2023

Essays in Household Finance

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Digital Object Identifier: https://doi.org/10.13023/etd.2023.219

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Essays in Household Finance

DISSERTATION

A dissertation submitted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in the Gatton College of Business and Economics at the University of Kentucky

By
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2023

ABSTRACT OF DISSERTATION

Essays in Household Finance

In my first chapter, I use new granular loan-level data and a novel instrumental variable to estimate the effect of competition among auto dealerships on the joint pricing of cars and car loans. I find that increased competition causes auto dealers to decrease vehicle prices to attract consumers. They, however, offset a large portion of their loss on vehicle prices through charging higher prices on a less transparent margin (i.e., loan markups). Consistent with the monthly payment targeting channel, I find that increased competition does not change consumers' monthly payments. My findings suggest that sophisticated sellers such as auto dealers may exploit the behavioral biases of consumers to maximize their profits. In my second chapter, we study the role of captive finance subsidiaries, vertically integrated lenders, in creating a potential channel for trade policy to affect consumer credit. Examining the Trump administration 2018 metal tariffs' impact on auto manufacturers, we find consumers received worse auto loan terms from captive lenders after the tariff relative to unaffected non-captive lenders. The average interest rate on captive loans increased by 26 basis points while average loan amounts, loan maturities, and loan-to-value ratios decreased. 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. Overall, our results suggest not only that captive finance divisions enable tariff cost pass-through to consumer finance but also that focusing solely on directly affected product prices may underestimate the impact of tariffs. In my third chapter, we study third party quality certification in the market for financial advice. Using the Barron's Top Financial Advisors rankings, we find evidence that being named a top advisor increases both assets under management and accounts for individuals and their firms. The effects increase sharply around thresholds for certification suggesting that clients value the certification itself and not solely the underlying quality. The certification effects are larger for those from smaller firms and newer advisors. Consistent with models of reputation in the financial advisory industry, after certification, advisors are less likely to engage in misconduct.

KEYWORDS: Auto loans, Competition, Shrouded attributes, Steel tariffs, Misconduct, Third party quality certification

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May 10, 2023

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ACKNOWLEDGMENTS

First, I would like to thank my Dissertation Committee: Russell Jame, David Sovich, Igor Cunha, Charlie Clarke, Carlos Lamarche, and Ana Herrera. Their willingness to support me in this program has been crucial. In particular, I would like to express my deepest appreciation to Russell Jame and David Sovich for the countless hours that they have donated in support of my professional development throughout the years. I could not have undertaken this journey without their patient guidance and mentorship. I would like to extend my sincere thanks to Igor Cunha and William Gerken for their thoughtful comments, valuable insights, and guidance along the way. I have had the pleasure of working with Will on Chapter 3. I must also express my gratitude to Kristine Hankins and Charlie Clarke. I have had the opportunity of working on Chapter 2 with Kristine and another research project with Charlie. Finally, I am grateful to my fellow Ph.D. students and the Department of Finance and Quantitative Methods at the Gatton College of Business and Economics.

Chapter 2 has benefited from comments and discussions with David Agrawal, Bronson Argyle, Matteo Benetton, Bo Bian, Felipe Benguria, Richard Friberg, Mark Hoekstra, Ali Hortaçsu, Brittany Almquist Lewis, James Weston, and audiences at Arizona State University, Cal State Fullerton, the Federal Reserve Bank of Atlanta, Miami University, Loyola Marymount University, the Michigan State FCU Conference, Virginia Tech, Rice University, University of Arizona, the University of British Columbia Summer Finance Conference, University of Kentucky, the Financial Research Association Conference Early Ideas Session, and the Indiana University Craig Holden Memorial Conference.

Chapter 3 has benefited from comments and discussions with Steve Dimmock, Eliezer Fich, Andrey Golubov, Russell Jame, Jon Karpoff, Amit Kumar, and David Sovich. We also thank seminar participants at the American Finance Association Annual Meeting, Financial Management Association Annual Meeting, Boise State University, and University of Kentucky. We also acknowledge the support of the Institute for the Study of Free Enterprise.

Lastly, I am deeply indebted to my parents and brothers for their continued love and support. This endeavor would not have been possible without Sarah Najafzadeh Khoei. Her endless love and support have made this process more rewarding and manageable.

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Chapter 1 Competition and Shrouded Attributes: Evidence from the Indirect Auto Loan Market

1.1 Introduction

In many markets, sellers tend to make the total cost to purchase a product less transparent and more difficult to process by dividing the total cost into salient and non-salient components. Hidden fees on credit cards, fine-print shipping and handling charges, and mortgages with complex features are just a few examples.[1] These price partitioning practices or shrouding attributes may help sellers to maximize their profits by "ripping off" consumers (Ellison 2005, Spiegler 2006, Gabaix and Laibson 2006, Carlin 2009, Piccione and Spiegler 2012, Ellison and Wolitzky 2012, Chioveanu and Zhou 2013). Competition may be a potential remedy to eliminate this inefficiency: increased competition could lead sellers to reveal non-salient prices and win over consumers. Recent theoretical models, however, predict that when consumers are myopic (or unaware), sellers respond to greater competition with greater efforts to shroud prices (Spiegler 2006, Gabaix and Laibson 2006, Carlin 2009). Therefore, the price effect of the competition is ambiguous in these markets.

In this paper, I use the U.S. indirect auto loan market as a laboratory to answer this question. This market is an ideal setting for several reasons. First, the total cost charged by auto dealers can be separated into a salient margin (i.e., vehicle prices) and a non-salient margin (i.e., loan markups). Second, a car purchase is a financially complex process that the majority of households go through only a few times in their lives. For example, auto loans are customized depending on the household's creditworthiness and the details of the auto loan (loan size, loan maturity, loan to value ratio, etc.), making the comparison of price quotations very difficult. Third, auto dealers intermediate about 80 percent of auto loans in the United States. In aggregate, auto loan debt is the third largest form of household debt in the United States, behind mortgages and student loans. The total auto debt in the U.S. is more than $1.4 trillion, with over 113 million outstanding loans (FRBNY 2021). Given the economic importance of auto dealers, the extent to which local competition among them affects the joint pricing of cars and car loans is a first-order question.

Empirical identification of this effect comes with several challenges. First, few data sets have comprehensive information on features of vehicles and characteristics of borrowers. For example, auto loan data from the credit bureaus lack information on vehicle features, making the identification of homogeneous products almost impossible. Second, identifying the effect of competition on consumer welfare is challenging. Naïve regressions of vehicle prices or loan markups on the number of auto dealers are unlikely to provide causal estimates. In particular, reverse causality and omitted variable bias could be problematic. For example, auto dealers are more likely to do business in markets where they have a higher chance of charging substantial markups.

[1] See Greenleaf et al. 2016 for an overview of non-salient charges across industries.

In this paper, I overcome these challenges using a novel loan-level dataset with comprehensive information on features of vehicles and characteristics of borrowers. The granularity of the data allows me not only to better control for the borrower's characteristics at the time of origination, but also to estimate the coefficient of interest for homogeneous vehicles (e.g., new 2018 Toyota Camry). The data also includes loan performance histories over the entire life of each loan, allowing me to measure ex-post default and paid-off rates. The data also covers major auto lenders in the United States, lessening concerns regarding data representativeness.

To overcome identification issues, I exploit plausibly exogenous variation in the potential number of dealerships selling new cars. The variation stems from the interaction of the state-level relevant market area and the amount of developable land in a given market. The intuition of my identification strategy is straightforward. Starting in the late 1950's, virtually all states enacted automobile franchise laws to protect car dealerships against car manufacturers' superior bargaining power. Specifically, from the late 1950's to the early 2000's, most states enacted laws prohibiting a car manufacturer from granting a new car dealership selling the same line-make vehicles within the exclusive relevant market area (RMA), measured as a radii of X miles from an existing dealership. For example, the relevant market area in Kentucky is 10 miles. The number of dealerships selling new cars in a market, however, is limited not only by the size of the relevant market area, but also by the amount of developable land in a market. For example, assume two local markets with the same relevant market area of 10 miles, where one is severely land-constrained by its geography, and another is completely flat with no area lost to internal water bodies, wetlands, or lands with slopes above 15%. The land-constrained market should experience less local competition because the potential number of dealerships is limited, which leads to higher market concentration among dealerships selling new cars. In contrast, the local market with greater amount of developable areas should experience more local competition because entry to this market is easier and the number of dealerships selling new cars is not limited by the predetermined geographic features of the market. Since the variation in the predetermined geographic features is not fundamentally randomly assigned, I first include developable land quartile fixed effects to preclude the possibility that the results are driven by states with a very different amount of developable lands. I also include granular fixed effects to rule out potential omitted variables.

I also provide some evidence to support the validity of my instrumental variable. First, I find that the instrument is strongly correlated with the endogenous number of dealers selling new cars. Next, I find that the instrument does not predict state-level macroeconomic outcomes. This suggests that the instrument is not systematically correlated with time-invariant differences across states. This increases the confidence in supporting the validity of the instrument. Furthermore, I use a unique feature of my instrumental variable, in which it affects only the number of dealerships selling new cars. Using a placebo test, I find in reduced form that the instrument does not statistically or economically predict (1) the number of dealerships that exclusively sell used cars[2], or (2) the number of banks in a market, suggesting that the instrument is not correlated with the general demand for vehicles or auto loans across states. This also suggests that the instrument should be less relevant in predicting the variation in vehicle prices and/or loan markups for older used cars. I find no evidence that the instrument statistically or economically predicts vehicle prices and loan markups for old used cars. Overall, this suggests that the instrument has no direct effect on vehicle prices and loan markups other than through the number of dealerships selling new cars.

I find that on average, an increase in the instrumented number of dealerships is associated with a $88.6 decrease in vehicle prices. The economic magnitude of this effect is large given that the average markup on new cars is between 2-5%.[3] I also find that on average, an increase in the instrumented number of dealerships is associated with a 16.8 basis point increase in loan markups. This increase offsets the average decline in vehicle prices, resulting in no change in the average monthly payments and other contract terms. My findings suggest that an average consumer does not benefit from competitive pressure in the indirect auto loan market. Auto dealers, however, offset a big portion of their loss on vehicle prices: the revenue generated from the increase in loan markups is split between auto dealers and auto lenders. Grunewald et al. 2020 show that on average, auto dealers capture around 75% of this revenue.

Next, I investigate potential channels through which local competition among auto dealers affects the joint pricing of cars and car loans. I find that the monthly payment targeting channel is driving my results. Argyle, Nadauld, and Palmer 2020a find that many consumers in the auto loan market target specific monthly payment amounts (e.g., $200, $300, and $400 per month). If increased competition leads to lower vehicle prices, then the corresponding monthly payment amounts mechanically decrease too. Auto dealers may exploit consumers' monthly payment targeting bias by charging higher prices on loan markups such that consumers' monthly payment amounts stay the same across markets. Consistent with this channel, I find no evidence of the effect of competition on monthly payments.

This paper contributes mainly to three strands of literature. First, a related empirical literature suggests that sellers can gain financial benefits — at least in short-term — by engaging in shrouded pricing strategies (Hossain and Morgan 2006, Ellison and Ellison 2009, Brown, Hossain, and Morgan 2010a, Xia and Monroe 2004). My paper complements this literature by studying whether competitive forces may be a remedy to eliminate such exploitation. The closest study to my paper is Agarwal, Song, and Yao 2022, which investigates the effect of baking competition on contract terms in the U.S. mortgage market. My paper is distinct from theirs because I show that increased competition leads to a price adjustment at the intensive margin.

This paper also contributes to the empirical literature on the effects of competition in consumer credit markets, including payday loans (Melzer and Morgan 2015), auto loans (Yannelis and Zhang 2021; Gissler, Ramcharan, and Yu 2020; Argyle, Nadauld,

[2]Unlike franchise dealerships, these dealerships mainly sell low-quality (high-mileage) used cars.

[3]Auto dealers' profit margin for new vehicles is razor thin (Beard and Ford 2016) and is constantly decreasing over time (Levitin 2019). According to The National Automobile Dealers Association (NADA), the average net profit before tax for new vehicles is 2-5%.

and Palmer 2020b), mortgages (Allen, Clark, and Houde 2014; Buchak and Jørring 2021), and credit cards (Dick and Lehnert 2010). For example, Melzer and Morgan 2015 find that banks and credit unions reduce overdraft credit limits and prices when payday credit is prohibited. Gissler, Ramcharan, and Yu 2020 find that competition changes the composition of borrowers, with a reallocation of credit toward subprime borrowers. Dick and Lehnert 2010 find that increased competition is associated with an improvement in screening technologies, which expand consumer credit to both low- and high-risk borrowers. Buchak and Jørring 2021 find the effects of competition on credit access and pricing. They show that lower competition reduces credit access for all borrowers, in particular for female borrowers and borrowers belonging to racial minorities. Yannelis and Zhang 2021 study the effect of competition in presence of costly lender screening and show that competition among lenders has two opposing effects on interest rates. My paper complements this literature by studying the effect of competition among loan intermediaries not lenders.[4] Unlike many credit markets, lenders in the indirect auto loan market compete for auto dealers' business, not borrowers' business. This distinction highlights the important role of intermediaries in this market and may explain why consumers may not fully benefit from the competitive force characterized by many lenders and consumers in the auto loan market. Furthermore, my paper provides a more complete picture of the overall effect of competition by estimating this effect on the joint pricing of cars and car loans. My findings suggest that by ignoring this joint pricing, we may overestimate the price effect of competition.

The closest study to my paper is Allen, Clark, and Houde 2014, which study the relationship between competition and price dispersion in the Canadian mortgage market. They show that increased competition has a heterogeneous impact on borrowers. They argue that search frictions explain this heterogeneity. My paper is distinct from theirs since the heterogeneity in my findings comes from behavioral biases of consumers (i.e., monthly payment targeting). My findings suggest that auto dealers exploit such biases to maximize their profits.

This paper is related to a growing literature on auto loan markets.[5] A major theme of this literature is inherent information asymmetry in the auto lending process. Purchasing a vehicle is a complex and opaque process requiring multiple stages of search (Busse and Silva-Risso 2010). This complexity and opaqueness may result in borrowers' irrational behavior (Grunewald et al. 2020; Argyle, Nadauld, and Palmer 2020a; Argyle, Nadauld, and Palmer 2020b), dealers' exploitation of borrowers (But-

[4] Auto dealers are different from many intermediaries in credit markets. They not only sell a product but also finance it. This is not common for other intermediaries in the credit markets, including mortgage brokers (Ambrose and Conklin 2014; Allen, Clark, and Houde 2014; Woodward and Hall 2012; Robles-Garcia 2022), real estate brokers (Yinger 1981; Anglin and Arnott 1999; Elder, Zumpano, and Baryla 1999; Beck, Scott, and Yelowitz 2012), financial advisors (Egan 2019; Egan, Matvos, and Seru 2019a; Dimmock, Gerken, and Graham 2018b; Gerken and Momeni 2022), and the insurance brokers (Anagol, Cole, and Sarkar 2017).

[5] Zinman 2015 mentions that auto loan markets are understudied despite its economic importance. He argues that the main hurdle is lack of granular dataset. To overcome this concern, I use publicly available loan-level data from Regulation AB II. Please see Momeni and Sovich 2022 for more information.

ler, Mayer, and Weston 2021; Lanning 2021; Cohen 2012; Brown and Jansen 2020; Jansen et al. 2021; Melzer and Schroeder 2017), screening mechanism or technologies to improve quality of borrowers (Einav, Jenkins, and Levin 2012; Jansen, Nguyen, and Shams 2021; Yannelis and Zhang 2021; Jansen, Kruger, and Maturana 2021), and lenders' ability to pass-through costs (Hankins, Momeni, and Sovich 2022; Benneton, Mayordomo, and Paravisini 2022). The closest study to my paper is Grunewald et al. 2020, which find that consumers respond substantially more to vehicle prices than loan prices. My work is distinct from theirs since I investigate how and how much competition among auto dealers affects the joint pricing of cars and car loans.

1.2 Institutional Details and Identification Strategy

This section first gives an overview of the institutional details of the indirect auto lending market in the United States and then provides details on the identification strategy.

1.2.1 Institutional Details of Indirect Auto Lending Market

Indirect auto lending refers to auto financing through a car dealership.[6] The majority of auto financing is indirect: about 90% of consumers finance their vehicles through car dealerships. In a typical auto purchase transaction, after a consumer first searches for a make and model of vehicle, a sales agent negotiates with her about vehicle specifics such as vehicle price and available options. Then, she is sent to the dealer's Finance and Insurance (F&I) agent to finalize her purchase by arranging her financing terms. In particular, the F&I agent may submit her credit application to more than 1,500 lenders through major dealer management systems such as *DealerTrack* or *RouteOne* (Grunewald et al. 2020). After receiving the credit application of the consumer, lenders review her information and decide either to deny it or offer a buy rate, which is the minimum interest rate at which the lender will acquire the loan from the dealer.[7] The lender's buy rate is a risk-adjusted rate that captures the credit risk of the consumer. This process varies across consumers' creditworthiness. For prime borrowers, it is fully automated and happens quickly. For subprime borrowers, however, it may take longer due to additional verification steps.

As compensation for processing the paperwork, the lender may allow the dealer to add a markup to the lender's buy rate.[8] The markup is discretionary and does not reflect the credit risk on the loan. While the loan markups are discretionary, some lenders may impose caps to not only avoid potential class-action lawsuits, but also lower the consumer's default and prepayment risk (Cohen 2012). Under pre-specified

[6] Another form of auto financing is commonly referred to as "direct auto lending", in which a consumer directly applies for an auto loan. For new vehicles, only about 10% of auto loans in the United States are financed through direct lending.

[7] Technically, the dealer originates the loan and then the lender will buy the loan from the dealer at the lowest interest rate. In practice, we can assume that the lender originates the loan since the dealer already knows the buy rate and sells the loan immediately (Grunewald et al. 2020; Levitin 2019).

[8] Some lenders may offer a flat fee or a combination of a flat fee and a markup for compensation.

contracts between dealers and lenders, the revenue generated by markups may be split between them. The dealer's share of revenue is commonly called "dealer reserve" or "dealer participation".

Under the existing institutional structure of this market, the consumer has no information about whether her auto loan is marked up since dealers are not required to disclose the lender's buy rate and the dealer's markup. In other words, the final loan rate offered to the consumer is the sum of the lender's buy rate and the dealer's discretionary markup rate. Furthermore, dealers are not required to disclose all offers a consumer is eligible for. Thus, to learn about the market interest rate available to her, the consumer must visit another dealer and go through a formal application process again. This gives the dealer substantial leverage in handling auto loans.

1.2.2 Identification Strategy

To provide evidence on the causal effect of competition among auto dealers on vehicle prices and loan markups, I should address the endogeneity concern coming from a naïve regression of vehicle prices or loan markups on the number of auto dealers in a market. Omitted variables and reverse causality are likely to prevent the causal interpretation of the point estimate from a naïve regression. For example, market-specific characteristics and consumer sophistication could affect both demand for vehicles, loan markups, and vehicle prices. To address these concerns, I use an instrumental variable stemming from the interaction of the state-level relevant market areas and the amount of developable land. My identification strategy is designed to exploit the variation in the potential number of dealerships selling new cars as an instrument for the number of dealerships selling new cars in a given market.

The intuition of my identification strategy is straightforward. Historically, auto manufacturers had superior bargaining power over car dealerships (Marx 1985).[9] Federal and state legislators enacted several laws to protect new car dealerships against manufacturers' abuse of their bargaining power (Lafontaine and Scott Morton 2010; Brown 1980). In particular, some states have prohibited an automaker from granting a new car dealership selling the same line-make vehicles within the relevant market area (RMA), measured as a radii of X miles from an existing dealership.[10] Starting

[9] For example, a car manufacturer could terminate the franchise agreement at will and without providing any cause or force car dealers to purchase unwanted vehicles. In an infamous example, during the 1929 Great Depression, Ford Motor Company forced its dealers to buy new, unordered vehicles, despite the fact that dealers had a very small chance of selling them (Surowiecki 2006).

[10] The definition of relevant market area may vary across states. Many states measure relevant market area as a radii of X miles from an existing dealership. Some states, however, may be more specific about it. For example, Massachusetts franchise law defines the relevant market area as "the geographic area surrounding the boundary of a dealership, determined as follows: (1) If all boundaries of a dealership located in the counties of Bristol, Essex, Hampden, Middlesex, Norfolk, Plymouth or Suffolk are 8 or more miles from the border of the counties of Barnstable, Berkshire, Dukes, Franklin, Hampshire, Nantucket and Worcester, then the geographic area shall be the entire land mass encompassed in a circle with a radius of 8 miles from any boundary of the dealership. (2) If all boundaries of a dealership located in the counties of Barnstable, Berkshire, Dukes, Franklin, Hampshire, Nantucket or Worcester are 14 or more miles from the border of the counties of Bristol, Essex, Hampden, Middlesex, Norfolk, Plymouth and Suffolk, then the geographic area shall be

in the late 1950's until the early 2000's, almost all states have imposed a dealers' exclusive territory requirement to some extent to limit the number of dealerships selling new cars in a given market. 35 states explicitly imposed a dealers' relevant market area. For example, the relevant market area in Kentucky is 10 miles. To provide an example, I map Toyota dealerships located in Lexington, KY. Figure 1.1 shows that Toyota dealerships in Kentucky are approximately 10 miles apart from each other.

Furthermore, all states have prohibited car manufacturers from selling new vehicles directly to consumers. Virtually all new vehicles in the United States must be sold through only franchise dealerships. This restriction, along with the restriction on the location of new car dealers, provides an ideal setting to examine the effect of local competition among auto dealerships. These restrictions ensure that in a given market, the number of new car dealers selling the same new line-make vehicles is limited by the size of the relevant market area and the amount of developable land in a given market.

State-level relevant market areas vary from 5 to 25 miles, and 54 percent of states have a relevant market area of 10 miles. Figure 1.2 shows the distribution of relevant market area across states. This variation in the relevant market areas may be driven by state-level differences in the political power of dealership associations, population, economic conditions, or the financial literacy of consumers at the time of franchise law enactment. For example, more (less) populated states may have smaller (larger) relevant market areas. This may mechanically correlate with the number of dealerships selling new cars in a market, ceteris paribus.

To address this challenge, I use an instrumental variable similar in spirit to Mian and Sufi 2011, which use housing supply elasticity based on developable lands as an instrument for house price growth. I, however, use the interaction of the state-level relevant market area and the amount of developable land as my instrumental variable. Using satellite-based data on terrain elevation and presence of water bodies, Saiz 2010 precisely estimates the amount of developable land within a 50-km radius of each U.S. metropolitan central city. He first measures the area that is unavailable for residential or commercial real estate development.[11] Using elevation data from the USGS Digital Elevation Model (DEM) at its 90-m resolution, he uses a GIS software to calculate the exact share of the area corresponding to land with a slope above 15% within a 50-km radius of each metropolitan central city. Next, Saiz 2010 uses the 1992 USGS National Land Cover Dataset and contour maps to calculate land forgone to oceans, the Great Lakes, wetlands, lakes, rivers, and other internal water bodies. He then measures the amount of developable land at the MSA level.

the entire land mass encompassed in a circle with a radius of 14 miles from any boundary of the dealership. (3) For all dealerships in the commonwealth which are not included in (1) or (2), inclusive, of this definition, the geographic area shall be a land mass comprised of circular arc segments with a radius of 8 miles from any boundary of the dealership for the arc segments that fall within the counties of Bristol, Essex, Hampden, Middlesex, Norfolk, Plymouth and Suffolk; and with a radius of 14 miles from any boundary of the dealership for the arc segments that fall within the counties of Barnstable, Berkshire, Dukes, Franklin, Hampshire, Nantucket and Worcester."

[11]Under architectural development guidelines, land with slope above 15% is severely constrained for real estate development.

To construct my instrumental variable, I use the interaction of the state-level variation in the predetermined developable land and the state-level relevant market area. To illustrate, assume two local markets with the same relevant market area of 10 miles; one is severely land-constrained by its geography, and another is completely flat with no area lost to internal water bodies, wetlands, or lands with slopes above 15%. The land-constrained market should experience less local competition among dealerships because the potential number of dealerships selling new cars is limited, which leads to higher market concentration. In contrast, the local market with more developable areas should experience more local competition because entry to this market is easier and the potential number of dealerships selling new cars is not limited by the predetermined geographic features.

To provide suggestive visual evidence of the effect of the predetermined developable lands on the potential number of dealerships selling new cars, I illustrate two hypothetical markets, in which a market is defined as a 50-kilometer (or 31.07-mile) radius. Figure 1.3 Panel A shows that Market 1 is a flat land with no area forgone to the sea, internal water bodies and wetlands, and the lands have slopes above 15%. Market 2, however, is land-constrained by its geography. These predetermined geographic features limit the potential number of dealerships selling new cars in Market 2.

Figure 1.3 Panel B shows that the maximum number of dealers selling new cars in Market 1 (Market 2) is 7 (5).[12] This variation exclusively comes from the predetermined geographic features of each market. One obvious concern with this instrumental variable is that the amount of the predetermined developable lands may affect vehicle prices and/or loan markups other than through the number of dealerships selling new cars, violating the exclusion restriction condition.

To address this concern, first, I use developable land quartile fixed effects as well as a set of borrowers and vehicle characteristics to control for potential omitted variables. Second, I use a unique feature of this instrument, in which it affects only car dealerships selling new cars. As a placebo test, I show that the instrument does not predict the number of dealerships selling used cars. This suggests that the results are not driven by unobservable differences across states.

To construct my instrumental variable, I use satellite-based geographic data from Saiz 2010.[13] I first calculate *Developable Areas* at the MSA level by subtracting the areas forgone from total available areas within 31-mile radii (or 961π square miles). Since the granularity level of auto loan data is at the state level, I then construct *Developable Areas* at the state level by calculating a simple arithmetic average of *Developable Areas* at the MSA level using Equation 3.1:

$$Developable\ Area_s = \frac{\sum_{i=1}^{n} Developable\ Area_{s,i}}{n_s} \tag{1.1}$$

[12] In this example, I assume car dealers have perfect market power within their relevant market area. In other words, I assume there is no overlap between dealers' relevant market areas. This understates the potential number of dealers in a given market. This should have no effect on my main results since I use a relative measure.

[13] I thank Albert Saiz for sharing this data.

where n_s is the total number of MSAs in state s. Next, I divide the average developable areas at the state level by the corresponding state-level relevant market area squared (πRMA_s^2) using Equation 3.2:

$$\mathit{Potential\ Number\ of\ Dealers}_s = \frac{\mathit{Developable\ Area}_s}{\pi\ RMA_s^2} \tag{1.2}$$

where *Potential Number of Dealers*$_s$ measures the maximum number of new car dealerships selling the same line-make vehicles in state s. Next, using the location of new car dealerships from AtoZ databases, I calculate the number of new car dealerships selling the same line-make vehicle v (e.g., new 2018 Toyota Camry) within a 50-kilometer (31.07-mile) radius from metropolitan central cities. To be consistent with the granularity level of the instrument, I then calculate the number of new car dealers selling the same line-make vehicle v at the state level using Equation 3.3:

$$\mathit{Number\ of\ Dealers}_{v,s} = \frac{\sum_{i=1}^{n} \mathit{Number\ of\ Dealers}_{i,v,s}}{n_s} \tag{1.3}$$

where *Number of Dealers*$_{v,s}$ measures the endogenous number of new car dealers selling the same line-make vehicles v in state s. *Number of Dealers*$_{i,v,s}$ is the number of new car dealerships selling the same line-make vehicles v in the MSA i in the state s, and n_s is the total number of MSAs in state s.

The reverse causality and omitted variables could be a problem in a naïve regression of loan markups on the endogenous number of dealerships selling new cars in a market. For example, the number of dealers selling new cars may be correlated with market-specific characteristics in a way that confound the causal interpretation of my point estimate. To address the endogeneity concerns, I instrument the number of dealerships selling new cars by the potential number of dealerships selling new cars stemming from the relationship between the state-level relevant market area and the amount of developable land in a given market. To formalize the instrumental variable approach, I run the two-stage least squares (2SLS) regression outlined in Equations 3.4 and 3.6:

$$Num.Dealers_{v,s} = \alpha + \beta_1 Poten.Num.Dealers_s + \delta_{rma} + \delta_{dl} + \delta_{l,t} + \delta_{v,t} + \delta_{i,t} + \delta_{c,t} + \epsilon \tag{1.4}$$

$$LoanMarkups_{v,l,c,i,s,t} = \alpha + \gamma_1 \widehat{Num.Dealers}_{v,s} + \delta_{rma} + \delta_{dl} + \delta_{l,t} + \delta_{v,t} + \delta_{i,t} + \delta_{c,t} + \epsilon \tag{1.5}$$

where *Loan Markups*$_{v,l,c,i,s,t}$ is the loan markup for auto loans originated by lender l for borrowers with income i and credit score s, who purchase new vehicle v at time t and state s. *Potential Number of Dealers*$_s$ is the instrumental variable based on the interaction of the state-level relevant market area and the amount of developable land in a given market. *Number of Dealers*$_{v,s}$ is the number of new car dealerships selling the same line-make vehicles v in state s. The key variable of interest, $\widehat{\mathit{Number\ of\ Dealers}}_{v,s}$, is a continuous variable, that captures the instrumented number of new car dealerships selling the same line-make vehicles v in state s. δ_{rma} is relevant market area fixed effects and controls for unobservable differences across relevant market areas. δ_{dl} is developable land quartile fixed effects and ensures that the variation in the amount of developable land is not driven by states with an extreme amount of developable lands. I also control for quality of borrowers by adding income fixed effects ($\delta_{i,t}$) and credit score fixed effects ($\delta_{c,t}$). The income fixed effects are defined as \$50,000 income bins and the credit score fixed effects are defined as a 25-point credit bin. Moreover, I include lender fixed effects ($\delta_{l,t}$) to ensure that the variation in loan markups is not driven by variation in the pricing strategies of auto lenders. Finally, I add vehicle fixed effects ($\delta_{v,t}$) to estimate the effect of competition for homogeneous vehicles. Vehicle fixed effects refer to vehicle make-model-model year combinations. This ensures that the variation in loan markups is not driven by variation in quality of vehicles. Standard errors are clustered at the state level.

1.3 Data and Sample Selection

My main data source comes from Regulation AB II. As part of the Dodd-Frank Act, the Securities and Exchange Commission (SEC) reformed the rules governing the asset-backed securities (ABS) market, which resulted in Regulation AB II (Reg AB II). Under this regulation, issuers of public auto loan asset-backed securities (ABS) are required to report loan-level information to the Securities and Exchange Commission at a monthly frequency (Sweet, 2015). The data includes information on loan, vehicle, and borrower characteristics as of the loan origination date, as well as loan performance histories over the entire life of each loan.

As of May 2020, there are more than 11 million unique loans (183 million loan-month observations) in the dataset. The loans come from 181 distinct ABS and 19 lenders. Fourteen of the top 20 auto lenders in the United States are in the Reg AB II data. The data contains two types of lenders: (1) indirect lenders that mainly originate loans through car dealerships, and (2) lenders that originate both direct and indirect loans. Indirect lenders include AmeriCredit, BMW Financial Services, Ford Motor Credit Company, GM Financial, American Honda Finance Corporation, Hyundai Motor Finance, Mercedes-Benz Financial Services, Nissan Finance, Toyota Financial Services, Volkswagen Financial Service, and World Omni Financial Corporation. Loans from indirect lenders make up 68 percent of the data. Lenders that originate both direct and indirect loans include Ally Bank, Mechanics Banks (California Republic Bank), Capital One Financial Corporation, CarMax Auto Finance, Fifth Third Bank, Santander Bank, and the United Services Automobile Association (USAA).

I restrict the estimation sample to auto loans originated after 2017 and loans originated within the United States. I remove loans with income above \$250,000, vehicle values above \$100,000, and interest rates above 30 percent. I also restrict my estimation sample to indirect lenders, eliminating concerns regarding different compensation schemes across auto loan brokers. I also drop subvented loans[14] from my estimation sample for two reasons: (1) car dealers are not allowed to mark up

[14]Subvented loans are commonly referred to auto loans that a car manufacturer reduces the cost of financing through cash back programs or rate rebate programs.

interest rate for subvented loans, and (2) subvented loans may add measurement errors in the estimated buy rates and markups.

I also remove loans with credit scores below 620 for two reasons. First, Jansen, Kruger, and Maturana 2021 find that car dealerships may treat prime and subprime borrowers differently. They show that subprime borrowers receive subsidized financing in terms of a discount at which the lender is willing to purchase the loan from the dealer. Car dealers, however, mark up interest rates to borrowers with higher credit scores. This filter ensures that markups for borrowers in my sample are rarely negative. Second, to calculate risk-adjusted interest rates for subprime borrowers, lenders may rely not only on hard information (credit score, loan to value ratio, loan maturity, etc.), but also on soft information (Grunewald et al. 2020). Excluding subprime borrowers increases my confidence that the estimated risk-adjusted interest rates come solely from hard information. This may mitigate the measurement errors in estimated loan markups.

My primary outcome variables are loan markups and vehicle prices. In the indirect auto lending, the interest rate that is offered to a borrower consists of two parts: the lender's buy rate and the dealer's markup. The lender's buy rate is a risk-adjusted rate. The dealer's markup, however, is discretionary. In other words, the lender's buy rate is the minimum interest rate that the indirect lender will require for the loan. I use this feature of the indirect auto lending to estimate the dealer's markups. Specifically, I first estimate the lender's buy rate by calculating the minimum interest rate among loans originated from the same lender for similar borrowers for the same vehicle at the same time and state. Then, I calculate estimated markups by subtracting the estimated lender's buy rate from interest rates of similar loans. I construct the loan markup variable using Equation 1.6.

$$LoanMarkup_{j,v,l,c,i,s,t} = InterestRate_{j,v,l,c,i,s,t} - Min[InterestRate_{v,l,c,i,s,t}] \tag{1.6}$$

where the outcome variable, $LoanMarkup_{j,v,l,c,i,s,t}$, is the difference between interest rate for loan j and the minimum interest rate charged by the same lender (e.g., Toyota Financial Services) for borrowers with the same income and credit score, who buy the same vehicle (e.g., new 2018 Toyota Camry) in the same quarter and state. In the estimation sample, I drop cells with only one observation, ensuring that there is enough variation in each cell.

Since the Regulation AB II data does not include vehicle prices, I use a new dataset from the Texas Department of Motor Vehicles. The data consists of information on the make, model, and model year of the purchased car as well as the car sales price and the time of purchase. Using the average car sales price at the make-model-model year-month of purchase level, I estimate Equation 1.7:

$$VehiclePrice_{v,t} = \alpha + \eta_1 LoanSize_{v,t,j} + \eta_2 CarValue_{v,t,j} + \delta_t + \delta_{vehiclevaluesource} + \epsilon \tag{1.7}$$

where $Vehicle\ Price_{v,t}$ is the average vehicle sales price from the Texas Department of Motor Vehicles, $Loan\ Size_{v,t,j}$ is the loan amount for borrower j who purchased vehicle v at time t, $Car\ Value_{v,t,j}$ is the car value for borrower j who purchased vehicle v at time t, δ_t is a list of dummy variables for each month, $\delta_{vehicle\ value\ source}$

is a list of dummy variables for the source of vehicle value. The R^2 for the above regression is 0.871, suggesting that the explanatory variables in the regression explain the vast majority of the variation in vehicle sales prices from the Texas Department of Motor Vehicles. Next, I estimate vehicle prices in the Regulation AB II data by using estimated coefficients from the above regression.

The second data source I use is AtoZ Databases. The data provides information on both new and used car dealerships, including business name, physical address, website, employees' name and gender, primary SIC number, NAICS number, and year established. I first use the latitude and longitude of dealerships selling new cars to calculate the number of dealerships selling new cars within 50 kilometers (31 miles) of each metropolitan central city. I then repeat this procedure to calculate the number of dealerships selling used cars in a given market. The third data source comes from the Summary of Deposits (SOD) provided by the Federal Deposit Corporation (FDIC). The data provides the physical location of every bank branch in the United States. I use the longitude and latitude of each bank branch to calculate the number of banks within 50 kilometers (31 miles) of each metropolitan central city.

Table 1 presents the summary statistics for the estimation sample as of the origination date. The sample contains of 91,405 unique loans. The average loan in the sample has an interest rate of 449 basis points, a markup of 236 basis points, a scheduled monthly payment of \$465, a vehicle price of \$24,974, a maturity of 68 months, and an initial principal of \$27,250. The average loan to value ratio is 89.63 percent. The average borrower has a credit score of 754 and a household income of \$80,412. The average number of dealerships selling new cars in a market is 3.62 and the potential number of dealerships selling new cars is 8.13.

1.4 Empirical results

In this section, I first provide some empirical evidence to support the validity of my instrumental variable, then I explore how the local competition among dealerships selling new cars affects the joint pricing of cars and car loans. I then discuss potential channels through which this effect can be explained.

1.4.1 Validity of the instrument

Since the number of dealerships selling new cars, vehicle prices and loan markups are likely to be jointly determined, I use the instrumental variable outlined in Section 1.2.2 to estimate the causal effect of local competition among dealerships on the joint pricing of cars and car loans. In particular, I instrument the endogenous number of dealerships selling new cars via the potential number of dealers selling new cars stemming from the interaction between the state-level relevant market area and the amount of developable land in a given market. To have unbiased point estimates, a valid instrument should satisfy two conditions: the relevance and exclusion restriction conditions.

Relevance condition

To begin, I first provide evidence to satisfy the relevance condition by estimating Equation 3.4. Table 1.2 reports that the instrument statistically and economically predicts the number of dealerships selling new cars. Columns (1) through (3) show that the economic magnitude of the coefficient of interest, (β_1), is stable across different specifications: a one unit increase in the potential number of dealerships selling new cars is associated with a 1.22 unit increase in the number of dealerships selling new cars.[15] This suggests that the instrument is not driven by omitted variables correlated with the quality of borrowers across markets.

In Column (3), I also find that the first-stage f-statistic is 19.74, which exceeds the rule of thumb for strong instruments ($F >= 10$) proposed by Staiger and Stock 1997. This satisfies the relevance condition and confirms that the weak instrument problem is less likely to be a concern. Since my instrumental variable meets the relevance condition of being a valid instrument, we would expect to observe the reduced form relation between the instrument and vehicle prices. In Columns (4) and (5), I find exactly this relation. I find that a one unit increase in the potential number of dealerships selling new cars is associated with a statistically significant decrease in vehicle prices and increase in loan markups.

Exclusion restriction condition

Next, for the causal interpretation of my results, I should satisfy the exclusion restriction condition, in which the potential number of dealerships selling new cars has no direct effect on vehicle prices or loan markups other than through the number of dealerships selling new cars. Although the exclusion restriction cannot be tested directly, I provide some evidence to support its validity.

Since the potential number of dealerships selling new cars in a market is not fundamentally randomly assigned, it is reasonable to be concerned that market-specific characteristics could violate the exclusion restriction condition. For example, the interaction of the state-level relevant market area and the amount of developable land in a market might be systematically correlated with state-level macroeconomic variation in a way that would confound the causal interpretation of my findings. To address this concern, I provide some evidence to support the validity of the instrument.

State-level outcomes: First, I test in the reduced form if the instrument predicts state-level macroeconomic outcomes by estimating Equation 3.7.[16]

$$Y_{s,t} = \alpha + \beta_1 \textit{Potential Number of Dealers}_s + \delta_{rma} + \delta_{dl} + \delta_t + \epsilon \tag{1.8}$$

where the outcome variable (Y) is a list of state-level macroeconomic variables. Observations are at the state and quarter level. $\textit{Potential Number of Dealers}_s$ is the

[15] I assume no overlaps between the potential number of dealerships. This leads to the underestimation of the potential number of dealerships.

[16] To be consistent with the weighting scheme of my specifications throughout the paper, I also estimate this equation at the loan level after including lender fixed effects, vehicle fixed effects, income fixed effects, and credit score fixed effects. The results are quantitatively and qualitatively robust.

potential number of dealers selling new cars in state s. I also include relevant market area fixed effects (δ_{rma}) and developable land quartile fixed effects (δ_{dl}) to ensure that the coefficient of interest (β_1) is estimated from the within variation in the developable land quartiles, similar in spirit to Mian and Sufi 2011. I also include time fixed effects (δ_t) to control for time trends.

Table 1.3 reports the coefficient of interest (β_1) from Equation 3.7. In Columns (1) and (2), I find no evidence that the instrument is correlated with state-level GDP per capita and income per capita, suggesting that the instrument does not capture the state-level variation in the economic condition. In Column (3), I find no evidence that the instrument is correlated with the state-level variation in the regional price parity across states. This suggests that my instrumental variable does not capture the differences in price levels across states. In Column (4), I find that the instrument does not statistically or economically predict state-level unemployment rate. In Column (5), I find that the instrument does not statistically or economically predict the fraction of people with a bachelor's degree as a proxy for consumers financial sophistication. In Column (6), I find no evidence that the instrument predicts state-level variation in access to the internet as a proxy for consumers search cost. Finally, in Column (7), I also find that the instrument is not correlated with the state-level sales tax.

Selection on unobservables: Despite finding no evidence that the instrument is correlated with the state-level macroeconomic outcomes, it is still possible that states differ on some unobservable characteristics that may explain my findings. To address this concern, I provide out-of-sample evidence to support the validity of exclusion restriction condition. I exploit a unique feature of my instrumental variable, in which it affects only the number of dealerships selling new cars in a market. In other words, as a placebo test, I test if the instrument is correlated with (1) the number of dealerships that exclusively sell used cars, or (2) the number of banks in a market. I estimate Equation 1.9:

$$Y_{v,l,c,i,s,t} = \alpha + \beta_1 Pot.Numb.Dealers_s + \delta_{rma} + \delta_{dl} + \delta_{l,t} + \delta_{v,t} + \delta_{i,t} + \delta_{c,t} + \epsilon \quad (1.9)$$

where $Y_{v,l,c,i,s,t}$ is the number of dealerships selling only used vehicles or the number of banks in a given market. As an additional robustness check, I also re-estimate this equation at the state and quarter level and show that the results are robust. In Table 1.4 Column (1), I find that the instrument does not statistically or economically predict the number of car dealerships that exclusively sell used cars. In Column (2), I find that the instrument does not statistically or economically predict the number of banks. These results suggest that the instrument is not correlated with the general demand for vehicles or auto loans across states. This also outlines my next tests, in which the instrument should be irrelevant in predicting the variation in vehicle prices and loan markups for old used vehicles. Columns (3) to (4) in Table 1.4 show that the instrument becomes statistically and economically insignificant in predicting vehicle prices for older used cars. Columns (5) to (6) show the results for loan markups. These results suggest that the instrument is less likely to be correlated with unobservable differences across markets. Overall, my findings suggest that the instrument has no direct effect on vehicle prices and loan markups other than through the number of dealerships selling new cars.

1.4.2 Baseline results

In this section, I first present the instrumental variable results, then I do a back-of-the-envelope calculation to measure the aggregate cost.

Local competition among auto dealers and prices

To begin, I use the instrument outlined in Section 1.2.2 to explore the causal effect of local competition among auto dealers on the joint pricing of cars and car loans by simultaneously estimating Equations 3.4 and 3.6. Table 1.5 Columns (1) and (2) present my main findings. The key explanatory variable of interest is the instrumented number of dealerships selling new cars.

In Column (1), I find that a one unit increase in the instrumented number of dealerships selling new vehicles is associated with a \$88.6 decrease in vehicle prices. This effect is statistically and economically important given that the net profit margin on new vehicles is only 2-5%. To be more specific, this translates to a 7.1-17.7% decrease in auto dealers net profit margins.[17] In Column (2), I find that a one unit increase in the instrumented number of dealerships selling new vehicles is associated with a 16.8 basis point increase in loan markups. This effect is statistically and economically important, the unconditional average markup in my estimation sample is 236 basis points. In other words, a one unit increase in the number of dealerships is associated with a 7.2% increase in loan markups. This increase in loan markups offsets the decline in vehicle prices induced by increased competition. In Column (3), I show that increased competition does not statistically and economically affect monthly payment, suggesting that auto dealers recover their losses on vehicle prices by charging higher prices on loan markups such that consumers' monthly payments do not change.

1.4.3 Economic channel: Monthly payment targeting

In this section, I examine if monthly payment targeting is the channel through which local competition among auto dealers affects the joint pricing of cars and car loans. Argyle, Nadauld, and Palmer 2020a find that many consumers in the auto loan market target specific monthly payment amounts (e.g., \$200, \$300, and \$400 per month). If increased competition leads to lower vehicle prices, then the corresponding monthly payment amounts mechanically decrease too. Auto dealers may adjust auto loan contract terms such that consumers' monthly payments are the same across markets.

To investigate this prediction in my sample, I first show bunching around salient monthly payments. Figure 1.4 plots the McCrary bunching test of normalized monthly

[17] The average price for new vehicles in my sample is about \$25,000. Thus, the average net profit margin is \$500-\$1250.

payments around hundred dollar increments from \$100 to \$600. This is consistent with the monthly payment targeting bias in which consumers in the auto loan market target specific monthly payments. Next, I find no evidence that increased competition affects monthly payments. Table 1.5 Column (3) shows that increased competition does not statistically or economically affect monthly payments. Furthermore, I find that increased competition has no effect on other contract terms. Table 1.6 Columns (1) through (3) show that the effect of competition on other contract terms is economically insignificant. Table 1.6 Columns (4) through (5) show that increased competition has little to no effect on the ex-ante measures of quality of borrowers. Column (6) complements this analysis by showing that the competition has no effect on the 24-month default rate, suggesting that increased competition does not lead to a change in the composition of borrowers.

I also show that the results are stronger among less financially savvy consumers. Table 1.7 shows that the monthly payment targeting bias is stronger among low credit score borrowers. Table 1.8 repeats the same analysis across income and shows that low-income borrowers are suffering more from this bias.

1.4.4 Alternative explanations

In this section, I discuss other potential explanations for my findings. I can rule out adverse selection, costly lender screening, moral hazard, search costs as explanations for my findings.

Adverse selection

A plausible explanation for my findings is adverse selection. Intuitively, if auto dealers charge higher prices on loan markups, price-sensitive borrowers may walk away, resulting in a change in the composition of borrowers. This explanation also implies that increased competition among auto dealers should shrink the pool of borrowers. This, however, is in contrast with the positive relation between the local competition among auto dealers and number of loan originations, suggesting that the adverse selection explanation is less likely to be the main channel through which the competition among auto dealers affects the joint pricing of cars and car loans.

Costly lender screening

Another plausible explanation for my findings is the variation in costly lender screening across markets. Yannelis and Zhang 2021 find that in more competitive markets, lenders have less incentives to monitor borrowers through investing in screening technologies, which can lead to a riskier pool of borrowers and higher interest rates. The intuition of their paper is largely irrelevant in my setting for several reasons.

First, Yannelis and Zhang 2021 investigates the competition effect in the direct auto loan market, in which auto lenders originate loans directly to borrowers. I, however, test the effect of competition among auto dealers in the indirect auto loan market, which is at the national level. Second, Yannelis and Zhang 2021 documents that the positive relation between the level of competition and interest rate is concentrated among subprime borrowers with a credit score of 620 or below. I, however, restrict my estimation sample to prime borrowers with a credit score of 620 or above. Third, all my analyses are within the lender level, suggesting that the lenders' screening costs are irrelevant in my setting. All in all, my findings suggest that the costly lender screening cannot be the channel through which the local competition among auto dealers affects loan markups.

Moral hazard

A change in consumers' behavior could appear to explain the positive relation between the number of dealers selling new cars and loan markups. Borrowers with higher loan markups are more likely to default than borrowers with low loan markups. The implied assumption of this explanation is that the existing borrowers change their behavior through stopping their payments. This assumption is, however, in contrast with my results that increased competition does not change both ex-ante and ex-post measures of the quality of borrowers, suggesting that the moral hazard channel is less likely to be the main channel driving my results.

Search costs

Another plausible explanation for my findings is a change in demand induced by a change in consumers' search cost. An increase in the number of auto dealers may reduce consumers' search cost, leading to higher demand for vehicles. This increases both the number of loan originations and vehicle prices and their financing. This intuition, however, is largely incorrect since in all of my specifications, I control for consumers search costs by adding relevant market area fixed effects. This ensures that my findings are not driven by a change in consumers' search costs.

1.4.5 Robustness

Intra-brand vs. inter-brand competition

Throughout this paper, I estimate the effect of intra-brand local competition of new car dealers on vehicle prices and loan markups for at least two reasons. First, the state automobile franchise laws directly affect new car dealerships selling the same line-make vehicles. Second, investigating the intra-brand competition for new vehicles ensures that the variation in vehicle prices and loan markup is not driven by differentiated products. After establishing my main findings, a natural question is whether the local competition of auto dealers is limited to the same line-make vehicles or whether it affects vehicles across different models or makes. To answer this question, I estimate the effect across (1) vehicle body types, and (2) car value bins.

To begin, I first estimate the effect of local competition among auto dealers after adding vehicle body types fixed effects. I define vehicle body types as sedan, coupe, sports car, station wagon, hatchback, convertible, sport-utility vehicle (SUV), minivan, and pickup truck. Table A.1 Panel A shows that the results are consistent with my main results, suggesting that my results are not exclusive to intra-brand local competition of dealerships selling new cars.

Next, I estimate the effect of local competition among auto dealers on price of cars and car loans after adding car value fixed effects. I define car value bins as car value quartiles. Consistent with the prior results, Table A.1 Panel B shows that increased competition leads to lower vehicle prices, higher loan markups, and higher default rates.

Standard errors

In this section, I test whether my results are robust to different clustering schemes (Cameron and Miller 2015). In Table A.2, I re-estimate my main specification clustering standard errors at different levels, including state-lender, lender, make-model-model year, and year quarter levels. Overall, I find that my findings do not change when using different clustering schemes.

Alternative bins

As a robustness check, I also re-estimate my main specification using different bins. The purpose of these tests is to examine whether my results are robust to different bin sizes. I test my main findings across four different bin sizes. In *Bin size 1*, I calculate loan markups while conditioning on loan contract terms such as loan to value ratio and loan maturity. Column (2) reports the results for cells with at least two observations. In *Bin size 2*, I test if my findings are still robust if I apply tighter filters such as 5-point credit bins and $10,000 income bins. Columns (3) and (4) show that the results are robust to tighter filters. Column (4) reports the results for cells with at least two observations. In *Bin size 3*, I investigate if my results are robust if I apply more generous filters such as 10-point credit bins and $25,000 income bins. In Columns (5) and (6), I show that the results are robust. In *Bin size 4*, I explore if my findings are robust if I apply more generous filters. In Columns (7) and (8), I show that the results are robust if I calculate loan markups using 50-credit bins, $50,000 income bins, and semi annual bins. Overall, my findings do not change when I use different bin sizes.

Sample filters

Next, I show that my sample filters do not affect my results. In Table A.4, I re-estimate my main specification after adjusting the filters applied in Section 1.3. In Columns (1) through (3), I find that the results are robust when I relax the credit score thresholds. Column (4) shows that my results are robust when I relax the income threshold. Columns (5) shows that my findings are robust if I limit my sample to non-zero markup loans. Columns (6) shows that my findings are robust if I control for more granular developable land fixed effects. Overall, Table A.4 shows that adjusting for sample filters does not change my main findings.

Measurement error

One may be concerned that the loan markup measurement error is likely to be correlated with the market size in a way that confound the causal interpretation of my point estimate. To address this concern, in Table A.5, I show that the results are robust for cells with at least 5, 20 or 30 observations.

One concern could be that estimated loan markups are a very noisy proxy of actual loan markups. To address this concern, in Figure A.1, I show the kernel density distribution of both my sample and a sample of subvented loans. This result shows that the majority of estimated loan markups for subvented loans are zero, consistent with the idea that auto dealers are not allowed to add a loan markup on top of a lender's baseline interest rate. This suggest that the estimated loan markup is a reliable proxy for the actual loan markup. In Table A.6, I also show that the instrument does not statistically or economically predict the likelihood of subvented loans to address concerns regarding automakers' selling strategies across markets.

1.5 Conclusion

In this paper, I provide empirical evidence of the causal effect of local competition among auto dealers on the joint pricing of cars and car loans. Similar in spirit to Mian and Sufi 2011, I construct an instrumental variable based on variation in the number of dealers imposed by predetermined geographic features of a market and state-level franchise laws. After supporting the validity of my instrumental variable, I find that increased competition leads auto dealers to decrease vehicle prices to attract consumers and to charge higher prices on loan markups. By doing so, auto dealers offset about 75 percent of their losses on vehicle prices. I also provide evidence that this price adjustment at the intensive margin comes from the monthly payment targeting channel. My findings support that sophisticated sellers such as auto dealers exploit behavioral biases of consumers to maximize their profits.

Table 1.1: Descriptive statistics

	Mean	SD	P10	P25	P50	P75	P90
Coobligor (0/1)	0.31	0.46	0.00	0.00	0.00	1.00	1.00
Monthly Payment	465	167	275	352	446	559	674
Credit Score	754	57	675	713	756	803	830
VehiclePrice	24974	6570	17645	20160	23678	29007	34222
Loan Size	27250	10093	15212	20223	26090	33251	40748
Loan Term	68	9	61	62	73	76	77
Income	80412	42420	36000	48996	71477	99996	139574
Loan to value (%)	89.63	24.87	54.73	73.78	91.74	107.53	120.81
Interest rate (%)	4.49	2.02	2.64	3.14	3.99	5.25	6.90
24-month default (%)	0.86	9.23	0.00	0.00	0.00	0.00	0.00
Loan markup (%)	2.36	2.20	0.00	0.31	1.99	3.84	5.19
Number of dealers	3.62	2.16	1.62	1.84	2.76	5.43	8.03
Potential number of dealers	8.13	4.69	4.61	4.61	6.83	12.99	12.99
Developable land (sq. miles)	1998	445	1449	1449	1958	2422	2612
RMA (miles)	9.60	2.08	8.00	8.00	10.00	10.00	10.00
Number of used dealers	24.12	10.75	14.17	14.47	19.02	34.40	41.41
Number of banks	121.77	68.94	41.78	79.08	87.53	187.94	187.94
Internet access (%)	85.20	3.15	80.30	83.70	84.60	89.10	89.10
Personal income per capita	50409	7545	42162	44629	47057	58456	60344
Regional price parities	99.03	7.63	90.29	93.24	95.60	109.75	109.76
Unemployment rate (%)	4.32	0.50	3.90	3.90	4.40	4.80	4.90
PPL with bachelor's degree (%)	32.54	3.77	26.34	30.22	32.50	35.00	35.75
GDP per capita	58.90	10.11	45.00	53.70	56.06	68.61	71.53
Sales tax (%)	5.32	1.92	2.90	3.00	6.25	7.25	7.25

NOTE.—This table describes our sample of 91,405 auto loans originated by indirect lenders from 2017 to 2019. I also require that the loan passes other data quality filters. Descriptive statistics are as of the loan origination date.

Table 1.2: Validity of the instrument: Relevance condition

	(1) Num.dealers	(2) Num.dealers	(3) Num.dealers	(4) VehiclePrice	(5) LoanMarkup	(6) MonthlyPayment
Pot.num.dealers	1.2788***	1.2244***	1.2196***	-108.0467**	0.2048***	-1.4310
	(4.04)	(4.44)	(4.44)	(-1.99)	(5.90)	(-0.57)
RMA FE	Yes	Yes	Yes	Yes	Yes	Yes
Developable land FE	Yes	Yes	Yes	Yes	Yes	Yes
Vehicle×Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Lender×Time FE		Yes	Yes	Yes	Yes	Yes
Credit Score×Time FE			Yes	Yes	Yes	Yes
Income×Time FE			Yes	Yes	Yes	Yes
R^2	0.799	0.817	0.818	0.769	0.574	0.426
Observations	91,307	91,307	91,307	91,307	91,307	91,307
F-statistics	16.299	19.679	19.740			

NOTE.—Columns (1) through (3) report the results from the first-stage regressions (Equation 3.4). The dependent variable is the number of dealerships selling new cars in a market. Columns (4) and (5) report the results from the reduced form regressions. In Column (4), the dependent variable is the vehicle prices. In Columna (5) and (6), the dependent variable is the loan markups and monthly payments. The income fixed effects ($\delta_{i,t}$) are defined as $50,000 income bins and the credit score fixed effects ($\delta_{c,t}$) are defined as a 25-point credit bins. Vehicle fixed effects ($\delta_{v,t}$) refer to vehicle make-model-model year combinations. The developable land fixed effects (δ_{dl}) refer to developable land quartiles. The rma fixed effects (δ_{rma}) refers to the relevant market area defined under state franchise laws. t-statistics, presented below the coefficient estimates, are calculated by clustering at the state level.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

Table 1.3: Validity of the instrument and observables: State-level macroeconomic outcomes

	(1) Log(GDP per capita)	(2) Log(Income per capita)	(3) Log(Regional price parity)	(4) Unemployment rate	(5) Bachelor degree	(6) Internet access	(7) Sales tax
Pot.num.dealers	0.0311	0.0375	0.0097	0.0867	0.7809	0.0192	-0.0671
	(0.71)	(1.02)	(0.64)	(0.58)	(0.67)	(0.02)	(-0.13)
RMA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Developable land FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.430	0.462	0.448	0.342	0.342	0.267	0.166
Observations	270	270	270	270	270	270	270

NOTE.—This Table reports the results for the state-level macroeconomic outcomes. The coefficient of interest is estimated at the state-quarter level. The income fixed effects ($\delta_{i,t}$) are defined as \$50,000 income bins and the credit score fixed effects ($\delta_{c,t}$) are defined as a 25-point credit bins. Vehicle fixed effects ($\delta_{v,t}$) refer to vehicle make-model-model year combinations. The developable land fixed effects (δ_{dl}) refer to developable land quartiles. The rma fixed effects (δ_{rma}) refers to the relevant market area defined under state franchise laws. t-statistics, presented below the coefficient estimates, are calculated by clustering at the state level.

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

Table 1.4: Validity of the instrument and unobservables (placebo tests)

	(1) Num. used.dealers	(2) Num. banks	(3) < 4years old Vehicle price	(4) > 4years old Vehicle price	(5) < 4years old Loan Markup	(6) > 4years old LoanMarkup
Pot.num.dealers	-1.0606	-6.9239	-56.4408	-18.3877	0.0233**	0.0079
	(-0.56)	(-0.62)	(-1.06)	(-0.29)	(2.01)	(1.24)
RMA FE	Yes	Yes	Yes	Yes	Yes	Yes
Developable land FE	Yes	Yes	Yes	Yes	Yes	Yes
Lender×Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Vehicle×Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Credit Score×Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Income×Time FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.888	0.808	0.817	0.788	0.078	0.082
Observations	91,307	91,307	78,700	47,858	78,700	47,858

NOTE.—This Table reports the results for the selection on unobservables. In Column (1), the dependent variable is the number of dealerships selling exclusively used cars in a market. In Column (2), the dependent variable is the number of banks in a market. In Columns (3) and (4), the dependent variable is vehicle prices and in Columns (5) and (6), the dependent variable is loan markups. Columns (3) and (5) report the results for used vehicles that are less than 4 years old. Columns (4) and (6) report the results for used vehicles that are more than 4 years old. The income fixed effects ($\delta_{i,t}$) are defined as $50,000 income bins and the credit score fixed effects ($\delta_{c,t}$) are defined as a 25-point credit bins. Vehicle fixed effects ($\delta_{v,t}$) refer to vehicle make-model-model year combinations. The developable land fixed effects (δ_{dl}) refer to developable land quartiles. The rma fixed effects (δ_{rma}) refers to the relevant market area defined under state franchise laws. t-statistics, presented below the coefficient estimates, are calculated by clustering at the state level.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

Table 1.5: Price effect of Local competition among auto dealers

	(1) Vehicle price	(2) Loan markup	(3) MonthlyPayment
Number of dealers	-88.5898**	0.1679***	-1.1733
	(-2.07)	(5.45)	(-0.56)
RMA FE	Yes	Yes	Yes
Developable land FE	Yes	Yes	Yes
Lender×Time FE	Yes	Yes	Yes
Vehicle×Time FE	Yes	Yes	Yes
Credit Score×Time FE	Yes	Yes	Yes
Income×Time FE	Yes	Yes	Yes
Observations	91,307	91,307	91,307

NOTE.—This Table reports the effect of local competition among auto dealers on the joint pricing of cars and car loans by simultaneously estimating Equations 3.4 and 3.6. In Columns (1) and (2), the dependent variable is vehicle prices and loan markups respectively. In Column (3), the dependent variable is monthly payments. The income fixed effects ($\delta_{i,t}$) are defined as \$50,000 income bins and the credit score fixed effects ($\delta_{c,t}$) are defined as a 25-point credit bins. Vehicle fixed effects ($\delta_{v,t}$) refer to vehicle make-model-model year combinations. The developable land fixed effects (δ_{dl}) refer to developable land quartiles. The rma fixed effects (δ_{rma}) refers to the relevant market area defined under state franchise laws. t-statistics, presented below the coefficient estimates, are calculated by clustering at the state level.

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

Table 1.6: Other contract terms and composition of borrowers

	(1) LoanAmount	(2) Maturity	(3) LtV	(4) Income	(5) CreditScore	(6) 24-month default
Num.dealers	-0.0067	-0.0035*	-0.2042	0.0170**	-0.0028*	0.0246
	(-1.19)	(-1.93)	(-0.38)	(2.12)	(-1.93)	(0.60)
RMA FE	Yes	Yes	Yes	Yes	Yes	Yes
Developable land FE	Yes	Yes	Yes	Yes	Yes	Yes
Lender×Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Vehicle×Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Credit Score×Time FE	Yes	Yes	Yes	Yes		Yes
Income×Time FE	Yes	Yes	Yes		Yes	Yes
Observations	91,307	91,307	91,307	91,307	91,307	66,659

NOTE.—This Table reports the results for other contract terms and borrower characteristics. In Columns (1) through (3), the dependent variable is the logarithm of the loan amount, the logarithm of maturity, and the loan-to-value ratio at the time of origination. In Columns (4) and (5), the dependent variable is the logarithm of income and the logarithm of credit score. In Column (6), the dependent variable is the 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 income fixed effects ($\delta_{i,t}$) are defined as \$50,000 income bins and the credit score fixed effects ($\delta_{c,t}$) are defined as a 25-point credit bins. Vehicle fixed effects ($\delta_{v,t}$) refer to vehicle make-model-model year combinations. The developable land fixed effects (δ_{dl}) refer to developable land quartiles. The rma fixed effects (δ_{rma}) refers to the relevant market area defined under state franchise laws. t-statistics, presented below the coefficient estimates, are calculated by clustering at the state level.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

Table 1.7: Cross-sectional test: Credit scores

	(1) Vehicle price	(2) Loan markups	(3) Monthly payment	(4) 24-month default
Number of dealers	-103.5003*	0.0985***	-1.2041	-0.0037
	(-1.80)	(3.84)	(-0.47)	(-0.13)
Number of dealers×Low credit score	47.9191	0.1536***	1.6972	0.0750
	(1.04)	(5.64)	(0.92)	(1.52)
RMA FE	Yes	Yes	Yes	Yes
Developable land FE	Yes	Yes	Yes	Yes
Lender×Time FE	Yes	Yes	Yes	Yes
Vehicle×Time FE	Yes	Yes	Yes	Yes
Income×Time FE	Yes	Yes	Yes	Yes
Observations	91,220	91,220	91,220	66,590

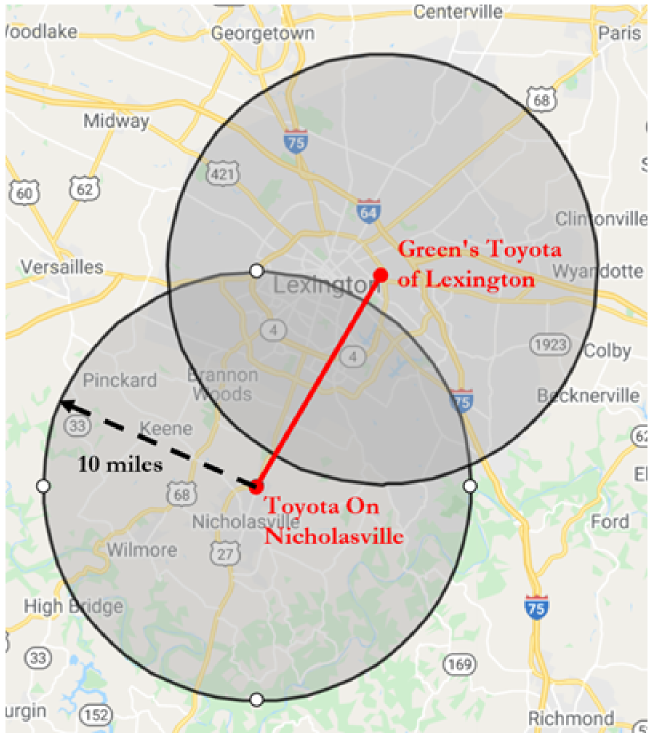
NOTE.—This Table reports the effect of local competition among auto dealers on the joint pricing of cars and car loans across credit scores. *Low credit score* equals one for borrowers with a credit score below median and zero otherwise. In Columns (1) and (2), the dependent variable is vehicle prices and loan markups respectively. In Columns (3) and (4), the dependent variable is monthly payments and the 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 income fixed effects ($\delta_{i,t}$) are defined as $50,000 income bins. Vehicle fixed effects ($\delta_{v,t}$) refer to vehicle make-model-model year combinations. The developable land fixed effects (δ_{dl}) refer to developable land quartiles. The rma fixed effects (δ_{rma}) refers to the relevant market area defined under state franchise laws. t-statistics, presented below the coefficient estimates, are calculated by clustering at the state level.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

Table 1.8: Cross-sectional test: Income

	(1) Vehicle price	(2) Loan markups	(3) Monthly payment	(4) 24-month default
Number of dealers	-86.9999*	0.1318***	-1.9686	0.0159
	(-1.79)	(4.15)	(-0.75)	(0.35)
Number of dealers×Low income	-2.0352	0.0654***	1.8102	0.0219
	(-0.09)	(4.39)	(1.01)	(0.44)
RMA FE	Yes	Yes	Yes	Yes
Developable land FE	Yes	Yes	Yes	Yes
Lender×Time FE	Yes	Yes	Yes	Yes
Vehicle×Time FE	Yes	Yes	Yes	Yes
Credit score×Time FE	Yes	Yes	Yes	Yes
Observations	91,195	91,195	91,195	66,555

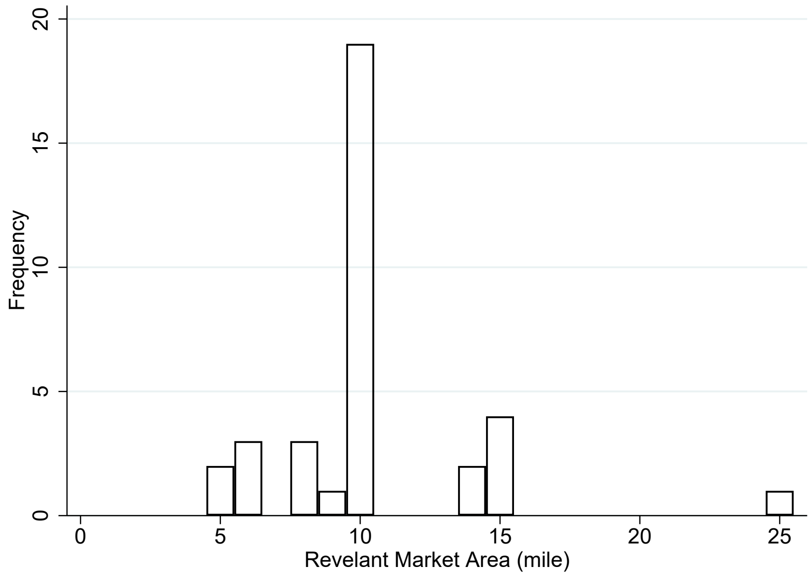
NOTE.—This Table reports the effect of local competition among auto dealers on the joint pricing of cars and car loans across income. *Low income* equals one for borrowers with an income below the median and zero otherwise.In Columns (1) and (2), the dependent variable is vehicle prices and loan markups respectively. In Columns (3) and (4), the dependent variable is monthly payments and the 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 credit score fixed effects ($\delta_{c,t}$) are defined as a 25-point credit bins. Vehicle fixed effects ($\delta_{v,t}$) refer to vehicle make-model-model year combinations. The developable land fixed effects (δ_{dl}) refer to developable land quartiles. The rma fixed effects (δ_{rma}) refers to the relevant market area defined under state franchise laws. t-statistics, presented below the coefficient estimates, are calculated by clustering at the state level.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

Figure 1.1: Toyota dealerships in Lexington, KY.



NOTE.——This figure plots Toyota dealerships in Lexington, KY. The relevant market area in the state of Kentucky is 10 miles. Each circle corresponds to the relevant market area for each Toyota dealership.

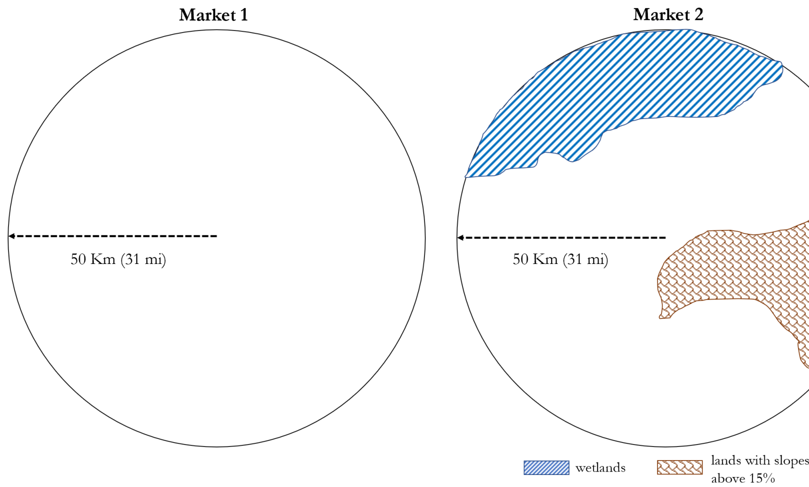
Figure 1.2: Distribution of relevant market areas across states



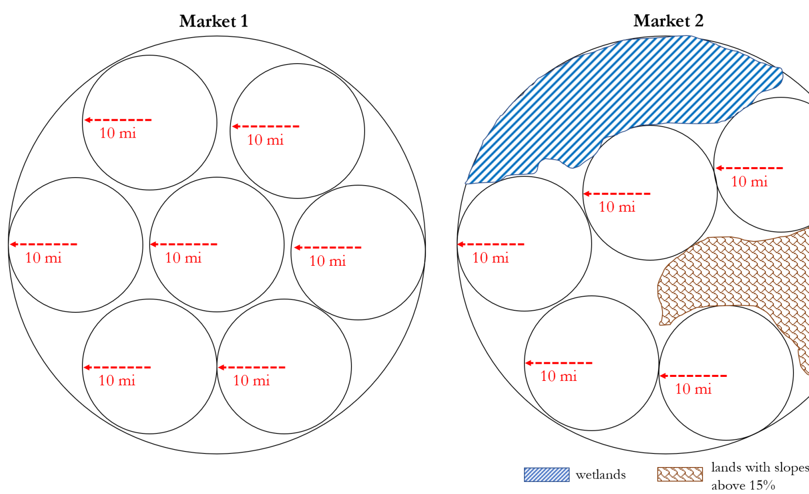
NOTE.——This figure plots the distribution of relevant market areas across states. The x-axis presents the radius of relevant market area in miles.

Figure 1.3: A hypothetical example

(a) Predetermined developable land

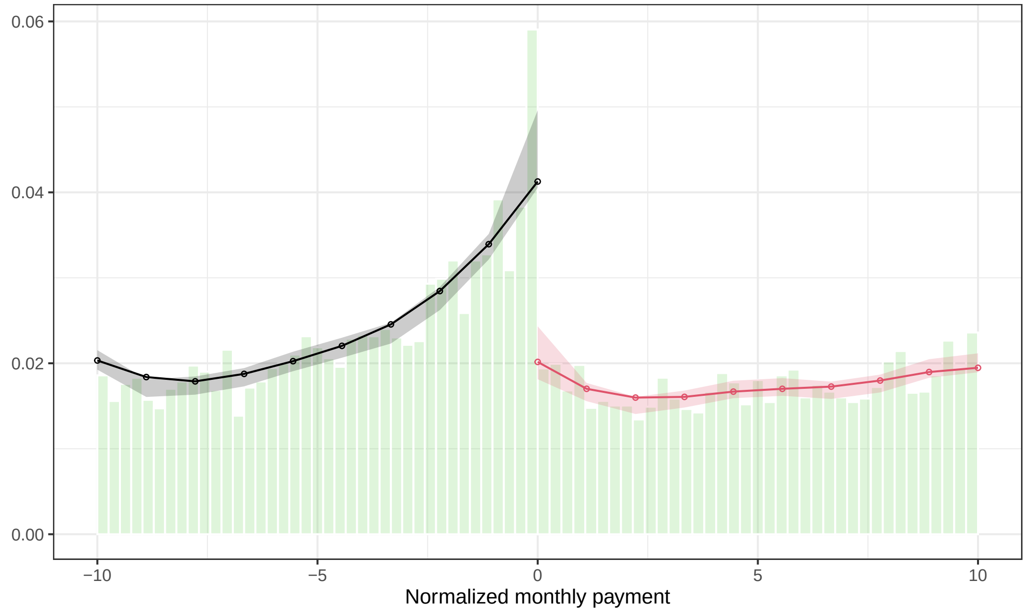


(b) Potential number of dealerships



NOTE.——This figure plots a hypothetical example to clarify the instrumental variable outlined in Section 1.2.2. Panel A presents the amount of available land in Market 1 and Market 2. The blue shaded area represents wetlands and the brown shaded area represents lands with slope above 15%. Panel B presents the maximum number of new car dealerships in each hypothetical market. The relevant market area (RMA) in each market is 10 miles.

Figure 1.4: Monthly payment distribution around salient cutoffs



NOTE.——This figure plots McCrary bunching tests of normalized monthly payments around hundred dollar increments from \$100 to \$600.

Chapter 2 Does Trade Policy Affect Consumer Credit? The Role of Captive Finance

2.1 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 auto 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 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. Moreover, 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 cost pass-through (much akin to Nakamura and Zerom 2010).

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 2.3.1, 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 2.4.5 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

[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 2.4.1, 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.

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 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 of the average household incomes, credit scores, or future default rates in 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 2020b). 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. 2020 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 2020a, 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 (Nakamura and Zerom 2010). 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

[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 2.4.5, we provide a more thorough comparison of our pass-through estimates to estimates from these literatures.

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. Indeed, the magnitude of our results suggests that captive finance companies play an important role in preserving the margins of manufacturing firms during periods of rising costs. Moreover, the role of captive finance lenders is not restricted to auto manufacturers. Numerous other durable goods firms – including Boeing, Caterpillar, and Polaris – also reported both higher input costs due to the metal tariffs as well as higher captive financing revenues in their annual reports during this period.[3] Our results also suggest that captive lenders allow manufacturers to shroud their price increases along margins where consumers are less price sensitive (Grunewald et al. 2020), consistent with the literature on shrouded attributes and add-on pricing (Ellison 2005; Gabaix and Laibson 2006; Brown, Hossain, and Morgan 2010b).

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 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

[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."

[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 B.2, we show our results hold for both used and new vehicles.

the strategic responses of integrated manufacturer-lenders will depend on the nature of the shock.

The remainder of the paper is organized as follows. Section 2.2 provides institutional background on the auto loan market and the 2018 Trump import tariffs. Section 2.3 describes the Regulation AB II data and presents our main sample. Section 2.4 documents the effect of the tariffs on captive auto loans, and Section 2.5 examines the channels driving our results. Section 2.6 concludes.

2.2 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.

2.2.1 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.

Captive lenders finance their operations using a combination of internal cash, unsecured debt, and securitizations. Around one-third of captive auto loans are se-

[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.

curitized 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 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 vehicle sales price is finalized, and 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. 2020 find that auto dealers solicit bids from 4.35 non-captive lenders, on average, for each financing transaction.

2.2.2 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 tariff options to boost domestic metal production. On March 1, 2018, President Trump followed Commerce's

[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.

[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. 2020. 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).

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 2.1). 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.

2.2.3 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 announcements.[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

[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.

[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.

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 2.4.5, 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 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 2.2 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

[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 B.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.

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. 2020, 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 2010b; 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).

2.2.4 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 2.4 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 2.4 and 2.5, 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 2.4.1, 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.

2.3 Data and sample selection

2.3.1 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 2020b) 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-captives.[15] During

[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...".

[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

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]

One drawback of the Regulation AB II data is that it does not include information on the vehicle sales price.[17] This prevents us from examining whether the tariffs had a differential price impact on new vehicles based on their source of financing, as well as from measuring the relative importance of pass-through on the loan and vehicle price margins. Although we are ultimately unable to address the issue of differential price impacts because of data limitations, we attempt to resolve the second concern using 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, which we can then compare to the impact of the tariffs on loan prices.

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 B.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 B.1 compares the features of their securitized loan pool to their entire 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 2.4.7, 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),

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.

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.

2.3.2 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 2020a). 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 3.5, 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 3.5, 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 2.3 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

[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 2.4, 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.

[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.

non-captive lenders (Ally Bank, CarMax, Fifth Third, Santander, USAA, and World Omni) make up the remaining 39 percent.[20] Table 2.1 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 2.1 compare loans from captive (i.e., treated) lenders to non-captive (i.e., control) lenders. For these comparisons, we restrict the sample to loans originated prior to the treatment date (982,095 loans). There are several noticeable differences between loans from captive and non-captive lenders. Captive loans have higher average initial principals (\$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 2.4 removes most of them through the inclusion of various lender, vehicle, and borrower fixed effects. Indeed, as shown later in Figures 2.4 and 2.5, 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 2.2 reports coefficient estimates from the following regression model:

$$y_{i,t} = \alpha + \Gamma \cdot \text{Treated}_l + \delta_{v,t} + \delta_{s,t} + \delta_{w,t} + \delta_{c,t} + \varepsilon_{i,t}, \tag{2.1}$$

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

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

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 2.2 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. 2020 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 2.4.6.

2.4 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.

2.4.1 Interest rates

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

$$y_{i,t} = \alpha + \Gamma \cdot \text{Treated}_l \cdot \text{Post}_t + \delta_l + \delta_{v,t} + \delta_{s,t} + \delta_{w,t} + \delta_{c,t} + \varepsilon_{i,t}, \tag{2.2}$$

where the outcome variable is the interest rate of loan i originated in quarter t. As in Equation 2.1, 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 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 2.3 reports the coefficient estimates from Equation 2.2. Relative

[21]We emphasize that the inclusion of vehicle make-model-condition fixed effects in Equation 2.2 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 2020b). Later, in Section 2.4.6, 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 2020b.

[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. 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.

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.

Given that the tariffs became more binding over time, the pooled coefficient estimate in Table 2.3 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:

$$y_{i,t} = \alpha + \sum_{\tau=-4}^{4} \Gamma_\tau \cdot \text{Treated}_l \cdot \text{D}_{t,\tau} + \delta_l + \delta_{v,t} + \delta_{s,t} + \delta_{w,t} + \delta_{c,t} + \varepsilon_{i,t}, \tag{2.3}$$

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 2.4. 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 2.3. 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.

Spillover effects

As mentioned in Section 2.2.4, captive lenders compete with non-captive lenders for auto loan originations. Hence, in response to a cost shock that forces captive lenders

[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 2020b.

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).[24]

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 × 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.

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 B.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 2020a, Argyle et al. 2021, Benneton, Mayordomo, and Paravisini 2022, and Argyle, Nadauld, and Palmer 2020b).

2.4.2 Non-price loan terms

Next, we examine how the metal tariffs impacted non-price loan terms. Panel A in Table 2.3 reports the coefficient estimates from Equation 2.2 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

[24] 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.

positive, we continue to find no material economic improvements in non-price loan terms for captive auto loans.

Figure 2.4 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 2020a 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 2020a 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. 2020).

2.4.3 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.[25]

[25]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,

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

$$y_{i,t} = \alpha + \Gamma \cdot \text{Treated}_l \cdot \text{Post}_t + \delta_l + \delta_{v,t} + \delta_{s,t} + \varepsilon_{i,t}, \tag{2.4}$$

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

Panel A in Table 2.4 reports the coefficient estimates from Equation 2.4. 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 2.3, but that it is also of the wrong sign.

Figure 2.5 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 2020a and Argyle, Nadauld, and Palmer 2020b 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. Finally, 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

along with an estimate of the effect on demand.

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.

2.4.4 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:

$$p_{i,t} = \alpha + \Gamma \cdot \text{Post}_t + \delta_{\tilde{v}} + \varepsilon_{i,t}, \tag{2.5}$$

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 2.5 reports the coefficient estimates from Equation 2.5. 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:

$$p_{i,t} = \alpha + \Gamma \cdot \text{New Vehicle}_i \cdot \text{Post}_t + \beta \cdot \text{New Vehicle}_i + \delta_{\tilde{v}} + \varepsilon_{i,t}, \tag{2.6}$$

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 2.5 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.[26]

2.4.5 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:

$$\rho(l) = \frac{F \cdot \Delta P}{N \cdot \Delta C}, \tag{2.7}$$

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

Given Equation 2.7, calculating the pass-through rate just involves plugging in values for ΔP, ΔC, and M. From Section 2.4.1, 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

[26] We also re-estimate Equation 2.2 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 2020b, this also suggests that captive borrowers are not substituting across trims within a given make-model in response to higher rates.

that the average increase in costs per vehicle is $\Delta C = \$295$.[27] Thus, the 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 **??** 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 2.4.4, 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.[28] To do so, we rearrange Equation 2.7 as follows:

$$F \cdot \Delta P = \rho(l) \cdot N \cdot \Delta C, \tag{2.8}$$

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 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

[27]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 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 of this cost increase from the popular press which are much larger than ours. However, these specific estimates often mistakenly refer to a hypothetical vehicle import tariff that was never enacted, and not the steel and aluminum tariffs that we focus on in our paper.

[28]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 2.4.1.

coffee ground prices of 0.38.[29] 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.

2.4.6 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:

$$y_{f,s,v,t} = \alpha + \Gamma \cdot \text{Treated}_f \cdot \text{Post}_t + \delta_f + \delta_{s,t} + \delta_{v,t} + \varepsilon_{f,s,v,t}, \tag{2.9}$$

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 vehicle make-model-condition v.[30] 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 2.6 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 2020a and -0.94 in Argyle, Nadauld, and Palmer 2020b. (In addition, Karlan and Zinman 2008 estimate

[29]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.

[30]To better account for the count-data structure of the number of loan originations, we also re-estimate Equation 2.9 using a Poisson model and report the results in Column 2 in Table 2.6 (Cohn, Liu, and Wardlaw 2021). 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 × state × vehicle level ($t = -15.17$) and using a bootstrap procedure ($t = -10.45$).

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 2.9 follows Benneton, Mayordomo, and Paravisini 2022, our results are also robust to different levels of aggregation. For instance, in Column 3 in Table 2.6, 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.[31] 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 2020b suggest that both margins should be active. Third, we note that the decline in loan originations that we document in Table 2.6 does not contradict the absence of borrower composition effects that we document in Table 2.4. Indeed, both Argyle, Nadauld, and Palmer 2020a and Argyle, Nadauld, and Palmer 2020b find that loan originations decline in response to higher offered interest rates, and that the decline in originations is not correlated with observable borrower characteristics or future default rates.

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 2020b 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 B.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.[32]

[31] Results for additional aggregations at the lender instead of the captive level are shown in Table B.3.

[32] As before, one reason that our results might differ from those in Argyle et al. 2021 and Argyle, Nadauld, and Palmer 2020b 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,

2.4.7 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.

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 2.2 after controlling for lender-specific measures of unsecured borrowing costs and their interactions with the treatment indicator. As shown in Table B.5, controlling for unsecured borrowing costs in a flexible manner does not overturn our results.

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).[33] One potential concern could be that the increase in captive interest rates in Table 2.3 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.[34] 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 B.6, we find a significant increase in interest rates for subvented captive auto loans, which are loans that dealers are not allowed to mark up (Grunewald

whereas we primarily examine the indirect auto loan market where the sequence of events is often the opposite. See Grunewald et al. 2020.

[33]The additional revenue from the mark-up is split between the dealer and the lender according to a prespecified formula. Grunewald et al. 2020 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.

[34]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. 2020 find that 78 percent of indirect auto loans from non-captive lenders are marked up.

et al. 2020).[35] 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.

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.[36] To test this alternative explanation, Figure B.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.[37] Two other pieces of evidence are also inconsistent with this alternative explanation. First, Figure 2.4 shows there is no reversal in the increase in captive interest rates during the post-treatment period. Second, Table 2.6 shows that captive loan origination volumes decreased, not increased, relative to non-captive loan origination volumes following the announcement of the tariffs.

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 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 2.5). Third, we find no differential changes in observable characteristics or default rates for captive borrowers (Table 2.4). Although we cannot entirely rule out

[35] 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).

[36] 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.

[37] 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 will be absorbed into our various time fixed effects.

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. 2020 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 2.3 shows no significant increase in loan maturities in response to the tariffs. Finally, our falsification test in Section 2.4.7 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.

Do borrowers undo the effects 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 2.4.5 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 B.7, we find no differential increase in the likelihood that captive loans are paid off within 12 or 24 months of their origination dates.

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 2.3.1. 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 B.8 reports the results. Inconsistent with securitization-related factors driving our results, we find no differential changes in the above variables following the tariffs.

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.[38] 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 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 2.2 across our two groups of captive lenders, where the control group consists of all non-captive loans. Table B.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 B.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.

2.4.8 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.

2.5 Economic channels

2.5.1 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. 2020 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

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

on demand.[39] 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. 2020, 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:

$$\begin{aligned} y_{i,t} = \alpha &+ \beta \cdot \text{Low income}_i \cdot \text{Treated}_l \cdot \text{Post}_t + \Gamma \cdot \text{Treated}_l \cdot \text{Post}_t \\ &+ \theta \cdot \text{Low income}_i \cdot \text{Treated}_l + \delta_l + \delta_{v,t} + \delta_{s,t} + \delta_{w,t} + \delta_{c,t} + \delta_{\text{Low income},t} + \varepsilon_{i,t}, \end{aligned} \tag{2.10}$$

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.[40] 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 2.7 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 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. 2020 find that consumers with lower credit scores are less sensitive to increases in loan prices. (See also Argyle, Nadauld, and Palmer 2020a.) Therefore, we repeat the above test using credit scores as an alternative measure of loan price sensitivities. Table 2.7 reports the coefficient estimates from Equation 2.10 after replacing Low income_i with the variable $\text{Low credit score}_i$ that is equal to one when loan i has a below-median credit score

[39] As mentioned in Grunewald et al. 2020, 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 2020a). 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.

[40] 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.

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 2.7 reports the coefficient estimates from Equation 2.10 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 2.7, 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 2.6 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.

2.5.2 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.[41] Within our setting, one of the most important factors to consider is that the tariffs affected the marginal costs of captive lenders but not non-captive lenders. For such a firm-specific cost shock, a wide range of models predicts that the pass-through rate will be increasing as the level of competition declines.[42]

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 2020b 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.[43] 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). Fi-

[41]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 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.

[42]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.

[43]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.

nally, we re-estimate Equation 2.10 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 2.8 reports the coefficient estimates from the model. Consistent with our predictions, we find 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 2.10 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 2.10 after setting Low competition$_s$ equal to one when state s is in the lowest decile. Columns 2 and 3 in Table 2.8 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.

2.6 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.

Table 2.1: 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 2.2: 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 2.1. 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 2.3: 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 2.2. 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 2.4: 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 2.4. 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 2.5: 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 2.5 in Columns 1 and 3 and Equation 2.6 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 2.6: 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 2.9. 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 2.7: 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	0.130**			0.000		
× Low income	(2.42)			(-0.24)		
Treated × Post		0.209*			0.000	
× Low credit score		(1.89)			(-0.82)	
Treated × Post			0.237*			0.000
× Low loan amount			(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

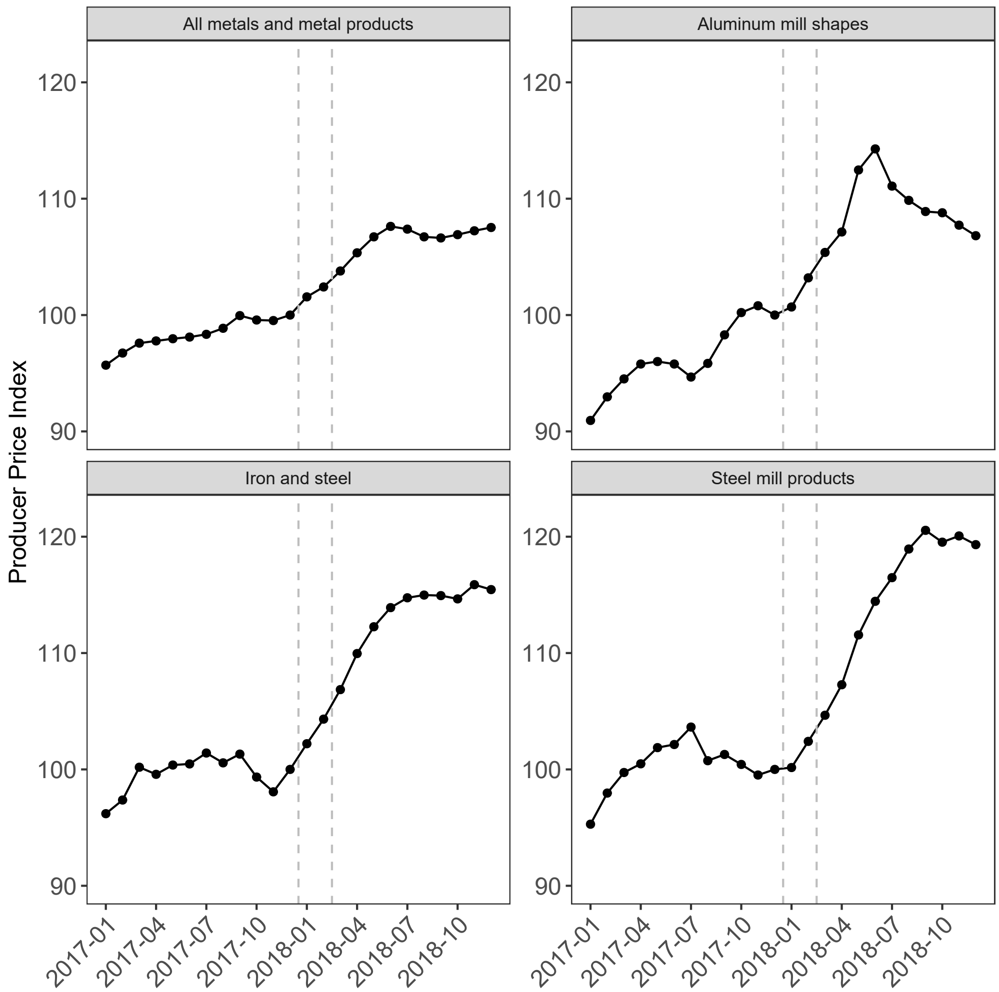
NOTE.—This table reports coefficient estimates from Equation 2.10. The dependent variable is either the interest rate or the 12-month default rate. A loan is considered to be in default if it is 90 or more days past due (including charge-offs and repossessions). The sample is restricted to auto loans 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 2.8: 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 × Low competition (median)	0.037**			0.001		
	(2.29)			(0.92)		
Treated × Post × Low competition (25th, 75th)		0.086**			0.001	
		(2.08)			(1.15)	
Treated × Post × Low competition (10th, 90th)			0.168**			0.001
			(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 2.10. 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 2.1: 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 2.2: Financial statement data from GM

Year ending December 2017 (pre-tariff):

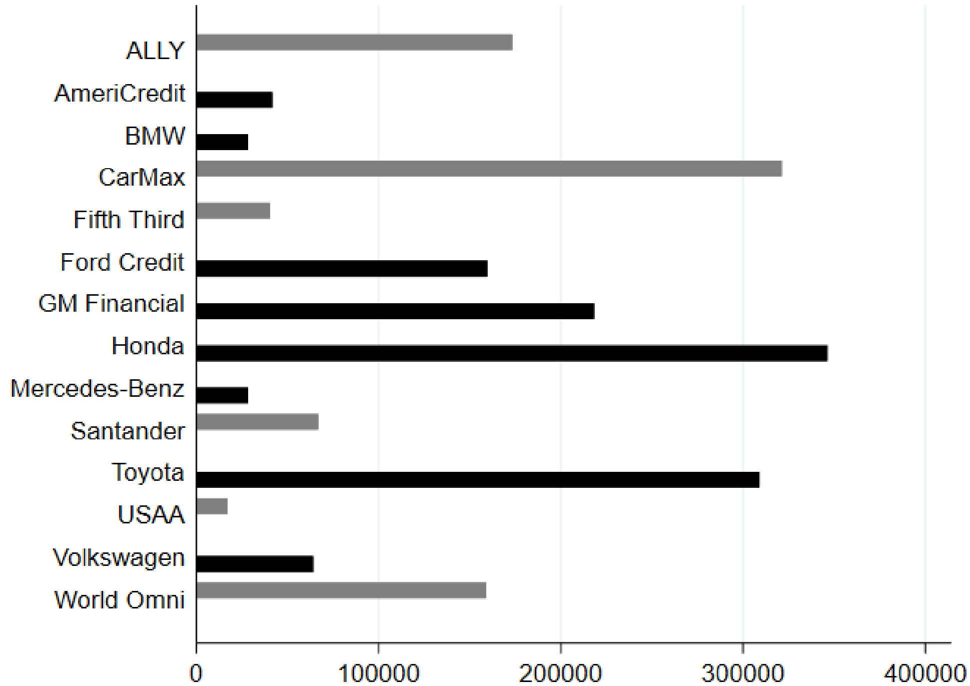
	Total automotive (1)	GM Financial (2)
Net sales and revenues	\$133,607	\$12,151
Earnings (loss) before interest and taxes	\$12,268	\$1,196

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

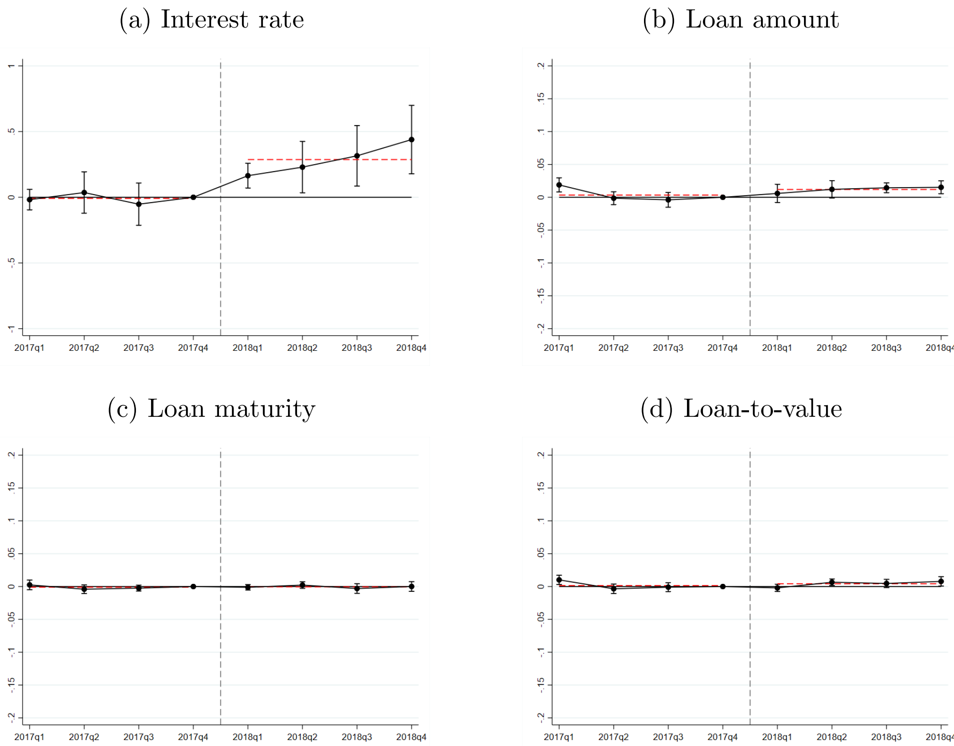
NOTE.——This figure displays GM's revenues and earnings in the year before the tariffs (2017) and the year of the tariffs (2018). Revenues and earnings are split between GM's vehicle sales segment (Total Automotive) and GM's captive financing segment (GM Financial).

Figure 2.3: 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.

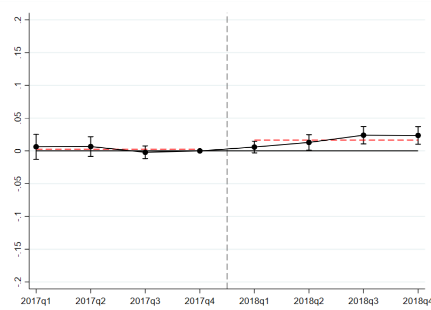
Figure 2.4: Response of captive auto loan terms



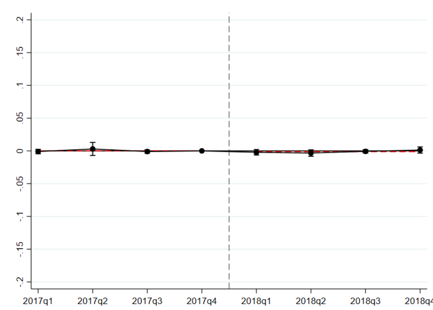
NOTE.——This figure plots coefficient estimates from Equation 2.3. The dependent variable is either the interest rate, log loan amount, log loan maturity, or loan-to-value ratio. The x-axis corresponds to the number of quarters from the treatment date. The quarter $\tau = -1$ is the reference quarter. 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 2.5: Response of captive borrower characteristics

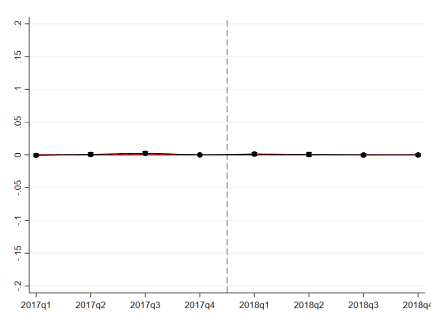
(a) Income



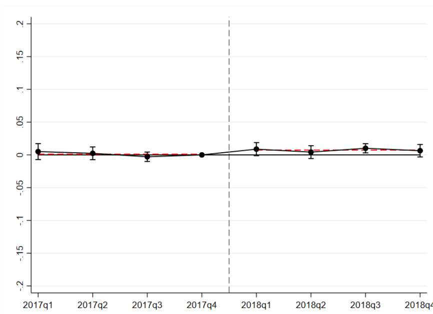
(b) Credit score



(c) 12-month default



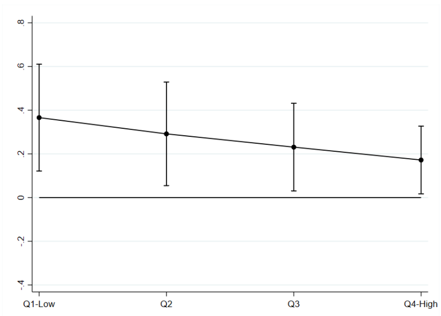
(d) Vehicle value



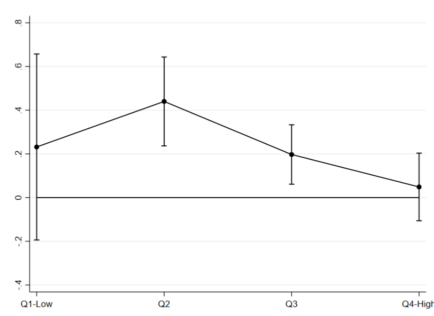
NOTE.——This figure plots coefficient estimates from Equation 2.3. The dependent variable is either the log household income, log credit score, 12-month default rate, or log vehicle value. A loan is considered to be in default when is 90 or more days past due (including charge-offs and repossessions). 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 2.6: Response of captive interest rates across borrower characteristics

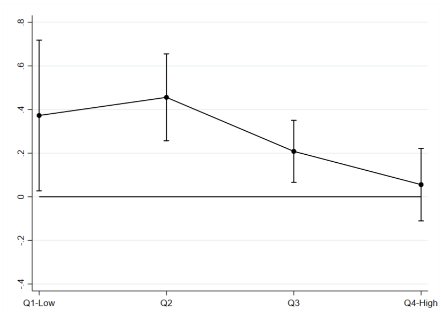
(a) Income



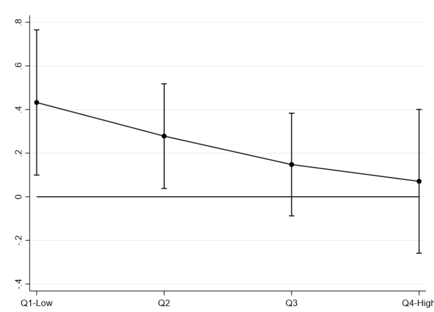
(b) Credit score



(c) Credit score (exlcluding AmeriCredit)



(d) Loan size



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

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

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.

Chapter 3 Third Party Quality Certification in the Market for Financial Advice

3.1 Introduction

Due to a widespread lack of financial literacy among many households (see e.g.,Lusardi and Mitchell 2011), the financial advisory industry in the United States is large[1] and essential to ensure the financial well-being of these households. This lack of financial literacy that creates the demand for the service also hinders the households' ability to search for financial service and ascertain the quality of the service they receive. Studies suggest households may justifiably distrust advisors as low quality and conflicted advice is prevalent.[2] For example, Egan, Matvos, and Seru 2019b find that a significant fraction of advisors engage in misconduct against their clients.

Attempts to police this industry face many challenges. Berk and Van Binsbergen 2022 point out that while FINRA is charged with regulating the industry, it is actually a trade group that represents the interest of producers (advisors). Firms are similarly conflicted and may help their employees obscure their past history of malfeasance Honigsberg and Jacob 2020. Thus, most regulatory disclosures are crude measures of overall quality (e.g., the assurance of the acquisition some minimum licensing or disclosure of wrongdoing) and while they may be of some use in predicting extreme poor quality Qureshi and Sokobin 2015, these measures do little to assist households in finding high quality advice as households lack the literacy necessary to understand them.

In markets in which buyers are uncertain about which product is best for them and sellers can face a reputational costs, the presence of a credible third party to certify quality can enhance the function of reputation. Empirically, however, the evidence across a number of domains (e.g. Moody's bond ratings, U.S News and World Reports ranking, TripAdvisor) is mixed about whether third party certification helps households make better choices and whether it encourages sellers to improve quality Dranove and Jin 2010 .

In this paper, we examine third party quality certification in the market for financial advice. Specifically, we document client and advisor behavior around changes in the Barron's Top Advisor lists.[3] The Barron's rankings are among the most presti-

[1] 617,549 registered representatives as of 2020 according to FINRA's website https://www.finra.org/newsroom/statistics

[2] The 2015 Edelman Trust Barometer ranked financial advisors among the less trustworthy professionals. Guiso, Sapienza, and Zingales 2008 and Georgarakos and Inderst 2014 provide evidence that many households lack of trust in financial advisors and thus do not seek out their services which has an economically large negative effect on stock market participation. Bergstresser, Chalmers, and Tufano 2009 find that mutual funds sold by brokers have inferior performance and higher fees. Hackethal, Inderst, and Meyer 2011 find that investors using an advisor incur significantly higher trading costs and tend to invest in products for which their advisor received sales targets.

[3] Since 2009, Barron's has published both a Top 100 and Top 1000 list. Barron's changed their ranking methodology to include 1,200 advisors in 2014. For brevity, we say Top 1000 to also mean

gious and respected rankings in the financial advisory industry in the United States and widely followed by investing households.[4] We find evidence that being named a top advisor increases both client assets and number of accounts. Importantly, we can exploit distinctive aspects of the creation of the ranking to separate the granting of certification from the underlying quality. This allows us a unique ability to see how certification itself can affect client and advisor behavior in this industry.

Since applying for Barron's rankings may be influenced by a number of financial advisor characteristics, the effect of Barron's certification of quality on client and advisor behaviors may be endogenous. A fundamental challenge of identifying the effect of these rankings on client behavior is the possibility that rank changes are correlated with changes in financial advisor quality that are observed by clients but unobserved by the econometrician.

To attempt to get a causal estimate of the Barron's certification of quality effect, we employ several different approaches: firm-level movement in and out of Barron's rankings using a matching technique for panel data; within-advisor movement between the Barron's Top 1000 list and Top 100 list; and, most importantly, by exploiting randomness due to discrete state-level based cutoffs in the ranking methodology.

In the first approach, we use firm-level data from the SEC's Form ADV to study the effect of third party quality certification in the market for financial advice. Collective reputation models, such as Tirole 1996, suggest that a group's reputation is an aggregate of individual reputations. Thus, individual recognition of credibility may confer reputation benefits to the wider firm. We identify advisory firms that have an owner who is named to the top Barron's advisor rankings in a particular year. Exploiting within-firm variation, we find evidence at the firm-level of a 9-10% increase in client assets and number of accounts when a firm has a Top Advisor owner. To improve inference, we follow Imai, Kim, and Wang 2018 and first match each treated observation with control observations from other firms in the same time period that have an identical treatment history (i.e. controlling for similar pre-trends in AUM) and find consistent results.

In the second approach, we examine within-advisor movement into the Top 100 ranking among Top 1000 advisors. Importantly, all advisors in this sample have applied to be ranked by Barron's eliminating any potential selection effect based on applying to Barron's.[5] The inclusion of advisor fixed effects helps to control for time-invariant confounding factors such as advisor quality. The inclusion of advisor fixed effects control for all time-invariant characteristics of advisors such as their general propensity to commit misconduct, education, and religious background. In certain specification, we also include state×year or firm×state×year fixed effects which help to control for time-varying confounding local factors such as local economic and regulatory conditions or time-varying confounding factors at the firm level such as the type of products they offer over time. We find that after inclusion in the Top 100,

Top 1200 list during these years.

[4]Barrons.com has on average, over 2.5 million unique visitors and more than 11 million pages views per month. https://images.dowjones.com/wp-content/uploads/sites/183/2018/05/09164150/Barrons.com-Audience-ProfileQ12017.pdf

[5]The fixed cost is quite low as there is no fee to submit.

a financial advisor attracts more assets under management and accounts suggesting that households value the certification even holding the advisor fixed. One channel that may help to explain this increase is due to increased media coverage of Top 100 advisors. We find some modest evidence of an increase coverage after an advisor is certified.

In the third approach, we exploit randomness around state-level ranking cutoffs. Mullainathan, Schwartzstein, and Shleifer 2008 suggest that individuals "think coarsely" about categories and apply the same model of inference to all situations within a category. In the context of rankings, consumers may have limited cognitive ability and fixate on easier to understand measures that third-parties can provide. For example, Pope 2009 estimates the response to rankings in the hospital market and finds that hospitals that improve their rank are able to attract significantly more patients. However, he found changes in discrete rankings (e.g. from "B" to "A") affected patient choice, even after controlling for continuous quality.

The Barron's rankings also has this type of discrete component that generates plausibly exogenous variations that effectively randomize advisors near state-level cutoffs. The number of advisors in the Top 100 ranking is fixed (i.e., 100 advisors). Compared to the advisor that just missed the Top 100 cutoff, the difference between the advisor in the same state that just made the Top 100 cutoff should be similar to the difference between the advisor that just missed the Top 100 cutoff and the next best in the same state who similarly missed the Top 100. To illustrate, consider Barron's Top 100 ranking in 2018. In 2018, Florida has 84 advisors in the Top 1000 ranking. Of these, four advisors are in Barron's Top 100 ranking. Thomas Moran, Patrick Dwyer, Adam Carlin, and Charles Mulfinger II are ranked 27 (1), 30 (2), 65 (3), and 91 (4) in the Top 100(1000) ranking, respectively. As shown in Figure 3.1, all these advisors use Barron's Top 100 ranking as a badge of honor on their public websites. However, Louis Chiavacci who is ranked 5^{th} in Florida is not in Barron's Top 100 ranking. As shown in Figure 3.2, we exploit this state-level cutoff to estimate the effect of third party quality certification on misconduct by comparing only Charles Mulfinger II (lowest ranked Top 100) to Louis Chiavacci (highest ranked non-Top 100). We repeat this process for each state in a given year. A key assumption of this approach, which we verify, is that advisors who are just above the threshold and advisors who are just below the threshold appear to be very similar based on their observable pre-ranking characteristics such that their assignment to the treatment group (Top 100 status) can be thought as a randomized experiment.

We find evidence that relative to an advisor just below the threshold, an advisor just above the threshold who makes it into the Top 100 experiences a significantly higher growth in assets under management (around 21-31%). We employ a similar design around alternative (placebo) cutoffs and find no significant differences around the 25^{th}, 50^{th}, and 75^{th} cutoffs suggesting that there is something special about being in the top 100 ranking. We run a similar placebo test using state-level rankings and confirm that our results are unlikely to have occurred by chance.

We also document considerable heterogeneity in the effect. Consistent with certification effects being more important for those with less reputation, we find the effect size nearly doubles for those advisors from smaller and presumably less well-known firms as well as for newer advisors. Importantly, we show that the effects remain even for those advisors working large established firms suggesting that the effects are present across all types of advisors.

In addition to the evidence that investors and media respond to rankings changes, we find evidence that advisors also shift their behavior. On one hand, third-party certification can enhance potential reputation costs making engaging in misconduct more costly as advisors do not want to lose their valuable ranking Becker 1968. On the other hand, "superstar" status may encourage advisors to spend more time on public and private activities outside their focus on clients Malmendier and Tate 2009. The popular press has highlighted a so-called "Barron's curse" of misconduct by featured advisors.

We find evidence consistent with the former prediction at both the firm and individual levels; advisors are less likely to commit misconduct after inclusion in the Top 100 ranking. One concern could be that our results capture a shift in clients' propensity to complain after receiving the Barron's ranking not a shift in advisors' behavior. To address this concern, we show that after getting into the Top 100 ranking, advisors are similarity less likely to commit regulatory misconduct, e.g., misconduct excluding customer complaints and firm terminations suggesting that the reduction of misconduct is due to advisor (and not customer) behavior. These results may superficially seem at odds with folk industry wisdom such as the "Barron's curse". However, the disproportionately high percentage of customer complaints among Barron's list members due to in large part to the large scale of business and long careers of this population. Within-advisor analysis shows that the risk of misconduct drops once an advisor is added. This seemingly paradoxical result also illustrates why simple regulatory disclosures may be useless or even worse misleading to households: advisors with little or no experience will have clean histories on disclosure websites but may be of inferior quality to experienced advisors with minor dings on their records after a distinguished career.

Our study is especially relevant for the growing literature on the determinants of financial misconduct by financial advisors. Egan, Matvos, and Seru 2019b show that financial misconduct is common, is concentrated among repeat offenders, and clusters across geographic areas. There is also evidence that, while malleable, advisors have relatively inflexible ethical tendencies that are related to where they are raised Clifford, Ellis, and Gerken 2019. There are also a number of papers that document the dynamic nature of misconduct: advisors are more likely to engage in misconduct if they are exposed to co-workers that have a record of misconduct Dimmock, Gerken, and Graham 2018a, face weaker local regulatory oversight Charoenwong, Kwan, and Umar 2019, experience negative shocks to their personal wealth Dimmock, Gerken, and Van Alfen 2020, or face discriminatory discipline from managers Egan, Matvos, and Seru 2020.

Our paper contributes to the latter stream by showing how a reputational shock can mitigate the advisor's incentive to engage in misconduct. While the focus of this literature has been on the determinants of bad quality advisors, our paper focuses on third-party certification and the ability to identify good quality advisors. As clients and advisors react to certification (and not just underlying quality), this suggests third-party certification can provide an additional layer of discipline in this market.

Our study also relates to the literature on customer response to quality disclosure and certification by third-parties. Several studies of report card style grades have found mixed evidence about customers ability to infer underlying quality rather than focusing more straightforward, but coarse measures. 2002 find that GM employees respond to overall quality report card ratings but not to specific quality measures. Pope 2009 finds that changes in discrete rankings affected patient choice, even after controlling for continuous measures of quality. However, Bundorf et al. 2008 find evidence of consumer sophistication when they evaluate the quality of fertility clinics despite differences in patient mix suggesting that consumers could see through the simple statistics. Similar to the studies of health report cards, our findings suggest clients in the financial advisory industry may have limited sophistication and therefore tend to focus on a simple measure such as Top 100 ranking that is easier to understand.

3.2 Institutional Background and Data

In this paper, we rely on three primary data sources: Barron's rankings, SEC Form ADV downloads, and FINRA's Brokercheck database.

3.2.1 Barron's Rankings

Barron's has been ranking financial advisors since 2004 to identify the best advisors in the industry and raise standards in the financial advisory industry. According to its website, Barron's uses "a deeply researched, quantitative approach to identify the best in the business". It collects 102 points of data from the advisors who wish to be ranked. After verifying the data provided by advisors with the advisors' firms and with regulatory databases, Barron's applies its ranking formula to the data to generate a ranking. Barron's ranks advisors based on a proprietary methodology consisting of three main components: assets under management, revenue generated for their firms, and quality of service and regulatory records. All the rankings are based on hard numbers such as advisors' assets under management (AUM) and annual revenue generated, referral-driven organic growth, length of service, client retention, customer satisfaction, community involvement, and philanthropic work. Investment return is not a component in Barron's rankings since (1) not all advisors have audited results, and (2) an advisor's returns are significantly driven by the risk tolerance of clients. To be eligible for any of Barron's rankings, advisors are required to have at least seven years of financial services experience and have been employed at their current firm for at least one year. Barron's publishes an advisor's new ranking as well as his or her last year ranking, AUM, typical account, typical net worth, and type of customers, e.g., individuals, high net worth, ultra-high net worth, foundations, institutional, and endowments. In addition, to mitigate conflicts of interest, Barron's does not receive any compensation from advisors, participating firms and their affiliates, or the media in exchange for rankings.

Since 2004, Barron's has ranked Top 100 financial advisors in the United States. This ranking is nationwide, and each state may or may not have a representative in this ranking. For example, in 2009, New York and Wyoming have 26 and 0 representatives in this ranking. Using year-end numbers, Barron's ranks advisors nationwide and publishes the ranking in April. In addition, only financial advisors in the Top 100 ranking are eligible for exclusive, invitation-only Barron's 100 Summit. Attendees have the opportunity to share ideas, learn the latest innovations in wealth management, network with other elite peers, and discuss new challenges in the industry.[6]

Barron's started ranking Top 1000 financial advisors in the United States in 2009. Since 2014, it has expanded its Top 1000 ranking from 1000 to 1200. This ranking includes all 50 states plus the District of Columbia and each state has at least 5(6) representatives in the Top 1000 (1200) ranking. The number of representatives in the ranking varies with the population and wealth of each state. For example, in 2009, New York and Wyoming have 100 and 5 representatives in this ranking. Using third-quarter numbers, Barron's ranks advisors in each state and publishes this ranking in March. Figure C.1 provides an annual timeline and Figure 3.3 shows the state-level breakdown of top advisors across all states in 2018. To be eligible for this ranking, advisors are required to be registered by FINRA or SEC and have at least an AUM minimum that varies by state. The Top 1000 financial advisors are eligible for one-day, invitation-only regional summits.

Barron's receives 3,000 to 4,000 applications every year. To generate the ranking, Barron's follows a three-step procedure. First, verifying the data provided by advisors, Barron's creates a pool of advisors based on some characteristics such as assets under management. For each state, Barron's sets a prespecified quota for the number of advisors in the Top 1000 ranking. For example, Barron's selects about 150 (9) advisors in New York (Wyoming) in 2009. Barron's does additional research on these advisors in the pool (e.g. searching Brokercheck) for approximately 150 percent of each state's quota to ensure that each state has enough advisors to be ranked.

Next, Barron's puts all advisors in a large pool and calculates a single continuous score for each advisor, using its proprietary methodology. This step is important in our identification strategy for at least three reasons: (1) Continuous scores are relative. Therefore, an advisor's continuous score depends on not only his/her performance, but also other advisors'. (2) Barron's does not disclose its proprietary methodology. Therefore, advisors are less likely able to reverse engineer and manipulate the ranking by strategically exerting extra effort on some inputs. (3) Barron's does not publish all inputs used in the ranking. In other words, an advisor can see only a subset of the inputs for all advisors in the ranking. This can even make the manipulation of the ranking less likely.

In the last step, after ranking all advisors in the pool, Barron's assigns advisors a state-level rank based on their continuous score. Specifically, Barron's assigns the highest-ranked advisor in the pool to the first place in his or her state. It assigns the second highest-ranked advisor in the pool to the remaining highest-ranked place in his or her state and so on. Barron's repeats this process until all advisors in the pool are assigned to their own home state. As part of the final product, Barron's publishes only the continuous scores for Top 100 advisors. Advisors who are not in

[6] https://www.barrons-top100.com/

the Top 100 ranking are less likely able to manipulate the ranking because they do not know their own score and therefore they do not know how far they are from the cutoffs. Therefore, it is less likely for an advisor to strategically self-select to be just above those cutoffs.

A concern regarding ranking manipulation is that advisors can move from a state with high competition to a state with less competition to increase their chance of getting into the ranking. This is unlikely for at least two reasons. First, most advisors who apply for the ranking are highly experienced and probably well-known in their own city and state. Clientele are typically local. Therefore, moving from their own home state to another one is costly. Second, even if they move to another state to increase their chance, it does not mean they can be right above the threshold. In other words, since Barron's puts all advisors in a large pool and calculates a continuous score for each advisor relative to all other advisors in that pool, an advisor is less likely able to manipulate the ranking in a way that he or she can be right above the thresholds at his or her state.

To better describe how Barron's ranks top advisors, we provide a hypothetical example. To illustrate, assume there are fifty one states {State 1, State 2 , ... , State 51}. Due to a variation in population and wealth, each state may have a different prespecified quota in the Top 1000 ranking. In our hypothetical example, the quota for State 1 (State 49) is 22 (14) in the Top 1000 ranking. To rank advisors from the application pool, Barron's selects 1500 advisors (1000*1.5) based on some characteristics such as AUM. Next, using its algorithm, Barron's calculates a continuous score for all 1500 advisors. Next, it assigns advisors a state-level rank based on their final score. For example, it assigns advisor A1 to the first place in State 48, advisor A2 to the first place in State 23, advisor A3 to the first place in State 1, and so on. Barron's repeats this process to assign all advisors to their own state till all prespecified slots are filled. Figure 3.7 shows this process in detail. This process generates state-level cutoffs that effectively randomize advisors. These cutoffs vary unpredictably from year to year and state to state. The intuition behind our identification strategy is that the somewhat arbitrary nature of these state-level cutoffs suggests that being just above or below them is locally random, that is, advisors around the cutoffs are similar across all characteristics except for being in Barron's ranking. We exploit these cutoffs for identification in Section 3.3.3.

3.2.2 Regulatory Databases

The Brokercheck data on individual advisors collected from multiple downloads from https://brokercheck.finra.org/ which comes from historical Form U4 filings contains information about all brokers and most investment advisors from 2009 to 2020. The data contains detailed information about disclosures, employment history, and qualifications for all financial advisors in the United States. For example, for each advisor, we observe an advisor's registrations, industry exams, licenses, customer disputes, regulatory disputes, and employment history. See Dimmock, Gerken, and Graham 2018a and Egan, Matvos, and Seru 2019b for detailed explanations of the data.

We obtain data from Form ADV in the SEC investment advisor website. Each Form ADV filing contains information on financial advisory firms, including their address, number of employees, assets under management, and any disciplinary events (i.e., civil, criminal, and regulatory violations). We download all Form ADV data directly from the SEC website and constructed a panel of financial advisory firms from 2009 to 2020. The data consists of 143,539 observations at the firm-year level. Since our firm level analyses mainly focus on small firms, we restrict our sample to firms with lower than 50 employees. Table 3.1 Panel A reports the summary statistics for the estimation sample. The sample contains of 127,299 observations at the firm-year level. An average financial advisory firm in our small firm sample has assets under management of \$1.18 billion, 615 accounts, 10.7 employees, and a rate of misconduct of 0.42%. Firms with top advisor owner have more accounts and employees and less misconduct than firms without top advisor owner.

As shown in Table 3.1 Panel B, advisors in the Top 100 ranking, on average, have more assets under management and media coverage relative to those in Top 1000 but not in the Top 100. On average, they have more than \$6.17 billion in assets under management. Advisors in both Top 100 and Top 1000 rankings are highly experienced, e.g., on average, they have more than 25 years of experience. Also, advisors in the Top 100 ranking are more likely to provide services to individual clients with more than \$10 million in assets, e.g., ultra-high net worth clients. In addition, they are more likely to have foundations and endowments as their clients.

3.3 Empirical Strategy and Results

We explore the effect of third party quality certification on clients in the market for financial advice, both at the firm level and individual advisor level using a variety of empirical approaches.

3.3.1 Firm Panel Regression

To begin, we use firm-level data from Form ADV to examine the effect of third party quality certification on firm-level outcomes. The utilization of firm-level data has two advantages. First, the SEC requires advisory firms with regulatory assets under management of \$100 million or more to annually file their Form ADV. This may address concerns regarding Barron's self-reported survey data. Second, the SEC requires advisory firms to disclose information on assets under management, number of accounts, and any disciplinary events. This may help confirm our advisor-level analyses and address external validity concern regarding Barron's advisors. In this section, we use two approaches to examine this effect in the panel of financial advisory firms with regulatory assets under management of \$100 million or more. First, we use linear regression models with firm and state×year fixed effects Angrist and Pischke 2009. Specifically, we estimate Equations 3.1 and 3.2:

$$Log(AUM)_{f,s,t+1} = \beta + \beta_1 TopAdvisorOwner_{f,s,t} + \theta Cont._{f,s,t} + \mu_f + \mu_{s,t} + \epsilon_{f,s,t} \quad (3.1)$$

$$Log(Accounts)_{f,s,t+1} = \beta + \beta_1 TopAdvisorOwner_{f,s,t} + \theta Cont._{f,s,t} + \mu_f + \mu_{s,t} + \epsilon_{f,s,t} \tag{3.2}$$

where $Log(AUM)_{f,s,t+1}$ is the logarithm of firm f' assets under management at year *t+1* and $Log(Accounts)_{f,s,t+1}$ is the logarithm of firm f' number of accounts at year *t+1*. The key variable of interest, $Top\_Advisor\_Owner_{f,s,t}$, is an indicator variable equals to one if the firm f's owner at year t is in Barron's Top 1000 Ranking and zero otherwise.[7] μ_f is firm fixed effects and controls for all time-invariant characteristics of firms such as their monitoring policies and the type of products they offer. The coefficient of interest in Equation 3.1 and Equation 3.2 is β_1. If Barron's Ranking is a valuable third-party quality certification for potential clients, then β_1 should be positive and statistically different from zero. In Table 3.2 Column (1) through Column (4), we find strong evidence that third-party quality certification has a positive effect on the financial advisory firms: on average, being named a Top Advisor is associated with a 9.4% and 10.6% increase in a firm's assets under management and number of accounts. The economic magnitude of the coefficients of interest is large. The unconditional average of assets under management is $1.18 billion and the unconditional average of number of accounts is 615. This suggests that being named a Top Advisor increases a firm's assets under management by $111 million and a firm's number of accounts by 65 new accounts.

Next, we use matching methods for time-series cross-sectional data Imai, Kim, and Wang 2018 to complement our linear approach. This matching method relaxes the parametric assumptions inherited in the linear models. In this method, for each treated advisory firm, we first select a set of control firms in the same year and state. Next, we match treated firms with control firms based on lagged misconduct, lagged number of employees, lagged log(AUM), and lagged log(accounts). This ensures that firms in the control group have an identical treatment history for a pre-specified time span. We next, estimate the average treatment effects using the difference-in-differences estimator.[8]

Figure 3.5 shows the standardized mean differences for observable covariates, which documents that the standardized mean differences for number of employees, log (AUM), and log (accounts) are both statistically and economically insignificant suggesting that the parallel trend assumption is plausible. Next, we estimate the average treatment effects using the difference-in-differences estimator. We test whether having a Top Advisor affects a firm's assets under management and number of accounts. Figure 3.4a shows the effect of third party quality certification on the firm's assets under management in event time. Figure 3.4b repeats the same analysis for the firm's number of accounts as the main outcome variable. Consistent with prior results, these results suggest that being named a Top Advisor has a substantial effect

[7]Since our analysis is at the firm level, we only look for the presence of any Top 100 or Top 1000 ranked owner at the advisor firm. For small firms, a top advisor typically has ownership of the firm. We also limit our sample to small firms (i.e. firms with less than 50 employees) as top advisors at large wirehouse firms typically do not have ownership and are less likely to share reputation across the firm in the same sense as happens in small firm. The results are quantitatively and qualitatively robust if we relax this filter.

[8]For more information, please visit https://imai.fas.harvard.edu/research/tscs.html

on client behavior after (but not before) the certification is granted. These results confirm our hypothesis that clients pay attention to firms' quality certifications.

3.3.2 Individual Panel Regression

In this section, we use the panel of individual financial advisors who are ranked by Barron's from 2009 to 2020. Using granular data from advisor-level panel, we are able to better identify the effect of third party quality certification in the market for financial advice. In particular, we exploit within-advisor movement into Barron's rankings. In this approach, we examine within-advisor movement into the Top 100 ranking among Top 1000 advisors. This approach has two main advantages. First, all advisors in this sample have applied to be ranked by Barron's, eliminating any potential selection effect. Second, we are able to isolate the effect of third party quality certification on advisor-level outcomes via adding the state-level Top 1000 ranking as a measure of advisors' quality.

To begin, we examine whether potential clients value Barron's Top 100 ranking as a third-party quality certification. If this quality certification is important for potential clients, then being named a Top 100 Advisor should have a positive and significant effect on the advisor's assets under management and number of accounts. In other words, advisors with this quality certification should experience higher assets under management and number of accounts relative to advisors in the Top 1000 ranking. Basically, we estimate Equations 3.3 and 3.4:

$$Log(AUM)_{i,s,f,t+1} = \beta + \beta_1 Top100_{i,s,f,t} + \theta Controls_{i,s,f,t} + \mu_i + \mu_f + \mu_{s,t} + \epsilon_{i,s,f,t} \quad (3.3)$$

$$Log(Account)_{i,s,f,t+1} = \beta + \beta_1 Top100_{i,s,f,t} + \theta Controls_{i,s,f,t} + \mu_i + \mu_f + \mu_{s,t} + \epsilon_{i,s,f,t} \quad (3.4)$$

where $Log(AUM)_{i,s,f,t+1}$ is the logarithm of financial advisor i' assets under management at year *t+1* and $Log(Account)_{i,s,f,t+1}$ is the logarithm of financial advisor i' number of accounts at year *t+1*. The key variable of interest, $Top100_{i,s,f,t}$, is an indicator variable equal to one if financial advisor i in firm f at year t is in the Top 100 ranking and zero if the advisor is not in the Top 100 ranking. μ_i is advisor fixed effects and controls for all time-invariant characteristics of advisors such as their overall propensity to commit misconduct, education, and religious background. μ_f is firm fixed effects and controls for time-invariant confounding factors at the firm level such as firms' monitoring policies or the type of products they offer.[9] $\mu_{s,t}$ is state-year fixed effects and controls for time-varying confounding factors at the state level such as state's income, policies, and population. By including advisor and firm fixed effects, we benchmark an advisor against his or her own behavior throughout the whole sample and limit the comparison to advisors who work for the same firm. $Controls_{i,s,f,t}$ include advisors' $NormalizedRank$, experience, and tests (Series 6, 7, 24, 65, and 66).[10] $NormalizedRank$ is a continuous variable between zero and one

[9] Clifford and Gerken 2020 and Gurun, Stoffman, and Yonker 2021 document that clients tend to be "sticky" as advisors move across firms, so advisor level effects likely are more important than firm-level ones in this context.

[10] In Table C.1 we also include $\Delta Log(AUM)$ to control for prior growth rate and find similar results.

(i.e. the highest ranked advisor in a state receives a one, whereas the lowest ranked advisor in a state receives a zero). It measures an advisor's relative quality at the state level. By normalizing between zero and one, we account for different sized states. Controlling for $NormalizedRank$ is important in our regression models. If being named a Top 100 advisor has no additional value for clients as it reflects the same information as being in the Top 1000 ranking, then after controlling for the $NormalizedRank$, the effect of being named a Top 100 Advisor should disappear. For example, Lance Lemmons is ranked 6^{th} in Florida in the Top 1000 ranking in 2018. If only Barron's score (their measure of quality) mattered, then the difference between Lance Lemmons (6^{th}) and Louis Chiavacci (5^{th}) should be the same as Louis Chiavacci (5^{th}) and Charles Mulfinger (4^{th}, but also in the Top 100). If being in the Top 100 ranking is uninformative to clients beyond the continuous measure of quality that is embedded in the Top 1000 score, then controlling for $NormalizedRank$ should absorb the coefficient of variable of interest, $Top100_{i,s,f,t}$.[11] Note, we more explicitly examine this discontinuity in Section 3.3.3.

The coefficient of interest in Equation 3.1 is β_1. If Barron's ranking is a valuable third party quality certification for potential clients, then β_1 should be positive and statistically different from zero. Table 3.3 Panel A reports the estimation results of Equation 3.3. Columns (1) and (2) show the expected positive association between advisors' quality and their growth of assets under management. In Columns (3) to (5), we include $Top100_{i,s,f,t}$ to capture the effect of being named a Top 100 advisor after controlling for quality of advisors. The stability of coefficients of $NormalizedRank$ across different specifications confirms that $Top100_{i,s,f,t}$ captures the effect of third party quality certification and not the quality of advisors. In Columns (3) to (5), we find strong evidence that the effect of third party quality certification on assets under management is positive and statistically significant: on average, being named a Top 100 Advisor is associated with a 13% to 17% increase in an advisor's assets under management. The economic magnitude of this effect is large. In this sample, the unconditional average of assets under management is $2.27 billion. This suggests that being named a Top 100 Advisor increases an advisor's assets under management by $297 million. Table 3.3 Panel B shows the estimation results of Equation 3.4. In Columns (1) and (2), we find the expected positive association between our measure of advisors' quality and advisors' growth of number of accounts. In Columns(3) to (5), consistent with the results from Panel A, we find strong evidence that the effect of third party quality certification on number of accounts is positive and statistically significant: on average, being named a Top 100 advisor is associated with a 8.4% to 11.1% increase in an advisor's number of accounts. The economic magnitude of this effect is large. In this sample, the unconditional average of number of accounts is 488. This suggests that being named a Top 100 advisor increases an advisor's number of accounts by 41 new accounts. We also note that these results are consistent with our results from firm panel regressions.

[11]Controlling for $NormalizedRank$ should also assuage concern that the Top 100 result is being driven by a mechanical relation with AUM and accounts as those are two of the 102 factors in Barron's scoring model.

Media coverage

In this section, we explore one channel that may explain this increase is due to increased media coverage of Top 100 advisors. Specifically, we examine the effect of being named a Top 100 Advisor on media coverage by estimating Equation 3.5:

$$Log(1+Num.articles)_{i,s,f,t+1} = \beta+\beta_1 Top100_{i,s,f,t}+\theta Cont._{i,s,f,t}+\mu_i+\mu_f+\mu_{s,t}+\epsilon_{i,s,f,t} \tag{3.5}$$

where $Log(1 + Numberofarticles)_{i,s,f,t+1}$ is the logarithm of financial advisor i' 1 plus number of articles covering full name of financial advisor i in LexisNexis at year $t+1$.[12] The key variable of interest, $Top100_{i,s,f,t}$, is an indicator variable equals to one if financial advisor i in firm f at year t is in the Top 100 ranking and zero if the advisor is in the Top 1000 ranking. μ_i is advisor fixed effects and μ_f is firm fixed effects. $\mu_{s,t}$ is state-year fixed effects and controls for time-varying confounding factors at the state level such as state's income, policies, and population. In Table 3.3 Panel C, we find modest evidence of an increase in media coverage after an advisor being listed to the Top 100 ranking: on average, being named a Top 100 advisor is associated with a 4.5% increase in media coverage. The economic magnitude of the coefficients of interest is large. The unconditional average of media coverage is 14% in this sample. Overall, we document perhaps unsurprisingly that advisors are more likely to receive media attention after receiving a top advisor ranking.

Heterogeneity

In this section, we test how cross sectional variation in firm size, advisors' experience, and advisors' history of past misconduct affects the relation of third party quality certification and assets under management. Prior studies use firm size and experience as proxies for reputation Dranove and Jin 2010. In other words, large firms and seasoned advisors are more reputable. Thus, all else held equal, we expect the relation between being named a Top 100 advisor and assets under management will be stronger for (1) smaller firms (i.e., firms with fewer number of employees) and for (2) newer advisors (i.e., advisors with lower years of tenure).

As our first cross-sectional test, we find strong evidence that the effect of third party quality certification on assets under management is more pronounced among advisors working for smaller firms. The results, reported in Column (1) of Table 3.4, show that the coefficient on the interaction term $Top100\ Indicator\ X\ Small\ Firms$ is significant and positive, suggesting that advisors from smaller firms benefit more from certification of quality than those for larger firms. Next, we test if newer advisors benefit more from being listed in the ranking. In Table 3.4, Column(2), we find strong evidence that certification of quality has stronger effect on newer advisors: the coefficient on the interaction term $Top100\ Indicator\ X\ Young\ Advisors$ is significant and positive.

Prior studies show that advisors with a history of misconduct are more likely to be repeat offenders and engage in misconduct in the future Egan, Matvos, and Seru

[12] We exclude Barron's articles to avoid a mechanical bias.

2019b. Thus, high-quality advisors with a blemish on their record may have greater need for certification to avoid pooling with low quality advisors. Thus, all else held equal, we expect the relation between the effect of quality certification and assets under management will be stronger for advisors with a history of misconduct. The results, presented in Column (3) of Table 3.4, show that the sign of the coefficient of the interaction term $Top100\ Indicator\ X\ Past\ Misconduct$ is positive (but statistically insignificant), suggesting that while both groups experience an increase in their assets under management, advisors with a history of misconduct might benefit more.

3.3.3 State-level Cutoffs

As discussed previously in the institutional detail, the construction of the rankings allows an even sharper set of tests based on discontinuities around the bottom of the Top 100 and state-level rankings. Recall, Barron's puts all advisors in a large pool to rank them. Therefore, being named a Top 100 advisor is not only a function of an advisor's characteristics, but also a function of other advisors' characteristics which change each year. Further, it seems implausible that an advisor could manipulate treatment as Barron's uses a proprietary methodology that would be difficult to precisely reverse engineer.[13]

One concern could be that advisors who are above the cutoffs are significantly different from advisors who are below the cutoffs. We address this concern in two ways. First, we show that these advisors are very similar based on their observable pre-ranking characteristics. Second, in a robustness test, we exclude elite advisors who are known to be far away from the cutoff (e.g., Top 50) and show that the results are robust.[14]

Thus, we next exploit this randomness around state-level cutoffs to estimate Equation 3.6:

$$Log(AUM)_{i,s,f,t+1} = \beta + \beta_1 Top100_{i,s,t} + \theta Controls_{i,s,t} + \mu_f + +\mu_{s,t} + \epsilon_{i,s,t} \quad (3.6)$$

where $Log(AUM)_{i,s,f,t+1}$ is the logarithm of financial advisor i' assets under management at year *t+1*. The key variable of interest, $Top100_{i,s,f,t}$, is equal to one if financial advisor i in state s at year t is right above the threshold and zero if the advisor is right below the threshold. μ_f is firm fixed effects and controls for all time-invariant characteristics of firms such as their advertising budget and overall propensity to commit

[13]Barron's discloses only a small subset of inputs used in the ranking (e.g., AUM and type of clients), making the replication of the exact ranking almost impossible. Also, Barron's only discloses continuous scores for the Top 100 Advisors. This makes the manipulation of the ranking more difficult since advisors who are not in the Top 100 ranking do not know how far they are from the cutoffs.

[14]Moreover, using the continuous scores from the Top 100 ranking, we estimate the effect of an increase in the Top 1000 ranking on the continuous scores. In Figure C.2, each dot represents the estimated effect of an increase in the Top 1000 ranking at the state-year level. For example, one dot shows that an increase in the Top 1000 ranking in Massachusetts in 2015 is associated with a 0.39 decrease in continuous scores. Given that the average effect of an increase in the Top 100 ranking is -0.05, Figure C.2 suggests that advisors who are below the cutoffs are not very different from advisors who are just above the cutoffs.

misconduct. $\mu_{s,t}$ is state-year fixed effects and controls for time-varying confounding factors at the state level such as state income and population as well as changes in policies at the state level. The identification assumption is that after controlling for these observable characteristics and fixed effects, an advisor just above the threshold in a particular state is the same as an advisor right below the threshold in the same-state other than Top 100 status. In support of this assumption, we show that observable characteristics a year prior to publication of Barron's ranking are similar for advisors who are right above the thresholds and advisors who are right below the thresholds. Table 3.5 reports the summary statistics for advisors in the treatment group and advisors in the control group. Table 3.5 shows that advisors in the treatment group and advisors in the control group are not significantly different on observable characteristics at time t.[15]

Initially, we employ this methodology on the sample of states that have at least one Top 100 advisor and one non-Top 100 advisor (see Figure 3.3) and select the lowest ranked advisor in the Top 100 and the highest ranked advisor outside of the Top 100 in each state. Note, by including state-year fixed effects, we are effectively comparing at this pair level. Table 3.6 reports the results from estimating Equation 3.6. In Column (1), we find strong evidence that clients value third-party quality certification: being named a Top 100 Advisor is associated with a 29% increase in assets under management. The economic magnitude of the coefficient of interest is large given that the unconditional average of assets under management in this sample is $4.6 billion. In Column (2), we exclude states with only one advisor in the Top 100 ranking under the idea that such small states may have their advisors far from the actual threshold. Consistent with prior results, we find robust evidence on the effect of Barron's Top Ranking on assets under management. Furthermore, in Column (3), we expand the sample to include all treated (i.e. Top 100) advisors in each state that are ranked outside of the top 50. In this test, the treatment group consists of advisors who are ranked from 51 to 100 and the control group consists of advisors who are not in the Top 100 ranking. To have the same number of advisors in the control and treatment groups, we match the number of advisors in the control group with the number of advisors in the treatment group. To illustrate, consider advisors in Florida, New York, and Colorado. In 2018, Florida, New York, and Colorado have 4, 34, and 1 representative in the Top 100 ranking. Out of them, 2, 21, and 1 representative are in the bottom half of the Top 100 ranking (51-100) in Florida, New York, and Colorado respectively. Therefore, we increase the bandwidths in the same proportion, e.g., we assign 2, 21, and 1 advisor in Florida, New York, and Colorado below the threshold in the Top 1000 ranking to the control group. We then estimate Equation 3.6, in which $Top100_{i,s,f,t}$ is an indicator variable equals to one if an advisor is in the treatment group and zero if the advisor is in the control group. The results are consistent with the prior results in Columns (1) and (2). We find that being named a Top 100 Advisor is associated with a 30.8% increase in assets under management. These results are

[15] We use the Holm-Bonferroni method to correct for problem of multiple comparisons as Table 3.5 contains nine separate variables. None of the nine variables are significantly different between treatment and control once we account for the multiple comparisons.

consistent with the results from firm panel regression and individual advisor panel. Overall, our results suggest that clients value third party quality certification in the market for financial advice. In Table 3.6, the coefficients of $NormalizedRank$ suggest that the quality of advisors just right above the thresholds and quality of advisors right below the thresholds are not statistically different from each other within a state×year. This is consistent with the plausibly exogenous variations of third party quality certification near state-level cutoffs.

Robustness Check: Alternative Cutoff Points in the Top 100 Ranking

One concern could be that our results capture a general, potentially non-linear, trend in the ranking, and we would expect to find similar outcomes around other alternative arbitrary thresholds within the Top 100 ranking. To mitigate this concern, we use 25^{th}, 50^{th}, and 75^{th} rank in the Top 100 ranking as alternative arbitrary thresholds for the placebo tests. We re-estimate Equation 3.6 for these three arbitrary thresholds. We replace $Top100$ with *Above Cutoff Indicator* which is an indicator variable equals to one if an advisor is above the arbitrary thresholds and zero if the advisor is below the arbitrary thresholds. We also control for *Barron's score* which measures quality of advisors (but only publicly available for the Top 100 advisors). Table 3.7 shows the results for assets under management. The coefficient of interest, *Above Cutoff Indicator*, is economically and statistically insignificant across all three arbitrary thresholds. This suggests that the effect of being named a Top 100 advisor could not be a result of a general trend in the ranking.

Robustness Check: Alternative Cutoff Points in the Top 1000 Ranking

As a complementary robustness check, we consider alternative arbitrary thresholds within the Top 1000 ranking. We employ a bootstrapping procedure as a placebo test. For each repetition, after excluding financial advisors in the Top 100 ranking from the Top 1000 ranking, we randomly assign a cutoff point in each state that has at least one representative in the Top 100 ranking and estimate Equation 3.6. We repeat this procedure 20,000 times. To illustrate, consider Florida and New York. In 2018, Florida and New York have 4 (84) and 34 (120) representatives in the Top 100(1000) ranking respectively. First, we exclude advisors in the Top 100 ranking from the Top 1000 ranking. Therefore, Florida and New York end up with 80 and 86 advisors. Next, we randomly assign a cutoff point to each state. For example, in Florida, we assign advisor number 10 to the treatment group and advisor number 11 to the control group. In New York, we assign advisor number 50 and 51 to the treatment and control groups respectively. After randomly assigning advisors into treatment and control groups, we estimate Equation 3.6. Figure 3.6 plots a histogram of the magnitude of the coefficient of interest for 20,000 simulation runs. The mean and standard deviation of coefficient of interest are -0.003 and 0.101 respectively. However, the actual coefficient of interest from Table 3.6 Column (1) is 0.290 with corresponding p-value of 0.0017. This result along with the results from alternative cutoff points in the Top 100 ranking suggest that the thresholds caused by mapping advisors in the Top 100 ranking into the Top 1000 ranking at the state level are not random.

3.4 Certification and Misconduct

Given that advisors receive significant gains due to certification itself, losing certification becomes costly. This makes the cost-benefit from engaging in misconduct less attractive Becker 1968. Thus, we might expect less misconduct for newly minted top advisors. This goes against the folk wisdom in the industry, which has noticed a so-called "Barron's curse"of misconduct by featured advisors. It could be that such "superstar" status may encourage advisors to spend more time on public and private activities outside their focus on clients Malmendier and Tate 2009.

In this section, we examine whether third party quality certification can function as a disciplinary device to mitigate advisors' misconduct or acts to distract elevated advisors. We utilize both the panel of financial advisory firms and the panel of financial advisors from 2009 to 2020. Using firm-level data from Form ADV, we first examine the effect of being named a Top 1000 advisor on misconduct by estimating Equation 3.7:

$$Misconduct_{f,s,t+1} = \beta + \beta_1 TopAdvisorOwner_{f,s,t} + \theta Cont._{f,s,t} + \mu_f + \mu_{s,t} + \epsilon_{f,s,t} \quad (3.7)$$

where $Misconduct_{f,s,t+1}$ is a dummy variable equals to one if firm *f* at year *t+1* has any disciplinary event and zero otherwise. The variable of interest ,$Top1000Owner$, is an indicator variable equals to one if the firm *f*'s owner at year *t* is in Barron's Top 1000 Ranking and zero otherwise. μ_f is firm fixed effects and controls for all time-invariant characteristics of firms such as their monitoring policies and the type of products they offer. The coefficient of interest in Equation 3.7 is β_1. If Barron's Ranking can function as a disciplinary device, then β_1 should be negative and statistically different from zero. Consistent with theoretical models of reputation, we find strong evidence that third party quality certification may help reduce misconduct in the market for financial advice. In Table 3.8 Column (1), we find that on average, being named a Top 1000 advisor is associated with a 0.8 percentage point decline in firm's likelihood of having any disciplinary events. The economic magnitude of the coefficient of interest is large: the unconditional average of misconduct in this sample is 0.42 percentage point.

As before, we use matching methods for time-series cross-sectional data from Imai, Kim, and Wang 2018. Following the same procedure as in Section 3.3.3, we first match treated firms with control firms on lagged misconduct, lagged number of employees, lagged log(AUM), and lagged log(accounts). We then estimate the average treatment effects using the difference-in-differences estimator. Figure C.3 shows the negative effect of third party quality certification on the firm's likelihood of engaging in misconduct in event time. Consistent with results from firm panel regression, this result suggests that being named a Top 1000 advisor has a substantial effect on the firm's disciplinary events. These results confirm our hypothesis that third party quality certification can function as a disciplinary device to mitigate misconduct in the market for financial advice.

Next, we utilize the advisor-level data from 2009 to 2020. Using within-advisor movement into Barron's rankings, we estimate the effect of being named a Top 100 advisor on misconduct by estimating Equation 3.8:

$$Misconduct_{i,s,f,t+1} = \beta + \beta_1 Top100_{i,s,f,t} + \theta Cont._{i,s,f,t} + \mu_i + \mu_f + \mu_{s,t} + \epsilon_{i,s,f,t} \quad (3.8)$$

where $Misconduct_{i,s,f,t+1}$ is a dummy variable equals to one if financial advisor i in firm f at year *t+1* engages in misconduct and zero otherwise. In this paper, we use two measures of misconduct introduced by Dimmock, Gerken, and Graham 2018a and Egan, Matvos, and Seru 2019b.[16] The key variable of interest, $Top100_{i,s,f,t}$, is a dummy variable equals to one if financial advisor i in firm f at year t is in Barron's Top 100 Ranking and zero if the advisor is in Barron's Top 1000 Ranking at year t. The coefficient of interest in Equation 3.8 is β_1. If Barron's Top 100 Ranking has a disciplinary effect on financial advisors' behavior, then β_1 should be negative and statistically different from zero. Table 3.8 Column (2) reports the results from estimating Equation 3.8. Consistent with prior results from firm panel regressions, we find that being named a Top 100 advisor is associated with a 1.5 percentage point decline in financial advisor's likelihood of engaging in misconduct. The economic magnitude of the coefficient of interest is large: the unconditional average of misconduct in this sample is 1.4 percentage point.

To further investigate the effect of third party quality certification on misconduct, as in Section 3.3.3, we use state cutoffs to estimate Equation 3.9.

$$Misconduct_{i,f,s,t+1} = \beta + \beta_1 Top100_{i,f,s,t} + \theta Cont._{i,f,s,t} + \mu_f + +\mu_{s,t} + \epsilon_{i,f,s,t} \quad (3.9)$$

where $Misconduct_{i,f,s,t+1}$ is equal to one if financial advisor i in state s at year *t+1* engages in misconduct and zero otherwise. $Top100_{i,s,f,t}$ is equal to one if financial advisor i in state s at year t is right above the thresholds and zero if the advisor is right below the thresholds. μ_f is firm fixed effects and $\mu_{s,t}$ is state$\times$year fixed effects. Table 3.8 Column (3) shows that being listed in the Top 100 ranking is associated with a 5.2 percentage points decrease in the likelihood of engaging in misconduct. The economic magnitude of the coefficient of interest is large: the unconditional probability of misconduct in this sample is 2.2 percentage points. The results and inferences from our third approach are similar to our first two approaches, i.e., firm panel regression and individual panel regression. This may help confirm our empirical results regarding the predictions of theoretical models on the effect of reputation in the market for financial advice.

One concern about our results is that the decline in engaging in misconduct comes from changes in clients' propensity to complain not from changes in advisors' behavior. To address this concern, we re-estimate Equation 3.8 with regulatory misconduct (e.g., excluding customer complaints and firm terminations) as the dependant variable. In Table C.2, we find strong evidence that the third party quality certification shifts advisors' behavior.

[16] The measure of misconduct introduced by Dimmock, Gerken, and Graham 2018a includes only customer disputes. On the other hand, the measure of misconduct introduced by Egan, Matvos, and Seru 2019b covers customers, regulatory, civil, and criminal disputes. The results are robust for both measures.

3.5 Conclusion

We study the Barron's Top Advisor rankings to examine the role of third party quality certification in the market for financial advice. We find evidence at both the individual and firm level that clients value the certification based on their flows. By examining discontinuity around state-level cutoffs, we find that clients appear to react to the certification itself rather than the more nuanced information about the advisor's quality. Investors appear exhibit coarse thinking about rankings Mullainathan, Schwartzstein, and Shleifer 2008. We employ a similar design around alternative (placebo) cutoffs and find no significant differences suggesting that there is something special about being certified. While we cannot directly measure investor welfare, the fact that investors follow the certification itself is suggestive that this certification enables greater trust in the advisors Gennaioli, Shleifer, and Vishny 2015.

Further, consistent with theoretical models of reputation in the financial advisory industry, advisors appear alter their behavior due to the higher potential reputation cost of misconduct. These findings are counter to the idea that the rankings serve as a distraction or lead to a "Barron's curse". The disproportionately high percentage of customer complaints among Barron's list members due to in large part to the large scale of business and long careers of this population. Our within-advisor analysis shows that the risk of misconduct drops once an advisor is certified. This seemingly paradoxical result also illustrates why simple regulatory disclosures may be useless or even worse misleading to households: advisors with little or no experience will have clean histories on disclosure websites but may be of inferior quality to experienced advisors with minor dings on their records after a distinguished career. Currently, FINRA does not allow commercial users to "sell, lease, loan, distribute, transfer, or sublicense BrokerCheck or the data contained therein or access thereto or derive income from the use or provision thereof, whether for direct commercial or monetary gain or otherwise", which inhibits third-parties from collecting and certifying advisors on a greater scale. Overall, our findings have implications regarding the sophistication of the clientele of the financial advisory industry and the effect that the dissemination of quality information in financial advice markets can have on investor choice and market discipline.

(a)

(b)

(c)

(d)

(e)

Figure 3.1: Websites of Florida's 2018 Barron's Ranked Advisors

NOTE.——Top 100 Rank [Florida Rank] (a) Thomas Moran #27 [1^{st} in FL]; (b) Patrick Dwyer #30 [2^{nd}]; (c) Adam Carlin #65 [3^{rd}]; (d) Charles Mulfinger II #91 [4^{th}] ; and, (e) Louis Chiavacci Not ranked in Top 100 [5^{th}]

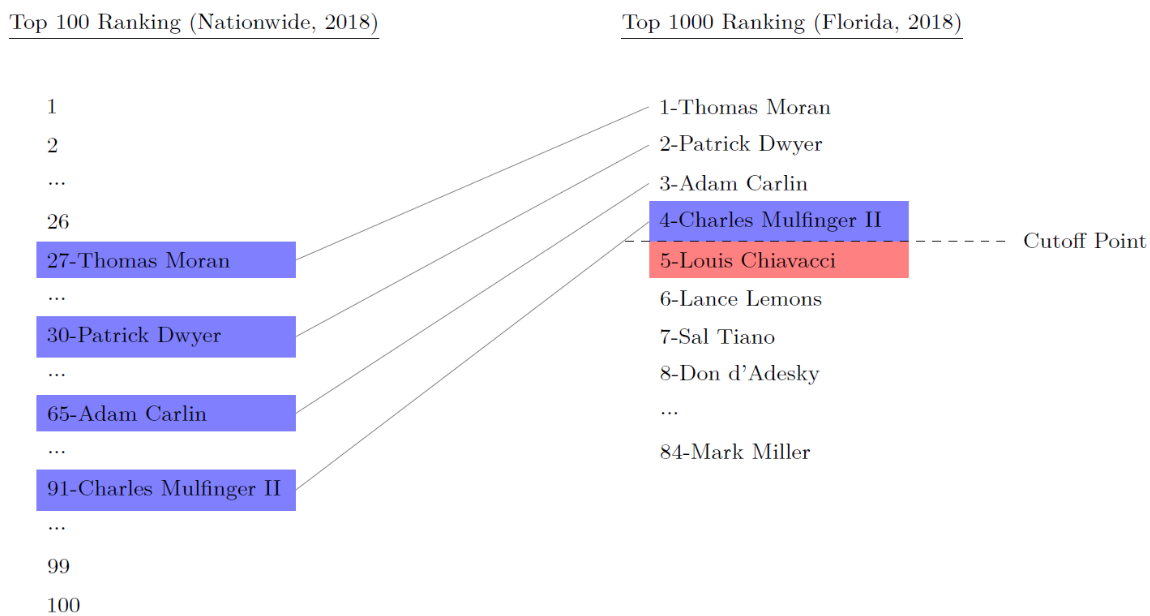


Figure 3.2: State-Level Cutoff Example - Florida 2018

This figure illustrates a state-level cutoff from the 2018 Barron's rankings in the state of Florida. In the main tests, the advisor who just makes the list in the state (Charles Mulfinger II) is considered treated, while the advisor who just misses (Louis Chivacci) is the control.

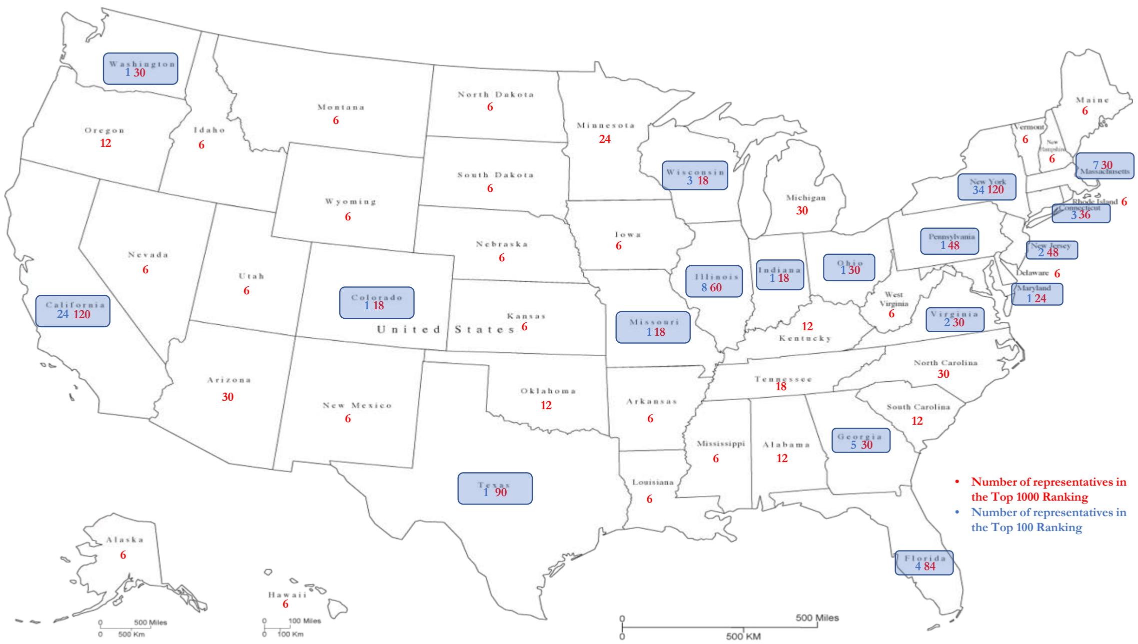
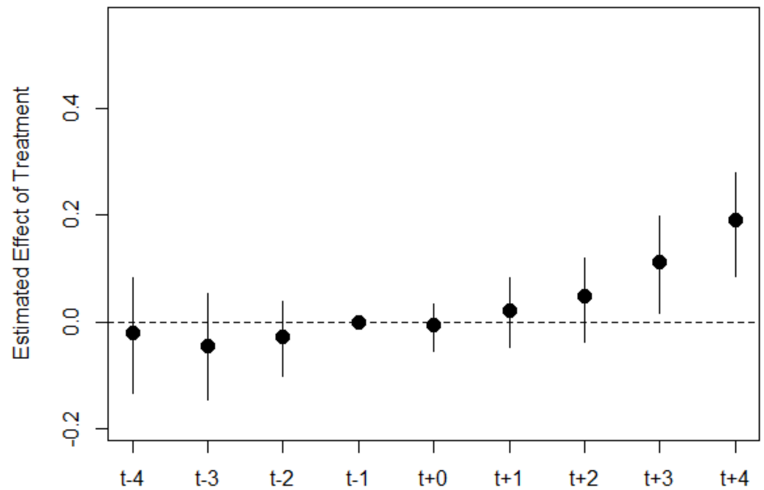
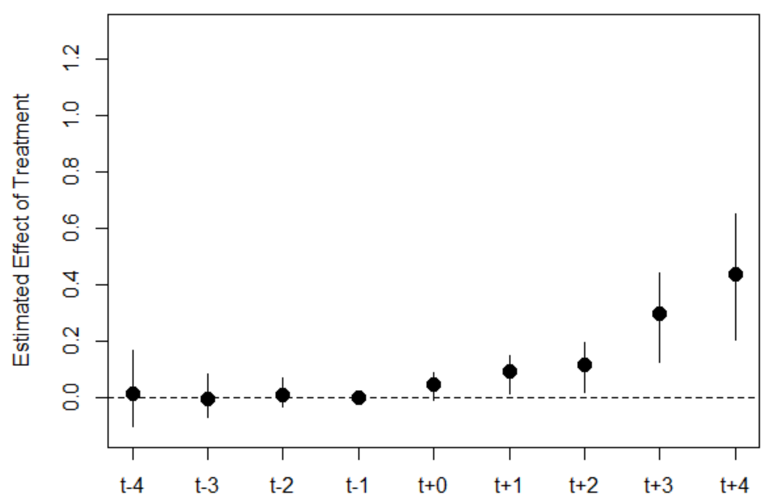


Figure 3.3: State Level Breakdown of Top Advisors
This figure displays the number of Top 100 ranked and Top 1000(1200) ranked advisors by state for the year 2018.



(a) Changes in Log(AUM) in Event Time



(b) Changes in Log(Accounts) in Event Time

Figure 3.4: Matching Estimates in Event Time - Firm Level
This figure shows the average treatment effect using the difference-in-difference estimator. The outcome variables are logarithm of AUM in Panel A and logarithm of number of accounts in Panel B. The Y-axis shows the estimated effect of treatment and the X-axis shows the event time. Treated firms are matched with control firms on past misconduct, lagged number of employees, lagged log(AUM), and lagged log(Accounts).

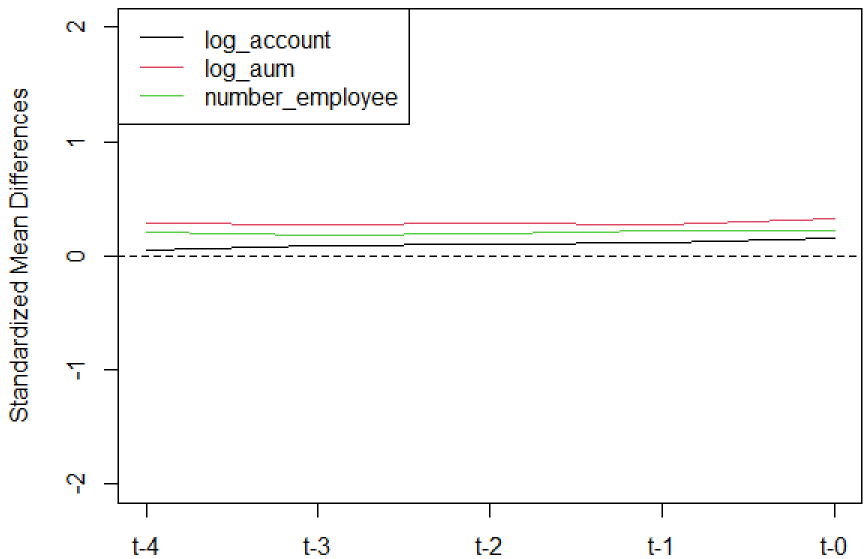


Figure 3.5: Covariate Balance - Firm Level
This figure shows the standardized mean differences for Log(AUM), Log(Accounts), and number of employees for the pre-treatement period (i.e. t-4 to t-0). The standardized mean difference is is a measure of distance between two group means in terms of one or more variables.

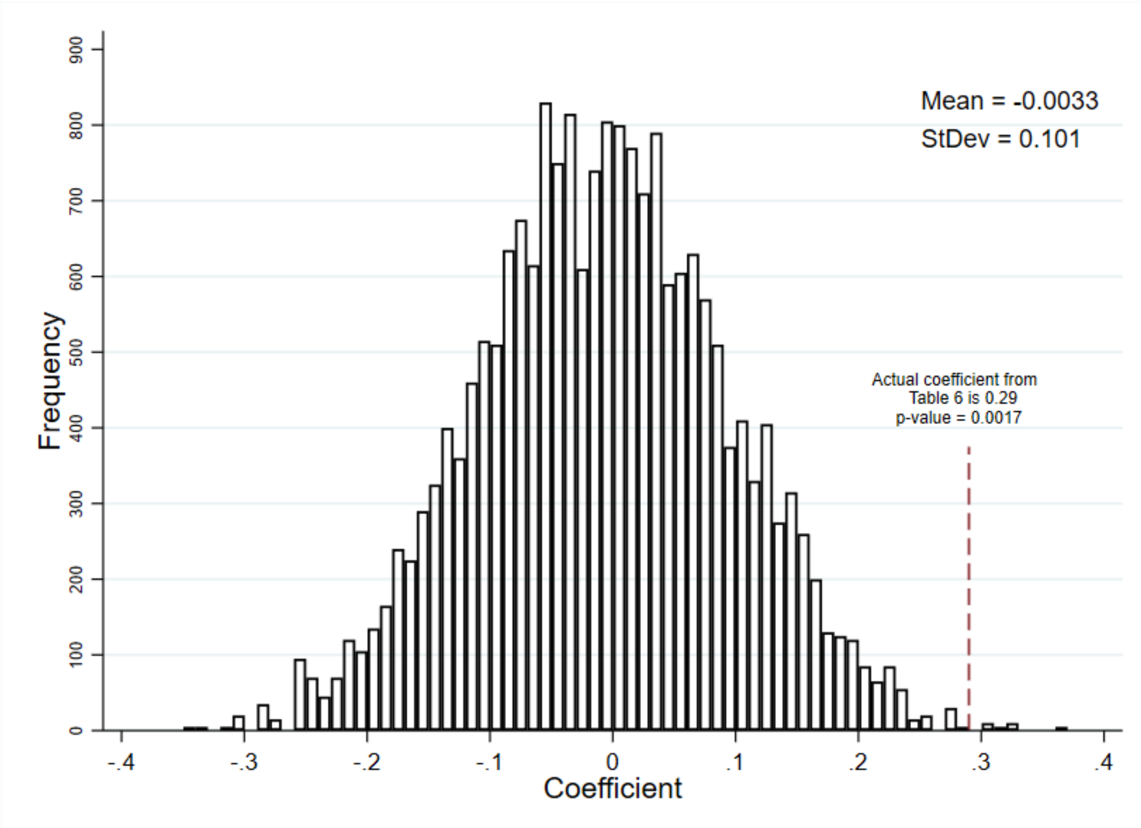


Figure 3.6: Placebo Threshold Test within Top 1000 for AUM

The figure shows a histogram of the magnitude of *Pseudo Top 100* from a model similar to the model in Table 3.6, Column (1) for 20,000 iterations with placebo thresholds. For each repetition, after excluding financial advisors in the Top 100 ranking from the Top 1000 ranking, we randomly assign a cutoff point in each state that has at least one representative in the Top 100 ranking. All other advisor characteristics remain the same. We then estimate the corresponding specification in Table 3.6, Column (1).

Top 1000 Ranking	Score	State
1	100.00	State 48
2	99.90	State 23
3	99.80	State 1
4	99.75	State 13
5	99.66	State 28
6	99.61	State 11
7	99.42	State 29
8	99.36	State 13
9	99.10	State 42
10	99.08	State 30
11	99.05	State 51
12	99.01	State 28
13	98.82	State 39
14	98.74	State 51
15	98.65	State 40
...	...	...
...	...	...
995	1.05	State 50
996	1.01	State 3
997	0.95	State 33
998	0.93	State 41
999	0.89	State 30
1000	0.30	State 19

State 1	
Top 1000 Ranking	Score
3	99.8
24	98
48	95.18
69	93.53
77	92.99
91	91.6
121	88.79
126	88.26
144	86.51
148	86.23
189	82.41
233	78.04
244	77.15
268	74.19
296	71.42
337	68.12
370	64.15
754	27.57
771	25.85
818	20.83
820	20.49
974	2.88

State 2	
Top 1000 Ranking	Score
16	98.65
46	95.44
85	92.03
99	90.96
193	82.09
267	74.24
273	73.91
274	73.81
366	64.34
416	59.33
441	56.4
454	54.93
467	54.28
555	45.03
654	36.57
660	36.35
724	30.14
758	27.17
767	26.57
865	14.85
968	3.4
975	2.86
978	2.47

...

State 49	
Top 1000 Ranking	Score
190	82.32
303	71.01
313	70.16
347	66.84
405	60.61
488	52.7
615	39.22
621	38.68
626	38.45
753	27.63
836	18.97
855	16.62
905	11.13
979	2.47

State 50	
Top 1000 Ranking	Score
58	94.54
168	84.18
216	79.53
218	79.44
335	68.39
368	64.2
394	62.11
418	58.79
534	48.13
610	39.77
678	34.65
756	27.24
800	23.12
856	16.43
923	9.11
969	3.32
995	1.05

State 51	
Top 1000 Ranking	Score
11	99.05
14	98.74
19	98.43
28	97.69
68	93.58
102	90.52
139	87.04
152	85.39
156	85.25
223	79.2
238	77.85
294	71.8
308	70.67
311	70.52
331	68.5
393	62.24
413	59.71
450	55.33
535	48.06
742	28.83
773	25.52
804	22.67
846	17.96
890	12.28

Figure 3.7: A Hypothetical Example

This figure displays a hypothetical example to illustrate our identification strategy. Red lines present state-level cutoffs. Dark blue cells represent advisors right above the cutoffs and light orange cells represent advisors right below the cutoffs. *Score* is a continuous measure to rank all advisors in Barron's Top 1000 (1200) Rankings.

Table 3.1: Descriptive statistics

Panel A: Firm Level	Full Sample			Firms without Top Advisor Owner			Firms with Top Advisor Owner		
	Mean	SD	Median	Mean	SD	Median	Mean	SD	Median
Assets Under Management (AUM)($bil)	1.18	6.89	0.22	1.18	6.92	0.22	1.15	1.33	0.79
Number of Accounts	615	12401	124	605	12456	120	1758	1618	1336
Number of Employees	10.67	10.60	7.00	10.59	10.55	7.00	19.10	11.80	17.00
Misconduct	0.0042	0.0645	0.00	0.0042	0.0646	0.00	0.0026	0.0516	0.00

Panel B: Advisor Level	Full Sample			Top 1000 (but not Top 100)			Top 100		
	Mean	SD	Median	Mean	SD	Median	Mean	SD	Median
Advisor Characteristics									
Assets Under Management ($bil)	2.27	6.09	1.06	1.72	4.35	0.96	7.89	13.71	3.99
Number of accounts	488	1015	260	467	904	258	708	1779	277
Media Coverage	0.14	1.03	0	0.12	0.94	0	0.38	1.68	0
Experience (years)	25.83	8.24	26	25.69	8.24	26	27.25	8.05	27
Misconduct	0.01	0.11	0	0.01	0.11	0	0.02	0.14	0
Advisor Licensing									
Series 6	0.08	0.27	0	0.08	0.27	0	0.07	0.26	0
Series 7	0.94	0.23	1	0.95	0.23	1	0.94	0.24	1
Series 24	0.13	0.34	0	0.13	0.34	0	0.13	0.34	0
Series 26	0.01	0.09	0	0.01	0.09	0	0.01	0.09	0
Series 63	0.92	0.28	1	0.91	0.28	1	0.95	0.21	1
Series 65	0.73	0.45	1	0.72	0.45	1	0.75	0.43	1
Series 66	0.14	0.35	0	0.14	0.35	0	0.12	0.32	0
Client Type									
Individuals (<$1mil)	0.65	0.48	1	0.68	0.47	1	0.33	0.47	0
High Net Worth ($1-10 mil)	0.95	0.21	1	0.96	0.18	1	0.83	0.38	1
Ultra-High Net Worth (>$10 mil+)	0.9	0.3	1	0.89	0.31	1	0.98	0.14	1
Foundations	0.34	0.47	0	0.33	0.47	0	0.5	0.5	0
Corporation	0.01	0.11	0	0.01	0.11	0	0.02	0.13	0
Endowments	0.21	0.4	0	0.2	0.4	0	0.28	0.45	0
Institutional	0.31	0.46	0	0.31	0.46	0	0.31	0.46	0

NOTE.—This table reports the summary statistics corresponding to the panel of all financial advisory firms with regulatory asset under management of $100 million or more (Panel A) and the panel of individual financial advisors ever in the Barron's Top 100 and Top 1000(1200) Rankings from 2009 to 2020. The unit of observation in Panel A (B) is the firm-year (advisor-year) level. The panel of firms consists of 127,229 observations and all variables from ADV forms are observed as of April 1 of the year. The panel of individual financial advisors in Barron's Top 100 and Top 1000 Rankings consists of 13,444 observations and all variables from Brokercheck are observed as of January 1 of the year. Also, all variables from Barron's are observed as of October 1 of the prior year. *AUM* is an advisor's asset under management in a given year. *Misconduct* is a dummy variable equal to 1 if a financial advisor engages in misconduct, as defined in Egan, Matvos, and Seru 2019b, and zero otherwise. *Media Coverage* is the logarithm of one plus the total number of articles resulted from searching a financial advisor's full name in LexisNexis for the year starting April 1 (after publication of Barron's Top 100 Ranking).

Table 3.2: Firm Level Analysis

	(1) Log(AUM)	(2) Log(AUM)	(3) Log(Account)	(4) Log(Account)
Top Advisor Owner	0.917***	0.0940**	1.933***	0.106**
	(0.0731)	(0.0386)	(0.1106)	(0.0457)
Firm FE		Yes		Yes
State×Year FE	Yes	Yes	Yes	Yes
R^2	0.088	0.904	0.132	0.960
Observations	95,234	92,911	95,234	92,911

NOTE.—This table shows the results for the panel of financial advisory firms. The data consists of all financial advisory firms with regulatory asset under management of $100 million or more. The dependent variable in Columns (1) and (2) is logarithm of asset under management at year $t+1$. The dependent variable in Columns (3) and (4) is logarithm of one plus number of accounts at year $t+1$. *Top Advisor Owner* is a dummy variable equals to 1 if a firm's owner is in Barron's Top 1000 Ranking at time t and zero otherwise. Observations are at the firm-by-year level over the period 2009-2020. Robust standard errors are in parentheses and are clustered by firm. ***,**, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Table 3.3: Individual Level Panel Regressions

Panel A: Assets Under Management	(1) Log(AUM)	(2) Log(AUM)	(3) Log(AUM)	(4) Log(AUM)	(5) Log(AUM)
Top 100 Indicator			0.169***	0.131***	0.163***
			(0.0256)	(0.0313)	(0.0347)
Normalized Rank	1.119***	0.211***	0.193***	0.217***	0.201***
	(0.1006)	(0.0418)	(0.0399)	(0.0356)	(0.0599)
Controls		Yes	Yes	Yes	Yes
Advisor FE		Yes	Yes	Yes	Yes
Firm FE				Yes	
State×Year FE				Yes	
Firm×State×Year FE					Yes
R^2	0.119	0.929	0.930	0.951	0.957
Observations	9,889	9,083	9,083	9,069	7,135

Panel B: Number of Accounts	(1) Log(Account)	(2) Log(Account)	(3) Log(Account)	(4) Log(Account)	(5) Log(Account)
Top 100 Indicator			0.111***	0.0842**	0.106**
			(0.0359)	(0.0377)	(0.0426)
Normalized Rank	0.321***	0.102***	0.0903***	0.0838*	0.0802
	(0.1087)	(0.0349)	(0.0342)	(0.0424)	(0.0761)
Controls		Yes	Yes	Yes	Yes
Advisor FE		Yes	Yes	Yes	Yes
Firm FE				Yes	
State×Year FE				Yes	
Firm×State×Year FE					Yes
R^2	0.008	0.893	0.893	0.906	0.922
Observations	9,888	9,082	9,082	9,068	7,134

Panel C: Media Coverage	(1) Media coverage	(2) Media coverage	(3) Media coverage	(4) Media coverage	(5) Media coverage
Top 100 Indicator			0.0610**	0.0450	0.0357
			(0.0287)	(0.0285)	(0.0258)
Normalized Rank	0.0828***	0.0249	0.0194	0.0332**	0.0414**
	(0.0191)	(0.0170)	(0.0164)	(0.0149)	(0.0201)
Controls		Yes	Yes	Yes	Yes
Advisor FE		Yes	Yes	Yes	Yes
Firm FE				Yes	
State×Year FE				Yes	
Firm×State×Year FE					Yes
R^2	0.009	0.472	0.473	0.524	0.590
Observations	8,275	7,540	7,540	7,519	6,008

NOTE.—This table shows the results for the panel of individual financial advisors. The dependent variable in Panel A is logarithm of assets under management at year $t + 1$ (measured after publication of the rankings). The dependent variable in Panel B is logarithm of one plus number of accounts at year $t + 1$ (measured after publication of the rankings). The dependent variable in Panel C is logarithm of one plus number of articles resulted from searching a financial advisor's full name in LexisNexis at year $t + 1$ (measured after publication of the rankings). *Top 100 Indicator* is a dummy variable equals to 1 if an advisor is in Barron's Top 100 Ranking at time t and zero otherwise. *Normalized Rank* is a continuous variable between zero and one. It measures an advisor's quality at the state level. *Controls* include experience and tests (Series 6, 7, 24, 65, and 66). Observations are at the advisor-by-year level from 2009 to 2020. Robust standard errors are in parentheses and are clustered by firm. ***,**, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Table 3.4: Cross Sectional Tests

	(1) Log(AUM)	(2) Log(AUM)	(3) Log(AUM)
Top 100 Indicator	0.0979**	0.0836**	0.120***
	(0.0381)	(0.0370)	(0.0260)
Top 100 Indicator X Small Firms	0.0978**		
	(0.0430)		
Young Advisors		-0.124**	
		(0.0491)	
Top 100 Indicator X Young Advisors		0.101***	
		(0.0332)	
Past Misconduct			0.0402
			(0.0395)
Top 100 Indicator X Past Misconduct			0.0456
			(0.0683)
Normalized Rank	0.224***	0.217***	0.219***
	(0.0444)	(0.0359)	(0.0361)
Controls	Yes	Yes	Yes
Advisor FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
State×Year FE	Yes	Yes	Yes
R^2	0.951	0.951	0.951
Observations	7,914	9,069	9,069

NOTE.—This table shows the effect of being in Barron's Top Ranking across firm size, advisors' years of experience, and advisors' history of past misconduct. The dependent variable is logarithm of assets under management at year $t+1$. *Top 100 Indicator* is a dummy variable equals to 1 if an advisor is in Barron's Top 100 Ranking at time t and zero otherwise. *Small Firms* is a dummy variable equals to 1 if a firm has only one advisor in Barron's Top 1000 ranking in year 2009 and zero if the firm has more than one advisor in Barron's Top 1000 ranking in year 2009.*Young Advisors* is a dummy variable equals to 1 if an advisor's experience is in the lowest quartile and zero otherwise. *Past Misconduct* is a dummy variable equals to 1 if an advisor has engaged in misconduct before time t and zero otherwise.*Normalized Rank* is a continuous variable between zero and one. It measures an advisor's quality at the state level. *Controls* include experience (with the exception of Column(2)) and tests (series 6, 7, 24, 65, and 66). For Column (1), observations are at the advisor-by-year level over the period 2010-2020. For Columns (2) and (3), observations are at the advisor-by-year level over the period 2009-2020. Robust standard errors are in parentheses and are clustered by firm. ***,**, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Table 3.5: Descriptive statistics for the advisors around the cutoff

	Treatment Just Above Cutoff	Control Just Below Cutoff	Difference
Log(AUM)	7.94	7.93	-0.01
	(0.058)	(0.069)	(0.081)
Experience (years)	26.51	24.41	2.10**
	(0.650)	(0.822)	(1.036)
Misconduct	0.007	0.023	-0.017
	(0.007)	(0.013)	(0.017)
Individual	0.424	0.462	-0.038
	(0.039)	(0.039)	(0.062)
High Net Worth	0.879	0.898	-0.019
	(0.025)	(0.024)	(0.037)
Ultra-High Net Worth	0.987	0.962	0.025
	(0.009)	(0.015)	(0.017)
Foundations	0.386	0.392	-0.006
	(0.039)	(0.038)	(0.052)
Endowments	0.227	0.259	-0.032
	(0.033)	(0.034)	(0.042)
Institutional	0.291	0.291	0.00
	(0.036)	(0.036)	(0.047)
Observations	158	158	

NOTE.—This table reports the summary statistics of the variables used in the study for financial advisors who are just above the cutoff (*Treatment*) and advisors who are just below the state-year cutoff (*Control*) at time t (measured before publication of the rankings).

Table 3.6: State-Level Cutoffs

	(1) Log(AUM)	(2) Log(AUM)	(3) Log(AUM)
Top 100 Indicator	0.290**	0.215***	0.308***
	(0.1107)	(0.0624)	(0.1043)
Normalized Rank	0.456	-0.795	-0.375
	(1.5380)	(1.3283)	(0.7533)
Controls	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
State×Year FE	Yes	Yes	Yes
Bandwidth	+/- 1	+/- 1	Matched #
Sample	All states	Exclude states w/ only 1 Top 100 advisor	Exclude Top 50 advisors
R^2	0.802	0.841	0.506
Observations	240	160	746

NOTE.—This table reports the estimation results from advisors who are right above the state-level cutoffs and advisors who are right below the state-level cutoffs. The dependent variable is logarithm of assets under management. *Top 100 Indicator* is a dummy variable equals to 1 if a financial advisor is right above the threshold and 0 if the advisor is right below the threshold. *Normalized Rank* is a continuous variable between zero and one. It measures an advisor's quality at the state level. *Controls* include experience and tests (series 6, 7, 24, 65, and 66). Column (1) shows the results when the sample includes all advisors. Column (2) shows the results when we exclude states with only one advisor in Barron's Top 100 Ranking. Column (3) shows the results when we exclude elite advisors (i.e., top 50 advisors in Barron's Top 100 Ranking) from our sample and expand advisors in the control group by the same number of advisors who are ranked from 51 to 100 in a given state. Observations are at the advisor-by-year level over the period 2009-2020. Robust standard errors are in parentheses and are clustered by firm. ***,**, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Table 3.7: Placebo Tests for Alternative Cutoffs within Top 100

Placebo Cutoff	25th	25th	50th	50th	75th	75th
Above Cutoff Indicator	0.031	0.025	0.069	0.083	0.111	-0.051
	(0.104)	(0.098)	(0.084)	(0.081)	(0.099)	(0.099)
Barron's Score	0.068***	0.067***	0.048***	0.062***	0.019	0.360***
	(0.013)	(0.020)	(0.007)	(0.012)	(0.034)	(0.064)
Cutoff × Barron's Score		-0.006		-0.021		-0.404***
		(0.026)		(0.014)		(0.070)
Controls		Yes		Yes		Yes
Observations	633	631	1,229	1,226	609	608
R^2	0.109	0.161	0.118	0.163	0.007	0.120

NOTE.—This table reports the results from the placebo tests with arbitrary thresholds in the Top 100 ranking. In these placebo tests, we only include advisors in the Top 100ranking since we do not have the ranking for the financial advisors who are not in the Top 100 ranking. We use three alternative thresholds; 25^{th}, 50^{th}, and 75^{th}. The dependent variable is logarithm of asset under management in year $t+1$ (measured after publication of the rankings). *Above Cutoff Indicator* is a dummy variable equals to 1 if a financial advisor is above the arbitrary threshold and zero otherwise. *Controls* include experience and tests (Series 6, 7, 24, 65, and 66). Observations are at the advisor-by-year level over the period 2009-2020. Robust standard errors are in parentheses and are clustered by running variable. ***,**, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Table 3.8: Misconduct

	(1) Misconduct	(2) Misconduct	(3) Misconduct
Top Advisor Owner	-0.00815*		
	(0.0046)		
Top 100 Indicator		-0.0153**	-0.0519**
		(0.0068)	(0.0225)
Normalized Rank		0.00794	0.353
		(0.0074)	(0.2317)
Advisor FE		Yes	
Firm FE	Yes	Yes	Yes
State×Year FE	Yes	Yes	Yes
Design	Firm Panel	Advisor Panel	State Cutoffs
R^2	0.286	0.292	0.615
Observations	95,237	11,431	242

NOTE.—This table shows the effect of being in Barron's ranking on misconduct.The dependent variable in Column (1) is a dummy variable indicating whether a firm has any disciplinary events at year $t + 1$. The dependent variable in Columns (2) and (3) is a dummy variable indicating if an advisor has any disciplinary events at year $t + 1$. *Top Advisor Owner* is a dummy variable equals to 1 if a firm's owner is in Barron's Top 1000 Ranking at time t and zero otherwise. *Top 100 Indicator* is a dummy variable equals to 1 if an advisor is in Barron's Top 100 Ranking at time t and zero otherwise. *Normalized Rank* is a continuous variable between zero and one. It measures an advisor's quality at the state level. Observations in Column (1) are at the firm-by-year level over the period 2009-2020. Observations in Columns (2) and (3) are at the advisor-by-year level over the period 2009-2020. Robust standard errors are in parentheses and are clustered by firm. ***,**, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Chapter 1 Internet Appendix

Table A.1: Inter-brand vs. intra-brand competition

Panel A: Vehicle body type	(1) Vehicle price	(2) Loan markup	(3) Monthly payment	(4) 24-month default
Number of dealers	-263.4535***	0.1651***	-4.3872	0.0557
	(-3.01)	(6.59)	(-1.45)	(1.13)
RMA FE	Yes	Yes	Yes	Yes
Developable land FE	Yes	Yes	Yes	Yes
Lender×Time FE	Yes	Yes	Yes	Yes
Vehicle Type×Time FE	Yes	Yes	Yes	Yes
Credit Score×Time FE	Yes	Yes	Yes	Yes
Income×Time FE	Yes	Yes	Yes	Yes
Observations	91,404	91,404	91,404	66,743

Panel B: Car value bins	(1) Vehicle price	(2) Loan markup	(3) Monthly payment	(4) 24-month default
Number of dealers	-90.0919	0.1649***	-1.2608	0.0511
	(-1.36)	(6.34)	(-0.45)	(1.04)
RMA FE	Yes	Yes	Yes	Yes
Developable land FE	Yes	Yes	Yes	Yes
Lender×Time FE	Yes	Yes	Yes	Yes
Car Value×Time FE	Yes	Yes	Yes	Yes
Credit Score×Time FE	Yes	Yes	Yes	Yes
Income×Time FE	Yes	Yes	Yes	Yes
Observations	91,404	91,404	91,404	66,743

NOTE.—This Table reports the effect of inter-brand competition on vehicle prices and loan markups. The effect is simultaneously estimated using Equations 3.4 and 3.6. Panel A reports the coefficient of interest after the inclusion of vehicle body type fixed effects ($\delta_{vbt,t}$) instead of vehicle fixed effects ($\delta_{v,t}$). Panel B reports the coefficient of interest after the inclusion of car value fixed effects ($\delta_{cv,t}$) instead of vehicle fixed effects ($\delta_{v,t}$). The dependent variable in Column (1) is vehicle prices. The dependent variable in Column (2) is loan markups. The dependent variable in Column (3) is the 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 income fixed effects ($\delta_{i,t}$) are defined as $50,000 income bins and the credit score fixed effects ($\delta_{c,t}$) are defined as a 25-point credit bins. The vehicle body type fixed effects ($\delta_{vbt,t}$) refer to sedan, coupe, sports car, station wagon, hatchback, convertible, sport-utility vehicle (SUV), minivan, and pickup truck. The car value fixed effects ($\delta_{cv,t}$) refer to car value quartiles. The developable land fixed effects (δ_{dl}) refer to developable land quartiles. The rma fixed effects (δ_{rma}) refers to the relevant market area defined under state franchise laws. t-statistics, presented below the coefficient estimates, are calculated by clustering at the state level.

Table A.2: Alternative clustering

Panel A: Vehicle prices	(1) Vehicle price	(2) Vehicle price	(3) Vehicle price	(4) Vehicle price
Number of dealers	-88.5898**	-88.5898**	-88.5898***	-88.5898***
	(-2.16)	(-2.36)	(-2.72)	(-3.23)
RMA FE	Yes	Yes	Yes	Yes
Developable land FE	Yes	Yes	Yes	Yes
Lender×Time FE	Yes	Yes	Yes	Yes
Vehicle×Time FE	Yes	Yes	Yes	Yes
Credit Score×Time FE	Yes	Yes	Yes	Yes
Income×Time FE	Yes	Yes	Yes	Yes
Alternative clustering	State lender	Lender	Vehicle	Year quarter
Observations	91,307	91,307	91,307	91,307
Panel B: Loan markups	**(1) Loan markup**	**(2) Loan markup**	**(3) Loan markup**	**(4) Loan markup**
Number of dealers	0.1679***	0.1679***	0.1679***	0.1679***
	(6.78)	(9.54)	(8.57)	(8.96)
RMA FE	Yes	Yes	Yes	Yes
Developable land FE	Yes	Yes	Yes	Yes
Lender×Time FE	Yes	Yes	Yes	Yes
Vehicle×Time FE	Yes	Yes	Yes	Yes
Credit Score×Time FE	Yes	Yes	Yes	Yes
Income×Time FE	Yes	Yes	Yes	Yes
Alternative clustering	State lender	Lender	Vehicle	Year quarter
Observations	91,307	91,307	91,307	91,307
Panel C: Monthly payment	**(1) Monthly payment**	**(2) Monthly payment**	**(3) Monthly payment**	**(4) Monthly payment**
Number of dealers	-1.1733	-1.1733	-1.1733	-1.1733
	(-0.69)	(-1.22)	(-0.82)	(-1.00)
RMA FE	Yes	Yes	Yes	Yes
Developable land FE	Yes	Yes	Yes	Yes
Lender×Time FE	Yes	Yes	Yes	Yes
Vehicle×Time FE	Yes	Yes	Yes	Yes
Credit Score×Time FE	Yes	Yes	Yes	Yes
Income×Time FE	Yes	Yes	Yes	Yes
Alternative clustering	State lender	Lender	Vehicle	Year quarter
Observations	91,307	91,307	91,307	91,307

NOTE.—This Table reports the effect of local competition among auto dealers on vehicle prices and loan markups for different clustering schemes, including state-lender, lender, vehicle, and year-quarter levels. The effect is simultaneously estimated using Equations 3.4 and 3.6. In Panel A, the dependent variable is vehicle prices. In Panel B, the dependent variable is loan markups. The income fixed effects ($\delta_{i,t}$) are defined as \$50,000 income bins and the credit score fixed effects ($\delta_{c,t}$) are defined as a 25-point credit bins. Vehicle fixed effects ($\delta_{v,t}$) refer to vehicle make-model-model year combinations. The developable land fixed effects (δ_{dl}) refer to developable land quartiles. The rma fixed effects (δ_{rma}) refers to the relevant market area defined under state franchise laws. t-statistics, presented below the coefficient estimates, are calculated by clustering at the state level.

Table A.3: Robust to different bin sizes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Bin size 1		Bin size 2		Bin size 3		Bin size 4	
	LoanMarkup	LoanMarkup	LoanMarkup	LoanMarkup	LoanMarkup	LoanMarkup	LoanMarkup	LoanMarkup
Num.dealers	0.0085***	0.0312***	0.0219***	0.0623***	0.1115***	0.1130***	0.2795***	0.2179***
	(5.12)	(4.30)	(5.60)	(4.27)	(6.83)	(5.17)	(7.13)	(5.63)
RMA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Developable land FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lender×Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Vehicle×Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Credit Score×Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income×Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	136,312	19,600	136,306	25,144	137,139	66,825	137,400	119,996
Average cell size	1.152	2.071	1.209	2.143	2.392	3.871	39.334	44.986
Unconditional markup	0.024	0.166	0.050	0.273	0.353	0.728	2.355	2.702

NOTE.—This Table reports the effect of local competition among auto dealers on loan markups for different bin sizes. In *Bin size 1*, I calculate loan markups while conditioning on loan contract terms such as loan to value ratio and loan maturity. In Column (2), I require each cell has at least two observations. In *Bin size 2*, I calculate loan markups after applying tighter filters such as 5-point credit bins and \$10,000 income bins. In Column (4), I require each cell has at least two observations. In *Bin size 3*, I calculate loan markups after applying generous filters such as 10-point credit bins and \$25,000 income bins. In Column (6), I require each cell has at least two observations. In *Bin size 4*, I calculate loan markups after applying more generous filters such as 50-credit bins, \$50,000 income bins, and semi annual-year bins. In Column (8), I require each cell has at least two observations. The income fixed effects ($\delta_{i,t}$) are defined as \$50,000 income bins and the credit score fixed effects ($\delta_{c,t}$) are defined as a 25-point credit bins. Vehicle fixed effects ($\delta_{v,t}$) refer to vehicle make-model-model year combinations. The developable land fixed effects (δ_{dl}) refer to developable land quartiles. The rma fixed effects (δ_{rma}) refers to the relevant market area defined under state franchise laws. t-statistics, presented below the coefficient estimates, are calculated by clustering at the state level.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

Table A.4: Adjusted sample filters

Panel A: Vehicle prices	(1) Vehicle price	(2) Vehicle price	(3) Vehicle price	(4) Vehicle price	(5) Vehicle price	(6) Vehicle price
Number of dealers	-89.9997**	-90.2988**	-93.5721**	-79.4367*	-131.5225***	-990.1156
	(-2.04)	(-2.11)	(-1.98)	(-1.85)	(-3.03)	(-0.73)
Sample filter	+500	+650	+700	1*mill*	Non-zero markup	DL decile FE
RMA FE	Yes	Yes	Yes	Yes	Yes	Yes
Developable land FE	Yes	Yes	Yes	Yes	Yes	Yes
Lender×Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Vehicle×Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Credit Score×Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Income×Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	93,643	88,654	75,391	93,116	74,848	92,047

Panel B: Loan markups	(1) Loan markup	(2) Loan markup	(3) Loan markup	(4) Loan markup	(5) Loan markup	(6) Loan markup
Number of dealers	0.2149***	0.1841***	0.1619***	0.2070***	0.1436***	0.0904
	(5.91)	(5.50)	(5.05)	(5.88)	(5.46)	(0.35)
Sample filter	+500	+650	+700	1*mill*	Non-zero markup	DL decile FE
RMA FE	Yes	Yes	Yes	Yes	Yes	Yes
Developable land FE	Yes	Yes	Yes	Yes	Yes	Yes
Lender×Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Vehicle×Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Credit Score×Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Income×Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	93,643	88,654	75,391	93,116	74,848	92,047

NOTE.—This Table reports the effect of local competition among auto dealers on vehicle prices and loan markups after adjusting sample filters. The effect is simultaneously estimated using Equations 3.4 and 3.6. In Panel A, the dependent variable is vehicle prices. In Panel B, the dependent variable is loan markups. In Columns (1) through (3), I restrict my sample to auto loans with a credit score of +500, +650, and +700. In Column (4), I restrict my sample to borrowers with an income of $1 million or less. In Column (5), I restrict the estimation sample to auto loans with non-zero markups. In Column (6), I include the developable land decile fixed effects. The income fixed effects ($\delta_{i,t}$) are defined as $50,000 income bins and the credit score fixed effects ($\delta_{c,t}$) are defined as a 25-point credit bins. Vehicle fixed effects ($\delta_{v,t}$) refer to vehicle make-model-model year combinations. The developable land fixed effects (δ_{dl}) refer to developable land quartiles. The rma fixed effects (δ_{rma}) refers to the relevant market area defined under state franchise laws. t-statistics, presented below the coefficient estimates, are calculated by clustering at the state level.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

Table A.5: Robust to different number of observations in each cell

	(1) Loan markup	(2) Loan markup	(3) Loan markup
Number of dealers	0.1328***	0.2783*	0.5200
	(3.49)	(1.88)	(1.81)
Min obs in cell	5	20	30
Unconditional markup	2.823	3.649	3.917
RMA FE	Yes	Yes	Yes
Developable land FE	Yes	Yes	Yes
Lender×Time FE	Yes	Yes	Yes
Vehicle×Time FE	Yes	Yes	Yes
Credit Score×Time FE	Yes	Yes	Yes
Income×Time FE	Yes	Yes	Yes
Observations	58,997	16,593	8,777

NOTE.—This Table reports the effect of local competition among auto dealers on vehicle prices and loan markups conditional on different number of observations in each cell. The income fixed effects ($\delta_{i,t}$) are defined as \$50,000 income bins and the credit score fixed effects ($\delta_{c,t}$) are defined as a 25-point credit bins. Vehicle fixed effects ($\delta_{v,t}$) refer to vehicle make-model-model year combinations. The developable land fixed effects (δ_{dl}) refer to developable land quartiles. The rma fixed effects (δ_{rma}) refers to the relevant market area defined under state franchise laws. t-statistics, presented below the coefficient estimates, are calculated by clustering at the state level.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

Table A.6: Subvented loans

	(1) Subvented loan	(2) Subvented loan	(3) Subvented loan
Potential number of dealers	-0.0014	-0.0006	0.0006
	(-0.07)	(-0.16)	(0.15)
RMA FE	Yes	Yes	Yes
Developable land FE	Yes	Yes	Yes
Vehicle×Time FE	Yes	Yes	Yes
Lender×Time FE		Yes	Yes
Credit Score×Time FE			Yes
Income×Time FE			Yes
R^2	0.461	0.524	0.531
Observations	450,108	450,108	450,108

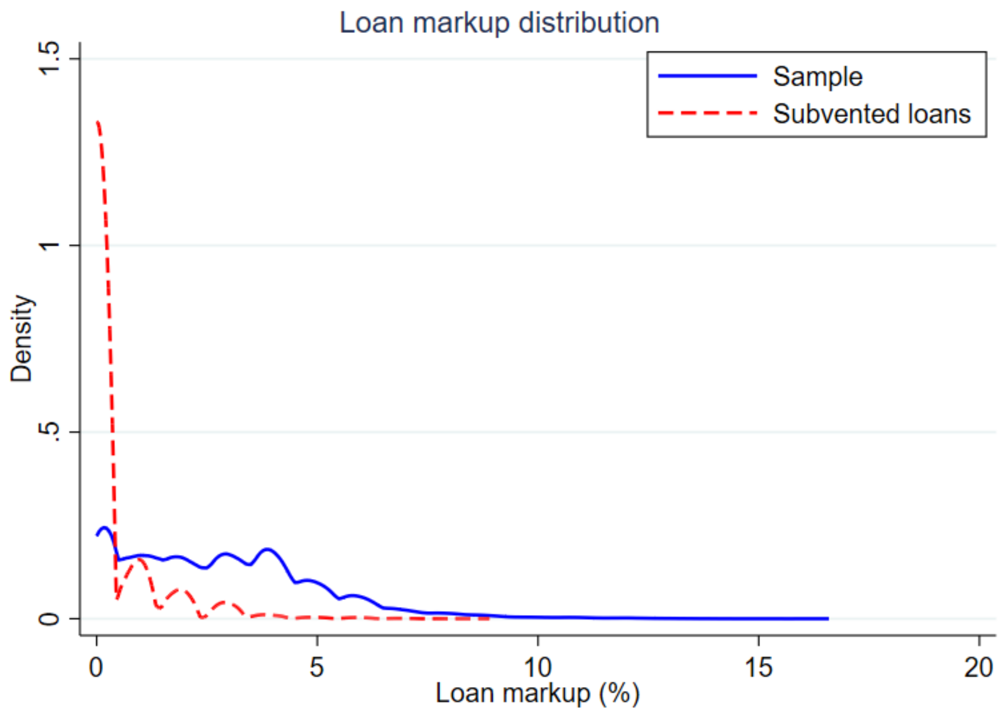
NOTE.—This Table reports the correlation between the instrumental variable and subvented loans. *Subvented loan* is a dummy variable equals to 1 if a loan has either cash or rate rebates and zero otherwise. The income fixed effects ($\delta_{i,t}$) are defined as $50,000 income bins and the credit score fixed effects ($\delta_{c,t}$) are defined as a 25-point credit bins. Vehicle fixed effects ($\delta_{v,t}$) refer to vehicle make-model-model year combinations. The developable land fixed effects (δ_{dl}) refer to developable land quartiles. The rma fixed effects (δ_{rma}) refers to the relevant market area defined under state franchise laws. t-statistics, presented below the coefficient estimates, are calculated by clustering at the state level.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

Table A.7: OLS regressions

	(1) Vehicle price	(2) Loan markup	(3) Monthly payment
Number of dealers	-47.0987**	0.1122***	0.9046
	(-2.14)	(7.36)	(0.82)
RMA FE	Yes	Yes	Yes
Developable land FE	Yes	Yes	Yes
Vehicle×Time FE	Yes	Yes	Yes
Lender×Time FE	Yes	Yes	Yes
Credit Score×Time FE	Yes	Yes	Yes
Income×Time FE	Yes	Yes	Yes
R^2	0.769	0.575	0.426
Observations	91,307	91,307	91,307

NOTE.—This Table reports the effect of local competition among auto dealers on vehicle prices and loan markups using an OLS regression. The income fixed effects ($\delta_{i,t}$) are defined as $50,000 income bins and the credit score fixed effects ($\delta_{c,t}$) are defined as a 25-point credit bins. Vehicle fixed effects ($\delta_{v,t}$) refer to vehicle make-model-model year combinations. The developable land fixed effects (δ_{dl}) refer to developable land quartiles. The rma fixed effects (δ_{rma}) refers to the relevant market area defined under state franchise laws. t-statistics, presented below the coefficient estimates, are calculated by clustering at the state level.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

Figure A.1: Distribution of loan markups



NOTE.——This figure plots the distribution of loan markups for my main sample as well as a sample including subvented loans. The x-axis presents loan markups in percentage. The blue line represents the kernel density distribution of loan markups for my main sample. The dotted red line represents the kernel density distribution of loan markups for a sample including only subvented loans. *subvented loans* refers to loans with cash or rate rebates.

Chapter 2 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 2.4.8.

2.A.1 Robustness

2.A.1.1 Standard errors and fixed effects

In Table B.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 B.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 B.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 2020a).

2.A.1.2 Sample filters

As a final robustness test, we confirm that our sample filters do not affect our conclusions. In Table B.13, we re-estimate our baseline difference-in-differences model after adjusting the sample filters that were applied in Section 2.3.2. 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.[17]

We also re-estimate our baseline model after reincluding the removed lenders from Section 2.3.2, and again after removing World Omni from the sample (see Footnote 20). As shown in Tables B.14 and B.15, we find that our main results continue to persist.

[17] 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 B.1: Comparison of loan terms in Regulation AB II data and population credit bureau data

Panel A: All lenders

	Mean	SD	P25	P50	P75
	(1)	(2)	(3)	(4)	(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	SD	P25	P50	P75
	(1)	(2)	(3)	(4)	(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 B.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 2.2. 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 B.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 2.9 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 B.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 2.2 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 B.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 2.2 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 B.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 2.2. 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 B.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 2.2. 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 B.8: Alternative explanation: Changes in securitization practices

Panel A: All lenders

	Originations (1)	Loan amount (2)	Loan maturity (3)	Monthly payment (4)
Treated × Post	0.04	0.02	0.00	0.00
	(0.33)	(0.52)	(0.01)	(0.09)
Lender FE	Y	Y	Y	Y
Quarter FE	Y	Y	Y	Y
R^2	0.72	0.72	0.80	0.77

Panel B: Restricted sample of lenders

	Originations (1)	Loan amount (2)	Loan maturity (3)	Monthly payment (4)
Treated × Post	0.01	0.02	-0.01	0.00
	(0.06)	(0.44)	(-0.64)	(-0.21)
Lender FE	Y	Y	Y	Y
Quarter FE	Y	Y	Y	Y
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 B.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 2.2 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 B.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 2.2 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 B.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 2.2 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 B.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 2.2. 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 B.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 2.2. The dependent variable is the interest rate. Across the columns, we adjust our sample filters from Section 2.3. 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 B.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

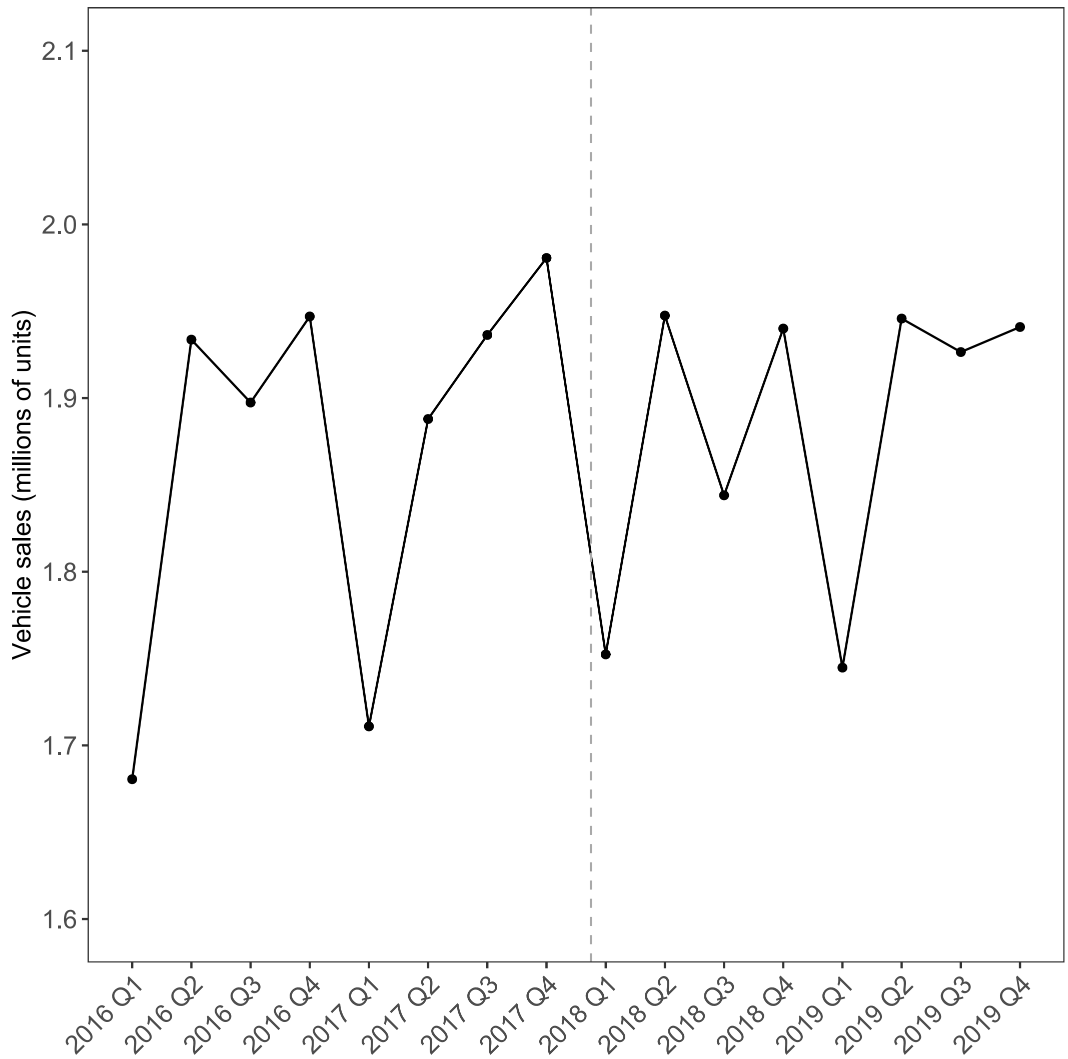
NOTE.—This table reports coefficient estimates from Equation 2.2 after adjusting the sample of lenders. The dependent variable is either the interest rate, log loan amount, log loan maturity, or loan-to-value ratio. The sample is restricted to auto loans originated between January 2017 and December 2018. In Panel A, we reinclude all removed lenders from Section 2.3.2 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 B.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 2.2 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.

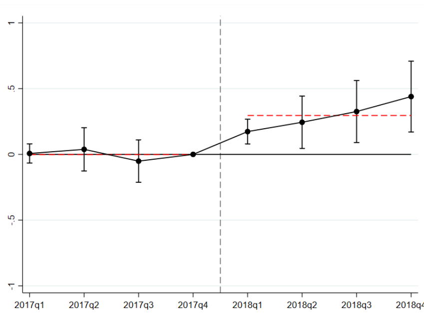
Figure B.1: Alternative explanation: Time series of vehicle sales



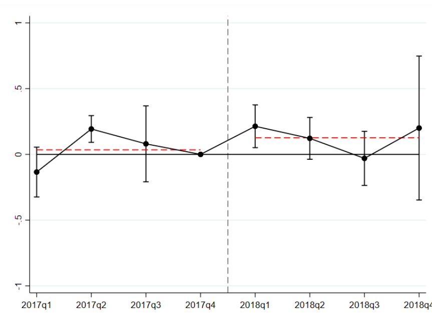
NOTE.——This figure plots the number of vehicles sold in the United States between January 2017 and December 2018 for BMW, Ford, General Motors, Honda, Mercedes-Benz, and Volkswagen. For each manufacturer, we include all of its affiliated brands (i.e., we include Chevrolet sales for General Motors).

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

(a) More exposed



(b) Less exposed



NOTE.——This figure plots coefficient estimates from Equation 2.3 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.

Chapter 3 Internet Appendix

Table C.1: Robustness check: Control for the growth of AUM

	(1) Log(AUM)	(2) Log(AUM)	(3) Log(AUM)	(4) Log(AUM)	(5) Log(AUM)
Top 100 Indicator			0.146***	0.130***	0.170***
			(0.0298)	(0.0310)	(0.0298)
Normalized Rank	1.180***	0.200***	0.183***	0.188***	0.152*
	(0.1176)	(0.0535)	(0.0519)	(0.0506)	(0.0849)
$\Delta Log(AUM)_t$	0.131***	0.0940***	0.0944***	0.0989***	0.114***
	(0.0437)	(0.0250)	(0.0248)	(0.0237)	(0.0226)
Controls		Yes	Yes	Yes	Yes
Advisor FE		Yes	Yes	Yes	Yes
Firm FE				Yes	
State×Year FE				Yes	
Firm×State×Year FE					Yes
R^2	0.129	0.941	0.941	0.953	0.959
Observations	7,635	7,049	7,049	7,027	5,424

NOTE.—This table shows the results for the panel of individual financial advisors. The dependent variable is the logarithm of asset under management at year $t+1$ (measured after publication of the rankings). *Top 100 Indicator* is a dummy variable equals to 1 if an advisor is in Barron's Top 100 Ranking at time t and zero otherwise. *Normalized Rank* is a continuous variable between zero and one. It measures an advisor's quality at the state level. $\Delta Log(AUM)_t$ is a continuous variable to control for the growth of asset under management.*Controls* include experience and tests (Series 6, 7, 24, 65, and 66). $\Delta Top100Indicator$ is the change in *Top 100 Indicator* from time *t-1* to time *t*. $\Delta NormalizedRank$ is the change in *Normalized Rank* from time *t-1* to time *t*. Observations are at the advisor-by-year level over the period 2009-2020. Robust standard errors are in parentheses and are clustered by running variable. ***,**, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Table C.2: Regulatory Misconduct

	(1)	(2)	(3)	(4)	(5)
Top 100 Indicator			-0.00283	-0.00264	-0.00654**
			(0.0033)	(0.0034)	(0.0025)
Normalized Rank	0.000974	0.00307	0.00329	0.00258	0.00483
	(0.0010)	(0.0035)	(0.0035)	(0.0025)	(0.0030)
Controls		Yes	Yes	Yes	Yes
Advisor FE		Yes	Yes	Yes	Yes
Firm FE				Yes	
State×Year FE				Yes	
Firm×State×Year FE					Yes
R^2	0.000	0.205	0.206	0.249	0.494
Observations	12,150	10,900	10,900	10,882	8,838

NOTE.—This table reports the results for the panel of individual financial advisors. The dependant variable is regulatory violations (i.e. excluding customer complaints and firm terminations) at year $t+1$. *Top 100 Indicator* is a dummy variable equals to 1 if an advisor is in Barron's Top 100 Ranking at time t and zero otherwise. *Normalized Rank* is a continuous variable between zero and one. It measures an advisor's quality at the state level. *Controls* include experience and tests (Series 6, 7, 24, 65, and 66). Observations are at the advisor-by-year level over the period 2009-2020. Robust standard errors are in parentheses and are clustered by firm. ***,**, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

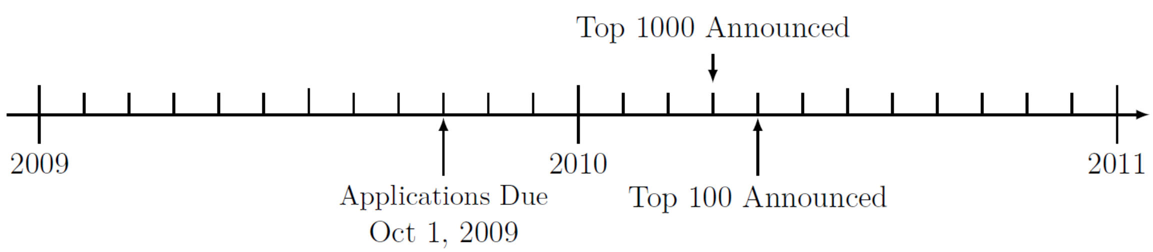


Figure C.1: Barron's Timeline
This figure illustrates the timeline for the 2010 Barrons rankings. Applications for the Top 1000(1200) are due as of October 1st of the prior year. The Top 1000 advisors are announced in March of the ranking year, and the Top 100 are announced in April of the ranking year.

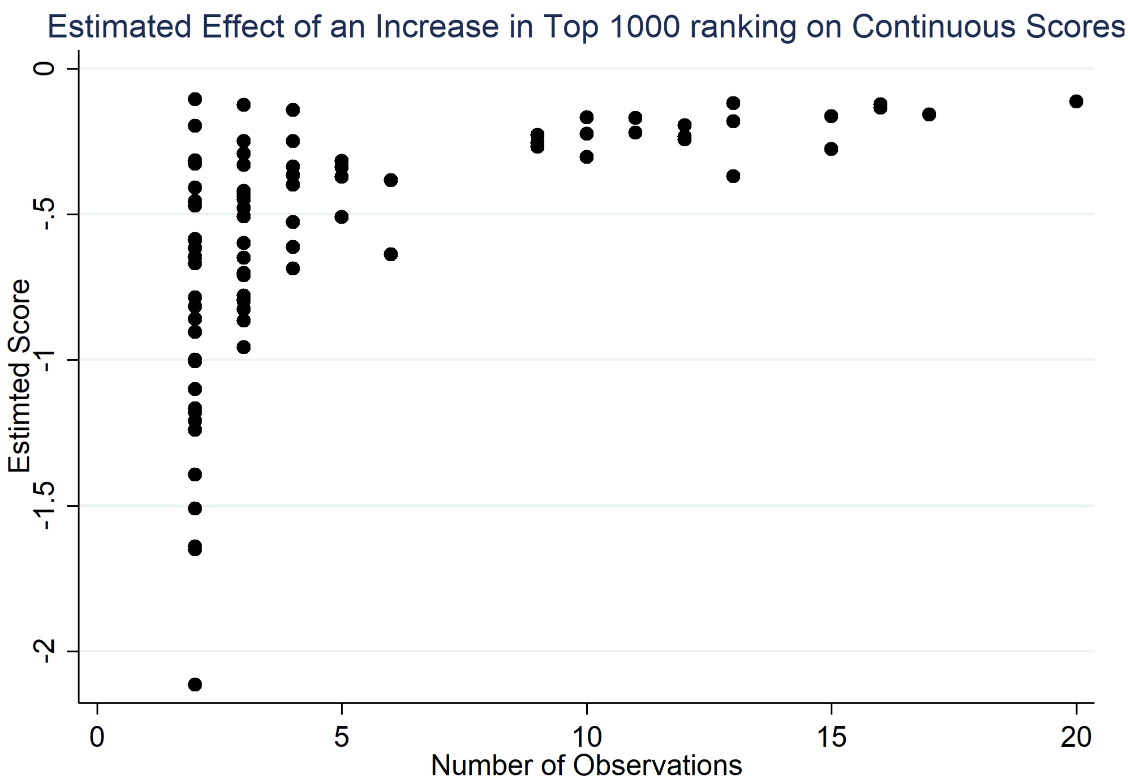


Figure C.2: Estimated Scores

This figure shows the changes in continuous scores as an increase in the Top 1000 ranking at the state-year level. The X-axis shows the number of observations in regressions. The Y-axis shows the estimated scores at the state-year level.

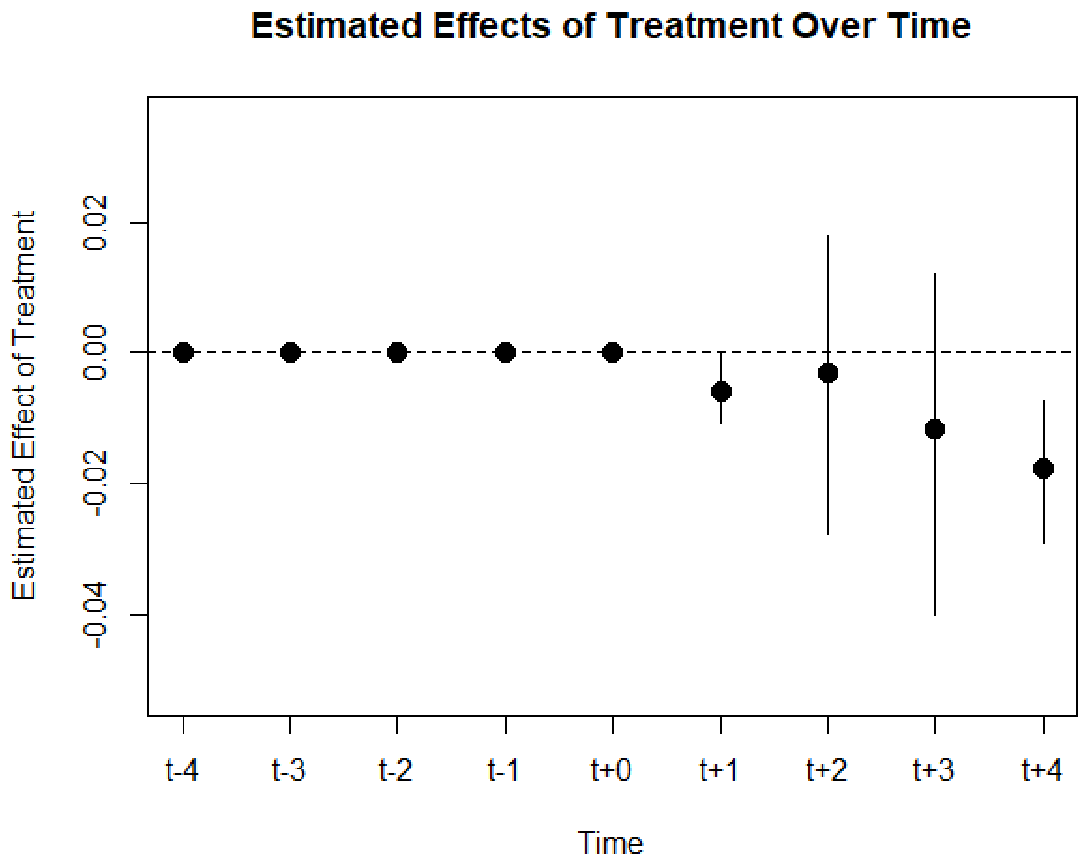


Figure C.3: Changes in Misconduct in Event Time - Firm Level
This figure shows the average treatment effect using the difference-in-difference estimator. The outcome variable is misconduct as defined in Egan, Matvos, and Seru 2019b. Treated firms are matched with control firms on past misconduct, lagged number of employees, lagged log(AUM), and lagged log(Accounts).

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Curriculum Vitae

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- **EDUCATION**

  - Master of Business Administration - University of Missouri-Kansas City, MO, USA (2017)
  - B.Sc. in Industrial Engineering-System Analysis & Planning - Iran University of Science and Technology, Tehran, Iran (2012)

- **RESEARCH INTEREST**

  - Household Finance, Financial Intermediation, and Empirical Asset Pricing

- **WORKING PAPERS**

  - Competition and Shrouded Attributes: Evidence from the Indirect Auto Loan Market
  - Third Party Quality Certification in the Market for Financial Advice (with William Gerken)
    - Institute for the Study of Free Enterprise Research Grant ($7,500)
    - AFA(2022)*, FIRS (2020)†, FMA (2020)
  - Does Trade Policy Affect Consumer Credit? The Role of Captive Finance (with Kristine Hankins and David Sovich)
    - MFA (2023)* MSU FCU Conference (2022)*, UBC Summer Finance Conference (2022)*,Craig W. Holden Memorial Conference (Indiana University) (2022)*, FMA (2022), FRA (2021)*, Arizona State University*, University of Arizona*, Virginia Tech*, Miami University*
  - Testing Asset Pricing Models on Individual Stocks (with Charlie Clarke)
    - AFA (2023)*
  - The Marginal Propensity to Default (with David Sovich)
    - The Best Paper Award, FMA 2021, Semi-Finalist

(presentation by coauthors ∗, canceled due to covid †)

- **TEACHING**

  - Primary Instructor: Fin 410: Investments (4.9 out of 5) (Fall 2021)

      * Nominated for Gatton Teaching Excellence Award

- **MEDIA MENTIONS**

  - WalletHub.com
  - Janus Henderson Investors