Real Effects of Search Frictions in Consumer Credit Markets*

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Abstract

We estimate how search frictions in credit markets distort consumption, contribute to substantial price dispersion, and modulate the pass-through of interest-rate shocks. Using rich microdata from millions of auto-loan applications and originations by hundreds of financial providers, we isolate plausibly exogenous variation in interest rates due to institution-specific step-function pricing rules. These discontinuities lead to substantial variation in the benefits of search, affect physical search behavior, and distort extensive- and intensive-margin loan and car choices through quasi-random interest-rate markups. We further show that these discontinuities are more consequential in areas we measure as having high search costs. Overall, our results provide evidence of the real effects of the costliness of shopping for credit, the continued importance of local bank branches, and how search frictions inhibit the transmission of monetary policy to durable goods purchases. More broadly, we conclude that the welfare consequences of costly search include inefficient consumption in both primary and related markets.

Keywords: credit markets, search, auto loans, durables, regression discontinuity
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1 Introduction

Some of the most important open questions in household finance center around how credit-market imperfections affect consumption, including the role of adverse selection in consumer credit markets (Adams, Einav, and Levin, 2009), the importance of credit constraints in explaining high marginal propensities to borrow and consume out of credit (Gross and Souleles, 2002), and identifying the inhibition of credit expansions to the household sector (Agarwal et al., 2017a). In this paper, we provide evidence that costly search represents an additional friction in consumer debt markets that not only leads to interest-rate dispersion among similar loans but can distort extensive- and intensive-margin loan and consumption choices. This finding that consumption in related markets is affected also contributes to the empirical search literature that has previously focused on costly search in final goods markets for homogenous products.

Using administrative data on 2.4 million auto loans extended by 326 different financial institutions in all 50 states and loan application data on 1.3 million potential loans from 41 institutions, we establish four main empirical facts. First, there is significant price dispersion for the same credit product across providers—most borrowers in our data could access significantly dominating loan offers if they queried two additional financial institutions (see Figure 1). Second, such search is costly, and borrowers' propensity to search for loans with better terms is lower in areas likely to have higher search costs.[1] Third, the segment of the auto lending market we study does not feature pure risk-based pricing; we observe large loan-rate discontinuities at various institution-specific FICO thresholds. Fourth, consumer purchasing and financing decisions are distorted by the resulting interest rate dispersion around these lending thresholds. Taken together, we argue that consumers fail to consistently identify optimal financing terms because of costly search in the retail auto loan market; this distorts

[1]Nationally representative survey evidence points to the apparent costliness of consumer search in credit markets. According to the 2013 Survey of Consumer Finances, one in five people self-report doing "almost no searching" when taking out a new loan. While such behavior could be driven by low expectations about the benefits of search, our results provide evidence that the benefits of search are likely substantial for many borrowers.

financing decisions as well as consumption decisions of durable goods.

We focus on the market for automobile-secured loans for several reasons. Auto loans are ubiquitous and play an important role in the consumer credit complex. Auto debt is the fastest-growing and the third-largest category of consumer debt (behind only mortgages and student loans), with over 106 million outstanding loans (0.84 per U.S. household) comprising \$1.15 trillion in aggregate auto debt (NY Fed, 2017). Over 85% of car purchases are financed (Bartlett, 2013), and vehicles represent over 50% of total assets for low-wealth households (Campbell, 2006). From an empirical-design standpoint, auto loans are a relatively homogeneous credit product and can essentially be described completely by their interest rate, term, and amount. Finally, auto loan markets are quite local. The median borrower in our sample borrowing directly from a lender (as opposed to indirect loans originated via auto dealers) originates a loan from a branch that is within a 15-minute drive of her home, whereas the median worker in the United States commutes 26 minutes to work. The stylized fact that direct auto loan markets are more local than labor markets motivates our inquiry into the distortions that physical search frictions might cause in consumer debt markets.

Our empirical strategy features a setting where potential gains to search are high and quasi-randomly assigned. We document large discontinuities in offered loan terms around FICO thresholds across lending institutions. Lending policies that jump discontinuously at various FICO thresholds appear to exist in 173 of the 326 lending institutions in our sample. Notably, the location of the thresholds along the FICO spectrum varies across institutions; while some thresholds appear more popular than others, there is no consensus set of thresholds used by a plurality of lenders. Variation in the location of thresholds for lenders even in the same geography means that borrowers on the "wrong" side of a threshold at one institution could be on the "right" side of a threshold at another institution. We document in first-stage results that borrowers on the right of FICO thresholds are offered lower interest rates. On average, borrowers to the right of an institution's FICO threshold are offered loans with 1.46 percentage point lower interest rates as compared to otherwise similar borrowers just below a FICO threshold. As a result, borrowers just to the left of a lender's threshold would benefit from searching for loan offers from institutions with either no discontinuity in offered rates or from institutions where borrowers would be on the right side of a given threshold.

Figure 2 provides examples of such interest-rate discontinuities for three different credit unions in our data with detected discontinuities using the lending policy rule estimation procedure described in Section 5.2. As discussed in Section 5.3 below, the observed FICO thresholds isolate supply-side changes in loan characteristics from demand-driven factors under the assumption that demand-side factors (e.g., preferences, income, financial sophistication) are not likely to also change discontinuously at quasi-random FICO thresholds that vary across institutions in the same geography. We support this assumption with evidence that ex-ante borrower characteristics (including age, gender, ethnicity, application debt-to-income ratio (DTI), application loan size, and the number of loan applications per FICO bin) are balanced around FICO thresholds.

Potential explanations for equilibrium differences in offered interest rates around lending thresholds include unobserved heterogeneity, measurement error, and search costs. Identifying search costs as a meaningful friction in consumer credit markets requires direct evidence that explains variation in measures of consumer search using measures of the costliness of consumer search. We discuss potential aspects of shopping for a car that may have particular utility costs associated with them in Section 6.2 below; the process often entails time, effort, and stress—each of which may be in short supply while simultaneously shopping for a car. We show that borrowers on the expensive side of FICO thresholds reject high-interest-rate loans most often when the number of nearby alternative lenders is high. Using the physical branch locations of every bank and credit union in the United States, we calculate the number of financial institutions within a 20-minute drive from each borrower as a proxy for search costs. We find that differences in loan take-up rates across FICO thresholds are smaller for borrowers in high search-cost areas. Borrowers who would presumably have to exert more effort to search for a loan with better terms are more likely to accept the loan pricing they are offered even though these terms are strongly dominated by nearby alternatives. Using a subsample of our data that allows us to link borrowers across loan applications to different lenders, we verify that borrowers are more likely to submit multiple loan applications when our search-cost measure is low.

What impact does sharp variation in loan pricing for otherwise identical borrowers have on borrower outcomes? On average, borrowers quasi-randomly offered expensive credit purchase cars that are 3.4 months older, spending an average of $715 less. The similarities mentioned above in borrowers across FICO thresholds suggest that borrowers arriving quasi-randomly on the expensive side of an arbitrary FICO threshold have similar preferences to those on the low interest-rate side of a pricing discontinuity and would thus presumably also like to purchase a more expensive and newer car had they not been offered higher interest rates. Given that these high markups (the treatment) are as good as randomly assigned, we further ask whether there is selection in the loan take-up decision by examining ex-post borrower outcomes. Subsequent changes in credit scores and ex-post loan performance do not change differentially by cutoffs, which we interpret as evidence that borrowers who take up dominated loan offers are not disproportionately likely to be low-quality borrowers, allowing us to interpret conditional-on-origination effects on second-stage consumption outcomes as causal. By using above-threshold borrowers as a counterfactual for below-cutoff borrowers, we are the first paper to our knowledge to quantify how search frictions distort consumption.

We also consider a series of robustness tests to address potential omitted variables that could be correlated with our physical measure of search costs. A Bartik strategy based on the 1990 network of lender branches in the United States allows us to address potential time-varying endogeneity in the number of proximate lenders (e.g., that banks close branches in response to hyperlocal economic conditions that also determine interest-rate elasticities). Because shift-share instruments essentially rely on the exogeneity of preexisting conditions, we also present a difference-in-differences strategy and a hyperlocal fixed effects strategy that allows us to rule out time-invariant explanations for our results (for example, the concern that low financial sophistication or brand loyalty covaries across space with interest-rate sensitivity and the density of financial providers).

Finally, we show that search frictions have aggregate consequences for the transmission of monetary policy. When we examine how the interest rates of new originations change in response to contemporaneous changes in five-year Treasury yields, we find that high search-cost areas have 10% less pass-through. Taken together, our evidence suggests that search costs represent a meaningful market friction that enables the persistence of equilibrium price dispersion and ultimately distorts consumption in the retail auto loan market.

The remainder of the paper proceeds as follows. After contextualizing our work in several related literatures in Section 2, Section 3 details the administrative data we use throughout the paper, including an analysis of its representativeness. Section 4 documents price dispersion in the market for auto loans. Section 5 presents results detecting discontinuities in lender price rules and introduces our regression-discontinuity identification strategy. In Sections 6 and 7, respectively, we present evidence that consumers' propensity to search is correlated with measures of search costs, and we estimate the effects of costly search on loan and durable-purchase outcomes. To demonstrate the aggregate importance of search frictions beyond the sample of borrowers we consider here, Section 8 examines the differential transmission of interest-rate shocks to areas with high and low search costs. Section 9 concludes.

2 Related Literature

In this section, we motivate our work in the context of literatures on search frictions, auto loans, and FICO-based regression discontinuities.

Theories of costly search (e.g., Stigler, 1961 and Stahl, 1989) suggest that when some agents find it too costly to solicit the full menu of offered prices, equilibrium prices will reflect the distribution of offered prices and the random draw that each agent acquires from the offered price distribution. In a credit market under the Stahl model, lenders can expect to originate loans without offering the lowest rates because a randomly arriving customer will not exert the effort required to find better rates given the equilibrium (dispersed) price distribution. Consider a financial institution that offers an interest rate on auto loans that is high relative to competitors, conditional on borrower quality. If search is costly, consumers are more likely to accept the offered rate despite the existence of better available rates. Similarly, entering lenders cannot profitably undercut overpriced competitors because of entrants' inability to inform and attract consumers. Lowering search costs should therefore result in lower price dispersion as consumers increase their propensity to shop around given a fixed distribution of prices. In equilibrium, if consumer search costs decline, lenders would offer more competitive rates, essentially facing a decline in market power.

Whereas the Stahl model features sequential search by shoppers with full information about the distribution of available prices, several papers feature alternative formulations of consumer search.[2] De Los Santos, Hortaçsu, and Wildenbeest (2012) examine online shopping behavior and find support for fixed sample size search instead of the optimal stopping rule of sequential search models. Ellison and Ellison (2009) present evidence consistent with sellers increasing search costs via the opacity of the product characteristics and prices menu. Zhu (2012) models the penalty buyers or sellers may face when they return to a previously obtained quote and thereby signal the level of surplus in the transaction. While our setting does not allow us to cleanly distinguish between competing models of search, our estimated gains from search exceed typical measures of the opportunity cost of time, suggesting perhaps that potential borrowers are unaware of the degree of price dispersion in the auto-loan market.

Multiple empirical papers establish the existence of equilibrium price dispersion (necessitating ruling out product heterogeneity as a driver of price variation) and connect it to

[2]For a comprehensive treatment of the history of thought in the theoretical and empirical search and price dispersion literature, see Baye, Morgan, and Scholten (2006).

evidence that consumer search is costly in a given domain. For example, Sorenson (2000) documents dispersion in prices of prescription drugs that are driven by proxies for likely search intensity. In consumer finance, Hortaçsu and Syverson (2004) find large dispersion in the fees charged by very similar mutual funds that are driven by information/search frictions. Woodward and Hall (2012) document that mortgage borrowers overpay for mortgage broker services due to a reluctance to shop for mortgages. In addition to documenting price dispersion in mortgage rates, Alexandrov and Koulayev (2017) provide survey evidence that close to half of consumers do not shop for a mortgage before origination and are generally unaware of price dispersion. Zinman and Stango (2015) use a self-reported measure of shopping intensity to explain variation in price dispersion in the credit-card market. All of these results are consistent with questions on search intensity in the 2013 Survey of Consumer Finance wherein many borrowers self-report doing very little shopping around for a loan.

Relative to the literature on price dispersion and search intensity that often models consumers as having inelastic unit-demand for a final good, our setting allows for measurement of distortions in consumption that can result from costly search. Analogous to supply-side frictions that modulate the pass-through of monetary policy (e.g., Scharfstein and Sunderam, 2016, and Agarwal et al., 2017a), we further show that search frictions have the potential to temper the efficacy of monetary policy if consumers are unwilling or unable to search out the distribution of available credit to find the rates that have responded to declining risk-free rates.

Recent work by Agarwal et al. (2017b) shows that in the cross section, intensive loan search is correlated with higher interest rates, running counter to the standard prediction that search and selected prices are inversely correlated. Agarwal et al. (2017b) explain this with a model of borrower private information about the returns to search—low credit-worthy borrowers search until they find a lender who offers them an advantageous interest rate, albeit higher than the rates offered without search to (observably) high-quality borrowers. In our setting, the quasi-random assignment of our regression-discontinuity design effectively allows us to abstract away from cross-sectional variation in private information and rely on the conceptual argument that for a given borrower, the relationship between search and interest rates should be negative.

We are not the first paper to exploit FICO-based discontinuities in treatment variables. Keys et al. (2009 and 2010) find that the probability of securitization (and thus loan screening) changes discontinuously at a FICO score of 620. Bubb and Kaufman (2014) provide evidence for other discrete FICO thresholds in the mortgage underwriting process, including detailing the likely genesis of threshold-based policies. More recently, Agarwal et al. (2017a) use sharp FICO-based discontinuities in credit limits to estimate heterogeneity in marginal propensities to borrow and lend, and Laufer and Paciorek (2016) evaluate the consequences of minimum credit-score thresholds for mortgage lending. Building on this collection of papers that either use FICO-based discontinuities as natural experiments or explicitly study their consequences, we are the first to identify credit-score–based discontinuities in pricing rules and to link those discontinuities to price dispersion, costly consumer search, and distortions in consumption.

Finally, we contribute to a growing literature studying the automobile loan market and the frictions therein, including Attanasio, Goldberg, and Kyriazidou (2008), Adams, Einav, and Levin (2009), Busse and Silva-Risso (2010), and Einav, Jenkins, and Levin (2012 and 2013).

3 Data

We analyze the loan contract terms and auto purchasing decisions of 2.4 million individual borrowers in the United States from 326 retail lending institutions between 2005 and 2016. The loan data are provided by a technology firm that provides administrative data warehousing and analytics services to retail-oriented lending institutions nationwide. Roughly two-thirds of the lending institutions represented in the data set are credit unions ranging between \$100 million and \$4 billion in asset size. The remainder are non-bank finance companies of unknown total asset size, although the vast majority (98.5%) of the loans in our data are originated by credit unions.[3] Borrowers from all 50 states are represented in the data, but the five largest states in the data are Washington (321,096 loans), Texas (222,062 loans), California (174,443 loans), Minnesota (124,910 loans), and Tennessee (117,495 loans).

The dataset contains information capturing all three stages of a loan's life: application, origination, and ex-post performance, although we have loan application data for only approximately 1.3 million loans from 41 different institutions. The available loan application data report borrower characteristics (ethnicity, age, gender, FICO scores, and debt-to-income (DTI) ratios at the time of application), whether a loan application was approved or denied, and whether it was subsequently withdrawn or originated. For originated loans, the data additionally include information on loan amounts, loan terms, car purchase prices, and collateral characteristics. Using Vehicle Identification Numbers (VINs), we learn about the make, model and model year of the purchased car. We restrict our sample to direct loans in an effort to address concerns that indirect loans are potentially endogenously steered to specific financial institutions (perhaps because car dealers become aware of lenders' pricing rules over time).[4] Finally, to measure ex-post loan performance, we observe a snapshot of the number of days each borrower is delinquent, whether individual loans have been charged off, and updated borrower credit scores as of the date of our data extract.

Panels A, B, and C of Table 1 present summary statistics on loan applications, loan originations, and measures of ex-post performance, respectively. As reported in Panel A of Table 1, the median loan application in our data seeks approval for a five and a half year \$18,136 loan at a median interest rate of 4.00%.[5] Borrowers applying for loans in our data

[3]Our results are unchanged if we exclude loans from finance companies, which are generally of lower credit quality.

[4]The terms direct and indirect loans refer, respectively, to whether the borrower applied for a loan directly to the lending institution or through an auto dealership that then sent the loan application to lending institutions on the buyer's behalf. See Appendix Table A1 for summary statistics for the excluded indirect loans.

[5]Interest rates in the loan application data refer to approved loans, whether they were subsequently originated or not.

have an average credit score of 648 and an average DTI ratio of 26.0%. The percentage of loans approved is 50.2%, with 78.4% of the approved borrowers subsequently originating a loan. Throughout the paper we refer to the number of loans originated divided by the number of applications approved for a particular group as the loan take-up rate. We exploit variation in the loan take-up rate in Section 6.2.

Panel B of Table 1 reports summary statistics on loan originations, revealing several interesting patterns. Compared with loan applications, originated loans have smaller average sizes, similar interest rates, shorter terms, and are from more creditworthy and less constrained borrowers. Average monthly payments for originated loans are $324 per month with an interquartile range of only $195.

Panel C tabulates measures of ex-post loan performance. While the average loan is 23.4 days delinquent, most loans are current; the 75th percentile of days delinquent is zero and only 2.1% of loans have been charged off (accounted as unrecoverable by the lender). Defining default as a loan that is at least 90 days delinquent, default rates average 2.2%. In untabulated results, default rates for borrowers with sub-600 FICOs average 6.8%, compared to a default rate of 2.6% for borrowers with FICOs between 600 and 700 and 1.6% for over-700 FICO borrowers. Lending institutions periodically check the credit score of their borrowers subsequent to loan origination, creating a novel feature of our data. Summary statistics for ΔFICO represent percent changes in borrowers' FICO scores from the time of origination to the lender's most recent (soft) pull of their FICO score.[6] Updated FICO scores indicate that borrowers on average experienced a 1% reduction in FICO score since origination, although borrowers with FICO scores below 600 on average realized a 5.7% increase in FICO score.

[6]The time between FICO queries varies by institution, but institutions that provide updated FICO scores do so at least once a year such that conditional on having an updated FICO score, the amount of time between the original FICO recording and the current FICO is roughly equal to loan age.

3.1 Data Representativeness

The bulk of our auto loan data come from credit unions, prompting questions about the representativeness of the data. Experian data from 2015 indicates that credit unions originated 22% of all used car loan originations and 10% of new car originations in the United States. The Experian data do not differentiate direct lending from indirect lending, but in the auto loan data made available to our data provider by its clients, roughly half are direct loans. Data on the performance of auto loans as reported in the New York Federal Reserve Consumer Credit Panel (CCP), a representative 5% sample of U.S. borrowers, suggests that auto loans originated by credit unions and banks have substantially lower default rates as compared to loans originated by auto finance companies. We discuss issues associated with digital lending in Section A.3.

Our data confirm the popular perception that credit union usage is concentrated among older consumers. Over 41% of borrowers in our sample were between 45 and 65 years old at loan origination whereas 34% of the adult U.S. population is between the ages of 45–65. Borrowers in our sample are also less racially diverse than the general public. Over 73% of our sample is estimated to be white (as of 2015), compared to an estimated 65% of adults in the general population as recorded by the 2015 American Community Survey.[7] Borrowers in our data report median FICO scores at origination of 714 (Table 1, Panel B) over the full 2005-2016 sample period. The CCP reports median FICO scores for originated auto loans of 695 during the period our sample was collected. In summary, our sample contains borrowers who are slightly older, less racially diverse, and of a higher average credit quality than national averages. These sample biases should not limit our ability to draw inference given that we rely on a regression discontinuity (RD) design that leans crucially on an assumption of smoothness in borrower demographics across discontinuities at a given institution. Moreover, while borrowers in our data may have different search costs than non-

[7] Borrowers do not report race at the time of loan origination, but most lenders in our sample estimate minority status to document compliance with fair lending standards.

credit-union borrowers, our data still represents a very large share of all auto-loan borrowers in the United States.

A second data validity issue involves the distribution of loan originations through time. Over 70% of loan originations in our sample occurred between 2012 and 2015, despite a sample period that runs from 2005 to 2016. The large increase in loans through time reflects the increase in the client base of our data provider through time rather than auto credit origination in general. CCP data shows that auto loan originations in the general population have increased through time, from an aggregate outstanding balance of $725 billion in Q1 2005 to just over $1.15 trillion in Q4 2016, but not at the rate reflected in our dataset. We view the non-representative time series of our data as less relevant given our cross-sectional identification strategy.

4 Documenting Price Dispersion

Diagnosing a market with dispersed prices requires ruling out any product differentiation, i.e., establishing that differences in prices truly represent identical goods being sold for different prices in the same market. For any given borrower with an observable set of attributes, we estimate the spread between the borrower's interest rate and the lowest available interest rate at another lender in our data for another borrower with very similar attributes. To calculate this spread, we group borrowers in the same Commuting Zone (CZ), six-month transaction date window, five-point FICO bin, $1,000 purchase-price bin, same loan maturity, and 10 percentage-point DTI bin. We consider loans originated to borrowers within the same CZ × time × price × FICO × maturity × DTI cell to be effectively identical. Although there may be some degree of residual heterogeneity within a cell, the magnitude of the variation we find is sufficiently large that it would be difficult to explain with borrower-level heterogeneity alone. Moreover, in 43% of the cells, the best rate in the cell is achieved by a borrower with a lower FICO and higher DTI than other borrowers in the cell. Nevertheless, our RD design below establishes the existence of large pricing disparities for identical credit risks. Owing to the strictness of this criteria, many borrowers in our data are in their own cell, limiting our ability to calculate a spread. Note, too, that because we do not observe interest-rate offers from lenders that are not clients of our data provider, these spreads are lower bounds (having the universe of interest rates offered to a given cell could only weakly decrease the best available rate).[8] Albeit incomplete, because of the richness of our data coverage across hundreds of providers, we have thousands of cells with multiple borrowers.

Figure 1 plots the density of the spread to the best available rate in percentage points for the 54% of borrowers who did not attain the best rate in their cell. The mean and median of this distribution are 234 and 125 basis points, respectively. Including the 46% of borrowers who are getting the best available rate given their discrete borrower type, the average borrower in our data is thus paying 1.3 percentage points more than an observationally equivalent borrower at the same time in the same place. Simulating random markup draws from the distribution implied by the density in Figure 1, we find that the average borrower would need to obtain three price quotes to find the best available rate for that borrower's type. We provide further evidence of price dispersion in Section 6.

Exclusivity of Credit Unions By definition, a credit union is a member-owned cooperative financial institution that requires membership to receive financial services. Often, credit unions' membership requirements restrict eligibility to well-defined groups. Because most of the loans in our sample were originated by credit unions, one concern is whether a given borrower could have joined the credit union providing the best available rate in her cell in our data. For example, if the lowest available interest rate by a borrower was offered by a firefighters' credit union, then the borrower's search costs would not only involve finding the low rate but also the effort required to become a firefighter. To address this concern, we recalculate the spread-to-lowest-available rate measures using a sample comprised entirely of credit unions whose primary membership requirement is residence in a specified geographic

[8] We discuss the particular case of digital lenders in Section A.3 below.

area. In other words, all borrowers in our CZ-based matched portfolios are eligible to become a member at any of the credit unions included in their cell by virtue of living in the same CZ as others in their cell. Our results are nearly identical after making this restriction. We also note that the finance companies in our sample have no membership requirements.

5 Estimating the Effects of Search Costs

In this section, we introduce an empirical strategy designed to identify the impact of costly search in equilibrium outcomes in both credit and durables markets. As previously noted, heterogeneity in observed prices could alternatively be explained by measurement error or unobserved product heterogeneity, particularly in credit markets where borrowers may be differentiated by so-called soft information. We exploit exogenous variation in our data that creates a setting where the potential gains to search are quasi-randomly assigned across borrowers. Using a regression-discontinuity (RD) design that allows us to ignore measurement error and unobserved heterogeneity, we estimate whether borrowers' propensity to search is correlated with proxies for the cost of search. We then use the RD laboratory to document distortions in auto purchases resulting from search frictions in the financing market.

5.1 Detecting Discontinuities

Lending institutions make underwriting decisions about whether to approve a loan application using a combination of hard and soft information on borrower credit quality (Petersen, 2004). Hard information generally consists of quantifiable credit metrics provided by credit bureaus or verified with paystubs and tax statements such as credit scores, debt-to-income ratios, bankruptcy history, and income. Soft information, loosely defined as information that cannot be easily quantified related to the likelihood of a borrower's future willingness or ability to repay a loan, is by definition unobservable to the econometrician. Any econometric analysis that specifies loan outcomes as the dependent variable is subject to the critique that equilibrium loan outcomes are influenced by unobservable soft information, complicating inference related to factors causing an outcome of interest. Our setting is no exception. While our dataset consists of millions of lending outcomes, our ability to draw inference is hindered by the possibility that unobserved soft information plays a role in jointly determining selection into application and origination, observed loan terms, and subsequent loan performance. For example, soft information in our setting could be generated from the relationship between credit unions and their long-term customers, observable to a loan officer.

We address this possibility, and other potential omitted variables, with an RD design leveraging observed discontinuities in offered loan terms across several FICO thresholds. Unlike the 620 FICO heuristic in mortgage *underwriting* first exploited by Keys et al. (2009 and 2010) that affects screening at both origination and securitization (Bubb and Kaufman, 2014), we focus on discontinuities in loan *pricing*, i.e., the interest rate offered to a borrower conditional on having a loan application approved by underwriting. Moreover, no industry standard set of thresholds exist in auto lending as opposed to mortgage lending. Still, while auto loan lending institutions do not adhere to a common set of FICO cutoffs, the use of a given threshold at some point across the FICO spectrum is prevalent for most lenders in our data. See Bubb and Kaufman (2014), Livshits, Mac Gee, and Tertilt (2016), and Agarwal et al. (2017a) for models of credit risk processing costs and Al-Najjar and Pai (2014) for a model of overfitting that could each rationalize binning risk types in pricing decisions.[9] Also in contrast to Keys et al. (2010), FICO thresholds observed in our data have little to do with secondary markets given that many auto loans are retained by the lending institutions in our dataset. Rather than reflecting demand for securitization or a loan's subsequent marketability on a secondary market, FICO discontinuities may have been incorporated into software systems as a holdover from a time when pricing was done via rate sheets instead

[9]Anecdotally, lending institutions have confirmed that their pricing functions explicitly incorporate discrete FICO thresholds to set interest rates and loan terms. One executive pointed to a FICO score of 610 as the explicit cutoff that determines the loan terms offered to prospective borrowers at that executive's credit union. Applicants with a FICO score just below 610 were offered higher rates and loan terms below 60 months in contrast to applicants with FICO scores above 610.

of automated algorithms and could persist in part because costly consumer search prevents more accurately risk-based pricers from gaining market share.[10]

To illustrate the effect of FICO thresholds on equilibrium interest rates, we estimate lender-specific interest-rate and loan-term policies nonparametrically. For each lender c in our data, we characterize their lending policies across FICO bins with a set ψ of parameters $\{\psi_{ck}\}$ where k indexes FICO bins denoted $\mathcal{F}_k$. Pooling loan-level data for loan i from institution c, we estimate ψ by regressing interest rates r_{ic} on a set of indicator variables for each 5-point FICO bin $\mathcal{F}_k$

$$r_{ic} = \sum_k \psi_{ck} \mathbb{I}(FICO_i \in \mathcal{F}_k) + \varepsilon_{ic} \tag{1}$$

where ε_{ic} includes all other factors that influence loan pricing for a given loan. The five-point FICO bins begin at a FICO score of 500 where the first bin includes FICO scores in the 500-504 range, the second bin includes 505-509, etc., up through FICO scores of 800. The estimated coefficients on each FICO bin represent the average interest rate for loans originated to borrowers with FICO scores in that bin relative to the estimated constant (the omitted category is loans outside this range—we focus on relative magnitudes for this exercise).

Figure 2 presents interest-rate plots for three different financial institutions. The point estimates $\hat{\psi}$ represent how that lender's pricing rules appear to vary with borrower FICO score, and the accompanying 95% confidence intervals provide a sense of how reliant on FICO scores each lender's pricing rule was. Panel A of Figure 2, estimated on one institution in our data with approximately 12,000 borrowers (rounded to preserve lender anonymity), illustrates breaks in average interest rates for borrowers with FICO scores around cutoffs at 600, 660, and 700. The breaks in interest rates at the FICO cutoffs are large, representing

[10] In the mortgage industry, Bubb and Kaufman (2014) write that "Though [Automated Underwriting Systems] calculate default risk using smooth functions of FICO score, they also employ a layer of 'overwrites' which trigger a 'refer' recommendation when borrowers fall into certain categories—for instance, borrowers with FICO scores below 620." See Hutto & Lederman (2003) for a history of the incorporation of discrete credit score cutoffs into automated underwriting systems for mortgage lending, such as those created by Fannie Mae and Freddie Mac.

jumps of over 2 percentage points. Average interest rates for borrowers in the 595-599 FICO bin are 2.5 percentage points higher than the average interest rate for borrowers in the 600-604 FICO bin, and the difference in average interest rates between the two bins are statistically significant at the .001 level. Panels B and C illustrate similar rule-of-thumb FICO breaks for unique institutions with approximately 6,000 and 25,000 loans, respectively. One important observation arising from these anecdotal plots is the fact that the breaks occur at different FICO scores across different institutions, consistent with our understanding that the discontinuities are reflective of idiosyncratic pricing policies across institutions.

In order to standardize our analysis to include every institution that employs discontinuous pricing rules, we empirically identify the existence of discontinuities at each institution (if they exist at all) in our sample through the following criteria. We first estimate the interest-rate FICO bin regressions following equation (1) for each institution in our sample separately. To establish the existence of an economically and statistically significant interest-rate discontinuity, we require interest rate differences across consecutive bins to be larger than 50 basis points and to be estimated with p-values that are less than 0.001. We further refine the set of discontinuities by requiring that differences between leading and following FICO bin coefficients ψ_{ck} have a p-value of at least 0.1 and that an identified discontinuity not lie within 20 FICO points of another identified discontinuity at the same institution.[11] This restriction limits any potential contamination that could occur if borrowers simultaneously fall into a treated sample at one observed threshold but serve as a control for a sample at a different threshold. We further examine each potential threshold visually to ensure that the identified discontinuities are well behaved around the candidate thresholds. Finally, in an effort to maximize the statistical power in our RD design, we require that each candidate threshold contain 100,000 loans within the span of 38 FICO points around the candidate threshold, forming a discontinuity sample (Angrist and Lavy, 1999). The 38 FICO points represent 19 points on either side of a threshold that do not bump up against a different

[11] See Appendix C of Agarwal et al., 2017a for a discussion of overlapping cutoffs.

threshold that could exist within 20 FICO points. Implementing each of these restrictions ultimately results in large and meaningful discontinuities in interest rates and loan terms at FICO scores of 600, 640, and 700 across 57 institutions and 307,061 loans.[12] Table 2 reports summary statistics for the discontinuity estimation sample (the set of loans within 19 points of one of our thresholds). All of the RD estimates reported in the paper use the discontinuity sample.

5.2 First-Stage Results

To validate our RD design, we present a series of diagnostics designed to test whether our data meet the two main identifying assumptions required of valid RD estimation. First, the RD approach assumes that the probability of borrower treatment (i.e., offered interest rates) with respect to loan terms is discontinuous at FICO thresholds of 600, 640, and 700. Second, valid RD requires that any borrower attribute (observed or unobserved) that could influence loan outcomes change only continuously at interest-rate discontinuities. This smoothness condition requires that borrowers on either side of a FICO threshold are otherwise similar, such that borrowing outcomes on either side of a threshold would be continuous absent the difference in treatment induced by policy differences at the threshold.

In our remaining specifications, we normalize FICO scores to create a running variable $\widetilde{FICO}_{ict}$ that measures distance from an interest-rate discontinuity. For example, for loans near the 600 FICO score threshold, $\widetilde{FICO}_{ict} = FICO_{ict} - 600$. Panel A of Figure 3 plots average interest rates against normalized borrower FICO scores for a sample restricted to loans with borrower FICO scores between 581 and 619. The plots demonstrate smoothness in the conditional expectation function except for the points corresponding to a FICO score of 599 and 600, where interest rates jump discontinuously. We repeat the plot using similar 38-point FICO ranges for the 640 and 700 FICO thresholds in panels B and C of the same figure.

[12]We reiterate that not all institutions have thresholds at 600, 640, and 700—these are merely the most popular detected discontinuities satisfying our criteria. Relaxing the requirement of 100,000 loans within 38 FICO points around the threshold results in a larger set of identified thresholds.

These plots confirm the existence of large interest-rate discontinuities at these thresholds. The magnitude of the discontinuities appears to be smaller at higher FICO thresholds, which might arise from smaller relative differences in credit quality at high FICO score levels. The narrow confidence intervals in Figures 2 and 3 also indicate that interest rates in this market seem to be strongly determined by FICO. If there were substantial residual variation after controlling for FICO scores nonparametrically, the confidence intervals would be much larger.

To establish statistical significance and introduce our RD design, we estimate first-stage regressions of the form

$$y_{ict} = \beta_1 \widetilde{FICO}_{ict} + \beta_2 \mathbb{I}(\widetilde{FICO}_{ict} \geq 0) + \beta_3 \widetilde{FICO}_{ict} \cdot \mathbb{I}(\widetilde{FICO}_{ict} \geq 0) + \alpha_{cz(i)} + \delta_t + \varepsilon_{ict} \quad (2)$$

where y_{ict} is the outcome for loan i originating from lending institution c in quarter t, $\mathbb{I}(\widetilde{FICO}_{ict} \geq 0)$ is in indicator variable equal to one if the normalized FICO score $\widetilde{FICO}_{ict}$ is above the threshold, and $\alpha_{cz(i)}$ and δ_t are Commuting Zone and quarter fixed effects, respectively. In practice, we conservatively estimate equation (2) using the Robust RD estimator of Calonico, Cattaneo, and Titiunik (2014), estimating the effect of the running variable $\widetilde{FICO}$ above and below the cutoff at $\widetilde{FICO} = 0$ using local linear regression (as opposed to the unweighted linear specification we provide for intuition in equation 2) and a local quadratic bias correction.[13] Our baseline regression specification pools each of the three discontinuities into one dataset using the FICO normalization described above. Standard errors are clustered by normalized FICO score.

Table 3 presents results of this exercise (also plotted separately for each cutoff in Figure 3). Interest rates for borrowers with FICO scores immediately above one of our thresholds are estimated to be 1.46 percentage points lower than borrowers just below (column 1). Column 2 reports that loan maturities for borrowers just above a FICO threshold are 1.19 months longer than otherwise similar borrowers below the threshold. Given an average interest rate in our estimation sample of 6.8% (Panel B of Table 2), the magnitude of this coefficient is

[13]While our reported results use a uniform kernel with a bandwidth of 19, our results are robust to alternative kernels and a wide range of bandwidths.

economically meaningful and shows that landing on the expensive side of an interest-rate discontinuity has material consequences on the cost of credit. For the remainder of the paper, we refer to those on the expensive side of thresholds or below thresholds as LOT (left-of-threshold) borrowers. Borrowers above thresholds, or borrowers on the lower-interest rate side of thresholds, are referred to as ROT (right-of-threshold) borrowers.

5.3 Testing Quasi-Random Assignment

To test whether other observables beside the treatment variables (interest rate and loan maturity) also change discontinuously at our detected FICO thresholds, in Figure 4, we pool loans in the neighborhood of all three FICO thresholds and plot the average value of other borrower characteristics around these FICO thresholds along with the Calonico et al. (2014) estimated RD function and associated confidence intervals. Importantly, these graphs are constructed with loan application data in order to ensure that borrowers are similar along observable characteristics around FICO thresholds at the time of application. Panels A–E plot borrower DTI ratios, loan amounts, borrower age in years, borrower gender (an indicator for male), and borrower ethnicity (an indicator for white), respectively. These plots indicate smoothness in ex-ante borrower characteristics around FICO thresholds. Borrowers on either side of FICO thresholds do not appear meaningfully different in terms of their debt capacity, their willingness to borrow, or demographics. Finally, Panel F plots the number of applicants within each normalized FICO bin, along with the McCrary (2008) test for bunching in the running variable, showing that borrowers do not appear to select into applying for a loan based on where their FICO score falls relative to a lender's cutoff. Such manipulation of the running variable—a discontinuity in the propensity to apply for a loan at a FICO threshold—would raise selection concerns but would be difficult to accomplish given the uncertainty applicants face about their own credit scores (owing to the volatility of FICO scores, uncertainty about which credit bureau(s) a lender will query, and general unawareness) and the low likelihood that prospective borrowers are aware of the precise thresholds used by a given lender.

Table 4 reports the magnitude and significance of the discontinuity coefficients using the loan-application data, available for a subset of lending institutions. The estimates indicate no statistical difference in requested loan amounts for borrowers on either side of the threshold (column 1). In column 2, we present estimates of differences in debt-to-income ratios around the thresholds. Ex-ante debt-to-income ratios of borrowers on either side of the thresholds are statistically indistinguishable. Finally, we count the number of applications received from borrowers of each normalized FICO score and examine these counts at the FICO-score level using our RD estimator. Column 3 shows that the number of borrowers applying for loans is also not statistically different on either side of a threshold.

In sum, our empirically detected discontinuities in loan pricing at specific FICO thresholds are large (nearly 150 basis points) and are unaccompanied by similar discontinuities in borrower composition, supporting the validity of our RD design.

6 Evidence on Loan Search and Search Costs

6.1 Measuring Potential Gains to Search

The documented discontinuities represent exogenous variation in the benefits to search because the returns to search are higher for LOT than ROT borrowers. Borrowers who find themselves on the expensive side of a pricing threshold could reasonably expect to find a lower interest rate (all else equal) were they to search, a fact we document empirically below.[14]

Figure 5 provides visual evidence of better available rates by plotting the spread to the lowest available rate for LOT and ROT borrowers. Dotted lines in each plot are for LOT borrowers, and solid lines are for ROT borrowers. LOT borrowers, those offered exogenously higher rates, exhibit substantially higher average spreads from the lowest available rate and larger variance in those spreads. This pattern holds for each of the three thresholds. Taken

[14]Note that this would not be the case if every institution shared the same FICO cutoffs.

together, these plots confirm that price dispersion is largest for borrowers exogenously offered higher interest rates. We further note that Figure 5 plots also confirm that a larger fraction of LOT borrowers than ROT counterfactual borrowers accepted interest rates that were dominated by nearby available loans.

We quantify the magnitude of better available rates for LOT borrowers in Table 5, which tabulates the average spread-to-the-best-available rate for borrowers with FICO scores from 595–599, 635–639, and 695–699 as being 3.8 percentage points (pp), 2.3 pp, and 1.1 pp, respectively. That is, for borrowers with FICO scores between 595 and 599, there was a loan with a 3.8 pp lower interest rate originated to someone with the same FICO and DTI in the same CZ at the same time and used to secure a similarly priced car. The standard deviation of the spread across these cells is 2.9%, 2.1%, and 1.0%.

6.2 Measures of Loan Search and Search Costs

Why did borrowers treated with expensive rates not avail themselves of better lending opportunities? In this section, we evaluate whether proxies for search costs can explain borrowers' reluctance to shop. This analysis requires the construction of two measures: a measure of search propensity and a measure of search costs.

Potential borrowers face a variety of non-monetary costs when shopping for a car loan. While many car buyers choose to finance their purchase through a lender vertically integrated with a dealer (especially in the case of new car buyers), used car buyers frequently finance their purchase from a separate source.[15] Borrowers may seek loan preapproval before negotiating with the seller over purchase price to refine their own budget or avoid double marginalization (Busse and Silva-Risso, 2010). Alternatively, buyers may finalize a purchase price and then shop around for car loans to complete the purchase.

As car loan pricing is specific to the credit risk of each individual, obtaining price quotes

[15] Recall that our sample consists of so-called direct auto loans originated through a lending institution, as opposed to indirect auto loans where dealers broker a digital search across multiple lenders at the time of the auto purchase and mark up the resulting loan offer. We discuss the question of digital search in A.3.

most often entails filling out a loan application, undergoing a credit check, and potentially verifying assets and income. Here, we focus on the dimension of search costs that scales with time and distance, such as the time and hassle required to travel to a branch and physically sign financial paperwork or the cost of ascertaining the choice set of potential lenders (see Appendix Section (A.3) for a discussion of digital search). However, we note that there are many other dimensions that we do not measure over which search is costly, for example the disutility of filling out financial paperwork, general unawareness of price dispersion, and potential concerns that numerous credit-registry queries negatively impact credit scores (Liberman, Paravisini, and Pathania, 2016).

To proxy for search costs, we use FDIC and NCUA data to identify the physical location of every bank branch and credit union branch in the United States for each year in our application data. We then create a measure we call proximity to financial institutions (PFI), which measures the driving-time density for an individual borrower by geocoding and counting the number of physical branch locations within a 20-minute drive of the borrower, similar to the approach employed by Degryse and Ongena (2005). The calculation of actual driving times relies on posted speed limits along current driving routes and abstracts from traffic conditions and any changes to the road network between the time of loan origination and 2016 (the date of our driving-time data). We calculate driving distances and trip durations only for those institutions that existed at the time of loan origination. This driving-time density measure PFI is designed to capture the effort, proxied by travel time and physical distance, for each borrower to search out a lending institution that is within a reasonable distance from their home.[16] Our PFI measure finds external validity in work by Goodstein and Rhine (2017), who show that households that live within close proximity to bank branches are more likely to have a bank account and thus less likely to use nonbank transaction products.

[16]Distance can also proxy for soft-information producing relationships (see Nguyen, 2017), although we do not believe auto-loan lending to be a particularly relationship-intensive credit product, a fact that is supported by the lack of adverse selection around discontinuities and the high R^2 statistics in our lending-rules regressions.

Our first measure of search propensity is the fraction of borrowers that accept an offered loan, a measure we refer to as the loan take-up rate. The loan take-up measure proxies for search insofar as borrowers who applied for but then decline an offered loan do so in favor of accepting a different loan. While borrowers could reject loans for different reasons, including the decision not to originate any loan, we view differences in the decision to accept an offered loan around lending thresholds as a reasonable measure of differences in borrowers' propensity to engage in additional loan searching. Using reported FICO scores at the time of loan application, we estimate differences in take-up rates around FICO thresholds.

If PFI is correlated with search costs, and thus influences the propensity to search, differences in loan take-up-rates around FICO thresholds should be larger in areas with higher PFI (i.e., more lenders live close by, thus lowering search costs). If a borrower faces higher search costs (lower PFI, i.e., lower driving-time densities), LOT borrowers in that area (those offered unfavorable loan terms) would be less likely to reject unfavorable loan terms in favor of searching for better terms elsewhere. Importantly, in a difference-in-differences spirit, our empirical specification measures *differences* in loan take-up rates around FICO thresholds. We then compare differences in loan take-up rates for borrowers in high versus low PFI areas.

The median borrower in our application data lives within a 20-minute drive of 72 lending institutions. Borrowers in the 25th percentile of driving distance live less than a 20-minute drive from 23 institutions, as compared to 168 institutions for borrowers in the 75th percentile. Just under 4% of applicants in our sample live in an area with one or fewer lending institutions within a 20-minute drive. In contrast, 45% of applicants live within a 20-minute drive of at least 100 different lending institutions. Our baseline results categorize borrowers with fewer than 10 lending institutions within a 20-minute drive as facing high search costs (lowest 15% in the density distribution) to capture nonlinearity in the effect of additional nearby lenders on search costs.[17] In Section 6.2.1 below, we verify robustness of our results

[17]In unreported results, we split the sample in other ways based on quantiles of the driving-density distribution. Top quartile versus bottom quartile driving densities offer qualitatively similar results to our

to the definition of high search-cost area.

Using our standard RD framework described by equation (2), we specify the dependent variable as an indicator equal to one for accepted loans and estimate differences in the take-up rate around FICO thresholds. In Table 6, we estimate RD regressions of equation (2) using the Calonico et al. (2014) estimator for the full sample and separately for borrowers with more than and less than 10 lending institutions within a 20-minute drive. For the full sample, column 1 documents differences in take-up rates of 9.7 pp around the threshold; that is, borrowers randomly drawing high markups from a dispersed distribution of prices are 9.7 percentage points less likely to accept the offered loan. We estimate the same regression in high and low search-cost areas and report results in columns 2 and 3, respectively. In high search-cost areas, take-up rates are essentially identical between LOT and ROT borrowers. In contrast, differences in take-up rates around FICO thresholds are 11.6 pp in low search-cost areas (column 3). In other words, borrowers in low search-cost areas are much less likely to accept a loan if they are assigned a high interest-rate markup, whereas high search-cost borrowers are likely to accept loan terms regardless of where they fall in the distribution of prices.[18] Finally, column 4 shows that the difference between the discontinuity coefficients in the high and low search-cost samples is statistically significant. Given the mean take-up rate of 0.52 (Table 2), being in a high search cost area has over a 20% effect on the likelihood a borrower will accept an expensive loan.

reported results. Above- and below-median splits produce similarly signed (but smaller) differences in the difference of take-up rates across thresholds.

[18]One threat to our interpretation of Table 6 is that the magnitude of the first stage may be different in high and low search-cost areas. Note, however, that for this to explain our results, low search-cost areas would have to have *larger* discontinuities in rates. Appendix Table A2 finds a small and insignificant difference in the size of the average interest-rate change at a FICO threshold across our measure of search costs. See Appendix Section A.1 for further discussion.

6.2.1 Robustness to Definition of High Search-Cost Area

Figure 6 probes whether our take-up results are sensitive to the choice of cutoff for high search-cost area. Each point and associated confidence interval plots $\hat{\gamma}_2$ from estimating

$$takeup_{ict} = f(\widetilde{FICO}_{ict}) + \gamma_1 HighSearchCost_i + \gamma_2 HSC_i \cdot \mathbb{I}(\widetilde{FICO}_{ict} \geq 0) + \alpha_{cz(i)} + \delta_t + \varepsilon_{ict} \tag{3}$$

via OLS where $f(\widetilde{FICO})$ is an RD quadratic in the normalized FICO score running variable ($f(\cdot)$ includes interactions between each term and an indicator for $\widetilde{FICO} \geq 0$) and $High\,Search\,Cost$ (HSC) is a dummy equal to one when the number of lenders within a 20-minute drive is less than a particular cutoff. We estimate equation (3) varying the maximum for high search cost to be five institutions, 10, 15, etc.

Two interesting patterns emerge from the plotted coefficients (note that the plotted coefficients correspond to the coefficient we report in column 4 of Table 6). First, most of the point estimates are negative. Recall that the main effect of a FICO discontinuity on take-up rates is positive; ROT borrowers are much more likely to accept an offered loan because they are being offered much more favorable interest rates than LOT borrowers. Being in a high search-cost area thus negates the main effect of an interest rate markup on take-up. Second, while borrowers with five or less PFIs are rare and so the resulting take-up discontinuity is estimated imprecisely, the estimates for borrowers who have less than 20 PFIs are significantly less than zero. Moreover, estimates for definitions near our baseline cutoff of 10 PFIs (15th percentile) are similar, suggesting that our conclusions are fairly insensitive to the exact definition of high search cost. Finally, the point estimates tend toward zero in the number of PFIs used in the definition of high search cost, consistent with the intuition that from a search-cost perspective, the value of an additional PFI diminishes (i.e., each lender matters a lot when there are only a handful of nearby lenders compared to when there are, for example, over 50 lenders nearby).

6.2.2 Explaining Variation in Price Dispersion with Search Costs

Our PFI measure of search costs also has power to explain some variation in price dispersion. One prediction of search theory is that lowering search costs should result in lower price dispersion, as consumers facing low search costs are more likely to be informed about the complete distribution of available prices. In Figure 7, we plot the cumulative density function of the spread-to-lowest-available-rate variable. The solid line is the CDF for borrowers facing low search costs and the dotted line is for high search cost borrowers. The CDF plots indicate that a smaller fraction of high search-cost area borrowers accept loans with lower spreads to the lowest available rate, i.e., price dispersion is larger in high search-cost areas. Although the magnitude of the difference between the two distributions is likely attenuated by the limited coverage of our data relative to the entire local auto-loan market, Kolmogorov-Smirnov tests confirm that the distributions are statistically different at the 1% confidence level.

6.3 Direct Evidence of Loan Search

In this section, we present a more direct measure of loan search by linking loan applications across financial institutions for the same borrower to evaluate whether the propensity of an individual borrower to search for an auto loan, as measured by the number of filed loan applications, varies with our PFI proxy for search costs. As before, we divide borrowers into high and low search-cost areas based on those borrowers with more/less than 10 lending institutions within a 20-minute drive, respectively. Table 7 reports that applicants in high search-cost areas (column 1) apply for an average of 1.08 loans per vehicle purchase. In contrast, applicants in low search-cost areas (column 2) apply for an average of 1.10 loans per vehicle purchase. Column 3 shows that this difference is statistically significant at a 1% significance level. Although the difference is small in magnitude, these average differences are a lower bound as we do not observe applications to any lender not in our data. If our data coverage is 1% of the prime auto loan market, inflating the difference then this would imply that borrowers with high search costs apply for two fewer auto loans than borrowers nearby many prospective lenders. Incidentally, as discussed above in the discussion of Figure 1, an additional two price quotes would be sufficient for the average borrower to find the best rate in our data. Note, too, that equilibrium applications need not change to be affected by search—in the sequential search equilibrium of Stahl (1989), consumers always buy from the first seller in equilibrium. Regressions of loan applications per purchase on the count of lending institutions within a 20-minute drive also confirm a positive and significant relationship. These results are consistent with the more comprehensive indirect evidence presented above that borrowers facing high search costs search less and accept worse rates than borrowers facing relatively lower search costs.

6.4 Comments on Identification

While we view our empirical setting as quasi-random assignment of interest-rate markups to borrowers around a cutoff, there could be several time-varying or time-invariant factors correlated with our search cost measure that could affect equilibrium price dispersion and take-up behavior. See the Appendix for additional results and a discussion of identification issues related to our measure of search costs.

7 Distortions to Consumption

To estimate how search frictions in credit markets can have real effects on consumption, we next establish that being treated with a higher interest rate causally distorts loan and purchase decisions. Whether a given credit-market imperfection constrains consumption is difficult to ascertain because it requires estimating counterfactual consumption in the absence of the alleged friction. However, we can determine the existence of potential distortions by evaluating the auto-purchasing decisions of borrowers on either side of the documented FICO thresholds. Given the empirical result that borrowers are ex-ante similar around FICO thresholds, we start with the null hypothesis that borrowers around FICO thresholds would also have similar demand for cars, conditional on obtaining the same set of financing terms. Exploiting our ability to observe the exact amount that each borrower spends on a car, we test whether borrowers spend differently around the observed FICO thresholds and whether the composition of borrowers accepting loans changes across thresholds.

Figure 8 plots car purchase amounts around the normalized FICO threshold. Purchase amounts are smooth leading up to the FICO threshold and then jump discontinuously at the threshold. Using the same RD design used in our first-stage analysis above, we formally test for statistical differences in purchase amounts. As before, we estimate equation (2) by controlling for commuting-zone fixed effects, quarter-of-origination fixed effects, allowing for a local linear function of the running variable, bias-correction using the local quadratic approach of Calonico et al. (2014), and using a bandwidth of 19 around the normalized FICO threshold with a uniform kernel. Table 8 presents these reduced-form results. Borrowers quasi-randomly offered more expensive loans spend an average of $715 less on the cars they purchase (a 4% effect on average). Column 2 presents results with loan amounts as the dependent variable. Originated loan sizes are an average of $1,143 lower (7%) on the expensive side of a detected FICO discontinuity. The fact that loan sizes increase by larger amounts around the threshold than purchase amounts indicates that, ex-post, borrowers on the right side of the cutoff are approved for and take up higher loan-to-value (LTV) ratios. Column 3 of Table 8 indicates that ex-post LTV ratios are an average of three percentage points higher for borrowers to the right of FICO thresholds. Given that ex-ante DTI ratios in the loan application data are continuous around the thresholds (Table 4), we interpret these results as further evidence of the easing of credit terms for ROT borrowers. That is, ex-post, ROT borrowers are offered lower rates, longer terms, and apparently allowed higher ex-post LTV and DTI ratios.

Detailed data on loan amounts and loan terms allow us to calculate the implied monthly payment of borrowers on either side of the thresholds. In column 4 of Table 8, we test whether ex-post monthly payments are different around the thresholds. On average, monthly payments increase by \$6.53 for ROT borrowers. Shorter terms and higher interest rates lead LOT borrowers to purchase less expensive cars and use less financing in their purchase than ROT borrowers, essentially purchasing less car and less credit for only a 2% reduction in monthly payment.[19]

This evidence of otherwise similar borrowers spending different amounts on the cars they purchase as a result of the financing terms they are offered is consistent with consumption distortion. One concern is that borrowers *who accept loans* on either side of FICO thresholds might differ ex-ante in their ability to service debt (even if borrowers are balanced at the application stage), violating the smoothness condition required for valid RD. An alternative explanation is the possibility that dealers price discriminate, exploiting borrowers' increased marginal willingness to pay by charging more for the exact same car than otherwise similar borrowers with more expensive financing.[20]

We test for this possibility by controlling for year-make-model (e.g., 2013 Honda Accord) fixed effects in our RD regressions. Column 1 of Table 9 reports results when controlling for make-model fixed effects. Even within a make and model category, borrowers quasi-randomly assigned expensive credit continue to spend \$647 less on cars, suggesting that the bulk of the purchasing behavior we observe in Table 8 is not driven by people choosing to purchase different model cars as a result of their assigned credit. Contrasting the coefficients in columns 1 and 2 provides indirect evidence as to the nature of the substitution patterns in this market. When we include year-make-model fixed effects in column 2, we find a much smaller change in purchase price at the discontinuity. To the extent that there is an effect on prices (our 95% confidence interval allows us to rule out effects larger than \$164), such an effect would be an order of magnitude lower than the effect of the average discontinuous rate change on the present value of interest payments if the loan size had remained fixed (roughly \$1,300 for the average loan). Because fixing the model year of a car has such large

[19]See Argyle et al. (2017a) for related evidence on monthly payment targeting in retail auto loans.

[20]See Argyle et al. (2017b) for further evidence on the capitalization of consumer financing terms into asset prices.

explanatory power on the effect of the interest rate change at the threshold, we conclude that much of the effect in column 1 is explained by substitution within a model and across model years. That said, we hesitate to overinterpret column 2 given the loss of power from including so many fixed effects; borrowers with more affordable credit could be paying slightly more for the same make-model-year either because their marginal willingness to pay increased and was extracted by the dealer or by choosing a nicer car within a make-model-year (extra add-on features, lower mileage, etc.). Reconciling the strong effect on purchase prices within make-models and the relatively weaker effect on purchase prices within make-model-years, column 3 provides direct evidence with vehicle age at purchase in months as the dependent variable (controlling for make-model fixed effects since vehicle age would be collinear with year-make-model fixed effects). Borrowers with access to easier credit purchase 3.4 months newer cars, suggesting that roughly one in four borrowers respond to being on the left (right) of interest rate discontinuities by buying a car that is one model year older (newer), keeping their monthly payments roughly constant.

How do borrowers respond to being arbitrarily offered more expensive credit than their creditworthiness would warrant? The evidence presented in Tables 8 and 9 indicates that borrowers offered expensive credit spend less on their car purchases by selecting an older car than they would have otherwise, potentially bargaining harder on purchase price, originating smaller loans, and having slightly smaller monthly payments. We view this as evidence that borrowers' inability to costlessly identify the best available loan terms distorts consumption away from efficient levels.

7.1 Evaluating Alternative Explanations: Adverse Selection

In this section, we address the possibility that LOT borrowers who take up loan offers are different on unobservable dimensions from ROT car buyers. While we have already demonstrated that interest-rate markups seem quasi-randomly assigned (borrowers seem similar on observable dimensions at the ex-ante application stage), an alternative explanation for our results is that (unobservably) high credit-quality borrowers who are arbitrarily offered expensive interest rates withdraw their loan applications and look elsewhere for credit.[21] Under this private-information scenario, borrowers who follow through with the origination of expensive loans are those who know they are of poor credit quality and unlikely to do better given their unfavorable soft attributes. Lending institutions could also recognize that borrowers who choose to accept the unfavorable terms are indeed lemons, as anticipated, and so the arbitrary thresholds reinforce an equilibrium that separates high credit-quality borrowers from low credit-quality borrowers, with the appropriate pricing differences offered to each borrower type.

We test for the possibility that adverse selection drives the observed equilibrium outcomes in our data by comparing ex-post borrower performance around the FICO thresholds. If an unobservable selection process guides differences in who accepts expensive loan offers, this should be revealed by ex-post credit outcomes as lower credit-quality borrowers eventually default more. To test this hypothesis, we first specify as a dependent variable in our RD setting the number of days a borrower is subsequently delinquent on their car loan. The coefficient in column 1 of Table 10 estimates that ROT borrowers are an average of 1.23 fewer days delinquent than LOT borrowers, indicating that borrowers on either side of the threshold do not exhibit economically meaningful or statistically significant differences in delinquency.[22] Similarly, ROT borrowers are 0.01% more likely to have their loan charged off (written off as a loss by the lender, column 2) and 0.01% less likely to be in default (over 90 days past due, column 3), both of which we view as relatively precise zeroes.

A novel feature of our dataset allows for a second test of private information as an explanation for our observed results. As a means of monitoring borrowers, many lending in-

[21] Adverse selection is not the only alternative explanation for our observed results around thresholds. For example, FICO thresholds could promote the steering of financially unsophisticated borrowers into higher rate loans. However, as any such borrower naïveté is not manifest in differences at loan application, more expensive car purchase prices paid, differential ex-post default rates, or differences in ex-post credit scores, it is unlikely to be a driving factor for the phenomena we document here.

[22] The sample size differs across columns in Table 10 because of inconsistent data coverage of all monitoring variables across lenders.

stitutions in our dataset pull credit scores on borrowers after loan origination. Ex-post credit score queries occur as frequently as every six months, and, in a few cases, as infrequently as once post-origination. The most common convention for the subset of institutions that pull credit ex-post is to pull credit scores once a year. Ex-post credit scores allow us to calculate changes in credit scores over time, capturing broad changes in borrower distress and financial responsibility. Any unobserved heterogeneity driving selection into loan take-up should impact credit scores over time if low credit-quality borrowers (for whom the below-threshold expensive interest rate is "fair") are the only ones to originate such loans. Using the subsample of institutions that collect updated FICO scores after origination, we use the percentage change between credit scores at origination and the most recently observable credit score as the dependent variable in our RD framework. Results presented in column 4 of Table 10 show no meaningful differences in credit score changes for borrowers around the threshold.

As an aside, a zero net effect on ex-post outcomes may still belie changes in borrower composition if there is a causal effect on loan performance of the change in contract terms at the discontinuity. Nevertheless, for a few reasons, we view it unlikely that there would be a direct effect of the FICO discontinuity on subsequent outcomes. First, if higher interest rates did cause LOT borrowers to default more, that would have to be combined with *advantageous* selection into origination on the expensive side of discontinuities to explain a net zero effect on discontinuities. Second, any increase in monthly interest expenses would be small relative to the size of a household's monthly budget in our sample. Moreover, because of endogenous changes in loan size and maturity in response to interest-rate discontinuities, the change in monthly payment that we observe is small (Table 8) such that there is little change in the debt-service capacity as a result of the discontinuities.

Taken together, the evidence on borrower delinquency, defaults, and ex-post changes in credit scores indicates that LOT borrowers do not represent meaningfully different credit risks as compared to otherwise similar ROT borrowers. While adverse selection is undoubtedly a motivator of many features of retail car loan markets (Adams, Einav, and Levin, 2009), information asymmetries do not appear to be a primary determinant of the acute differences in lending behavior around the observed FICO thresholds.

8 Interest-rate Pass-through

While the evidence above supports our conclusion that costly search significantly distorts consumption, in this section we investigate whether search frictions affect origination interest rates in aggregate. Market power, which could arise de facto from search costs or through more classical channels like the concentration of market shares, could theoretically increase or decrease pass-through (Bulow and Pfleiderer, 1983) depending on the shape of the residual demand curve faced by each lender. We examine the relationship between comparable-maturity risk-free interest rates, proxied by the five-year treasury rate, $Treas5_t$, and the origination interest rates of car loans. To do so, we estimate a pass-through regression explaining individual interest rates in our data r_{igt} in location g in month t

$$r_{igt} = \beta_1 Treas5_t + \beta_2 Treas5_t \cdot HighSearchCost_g + \beta_3 HSC_g + X_i'\gamma + \xi_{FICO(i)} + \alpha_{cz(g)} + \delta_t + \varepsilon_{igt} \tag{4}$$

where $High\,Search\,Cost$ is an indicator equal to 1 if location g has 10 or fewer lenders within a 20-minute drive, X_i is a vector of loan-level controls including loan amount, loan maturity, and an indicator for high-cost loans (loans with rates above 6% that are likely evaluated as high credit risks and less responsive to risk-free rates). The terms $\xi_{FICO(i)}$, $\alpha_{cz(g)}$, and δ_t are fixed effects for FICO bin, Commuting Zone, and month, respectively.

Table 11 reports the results of estimating specifications resembling equation (4). Column 1 shows that for a 100-basis-point (bp) decrease in the five-year Treasury note, car loan interest rates decrease on average by 61 bp that month. The interaction term between high search cost and the five-year note yield indicates that high search-cost areas decrease by 1.2 bp less for every 100 basis point decrease in the five-year, passing through less of innovations to the risk-free rate. Column 2 controls for month fixed effects, absorbing the main effect of the level of the five-year note and any other macroeconomic changes that affect interest rates nationwide. Column 3 additionally controls for loan amount, maturity, and a high-cost dummy for loans with interest rates more than 6%, whose rates are likely more a function of unobservables than monetary shocks. In column 3, the main effect of the five-year Treasury yield is 15 bp for every 100 bp movement in the five-year. Column 4 absorbs this main effect via month fixed effects that account for other changes in macroeconomic conditions (i.e., those that drive changes in the five-year). In all specifications, the interaction term between high search cost and interest rates is negative and significant, indicating attenuated pass-through of interest rate shocks to areas with high search costs. Taking the main effect of five-year rates in column 3 as the pass-through for low search-cost areas and the interaction term as the reduction in pass-through for high search-cost areas, we estimate that high search-cost areas have 12% (column 3) to 17% (column 4) less of a connection between monetary shocks and originated interest rates realized by households. The R^2 of estimating equation (4) indicates that this parsimonious model explains about half to three quarters of the variation in interest rates across time and space.

While less robustly identified than our RD design results above because there may be other correlates of high search cost that affect pass-through, we interpret these results as consistent with the idea that in areas with high search costs, originated car loan interest rates are less reactive to monetary policy because even if some lenders do decrease rates in response to changes in risk-free rates, borrowers are less likely to discover those prices. See Appendix Section A.4 for evidence that our search cost measure is not simply a proxy for the sort of market concentration that Scharfstein and Sunderam (2016) and Dreschler, Savov, and Schnabl (2017) demonstrate inhibit the pass-through of monetary policy.

9 Conclusion

Mounting evidence indicates that credit-market imperfections influence household debt outcomes. A parallel empirical search literature has established the costliness of learning prices in a wide variety of markets. In this paper, we make three main points.

First, we present evidence that substantial price dispersion exists in retail auto lending markets. About 54% of borrowers in our sample (for whom a comparable borrower exists) did not originate a loan with the lowest available interest rate. The average borrower in our data pays 130 bp more than the best rate available to observationally similar borrowers.

Second, we demonstrate the importance of considering search costs as a credit market imperfection with material effects on loan outcomes. In a setting where the gains to search are potentially very high, we show that borrowers' acceptance of dominated loan terms is related to measures of search costs. Because arbitrary pricing schedules vary across lending institutions within the same commuting zone, borrowers on the expensive side of FICO discontinuities in loan pricing at one institution would be more likely to find themselves on the favorable side of a pricing threshold at a different institution. Absent search frictions, borrowers would be much less likely to accept dominated loan terms. However, we find that for borrowers randomly offered high interest rates in our sample, a lower interest rate loan was frequently originated by an equally creditworthy borrower purchasing a similarly priced car on a similar date, suggesting that borrowers are either unwilling to search for more favorable loan terms or are unaware of the benefits of doing so because of the cost of information acquisition. Proxying for the costliness of acquiring pricing information on loan terms with the number of institutions within a 20-minute drive from a borrower, we show that in areas with higher search costs, borrowers apply for fewer loans, face more dispersed prices, and are more likely to accept inferior loan terms, limiting their access to marginal prices in credit markets. Although we have focused on probing the interaction between our detected discontinuities in pricing rules and search costs, the evidence suggests that the broader retail car loan market is subject to costly search and the resulting price dispersion.

Third, we estimate distortions in consumption that are associated with borrowers' acceptance of dominated loan terms. Borrowers that accept randomly-offered higher interest rates on average purchase cars \$715 lower in value, 3.4 months older, and borrow \$1,144 less than otherwise similar borrowers that accept randomly-offered lower rates. This highlights the importance of well-functioning consumer credit markets in determining durable goods consumption patterns. Moreover, when consumer search is costly, the pass-through of credit expansions (e.g., from easier monetary policy) may be muted. Relative to consumer search models with inelastic unit demand where dispersed prices have no associated deadweight loss and just represent a transfer from buyers to sellers, there are welfare consequences of costly search in the real world. Given demand with at least some degree of elasticity, consumers facing markups from a market characterized by price dispersion may choose not to purchase the given good at all or, conditional on purchasing it, may adjust the characteristics of the purchased good away from their first-best level of consumption.

Even with a well-developed financial sector including secondary markets for many forms of consumer debt, household consumption is still distorted by credit market imperfections such as costly search. At least one answer to Zinman's (2014) query as to why efficient risk-based pricing is still not ubiquitous in the era of big-data-based credit modeling appears to be demand-side obstacles to finding lowest available prices. While the existence of price dispersion has been documented in prior empirical work, we provide novel estimates of the cost of search at the extensive and intensive margin. Even with the possibility of shopping for interest rates online, searching for consumer credit products remains an opaque, local, and costly process for many borrowers. This relationship between costly search and distortionary credit market imperfections extends our understanding of equilibrium price dispersion to credit markets and could motivate extra regulatory attention on so-called banking deserts.

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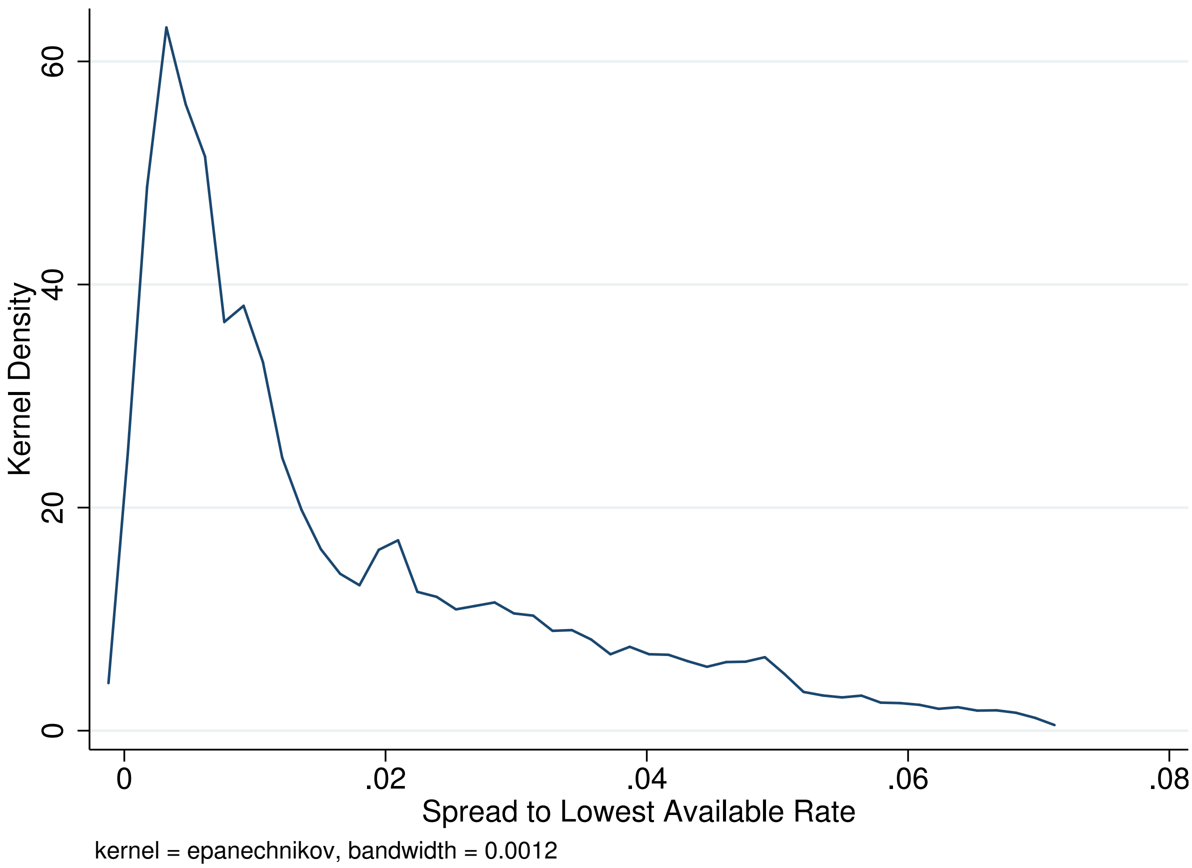
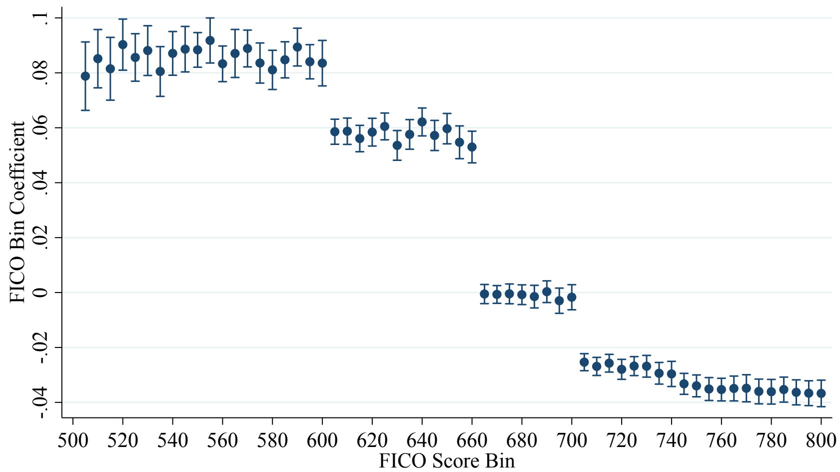
Figure 1: Density of Spread to Lowest Available Rate



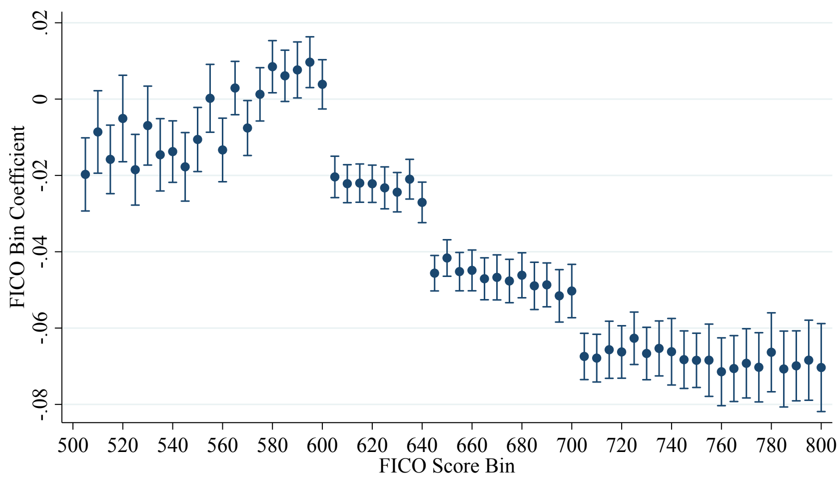
Notes: Figure reports the kernel densities of the spread (in percentage points) to the lowest available rate for borrowers not receiving the best available rate in their cell. A cell is defined as all borrowers in the same commuting zone taking out a loan in the same $1,000 collateral-value bin, five-point FICO bin, 10-point DTI bin, six-month time period, and loan maturity.

Figure 2: Examples of FICO-Based Discontinuities in Interest-Rate Policies

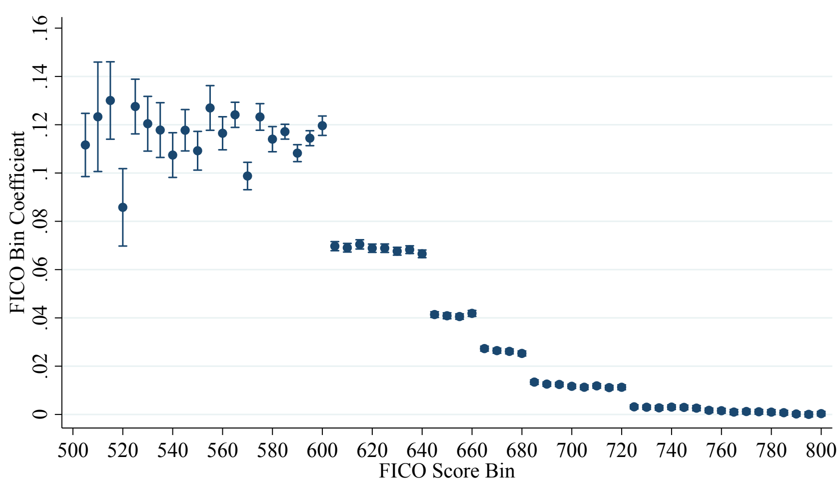
A. Sample Lender #1



B. Sample Lender #2



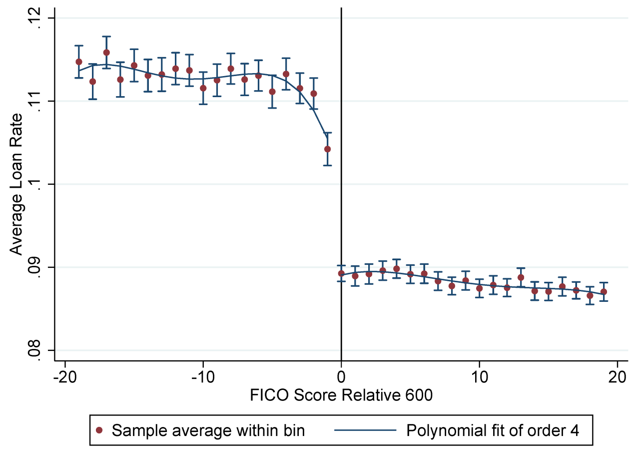
C. Sample Lender #3



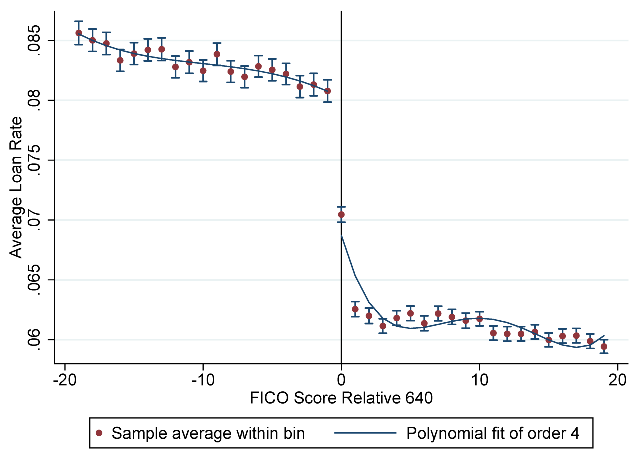
Notes: Each panel plots estimated interest-rate rules (with 95% confidence intervals) for a lender in our sample. Loan rates in percentage points are regressed on five-point FICO bin indicators as in equation (1).

Figure 3: FICO-Based Lending Policies - Interest Rates

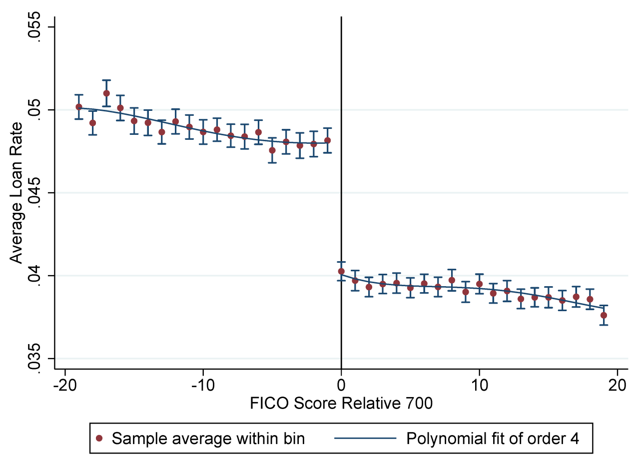
A. Interest Rates Around FICO = 600 Discontinuities



B. Interest Rates Around FICO = 640 Discontinuities

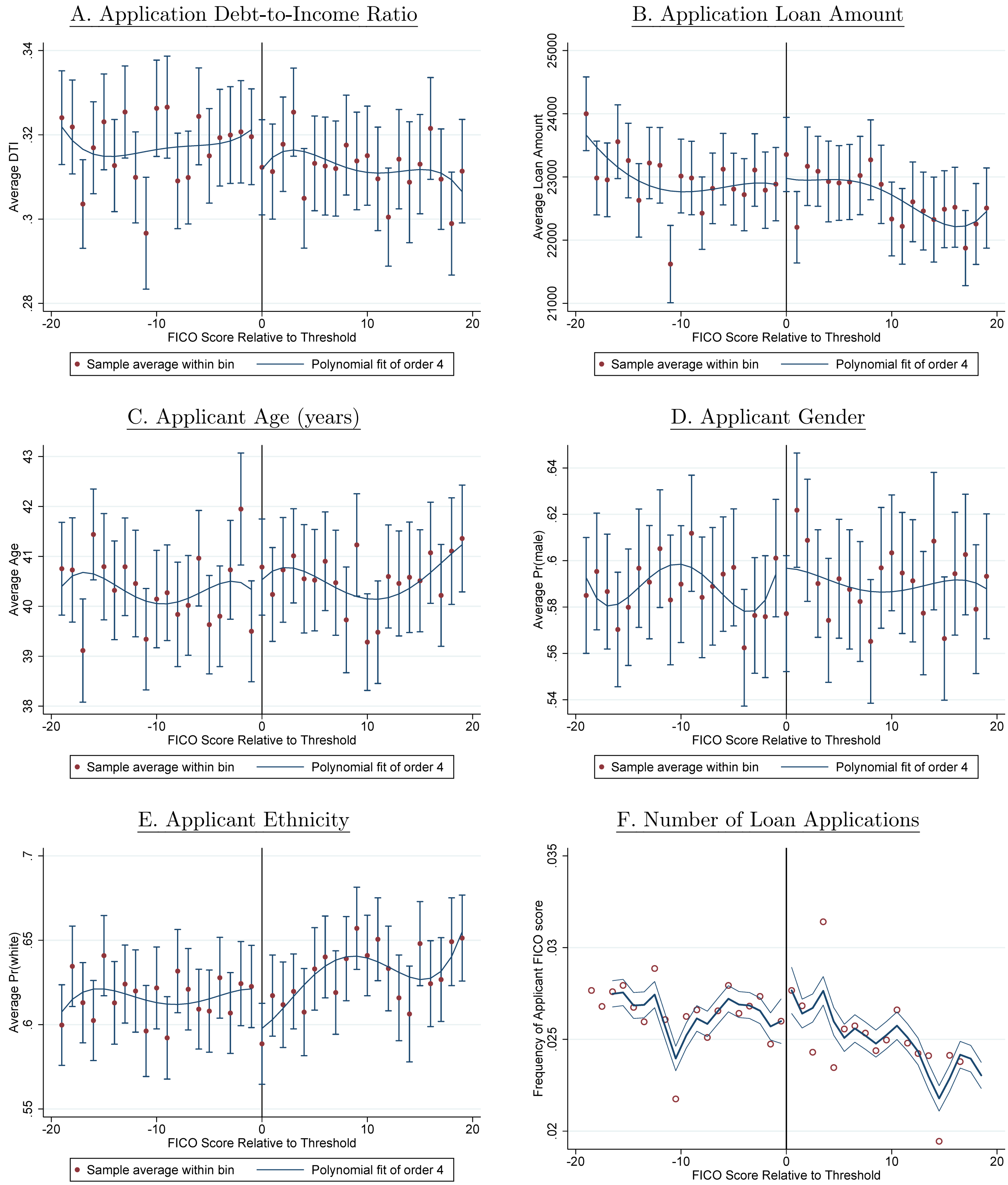


C. Interest Rates Around FICO = 700 Discontinuities



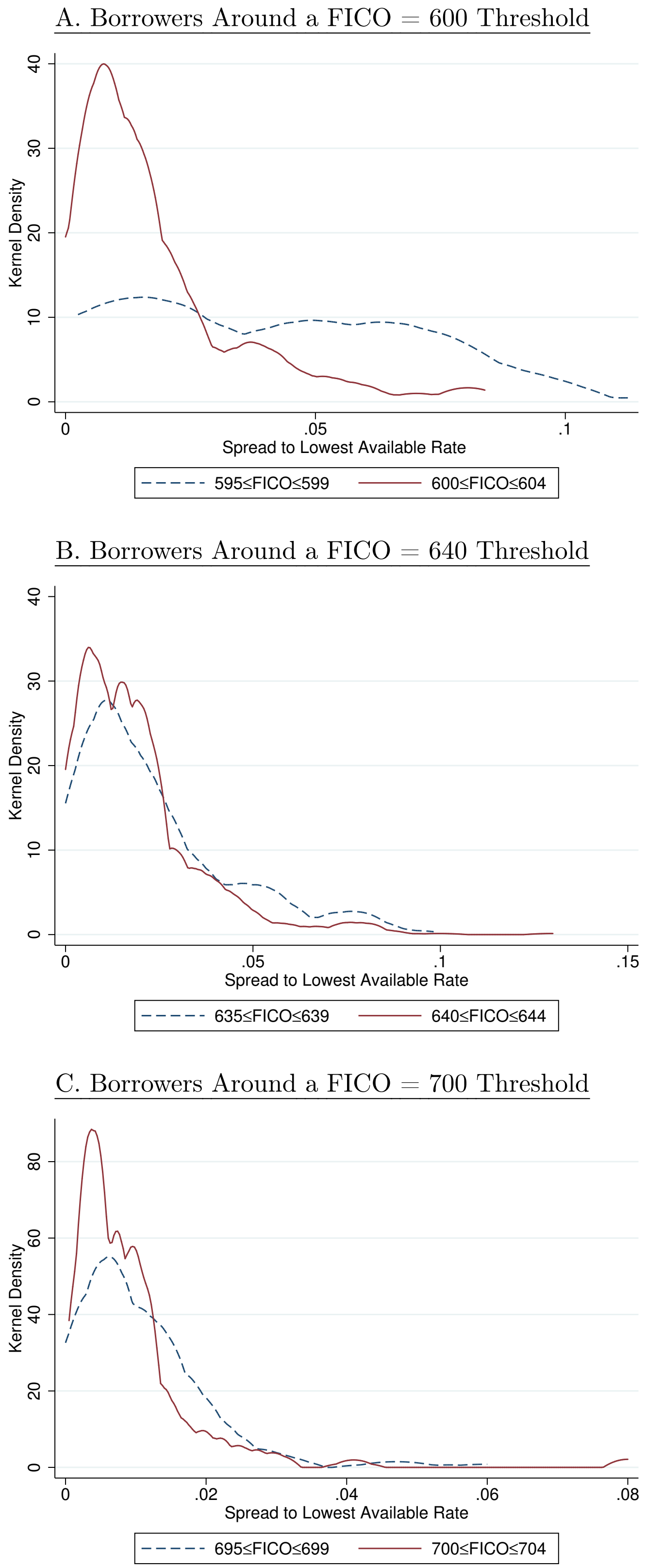
Notes: Figures plot average interest rates against borrower FICO scores normalized to pricing discontinuities using Calonico et al. (2014) local linear estimates and 95% confidence intervals for institutions with pricing discontinuities detected at FICO scores of 600, 640, and 700, respectively.

Figure 4: Balance of Borrower Characteristics Across FICO Thresholds



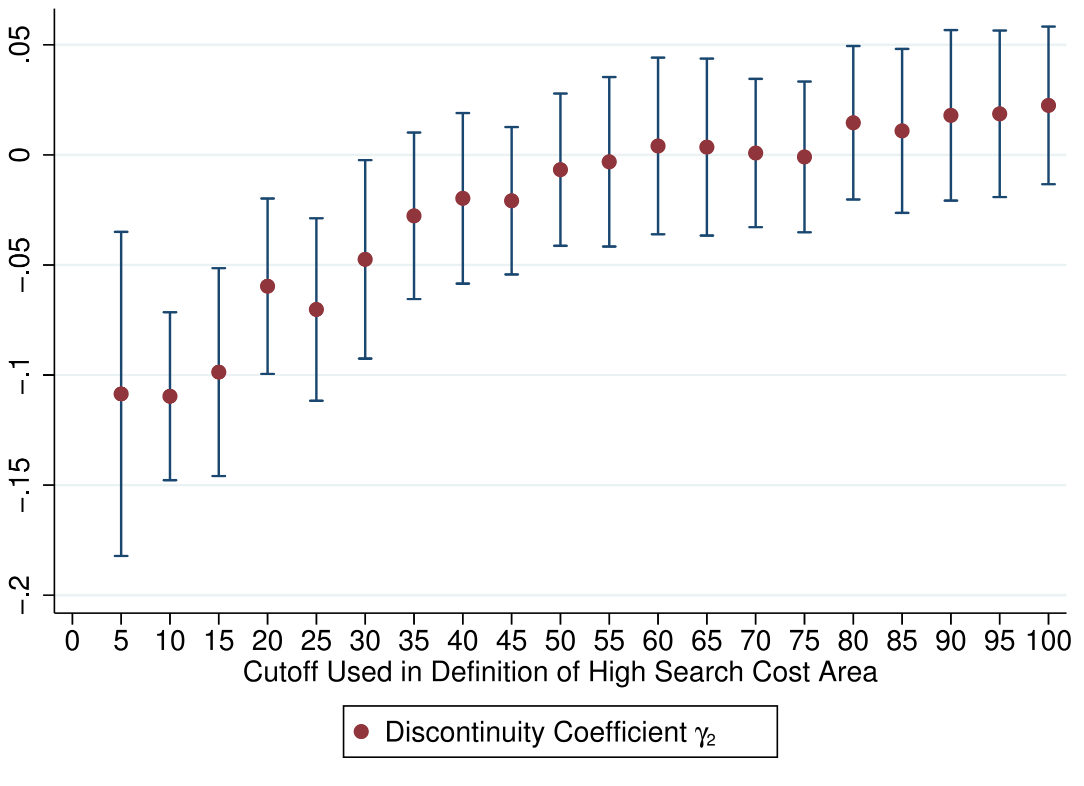
Notes: Figures plot average values of ex-ante borrower characteristics around FICO thresholds for institutions with detected discontinuities using Calonico et al. (2014) local linear estimates and 95% confidence intervals. Applicant gender in panel D is an indicator for male, and ethnicity in panel E is an indicator for whether the applicant is estimated as white by the lender. Panel F plots the application count within each normalized FICO bin along with the estimated McCrary (2008) test.

Figure 5: Density of Spread to Lowest Available Interest Rate Around FICO Thresholds



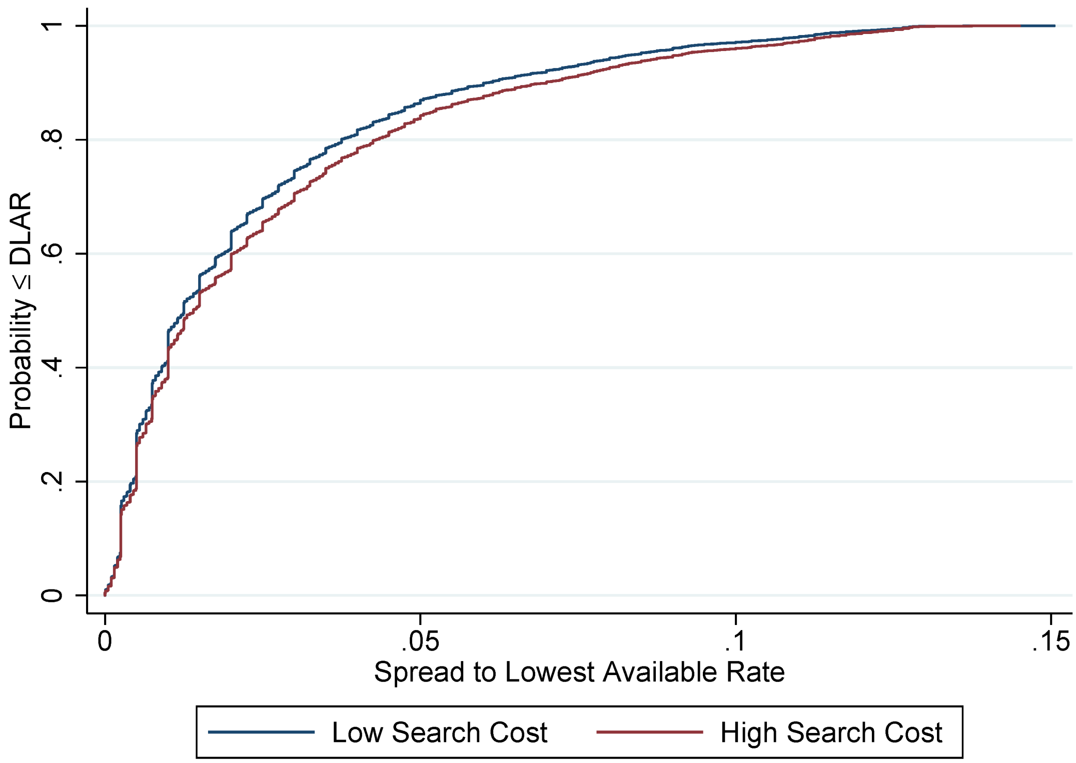
Notes: Figure reports the kernel densities of the spread (in percentage points) to the lowest available rate for borrowers with FICO scores just to the left of a threshold that borrowed from institutions with lending thresholds of 600, 640, and 700, respectively.

Figure 6: Take-up Rate Discontinuities Differences by Search Cost Definition



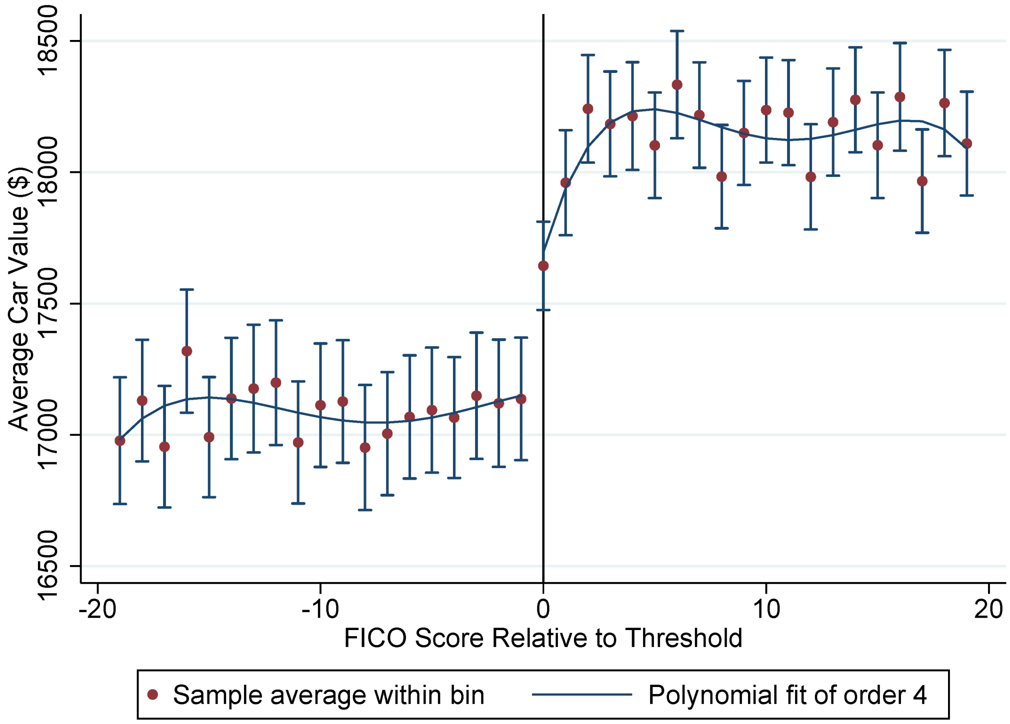
Notes: Figure reports RD estimates of γ_2 in equation (3) of take-up on an interaction of the discontinuity with a High Search Cost dummy defined as less than or equal to indicated cutoff.

Figure 7: CDFs of the Spread to Lowest Available Rate by Search Costs



Notes: Figure plots estimated cumulative density functions of the spread to the lowest available rate (the difference between a loan's interest rate and the best rate among similar borrowers in its cell) by our measure of search costs based on the number of nearby lenders. See notes to Figure 1.

Figure 8: Effect of FICO Threshold on Value of Car Purchased



Notes: Figures plot average car prices around normalized FICO scores using Calonico et al. (2014) local linear estimates and 95% confidence intervals for loans originated by lenders with detected interest discontinuities at FICO thresholds of 600, 640, and 700.

Table 1: Summary Statistics

	Count	Mean	Std. Dev.	Percentile 25th	Percentile 50th	Percentile 75th
	A. Loan Applications					
Loan Term (months)	1,119,153	67.25	24.43	60	72	72
Loan Amount ($)	1,320,109	21,927.26	11,660.68	13,296.02	20,000	28,932.14
Loan Rate	1,131,240	0.05	0.05	0.02	0.04	0.06
FICO	898,339	647.94	118.23	605	661	720
Debt-to-Income	833,854	0.26	0.3	0.13	0.27	0.39
Take Up	588,231	0.65	0.48	0	1	1
	B. Originated Loans					
Loan Rate	2,434,049	0.05	0.03	0.03	0.04	0.06
Loan Term (months)	2,434,049	62.73	22.08	48	60	72
Loan Amount ($)	2,434,049	18,136.52	10,808.97	10,094	16,034	23,892
FICO	2,165,173	710.55	74.89	661	714	770
Debt-to-Income (%)	1,276,585	0.25	0.32	0.05	0.26	0.37
Collateral Value ($)	2,434,049	19,895.13	10,929.1	12,046.81	17,850	25,562.28
Monthly Payment ($)	2,434,049	324.4	159.21	210.93	297.02	405.56
	C. Ex-Post Loan Performance Measures					
Days Delinquent	1,589,843	23.41	221.99	0	0	0
Charged-off Indicator	2,434,049	0.02	0.13	0	0	0
Default Indicator	2,434,049	0.02	0.14	0	0	0
Current FICO	1,719,848	705.5	83.28	654	714	772
%ΔFICO	1,697,700	-0.01	0.09	-0.04	0	0.03

Note: Panels A–C respectively report summary statistics for loan applications, originated loans, and ex-post loan performance. Loan Rate is the annual interest rate of the loan. Loan Term is the term (in months) of the loan. Debt-to-Income is the ratio of all debt service payments to income. Collateral Value is the value of the car at origination. Days Delinquent is the number of days that a borrower has missed one or more monthly payments. Charged-off Indicator is a dummy for whether a loan has been written off the books of the lending institution. Default is an indicator for whether a borrower has been delinquent for at least 90 days. Current FICO is an updated FICO score for each borrower as of the date of our data extract. ΔFICO is the change in FICO score since origination as a fraction of the FICO score at origination.

Table 2: Summary Statistics for Estimation Sample with Identified FICO Discontinuities

	Count	Mean	Std. Dev.	Percentile 25th	Percentile 50th	Percentile 75th
			A. Loan Applications			
Loan Term (months)	53,841	65.69	34.4	60	72	72
Loan Amount ($)	58,516	22,828.07	11,773.89	14,235.45	20,668	29,992.5
Loan Rate	51,391	0.04	0.04	0.02	.04	.06
FICO	58,516	679.84	36.44	681	693	705
Debt-to-Income	40,066	0.31	0.20	0.20	.32	.43
Take-up	22,704	0.52	0.49	0	1	1
			B. Originated Loans			
Loan Rate	307,061	0.07	0.03	0.04	0.06	0.09
Loan Term (months)	307,061	61.8	22.75	48	60	72
Loan Amount ($)	307,061	16,767.3	10,230.39	9,306	14,954	22,027
FICO	307,061	649.54	36.1	624	645	683
Debt-to-Income (%)	181,667	0.26	0.28	0.10	0.27	0.38
Collateral Value ($)	307,061	17,728.78	9,729.16	10,828.61	15,925	22,625
Monthly Payment ($)	307,061	314.89	149.12	208.64	291.04	391.44
			C. Ex-Post Loan Performance Measures			
Days Delinquent	212,015	37.09	256.56	0	0	0
Charged-off Indicator	307,061	0.03	0.17	0	0	0
Default Indicator	307,061	0.03	0.16	0	0	0
Current FICO	239,873	646.34	68.69	608	651	692
$\%\Delta$FICO	239,873	0	0.09	-0.05	0	0.05

Note: Table reports summary statistics for the discontinuity sample (restricted to a 19-point bandwidth around detected FICO discontinuities in lender pricing rules). Panels A, B, and C describe loan applications, loan originations, and ex-post loan performance, respectively. See notes to Table 1 for further details.

Table 3: First-Stage Effects of FICO Discontinuity on Loan Rate and Loan Term

	(1) Loan Rate	(2) Loan Term
Discontinuity Coefficient	-0.0146	1.19
	[-17.25]	[4.60]
Commuting Zone FE	✓	✓
Quarter FE	✓	✓
Number of Observations	274,029	274,029

Notes: Table reports regression discontinuity estimates of equation (2), pooling the three discontinuities shown in Figure 3 by normalizing FICO scores around each threshold and using the estimator of Calonico et al. (2014). All specifications include commuting-zone fixed effects and quarter-of-origination fixed effects. Robust t-statistics reported in brackets are clustered by normalized FICO score.

Table 4: Loan Application Covariate Balance Regressions

	(1) Loan Amount	(2) Debt-to-Income	(3) Number of Loan Applications
Discontinuity Coefficient	-68.75	-2.33	0.058
	[-0.26]	[-0.45]	[1.07]
Commuting Zone FE	✓	✓	✓
Quarter FE	✓	✓	✓
Number of Observations	52,816	35,427	39

Notes: Table reports reduced-form RD results for the subset of institutions for which we have detailed loan application data. See notes to Table 3 for details. Each observation in the data used for column 3 represents a normalized FICO score. Robust t-statistics reported in brackets are clustered by normalized FICO score.

Table 5: Spread to Lowest Available Rate Summary Statistics

FICO Range	# of Cells	Mean # Borrowers in Cell	Mean	Std. Dev.	Percentile 25th	Percentile 50th	Percentile 75th
$595 \leq FICO \leq 599$	74	2.19	0.038	0.029	0.01	0.03	0.06
$635 \leq FICO \leq 639$	250	2.23	0.023	0.021	0.01	0.02	0.03
$695 \leq FICO \leq 699$	161	2.15	0.011	0.01	0.003	0.01	0.02

Notes: Table reports summary statistics for the spread between a left-of-threshold borrower's interest rate and the best available interest rate for borrowers in the same cell. Cells are defined as borrowers within the same commuting zone, 5-point FICO bin, $1,000 purchase-price bin, 10 percentage point DTI bin, maturity, who take out loans in the same six-month window. Within each of the matched bins, we calculate the average difference between the lowest interest rate in the cell and each individual loan in the cell. Summary statistics are reported for only those cells that contain at least two borrowers.

Table 6: Effect of Search-Cost Proxies on Loan Offer Take-up Decisions

Search Costs	Full	High	Low	Difference
	(1)	(2)	(3)	(4)
Discontinuity	0.098	0.0003	0.12	-0.12
Coefficient	[4.62]	[0.01]	[4.89]	[-2.29]
Quarter FE	✓	✓	✓	
Commuting Zone FE	✓	✓	✓	
Number of Observations	19,905	3,820	16,085	

Notes: Table reports results for reduced-form RD regressions of loan take-up (conditional on being offered a loan) separately for the full sample (column 1) and for borrowers in areas with low and high search costs in columns 2 and 3, respectively, using the specification in equation (2) (see text for definition of high and low search costs). Search costs are estimated using the number of lending institutions within a 20-minute drive. Robust t-statistics reported in brackets are clustered by normalized FICO score. See notes to Table 3 for estimation details.

Table 7: Number of Loan Applications per Vehicle Purchase by Search Cost Group

	High Search Costs (1)	Low Search Costs (2)	Difference (1) - (2)
Mean	1.083	1.104	-0.021
Standard Deviation	(0.317)	(0.352)	[-5.90]
Number of Observations	10,846	75,837	

Notes: Table reports average number of applications per vehicle purchase for applications with reported birthdates and nine-digit addresses. Standard deviations are reported in parentheses. Column 3 calculates the difference in means along with the robust t-statistic in brackets for the statistical significance of the difference between columns 1 and 2.

Table 8: Reduced-Form Effects of FICO Discontinuity on Origination Outcomes

	(1) Purchase Price ($)	(2) Loan Amount ($)	(3) Loan-to-Value Ratio	(4) Monthly Payment ($)
Discontinuity Coefficient	715.41	1,143.86	0.03	6.53
	[8.55]	[10.31]	[3.92]	[4.97]
Commuting Zone FE	✓	✓	✓	✓
Quarter FE	✓	✓	✓	✓
Number of Observations	274,029	274,029	274,029	274,029

Notes: Table reports reduced-form RD estimates of equation (2) using the estimator of Calonico et al. (2014). Columns 1, 2, and 4 are measured in dollars, LTV is the loan size-to-value ratio. Robust t-statistics reported in brackets are clustered by normalized FICO score. See notes to Table 3 for more details.

Table 9: Reduced-Form Effects Robustness to Vehicle Heterogeneity

	(1) Purchase Price	(2) Purchase Price	(3) Car Age (months)
Discontinuity Coefficient	647.42	47.02	-3.42
	[6.41]	[0.79]	[-6.82]
Commuting Zone FE	✓	✓	✓
Quarter FE	✓	✓	✓
Make-Model FE	✓		✓
Year-Make-Model FE		✓	
Number of Observations	247,493	247,485	247,493

Notes: Table reports reduced-form RD estimates of equation (2) on car purchase prices (columns 1–2) and car age in months (column 3). Columns 1 and 3 include make × model fixed effects and column 2 includes year × make × model fixed effects. Robust t-statistics reported in brackets are clustered by normalized FICO score. See notes to Table 3 for more details.

Table 10: Effect of FICO Discontinuities on Ex-Post Credit Outcomes

	(1) Days Delinquent	(2) Charge-off	(3) Default	(4) %ΔFICO
Discontinuity Coefficient	1.2269	0.0008	0.0009	-0.0019
	[0.34]	[0.33]	[0.44]	[-0.83]
Commuting Zone FE	✓	✓	✓	✓
Quarter FE	✓	✓	✓	✓
Number of Observations	190,895	274,029	274,029	213,385

Notes: Table reports RD estimates of equation (2) on ex-post loan and borrower outcomes. Days delinquent is the number of days a borrower is delinquent as of our data extract. Charge-off is an indicator for whether a loan has been written off the books of the lending institution. Default is an indicator for whether a borrower has been delinquent for at least 90 days. ΔFICO is the change in FICO score since origination as a fraction of the FICO score at origination for the subsample of institutions that report credit scores after loan origination. Robust t-statistics reported in brackets are clustered by normalized FICO score. See notes to Table 3 for more details.

Table 11: Pass-through of Rate Shocks to Origination Interest Rates by Search Costs

	(1)	(2)	(3)	(4)
5-year Treasury	0.56		0.53	
	[13.99]		[14.73]	
5-year Treasury × 1(High Search-Cost Area)	-0.02	-0.03	-0.03	-0.04
	[-2.99]	[-5.36]	[-5.36]	[-7.69]
1(High Search-Cost Area)	0.001	0.001	0.001	0.001
	[8.74]	[10.18]	[12.50]	[13.86]
Commuting Zone Fixed Effects	✓	✓	✓	✓
FICO Bin Fixed Effects	✓	✓	✓	✓
Month Fixed Effects		✓		✓
Loan-level Controls			✓	✓
Observations	2,529,362	2,529,362	2,529,362	2,529,362
R-squared	0.59	0.61	0.76	0.78

Notes: Table reports OLS estimates of equation (4) of origination interest rates on the level of five-year Treasury yields, an indicator for high search-cost areas, and their interaction. Robust t-stats in brackets clustered by month. Loan-level controls include loan amount, maturity, and a high-cost loan indicator.

A Identification Appendix

In this appendix, we discuss some additional robustness checks that support the interpretation of our results in the main body of the paper.

A.1 First-stage Heterogeneity

Our take-up estimates represent unbiased estimates of borrowing elasticities at the extensive margin to the extent that the discontinuities satisfy the identifying assumptions required of valid RD estimation, with one important caveat. The proximity of financial institutions (PFI) could influence first-stage estimates. It would not be surprising to find that below-threshold borrowers search more in high PFI geographies if differences in offered rates across thresholds are correlated with PFI. Appendix Table A2 reports estimates of differences in offered interest rates around thresholds for high and low search-cost geographies. High search-cost areas (low PFI) have estimated average differences of 1.51 percentage points across thresholds, compared to differences of 1.45 percentage points in low search-cost areas (high PFI). The differences in the first-stage estimates between high and low PFI areas are not statistically significant (as reported in column 3), suggesting that our measure of search costs does create bias through differences in first-stage effects.

A.2 Potential Omitted Variables Correlated with High Search Costs

A more immediate challenge to our inference is the interpretation of differences in borrowing elasticities across high and low search cost geographies. The number of PFIs is plausibly correlated with a host of unobserved variables, including the financial sophistication of borrowers, the advertising of credit (which of course influences search costs), or local (within-CZ) preferences, or economic or demographic conditions, to name a few. Candidate omitted variables could be time varying (e.g., hyper-local economic conditions) or time invariant (e.g., financial sophistication, brand loyalty preferences).

A.2.1 Addressing Time-varying Omitted Variables

If a given location has few nearby lenders because lenders anticipated forthcoming local economic shocks, then our proxy for high search costs may be correlated with a growing inelasticity of local demand that could generate our results even if the number of PFIs had remained high. We address possible bias from such time-varying omitted variables in the following way. We create a Bartik measure of PFI, calculated as the PFI for each borrower in our sample using 1990 financial institution locations. Using NETS data to calculate the location of every financial institution in the U.S. as of 1990, we calculate PFI for our sample of borrowers relative to 1990 financial institution locations. We then grow the 1990 PFI measure using the national growth rate in financial institutions from 1990 through the year of each observation in our sample period, calculating the national growth rate for each location excluding the contribution of that location to the aggregate changes. Variation in the Bartik-PFIs is thus driven by local branching concentration as of 1990 and aggregate variation in national branching trends, neither of which is likely to be correlated with time-series variation in local economic conditions during our sample period of 2005–2015. We sort our sample based on high- and low-Bartik PFI and estimate take-up elasticities as in equation (2). Results are reported in Appendix Table A3. These results indicate that borrowing elasticities in high-Bartik PFI geographies (low search costs) are higher than in low-Bartik PFI geographies (high search costs), with high search-cost areas being relatively insensitive to interest rates and low search-cost areas that exhibit strong and statistically significant reactions. These results suggest that time-varying economic shocks that occur during our sample period do not appear to be a compelling explanation for differences in loan take-up rates across high and low PFI geographies.

A.2.2 Addressing Time-invariant Omitted Variables

A second challenge to our inference is the issue of time-invariant omitted variables, such as financial sophistication or brand loyalty. Ultimately, the strength of the Bartik instrumentation strategy relies on the exogeneity of the 1990 branch network, which may itself be correlated with local characteristics. For example, borrowers with low financial sophistication could exhibit lower borrowing elasticities if they do not appreciate the costs associated with differences in loan interest rates (see, e.g., Stango and Zinman, 2009, and Bertrand and Morse, 2011). Related, borrowers may have a preference for borrowing from their current depository (although this, too, could be driven by unawareness of price dispersion arising from high search costs). While our regression-discontinuity design uses random assignment to one side or the other of a FICO threshold and thus conceptually holds such unobservables fixed, when we compare the effect of discontinuities in high and low PFI areas, we need to concern ourselves with possible time-invariant correlates of the number of nearby lenders. In general, our specifications above attempted to address bias caused by time-invariant omitted variables with commuting-zone fixed effects. However, while commuting-zone fixed effects address time-invariant omitted variables at the metropolitan-area level, they do not address likely variation in sophistication correlated with high or low PFI within a commuting zone. We address this concern in two ways: using eight-digit zip code fixed effects in our RD specification and with a difference-in-differences design.

First, we augment our RD specification to include fixed effects $\alpha_{zip8(i)}$ for the eight-digit zip code of borrower i. We then estimate

$$takeup_{ict} = f(\widetilde{FICO}_{ict}, \beta) + \alpha_{zip8(i)} + \delta_t + \varepsilon_{ict} \tag{5}$$

separately for high and low search cost areas, as before, with $f(\cdot)$ the same local linear function of the running variable described in the context of equation (2) above. Results from estimating equation (5) in Table A4 are identified off of regression-discontinuity variation within a hyperlocal geography, allowing us to compare borrowers on the opposite sides of a FICO threshold who live in an area smaller than a census tract. While our High Search Cost indicator may be correlated with other unobservables that affect take-up decisions (e.g., stronger brand preferences or a weaker understanding of personal finance), to the extent that there is spatial correlation in such unobservables, this specification allows us to hone our RD apparatus on more comparable borrowers. Again, column 1 of Table A4 shows that even within an eight-digit zip code, there is essentially zero contrast between LOT and ROT borrowers' take-up decisions, whereas within an eight-digit zip code in low search cost areas, there is a large and significant difference in take-up rates across a FICO threshold (column 2). The difference between high and low search-cost area take-up rates shown in column 3 is even larger than the effects estimated using CZ fixed effects in column 3 of Table 6.

An alternate approach, having shown in Section A.2.1 above that potentially endogenous changes to the branch network cannot explain our results, is to explicitly use temporal variation in the branch network in a difference-in-differences setting that looks at changes in take-up rates within a very narrow location (nine-digit zip code). In doing so, we set aside the discontinuity sample and instead focus on ascertaining the reaction of shopping behavior of locations g that become high search-cost areas after not being high search-cost areas previously. We first restrict our attention to only those areas g in our data that eventually transition from having 10 or more PFIs to fewer than 10. This results in a small sample of 608 observations from locations that we observe before and after becoming high search-cost areas (note that nine-digit zip codes can often be a single address). We estimate the effect γ of becoming a high search-cost areas in a panel setting as follows

$$takeup_{igt} = \eta_g + \delta_t + \gamma High\,Search\,Cost_{gt} + \beta FICO_{igt} + \varepsilon_{igt}, \tag{6}$$

where $HighSearchCost$ is a dummy for whether location g was a high search-cost location in quarter t. The virtue of this specification is that it absorbs fixed differences across extremely local areas (e.g., financial sophistication or credit-union loyalty) and thereby identifies the effect of search costs solely off of the *timing* of changes in search costs, which we argued in Appendix Section A.2.1 seem unrelated to unobservables driving variation in demand elasticities across space. Column 1 of Appendix Table A5 reports estimates of equation (6), showing that when borrowers face a reduction in nearby potential lenders, they are 11.0 percentage points more likely to accept a given loan offer.

To include locations which did not change search-cost status in the control group to identify the coefficients on the control variables, we also estimate a version of equation (6) in first-differences. For each location g in our sample that we observe more than once, we calculate the change in take-up rates $\Delta takeup_{gt}$, which we then regress on commuting-zone and quarter-pair fixed effects (a fixed effect for the pair of quarters in which location g was observed to calculate $\Delta takeup_{gt}$); the change in the location's average FICO score; and an indicator $\Delta High\, Search\, Cost_{gt}$ equal to one if location g was a high search-cost area in quarter t but not in the last period location g's take-up rate was observed before period t.

$$\Delta takeup_{gt} = \eta_{cz(g)} + \delta_{t,\Delta t} + \gamma \Delta High\, Search\, Cost_{gt} + \beta \Delta FICO_{gt} + \varepsilon_{gt}.$$

Again, the identifying assumption behind this specification is that the timing of decreases in the number of PFIs is unrelated to counterfactual trends in take-up rates, supported by the Bartik results above. Including commuting-zone fixed effects in the differences specification allows for each metropolitan area to change its propensity to shop for a loan differentially. The prediction of our search-cost explanation for high take-up rates in high search-cost areas for this setting is that cross-sectional *changes* in take-up rates, having differenced out time-invariant factors, will be positively related to changes in our search cost measure. Column 2 of Appendix Table A5 shows that the estimated $\hat{\gamma}$ is similar to the levels specification $\hat{\gamma}$, again positive and significant and consistent with the idea that when search costs go up, take-up rates increase as borrowers shop around less.

A.3 Digital Search

Many consumers now search for loans on the internet (including using such information aggregators as Bankrate.com), potentially limiting the relevance of lender density and driving distances as a proxy for 21st-century search costs. An anecdote from one of the larger lenders in our sample suggests that formal digital search (actually filing out an application online) is less common than might be expected: only 8% of their total applications are digital. While credit-union clientele skew slightly older than the general population, another potential explanation for the ability of physical search measures to explain variation in search propensity is that, although borrowers can be easily preapproved on the internet, the actual closing of loans (signing documents, transfer of title, etc.) still most frequently occurs at physical branch or dealer locations, even for direct loans. Ultimately, however, we view price dispersion results as lower bounds given the possibility of digital lending, perhaps making our results with respect to PFI and loan search even more noteworthy given trends in online shopping for credit products.

A.4 Competition and Measuring Search Costs

Proxying search costs with driving-time density may not uniquely measure borrower search costs. Driving-time density, as constructed, might also be a correlate of other local factors such as market concentration that determine the degree of price competition among lenders. In an effort to differentiate between search costs and a market concentration story, we construct empirical measures of lending competition within CZs. We calculate the share of originated mortgage loans by each HMDA lender within a given CZ and use the origination shares to construct a CZ-level Herfindahl index to capture the idea that two locations with identical branching networks could face differing degrees of competition based on the distribution of market shares across branches.[23] Dividing loans into high and low (above and below median) competition areas based on our constructed Herfindahl index, we reestimate the specification of Table 6 for all four combinations of high and low search-cost areas and concentration combinations.

The results of this exercise in Appendix Table A6 highlight that even within a competition category, there are statistically significant differences by search costs in the difference of take-up rates across FICO thresholds. Even for areas with a highly competitive banking sector, borrowers in high search-cost areas are much more likely to accept dominated loan terms. For low-competition CZs in low search-cost areas, the difference in take-up rates around

[23]Naturally, this is only many measures of banking competition; see Scharfstein and Sunderam (2016) and Dreschler, Savov, and Schnabl (2017) for recent alternatives used in the context of monetary policy pass-through.

lending thresholds is 12 percentage points (treated borrowers are 12 pp more likely to walk away from an expensive loan than control-group borrowers). In comparison, borrowers in high search-cost areas in the same competition bin are not likely to walk away when offered an expensive loan (statistically insignificant coefficient of 3.0 pp). For the high competition bin, we find similar results with lower search costs (higher search costs) resulting in a take-up differential of 11 pp (-2 pp). These results suggest that regardless of the overall level of market concentration (driven by unequal market shares), borrowers in areas we expect to have high search costs (because of the geography of the branch network) are much less sensitive to interest rates in their extensive-margin loan take-up decisions.

Table A1: Summary Statistics for Excluded Sample of Indirect Loans

				Percentile		
	Count	Mean	Std. Dev.	25th	50th	75th
	A. Originated Loans					
Loan Rate	1,166,822	0.05	0.03	0.03	0.04	0.06
Loan Term (months)	1,166,822	69.97	17.97	60	72	75
Loan Amount ($)	1,166,822	22,051.64	11,318.28	13,790	20,146	28,324
FICO	1,013,915	718.77	68.06	672	719	770
Debt-to-Income (%)	462,116	0.25	0.52	0	0.22	0.35
Collateral Value ($)	1,166,822	21,997.7	11,176.38	13,983	19,965	27,800
Monthly Payment ($)	1,166,822	360.87	161.71	246.8	334.25	445.04
	B. Ex-Post Loan Performance Measures					
Days Delinquent	799,144	39.16	645.64	0	0	0
Charged-off Indicator	1,166,822	0.03	0.16	0	0	0
Default Indicator	1,166,822	0.03	0.17	0	0	0
Current FICO	705,754	704.76	81.71	656	712	769
%ΔFICO	695,114	-0.02	0.08	-0.05	-0.01	0.02

Note: Table reports summary statistics for the indirect loan portion of the original dataset. This portion is excluded for the balance of the analysis of the paper. See notes to Table 1 for further details.

Table A2: Effects of FICO Discontinuity on Origination Interest Rates by Search Costs

Search Costs Sample	High	Low	Difference
Discontinuity Coefficient	-0.0151	-0.0146	-0.0006
	[-18.94]	[-17.54]	[-0.59]
Commuting Zone FE	✓	✓	
Quarter FE	✓	✓	
Number of Observations	45,351	228,678	

Notes: Table reports reduced-form RD estimates of first-stage equation (2), splitting the sample into high and low search-cost areas using the number of institutions within a 20-minute drive as a proxy. Robust t-statistics reported in brackets are clustered by normalized FICO score. See notes to Table 3 for more details.

Table A3: Effect of Bartik Search-Cost Proxies on Loan Offer Take-up Decisions

Bartik Search Costs Sample	High	Low	Difference
Discontinuity Coefficient	0.03	0.11	-0.08
	[0.68]	[4.52]	[-1.84]
Commuting Zone FE	✓	✓	
Quarter FE	✓	✓	
Number of Observations	4,102	15,678	

Notes: Table reports results for reduced-form RD regressions of loan take-up (conditional on being offered a loan) separately for borrowers in areas with low and high search costs in columns 2 and 3, respectively, using the Bartik instrument discussed in the text. Robust t-statistics reported in brackets are clustered by normalized FICO score. See notes to Table 3 for estimation details.

Table A4: Take-up Decisions Including Zip-8 Fixed Effects

Search Costs Sample	High	Low	Difference
Discontinuity Coefficient	-0.04	0.11	-0.15
	[-0.55]	[2.55]	[-1.80]
8-digit Zip Code FE	✓	✓	
Quarter FE	✓	✓	
Number of Observations	3,820	16,085	

Notes: Table reports results for reduced-form RD regressions of loan take-up (conditional on being offered a loan) separately for borrowers in areas with low and high search costs in columns 2 and 3, respectively, including eight-digit zip code fixed effects as in equation (5). Robust t-statistics reported in brackets are clustered by normalized FICO score. See notes to Table 3 for estimation details.

Table A5: Effects of Search Costs and Competition on Take-up Decisions

	(1) Take-up	(2) ΔTake-up
High Search-Cost Area	0.11	
	[2.97]	
ΔHigh Search-Cost Area		0.03
		[1.78]
FICO	-0.0004	
	[-1.48]	
ΔFICO		-0.0002
		[-5.99]
Geographic Fixed Effects	Zip-9	CZ
Time Fixed Effects	Quarter	Quarter Pair
Number of Observations	608	29,321
R-squared	0.60	0.05

Notes: Table reports difference-in-differences regression results relating local take-up rates to a given location's high search-cost status. Column 1 estimates this relationship in levels at the individual $\times$ quarter level, restricting the sample to only those locations that transition to being high search cost. Column 2 estimates this relationship in changes, specifying the dependent variable as the take-up rate at the location $\times$ quarter level, and includes all locations for which we observe originated loans in multiple quarters. "Quarter Pair" fixed effects are time fixed effects for each pair of quarters over which the differences in column 2 are calculated. ΔFICO is the change in the average FICO score of a given location between observations for that location. Robust t-statistics in brackets are clustered by quarter (column 1) and quarter pair (column 2).

Table A6: Effects of Search Costs and Market Concentration on Take-up Decisions

		Market Concentration	
		LOW	HIGH
Search Costs	LOW	0.12	0.11
		[3.49]	[3.38]
	HIGH	-0.03	-0.02
		[-0.24]	[-0.23]

Notes: Table reports results for reduced-form RD regressions of loan take-up for borrowers in each combination of areas with low and high search costs and high and competition, (see text for definition of high and low search costs). Search costs are estimated using the number of lending institutions within a 20-minute drive. Market concentration uses CZ-level lender mortgage market shares in HMDA data to construct an HHI measure of competition. All regressions include lending institution fixed effects and quarter fixed effects. Robust t-statistics reported in brackets are clustered by normalized FICO score. See notes to Table 3 for estimation details.