a wholly owned subsidiary of Ford Motor) 2018 10-K lists "other automobile manufacturers' affiliated finance companies" as competitors along with banks and credit unions. Finally, Grunewald et al. 2023 find that auto dealers solicit bids from 4.35 non-captive lenders, on average, for each financing transaction.

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.[8] Commerce's report, submitted in January of 2018 and made public on February 16 of that year, recommended a range of possible

7. This process – known as indirect or dealer-arranged financing – is described in greater detail in Cohen 2012, Brown and Jansen 2020, and Grunewald et al. 2023. While our current description is brief, we highlight other important aspects of indirect auto lending (such as dealer markups) in later sections. We note that consumers also can obtain auto loans directly from non-captive lenders. However, indirect auto loans account for over 80 percent of financings in the United States (Romero 2017), and most captive lenders do not engage in direct lending.

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

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.[9] 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 as well as the steel mill products categories, while aluminum prices also rose around 3 percent (Figure 1). 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) and from strategic pricing responses by domestic producers (Amiti, Redding, and Weinstein 2019).[10] 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. This was apparent in both the stock market and their corporate announce-

9. 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." Further, 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.

10. Many models of imperfect competition predict that firms will raise their prices when their competitors experience a cost shock. Consistent with this prediction, Fajgelbaum et al. 2020 find that the 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. In a case study of the January 2018 tariffs on washing machine imports, Flaaen, Hortacsu, and Tintelnot 2020 find that domestic brands of washing machines (which were not subject to the tariffs) increased their prices by a similar amount as foreign brands (which were subject to the tariffs). See also Pierce 2011, Amiti, Itskhoki, and Konings 2016, and Feenstra and Weinstein 2017.

ments.[11] 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".[12] As detailed further in Section 4.5, we estimate that the average cost of producing a new vehicle domestically in the United States rose by around $300 in response to the tariffs.

Firms have a variety of tactics available 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

11. 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.

12. 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 response to the broad-based import tariffs on Chinese goods that began in mid-2018, China increased their tariffs on U.S. vehicle imports from 25 to 40 percent. We note that these tariffs on U.S.-made vehicles had a negligible impact on U.S. auto companies, as most vehicles from U.S. auto companies that are sold in China are also manufactured in China (Roh 2019). (Nevertheless, in Table A.13, 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. Finally, 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 did in fact rise during this period, and auto companies highlighted significant cost increases well into 2018.

to which consumer prices were affected (Cavallo et al. 2021). Automobile manufacturers, which can adjust both the wholesale price of the vehicle and the price of the captive financing in response to a cost shock, offer an interesting venue to revisit the measurement of tariff price incidence.

Anecdotal evidence suggests that auto manufacturers might have passed on some portion of the tariffs using their captive finance units. For example, Figure 2 presents GM's revenues and profits in the year before the tariffs and the year of the tariffs. Revenues and profits are split between GM's vehicle sales segment and its captive financing segment, GM Financial. During the year of the tariffs, GM's vehicle sales segment experienced a significant decline in profits. This decline in profits was not due to a decline in revenues, but rather a sharp increase in costs. In contrast, both revenues and profits rose at GM Financial in 2018.

The decision to pass through tariff costs to vehicle and/or financing prices should depend on several factors, including the relative elasticities and curvature of demand to vehicle prices and loan prices (Chen and Juvenal 2016) and the relative degrees of competition in the vehicle product and financing markets (Weyl and Fabinger 2013). Expanding on the evidence that the 2018 tariffs affected product prices, we document pass-through to financing costs. This pass-through to the less salient, add-on component of vehicle purchases is consistent with Grunewald et al. 2023, which finds that consumers are less sensitive to changes in loan prices than vehicle prices, as well as evidence that some consumers make suboptimal decisions when purchases have add-on features (Ellison 2005; Gabaix and Laibson 2006) or some features are less salient (Chetty, Looney, and Kroft 2009; Brown, Hossain, and Morgan 2010; Bordalo, Gennaioli, and Shleifer 2012). In addition, the fact that auto companies appear to have spread this cost shock across a bundled set of goods is consistent with Flaaen, Hortacsu, and Tintelnot 2020, which finds that washing machine and dryer prices both rose in response to tariffs on washers, as well as anecdotal evidence that retailers often spread cost increases across multiple products to limit the impact on sales (Kapner and Nassauer 2019).

2.4 Non-captive lenders as controls

To control for other factors influencing the auto loan market during this period, we use non-captive lenders as our control group in a difference-in-differences design. While captive lenders were exposed to the steel and aluminum tariffs through the manufacturing side of their business, non-captive lenders – i.e., their direct competitors – had no such exposure. (To the best of our knowledge, there were no concomitant economic shocks that were specific to non-captive lenders during our sample period.) Our baseline model in Section 4 compares loans from captive lenders to loans from non-captive lenders on the same vehicle and with similar borrower-level characteristics. Further, as we show later in Figures 4 and 5, we find no evidence of differential pre-trends between captive and non-captive lenders. This is consistent with the parallel trends assumption being satisfied.

One issue with using loans from non-captive lenders as our control group is that there could be spillover effects from the tariffs on these lenders. For instance, due to both competitive supply-side factors and demand-side responses, it might be optimal for non-captive lenders to raise their loan prices should the tariffs force captive lenders to raise theirs (Agrawal and Hoyt 2019; Berg, Reisinger, and Streitz 2021). As noted in Flaaen, Hortacsu, and Tintelnot 2020, the presence of such spillover effects complicates the measurement of tariff incidence because it causes difference-in-differences to understate the true price effects (i.e., introducing attenuation bias). Thus, in Section 4.1.1, we develop a simple model of imperfect competition in the auto loan market to gauge the theoretical magnitude of such spillover effects in our setting, and we use this model to adjust and interpret our difference-in-differences estimates later in the paper.

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 each month.[13] 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 common to most consumer credit datasets such as loan amounts and maturities, the Regulation AB II data also contains several unique variables that are crucial for 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.) This feature of the data 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 or loans with cash or interest rate subventions. 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.[14]

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 (183 million loan-months) in the data. The 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-

13. 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 issuers can include seasoned loans in their ABS offerings, and hence the earliest loan origination date in the Regulation AB II data is in 2010. For more information on Regulation AB II, see Sweet 2015 and Neilson et al. 2020.

14. 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. To induce a non-captive lender to provide subsidized financing, auto manufacturers must compensate the lender for the below-market rate of return. For example, Ally's 2018 annual report reads: "Automotive manufacturers may elect to sponsor incentive programs on retail contracts...by subsidizing finance rates below market rates. These marketing incentives are also referred to as rate support or subvention. When an automotive manufacturer subsidizes the finance rate, we are compensated at contract inception...".

captives.[15] 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 auto loan portfolios of the included lenders.[16]

As stated above, the Regulation AB II data includes the invoice price for new vehicles, which provides us with a coarse estimate of the auto manufacturers' revenues from the vehicle's sale to the dealership. This allows us one angle to measure the relative importance of pass-through on 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.[17] To evaluate the pass-through to final sales prices, we use vehicle sales price data from the Texas Department of Motor Vehicles (Hoekstra, Puller, and West 2017). 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 data does not contain borrower or loan characteristics, it does contain information on the make-model-year of each vehicle sold. Thus, the Texas data allows us to estimate the impact of the tariffs on new vehicle sales prices.

3.1.1 How representative is the Regulation AB II data?

The Regulation AB II data is representative of both the auto loan portfolios of the 19 included lenders as well as the population of auto loans in the United States. First, in terms of the former, Table A.1 compares average loan characteristics for the included lenders across two datasets: (i) the Regulation AB II data (which is limited to securitized loans) and (ii) population credit bureau data (which contains both securitized and unsecuritized loans). That is, for each lender in the Regulation AB II data, Table A.1 compares the features of their securitized loan pool to their entire

15. Of the top ten non-captive lenders, those that are not in the 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, instead of issuing public auto loan ABS, they hold most of their auto loans on their balance sheets. 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.

16. In the United States, 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 active auto loans at each point in time.

17. The data also does not contain information on down payment amounts, so we are unable to back out sales prices from loan amounts.

portfolio of loans. Inconsistent with most concerns related to selection during the securitization process, we find similar average loan characteristics across the two datasets. Later, in Section 4.7.6, we also show that the included lenders did not change their securitization practices in response to the tariffs.

Second, in terms of the latter, Momeni and Sovich 2022 compare the Regulation AB II data to population auto loan data and find that the Regulation AB II data is representative in terms of average loan characteristics. For instance, relative to the entire population of auto loans (and not just auto loans from the included lenders), the Regulation AB II data has similar average loan amounts, balances, maturities, scheduled monthly payments, and default rates. However, the average credit score and household income in the Regulation AB II data is somewhat higher than in the population, which is mostly due to the fact that the composition of lenders is different across these two data sources. Specifically, while the Regulation AB II data consists of securitized auto loans from captive lenders and large banks, the population of auto lenders also includes thousands of small banks, credit unions, and independent finance companies that do not utilize public securitization markets and lend to lower credit score borrowers to a greater extent. We note that this difference should not be an issue though because our paper focuses on captive auto lenders; all the major captives in the United States are present in the Regulation AB II data.

3.2 Sample

We restrict our sample to auto loans originated within 12 months of the January 2018 treatment date (January 2017 to December 2018).[18] We also require that loans have the the following data fields populated: 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 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

18. Our choice of treatment date is conservative as it reflects the date of the Department of Commerce's initial recommendation. As shown later in Figure 4, our results are more pronounced in the later part of the sample period when the tariffs bind to a greater extent and metals prices have risen higher.

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 discussed further in Section 4.8, our results are robust to relaxing or tightening these sample filters.

For various reasons, we remove 5 of the 19 lenders that are in our data from the 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.[19] Finally, we remove Nissan because it issued a large vehicle recall in October 2017 right before the tariffs. As shown later in Section 4.8, our results are robust to reincluding these lenders in the sample.

Our final sample consists of 1,973,067 auto loans from 127 distinct asset-backed securities and 14 lenders. Figure 3 plots the distribution of 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.[20] Table 1 presents descriptive statistics as of each loan's origination date. 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 auto loan default rate is 1.20 percent.

The right-most columns in Table 1 compare loans from captive (i.e., treated) lenders to non-

19. While having its own integrated steel manufacturer might 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.

20. 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 A.15, our results are also robust to excluding World Omni Financial from the sample.

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 ($26,914 versus $22,256), 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) than non-captive loans. 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 fixed effects. Indeed, as shown later in Figures 4 and 5, we find no evidence of differential 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.

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:

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

where the outcome variable, $y_{i,t}$, is the interest rate of loan i originated in quarter t, and the dummy 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). The coefficient of interest, Γ, captures the pre-treatment average difference in interest rates between captive and non-captive loans that are 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. Conditional on vehicle and borrower characteristics, the pre-treatment average interest rate for captive auto loans is 190 basis points lower than for non-captive auto loans (t = -3.61). Some of this difference can be attributed to captive lenders providing subsidized financing on select vehicle models. However, even after we remove subsidized loans in Column 2, the pre-treatment average interest rate for captive auto loans is still 98 basis points lower than for non-captive loans (t = -1.73). Among other explanations, this persistent gap could be due to institutional differences between captive and non-captive auto lenders. For example, due to their relationship with their manufacturer, 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 mark-ups. (Consistent with the latter explanation, Grunewald et al. 2023 find that the average dealer mark-up on non-captive auto loans is 108 basis points.). Stroebel 2016 documents a similar pattern of captive lenders charging lower interest rates in the mortgage market and attributes the result to adverse selection surrounding collateral values.

Later in the paper, we use the size of the above gap to rationalize the limited movement of borrowers away from captive lenders following the tariffs. That is, even if the average captive lender enacted a large across-the-board increase in interest rates, the average borrower might still be better off receiving a captive auto loan than a non-captive auto loan. We discuss such demand-side responses in greater detail in Section 4.6.

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 auto loan terms became worse following the tariffs, there was no change in the composition of captive borrowers.

4.1 Interest rates

To begin, we estimate the effect of the tariffs on the average interest rate of captive auto loans. The regression model is:

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

where the outcome variable is the interest rate of loan i originated in quarter t. As in Equation 1, the dummy 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 (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.[21] We also include separate origination quarter fixed effects for states ($\delta_{s,t}$), income bins ($\delta_{w,t}$), and credit score bins ($\delta_{c,t}$) to control for common borrower-level shocks across captive and non-captive lenders.[22] The coefficient of interest, Γ, measures the conditional average change in interest rates for

21. We emphasize that the inclusion of vehicle make-model-condition fixed effects in Equation 2 helps ensure that the Γ coefficient captures tariff pass-through and not demand-side purchasing responses. That is, our baseline model holds the choice of vehicle fixed when measuring the impact of the tariffs, which is important to do because prior studies have found that consumers might adjust their vehicle choices in response to changes in loan terms (Argyle, Nadauld, and Palmer 2023). Later, in Section 4.6, we relax these fixed effects to examine the scope of demand-side 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.

22. The vehicle and borrower fixed effects are also important because captive and non-captive lenders are not balanced along these dimensions prior to treatment. Hence, their exclusion would make our estimates susceptible to biases arising from differential time shocks along these dimensions. For example, the vehicle origination quarter fixed effects help control for the effects of differential price shocks to used versus new vehicle values following the tariffs.

captive auto loans relative to non-captive auto loans on the same vehicle that were issued to similar borrowers. The 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.

Considering the large number of fixed effects, the regression model can be conceptualized as follows. Consider two points in time: one before the tariffs and the other after. Consider also four individuals that decide to purchase used 2015 Ford F-150s from their local franchised Ford dealership. Suppose all four individuals live in the same state and have similar household incomes and credit scores. However, two of these individuals received their auto loans from Ford Credit (one before the tariffs and the other after) while the other two received their auto loans from Fifth Third (also one before the tariffs and the other after). In effect, our model compares the before-and-after change in interest rates for the two loans from Ford Credit to the before-and-after change in interest rates for the two loans from Fifth Third. We then re-calculate this change across all vehicle and borrower segments of the population to arrive at our main coefficient estimate.

Panel A in Table 3 reports the coefficient estimates from Equation 2. Relative to auto loans from non-captive lenders, auto loans from captive lenders experienced a 26 basis point increase in their average interest rates following the announcement of the tariffs. This represents a 10 percent increase in captive interest rates when compared to the pre-treatment average of 252 basis points, or a present value increase in total loan payments of $179 (0.66% of the pre-treatment average captive loan amount).[23] Panel B reports the coefficient estimates after removing subsidized loans from the sample. Similar to Panel A, we find that the average captive interest rate went up 29 basis points relative to non-captive loans. Thus, the increase in captive interest rates does not simply reflect fewer marketing promotions but is pervasive across the auto loan market.

And the income and credit score origination quarter fixed effects help control for the effects of differential income shocks across the income and credit score distributions. For instance, the tariffs might have had a positive impact on low-wage workers in the manufacturing sector, which in turn could have resulted in these individuals receiving lower interest rates for standard risk-based pricing reasons. We also note that we have ample observations within each dimension of our fixed effects. 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 origination quarter cell, and 10,279 observations within each credit score origination quarter cell.

23. Discounting at 5 percent, for a pre-treatment average $26,914 captive loan with 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 difference in total loan payments of $178.62. For a similar calculation, see Argyle, Nadauld, and Palmer 2023.

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 impact of the tariffs evolved during our sample period, we estimate the following regression model:

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

where $\text{D}_{t,\tau}$ is equal to one whenever quarter t is τ quarters from the treatment date. In the model, we exclude the quarter prior to the treatment date ($\tau = -1$) 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 the treatment date.

The results of the estimation are shown in Figure 4. Given that there is seasonal variation in subsidized loan offers that is 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 then 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 from Table 3. Consistent with the parallel trends assumption being satisfied in our setting, we find no 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.[24]

24. Later, in Section 4.7.7, we show that our results are concentrated within the subset of captive lenders that have significant domestic manufacturing exposure to the tariffs. This test helps further isolate the tariffs as the driving force behind our results, and it also helps alleviate two potential identification concerns. First, it helps alleviate the concern that our results might be capturing the effects of a time-varying, captive-specific omitted variable that coincides with the tariffs, and the not the effects of the tariffs per se. Second, it helps alleviate the concern that our results might be capturing the differential effects of a contemporaneous shock on captive lenders relative to non-captive lenders, such as differential exposure to various Federal Reserve policy changes during 2018.

4.1.1 Spillover effects

As mentioned in Section 2.4, captive lenders compete with non-captive lenders for auto loan originations. Hence, in response to a cost shock that forces captive lenders to raise their loan prices, non-captive lenders might find it optimal to raise their prices as well. In the context of our difference-in-differences model, such common spillovers will end up getting absorbed into the common time trends in the model. This, in turn, will cause our baseline coefficient estimate to understate the true price impact of the tariffs on captive auto loans (Berg, Reisinger, and Streitz 2021).[25]

Although we cannot measure the size of the above spillover effect from the data, in Appendix B we write down a simple model of the auto loan market to help us gauge its theoretical magnitude. Our model predicts that the spillover effect should be equal to the product of our baseline coefficient estimate and the pre-treatment market share of captive lenders, or 7 basis points in our pooled setting (= 26 basis points $\times$ 0.26). Combining this with our baseline coefficient estimate thus implies a spillover-inclusive increase in captive interest rates of 33 (= 26 + 7) basis points, on average, or $227 per loan in present value terms. Later, we use this spillover-inclusive estimate to form an upper bound for the rate of tariff pass-through coming from financing terms.

4.1.2 New versus used vehicles

Before we proceed, we note that captive lenders raised their interest rates for both new and used vehicles in response to the tariffs (Table A.2). This finding helps reinforce the interpretation that our results do not just reflect fewer marketing promotions on the behalf of captive lenders, and it also helps further differentiate our paper from existing studies on the auto loan market, most of which focus on used vehicle financing (e.g., Argyle, Nadauld, and Palmer 2020, Argyle et al. 2021, Benneton, Mayordomo, and Paravisini 2022, and Argyle, Nadauld, and Palmer 2023).

25. That is, our baseline coefficient estimate captures the differential effect of the tariffs on captive lenders and not the total effect, which also includes the common spillover. As noted in Berg, Reisinger, and Streitz 2021, most workhorse models of imperfect competition predict that the spillover effect from a marginal cost shock such as the tariffs will be homogenous across affected and unaffected firms. Thus, hetereogeneous spillover effects and their potential complications with fixed effects should not be a concern in our setting.

4.2 Non-price loan terms

Next, we examine how the metal tariffs impacted non-price loan terms. Panel A in Table 3 reports the coefficient estimates from Equation 2 when the outcome variable is either the log loan amount, log loan maturity, or the loan-to-value ratio. Although the effects are small, we find that non-price loan terms became somewhat less accommodating following the announcement of the tariffs. Relative to loans from non-captive lenders, auto loans from captive lenders experienced a 1.1 percent decrease in their average maturities and an 80 basis point decrease in their average loan-to-value ratios. The average captive loan amount also declined 0.8 percent in response to the tariffs, but the coefficient estimate is insignificant at the 10 percent level. Panel B reports the coefficient estimates after removing subsidized loans from the sample. While several of the coefficient estimates flip signs from negative to positive, we continue to find no material economic improvements in non-price loan terms for captive auto loans.

Figure 4 plots the evolution of non-price loan terms. Similar to before, we focus on the subsample of auto loans without subventions. Except for a small increase in average loan sizes near the end of the sample period, we find no significant improvements in non-price loan terms for captive auto loans. Moreover, consistent with the parallel trends assumption being satisfied, we find no meaningful evidence of differential pre-trends in our setting. Overall, our results suggest that captive lenders primarily responded to the tariffs by raising their interest rates. This choice appears to be consistent with profit maximization, as prior studies such as Attanasio, Goldberg, and Kyriazidou 2008 find that auto loan demand is less sensitive to interest rates than offered maturities.

Before we proceed, we note that although Argyle, Nadauld, and Palmer 2020 find that borrowers decrease their auto loan amounts to keep their monthly payments constant when faced with higher interest rates, we find no evidence of such monthly payment smoothing in our setting. Indeed, if we re-estimate our model with the log monthly payment as the outcome variable, then we find that the tariffs resulted in a 1.0 percent ($t = 1.84$) increase in average payments for all captive auto loans and a 1.5 percent ($t = 3.38$) increase for non-subvented captive auto loans. One reason that our results might differ from those in Argyle, Nadauld, and Palmer 2020 could be because our data primarily contains indirect auto loans for both new and used vehicles from captives and large banks, whereas their data contains direct auto loans for used vehicles from credit unions. Indeed, while borrowers in the direct auto loan market often know their rates prior to selecting their vehicle and negotiating the price, it is often the opposite in the indirect auto loan market (Grunewald et al. 2023).

4.3 Composition of borrowers

So far, we have framed our results in terms of the intensive margin: in response to the metal tariffs, captive lenders charged inframarginal borrowers higher interest rates. However, changes in the composition of borrowers along the extensive margin could also produce higher average captive loan prices. 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 captive 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.[26]

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

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

where the outcome variable is either the log credit score, log household income, or future default rate of loan i originated in quarter t. The coefficient of interest is Γ, which measures the average

26. Concerns about composition effects arise because our data contains information on originated loans and 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, along with an estimate of the effect on demand.

change in borrower characteristics for captive loans relative to non-captive loans.

Panel A in Table 4 reports the coefficient estimates from Equation 4. Consistent with our results capturing tariff pass-through along the intensive margin, we find no significant deterioration in captive borrower characteristics 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 changes in 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 removing subsidized loans from the sample. As in Panel A, we find that the pool of captive borrowers experienced a slight increase in household incomes and no significant decline in credit scores or default rates. We note that, from a risk-based pricing perspective, the observed increase in average household incomes is not only too small to explain the observed increase in captive interest rates in Table 3, but that it is also of the wrong sign.

Figure 5 plots the evolution of captive borrower characteristics around the treatment date. Similar to before, we find no significant declines in average household incomes or credit scores following the tariffs, and we also find no significant increases in the future default rates. In support of the parallel trends assumption, we find no meaningful evidence of differential pre-trends across the pools of captive and non-captive borrowers. Combined, our results suggest that the composition of captive borrowers did not change in response to the tariffs.

We note that other studies have also found no changes in the composition of auto loan borrowers in response to moderate changes in loan terms. Examining discontinuities in offered interest rates at discrete credit score thresholds, Argyle, Nadauld, and Palmer 2020 and Argyle, Nadauld, and Palmer 2023 find no difference between the observable characteristics of loan applicants on the more versus less expensive sides of a threshold. Moreover, for the set of borrowers that choose to originate a loan, these same studies find that observable borrower characteristics and future default rates do not change upon crossing a threshold. Argyle et al. 2021 find that the composition of auto loan borrowers does not react to changes in maximum offered loan maturities. Karlan and Zinman 2008 find similar null results in the unsecured credit market.

To the best of our knowledge, our paper is the first to document that auto manufacturers used their captive lenders to pass on higher input costs from the tariffs to consumers. In contrast, a handful of other studies have found that captives adjust their lending decisions along the extensive margin in response to market-wide and firm-specific funding shocks. For instance, Benmelech, Meisenzahl, and Ramcharan 2017 find that the collapse of the asset-backed commercial paper market during the Financial Crisis led to a curtailment in captive auto lending. Benneton, Mayordomo, and Paravisini 2022 find that increases in manufacturer credit default swap spreads are associated with higher captive auto loan interest rates and more relaxed lending standards.

4.4 New vehicle prices

A natural question is whether tariff pass-through was limited to vehicle financing costs or whether it also affected new vehicle prices. To answer this question, we start with a simple time series approach and calculate the average change in new vehicle prices for the same make-model between 2017 and 2018. The regression model is:

$$p_{i,t} = \alpha + \Gamma \cdot \text{Post}_t + \delta_{\tilde{v}} + \varepsilon_{i,t}, \tag{5}$$

where $p_{i,t}$ is the log price of new vehicle i purchased in quarter t, and $\delta_{\tilde{v}}$ are vehicle make-model fixed effects. Standard errors are clustered at the make-model level.

We examine two measures of new vehicle prices. The Regulation AB II data includes the invoice price for new vehicles, enabling us to observe how auto manufacturers adjusted their sales price to dealerships during the sample period. However, we also are interested in whether actual consumer prices changed. Since the Regulation AB II data does not include the final purchase price, we instead use our new vehicle sales price data from the Texas Department of Motor Vehicles. As stated earlier, this data is collected for car registration and tax purposes and includes the purchase price of all new vehicles sold in the state, regardless of the source of financing.

Table 5 reports the coefficient estimates from Equation 5. As shown in Column 1, we find that average new vehicle invoice prices rose 1.7 percent between 2017 and 2018, which is less than the 2.4 percent rate of inflation in 2018. Using our sales price data from the State of Texas in Column 3, we find that average new vehicle sales prices rose 1.4 percent during this same period. Thus, controlling for make-model, it appears that new vehicle invoice and sales prices moved similarly around the tariff announcement, consistent with the timing in Nakamura and Zerom 2010.

The results in Columns 1 and 3 suggest that new vehicle prices rose less than the general rate of inflation following the tariffs. To move closer to a causal estimate of the price effect of the tariffs, we next estimate a difference-in-differences regression. The main challenge with running a difference-in-differences is that the appropriate control group for new vehicles is unclear. By contrast, in our financing regressions, loans from non-captive lenders for the exact same vehicle served as natural controls. However, perhaps the most natural control group for new vehicles is "newer" used vehicles in good condition, such as those that are one or two years old with prices above $\$15,000$. Using these vehicles as our control group, the regression model is:

$$p_{i,t} = \alpha + \Gamma \cdot \text{New Vehicle}_i \cdot \text{Post}_t + \beta \cdot \text{New Vehicle}_i + \delta_{\tilde{v}} + \varepsilon_{i,t}, \tag{6}$$

where New Vehicle$_i$ is equal to one if vehicle i is new and zero if it is a "newer" used vehicle in good condition. Standard errors are again clustered at the make-model level.

Table 5 reports the coefficient estimates from the model. Relative to "newer" used vehicles in good condition, the average invoice price of new vehicles rose by 0.6 percent following the tariffs (Column 2), and the average sales price rose by 0.4 percent (Column 4). Given that the average new vehicle sales price is $\$31,049$ in the State of Texas data, this implies that the average new vehicle sales price rose by around \$124 following the tariffs. However, we note that the coefficient estimates from our difference-in-differences model are insignificant at the 10 percent level.[27]

27. We also re-estimate Equation 2 with the invoice price as the outcome variable to examine whether captive-financed purchases of new vehicles experienced a larger increase in their invoice prices than non-captive-financed purchases. (Since the Texas data does not have financing information, we cannot run this difference-in-differences regression with the sales price as the outcome variable.) Consistent with Benneton, Mayordomo, and Paravisini 2022, we find no differential change in new vehicle invoice prices across captive and non-captive financings ($\Gamma = 0.001$; $t = 1.35$). As noted in Argyle et al. 2021 and Argyle, Nadauld, and Palmer 2023, this also suggests that captive borrowers are not substituting across trims within a given make-model in response to higher rates.

4.5 The cost to American consumers

As discussed in Flaaen, Hortacsu, and Tintelnot 2020, an important metric for evaluating the economic impact of tariffs is the rate of pass-through to consumers. In our setting, this pass-through is made up of two components: (i) pass-through on the loan price margin and (ii) pass-through on the vehicle price margin. Separately estimating the degree of pass-through along each of these margins is useful for two reasons. First, by comparing pass-through along the loan price margin to the vehicle price margin, we can better understand the extent to which focusing on sticker prices alone might understate the economic impact of tariffs. Second, we can combine these two separate estimates into an overall rate of pass-through, which we can then compare to existing estimates from the cost pass-through literature.

We start by calculating the rate of pass-through along the loan price margin. To do this, we need to gather two inputs. First, we need to know how much auto manufacuturers' costs increased in response to the tariffs. This is equal to the number of new vehicles produced N times the average increase in costs per vehicle ΔC. Second, we need to know how much financing costs increased for captive borrowers. This is equal to the number of loans originated F (for both new and used vehicles) times the average present value increase in financing costs per borrower ΔP. Dividing these two quantities, the pass-through rate $\rho(l)$ is equal to:

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

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

Given Equation 7, calculating the pass-through rate just involves plugging in values for ΔP, ΔC, and M. From Section 4.1, we have that the (ex-spillover) present value increase in financing costs per captive borrower is $\Delta P = \$179$. From population data, we have that the captive loan penetration rate is $M = 0.59$. (See Appendix C for all calculations.) Finally, from Ford's 2018 annual report, we have that the average increase in costs per vehicle is $\Delta C = \$295$.[28] Thus, the

28. Specifically, 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

implied pass-through rate is $\rho(l) = 0.36$ ($= 0.59 \cdot \$179/\$295$), or auto manufacturers passed on 36 percent of tariff-related costs to consumers along the financing margin.

Table C.1 presents alternative estimates for the pass-through rate. One important alternative to consider uses our spillover-inclusive estimate of $\Delta P = \$227$ for the average present value increase in financing costs per captive borrowers, holding ΔC and M constant. In this scenario, the pass-through rate rises from 0.36 to 0.45. In general, we believe a reasonable range for the rate of pass-through along the loan price margin is 0.22 to 0.66.

We next calculate the rate of pass-through along the vehicle price margin. Doing so just involves taking the ratio of the average increase in new vehicle prices ΔV to the average increase in costs per vehicle ΔC. From Section 4.4, we have that the average increase in new vehicle prices is $\Delta V = \$124$. From the above, we have that the average increase in costs per vehicle is $\Delta C = \$295$. Dividing these two values, we have that the implied rate of pass-through along the vehicle price margin is $\rho(v)$ =0.42 ($= \$124/\$295$). Hence, the overall rate of pass-through to consumers is $\rho = 0.78$ ($= 0.36 + 0.42$), and focusing on sticker prices alone leads us to understate the economic impact of the tariffs by at least 54 percent ($= 0.42/0.78$).

From a policy perspective, it is also interesting to consider the aggregate dollar cost of the metal tariffs to American consumers.[29] To do so, we rearrange Equation 7 as follows:

$$F \cdot \Delta P = \rho(l) \cdot N \cdot \Delta C, \tag{8}$$

where the left-hand side of the equation is the total present value increase in financing costs for captive borrowers on an annual basis. From population data, there are around $N = 17$ million new vehicles sold in the United States each year. Combining this value with our prior estimates, we

increase of \$295 per vehicle. As discussed in Appendix C, several alternative methods of estimating this average cost increase – including a vehicle weight-based method and estimates from various other annual reports and automotive media outlets – produce similar values. 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 - not the steel and aluminum tariffs that we examine.

29. Similar to our pass-through calculations, we continue to focus on the direct cost to consumers coming from higher captive loan prices. A more complete accounting of the aggregate dollar cost might also call for including the estimated 7 basis point increase in interest rates for non-captive loans from Section 4.1.1.

thus have that the tariffs resulted in around \$1.8 billion ($= 0.36 \cdot 17,000,000 \cdot \$295$) in additional present value financing costs to American consumers each year. For reference, Flaaen, Hortacsu, and Tintelnot 2020 estimate that import tariffs on washers lead to \$1.5 billion in additional costs to consumers each year. Finally, we note that all the above estimates are partial equilibrium in the sense that they do not consider demand-side responses.

Within the literature on cost pass-through, the paper closest to our is Nakamura and Zerom 2010, which estimates a pass-through rate of coffee bean prices to wholesale coffee ground prices of 0.38.[30] The reason this paper is closest to ours is because it also examines how a cost increase for an intermediate good (coffee beans in their case, steel and aluminum in our case) is passed on to the price of an end good (coffee grounds in their case, automobiles in our case). In contrast, most other papers in the pass-through literature focus on cost shocks that affect end goods. For instance, Flaaen, Hortacsu, and Tintelnot 2020 examine how tariffs on washer imports are passed on to consumers and find a pass-through rate between 1.08 and 2.25. For a sample of 23 OECD countries between 1975 and 2003, Campa and Goldberg 2005 examine how exchange rate shocks are passed on to border prices and find an average long-run pass-through rate of 0.64.

4.6 The cost to captive lenders

Given that the composition of borrowers does not change, what tradeoffs do captive lenders face when deciding whether to raise their loan prices? To answer this question, we examine whether the tariffs impacted captive loan origination volumes. The regression model is:

$$y_{f,s,v,t} = \alpha + \Gamma \cdot \text{Treated}_f \cdot \text{Post}_t + \delta_f + \delta_{s,t} + \delta_{v,t} + \varepsilon_{f,s,v,t}, \tag{9}$$

where the outcome variable is the logged number of loans that were originated from either captive finance companies ($f = 1$) or non-captive finance companies ($f = 0$) in quarter t in state s for

30. Specifically, Nakamura and Zerom 2010 estimate that the long-run elasticity of wholesale coffee ground prices to coffee bean prices is 0.25, and that two-thirds of this incomplete pass-through is due to "local costs" other than coffee beans that factor into the production of coffee grounds. Thus, to convert this elasticity into a pass-through rate, we divide 0.25 by two-thirds, which is approximately 0.38.

vehicle make-model-condition v.[31] Our prediction is that higher loan prices will lead to lower loan originations, or that the Γ coefficient will be less than zero. If such a decline in loan demand exists, then it would also help explain why captives do not raise their loan prices prior to the tariffs.

Table 6 reports the coefficient estimates. Consistent with our prediction above, we find that the volume of captive loan originations declined 6.7 percent in response to the tariffs. Given that captive interest rates rose 10 percent during this period (= 26 basis points / 252 basis points), the implied interest rate elasticity of extensive margin loan demand is thus -0.67 (-6.7 / 10.0). This 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. (In addition, Karlan and Zinman 2008 estimate an interest rate elasticity of extensive margin loan demand of -0.28 in the unsecured credit market.)

Before we proceed, we highlight a few important points. First, while our original level of aggregation in Equation 9 follows Benneton, Mayordomo, and Paravisini 2022, our results are also robust to different levels of aggregation. For instance, in Column 3 in Table 6, we aggregate the number of loan originations at the captive $\times$ state $\times$ income bin $\times$ credit score bin $\times$ quarter level and find a 4.8 percent decline in captive loan originations in response to the tariffs.[32] 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 conditional on a sale, the findings in Gavazza and Lanteri 2021 and Argyle, Nadauld, and Palmer 2023 suggest that both margins should be active. Third, we note that the decline in loan originations that we document in Table 6 does not contradict the absence of borrower composition effects that we document in Table 4. 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

31. To better account for the count-data structure of the number of loan originations, we also re-estimate Equation 9 using a Poisson model and report the results in Column 2 in Table 6 (Cohn, Liu, and Wardlaw 2022). We use hetroskedasticity-robust standard errors for both our linear and Poisson models. 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 $\times$ state $\times$ vehicle level ($t = -15.17$) and using a bootstrap procedure ($t = -10.45$).

32. Results for additional aggregations at the lender instead of the captive level are shown in Table A.3.

is not correlated with observable borrower characteristics or future default rates.

Another potential tradeoff that captive lenders might face is that borrowers might change their vehicle choices in response to changes in their loan terms. Argyle, Nadauld, and Palmer 2023 find that consumers substitute towards older vintages of a particular vehicle make-model when their offered interest rates rise. Further, Argyle et al. 2021 find that a 100 basis point increase in offered interest rates causes the average borrower to spend 1.95 percent less on their vehicle, with 60 percent of this effect coming from substitution across vehicle make-models and 40 percent coming from lower negotiated vehicle prices. If captive borrowers substituted toward less profitable vehicles in response to higher interest rates, then the benefits that auto manufacturers received from raising their interest rates would have been further offset to some extent.

To examine whether captive borrowers adjusted their vehicle choices in response to higher interest rates, we re-estimate our baseline difference-in-differences model with less granular versions of our vehicle fixed effects. If substitution is present in our setting, then we should expect that the average assessed vehicle value of captive borrowers will decline once we condition on fewer aspects of their vehicle choices. (Recall that the assessed vehicle value is generally the invoice price for new vehicles and the Kelly Blue Book value for used vehicles.) However, as shown in Table A.4, we find no differential change in vehicle values for captive borrowers in response to 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.[33]

4.7 Alternative explanations

The tariffs had the potential to impact the auto lending market along multiple dimensions, including by changing the borrowing costs of captive lenders or through consumer demand. Below, we examine several alternative explanations for our results but find that none are supported in the data.

33. As before, one reason that our results might differ from those in Argyle et al. 2021 and Argyle, Nadauld, and Palmer 2023 could be because these papers examine the direct auto loan market where borrowers often know their rates prior to selecting their vehicle and negotiating the price, whereas we primarily examine the indirect auto loan market where the sequence of events is often the opposite. See Grunewald et al. 2023.

4.7.1 Borrowing costs

Captive lenders finance their operations using a combination of internal cash, asset-backed securities, and unsecured debt. If captive lenders experienced an increase in their cost of unsecured debt in response to the tariffs (e.g., because captive lenders were viewed as riskier credits), then it might have resulted in a mechanical increase in captive loan prices. To test this explanation, we re-estimate Equation 2 after controlling for lender-specific measures of unsecured borrowing costs and their interactions with the treatment indicator. As shown in Table A.5, controlling for unsecured borrowing costs in a flexible manner does not overturn our results.

4.7.2 Dealer mark-ups

Almost all captive auto loans are dealer-intermediated – i.e., indirect – auto loans. During the indirect auto loan process, dealers often have the discretion to charge consumers higher interest rates than what the lender has offered (Cohen 2012). This practice is known as dealer mark-up, and it is a major profit center at most auto dealerships (Brown and Jansen 2020).[34] One potential concern could be that the increase in captive interest rates in Table 3 is coming from an increase in dealer mark-ups and not offered interest rates. If this were the case, then we could not interpret our results as evidence of tariff pass-through from captive lenders.

However, for two reasons, we do not believe that changes in dealer mark-ups drive our results. First, the non-captive lenders in our sample are also subject to dealer mark-ups.[35] Hence, common changes in dealer mark-ups across captive and non-captive lenders should be netted out in our difference-in-differences specification. Second, in Table A.6, we find a significant increase in interest

34. The additional revenue from the mark-up is split between the dealer and the lender according to a prespecified formula. Grunewald et al. 2023 find that the average dealer receives around 75 percent of the present value of the mark-up via a one-time, upfront fee called the dealer reserve. Given an average mark-up of 108 basis points, the average dealer reserve turns out to be around $600, which is much larger than the average loan origination fee of $75. Because of several class-action lawsuits, most lenders cap mark-ups at around 200-250 basis points.

35. 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". Grunewald et al. 2023 find that 78 percent of indirect auto loans from non-captive lenders are marked up.

rates for subvented captive auto loans, which are loans that dealers are not allowed to mark up (Grunewald et al. 2023).[36] This helps rule out the related concern that auto dealers increased their mark-ups more for captive loans than non-captive loans in response to the tariffs.

4.7.3 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.[37] To test this alternative explanation, Figure A.1 plots vehicle sales for our sample of captive-affiliated auto manufacturers around the treatment date. Inconsistent with a short-term surge in loan demand driving our results, we find no noticeable increase in vehicle sales (and hence loans demanded) following the announcement of the tariffs.[38] Two other pieces of evidence are also inconsistent with this alternative explanation. First, Figure 4 shows there is no reversal in the increase in captive interest rates during the post-treatment period. Second, Table 6 shows that captive loan origination volumes decreased, not increased, relative to non-captive loan origination volumes following the announcement of the tariffs.

4.7.4 Unobservable selection on consumer price inelasticity

In response to higher nominal vehicle prices, some price sensitive consumers might have forgone vehicle purchases. As a result, the average consumer that purchased a vehicle – and hence the average borrower – might have become less price sensitive following the tariffs. If consumers that are more inelastic to vehicle prices are also less sensitive to loan prices, then selection on vehicle

36. 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).

37. Capacity constraints could arise because of both financial reasons (e.g., no immediate source of funding to originate more loans) and operational reasons (e.g., not enough loan underwriters to originate more loans). A similar phenomenon has been documented in the mortgage market. For instance, Fuster et al. 2013 find that capacity constraints help explain why mortgage originators make larger profits during refinancing waves.

38. 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 reduction in household incomes, should be common across captive and non-captive financed loans and hence should be absorbed into our various time fixed effects.

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 not drive our results. First, because both captive and non-captive borrowers are subject to higher vehicle prices, common forms of selection based on vehicle prices should be netted out in our difference-in-differences specification. Second, nominal vehicle price growth did not outpace inflation during the sample period (Table 5). Third, we find no differential changes in observable characteristics or default rates for captive borrowers (Table 4). Although we cannot entirely rule out that captive borrowers are becoming differentially less price sensitive along unobservable dimensions following the tariffs, we note that there are no differential changes in borrower-level characteristics that Grunewald et al. 2023 find to be correlated with loan price sensitivities, such as household incomes and credit scores. Moreover, although Attanasio, Goldberg, and Kyriazidou 2008 find that loan maturities correlate with higher inelasticities in car financing, Table 3 shows no significant increase in loan maturities in response to the tariffs. Finally, our falsification test in Section 4.7.7 provides further evidence that captive borrowers are not becoming differentially less price sensitive along unobservable dimensions, as our effects are concentrated in the subset of captive lenders that are most exposed to the tariffs and not captive lenders in general.

4.7.5 Do borrowers undo the effects of tariff pass-through by prepaying?

In response to higher interest rates, captive borrowers might have prepaid their loans at faster rates. If this occurred, then our estimate of tariff pass-through from Section 4.5 would be overstated. To examine whether captive borrowers undid the effects of higher interest rates by prepaying more, we re-estimate our baseline model with indicators for whether a loan is paid off within 12 or 24 months of its origination date. As shown in Table A.7, we find no differential increase in the likelihood that captive loans are paid off within 12 or 24 months of their origination dates.

4.7.6 Changes in securitization practices

As costs on the manufacturing side of their business rose, captive lenders might have adjusted their securitization practices to raise cash for their parents or to help smooth earnings. For example, captive lenders might have securitized a larger fraction of high-rate loans (which command higher prices) to raise cash despite not adjusting their offered interest rates. If this were true, then we would not be able to attribute the increase in captive interest rates to tariff pass-through.

To test whether differential changes in securitization practices drive our results, we combine the Regulation AB II data with population credit bureau data as in Section 3.1.1. We then re-estimate our difference-in-differences model at the lender-origination quarter level, where the outcome variable is either: (i) the share of loans that were originated in a given quarter that the lender later securitized, (ii) the ratio of the average securitized loan amount to the average overall loan amount, and (iii) the same ratio but for average loan maturities and (iv) average monthly payments. Table A.8 reports the results. Inconsistent with securitization-related factors driving our results, we find no differential changes in the above variables following the tariffs.

4.7.7 Falsification test

One potential concern is that our results 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. To help alleviate this concern, we perform a falsification test that leverages the fact that some of our captive lenders have large domestic manufacturing operations – and hence significant exposure to the metal tariffs – while others do not.[39] That is, 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 more exposure to the tariffs.

To conduct the above falsification test, we first split our sample of captive lenders into two exposure groups. Captives whose manufacturers have two-or-more domestic production plants are considered to be more exposed to the tariffs, whereas captives whose manufacturers have one or

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

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. 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 exposure based on the fraction of vehicles made in North America from the American Automobile Labeling Act and then split at the median.

Given the above classifications, we then re-estimate Equation 2 across our two groups of captive lenders, where the control group consists of all non-captive loans. Table A.9 reports the estimates. Consistent with our results capturing the causal effect of the tariffs, we find that the increase in captive interest rates is concentrated among more exposed captive lenders (Γ = 29 basis points; t = 3.26). Loans from less exposed lenders do not experience an increase in their average interest rates in response to the tariffs (Γ = -8 basis points; t = -0.48), and neither group of captive lenders exhibits significant changes in borrower characteristics or evidence of differential pre-trends (Figure A.2). In sum, the concentration of our results among more exposed captive lenders serves as evidence against an alternative explanation based on captive-specific correlated omitted variables.

4.8 Robustness

We conduct several tests to ensure that our results are robust to our choice of fixed effects, our assumptions about the standard errors, and our sample filters. For a more thorough discussion of these robustness tests, please see Section A.1. in Appendix A.

5 Economic channels

5.1 The demand channel

Theories of cost pass-through predict that firms will find it easier to pass on costs along margins where consumers are less price sensitive (Chen and Juvenal 2016). Given that Grunewald et al. 2023 find that consumers are less sensitive to increases in loan prices than vehicle prices, auto manufacturers might have chosen to pass on some portion of the tariffs through their financing terms to limit the overall impact on demand.[40] To further explore the role of borrower demand in determining tariff pass-through, we now test whether the increase in captive loan prices is larger for borrowers whom prior studies have found to be less sensitive to loan prices.

We explore the role of borrower demand using three proxies. First, we build on Attanasio, Goldberg, and Kyriazidou 2008 and Grunewald et al. 2023, which find that low-income borrowers are less sensitive to increases in loan prices than high-income borrowers. We split our sample of loans into two groups based on the median household income in our sample. We then estimate the following triple-differences model:

$$\begin{aligned} y_{i,t} = \alpha &+ \beta \cdot \text{Low income}_i \cdot \text{Treated}_l \cdot \text{Post}_t + \Gamma \cdot \text{Treated}_l \cdot \text{Post}_t \\ &+ \theta \cdot \text{Low income}_i \cdot \text{Treated}_l + \delta_l + \delta_{v,t} + \delta_{s,t} + \delta_{w,t} + \delta_{c,t} + \delta_{\text{Low income},t} + \varepsilon_{i,t}, \end{aligned} \tag{10}$$

where the outcome variable is the interest rate of loan i originated in quarter t, and Low income$_i$ is equal to one when loan i has a below-median household income and zero otherwise.[41] The coefficient of interest, β, measures the differential effect of the tariffs on loans with below-median incomes relative to loans with above-median incomes. If the borrower demand channel contributes to our results, then we should expect that the effect of the tariffs will be more pronounced among loans with below-median household incomes – i.e., β should be greater than 0.

Table 7 reports the coefficient estimates from the model. Consistent with the predictions of the demand channel, we find that pass-through to interest rates is higher when borrowers have lower incomes. Captive loans with above-median incomes experienced an average increase in interest rates of 20 basis points ($t = 2.41$) in response to the tariffs, whereas captive loans with below-median

40. As mentioned in Grunewald et al. 2023, consumers might be less sensitive to increases in loan prices than vehicle prices for several reasons. One reason could be credit constraints. For instance, credit-constrained consumers with limited resources might prefer contracts with higher back-end finance charges over contracts with higher upfront costs (Argyle, Nadauld, and Palmer 2020). Another reason could be behavioral factors. For instance, borrowers might underestimate the total costs associated with higher loan prices (Stango and Zinman 2009), or loan prices might act as less salient, shrouded attributes (Ellison 2005; Gabaix and Laibson 2006; Chetty, Looney, and Kroft 2009). We note that we cannot distinguish whether credit constraints or behavioral factors drive our cross-sectional results because these two explanations have identical predictions for the cross-sections that we examine.

41. The $\delta_{\text{Low income},t}$ fixed effects are somewhat redundant given our model includes $\delta_{w,t}$. Although we include them for completeness because of imperfect overlap, our results are robust to removing these fixed effects.

incomes experienced an average increase in interest rates of 33 basis points. The 13 basis point difference (= 33 - 20) between these two groups is significant at the 5 percent level.

In addition to income-based variation, Grunewald et al. 2023 find that consumers with lower credit scores are less sensitive to increases in loan prices. (See also Argyle, Nadauld, and Palmer 2020.) Therefore, we repeat the above test using credit scores as an alternative measure of loan price sensitivities. Table 7 reports the coefficient estimates from Equation 10 after replacing Low income$_i$ with the variable Low credit score$_i$ that is equal to one when loan i has a below-median credit score and zero otherwise. Consistent with the predictions of the demand channel, we find that pass-through is higher when borrowers have lower credit scores. Captive loans with above-median credit scores experienced an average increase in interest rates of 15 basis points ($t = 2.34$) in response to the tariffs, whereas captive loans with below-median credit scores experienced an average increase of 36 basis points. This 21 basis point difference is significant at the 10 percent level.

For our third test of the demand channel, we examine how tariff pass-through varies across loan amounts. Smaller loan amounts might be indicative of tighter credit constraints and lower loan price sensitivities (Adams, Einav, and Levin 2009). Hence, the demand channel would predict that borrowers with smaller loan amounts would bear a larger share of the tariffs. Table 7 reports the coefficient estimates from Equation 10 after we replace Low income$_i$ with the variable Low loan amount$_i$ that is equal to one when loan i has a below-median loan amount and zero otherwise. Again, consistent with the predictions of the demand channel, we find that pass-through to interest rates is higher when loan amounts are smaller.

In the right-most columns of Table 7, we examine whether differential changes in the composition of borrowers are driving our cross-sectional results. To do so, we re-estimate our triple-differences models with the default rate as the outcome variable. As shown in Columns 4 through 6, we find no significant changes in default rates in any of the subsamples. This indicates that composition effects do not explain the variation across our loan demand proxies.

To better understand how the degree of pass-through varies across borrower demand, Figure 6 plots the coefficient estimates from our model within each income, credit score, and loan amount quartile. As shown in Panel A, we find that the degree of pass-through is monotonically decreasing across the income quartiles. While captive loans in the lowest income quartile experienced an average increase in interest rates of 37 basis points ($t = 3.30$) in response to the tariffs, captive loans in the highest income quartile experienced an average increase in interest rates of just 17 basis points ($t = 2.37$). Panel B plots the coefficient estimates for each credit score quartile. Although the pattern is non-monotonic, we continue to find that pass-through to interest rates is much higher when borrowers have lower credit scores. Further, we note that the non-monotonic pattern stems in part 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. Finally, Panel D shows that the degree of pass-through is monotonically decreasing in loan size.

Combined, our results suggest that captive borrowers with lower incomes, lower credit scores, and smaller loan amounts shouldered a disproportionate share of the tariffs. This finding has immediate policy ramifications as the 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 observed rate of tariff pass-through.

5.2 The competition channel

Theories also predict that the rate of cost pass-through 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 shape of the demand curve.[42] Within our setting, one of the most im-

42. For oligopolistic markets with market-wide cost shocks, Weyl and Fabinger 2013 develop a general equation for pass-through that can be used to demonstrate how this theoretical relation is ambiguous. For instance, in a setting where marginal costs and the conduct parameter are constant and demand becomes more sensitive to prices as prices rise, the Weyl and Fabinger 2013 model predicts that pass-through should be decreasing with the level of competition. However, if demand is linear instead of log-concave, then this same model predicts that pass-through

portant 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 predicts that the pass-through rate will be increasing as the level of competition declines.[43]

One challenge that arises when estimating the relation between pass-through and competition is that most auto lenders face similar competitive environments. In general, competition in the auto loan market tends to be national in scope. More than 80 percent of auto loans are originated through automobile dealerships, and these dealerships have access to thousands of lenders across the United States through online platforms such as RouteOne and CUDL. However, the alternative to dealer financing is to borrow directly from a lender, and this market is 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 or regional lenders serving each state could create meaningful geographic variation in auto lending 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 competition. Like Yannelis and Zhang 2021, 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.[44] We then split our sample into two groups: loans in states with below-median lending market competition (i.e., above-median HHIs) and loans in states with above-median lending market competition (i.e., below-median HHIs). Finally, we re-estimate Equation 10 after replacing Low income$_i$ with the variable Low competition$_s$ that is equal to one when state s has below-median competition and zero otherwise.

Table 8 reports the coefficient estimates from the model. Consistent with our predictions, we find

should be increasing as competition rises. We note that empirical studies on the relation between pass-through and competition are mixed as well. For instance, while Genakos and Pagliero 2022 find that pass-through increases as competition rises in the gasoline market, Doyle and Samphantharak 2008 and Stolper 2018 find the opposite.

43. For instance, a common prediction from models of perfect competition is that the pass-through rate of a firm-specific cost shock will be zero. However, if we move from perfect competition to imperfect competition, the pass-through rate of that same cost shock will be positive. Holding the number of captive lenders fixed, our model of the auto loan market in Apppendix B predicts that pass-through will increase as the number of competitors declines.

44. We construct our HHIs using credit bureau data on the population of auto loans. That is, we use data from the entire universe of auto lenders and not just the auto lenders in the Regulation AB II data. Our 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 2021 and suggest that there is some local component of competition in addition to the national component.

that pass-through is higher in states with lower lending market competition. While captive auto loans in states with above-median competition experienced an average increase in interest rates of 25 basis points ($t = 2.71$), captive auto loans loans in states with below-median competition experienced an average increase in interest rates of 29 basis points. Albeit small, the 4 basis point difference between these two groups is significant at the 5 percent level.

It is possible that the above results are attenuated because there is not much variation in our competition measure near the median (Roberts and Whited 2013). Therefore, to better understand the role of competition in our setting, we focus our attention on the tails of the competition distribution. We first restrict our sample to loans that are in either the lowest or highest quartile of the competition distribution and then re-estimate Equation 10 after setting Low competition$_s$ equal to one when state s is in the lowest quartile. Afterwards, we further restrict our sample to loans that are in either the top or bottom decile of the competition distribution, and we then re-estimate Equation 10 after setting Low competition$_s$ equal to one when state s is in the lowest decile. Columns 2 and 3 in Table 8 report results. Consistent with our prior results being attenuated, we find that competition has a larger impact on pass-through as we move further out into the tails of the distribution. For example, while captive auto loans in the highest decile of competition experienced an average increase in interest rates of 24 basis points ($t = 2.15$), captive auto loans in the lowest decile experienced an average increase in interest rates of almost 41 basis points. This 17 basis point difference ($t = 2.29$) is more than four times as large as our above- versus below-median estimate.

6 Conclusion

We examine the pass-through of cost shocks to consumer credit using the unique laboratory of auto lending around the 2018 metal tariffs. Conditioning on auto loans originated in the same quarter, in the same state, for the same vehicle make-model, and to borrowers with similar incomes and credit scores, we compare loans from auto manufacturers' integrated captive lenders to loans from non-captive lenders and find the tariffs resulted in worse loan terms for captive loan borrowers. Our main result is that auto manufacturers passed on a non-trivial portion of tariff-related costs to consumers via higher auto loan prices. Moreover, consistent with standard theories of cost pass-through, the increase in captive interest rates was concentrated among low-income and low-credit score borrowers who have less elastic loan demand and in areas with lower levels of lending competition. Our results highlight that captive finance companies provide a channel for trade policies to affect the provision of consumer credit. Further, our results suggest that ignoring the impact of tariffs on financing costs understates the cost of trade policy to American consumers.

Our finding that the tariffs spilled over to captive auto loan terms has broad implications. As noted in Murfin and Pratt 2019, captive finance is common outside of the auto sector. Further, the annual reports of several manufacturing firms with captive lenders (including Boeing, Caterpillar, and Polaris) mention higher input costs from the tariffs being offset with higher captive financing revenues. Outside of tariffs, our findings also have important implications for the general measurement of cost pass-through. Indeed, many types of firms other than auto manufacturers sell bundled and complementary goods (Flaaen, Hortacsu, and Tintelnot 2020). For such firms, focusing just on directly affected goods' prices might understate the importance of cost pass-through to prices.

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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 1,973,067 auto loans. The sample is restricted to auto loans originated between January 2017 and December 2018. Descriptive statistics are as of the loan origination date. In Columns 8 through 10, we compare auto loans from captive lenders to auto loans from non-captive lenders 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 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 between January 2017 and 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.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

Table 3: Difference-in-differences regression: Auto loan terms

Panel A: All loans

	Interest rate (1)	Loan amount (2)	Loan maturity (3)	Loan-to-value (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 (1)	Loan amount (2)	Loan maturity (3)	Loan-to-value (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.

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

Table 4: Difference-in-differences regression: Composition of borrowers

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, log credit score, 12-month default rate, or 24-month default rate. A loan is considered to be in default if it is 90 or more days past due (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.

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

Table 5: Time series and difference-in-differences regressions: Vehicle prices

	log Invoice Price		log Sales Price	
	(1)	(2)	(3)	(4)
Post	0.017***		0.014**	
	(4.88)		(2.03)	
New Vehicle × Post		0.006		0.004
		(1.30)		(0.43)
Data source	Reg AB	Reg AB	Texas	Texas
Vehicle FE	Y	Y	Y	Y
Condition FE		Y		Y
Quarter FE		Y		Y
N	1,290,086	1,502,402	1,389,464	1,922,857
R^2	0.88	0.87	0.83	0.81

NOTE.—This table reports coefficient estimates from Equation 5 in Columns 1 and 3 and Equation 6 in Columns 2 and 4. In Columns 1 and 3, the sample is restricted to new vehicles purchased between January 2017 and December 2018. In Columns 2 and 4, the sample includes both new and "newer" used vehicles in good condition, which are used vehicles that are one or two years old with prices above $15,000, where "newer" used vehicles serve as the control group. The dependent variable in Columns 1 and 2 is the natural log of the assessed vehicle value. The dependent variable in Columns 3 and 4 is the natural log of the vehicle sales price. In Columns 1 and 2, the models are estimated using the Regulation AB II data. In Columns 3 and 4, the models are estimated using the Texas Department of Motor Vehicles data. New vehicles in the Texas data are also restricted to the same makes as in the Regulation AB II data. (This does not affect our results.) Vehicle fixed effects refer to vehicle make-model combinations, and condition refers to whether the vehicle is new or used. t-statistics, presented below the coefficient estimates, are calculated by clustering at the vehicle make-model level.

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

Table 6: Difference-in-differences regression: Loan originations

	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	Vehicle	Vehicle	Borrower Type	Borrower Type
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	321,016	183,824	183,824
R^2	0.49	0.70	0.76	0.76

NOTE.—This table reports coefficient estimates from Equation 9. 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. In Columns 1 and 2, we calculate the number of loan originations at the captive (f) × state (s) × vehicle make-model-condition (v) × quarter (t) level. (In the table, this is noted as "Level of Aggregation = Vehicle".) In Columns 3 and 4, we calculate the number of loan originations at the captive × state × income bucket (ω) × credit score (c) × quarter level. (In the table, this is noted as "Level of Aggregation = Borrower Type".) We estimate a regular linear regression model in Columns 1 and 3 and a Poisson model in Columns 2 and 4. The variable Treated is equal to one for loan originations from captive finance companies and zero otherwise. 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.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

Table 7: Triple-difference 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 10. The dependent variable is either the interest rate or the 12-month default rate. A loan is considered to be in default if it is 90 or more days past due (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 the above- versus below-median income cuts (Columns 1 and 4), the above- versus below-median credit score cuts (Columns 2 and 5), or the 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.

* Significant at the 10% level.

** Significant at the 5% level.

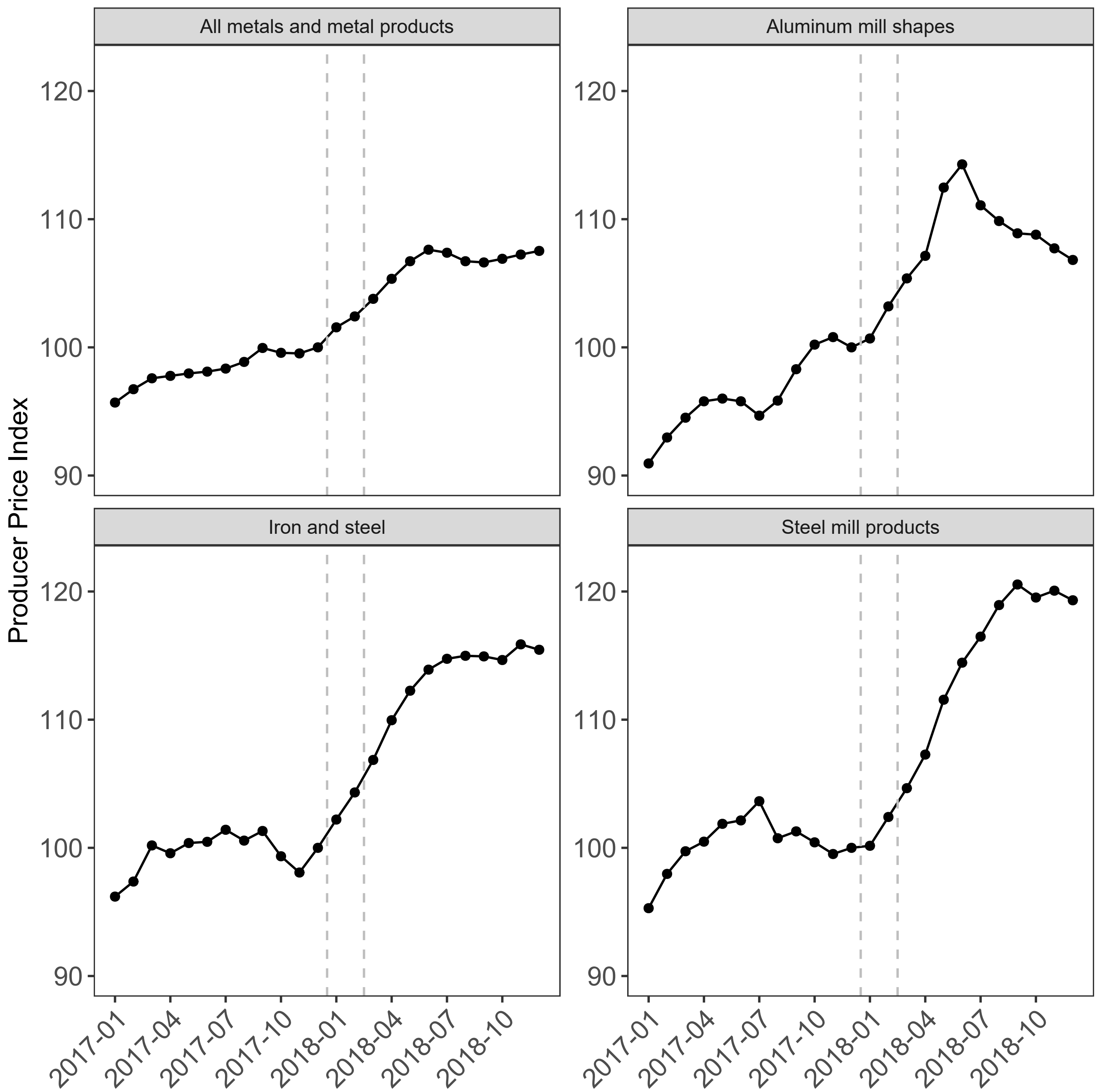
*** Significant at the 1% level.

Table 8: Triple-difference regression: Competition

	Interest rate			12-month default		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	0.248***	0.213**	0.241**	0.000	-0.001	0.001
	(2.71)	(1.99)	(2.15)	(-0.65)	(-1.16)	(0.92)
Treated × Post	0.037**			0.001		
× Low competition (median)	(2.29)			(0.92)		
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 10. The dependent variable is either the interest rate or the 12-month default rate. A loan is considered to be in default if it is 90 or more days past due (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.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

Figure 1: Metals prices



NOTE.——This figure plots scaled metals prices sourced from the 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 and March 2018.

Figure 2: Financial statement data from GM

Year ending December 2017 (pre-tariff):

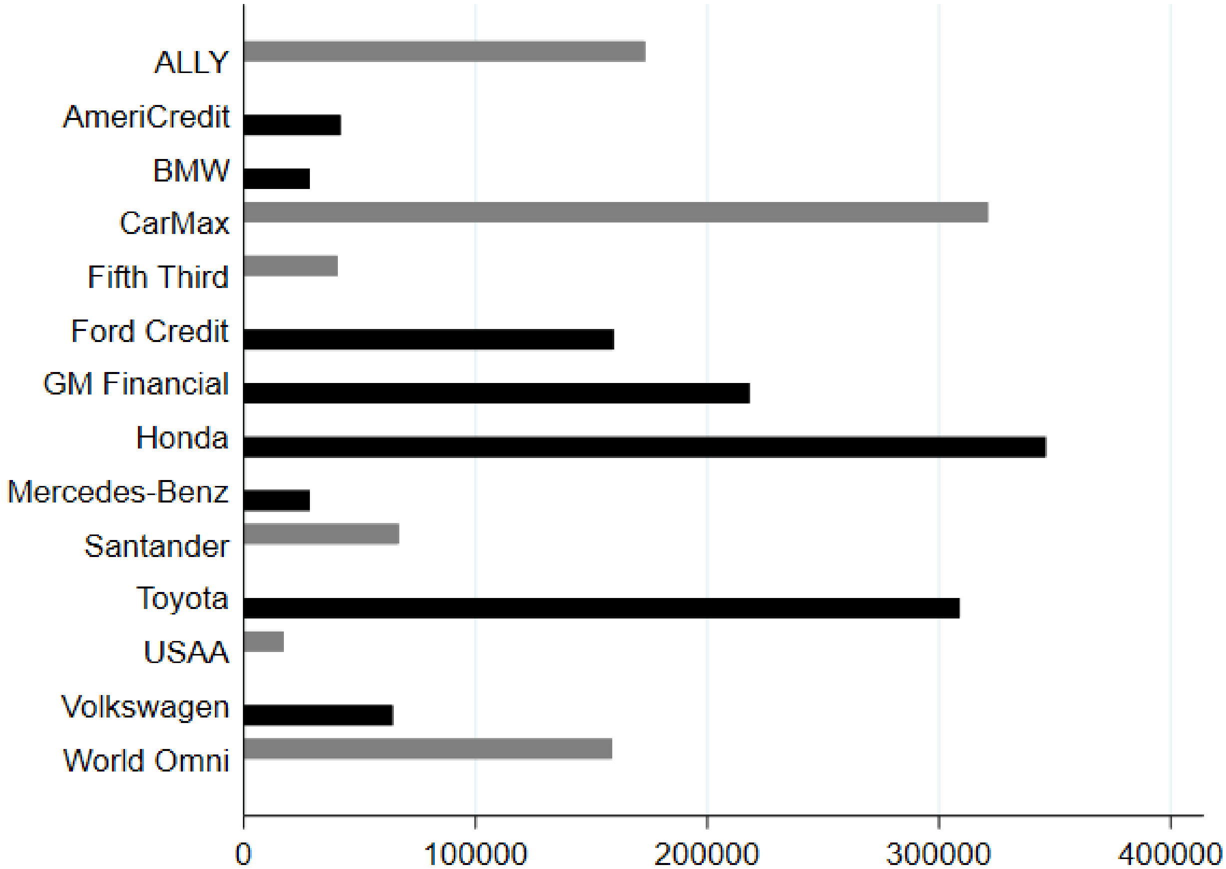
	Total automotive (1)	GM Financial (2)
Net sales and revenues	\$133,607	\$12,151
Earnings (loss) before interest and taxes	\$12,268	\$1,196

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

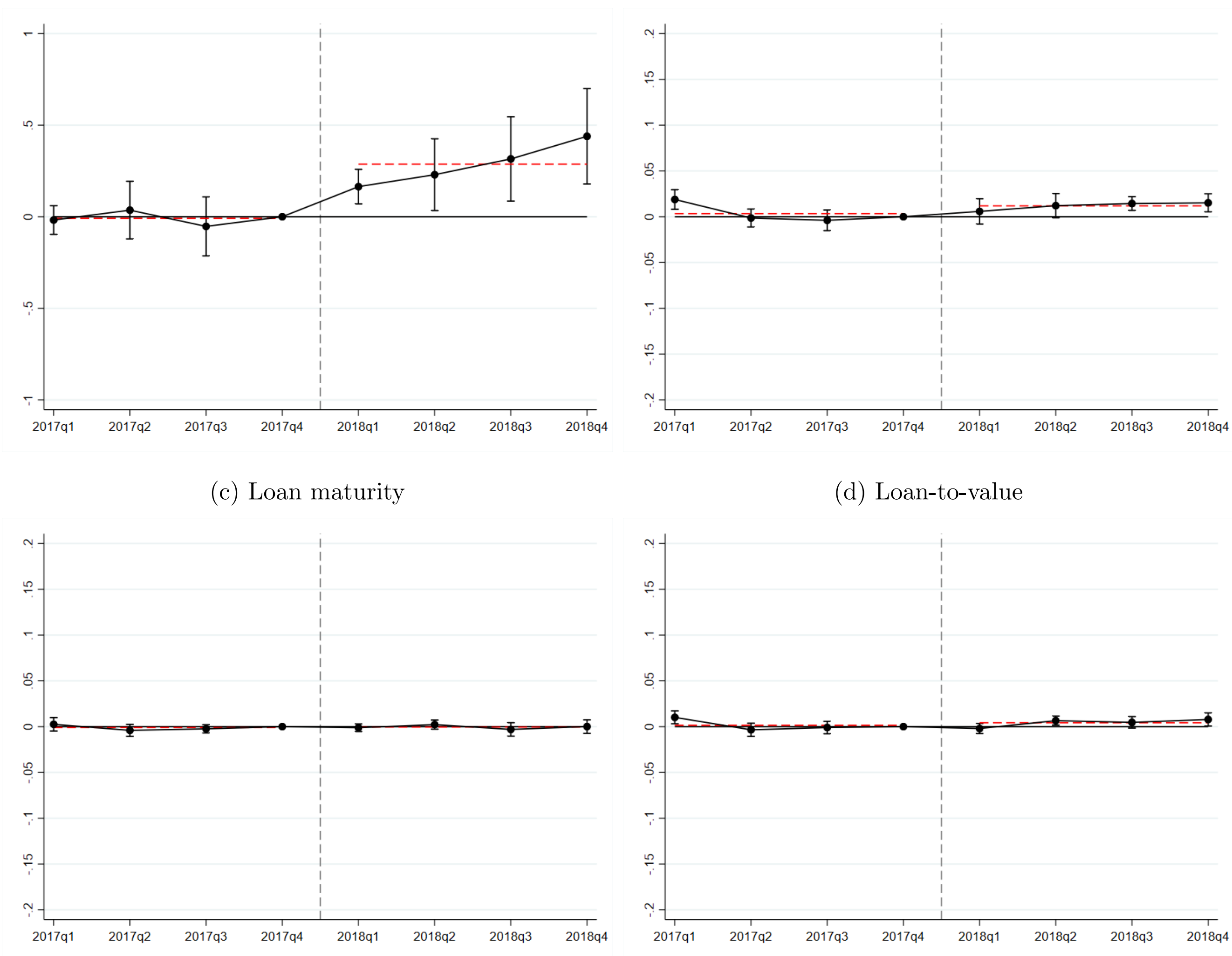
NOTE.——This figure displays GM's revenues and earnings in the year before the tariffs (2017) and the year of the tariffs (2018). Revenues and earnings are split between GM's vehicle sales segment (Total Automotive) and GM's captive financing segment (GM Financial).

Figure 3: Distribution of loans across lenders



NOTE.——This figure plots the sample distribution of loans across lenders. 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 for each lender.

Figure 4: Response of captive auto loan terms



(a) Interest rate

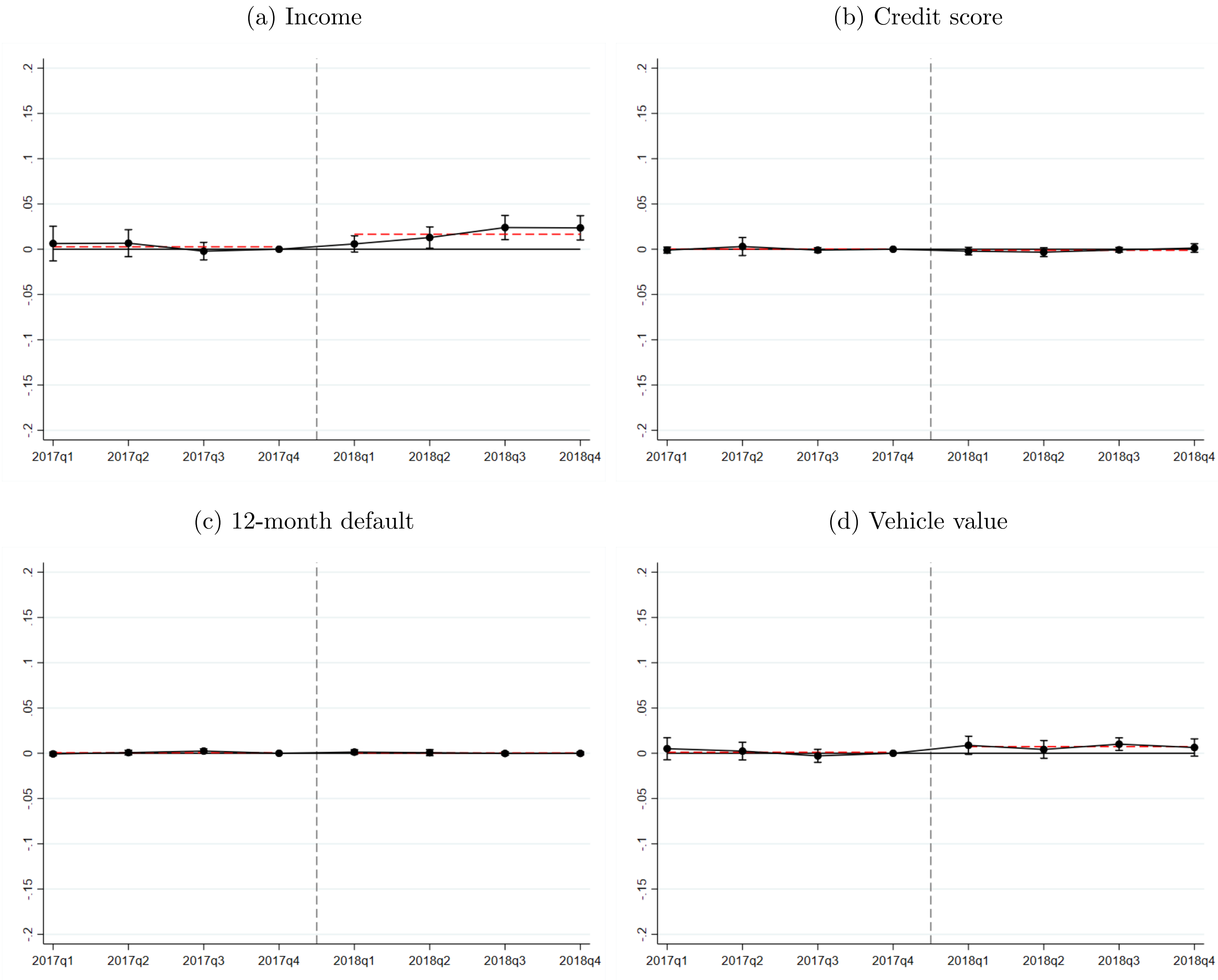
(b) Loan amount

(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.

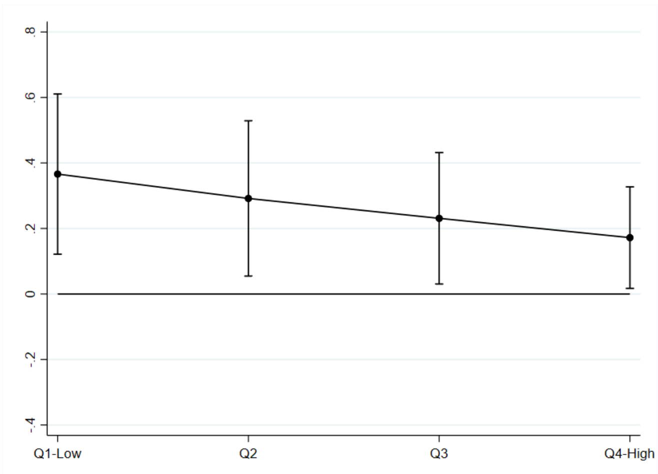
Figure 5: Response of captive borrower characteristics



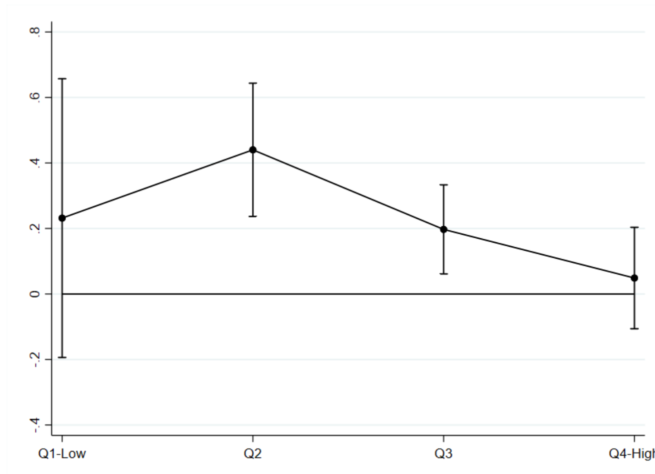
NOTE.——This figure plots coefficient estimates from Equation 3. The dependent variable is either the log household income, log credit score, 12-month default rate, or log vehicle value. A loan is considered to be in default when is 90 or more days past due (including charge-offs and repossessions). The 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 6: Response of captive interest rates across borrower characteristics

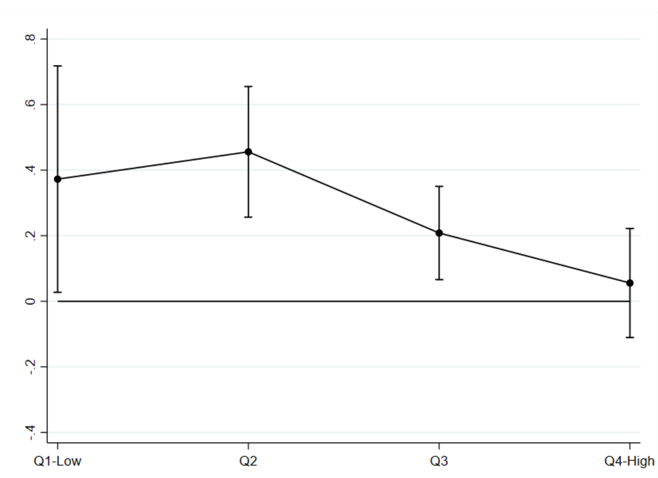
(a) Income



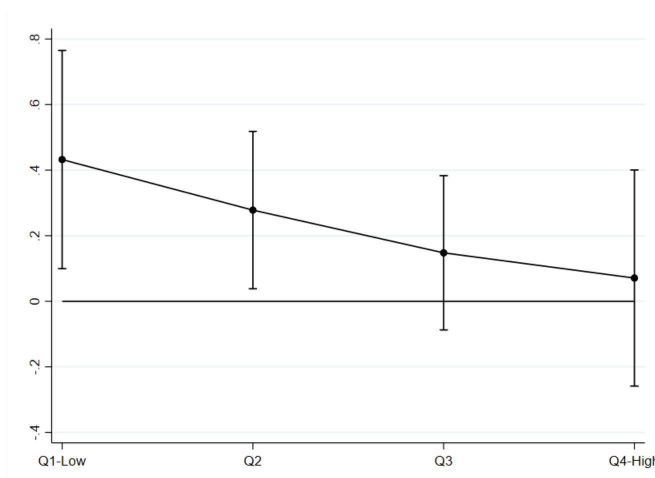
(b) Credit score



(c) Credit score (exlcluding AmeriCredit)



(d) Loan size



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

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

where the dependent variable, $r_{i,t}$, 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. We consider three different borrower characteristics: incomes (Panel A), credit scores (Panels B and C), and loan amounts (Panel D). In the figure, 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 Supplemental tables, figures, and robustness tests

This Appendix contains supplemental tables and figures as well as a detailed discussion of the robustness tests referenced in Section 4.8.

A.1 Robustness

A.1.1 Standard errors and fixed effects

In Table A.10, we examine whether our results are robust to different assumptions about the 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. Our results are also robust to calculating our standard errors using a wild bootstrap procedure with lender clustering (Cameron, Gelbach, and Miller 2008).

In Table A.11, we examine 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 impacts of other contemporaneous tariffs across states with different manufacturer market shares (i.e,. a manufacturer-state-time correlated omitted variable). Reassuringly, we find that our coefficient estimates are little changed after including more granular versions of our baseline fixed effects (Oster 2019).

Finally, in Table A.12, we show that the increase in captive loan prices persists even after controlling for other co-determined loan terms, such as loan amounts, maturities, and loan-to-value ratios. This helps reinforce that our baseline estimates capture tariff pass-through and not borrower-level adjustments to higher interest rates (Argyle, Nadauld, and Palmer 2020).

A.1.2 Sample filters

As a final robustness test, we confirm that our sample filters do not affect our conclusions. In Table A.13, we re-estimate our baseline difference-in-differences model after adjusting the sample filters that were applied in Section 3.2. Columns 1 and 2 adjust the credit score filter, 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 filter. For each of these cases, we find that our main results persist.[45]

We also re-estimate our baseline model after reincluding the removed lenders from Section 3.2, and again after removing World Omni from the sample (see Footnote 20). As shown in Tables A.14 and A.15, we find that our main results continue to persist.

45. Note that the fact that captive interest rates remain elevated in 2019 is inconsistent with an alternative explanation that centers on wholesale vehicle prices being difficult to adjust in the short-run (and hence incapable of offsetting higher input costs) due to purchase contracts with auto dealers or MSRP price stickiness. We note that while some of the metal tariffs were lifted on Canada and Mexico in May 2019, it was with the caveat that they might be reimposed.

Table A.1: Comparison of loan terms in Regulation AB II data and population credit bureau data

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 population credit bureau data. The comparisons are conducted at the lender level, and the sample of loans are those 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 A.2: Difference-in-differences regression: New-versus-used cars

Panel A: New cars

	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 cars

	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.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

Table A.3: Difference-in-differences regression: Loan originations

	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	596,568	795,360	795,360
R^2	0.42	0.73	0.43	0.53

NOTE.—This table reports coefficient estimates from Equation 9 with aggregations at the lender instead of the captive level. 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. In Columns 1 and 2, we calculate the number of loan originations at the lender (l) × state (s) × vehicle make-model-condition (v) × quarter (t) level. In Columns 3 and 4, we calculate the number of loan originations at the lender × state × income bucket (ω) × credit score bucket (c) × quarter level. We estimate a regular linear regression model in Columns 1 and 3 and a Poisson model in Columns 2 and 4. The variable Treated is equal to one if lender l is a captive lender and zero otherwise. 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.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

Table A.4: Difference-in-differences regression: Vehicle choices

Panel A: Dollar vehicle value

	All vehicles		New vehicles		Used vehicles	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated x Post	72	-359	550	-17	-96	-253
	(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	1,973,396	1,973,396	1,289,937	1,289,937	683,459	683,459
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 x Post	0.01	-0.01	0.02	-0.01	0.00	-0.01
	(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	1,973,396	1,973,396	1,289,937	1,289,937	683,459	683,459
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-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-quarter fixed effects to examine substitution within new and used vehicles. Column (2) includes vehicle condition-type (i.e., truck, SUV, or sedan)-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.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

Table A.5: Alternative explanation: Financing costs

Panel A: All loans

	Interest rate (1)	Interest rate (2)	Interest rate (3)	Interest rate (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)	Interest rate (2)	Interest rate (3)	Interest rate (4)
Treated × Post	0.446***	0.475***	0.367***	0.301***
	(3.36)	(2.65)	(2.58)	(2.97)
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	695,644	695,644	695,644	463,914
R^2	0.68	0.68	0.68	0.76

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.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

Table A.6: Alternative explanation: Dealer mark-ups

	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.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

Table A.7: Alternative explanation: 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 captive 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.

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

Table A.8: Alternative explanation: Changes in securitization practices

Panel A: All lenders

	Originations	Loan amount	Loan maturity	Monthly payment
	(1)	(2)	(3)	(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
R^2	0.72	0.72	0.80	0.77

Panel B: Restricted sample of lenders

	Originations	Loan amount	Loan maturity	Monthly payment
	(1)	(2)	(3)	(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
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. *Maturity* and *Monthly payment* are the same ratios but for average loan maturities, and monthly payments respectively.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

Table A.9: Robustness: Pre-treatment tariff exposure

	Interest rate		12-month default	
	More exposed	Less exposed	More exposed	Less exposed
	(1)	(2)	(3)	(4)
Treated × Post	0.293***	-0.084	0.000	0.001
	(3.26)	(-0.48)	(-0.37)	(0.98)
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	1,851,817	940,310	1,851,817	940,310
R^2	0.71	0.68	0.03	0.03

NOTE.—This table reports coefficient estimates from Equation 2 for the subsamples of more and less exposed captive lenders. The dependent variable is the interest rate. The sample is restricted to auto loans originated between January 2017 and December 2018. In Columns (1) and (3), the sample includes all non-captive auto loans and captive auto loans from our group of more exposed captive lenders (Ford, GM, Honda, and Toyota). In Columns (2) and (4), the sample includes all non-captive auto loans and captive auto loans from our group of more less captive lenders (BMW, Mercedes-Benz, and Volkswagen). 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.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

Table A.10: Robustness: Alternative forms of clustering

	Interest rate (1)	Interest rate (2)	Interest rate (3)	Interest rate (4)	Interest rate (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.

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

Table A.11: Robustness: More granular fixed effects

	Interest rate (1)	Interest rate (2)	Interest rate (3)	Interest rate (4)
Treated × Post	0.255***	0.324***	0.328***	0.347***
	(2.75)	(3.01)	(3.04)	(4.64)
Lender FE	Y	Y	Y	Y
State 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	1,973,067	1,973,067	1,031,917
R^2	0.70	0.73	0.73	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.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

Table A.12: Robustness: Fixed effects for other loan terms

	Interest rate (1)	Interest rate (2)	Interest rate (3)	Interest rate (4)
Treated × Post	0.255***	0.248***	0.300***	0.295***
	(2.75)	(2.69)	(2.94)	(2.88)
Lender FE	Y	Y	Y	Y
State 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	0.85

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.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

Table A.13: Robustness: 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***	0.349***	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	2,498,981	1,881,895	2,086,697	5,407,631	960,415	2,431,877
R^2	0.65	0.85	0.68	0.73	0.85	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.

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

Table A.14: Robustness: Re-including 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**	-0.004	-0.011***	-0.006*
	(2.26)	(-0.88)	(-4.90)	(-1.79)
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	2,436,888	2,436,888	2,436,888	2,436,888
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	0.205*	0.003	-0.009***	-0.002
	(1.74)	(0.68)	(-3.44)	(-0.52)
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	2,550,925	2,550,925	2,550,925	2,550,925
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.

* Significant at the 10% level.

** Significant at the 5% level.

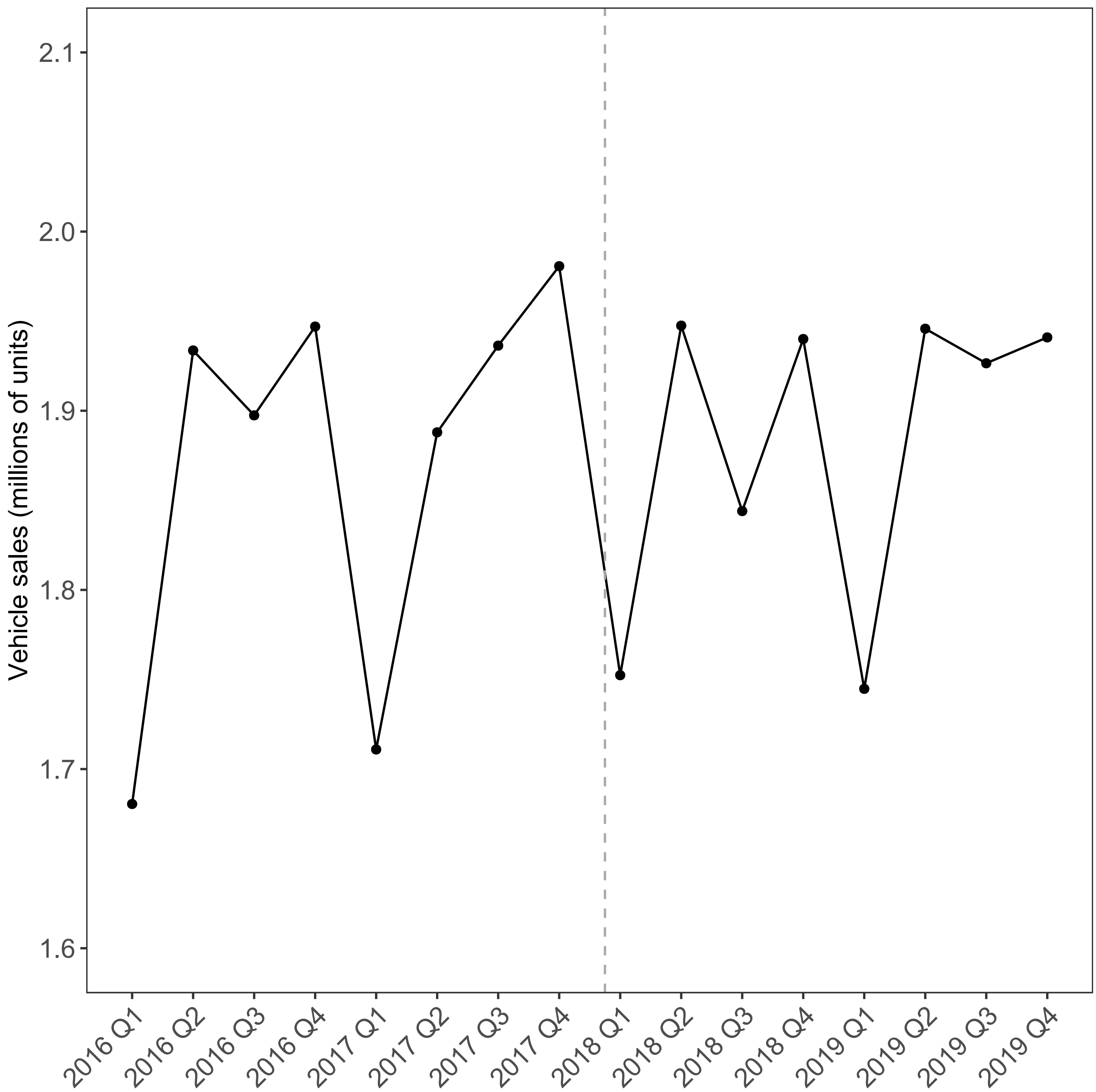
*** Significant at the 1% level.

Table A.15: Robustness: 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.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

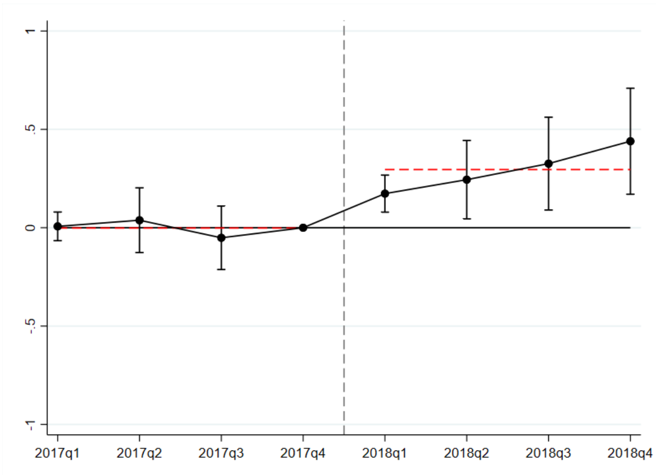
Figure A.1: Alternative explanation: Time series of vehicle sales



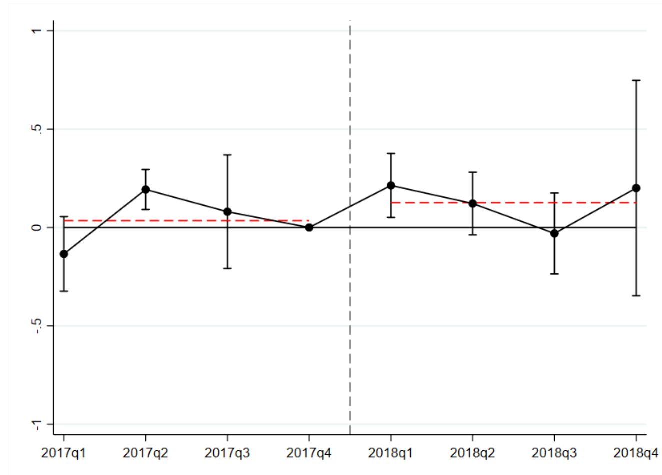
NOTE.——This figure plots the number of vehicles sold in the United States between January 2017 and December 2018 for BMW, Ford, General Motors, Honda, Mercedes-Benz, and Volkswagen. For each manufacturer, we include all of its affiliated brands (i.e., we include Chevrolet sales for General Motors).

Figure A.2: Robustness: More versus less exposed captive lenders

(a) More exposed



(b) Less exposed



NOTE.——This figure plots coefficient estimates from Equation 3 for the subsamples of more exposed captive lenders (Panel A) and less exposed captive lenders (Panel B). 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.

B Model

This Appendix introduces an imperfect competition model of the 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 $p^t = p^d + p^s = p^d \cdot (1 + \bar{d})$.

B.1 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.[46]

B.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.

B.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 $(p_i - c_i) \cdot q_i$, 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.

46. 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.

B.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.

B.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.

B.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.

B.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:[47]

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

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:

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

B.3.1 The size of the spillover effect in relation to the direct effect

As discussed further in Section B.4, empirical identification of the spillover effect p^s in a difference-in-differences setting is infeasible. This is problematic because it implies that difference-in-differences will recover just the direct effect p^d of the cost shock 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. 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:

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

B.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.

47. 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.

B.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.

B.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.

B.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 that this assumption also cannot be tested or falsified using the data, we prefer our model-based identification of the spillover effect over empirical identification.[48]

48. In terms of simple averages, non-captive interest rates rose from 6.30 percent to 6.52 percent following the tariffs, whereas captive interest rates rose from 2.52 percent to 3.63 percent. Our baseline difference-in-differences coefficient is different than the difference between these two estimates because of the various controls imposed.

C Calculations for tariff pass-through

This Appendix provides further details on the calculations from Section 4.5. First, we elaborate on how we estimate ΔP, ΔC, M, and N. Afterwards, we present a range of alternative estimates for the pass-through rate along the loan price margin.

C.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 amount of $\$26,914$ and 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

C.2 Production costs

C.2.1 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.

C.2.2 Media mentions

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."

C.2.3 Weight-based method

Another potential method for backing out the average cost of 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 other 60 percent 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.3 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 $M = 0.90 \cdot (0.55 + 0.07 \cdot 1.50) = 0.59$.

C.4 Number of new vehicles sold

From the U.S. Department of Transportation 2021, there are around $N = 17$ million new vehicles sold in the United States each year.

C.5 Range of estimates

In Table C.1, we present alternative estimates for the rate of pass-through along the loan price margin. We consider values of ΔP between \$150 and \$225 and values of ΔC between \$200 and \$400. Our estimates for the pass-through rate range between 0.22 on the low end (with $\Delta P = \$150$ and $\Delta C = \$400$) and 0.66 on the high end (with $\Delta P = \$225$ and $\Delta C = \$200$).

Table C.1: Range of estimates for rate of pass-through along loan price margin

	ΔC				
ΔP	\$ 200	\$ 250	\$ 300	\$ 350	\$ 400
\$ 150	0.44	0.35	0.30	0.25	0.22
\$ 175	0.52	0.41	0.34	0.30	0.26
\$ 200	0.59	0.47	0.39	0.34	0.30
\$ 225	0.66	0.53	0.44	0.38	0.33

NOTE.—This table presents a range of alternative estimates for the pass-through rate along the loan price margin. ΔP is the average present value increase in financing costs per borrower.ΔC is the average increase in costs per new vehicle.