Price Regulation in Credit Markets: A Trade-off between Consumer Protection and Credit Access*

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Abstract. Interest rate caps are widespread in consumer credit markets, yet there is limited evidence on its effects on market outcomes and welfare. Conceptually, the effects of interest rate caps are ambiguous and depend on a trade-off between consumer protection from banks' market power and reductions in credit access. We exploit a policy in Chile that lowered interest rate caps by 20 percentage points to understand its impacts. Using comprehensive individual-level administrative data, we document that the policy decreased transacted interest rates by 9%, but also reduced the number of loans by 19%. To estimate the welfare effects of this policy, we develop and estimate a model of loan applications, pricing, and repayment of loans. Consumer surplus decreases by an equivalent of 3.5% of average income, with larger losses for risky borrowers. Survey evidence suggests these welfare effects may be driven by decreased consumption smoothing and increased financial distress. Interest rate caps provide greater consumer protection in more concentrated markets, but welfare effects are negative even under a monopoly. Risk-based regulation reduces the adverse effects of interest rate caps, but does not eliminate them.

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1 Introduction

Consumer credit penetration has increased steadily over recent decades and there is currently more that $41 trillion U.S dollars in household debt in the world, equivalent to around 40% of GDP across countries.[1] The growth of household debt has sparked a debate among researchers and policymakers about whether consumer credit is under- or over-supplied. The former argue that households are credit constrained due to market power or adverse selection, whereas the latter argue that moral hazard or behavioral biases induce households to borrow too much (Zinman, 2015).[2] This disagreement motivates a varied array of regulatory interventions that seek to increase or restrict credit access, and often coexist in the marketplace. While the regulation of consumer credit markets has been in the policy agenda for decades, its relevance increased substantially after the 2008 financial crisis (Campbell et al., 2011a,b).[3]

Interest rate regulation has historically been one of the main policy instruments in consumer credit markets (Temin and Voth, 2008). Several developed and developing countries implement some form of interest rate regulation nowadays, often adopting interest rate caps (Maimbo and Henríquez, 2014). On the one hand, regulators argue that interest rate caps limit lender usury and exercise of market power for loan pricing, as well as their ability to exploit consumers' behavioral biases. On the other hand, detractors argue that interest rate regulation makes risky borrowers unprofitable and therefore may limit credit access. Therefore, welfare implications of stronger interest rate regulation are potentially heterogeneous along borrower risk, as it benefits *protected* borrowers and harms *excluded* ones. Despite the ambiguity in its welfare effects, research analyzing this type of regulation is somewhat limited, at least partially due to a lack of comprehensive data and compelling research designs.

In this paper, we study the consequences of regulating consumer credit markets by studying the equilibrium effects of interest rate caps on prices, credit access, loan performance, and consumer welfare. We exploit the Chilean consumer credit market for consumer loans as a setting, which is attractive because it combines policy variation in interest rate regulation with extensive administrative data. Interest rate regulation in this setting takes the form of interest rate caps, which vary across loan size, and were substantially strengthened between 2013 and 2015 for part of the market. Throughout that period, interest rate caps decreased by between 17 p.p and 24 p.p for loans smaller than $8,000, leaving larger loans unaffected.[4]

For our analysis, we combine this policy variation with administrative data on the supply and demand for consumer credit. The data cover contracts, repayment behavior and credit histories for each consumer in the market, and loan applications for a large share of such contracts. Moreover, we complement this data with a survey that we designed and collected from a sample of borrowers in order to describe their

[1] Calculations based on the IMF Global Debt Database (Mbaye et al., 2018), for 82 developed and developing countries with available data for 2016. Beyond the average, most countries display increases in household debt as a share of GDP over time and there is substantial cross-sectional dispersion. See Figure A.1 for an illustration of this evolution for a sample of countries.

[2] Research providing evidence of households being credit constrained includes Gross and Souleles (2002), Adams et al. (2009), Jappelli and Pistaferri (2010) and Mian and Sufi (2011); whereas examples of research that suggests households might be over-borrowing are Bertrand and Morse (2011), Stango and Zinman (2014), Bhutta and Keys (2016) and Beshears et al. (2018), among others.

[3] For example, the U.S. government introduced the CARD Act in 2009 and then established the Consumer Finance Protection Bureau (CFPB) in 2010 to improve regulation and overall functioning of consumer credit markets, and European Commission has also taken steps in a similar direction (European Commission, 2015).

[4] Unless otherwise noted, all monetary units are measured in U.S. dollars of December 31st, 2016. For reference, the exchange rate at that point was of $667.29 Chilean pesos per U.S. dollar, according to the Central Bank of Chile.

shopping process and support the interpretation of welfare effects. We focus on unsecured consumer loans, a simple product that 15% of households hold (EFH, 2014). The average contract is roughly for a three-year loan of $6,800 with an interest rate of 23 p.p, and there is substantial dispersion in interest rates.

We start by providing evidence for the effects of interest rate regulation on the distribution of transacted interest rates. The policy change made the cap binding. At the onset of the policy change, in November 2013, as much as 31% of contracts affected by it were offered at interest rates higher than their interest rate cap by the time the policy was fully in place, in December 2015. The policy shifted the distribution of interest rates downwards, and induced substantial bunching at the interest rate cap. One interpretation of this pattern is that banks hold market power, since under perfect competition banks would choose not to offer loans that were exposed to this regulation at rates below the interest rate cap. However, this interpretation is not conclusive, as the pool of applicants might also change under stronger interest rate regulation.

To provide evidence of the market-level effects of interest rate regulation, we exploit the variation across loan size and time in the intensity of interest rate regulation in a differences-in-differences framework. We find that the policy change had large effects on prices, quantities, and loan performance. Average transacted interest rates decreased by 9% (2.6 p.p) in response to the policy change. The quantity of credit in the market also decreased, as the number of loan contracts went down by 19%. Part of this effect stems from a decrease in loan applications driven by riskier borrowers. Both price and quantity effects are stronger for riskier borrowers, who were more exposed to interest rate caps due to risk pricing by banks. In particular, transacted interest rates for risky borrowers decreased by 11% (3.3 p.p) and the number of loans for them decreased by 24%. Indeed, the borrower pool became safer and default rates decreased by 18% (1.15 p.p).

This evidence suggests that interest rates caps have strong effects on credit markets, when binding. The trade-off between credit access and consumer protection is apparent in these results. Our estimates imply that 151,027 loans for an amount of $361.6 million in loan contracts yearly were not signed due to stronger regulation. At the same time, average monthly payments decreased by $3.26 in the regulated segment of the market, adding up to an aggregate reduction of $31.7 million in present value per year.

Motivated by this evidence, we develop and estimate an equilibrium model of the market for consumer loans, with two objectives. First, we couple the model with the variation available in our setting to estimate borrowers' willingness to pay for loans and banks' costs. Having those inputs, we then develop a welfare analysis of interest rate regulation. Second, we use the model to study how the effects of interest rate regulation relate to the competitive environment, and the effects of counterfactual designs of this regulation.

Our model consists of three stages that cover application, pricing and repayment. First, consumers decide whether to apply for loans or not given their credit needs. Applications depend on approval probability, expected loan price, and application cost. Second, consumers shop across banks that offer homogeneous loans produced at heterogeneous costs. We model this process as an English auction, in which consumers shop across banks for the best contract offer. In equilibrium, the bank with the lowest cost signs the contract with the consumer at an interest rate that leaves the second-lowest cost bank with zero profits. The source of market power in our model is thus cost heterogeneity. This modeling choice has also been adopted in recent work on markets with bargained prices (Allen et al., 2014, 2019; Salz, 2020), and overcomes a common problem when working with contract-level data, which is that the econometrician only observes chosen

contracts rather than the full choice set that consumers face.[5] By modeling the market as an auction, we rationalize observed contract prices as a function of banks' latent cost structure. Third, repayment risk is realized. The model incorporates imperfect competition and adverse selection. The comparative statics of the model are in line with our evidence on market-level effects and support our interpretation of it.

We estimate our model using data on loan applications, approvals, prices and repayment. On the demand side, we estimate that consumers facing lower approval probabilities are less likely to apply for loans; and that riskier borrowers have a higher willingness to pay for credit and are less price-sensitive than safe borrowers. In terms of repayment, borrower risk score is the main correlate of repayment. Moreover, we find no compelling evidence of adverse selection along the extensive margin of loan applications, after conditioning on borrower risk scores. On the supply side, our cost estimates reveal substantial cost heterogeneity that stems from differences across banks, banks' incumbency advantages over previously related borrowers, and idiosyncratic bank-borrower cost heterogeneity. Moreover, cost estimates reveal substantial bank market power: the average mark-up over bank marginal cost is of 29%, of which market power accounts for 90% and borrower risk only accounts for 10%.

Adopting a revealed preferences approach, we use our model to estimate welfare effects of interest rate regulation. We find that expected consumer surplus decreased by an average and median of $82.47 and $ 40.34 per month respectively, equivalent to 3.5% and 1.7% of average income. However, not all consumers in the market lose consumer surplus under stronger regulation. Rather, 16.2% of consumers benefit from it, although the gains of this group are substantially smaller than the losses of those for whom expected consumer surplus decreases. Borrowers are heterogeneous in their exposure to interest rate regulation and willingness to pay, which generates heterogeneity in consumer welfare effects. In particular, risky borrowers experience average decreases in expected consumer surplus three times those of safe borrowers, because they are more exposed to interest rate regulation in the presence of risk pricing, and because they display both higher willingness to pay and lower price sensitivity. Moreover, profits per consumer decrease by $2.41 per month, which adds up to 18% of profits in the market, and implies that overall welfare decreases.

Evidence from our survey complements our estimates of welfare effects. In particular, we study how the implications of economic hardships for households vary depending on whether they are able to access bank credit to deal with those hardships. We show that households that deal with hardships with bank credit are less likely to decrease consumption and register unpaid bills or loan payments. These results are consistent with our estimates of negative consumer welfare effects of interest rate regulation as reflecting that reduced credit access limits consumption smoothing and increases the risk of financial distress for households.

An important motivation for interest rate regulation is to protect consumers from the exercise of market power by banks. We exploit our estimated model to study how the effects of interest rate regulation vary across markets with different degrees of concentration. We simulate equilibrium outcomes for a range of scenarios in which we sequentially merge banks, from the baseline market structure to the monopoly case. We find that adverse welfare effects are smaller in more concentrated markets, which suggests that the consumer protection role of interest rate regulation increases in less competitive markets. However, we find that stronger interest rate regulation decreases welfare even under a monopoly.

[5]This approach provides both a reasonable characterization of the market and is convenient for empirical work in our setting. It is often the case in markets with bargained prices that only transacted prices are observed, while prices of the other options in consumers' choice sets remain unobserved. Some papers overcome this challenge by predicting prices using observed transactions, but this may be problematic due to selection concerns (e.g., Crawford et al. 2018). Our approach avoids this prediction step.

The design of interest rate regulation is often strikingly simple. Few countries implement designs that go beyond having interest rate caps specific to a few loan size and type brackets.[6] The mismatch between unsophisticated regulation and sophisticated risk pricing by banks reinforces the trade-off between consumer protection and credit access by increasing the exposure of risky borrowers to interest rate caps. We use our estimated model to address the extent to which this mismatch exacerbates the potential for adverse effects. In particular, we study risk-based interest rate caps, which combine the benefits of risk-based pricing for dealing with borrower heterogeneity, with the potential of interest rate regulation for limiting the exercise of market power by banks. In a simple example, this design reduces the average welfare loss of interest rate regulation by 27%, without substantially increasing average loan prices or bank profit margins.

Overall, these results show that while interest rate regulation is meant to protect consumers facing high interest rates, it mostly harmed consumers' credit access and overall welfare in this setting. We highlight that theoretical predictions regarding credit access and welfare are ambiguous and thus interest rate regulation might improve outcomes in other settings. Regardless, our results inform the design of interest rate regulation for consumer credit by providing a conceptual framework for the relationship between the implications of this regulation and market characteristics, such as market structure and borrower heterogeneity.

This paper contributes to different branches of the literature. First, it contributes to a literature that studies the effects of interest rate regulation. The most recent research finds mostly negative effects on that margin when regulation is binding (Bodenhorn, 2007; Temin and Voth, 2008; Benmelech and Moskowitz, 2010; Zinman, 2010; Rigbi, 2013; Fekrazad, 2016; Melzer and Schroeder, 2017). However, many of these papers focus on payday loans in the U.S. In some cases, they focus on a single lender or a single market. Moreover, most of the previous work adopts reduced form approaches and focuses on credit access as their main outcome. Instead, we exploits administrative data from a full market as an empirical application, and develop a framework for the equilibrium analysis of interest rate regulation that allows for welfare and counterfactual analysis. Moreover, we emphasize the role of two pervasive attributes of credit markets, which are imperfect competition and borrower risk heterogeneity. Our empirical application is also studied by Hurtado (2015), SBIF (2017b) Schmukler et al. (2019) and Madeira (2019), all of which adopt reduced form approaches to analyze the effects of the policy change on credit access.

Second, this paper contributes to a recent literature on imperfect competition in selection markets. This literature emphasizes that the effects of policies on selection markets depend on the degree of competition (Veiga and Weyl, 2016; Mahoney and Weyl, 2017). We relate to this literature by empirically studying the relationship between the effects of interest rate regulation and market structure. Recent research develops empirical models that allow for adverse selection, imperfect competition, and product differentiation (Einav et al., 2012, 2013; Crawford et al., 2018; Kawai et al., 2018; Allen et al., 2019; Agarwal et al., 2020; Benetton, 2020). In our model, we allow for adverse selection along the extensive margin of consumer credit and embed imperfect competition in bank cost heterogeneity. We then use this model to study equilibrium effects of interest rate regulation on prices, quantities, loan performance and welfare.

Finally, this paper also contributes to other branches of the literature in household finance. First, we

[6]For example, several states in the U.S. have a single interest rate cap on consumer loans, and there is a federal interest rate cap at 36% for payday loans. In Europe, many countries have impose caps at a mark-up over the average interest rate, including Germany and Italy. Other countries, such as Belgium and France, have more sophisticated designs that allow the cap to vary by a few loan type and size brackets. In the case of the Chilean market, the design imposes differentiated interest rate caps for a small number of loan-size brackets. See Maimbo and Henríquez (2014) for further examples of interest rate regulation in credit markets across countries.

contribute to a recent literature that studies the effects of regulation on other margins of contract pricing in credit markets, also exploiting administrative data (Agarwal et al., 2015; Benetton, 2020; Nelson, 2020). We focus on a key aspect of contract design: interest rates. Second, we also contribute to a literature that focuses on the welfare implications of access to expensive credit, which finds mixed effects (Melzer 2011; Morse 2011; Bhutta et al. 2015; Gathergood et al. 2018; Skiba and Tobacman 2018, among others). While most papers in this literature focus on payday lending, we study a segment of the market in which interest rates are lower and risk composition is safer than what common in payday lending. For our setting, we measure the welfare effects of a common class of regulation that affects credit access.

The remainder of the paper is organized as follows. In Section 2, we describe the setting and data. In Section 3, we provide evidence for the market-level effects of interest rate regulation. In Section 4, we develop an equilibrium model of supply and demand of consumer loans, and in Section 5 we estimate it. In Section 6, we use the estimated model to measure welfare effects and in Section 7 we use it to study outcomes under counterfactual competitive environments and policy designs. Finally, Section 8 concludes.

2 The Chilean Credit Market

Our empirical application focuses on the Chilean market for unsecured consumer loans. Consumer loan contracts can be characterized by their interest rate, term and amount. Banks require no collateral on these loans.[7] Every year, more than 1.1 million contracts are signed, adding up to more than 7 billion U.S. dollars. While the consumer loan market is large, it is not the only source of consumer credit in this market. The two main alternative sources of consumer credit are credit cards and credit lines (SBIF, 2017b), both of which have increased its market penetration throughout the period we study.[8] These products are covered by the same interest rate regulation described in Section 2.1 below. Payday loans, a relevant source of expensive credit in other countries, are not widely available in Chile. Moreover, informal lending is a relatively small segment of the market, and only 7% of households hold some form of informal debt (EFH, 2018).[9]

The market is concentrated, as the combined market share of the top-3 and top-5 banks is 56% and 76%, respectively. We focus on the 15 banks that offer consumer loans in the market, which covers 92% of consumer loan contracts (SBIF, 2017b).[10] The remaining 8% of market share consists of credit unions that offer loans paid through employers, a somewhat different product that we do not consider in our analysis.

Regarding risk assessment by banks, there are no market-wide risk scores such as FICO scores in the U.S. Instead, there are three sources of information that banks may use for risk assessment: (i) comprehensive

[7] These contracts impose prepayment penalties. Borrowers may prepay part or all of the loan balance as long as the amount paid is higher than 25% of it. Upon prepayment, the borrower must pay a penalty of one month of interest on the prepaid balance.

[8] Figure A.2 displays the evolution of household debt in credit cards and credit lines. Both products increased their penetration through our sample period in terms of number of consumers and amount of debt, although without a noticeable pattern around the policy change. Moreover, average credit card and credit line balances across consumers—which according to industry sources combine both transactional and borrowing uses in similar shares—are less than a fourth of the average consumer loan in the market. This suggests these products are used more often to finance smaller expenses than consumer loans.

[9] This statistic covers several sources of informal credit, including family and friends, informal lenders, pawn shops, among others. Figure A.3 shows that the share of households with informal debt remained between 2% and 10% since 2007, with no noticeable pattern around the policy change. In fact, it remained almost constant between 2014 and 2017.

[10] For comparison, this market structure is more concentrated than that in the U.S, where the average number of banks in a local market is around 45 (Aguirregabiria et al., 2020), but similar to those in Canada and the U.K, where 8 and 6 banks respectively dominate most of the credit the market (see Allen et al. 2019 and Benetton 2020, respectively).

information on consumer covariates and credit history across all banks, that the regulator collects and provides to banks; (ii) information banks may collect directly from loan applicants; and (iii) risk scoring services provided by private firms. We have access to the first of these sources, which provides substantial information on borrower risk. This is emphasized by Foley et al. (2019) in their study of the role of information for bank lending using this same data. We use this data to estimate risk scores in Section 2.2.3.

Consumer debt is very common in Chile. The 2014 Survey of Household Finance (*Encuesta Financiera de Hogares*, EFH), describes the relevance of consumer loans for households around the policy change (EFH, 2014). As much as 63% of households have some form of consumer debt and 15.4% have consumer loans. Among households with consumer debt, the average debt to income ratio is around 5 and every month households allocate 20.5% of income to debt repayment (SBIF, 2017a).[11]

2.1 Interest Rate Regulation

Interest rate regulation in the Chilean credit market has been in place in different forms since 1929.[12] We focus on a policy change enacted by Law 20,715, which aimed at further protecting low-income borrowers and providing access to credit at lower interest rates (SBIF, 2017b). This law was approved on December, 13th, 2013 and followed long-standing Law 18,010, which was in place since 1981 and subsequently modified in 1999. These laws cover virtually all credit market operations with a term of 90 days or more. The main policy tool determined by these laws is a set of interest rates caps that vary depending on loan size. These caps are called Conventional Maximal Rate (TMC, *Tasa Máxima Convencional*). The policy change changed both the definition of loan size brackets for interest rate caps and the formulas for their calculation. Interest rate caps are measured in terms of annualized interest rates. Loan size brackets are defined in UF (*Unidades de Fomento*), an inflation adjusted monetary unit commonly used in Chile.[13]

Both before and after the policy change, interest rate caps can be summarized by a simple linear function of a lagged reference rate. The interest rate cap for loan-size bracket ℓ at period t is:

$$\bar{\iota}_{\ell t} = \psi_{\ell} \tilde{\iota}_{\ell t-1} + \alpha_{\ell t} \tag{1}$$

such that caps $\bar{\iota}_{\ell t}$ are set as a combination of proportional and constant mark-ups over a reference rate $\tilde{\iota}_{\ell t-1}$. Before the policy change, only two loan size brackets were considered by the regulation, namely \$0-\$8,000 and \$8,000-\$200,000. For both brackets, the regulation considered $\psi_{\ell} = 1.5$ and $\alpha_{\ell t} = 0$. The reference rate $\tilde{\iota}_{\ell t-1}$ was calculated as a weighted average of interest rates for loans of size ℓ during the previous month.[14] Figure 1 displays the evolution of interest rate caps and shows that before the reform, interest rate caps were beyond 50 p.p and 25 p.p for loans in the \$0-\$8,000 and \$8,000-\$200,000 size brackets respectively.

The reform we study made four changes to the previous regulation. First, it split the \$0-\$8,000 size

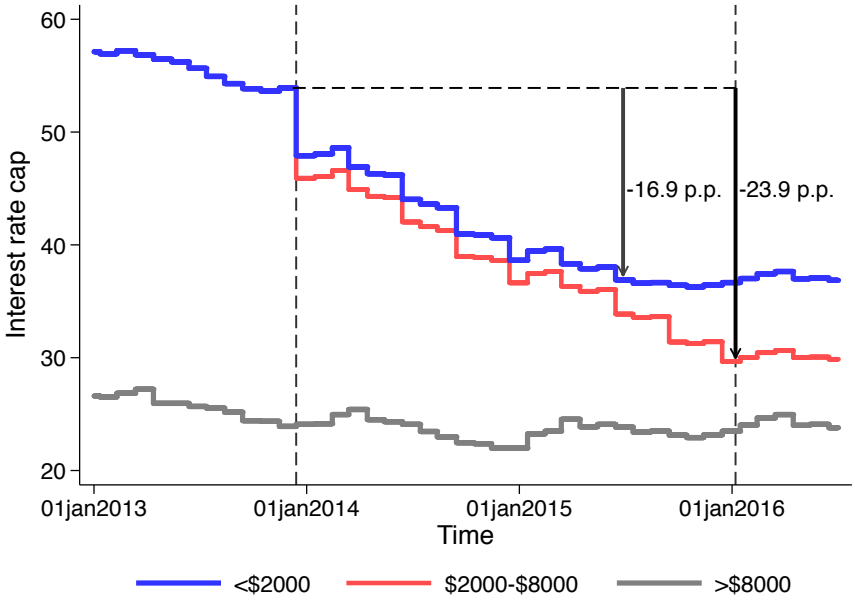
[11] In terms of utilization of loans, the share of households having consumer loans for different self-reported objectives varies as follows: 54% for household durables, 30% for clothing, 22% for debt consolidation, 11% for vehicles, 9% for medical treatment, 9% for home improvement and 5% for vacations (EFH, 2014).

[12] For more detail on the history of interest rate regulation in Chile, see Hurtado (2015), and SBIF (2017b).

[13] According to the Central Bank of Chile, one UF was equivalent to 39.48 U.S. dollars on December 31st, 2016. Relevant policy thresholds are set at 50UF and 200UF. For reference, 50UF is equivalent to \$1,970 and 200UF is equivalent to \$7,880. We refer to these two thresholds as \$2,000 and \$8,000 respectively for expositional simplicity. All analyses are conducted without such approximation.

[14] Throughout the paper, we ignore banks' potential incentives to adjust interest rates to affect the reference rate $\tilde{\iota}_{\ell t-1}$.

Figure 1: Evolution of interest rate caps



Notes: These figure displays the evolution of the level of interest rate caps for different loan size brackets. The first dashed black line indicates the implementation of Law 20,715, after which interest rate caps for all loans under $8,000 were reduced, in December 2013. The second dashed line indicates the date in which the policy was fully implemented, in December 2015.

bracket into two, namely \$0-\$2,000 and \$2,000-\$8,000.[15] Second, it set $\psi_{0-8000} = 1$ while $\psi_{>8000} = 1.5$ remained unchanged. Third, it set constant mark-ups over the reference rate of $\alpha_{0-2000,t} = 21$ p.p and $\alpha_{2000-8000,t} = 14$ p.p. Fourth, the reference interest rate was set to be a weighted average of interest rates in the \$8,000-\$200,000 bracket for all size brackets. Therefore, only regulation for loans under \$8,000 was directly affected by the policy change. Moreover, the main qualitative effect of the policy was to move from a regulation based on proportional mark-ups to one based on constant mark-ups for those two size brackets.

Had the policy been fully enacted by December 2013, interest rate caps would have fallen at once by 16.9 p.p and 23.9 p.p for loans in the \$0-\$2,000 and \$2,000-\$8,000 brackets respectively (SBIF, 2017b). Instead, the policy was staggered to avoid such sharp decrease. This transition was structured by an immediate fall of 6 p.p and 8 p.p respectively followed by quarterly decreases of 2 p.p for $\alpha_{\ell t}$. Under such schedule, the policy was fully in place by December 2015. Figure 1 displays the evolution of interest rate caps around the reform. The reduction in caps for the \$0-\$2,000 and \$2,000-\$8,000 size brackets is stark, and the difference between them is of 7 p.p. However, the cap on larger loans remained roughly constant over the period of study. We exploit these features as identifying variation to study the effects of this regulation below.

[15]This aspect relates to considerations of risky borrowers being potentially excluded from the credit market by this regulation. Exclusion was indeed part of the discussion around the policy approval by the Chilean Congress. Allowing for a less strict regulation for the smaller loan size bracket aimed at reducing such concern.

2.2 Data

We use administrative data collected by the market regulator, the Financial Markets Commission (*Comisión para el Mercado Financiero*, CMF).[16] The data cover the period between January 2013 and December 2015, which includes the roll-out of the policy change. Our population of interest is that of potential borrowers. We define this population as all consumers with some relationship with the consumer credit market, defined as having used any bank product, from checking accounts to mortgages. This set covered 2.5 million consumers in January 2013, as much as 25% of the working-age population in the country. We observe demographics, income and credit history for each potential borrower. We exploit two main administrative datasets: one that contains every loan contract signed and one that provides a large sample of loan applications.

We complement administrative data with a household survey that we designed and administered. We exploit this data to provide complementary evidence for our model assumptions and to aid the interpretation of the estimates of consumer welfare effects we obtain from our model. In particular, we collect data from 1,003 consumers who applied for loans at least twice between 2013 and 2015, and were rejected by at least one bank in that period. The objective of this sampling strategy was to target a population of risky borrowers that were likely to be affected by the policy change we study and that were familiar with the market. The survey collects information about financial literacy, familiarity with credit market, search and application behavior in the credit market, and the evolution of household finance over the period of interest.

2.2.1 Loan Contracts Dataset

The first dataset is a registry of all consumer loans in the market during our sample period.[17] This data have several features. First, borrower and bank identifiers are available for each contract, along with key contract attributes including interest rate, amount, and term. Second, the data track loan the performance, which allows us to observe loan defaults and their timing. Third, the data provide borrower attributes including age, gender, income, and county of residence. Fourth, the data collect the full credit history of each borrower in the system, including amount of consumer and mortgage debt held and amount of debt in 90-day default. Importantly, this is the same data that the regulator provides to banks for borrower risk assessment, and cover the relationships between each borrower and all banks in the market. In the absence of market-wide risk scores in the market, we exploit this information to construct risk scores for our analysis in Section 2.2.3. The fact that banks employ this same information when assessing borrower risk reinforces our approach. We measure all monetary variables in U.S. dollars and all interest rates are annualized.

2.2.2 Applications Dataset

The second dataset covers a large sample of consumer loan applications for the period of study. While the loan contracts dataset covers the entire market, the coverage of the applications dataset is only partial.[18] We

[16] In June 2019, CMF absorbed the regulator of banks during the period of study, which was the Superintendence of Banks and Financial Institutions (*Superintendencia de Bancos e Instituciones Financieras*, SBIF).

[17] We focus on loan originations. While renegotiation is available in the market, very few borrowers renegotiate their contracts. The share of renegotiations is 6% and does not change across our sample period.

[18] Banks' reporting practices for this dataset were not as rigorous as those for the contracts dataset, as this was a new requirement for them. In particular, three banks did not report this data to CMF.

link the applications and contracts datasets using borrower identifiers, and we are able to match application events for 64.5% of loan contracts in the data. Given we observe all loan contracts in the contracts dataset, the implication of this partial coverage is that we are unable to observe some rejected applications. For each application in the dataset, we observe the identity of the bank and the borrower, the application date, the loan size and term for which the borrower applies, and the outcome of the application. Whenever the application is approved by the bank, we also observe the interest rate.

We organize this data by constructing application events, and develop all our analysis using this definition of applications. We construct application clusters of a given borrower across—potentially—multiple banks in a short period of time. Concretely, we define an application event as a set of applications by a borrower such that no pair of applications are more than 30 days apart. We then merge these application events with loan contracts using borrower and bank identifiers.

2.2.3 Measuring Credit Default Risk

We exploit the availability of data on loan performance, consumer covariates, and credit history to estimate credit default risk. In particular, we estimate a logit model of default using data for the period before the policy. The model we estimate uses an indicator for loan default over the term of a loan as dependent variable, and a rich vector of borrower covariates x_i determined before signing the contract as independent variables. This is a standard risk scoring model (Ohlson, 1980). The set of features in x_i is similar to that employed by Liberman et al. (2019) for estimating borrower risk for the Chilean consumer credit market.

Table A.1 displays estimates of different specifications of this model.[19] The results point in the expected directions: borrowers with higher income and lower leverage default less frequently. Regarding credit history, borrowers with more consumer debt, without previous consumer loans, and with more consumer debt under default are more likely to default; while borrowers with more mortgage debt and whose mortgage debt is not in default are less likely to default. In terms of demographics, both older and female borrowers are less likely to default. The model predicts 69% of loan defaults correctly out of sample. We construct risk scores as the fitted probabilities from this model, such that borrowers with higher risk scores are riskier. For the rest of the paper, we refer to the income risk model as that in column (1) of Table A.1 and to the history risk model as that in column (5) of Table A.1. Our risk scores display positive correlations with rejections, interest rates and realized default.[20]

2.3 Descriptive Statistics

The contracts dataset contains more than 3.3 million loans for 2013-2015. Table 1 displays summary statistics for it. Average annualized interest rates are around 23 p.p, but more than 10% of the loans have rates higher than 35 p.p, which is partly what motivated the implementation of the regulation we study.[21] The average

[19] For the rest of the paper, we use results from a model that splits all covariates in twenty bins and includes dummies for such bins as regressors. This more flexible model accommodates potential non-linearities in the relationship between default and covariates.

[20] Figure A.4 displays relationships between market outcomes and our measures of predicted risk. Figures A.4-a and A.4-b display negative relationship between predicted risk and approvals, while Figures A.4-c and A.4-d display positive relationships between interest rates and predicted risk. Finally, Figures A.4-e and A.4-f display positive relationships between realized and predicted default.

[21] Most of the price dispersion is cross-sectional. While there is variation in the funding cost of banks through time, only 1.2% of the variation in interest rates can be explained by monthly dummies. See Figure A.5 for the evolution of bank funding cost through our

loan in the sample is about $6,700 and 33 months long, and has a monthly payment of $266, with substantial variation in these attributes.[22] Regarding the distribution of loan size across size brackets defined by the regulation in place, the share of loan contracts in the year before the policy change was 30.8%, 41.5% and 27.7% respectively for loans in the $0-$2,000, $2,000-$8,000 and $8,000-$200,000 brackets. In terms of loan performance, 5% of borrowers default on payments during the first year of the loan and 11% through the loan term. The average predicted default risk is 0.11, and most of the borrowers are under 0.2.

There is substantial heterogeneity among borrowers. The average borrower has an annual income of $18,685 and is almost 44 years old. Moreover, 40% of borrowers are female. Most of loan contracts in our data are signed by consumers that had previously dealt with banks in the consumer credit market, and 76% of them are signed with a bank that the borrower has previously used for banking. In terms of credit history, the average consumer holds $7,022 in consumer loans and $12,447 in mortgage debt. The median borrower in the contracts dataset takes out only one consumer loan throughout our sample period, although there is a group of borrowers that take several loans and the average borrower takes 1.8 loans. Finally, borrowers in the system hold relationships with multiple banks, and the average borrower is a customer of three banks.

Our applications dataset collects almost 3.7 million application events, and every month we observe 2% of potential borrowers in the market applying for a loan. Both loan amount and term are slightly larger on average in the applications dataset than those in the contracts dataset. In terms of outcomes, as much as 90% of application events end with an approval, whereas 10% of application events end with a rejection.

Additionally, there is substantial heterogeneity in market structure across local markets. We define local markets geographically as the 54 provinces in the country to provide a description of the competitive environment. The average market has 8 banks and 43 branches, although there is wide dispersion in both across markets. Most markets are dominated by a few banks. In particular, in the average market the top three banks hold 66% of market share in terms of loan contracts, and the top five banks hold 83% of it.

2.4 Descriptive Facts about the Chilean Consumer Credit Market

In this section, we present descriptive facts of the Chilean consumer credit market. We focus on the relationship between relevant outcomes and behaviors and two important borrower attributes, namely borrower risk score and previous relationships with banks. We focus on the period before the policy change for this descriptive analysis. We exploit these facts to interpret our findings for the effects of interest rate regulation in Section 3, and to motivate the model we develop in Section 4.

First, we focus on the correlates of loan application behavior. Column (1) in Table 2 displays results from a regression of an indicator for application on individual risk score and county-month fixed effects. The results show that the likelihood of application increases with borrower risk score, suggesting that observably riskier borrowers are more likely to select into the market. Additionally, they also show that potential applicants with previous experience in the market are also more likely to apply for loans.

Second, we study the drivers of banks' approval decisions. Column (2) in Table 2 displays results from

period of study. On the other hand, there is substantial heterogeneity across banks in interest rates: bank and month dummies jointly explain as much as 25.4% of the variation.

[22] Monthly payments are calculated using the formula $p = \frac{L\iota(1+\iota)^T}{(1+\iota)^T-1}$, where L is loan amount, ι is the interest rate, and T is loan term.

Table 1: Summary statistics

Variable	N	Mean	SD	p10	p50	p90
A - Loan attributes						
Interest rate	3,362,384	23.17	10.16	10.99	21.24	38.40
Amount	3,362,384	6,705.25	7,007.14	952.14	4,350.17	16,241.59
Term	3,362,384	33.02	16.19	12.17	36.17	50.87
Monthly payment	3,362,384	266.37	323.77	65.95	189.52	522.21
B - Loan performance						
Default during loan first year	3,362,384	0.05	0.21	0.00	0.00	0.00
Default during loan term	3,362,384	0.11	0.31	0.00	0.00	1.00
Amount of charge-off	3,362,384	291.71	1,793.79	0.00	0.00	0.00
Predicted default probability - Income	3,362,384	0.11	0.06	0.04	0.11	0.18
Predicted default probability - History	3,358,842	0.11	0.10	0.02	0.09	0.24
C - Borrower attributes						
Annual income	3,362,384	18,684.65	17,059.19	5,639.61	13,081.43	37,215.05
Age	3,358,842	43.80	13.30	28.00	42.00	63.00
Female	3,362,384	0.40	0.49	0.00	0.00	1.00
Consumer debt	3,362,384	7,021.96	10,514.91	70.72	3,149.40	18,285.16
Consumer debt to income ratio	3,362,384	4.58	5.29	0.07	2.92	10.97
Consumer debt under default	3,362,384	41.00	592.30	0.00	0.00	0.00
Mortgage debt	3,362,384	12,447.09	31,309.96	0.00	0.00	48,179.93
Mortgage debt to income ratio	3,362,384	5.87	13.59	0.00	0.00	24.20
Mortgage debt under default	3,362,384	11.67	664.78	0.00	0.00	0.00
Previously related to bank	3,362,384	0.76	0.43	0.00	1.00	1.00
Previously related to any bank	3,362,384	0.94	0.24	1.00	1.00	1.00
D - Borrowers through the dataset						
Number of loans	1,909,393	1.76	1.22	1.00	1.00	3.00
Amount in loans	1,909,393	11,807.75	14,451.15	1,518.89	6,878.77	28,224.96
Number of banks with loan contracts	1,909,393	1.21	0.48	1.00	1.00	2.00
Previously related banks	1,909,393	3.04	1.55	1.00	3.00	5.00
E - Application events						
Loan amount	3,014,213	7,099.37	7,252.63	1,036.93	4,827.14	16,960.44
Loan term	2,706,289	34.52	15.60	12.63	36.50	53.87
Approved application	3,014,322	0.83	0.38	0.00	1.00	1.00
Rejected application	3,014,322	0.17	0.38	0.00	0.00	1.00
F - Local Market Structure						
Number of banks	1,944	8.00	4.03	2.00	8.00	13.00
Number of branches	1,944	43.11	133.06	1.00	19.00	58.00
Top-1 market share	1,944	0.31	0.13	0.22	0.27	0.46
Top-3 market share	1,944	0.66	0.13	0.52	0.62	0.84
Top-5 market share	1,944	0.83	0.09	0.72	0.81	0.96
HHI	1,944	1,959.38	945.64	1,327.35	1,643.30	2,877.59

Notes: This table displays summary statistics for our datasets. All monetary variables are expressed in U.S. dollars for June 2016. Credit history variables are computed as average over the year previous to each loan.

regressions of an indicator for application approval on borrower covariates. Estimates from this regression show that banks are less likely to approve applications from borrowers with higher predicted default risk, which is also documented by Figure A.4-a. Moreover, they also show that banks are more likely to approve applicants with which they hold a previous relationship.

Third, we show that previous relationships affect bank choice. Figure A.15-a shows there is substantial variation in the number of bank-borrower previous relationships, and few contracts are signed by borrowers new to the system. Figure A.15-b shows that the likelihood of signing a loan contract with a previously related bank is high, and that it increases with the number of previous relationships and decreases with borrower risk. This pattern may relate to the fact that applications from previously related borrowers are approved more often, perhaps because relationships make applications less costly for borrowers and banks.

Additionally, we study the determinants of interest rates. Banks engage in risk pricing and offer higher loan prices to observably riskier borrowers. Column (3) in Table 2 displays results of regressions of interest rate margins over banks' funding cost on borrower and contract covariates. Interest rates are increasing in borrower default risk, as also documented by Figure A.4-b. Additionally, even after conditioning on contract attributes, borrower default risk, and other covariates, we find that previous relationships affect prices: borrowers with a previous relationship with a bank receive lower loan prices on average.

Moreover, there is substantial price dispersion. As much as 26% of the variation in interest rate margins remains unexplained after accounting for interacted month, bank, location, loan size, term and borrower risk fixed effects, as displayed in Figure A.16. The observation of substantial price dispersion even within narrow segments of the market is consistent with evidence from U.S. credit markets (Woodward and Hall, 2012; Stango and Zinman, 2016). The standard deviation of residualized interest rate margins remains high at 3.9 p.p, around a third of its unconditional standard deviation.[23] One potential source of price dispersion within observably similar contracts is discretion of banks' loan officers and bargaining over prices.

Finally, we show that riskier borrowers are more likely to default. Column (4) in Table 2 shows the results from a regression of an indicator of loan default on borrower and contract covariates. Estimates from this regression show that observably riskier borrowers are more likely to default on loan payments, conditional on contract amount and term.

3 The Effects of Interest Rate Regulation

In this section, we study the effects of interest rate regulation on market outcomes. As described in Section 2.1, this policy strongly decreased interest rate caps on loans, differentially so across loan size. Using different approaches, we provide evidence for price, quantity, and risk composition effects. Throughout this section, we emphasize heterogeneity across borrower risk. In particular, we split the sample according to the median predicted default risk before the reform and estimate effects for low- and high-risk borrowers.

3.1 Evidence from the Evolution of Interest Rates

The policy change we study reduced interest rate caps between December 2013 and December 2015 for loans smaller than $8,000. As a first piece of evidence, we visually inspect the evolution of interest rates. The left column of Figure 2 displays the evolution of the distribution of interest rates for loans of $0-$2,000,

[23] Evidence from our survey complements this fact by showing consumers are aware of this price dispersion. Figure A.17-a shows that consumers in the market perceive substantial price dispersion conditional on loan terms. In particular, the average perceived range of monthly payments in the market in our survey is 26%.

Table 2: Borrower risk, behavior and outcomes

	(1)	(2)	(3)	(4)
	1(Application)	1(Approval)	log(Interest rate)	1(Default)
Risk score	0.0003***	-0.011***	0.037***	0.035***
	(0.0000)	(0.000)	(0.000)	(0.000)
log(Loan size)		0.050***	-0.361***	-0.007***
		(0.000)	(0.000)	(0.000)
log(Loan term)		-0.098***	0.173***	0.071***
		(0.000))	(0.001)	(0.000)
Related to bank		0.099***	-0.008***	-0.042***
		(0.000)	(0.001)	(0.000)
Related to any bank	0.0144***	-0.042***	0.019***	0.022***
	(0.0001)	(0.001)	(0.001)	(0.001)
Mean of dep. var.	0.02	0.82	19.92	0.08
County-Month FE	Y	N	N	N
Bank-County-Month FE	N	Y	Y	Y
Observations	10,696,213	845,046	611,273	611,275
R-squared	0.002	0.138	0.609	0.080
Sample	Population	Applications	Contracts	Contracts

Notes: This table displays regressions of relevant behaviors and outcomes on borrower risk scores, contract covariates, previous relationships and fixed effects, for the period between January 2013 and November 2013, before the policy change. Column (1) displays results from a regression of an indicator for loan application on loan and borrower covariates. The sample includes a random sample of 10% of potential borrowers in the market. Column (2) does so for an indicator for approval conditional on application, for a sample of all applications in the market. Column (3) does so using interest rates as outcomes, for a sample of all loan contracts for which we observe applications and are approved. Finally, column (4) does so using an indicator for loan default as an outcome, using the same sample an in column (3). Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

$2,000-$8,000 and $8,000-$20,000, along with the evolution of the interest rate cap for each of those groups, for the period around the policy change. There are two relevant aspects to these figures. First, interest rate caps were mostly not binding within the treated size brackets.[24] Second, interest rate caps became increasingly binding for these groups after December 2013. On the other hand, the extent to which interest rates for loans larger than $8,000 were binding did not change noticeably over the period of study.

To further document the price effects of interest rates caps, we compare the distribution of interest rates before and after the policy change. The right column of Figure 2 shows the distribution of interest rates for the month before the policy change with that for the same month exactly two years after, when the policy was fully in place.[25] The policy displaced a substantial share of the density downwards for treated loan-size brackets, inducing bunching of interest rates at the interest rate caps. As much as 42% and 23% of loans of $0-$2,000 and of $2,000-$8,000 were exposed to the policy, respectively. In contrast, only 8% of loans of $8,000-$20,000 were exposed to it, and only marginally so. One interpretation for this response is as suggestive evidence of imperfect competition in this market. Had there been perfect competition, banks would have not offered exposed loans after the policy was in place, as those loans would be unprofitable.

[24]Previous research has shown that interest rate caps may play the role of focal points for collusion in credit markets (Knittel and Stango, 2003). The fact that caps for loans smaller than $8,000 were not binding before the policy change suggests this regulation was not playing such role in this setting, at least for that period.

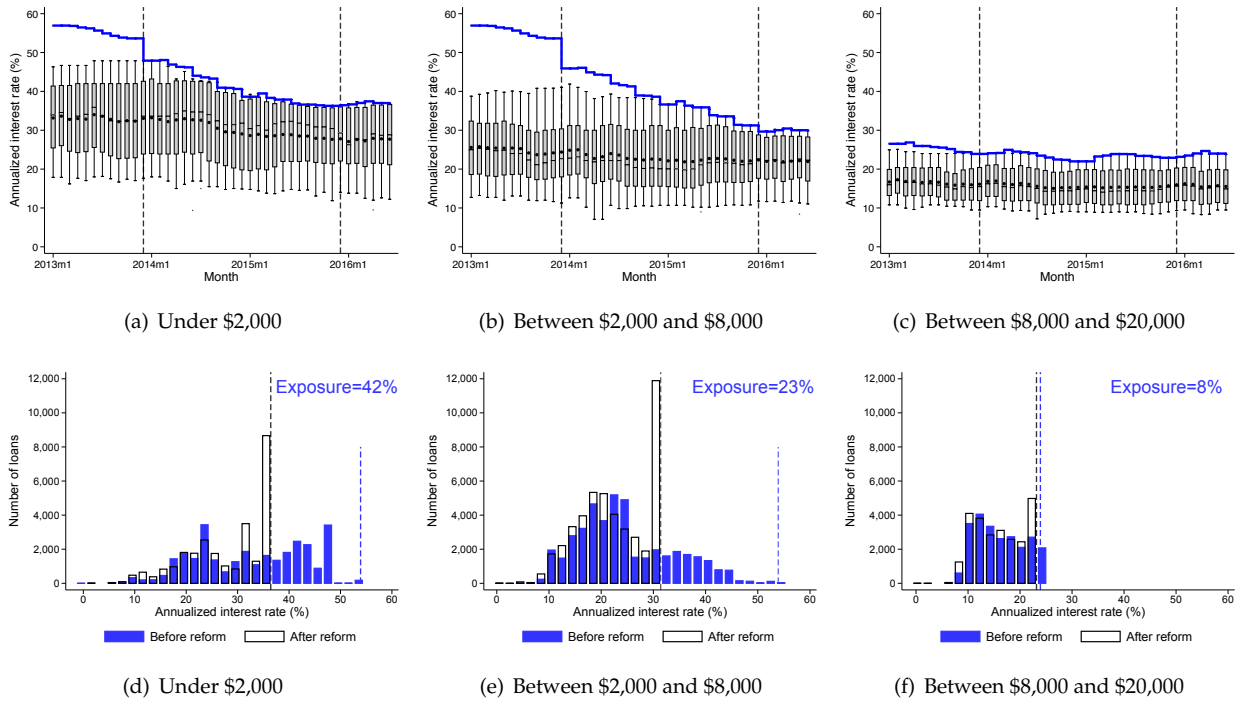
[25]The period we utilize for this exercise covers the second half of November and the first half of December of both 2013 and 2015. The pattern we find remains the same when focusing on longer time periods.

However, this interpretation is not conclusive, as the pool of applicants might have also changed between the two periods we analyze in response to the policy change. Overall, these patterns suggest that banks had market power, which allowed them to charge interest rates above expected costs.

Exposure also varies across borrower risk. As much as 49% and 31% of high-risk borrowers signing loan contracts for loans of \$0-\$2,000 and \$2,000-\$8,000 were exposed to the policy, compared to only 26% and 11% for low-risk borrowers. These patterns suggest that exposure to interest rate regulation was increasing in borrower risk, which is as expected in the presence of risk pricing. [26]

This evidence suggests that as interest rate caps were strengthened, the distribution of interest rates responded by bunching below the interest rate cap, and that riskier borrowers were more affected by the policy. This is not surprising and implies that the regulation was enforced. The magnitude of the effects is large and we aim at understanding its implications for other outcomes in the remainder of the paper.

Figure 2: Evolution of the distribution of interest rates



(a) Under \$2,000 (b) Between \$2,000 and \$8,000 (c) Between \$8,000 and \$20,000

(d) Under \$2,000 (e) Between \$2,000 and \$8,000 (f) Between \$8,000 and \$20,000

Notes: Panels (a), (b) and (c) display the evolution of the distribution of interest rates by loan size. Each box displays the 25th, 50th and 75th percentiles of such distribution. Spikes display the 5th and 95th percentiles. Black dots indicate the mean of it. In each plot, the blue line displays the current interest rate cap relevant for the loan size interval. Panels (d), (e) and (f) display frequency histograms of interest rates for December 2013 (blue) and December 2015 (white). The blue dashed line indicates the level of the interest rate cap for each size bracket before the reform was implemented, while the black dashed line does so for the month when the reform was fully in place. Exposure to the policy is calculated as the share of loans that were signed before the policy was implemented at interest rates higher than the interest rate cap once the policy was fully in place.

[26] See Figure A.6 for exposure rates by borrower risk within each policy size bracket.

3.2 Effects on Market Outcomes

The policy change provides two useful sources of variation to estimate the effects of interest rate regulation. First, it provides variation across time. Before December 2013, regulation was not binding for loans in \$0-\$8,000, but it became increasingly binding as the reform was phased in. Second, it provides variation across loan size. Regulation became more binding for loans of \$0-\$2,000 than for loans of \$2,000-\$8,000, and for loans of \$2,000-\$8,000 than for those of \$8,000-\$20,000, which remained essentially untreated. We exploit these two sources of variation. For our analysis, we aggregate the data to measure market-level effects. In particular, we construct bins for loan size and term indexed by k, and aggregate the data at that level.[27]

3.2.1 Evolution of Policy Effects

We start by studying the evolution of outcomes of interest around the policy change. We estimate differences-in-differences models that decompose effects though time. The goal is to provide graphical evidence for the effects of the policy change, while also addressing concerns related to trends in the outcomes leading to the policy change that could be correlated with the policy itself. We start by estimating the equation:

$$y_{krt} = \sum_{\tau} D_k \beta_{r\tau} + \alpha_{kr} + \delta_{rt} + \varepsilon_{krt} \tag{2}$$

where y_{krt} is the outcome of interest for product bin k and risk group r in month t; D_k indicates whether loans in k are smaller than \$8,000 and thus affected by the policy change; α_{kr} are fixed effects that control for unobservable shocks specific to a loan size, term and risk group, but are constant through time; and δ_{rt} are fixed effects that control for unobservable shocks specific to a month and risk group but are constant across loan size and term. The coefficients of interest are $\beta_{r\tau}$, which measure the difference in the outcome of interest between loans affected by the policy change and the comparison group for borrowers of risk r, τ months after the policy change.[28]

Figure 3 displays results from equation (2) for low- and high-risk borrowers. Figure 3-a shows that the average interest rate in the market decreased after the policy change. Figure 3-b shows that the effect is concentrated on the upper part of the distribution of interest rates, as the effect on the 90th percentile of interest rates is stronger and becomes apparent earlier after the policy change than that on the average. Moreover, Figure 3-c shows that the number of loan applications by high-risk borrowers decreases, whereas Figure 3-d shows that the number of loans in the market also decreased, and substantially more so than applications. Figure 3-e shows that the average risk score in the market decreased, such that the borrower pool became safer. Finally, Figure 3-f shows that a measure of expected mark-up also decreases after the policy change, although less than interest rates, given the decrease in default risk.[29]

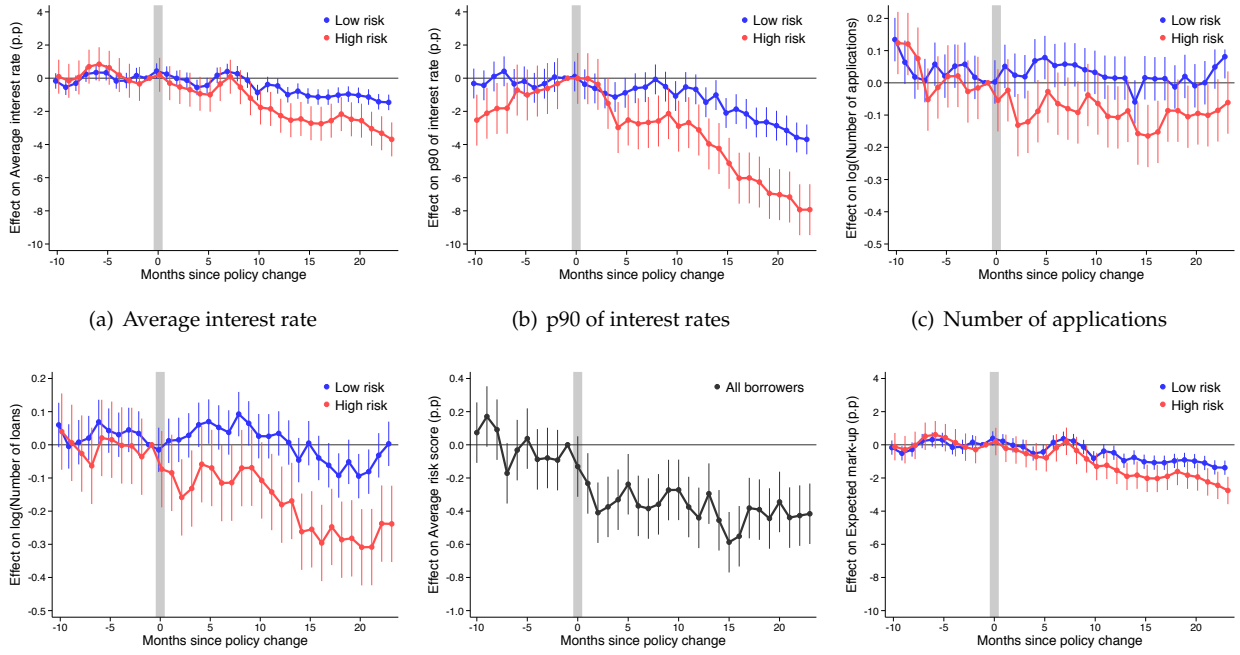
[27] Concretely, we define loan size bins in intervals of 50 UF (\$2,000) and employ a clustering algorithm to classify loan term in 8 bins, which adds up to 80 loan-type bins, indexed by k. We then compute averages or aggregate levels of the outcomes of interest for each bin and month. To study heterogeneous effects, we implement the same procedure but separately for low- and high-risk borrowers.

[28] We control for seasonal patterns specific to loan size for quantity outcomes by removing month-of-the-year fixed effects from the time series of each product type bin k, before estimating equation (2).

[29] We compute expected mark-up as $m_{krt} = \frac{1}{N_{krt}} \sum_{i \in \mathcal{I}_{krt}} [\iota_i(1 - d_i) - f_t]$, where m_{krt} is the average mark-up for loans in bin k for borrowers of risk r; ι_i is the interest rate charged to borrower i; d_i is the predicted default probability of such borrower; f_t is the funding rate faced by banks; and $\mathcal{I}_{krt}$ is the set of borrowers of risk r taking loans k in month t. This is a proxy of average mark-up in the market that does not account for other components of cost than risk and funding. We develop more comprehensive measures of mark-ups

These results share two patterns across outcomes. First, estimates display flat trends leading to the policy change and a steady decrease after it, which suggests that loans of \$8,000-\$20,000 evolve similarly to loans that were directly treated, reinforcing the extent to which the former serves as a comparison group for the latter. We further exploit that in a more extensive regression analysis below. Second, estimated effects are larger for high-risk borrowers than for low-risk borrowers, which suggests that the former were more affected, consistent with their higher exposure discussed above. These results readily suggest that both prices and quantities decreased under stronger interest rate regulation, which is consistent with the policy having effects both in terms of consumer protection and credit access.

Figure 3: Differences-in-differences effects through time



(a) Average interest rate (b) p90 of interest rates (c) Number of applications

(d) Number of loans (e) Average risk score (f) Average expected mark-up

Notes: Each figure displays results from estimating equation (2) for a different outcome. Within each plot, dots indicate estimated effects for a given month while dashed lines indicate standard errors. Effects for low- (high-) risk borrowers are displayed in blue (red). All regressions are weighted by the number of loans in the product-risk bin before the policy was implemented.

3.2.2 Regression Analysis

In this section, we exploit more granular variation in interest rate caps to estimate its effects on market outcomes. We define the following treatment intensity variable to exploit time variation in regulation within each size bracket, and to ease the interpretation of the results:

$$\Delta^{\bar{\iota}}_{\ell,t} \equiv (\bar{\iota}_{\ell,0} - \bar{\iota}_{\ell,t}) - (\bar{\iota}_{>8000,0} - \bar{\iota}_{>8000,t}) \tag{3}$$

using our model in the second part of the paper.

for each of the treated size brackets $\ell \in \{\$0\text{-}\$2{,}000, \$2{,}000\text{-}\$8{,}000\}$. The first term in equation (3) is the change in the interest rate cap for loan-size bracket l between current month t and baseline month $t = 0$ at December 2013. The second term in equation (3) is the change in the interest rate cap for the comparison group, i.e. loans larger than \$8,000. Subtracting the second term removes variation in economic conditions that influences interest rate caps, and thus isolates the policy variation that we exploit. Figure A.7 displays the evolution of these treatment intensity variables.

Using these variables, we estimate the following specification:

$$y_{krt} = \sum_{\ell} \beta_{\ell(k),r} \Delta^{\bar{\imath}}_{\ell(k),t} + \delta_{kr} + \phi_{krm(t)} + \gamma_{rt} + \varepsilon_{krt} \tag{4}$$

where y_{krt} is the outcome of interest for product bin k for borrower of risk r for month t; δ_{kr} is a set of fixed effects that controls for unobservable shocks specific to a loan size and term and borrower risk bin, but constant through time; $\phi_{krm(t)}$ is a set of fixed effects that controls for unobservable shocks specific to a product type, borrower risk bin and month-of-the-year $m(t)$; and similarly γ_{rt} is a set of fixed effects that controls for unobservable shocks specific to a borrower risk bin and month but constant across loan size and term. The coefficients of interest are $\beta_{0-2000,r}$ and $\beta_{2000-8000,r}$. Given how the treatment variable $\Delta^{\bar{\imath}}_{\ell,t}$ is constructed, these coefficients measure the effect of *reducing* interest caps by 1 p.p. on the outcome of interest for each policy size bracket respectively. We then compute full effects by scaling up these estimates by the full change in interest caps. All regressions are weighted by the number of loans in each product bin before the policy was implemented. Finally, standard errors are clustered at the product bin level to allow for potential correlation in errors within bins across time.

We study three sets of outcomes. First, we study effects on interest rates, focusing on maximum and average rates. Second, we focus on quantity, including number of applications, number of loans and credit volume. Third, we focus on risk selection, loan performance and expected profitability by estimating effects on borrower risk scores and on income, on 90-day loan default in the first year, and on expected mark-ups. In each case, we estimate regressions across all borrowers and separately for low- and high-risk borrowers.

Effects on Interest Rates. Stronger regulation reduced interest rates, consistent with evidence in Section 3.1. Table 3 displays estimates of equation (4) for maximum and average interest rates. We find pass-through of interest rate caps to maximum interest rates was high. Effects from a 1 p.p decrease in interest rate caps range from 0.96 p.p for low-risk borrowers to 1 p.p for high-risk borrowers for loans of \$0-\$2,000; and from 0.66 p.p for low-risk borrowers to 0.8 p.p for high-risk borrowers for loans of \$2,000-\$8,000. Full effects are large and close to the total change in the interest rate cap, particularly for riskier borrowers. These results verify that the policy was enforced, and that it was more binding for smaller loans and riskier borrowers.

Average interest rates decreased as a result of stronger regulation, as displayed in Table 3-B. We estimate that reducing interest rate caps by 1 p.p decreases average interest rates by 0.23 p.p and 0.07 for loans of \$0-\$2,000 and \$2,000-\$8,000, respectively. These effects are heterogeneous across borrower risk. The effects on low-risk borrowers are smaller at 0.13 p.p and 0.03 p.p, while those on high-risk borrowers are much larger at 0.26 p.p and 0.11 p.p respectively. The full effects on average interest rates were 3.8 p.p and 1.7 p.p for loans of \$0-\$2,000 and \$2,000-\$8,000.[30]

[30] These estimates measure effects on the average interest rate, regardless of whether loans were exposed to the policy. The effect on

Table 3: Effects on interest rates

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Maximum interest rate			Panel B: Average interest rate		
	All	Low-risk	High-risk	All	Low-risk	High-risk
Loans in \$0-\$2000						
Marginal effect (β)	-1.001***	-0.961***	-0.996***	-0.231***	-0.126***	-0.262***
	(0.010)	(0.024)	(0.012)	(0.032)	(0.021)	(0.041)
Full effect ($\beta \times \Delta\bar{\imath}$)	-16.369***	-15.720***	-16.298***	-3.771***	-2.060***	-4.286***
	(0.157)	(0.390)	(0.200)	(0.526)	(0.345)	(0.666)
Baseline mean	55.145	54.675	55.384	33.023	28.630	35.256
Loans in \$2000-\$8000						
Marginal effect (β)	-0.785***	-0.660***	-0.803***	-0.073***	-0.033**	-0.107***
	(0.030)	(0.042)	(0.032)	(0.020)	(0.013)	(0.025)
Full effect ($\beta \times \Delta\bar{\imath}$)	-18.307***	-15.405***	-18.744***	-1.714***	-0.776**	-2.496***
	(0.701)	(0.972)	(0.745)	(0.478)	(0.295)	(0.590)
Baseline mean	50.401	48.920	51.634	24.912	21.426	27.813
Observations	2,880	2,880	2,829	2,880	2,880	2,829
R-squared	0.986	0.976	0.984	0.984	0.985	0.975
Product bin FE	Y	Y	Y	Y	Y	Y
Product bin-month of year FE	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y

Notes: This table displays results from estimating equation (4). For each outcome, the regression is estimated across borrower risk bins and separately by borrower risk bin. All regressions include risk bin-product bin fixed effects and risk bin-month fixed effects. Marginal effects measure the effect of reducing interest rate caps by 1 p.p. Full effects are calculated as the product of the marginal effect of the policy and the magnitude of the policy change once fully implemented for each policy loan-size bracket. All regressions are weighted by the number of loans in the product bin-risk bin before the policy was implemented. Clustered standard errors at the product bin-risk bin level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Effects on Quantity Outcomes. Interest rate regulation may affect borrower application behavior. On the one hand, it weakly reduces interest rates upon approval and thus induces marginal borrowers to take loans. On the other hand, banks may be less willing to approve applications if they are constrained in terms of pricing, which may deter borrower applications if applying is costly. The latter should be more relevant for observably riskier borrowers. Table 4-A displays estimates of equation (4) for the number of applications. We find no statistically significant effects on average, nor for low-risk borrowers. However, we find suggestive evidence that risky borrowers apply less often for loans under stronger regulation. In particular, a 1 p.p decrease in interest rate caps reduced applications by 1% and 0.4% for loans of \$0-\$2,000 and \$2,000-\$8,000, although the latter is not statistically significant. These estimates imply that the full policy decreased applications by risky borrowers by 15% and 9% for loans in each size bracket.

How did stronger interest rate regulation affect equilibrium quantities? Tables 4-B and 4-C display estimates of equation (4) for number of loans and credit volume. We find that reducing interest rate caps by 1 p.p reduced the number of loans by 2% and 0.5% respectively for loans of \$0-\$2,000 and \$2,000-

loans not exposed to the policy should be close to zero, implying that effects on exposed loans should be larger. In absence of quantity effects, we would expect a perfect pass-through of changes in interest rate caps to the average interest rate of exposed loans. In that case, the ratio between our estimates and shares of exposed loans per group in Figure A.6 would equal one. However, such ratio is 0.55 p.p and 0.32 p.p for loans of \$0-\$2,000 and \$2,000-\$8,000, which readily suggests the policy had quantity effects.

$8,000. Again, we find substantial heterogeneity across borrower risk. For low-risk borrowers, we estimate decreases of 0.8% and 0.2% for loans of $0-$2,000 and $2,000-$8,000; whereas for high-risk borrowers, estimates are almost three times larger, at 2.5% and 0.7% respectively. Results are quantitatively similar for credit volume. Full effects of the policy change are large. The number of loans decreased by 27.6% and 11.9% for loans of $0-$2,000 and $2,000-$8,000. Effects are particularly large for high-risk borrowers, at 33.9% and 15.8% respectively. The fact that the effects on the number of loans and credit volume are much larger than those on applications implies that a large share of the quantity reduction comes from rejections.

Effects on Risk Selection, Loan Performance and Profitability. How do changes in applications and approvals affect the borrower pool? Table 5-A displays results from estimating equation (4) for ex-ante borrower risk measures. The policy change improved the borrower risk pool. A reduction of 1 p.p in the interest rate cap decreases average borrower predicted default rate by between 0.07 p.p and 0.04 p.p for loans of $0-$2,000 and by around 0.02 for loans of $2,000-$8,000, depending on the measure of predicted risk. The full policy decreased borrower predicted default risk by between 1.14 p.p and 0.7 p.p for loans of $0-$2,000, and by between 0.49 and 0.35 p.p for loans of $2,000-$8,000. Relatedly, we find that average borrower income increases with stronger regulation.

We now turn to estimate effects on loan performance. Effects on interest rates and screening could affect loan performance. On the one hand, lower interest rates may increase loan repayment by reducing moral hazard (Holmstrom and Tirole, 1997; Adams et al., 2009). On the other hand, a better borrower pool—due to stronger risk selection—may also lead to improvements in loan performance. Results in Table 5-B show that loan performance did in fact improve as a result of the policy. Reducing interest rate caps by 1 p.p decreased the share of loans under 90-day default in their first year by 0.09 p.p and 0.04 p.p respectively for loans of $0-$2,000 and $2,000-$8,000. This effect is higher among high-risk borrowers than among low-risk borrowers. The full policy was able to reduce the average share of loans under 90-day default in their first year by 1.52 p.p and 0.88 p.p, equivalent to 22.5% and 14.6% of their baseline levels.

Finally, we study effects on banks' expected mark-ups of signed contracts. These estimates combine effects on interest rates with effects on the composition of the borrower risk pool. We find that expected mark-ups decreased under stronger interest rate regulation, as displayed in Table 5-C. These effects are smaller than those on interest rates, which is driven by the fact the composition of the borrower pool is safer and, therefore, banks' expected costs decrease on average. Overall, these results suggest that interest rate regulation indeed constrains banks' exercise of market power.

3.2.3 Robustness Exercises

The analysis we develop exploits policy variation across loan size and time to estimate the effects of interest rate regulation on market outcomes. The main concern regarding our empirical strategy is that the policy change affected relative regulation across loan size, which might induce substitution across loan size brackets. In principle, given regulation becomes stronger for loans of $0-$8,000, we might expect consumers to substitute towards that group, which would imply our quantity effects are attenuated. However, baseline variation in interest rate caps limits such incentives, as the interest rate for loans larger than of $8,000 is lower than that for loans of $0-$8,000 throughout the period of study, as shown in Figure 1. On the other

Table 4: Effects on quantity outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Panel A: log(Applications)			Panel B: log(Number of loans)			Panel C: log(Credit volume)		
	All	Low-risk	High-risk	All	Low-risk	High-risk	All	Low-risk	High-risk
Loans in $0-$2000									
Marginal effect (β)	-0.003	-0.002	-0.010**	-0.020***	-0.008***	-0.025***	-0.018***	-0.008***	-0.022***
	(0.004)	(0.003)	(0.005)	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.004)
Full effect ($\beta \times \Delta\bar{\imath}$)	-0.053	-0.030	-0.164**	-0.323***	-0.129***	-0.414***	-0.291***	-0.135***	-0.362***
	(0.058)	(0.046)	(0.082)	(0.053)	(0.044)	(0.058)	(0.060)	(0.048)	(0.065)
Baseline mean	20,401.500	5,594.100	14,807.400	28,792.909	9,701.909	19,091.000	35,108.197	12,374.705	22,733.492
Loans in $2000-$8000									
Marginal effect (β)	0.000	-0.001	-0.004	-0.005*	-0.002	-0.007**	-0.006*	-0.003	-0.007*
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Full effect ($\beta \times \Delta\bar{\imath}$)	0.004	-0.017	-0.094	-0.127*	-0.046	-0.171**	-0.133*	-0.066	-0.159*
	(0.071)	(0.068)	(0.099)	(0.073)	(0.061)	(0.082)	(0.073)	(0.063)	(0.080)
Baseline mean	33,312.200	13,582.300	19,729.900	38,822.636	17,633.000	21,189.636	170,025.54	81,855.398	88,170.140
Observations	2,800	2,800	2,713	2,880	2,880	2,880	2,880	2,880	2,880
R-squared	0.994	0.989	0.993	0.990	0.988	0.989	0.985	0.986	0.979
Product bin FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Product bin-month of year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y

Notes: This table displays results from estimating equation (4). For each outcome, the regression is estimated across borrower risk bins and separately by borrower risk bin. All regressions include risk bin-product bin fixed effects and risk bin-month fixed effects. Marginal effects measure the effect of reducing interest rate caps by 1 p.p. Full effects are calculated as the product of the marginal effect of the policy and the magnitude of the policy change once fully implemented for each policy loan-size bracket. Baseline mean for credit volume is reported in thousands. All regressions are weighted by the number of loans in the product bin-risk bin before the policy was implemented. Clustered standard errors at the product bin-risk bin level are parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Effects on risk selection, loan performance and profitability

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Panel A: Risk selection			Panel B: Loan performance			Panel C: Profitability		
	Risk-I	Risk-H	log(Income)	Share of loans under 1Y default			Average expected mark-up		
	All	All	All	All	Low-risk	High-risk	All	Low-risk	High-risk
Loans in $0-$2000									
Marginal effect (β)	-0.067***	-0.043***	0.004***	-0.093***	-0.040***	-0.057**	-0.161***	-0.116***	-0.187***
	(0.003)	(0.007)	(0.001)	(0.018)	(0.010)	(0.027)	(0.027)	(0.020)	(0.032)
Full effect ($\beta \times \Delta\bar{\imath}$)	-1.136***	-0.700***	0.071***	-1.519***	-0.652***	-0.938**	-2.608***	-1.899***	-3.064***
	(0.052)	(0.115)	(0.014)	(0.287)	(0.161)	(0.450)	(0.447)	(0.320)	(0.530)
Baseline mean	6.925	6.616	13.013	6.747	3.353	8.472	22.491	20.773	23.364
Loans in $2000-$8000									
Marginal effect (β)	-0.021***	-0.015***	0.000	-0.038***	-0.004	-0.031*	-0.052***	-0.032***	-0.078***
	(0.004)	(0.005)	(0.001)	(0.011)	(0.006)	(0.018)	(0.016)	(0.012)	(0.020)
Full effect ($\beta \times \Delta\bar{\imath}$)	-0.486***	-0.348***	0.006	-0.879***	-0.101	-0.732*	-1.193***	-0.742***	-1.827***
	(0.101)	(0.113)	(0.014)	(0.259)	(0.143)	(0.411)	(0.380)	(0.271)	(0.471)
Baseline mean	5.782	6.011	16.641	6.026	2.873	8.649	16.013	14.285	17.452
Observations	2,880	2,880	2,880	2,880	2,880	2,829	2,880	2,880	2,829
R-squared	0.982	0.969	0.989	0.915	0.832	0.766	0.983	0.984	0.975
Product bin FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Product bin-month of year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y

Notes: This table displays results from estimating equation (4). For each outcome, the regression is estimated across borrower risk bins and separately by borrower risk bin. All regressions include risk bin-product bin fixed effects and risk bin-month fixed effects. Marginal effects measure the effect of reducing interest rate caps by 1 p.p. Full effects are calculated as the product of the marginal effect of the policy and the magnitude of the policy change once fully implemented for each policy loan-size bracket. Predicted risk is computed as described in Section 2.2.3 and measured in a 0-100 scale. Income is measured in thousands of U.S. dollars. Share of loans under default in first year is computed in a 0-100 scale. See footnote 29 for details on the construction of average expected mark-ups. All regressions are weighted by the number of loans in the product bin-risk bin before the policy was implemented. Clustered standard errors at the product bin-risk bin level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

hand, banks may attempt to offer multiple loans of smaller sizes below $8,000 rather than a single one of size larger than $8,000 in order to charge higher interest rates. However, we find no evidence of such behavior in the data, as more than 98% of borrowers take only one loan in months in which they borrow, and that share remains unchanged throughout the period of study, as displayed by Figure A.8.

We develop several robustness checks to assess the assumptions underlying this strategy. We summarize the main results from these exercises here, and leave an extended discussion for Appendix A. We showed in Section 3.2.1 that trends leading to the policy change are similar across groups. In a similar vein, we study whether placebo policies shifted along loan-size space relative to the actual policy change could generate effects similar to our estimates. Evidence on effects of placebo policies would be indicative of substitution concerns. Appendix A.1 shows that effects from such placebo policies are generally smaller and close to zero. Second, we study whether the distribution of loan size and term changes around the policy change for loans larger than $8,000, and Appendix A.2 shows we find no evidence of such pattern. Third, we study whether the distribution of loan application amount changes around the policy thresholds, which could reflect substitution across size brackets by borrowers. Appendix A.3 shows there are no such patterns. Fourth, we study whether alternative definitions of the comparison group affect our results, and show in Appendix A.4 the latter remain similar across a range of comparison groups. Overall, this evidence suggests that our main results are robust to concerns about substitution across loan size brackets. Finally, we study heterogeneity in estimated effects across banks to verify whether our results are driven by any particular bank. In Appendix A.5, we show that effects for most banks display the same patterns of our results.

3.3 Discussion

In this section, we provided evidence for equilibrium effects of interest rate regulation on market outcomes. Average interest rates decreased and the number of loan contracts decreased substantially as a result of the policy change. Effects are particularly large for risky borrowers, who were more exposed to the policy change, as they were charged higher interest rates before the policy change due to risk pricing. These results are consistent with recent research that also finds quantity effects from this regulation such as Benmelech and Moskowitz (2010). Additionally, we find improvements in the borrower pool risk and loan performance, which is in contrast to Rigbi (2013), who finds no effect on loan performance.

Overall, our estimates imply that 151,027 loan contracts per year were deterred by stronger regulation, equivalent to 19% of the number of loans signed during the year before the policy change and $361.6 million in consumer loans.[31] Our estimates of price effects imply that interest rates decrease on average by 9%, which translates into an average decrease in monthly payments across loans of $3.26. The present value of reduced monthly payments during the year before the policy change is $31.7 million.[32] These results provide a picture of the magnitude of aggregate effects, but do not allow to assess welfare effects.

While this analysis is informative of the effect of interest rate regulation on equilibrium outcomes,

[31] This aggregate effect is obtained by calculating the share of the credit volume originated during the year before the policy change that would be deterred by the policy for each treated policy size bracket according to estimates across risk bins in columns (1) and (4) of Table 4. We report the total across both policy loan-size brackets.

[32] This amount is calculated by computing counterfactual monthly payments using an interest rate adjusted downwards by average price effects in column (4) of Table 3. Then, we compute the difference between those monthly payments and actual monthly payments. We compute the present value of that difference using a discount rate of 5% and the term of each loan contract. Finally, we aggregate across loan contracts actually signed during the year before the policy change was implemented.

several questions remain unanswered. First, the welfare implications of the combination of policy effects we estimate are unclear. In order to develop a welfare analysis, we require knowledge about consumers' willingness to pay and banks' costs. Second, as we emphasized at the beginning of the paper, market power may have a role in determining the effects. However, it is hard to assess this argument using observational data given the endogeneity of market structure. Third, we also emphasized that interest rate regulation is remarkably unsophisticated in most markets. Nevertheless, this analysis does not allow us to draw conclusions on how alternative designs would affect market outcomes. We develop and estimate an equilibrium model for the consumer credit market in the next sections to address these aspects.

4 An Equilibrium Model of Applications, Pricing and Repayment

We develop an equilibrium model of applications, pricing and repayment in the consumer credit market, which we then estimate. Several modeling choices aim at moving from a theoretical model to an empirical one that can be estimated using the data available for our setting. We discuss these choices in Section 4.2.

4.1 Model

4.1.1 Setup

There are N consumers, denoted by i. There are J banks, denoted by $j \in \mathcal{J}$, where $\mathcal{J}$ is the set of banks in the market. The model is static. Consumers choose whether to apply for loans of a given amount and term (L_i, T_i), determined in a previous stage that we do not model. For a given (L_i, T_i), contracts are homogeneous and only differ by their monthly payments, which vary across banks due to cost heterogeneity. Therefore, consumers shop across banks for the lowest monthly payment. The price and bank signing a contract with a consumer are determined as the outcome of an English auction, as in Allen et al. (2019). Figure A.18 summarizes the structure and timing of the model.

Borrowers. Consumers are endowed with observable characteristics x_i and unobservable characteristics ε_i, such that (x_i, ε_i) summarizes the consumer type. The vector x_i collects all public information, including risk scores, borrower income, and borrower credit history, whereas $\varepsilon_i = (\varepsilon_{Ai}, \varepsilon_{Si})$ are potentially correlated application and repayment shocks that follow a joint distribution F_ε and are borrower private information.

Borrowers decide whether to shop for loans. If they shop for loans, they incur an application cost $\kappa(z_i)$ that depends on cost shifters z_i, draw a choice set of banks $\mathcal{J}_i$ and shop across them. If they do not to shop for loans, they obtain their outside option. Let the indirect utility from a contract and the outside option be:

$$\begin{aligned} u_{Ci} &= v_C(x_i, L_i, T_i) - p_i \\ u_{Oi} &= v_{Oi} \end{aligned}$$

where $v_C(x_i, L_i, T_i)$ is the indirect utility of a contract, which depends on borrower and loan attributes; p_i is the monthly payment offered to borrower i; and v_{Oi} is the indirect utility of the outside option. Borrowers

choose to apply for loans by comparing the expected value of both options, given by:

$$\begin{aligned} u_{Ai} &= P_{Ci} \underbrace{\int u_{Ci} f_{p|C}(p) dp}_{\text{Value of approval}} + (1 - P_{Ci}) \underbrace{u_{Oi}}_{\text{Value of rejection}} - \kappa(z_i) + \varepsilon_{Ai} \\ u_{NAi} &= u_{Oi} \end{aligned}$$

where P_{Ci} is the probability that the application is approved by some bank in the market, and where the borrower integrates the value of a loan contract over the density of loan prices they face conditional on approval, we denote by $f_{p|C}(p)$. Both P_{Ci} and $f_{p|C}(p)$ are equilibrium objects which borrowers know. Finally, ε_{Ai} is a shock to the utility that borrowers obtain from applying for a loan relative to not applying.

Given this structure, a borrower decides to apply for a loan whenever its expected utility is higher than that of remaining out of the credit market. The application probability is:

$$P_{Ai} = \Pr\left(P_{Ci} \int (u_{Ci} - u_{Oi}) f_{p|C}(p) dp - \kappa(z_i) + \varepsilon_{Ai} \geq 0 \right) \tag{5}$$

from where it is clear that application decisions are driven by the approval probability, the expected gains from a loan contract relative to the outside option, the density of loan prices conditional on approval, an application cost that borrowers face, and a shock to the utility of application. Let a_i indicate that borrower i applies for a loan and define $\mathcal{A}$ as the set of loan applicants. We set the utility of the outside option to $u_{Oi} = 0$ for the remainder of the paper, such that u_{Ci} is the utility of a loan contract relative to that outside option.

Conditional on applying for loans, the borrower solves a discrete choice problem to choose which bank to sign a loan contract with, which implies that utility from a loan contract is:

$$u_{Ci} = \max_{j \in \mathcal{J}_i} \quad v_C(x_i, L_i, T_i) - p_{ij} \iff p_i = \min_{j \in \mathcal{J}_i} p_{ij}$$

such that bank choice is driven by monthly payment, given there is no further product differentiation across banks in the choice set. As discussed below, all differentiation is concentrated in banks' costs.

Loan Repayment. After signing a loan contract, repayment is realized. Let $s_i \in [0, 1]$ be the share of payments made by borrower i relative to the total number of monthly payments in the contract:

$$s_i = s(x_i, L_i, T_i, \varepsilon_{Si}) \tag{6}$$

which is a function of borrower characteristics and non-price contract terms. Moreover, repayment is increasing in the repayment shock ε_{Si}. There is adverse (advantageous) selection if application and repayment display a negative (positive) correlation through unobservables to banks $(\varepsilon_{Ai}, \varepsilon_{Si})$.

Banks. We model competition among banks to attract borrowers as an English auction. Banks are heterogeneous in the cost of serving borrowers. There are three components of cost: (i) funding cost f_i; (ii) bank-borrower match-value ω_{ij}, which is an i.i.d. shock from a distribution G_ω that is unobserved to borrowers and may make it less costly for a bank to serve some borrowers than others; and (iii) repayment risk.

We combine the first two components in $m_{ij} = f_i - \omega_{ij}$. In terms of repayment risk, banks observe x_i and application choices a_i, which they employ to estimate repayment risk when pricing contracts.

Bank profits depend on a risky stream of payments and a stream of monthly bank costs. Let $\varphi(T_i) \equiv \frac{1}{r}(1 - \exp(-rT_i))$ be a present value operator that discounts a stream of payments for T_i months at a discount rate r; and $\varphi(S_i) \equiv \frac{1}{r}(1 - \exp(-rS_i))$ be a present value operator that discounts a stream of payments for $S_i = s_i T_i$ months, where S_i is repayment length by borrower i. The expected profit from a loan at price p_{ij} is:

$$E_\varepsilon[\pi_{ij}] = E_\varepsilon[\varphi(S_i)]p_{ij} - \varphi(T_i)(f_i - \omega_{ij})$$

where repayment risk and funding cost depend only on borrower-specific attributes, while match-value ω_{ij} depends on bank-borrower attributes. Therefore, ω_{ij} introduces cost heterogeneity across banks that can be thought of as the match-value of a contract. For instance, ω_{ij} could capture bank-borrower relationships and bank convenience in local markets, among other features. Conditional on x_i, banks with higher ω_{ij} face a lower cost of signing a loan contract with borrower i and can therefore offer such contract at a lower price.

A bank offers a contract if $E_\varepsilon[\pi_{ij}] \geq 0$, and otherwise rejects the borrower. Expected profits are decreasing in borrower repayment risk at a given price, and thus observably riskier applicants are less likely to be approved. Borrowers' application choices and banks' approval decisions are related. Given banks observe x_i and know F_ε, they make inference about borrower unobservable repayment shock ε_{Si} from application choices. Banks incorporate that information in their approval and pricing choices.[33]

Regulation. Interest rate regulation takes the form of an interest rate cap, which induces caps on monthly payments. In particular, banks are not allowed to charge monthly payments higher than $\bar{p}_i$.

4.1.2 Equilibrium

Equilibrium in this model is characterized by the pool of applicants, and loan approvals and prices. In absence of interest rate regulation, the outcome of an English auction in this setting is that the lowest cost bank wins the auction with a bid $b_{i(1)}$ such that the second lowest cost bank is indifferent between getting the loan contract or not at that price.[34] The solution to:

$$E_\varepsilon[\pi_{i(2)}] = E_\varepsilon[\varphi(S_i)]b_{i(1)} - \varphi(T_i)m_{i(2)} = 0$$

is thus the equilibrium unconstrained price:

$$p_i^u = \frac{\varphi(T_i)}{E_\varepsilon[\varphi(S_i)]}(f_i - \omega_{i(2)}) \tag{7}$$

[33] In particular, banks compute $E_\varepsilon[\pi_{ij}] = E_\varepsilon[\pi_{ij}|a_i = 1, x_i]$. This implies that, conditional on x_i, application choices reveal information about ε_{Ai}. Given banks know F_ε, a signal about ε_{Ai} is informative about repayment risk ε_{Si}.

[34] As usual in auction models, the notation $x_{(m)}$ indicates the mth order statistic of x.

which is increasing in repayment risk and funding cost, and decreasing in match-value of the closest competitor.[35] This price yields equilibrium expected profits $E_\varepsilon[\pi_{i(1)}] = \varphi(T_i)(m_{i(2)} - m_{i(1)}) = \varphi(T_i)(\omega_{i(1)} - \omega_{i(2)})$, from where it becomes clear that the source of banks' market power in this model is given by cost advantages.

Under interest rate regulation, there are three potential outcomes for an applicant. If not binding, then the bank offers the contract at the unconstrained price in equation (7). If regulation is binding, however, the unconstrained price is higher than the price cap, $\bar{p} < p_i^u$. In this case, the lowest cost bank offers the contract at price $p_i = \bar{p}$ as long as $E_\varepsilon[\pi_{i(1)}] = \bar{p} - \frac{\varphi(T_i)}{E_\varepsilon[\varphi(S_i)]} m_{i(1)} \geq 0$. Finally, if the cost of the lowest cost bank is high enough as to make lending at the cap unprofitable, $E_\varepsilon[\pi_{i(1)}] = \bar{p} - \frac{\varphi(T_i)}{E_\varepsilon[\varphi(S_i)]} m_{i(1)} < 0$, all banks reject the borrower. Therefore, equilibrium prices under interest rate regulation are:

$$p_i^* = \begin{cases} p_i^u & \text{if } p_i^u \leq \bar{p} \\ \bar{p} & \text{if } \frac{\varphi(T_i)}{E_\varepsilon[\varphi(S_i)]} m_{i(1)} \leq \bar{p} < p_i^u \\ . & \text{if } \bar{p} < \frac{\varphi(T_i)}{E_\varepsilon[\varphi(S_i)]} m_{i(1)} \end{cases} \tag{9}$$

The distribution of equilibrium prices determines application decisions by borrowers, which in turn determines the equilibrium set of applicants, $\mathcal{A}^*$. In this equilibrium, (i) borrowers optimally make application choices given both the application approval probability and the distribution of prices they face in the market, and their application costs, while (ii) banks optimally make price offers in a competitive environment given both their costs and the pool of loan applicants.

4.1.3 Effects of Interest Rate Regulation

Application Behavior. What are the implications on the demand side? Stronger regulation affects borrower application behavior by (i) reducing the approval probability, and by (ii) weakly reducing prices conditional on approval. These incentives jointly determine the effect of regulation on application behavior:

$$\frac{du_{Ai}}{d\bar{p}_i} = \underbrace{\frac{\partial P_{Ci}}{\partial \bar{p}_i} \int u_{Ci} f_{p|C}(p) dp}_{\text{Credit access } (\geq 0)} + \underbrace{P_{Ci} \frac{\partial}{\partial \bar{p}} \int u_{Ci} f_{p|C}(p) dp}_{\text{Consumer protection } (\leq 0)} \tag{10}$$

which is ambiguous and depends on which incentive dominates. If the approval probability decreases sharply in response to stronger regulation but expected prices conditional on approval do not respond as strongly, then borrowers will likely apply for loans less often. If instead the effects on approval probability are small relative to those on expected prices, borrowers will likely apply for loans more often.

[35] This expression of the unconstrained equilibrium price can be rewritten as:

$$p_i^u = \underbrace{\frac{\varphi(T_i)}{E_\varepsilon[\varphi(S_i)]}}_{\text{Risk adjustment}} \underbrace{(f_i - \omega_{i(1)})}_{\text{Mg. Cost}} + \underbrace{\omega_{i(1)} - \omega_{i(2)})}_{\text{Mark-up}} \tag{8}$$

where it is clear that unconstrained loan prices are comprised by risk-adjusted cost and a mark-up determined by the cost advantage of the bank signing the contract relative to its closest competitor.

Banks' Lending. We consider how interest rate regulation affects pricing and approval incentives for banks. The effect of stronger interest rate regulation on banks' expected profits depends on whether it is binding. For loan applicants who were already in the market, profits decrease under stronger regulation whenever it is binding, and are unaffected whenever it is not binding. There are two possible scenarios for the former set of applicants, as banks may (i) choose to sign those contracts as long as they yield non-negative profits; or instead (ii) choose to reject them if they yield negative profits at the lower interest rate cap. Given borrower expected profitability is decreasing in observable risk at a given price, the probability that a bank decides to reject an application under stronger interest rate regulation is increasing in observable risk.

Heterogeneity across Consumers. Interest rate regulation may potentially affect borrowers in four ways. First, consumers who remain in the market under stronger regulation and are offered contracts at a lower price are *protected* and increase their consumer surplus. That is, the policy is a transfer from banks to borrowers in the amount of the change in the interest rate cap. Second, consumers who become *excluded* from the market either by being discouraged from applying for loans or by having their applications rejected under stronger regulation. Third, consumers who enter the market because of stronger regulation are *included*. These are consumers that experience an improvement in their expected loan prices due to stronger interest rate regulation without a strong enough decrease in their approval probability, such that regulation induces them to enter the market and apply for loans. Finally, consumers for whom stronger regulation does not change their approval probability nor their expected loan prices are *unaffected*.

Welfare Effects. The effects of stronger regulation on expected consumer surplus are ambiguous and determined by the same forces as the effects on application behavior in equation (10). The effect on expected consumer surplus will have the same sign as that on application behavior. From an ex-post perspective, effects combine increases in consumer surplus for protected and included borrowers, with decreases in consumer surplus for excluded borrowers, and decreases in bank profits. The overall effect is ambiguous.[36]

Market power and selection are relevant for welfare effects. First, note that in a setting without market power, no borrower is protected by the policy, as all marginal borrowers become unprofitable for banks under stronger interest rate regulation. Second, if there is selection into the market on observable risk and willingness to pay for loans is correlated with risk, then the direction of selection will matter for welfare implications, given (observably) riskier borrowers are more likely to be excluded.

Loan Performance. If stronger interest rate regulation improves the borrower pool risk through rejecting marginally (observably) riskier borrowers, then the aggregate default rate in the market decreases under stronger regulation. In this model, where prices do not directly affect repayment, the effect of interest rate regulation on aggregate loan performance is thus purely compositional.

[36] Previous research on the welfare effects of price caps predicts mostly adverse effects on consumer surplus for perfectly competitive markets (e.g., Glaeser and Luttmer 2003; Bulow and Klemperer 2012). In contrast, our model predicts ambiguous effects on consumer surplus, because we study an imperfectly competitive market.

4.2 Model Discussion

The model provides a framework to study regulation in consumer credit markets. It accommodates many features common to these markets, such as price dispersion, risk pricing, the role of relationships for approvals and pricing, among others. However, it also has limitations that we discuss.

Static Demand. We model borrower application choices as a static problem. Theoretical models of demand for credit often involve intertemporal optimization problems where the trajectory of interest rates determines optimal borrowing and saving. However, that class of model only yields closed form solution in restricted cases, which often fail to accommodate heterogeneity in loan contracts (Attanasio et al., 2008). Instead, we focus on the static problem where a borrower chooses whether to finance credit needs by applying for loans or not. Previous empirical research of loan demand also adopts this static approach (e.g., Alessie et al. 2005; Attanasio et al. 2008; Einav et al. 2012). This assumption might not be appropriate for large loans such as mortgages, for which consumers often shop over long periods and for which evidence shows that consumers react dynamically to market conditions (Mian and Sufi, 2009). However, it is likely appropriate for markets for smaller loans, such as consumer loans in our setting. In fact, evidence from our survey suggests that borrowers spend a median of only 7 days searching for consumer loans, as displayed in Figure A.17-b. Moreover, as much as 66% of the respondents say that they search credit "quickly" in response to financing needs. These patterns suggest that focusing on static choices is meaningful in our context.

Exogenous Loan Amount and Term. We assume that loan amount and term are determined in a previous stage not in the model. This is in line with modeling loan demand as a response to shocks, but imposes a constraint on consumer behavior. The appeal of this assumption implies is that the application equation becomes a binary choice. Moreover, the fact that we find no effects of the policy change on the distribution of loan size in our analysis in Appendix A.2, suggests that not modeling this substitution dimension might be a reasonable assumption for our purpose and setting. Finally, the extent to which loan size and term signal borrower cost should be captured by including (L_i, T_i) in our repayment equation.

Bank Competition as English Auction. We model equilibrium interest rates as the result of an English auction, where banks compete for borrowersby offering lower interest rates. The appeal of this approach is that it provides a tractable model that accommodates price dispersion and imperfect competition. Moreover, it avoids the need to specify the prices of all alternatives in consumers' choice sets, which are unobserved to us.[37] This approach has been recently used for modeling markets with bargained prices (Allen et al., 2014, 2019; Salz, 2020), and is isomorphic to modeling the market as a standard Bertrand game where firms produce homogeneous goods with heterogeneous costs (Beckert et al., 2018). Under this framework, the source of bank market power in our model is cost heterogeneity, which translates into interest rates being set at a mark-up over expected costs, somewhat similar to the interpretation of market power in Petersen and Rajan (1995). Additionally, survey evidence in Figure A.17-c shows that for a given application 89%

[37] An alternative approach to study would be to model the game between borrowers and banks as a Bertrand-Nash game with posted prices and to predict the prices that competing banks would offer to each borrower using information from signed contracts (e.g., Crawford et al. 2018). Modeling the game between borrowers and banks as an English auction avoids that prediction step.

of borrowers considered more than two banks, and the median borrower considered three banks. This evidence suggests that borrowers indeed interact with several banks in their shopping process.

Search Frictions. We do not model other sources of market power such as search frictions, which have been the focus of recent research on credit markets (Woodward and Hall, 2012; Allen et al., 2019; Agarwal et al., 2020; Galenianos and Gavazza, 2020). A first implication of this assumption is that we disregard potential effects of interest rate regulation on search effort. Price caps reduce price dispersion and thus decrease search effort, which may lead to unintended effects such as increases in equilibrium prices (Fershtman and Fishman, 1994; Armstrong et al., 2009). Our model does not account for such channel. A second implication is that our estimates of the model might understate the amount of bank market power.

Moral Hazard. Loan price p_{ij} does not enter into the repayment equation, which implies the model rules out moral hazard in the form suggested by Holmstrom and Tirole (1997). We depart in this aspect from recent work on credit markets, such as Adams et al. (2009). While restrictive, this assumption substantially simplifies the analysis of bank pricing. Moreover, recent experimental evidence in Castellanos et al. (2018) suggests moral hazard might not be a first order concern in consumer credit markets.

5 Econometric Model

The model is summarized by equations (5), (6), and (9) for applications, repayment, and pricing. The structural objects of interest on the demand side are the parameters in the indirect utility function of consumers, $u_{Ci}(x_i, L_i, T_i, p_i)$; the parameters in the application cost, $\kappa(z_i)$; the parameters in the repayment equation, $s(x_i, L_i, T_i, \varepsilon_{Si})$; and the joint distribution of application and repayment shocks, F_ε. On the supply side, we are interested in the distribution of banks' costs, G_ω.

We estimate using three sets of observables in our data. First, we observe borrower covariates x_i, application shifters z_i, funding cost f_i, relationships with banks r_{ij}, and application choices a_i for all borrowers. Second, we observe loan amount and term for each applicant, (L_i, T_i). Finally, we observe loan monthly payment and repayment for each approved applicant, (p_i, s_i). In this section, we specify the model and state relevant statistical assumptions, and then develop an identification discussion before moving to estimation.

5.1 Model Specification

Application and Repayment. We specify the net indirect utility of a contract as a linear function of borrower attributes x_i, loan amount, term, and prices; and the application cost as a linear function of shifters z_i. In particular, we specify the application probability in equation (5) as:

$$P_{Ai} = \Pr\left(P_{Ci} \int (x_i'\delta_X + \delta_L L_i + \delta_T T_i - \delta_p p) f_{p|C}(p) dp - z_i'\kappa + \varepsilon_{Ai} \geq 0\right) \tag{11}$$

where x_i is a vector of borrower covariates that includes the borrower risk score, income, debt to income ratio, default to debt ratio, gender, and age along with market and month dummies. Additionally, z_i is a

vector of application shifters that includes the total number of banks' branches in the local market where the borrower is located, and the number of related banks of the borrower in the previous year. We discuss the role of these application shifters for identification below.

In terms of loan repayment, we adopt the same specification as Einav et al. (2012) for the repayment share. In particular, we specify the repayment share in equation (6) as a function of borrower covariates and contract terms:

$$s_i = \min\{\exp(x_i'\alpha_X + \alpha_L L_i + \alpha_T T_i + \varepsilon_{Si}), 1\} \tag{12}$$

which has the advantages that: (i) it is bounded in the unit interval, and that (ii) it accommodates the possibility of a mass point at full repayment, something we do observe in the data. The vector x_i in this specification is the same as that in the application equation above.

Moreover, we specify the joint distribution of application and repayment shocks F_ε as a bivariate normal:

$$\begin{pmatrix} \varepsilon_A \\ \varepsilon_S \end{pmatrix} \sim N\left(\begin{array}{cc} 0 \\ 0 \end{array}, \begin{array}{cc} \sigma_A^2 & \rho\sigma_A\sigma_S \\ \rho\sigma_A\sigma_S & \sigma_S^2 \end{array} \right) \tag{13}$$

where ρ determines the extent of adverse or advantageous selection in the market. In particular, $\rho < 0$ implies adverse selection, as riskier borrowers are more likely to apply for loans; whereas $\rho > 0$ implies advantageous selection, as then safer borrowers are more likely to apply for loans. Moreover, σ_A^2 and σ_S^2 are respectively the variance of application and repayment shocks, and we normalize σ_A^2 to 1. While restrictive, assuming a normal distribution has the advantage of providing a closed form relationship between the conditional and unconditional distributions of shocks, something that related previous work also exploits (e.g., Einav et al. 2012; Crawford et al. 2018). Under this specification, the demand side of the model takes the form of a standard selection model with a normality distributional assumption (Heckman, 1979).

Banks' Costs. We specify the cost function of banks as $m_{ij} = f_i - L_i\omega_{ij}$ such the bank-borrower idiosyncratic component is measured per loan unit. For the match-value component of cost, we assume that it follows an i.i.d. extreme value distribution, $\omega_{ij} \sim T1EV(\delta_{ij}, \sigma_\omega)$. We parametrize the location parameter as $\delta_{ij} = \tau_j + \gamma r_{ij}$, where τ_j is a bank-specific intercept, and r_{ij} is an indicator for a previous relationship between borrower i and bank j. Bank fixed effects τ_j allow for banks to hold cost differences that are constant across borrowers. Allowing for cost to depend on previous relationships is motivated by the differences in approvals and interest rates between previously related and non-related borrowers documented in Table 2. Therefore, the parameter γ captures the potential incumbency advantage that banks previously related to a loan applicant hold relative to non-related banks.[38] Finally, we denote the idiosyncratic component of ω_{ij} as $\varepsilon_{\omega ij}$, which captures variation in cost at the borrower-bank level, which could be driven by heterogeneity in banks' services in local markets or relationships between borrowers and local branch officers.[39]

This specification of banks' costs is consistent with several of the facts in Section 2.4. In particular, this specification allows for: (i) expected default cost to vary across borrowers according to borrower

[38]The advantage of assuming an extreme value distribution for the match-value component of cost is that it provides closed form expressions for distributions of order statistics of ω_{ij}, which are useful for estimation as discussed below. We summarize these properties in Appendix B.4. Proofs for these results are available in Froeb et al. (1998).

[39]As an example of the cost heterogeneity captured by $\varepsilon_{\omega ij}$, Drexler and Schoar (2014) use data from a large Chilean bank to show that loan officer turnover has sizable effects on loan approval and borrower default behavior.

observables; (ii) bank costs for a given borrower to vary across banks; and (iii) bank costs to depend on previous relationships with borrowers, thus introducing the potential for incumbency advantages.

5.2 Identification

We discuss how variation in the data identifies the model and describe our identification assumptions. We assume that borrower covariates, loan amount and term, and application cost shifters (x_i, L_i, T_i, z_i) are exogenous. The main identification assumption is conditional independence between the idiosyncratic component of cost shocks ω_{ij}, and application and repayment shocks $(\varepsilon_{Ai}, \varepsilon_{Si})$. Formally, this is:

$$\varepsilon_{\omega ij} \perp\!\!\!\perp (\varepsilon_{Ai}, \varepsilon_{Si}) | (x_i, L_i, T_i) \tag{14}$$

which implies that the idiosyncratic component of banks' costs is unrelated to unobservable determinants of application and repayment behavior, once borrower observables are accounted for. The economic implication of the assumption is that banks do not have any informational advantage relative to the econometrician in terms of the determinants of borrower application and repayment behavior, that affects banks' costs and, therefore, pricing. While restrictive, this assumption relies on the fact that our detailed dataset is the same dataset that the regulator provides to banks for pricing purposes. Note that this assumption does not imply that banks' costs are invariant to borrower attributes and application behavior. In fact, banks' consider observable risk for pricing and also infer unobservable risk from applications. Under this assumption, we can treat identification and estimation of the demand and supply sides of the model separately.

Applications and Repayment. The demand side of the model has the structure of standard selection models, where application is the selection equation and repayment is the outcome equation, and where the correlation between the unobservable components of them has the interpretation of adverse or advantageous selection. Parametric and non-parametric identification of this model is established in Heckman (1979) and Das et al. (2003), respectively. The latter emphasizes the importance of exclusion restrictions for identification. In that line, we exploit two application cost shifters in z_i as exclusion restrictions. First, we include the number of branches in the local market as a measure of local bank density, which should reduce application cost. This shifter is in line with papers that exploit distance as a shifter of school applications (e.g., Walters 2018). Second, we include the number of previously related banks as a measure of previous experience dealing with banks, which should make the application process easier to navigate and decrease application cost. Both of these variables arguably shift application choices, but they are are unlikely to directly affect the utility that borrowers obtain from loans or their repayment behavior.

Given the model specification and our identification assumption, the intuition for how variation in the data identifies the demand side of the model is as follows. Application responses to variation in (x_i, L_i, T_i, p_i) identify δ in the application equation. Moreover, repayment responses to variation in (x_i, L_i, T_i) identify α in the repayment equation. Regarding the joint distribution of application and repayment shocks F_ε, the intuition is that consumers observed applying for loans when the model predicts they should not, are likely to have a high ε_{Ai}. The conditional correlation between those shocks and observed repayment identifies ρ. In particular, if those borrowers are observed to repay less, then $\rho < 0$ and there is adverse selection.

A concern is the potential endogeneity of loan prices. Prices combine policy variation from changes in interest rate regulation with variation induced by bank pricing. Under the identification assumption in equation (14), (x_i, L_i, T_i) is all the information that enters both banks' pricing and consumers' application choices. Therefore, application responses to price variation conditional on such vector identify price sensitivity. However, if the assumption fails and banks observe drivers of applications that are unobservable to the econometrician and exploit them for pricing, then identification of price sensitivity fails. We use coefficient stability and control function approaches to assess this assumption, and provide support to it.

Banks' Costs. The identification of banks' costs follows from standard arguments in the auctions literature. Assuming independence in cost shocks ω_{ij} across banks and borrowers, the supply side of our model corresponds to an independent private values auction. As established by Athey and Haile (2002), the distribution of values in asymmetric independent private values auctions is non-parametrically identified from transaction prices and the identity of the auction winner. In our setting, we observe prices and the identity of the bank for all contracts in the market, and therefore the distribution of banks' costs is identified.

Figure A.19 provides a diagram that connects data to supply side primitives in our model. For unconstrained approvals, observed prices are a function of the cost of the second lowest bank. For constrained approvals, observed prices are those implied by the interest rate cap and are bounded from below by the chosen bank cost and from above by the unconstrained price. For rejections, prices implied by the interest rate cap are bounded from above by the cost of the lowest cost bank. Thus, we learn about the cost function of banks by combining our model with data on bank choices, loan prices and application outcomes.

We relate this argument to our specification of banks' costs. Conditional on funding cost f_i, identification of banks' costs relies on variation in contract prices, bank choices, and application outcomes. First, identification of constant cost differences across banks τ_j rely on differences in prices across chosen banks. Second, identification of incumbency advantage γ relies on variation in prices within chosen banks across applicants with and without a previous relationship with the bank. Finally, any remaining variation in loan prices within banks and bank-borrower relationships identifies the scale of idiosyncratic cost shocks, σ_ω.

5.3 Estimation

Estimation proceeds in three steps. First, we estimate the parameters of the application equation using data from the pool of potential applicants. Second, we estimate the repayment equation using data on loan performance for signed contracts. Third, we use estimates from the second step to compute fitted repayment risk and then proceed to estimate banks' cost by exploiting the auction structure of the supply side of the model. All three equations are estimated by maximum likelihood.

Applications and Repayment. The joint estimation of the application and repayment equations in (11) and (12) proceeds in three steps. The two first steps are related to estimation of components of the application equation in equation (11) that are not observed for every consumer in the sample, and that we then use as inputs in estimation of the key parameters in that equation.

The first step deals with the fact that loan terms (L_i, T_i) are not observed for non-applicants. We estimate

the conditional distribution of loan amount and term using data from applicants and then draw from that distribution for non-applicants. In order to deal with concerns related to selection into application, we implement a control function approach in this step, similar to Attanasio et al. (2008) and based on Das et al. (2003). In the first stage, we estimate a flexible probit model for applications on a rich vector of borrower covariates, and application shifters in z_i. In the second stage, we compute fitted propensity scores using estimates from the first stage and add that propensity score as a control function in a regressions of loan amount on the same set of borrower covariates. Finally, we estimate an ordered logit model for loan term monthly bins on the same set of borrower covariates and loan amount. We use estimates from that second stage to draw loan amount and term for non-applicants in the sample. As expected, predicted loan amount differs for applicants and non-applicants: loan amount for applicants is $1,000 larger on average, equivalent to 0.14 standard deviations. For further detail on this procedure, see Appendix B.1.

In the second step, we deal with the fact that the approval probability P_C and the density of loan prices conditional on approval $f_{p|C}$ enter the application equation and are not directly observed in data for each borrower. First, we estimate P_C using a probit model for application approval on a vector of borrower covariates, previous relationship variables, as well as application amount and term from the first step. We compute fitted approval probabilities for each consumer in the sample and use them as inputs in the third step below. Second, we estimate $f_{p|C}$ using a kernel density estimator after conditioning on the same vector of variables. We use draws from this estimated conditional density in the third step of estimation. We let both P_C and $f_{p|C}$ vary across time, to capture the effects that variation of interest rate caps over time have on them. The strategy of estimating these elements in a previous stage and use them as inputs for the last step is similar to that in Kawai et al. (2018). We provide more detail about this procedure in Appendix (B.1).

In the third step, we estimate the parameters in the application and repayment equation by maximum likelihood using inputs from the first and second steps above.[40] We exploit the joint normality of $(\varepsilon_{Ai}, \varepsilon_{Si})$ in equation (13) to derive the likelihood function, which provides closed form expressions for the distribution of repayment shocks ε_{Si} conditional on application shocks ε_{Ai}. For a detailed derivation of the likelihood function, see Appendix B.2.

Banks' Costs. We exploit the structure of the auction model and the distributional assumption imposed on ω_{ij} to estimate the distribution of banks' costs by maximum likelihood. We start by computing fitted repayment risk $E_\varepsilon[\varphi(S_i)]$ using estimates from the application and repayment equations and 100 Halton draws for $(\varepsilon_{Ai}, \varepsilon_{Si})$.[41] Given that input, we work separately on the likelihood for each of the potential application outcomes in equation (9), for every potential consideration set $\mathcal{J}_i$. We then integrate the likelihood of each outcome over the applicant consideration set. For the derivation of the likelihood function, see Appendix B.3.[42]

[40] We use 100 Halton draws from the estimated density of prices conditional on approval to compute the expected indirect utility from signing a loan contract. Train (2009) argues that Halton draws have better coverage properties than pseudo-random draws, which in practice implies that 100 Halton draws provide a similar level of efficiency than simulation with 1,000 pseudo-random draws.

[41] We employ a annual discount rate of $r = 5\%$ for all banks in the market for both estimation and counterfactuals.

[42] We describe useful properties of distributions of order statistics of the T1EV distribution in Appendix B.4. The relevance of such properties is that they allow for obtaining closed form expressions for the likelihood of each of the potential outcomes of the model in terms of observables, which greatly simplifies estimation.

Estimating Dataset. We estimate the model using a sample of potential applicants for 2013 and 2014, which are the years before and after the policy change. This sample includes 316,384 potential applicants, of which 49,883 apply for loans. Given application events are rare at a monthly frequency, we collapse the data at the yearly level by including data for the application month for consumers who apply within a year, and data for a random month in the year for consumers that do not apply within a year. We define the set of banks that consumers shop as the 9 largest banks in the market, which account for 98% of market share. All consumers in the estimating dataset are located in markets in which all 9 banks offer consumer loans.

We allow for observable heterogeneity on two sets of parameters. First, we let the price sensitivity coefficient differ across low- and high-risk borrowers. Second, we allow for the coefficients in banks' costs to differ by loan term, so as to allow for loans of different terms to have different monthly costs for banks.

5.4 Results

Application Behavior. Table 6-A displays our estimates for the application equation.[43] Borrowers are more likely to apply for loans when facing a higher approval probability, and increasing the approval probability by 5 p.p increases the probability of application by 5 p.p. Riskier borrowers are more likely to apply and, in particular, a 5 p.p increase in borrower predicted default probability increases the application probability by 1.75 p.p. Moreover, female and older consumers are less likely to apply. Loan amount increases the probability of application, and a loan amount $5,000 larger increases the probability of application 1.1 p.p. Finally, borrowers are price sensitive and higher expected prices reduce the application probability. High-risk borrowers are less price-sensitive than low-risk borrowers, for instance a $200 increase in expected monthly payment decreases the application probability of the former by 1.5 p.p, and of the latter by 2.4 p.p.

As discussed in Section 5.2, a concern for our strategy is the potential for unobservables that drive both application decisions by borrowers and pricing decision by banks, which would influence our estimates of price sensitivity, δ_p. We address this concern by implementing two robustness exercises. First, we assess the stability of $\hat{\delta}_p$ when estimated using a cumulative set of covariates in the application equation, in line with Altonji et al. (2005). Figure A.21 shows that $\hat{\delta}_p$ from specifications that do not include borrower risk scores and other borrower covariates differ remarkably from those that include such variables. Moreover, the results show that adding additional borrower covariates after accounting for risk score has only minor effects on $\hat{\delta}_p$. Second, we employ a control function approach to provide additional evidence for the robustness of $\hat{\delta}_p$. We follow Petrin and Train (2010) and implement a two-step procedure. In the first step, we regress monthly payments on covariates in x_i and a cost shifter of prices. We use funding cost as a shifter, where the funding rate provides variation across time, and heterogeneity in loan amount and term across consumers provides individual level variation. In the second step, we include the residuals from the first stage as an additional covariate in x_i, with the objective of controlling for unobservable drivers of prices. The last estimates in Figure A.21 are the result from this approach, and show that our estimates $\hat{\delta}_p$ do not change substantially.[44] While not conclusive, these results suggest that the set of covariates in x_i for

[43] We report standard errors based on the inverse of the hessian of the log-likelihood functions we maximize. This procedure does not account for the fact that estimation proceeds in steps. Therefore, our standard errors are possibly incorrect and likely overestimate the precision of our estimates. Bootstrapped standard errors are work in progress. While we are likely underestimating standard errors, the fact that most of our estimates are statistically significant at very high confidence levels suggests our conclusions are unlikely to change after adjusting standard errors.

[44] Our estimate for the coefficient on the control function is -18.21 with standard error 0.93, thus statistically significant. This

estimation might be able to deal with the concern about unobservable drivers of applications and pricing.

Estimates of application costs point in the expected directions. Both the number of branches in the local market and consumers' previous experience with banks reduce application costs, as expected. In particular, having 100 more branches in a local market increases application probability by 0.9 p.p, whereas holding a previous relationship with an additional bank increases the application probability by 2.9 p.p.

Loan Repayment Behavior. Estimates for the loan repayment equation are displayed in column (3) of Table 6-A. As expected, riskier borrowers repay less on their loan contracts. In fact, a 5 p.p increase in borrower risk score decreases repayment share by by 0.6 p.p. Moreover, female and older borrowers display better repayment behavior. In terms of loan terms, borrowers taking larger loans tend to repay less, while borrowers taking longer term loans display the opposite behavior. Finally, our estimate of σ_S implies there is substantial unobservable borrower risk.

Adverse Selection. We find no compelling evidence of adverse or advantageous selection, conditional on borrower risk scores. Our point estimate for ρ is close to 0 and is not statistically significant. However, our estimate is not precise and thus we cannot rule out that some degree of adverse selection in the market. This result implies that although there is substantial unobservable repayment risk σ_S, that risk does not drive application behavior, conditional on (x_i, L_i, T_i). This result does not imply there is no selection on observables. In fact, our results show that riskier borrowers are more likely to apply for loans and are less likely to repay them. However, that is accounted for in x_i and is therefore not reflected in our estimate for ρ.

We address the role of observables in determining our selection estimate. Figure A.22 shows estimates of ρ using different sets of borrower covariates in x_i in *both* the application and repayment equations. Not accounting for observables yields estimates that would provide strong evidence of adverse selection ($\hat{\rho} < 0$). However, once we include borrower risk scores and income, point estimates of ρ remain close to 0, in line with our preferred specification. These result suggest that observables in our data—which are the same provided by the regulator to banks for risk assessment—account for most of risk selection into the market.

Banks' Costs. Table 6-B displays estimates of banks' costs, which reveal substantial heterogeneity in banks costs. To interpret these estimates, we describe how they relate to the monthly payments associated with a loan of \$2,000, that have median and standard deviation (σ_p) of \$118.62 and \$69.90, respectively. Estimates of bank-specific components of ω_{ij} imply sizable cost differences across banks: on average, the cost difference between the most and least efficient banks for a given loan is of \$25.99 per month, equivalent to $0.37\sigma_p$. Moreover, we estimate that having a previous relationship with a bank provides an incumbency advantage to the bank. In particular, having a previous relationship reduces the monthly cost of providing a \$2,000 loan by \$32.74 per month, around $0.46\sigma_p$. Finally, our estimates show that the standard deviation of bank-borrower idiosyncratic shocks is large: a 1 s.d increase in this shock decreases cost by \$19.17 per month, equivalent to $0.27\sigma_p$. This suggests that unobserved variation in costs across banks is a relevant determinant of residual price dispersion. There is heterogeneity in estimates across cost bins, although without a clear

suggests that including it indeed controls for such potential unobservables. However, the fact that estimates $\hat{\delta}_p$ remain similar to those without the control function suggests that the relative importance of those unobservables relative to observables in (x_i, L_i, T_i) is minor.

Table 6: Model estimates

	(1)	(2)	(3)	(4)
Panel A - Demand side	Application		Repayment	
	Estimate	S.E.	Estimate	S.E.
Drivers of application (δ) and repayment (α)				
Constant	3.500***	(0.076)	0.049	(0.126)
Risk score	1.390***	(0.053)	-2.008***	(0.098)
Female	-0.124***	(0.006)	0.026**	(0.010)
Age $\in$ [33,55)	-0.032***	(0.006)	0.031***	(0.010)
Age $\in$ [55,+)	-0.309***	(0.009)	0.079***	(0.016)
log(Annual income)	0.016***	(0.005)	0.110***	(0.011)
Debt to income ratio	0.442***	(0.040)	0.024	(0.065)
Default to debt ratio	-0.838***	(0.039)	-0.006	(0.019)
Loan term	0.045***	(0.003)	-0.082***	(0.005)
Loan amount	0.009***	(0.001)	0.003***	(0.001)
Monthly payment, low-risk	0.479***	(0.016)		
Monthly payment, high-risk	0.304***	(0.017)		
Application cost (κ)				
Constant	4.607***	(0.099)		
Number of branches	-0.000***	(0.000)		
Previously related banks	-0.196***	(0.003)		
Application and repayment shocks				
Standard deviation (σ_A, σ_S)	1.000	—	0.525***	(0.008)
Correlation (ρ)	-0.010	(0.046)		
Month FEs	Y		Y	
Market FEs	Y		Y	
Panel B - Banks' costs	Short term		Long term	
	Estimate	S.E.	Estimate	S.E.
Range of bank fixed effects (τ)	[-0.038***,-0.024***]		[-0.038***,-0.025***]	
Previously related to bank (γ)	0.016***	(0.000)	0.016***	(0.000)
Bank-borrower shock (σ_ω)	0.009***	(0.000)	0.010***	(0.000)

Notes: Panel A displays estimates from the demand side of the model. Columns (1) and (2) display estimates and standard errors for parameters in the application equation. Columns (3) and (4) display estimates and standard errors for parameters in the repayment equation. The specifications of both the application and repayment equations include month and market fixed effects. Panel B displays estimates from the supply side of the model. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

pattern associated with loan term. This suggests that after accounting for funding cost the cost per dollar of loan does not correlate strongly with loan term.

It is useful to understand how these estimates relate to the data. First, estimates of bank cost fixed effects τ_j align with observed market shares, as displayed by Figure A.20-a. The model rationalizes high market shares as cost advantages, captured by higher fixed effects in ω_{ij}. Second, market shares and the share of previously related borrowers are positively correlated, as displayed by Figure A.20-b. The model rationalizes this correlation as that banks hold an incumbency advantage when serving previously related borrowers relative to rivals without those relationships. This explains our positive estimate for γ.

5.5 Model Fit

We examine model fit by using the estimated parameters to simulate equilibrium outcomes and compare simulated to observed outcomes. We run this simulation and all simulations in the next section on the estimating dataset. In particular, we proceed as follows:

1. Draw shocks for applications, repayment and cost. Specifically, (i) draw application and loan repayment shocks for each borrower in the sample from the estimated joint distribution, $\{\varepsilon_{Ai}, \varepsilon_{Si}\}$; and (ii) draw a cost shock for each bank-borrower in the sample, ω_{ij}.

2. Draw shocks for integration steps. Specifically, (i) draw a vector of N_ω bank-borrower cost shocks for integration of prices by borrowers, $\{\omega_{ij}^{(s)}\}_{s=1, j\in\mathcal{J}}^{s=N_\omega}$; and (ii) draw a vector of N_S loan repayment shocks per borrower for integration of repayment risk by banks, $\{\varepsilon_{Si}^{(s)}\}_{s=1}^{s=N_S}$.

3. Simulate optimal prices and approval decisions for each of the N_ω vectors of cost shocks for a given interest rate regulation $\bar{p}_i$, which are required for simulating application decisions. This step requires solving a fixed point problem, because banks take the expectation of repayment risk conditional on application into account, and application in turn depends on expected approval probability and prices. We proceed by: (i) computing simulated unconditional repayment risk as a starting point, (ii) computing simulated application decisions, (iii) computing expected approval probability P_{Ci} and monthly payments conditional on approval $\{p_i^{(s)}\}_{s=1}^{s=N_\omega}$ given simulated repayment risk, (iv) computing simulated conditional repayment risk, and (v) repeating (ii)-(iv) until convergence of simulated monthly payments. The outputs of this step are simulated approval probability P_{Ci}, monthly payments $\{p_i^{(s)}\}_{s=1}^{s=N_\omega}$, and expected repayment risk $E_\varepsilon[\varphi(S_i)]$.

4. Simulate application decisions for each borrower a_i, by computing application probabilities using simulated approval probabilities and monthly payments from Step 3 along with draws for application shocks from Step 1.i.

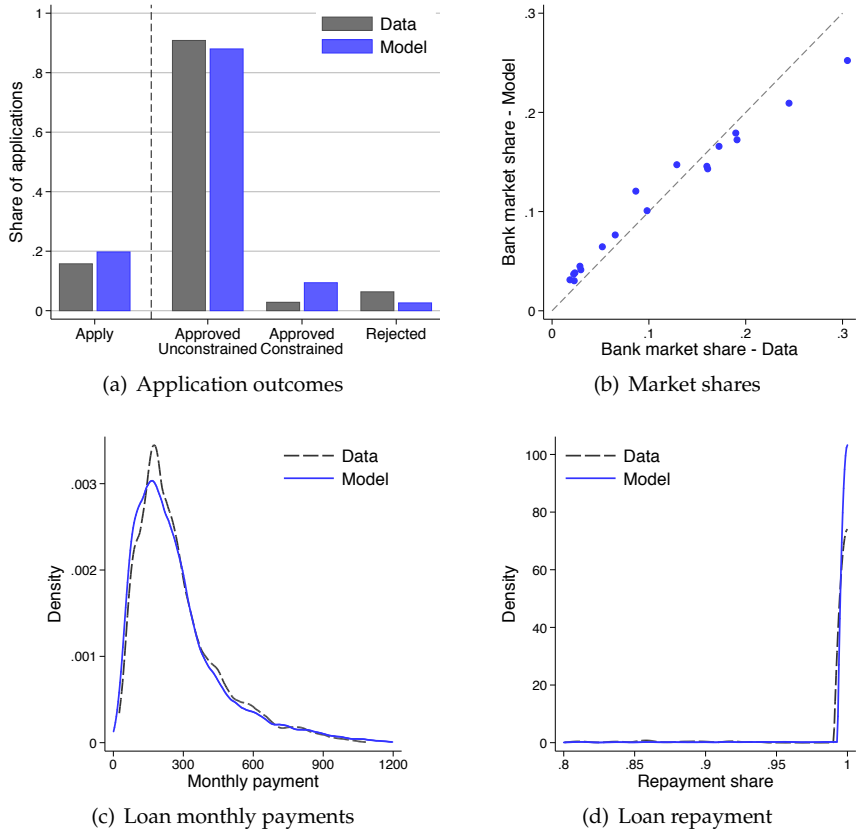
5. Simulate approval and pricing decisions by banks (L_i, p_i), using draws for cost shocks from Step 1.ii and simulated repayment risk from Step 3.

6. Simulate repayment outcomes for borrowers s_i, using estimates for the repayment equation along with repayment draws in Step 1.i.

Figure 4-a shows that simulated application outcomes are close to observed outcomes, although the model overpredicts constrained approvals and underpredicts unconstrained approvals relative to the data. This suggests applicants may face frictions in their choice sets formation that our model does not account for. Figure 4-b shows that predicted market shares track observed market shares closely. Moreover, Figure 4-c shows that the model fits the distribution of loan prices well, with a correlation between predicted and observed prices of 0.95. Finally, Figure 4-d shows that the estimated model provides a good fit of the density of loan repayment share.

Finally, Figure A.23 shows estimated expected profit margins, which are 29.6% on average, and display substantial dispersion. We use the simulated model under actual regulation to decompose prices into three components: cost, risk, and market power. This decomposition follows a rearranged version of equation

Figure 4: Model fit



(a) Application outcomes

(b) Market shares

(c) Loan monthly payments

(d) Loan repayment

Notes: This figure displays results for model fit. Simulations are implemented as detailed in Section 5.4. Panel (a) displays observed and predicted shares of approved, constrained and rejected applications. Panel (b) displays observed and predicted bank market shares. Panel (c) displays the observed and predicted distribution of loan monthly payments. Panel (d) displays the observed and predicted distribution of loan repayment.

(8). Our results show that, on average, funding and banks' costs jointly account for 71.2% of loan prices. Risk accounts for 9.8% of the spread between loan monthly payment and cost, while market power accounts for the remaining 90.2%. Our estimates thus imply that banks hold substantial market power in this setting.

6 Equilibrium Effects of Interest Rate Regulation

6.1 Effects on Market Outcomes

We simulate equilibrium outcomes for different regulation levels, corresponding to the level at the moment of the policy change, and those 1 and 2 years after the policy change, which is November 2013, November 2014 and November 2015. We then compare simulated effects to estimated effects to assess model predictions.

Results from these simulations are mostly in line with the evidence presented in Section 3 and are summarized in Table 7. The model predicts that stronger interest rate regulation decreases the number

of loans by 23.7%, which combines a decrease in applications and an increase in rejections by banks. As highlighted in Section 4.1, the effect of interest rate regulation on application choices depends upon its relative effects on decreased approval probability and decreased expected loan prices. In this case, we find a decrease in applications that in turn implies that the former effects dominates the latter. Moreover, loan monthly payments on loans approved under stronger regulation decrease by $2.59 and the mark-up on such loans decreases by 2 p.p, reflecting that stronger interest rate regulation is in fact protecting consumers who remain in the market. These simulated effects are in line with our analysis in Section 3, where we estimated a 19% decrease in the number of loans and a $3.26 decrease in loan monthly payments.

Table 7: Simulated effects of interest rate regulation

	(1)	(2)	(3)
Outcome	Baseline Nov/2013	Mid effect Nov/2014	Full effect Nov/2015
Apply for loans (p.p)	19.92	-2.20	-4.11
Unconstrained|Apply (p.p)	90.07	-9.50	-17.35
Constrained|Apply (p.p)	7.58	7.74	13.57
Rejected|Apply (p.p)	2.34	1.75	3.78
Number of loans (%)	-	-12.69	-23.73
Monthly payment ($)	251.73	-5.29	3.98
Monthly payment on approved under full policy ($)	258.30	-2.00	-2.59
Mark-up (p.p)	29.11	0.73	2.00
Mark-up on approved under full policy (p.p)	31.80	-0.37	-0.67
Default probability (p.p)	7.26	-0.10	-0.27
Consumer surplus ($)	-	-44.20	-82.47
Monthly profit ($)	73.46	-0.19	6.13
Monthly profit on approved under full policy ($)	82.15	-1.97	-2.55
Average monthly profit ($)	13.26	-1.77	-2.40
Average welfare ($)	-	-46.15	-84.84

Notes: This table displays results for simulated policy effects of moving from the baseline interest rate regulation in November 2013 to interest rate regulation in November 2014 and November 2015, when the policy change was fully in place. Mid and full effects are measured relative to baseline levels. Column (1) displays simulated equilibrium outcomes for regulation at November 2013. Column (2) displays simulated changes in equilibrium outcomes under regulation present in November 2014 and baseline regulation, while column (3) does the same for regulation by the end of the policy change in November 2015 and baseline regulation.

6.2 Welfare Analysis

We exploit our estimated model to estimate the welfare effects of interest rate regulation. In particular, we adopt a revealed preferences approach and exploit observed application choices along with our estimates of willingness to pay to estimate changes in expected consumer surplus; whereas we exploit observed prices and our estimates of banks' costs to estimate changes in banks' profits.[45]

[45] All our calculations related to changes in bank profits focus on variable profits from loan contracts. Therefore, any changes in fixed costs or screening costs associated with stronger interest rate regulation are not accounted for.

Expected consumer surplus for consumer i under an interest rate cap $\bar{p}_i$ is given by:

$$E[CS_i(\bar{p}_i)] = \frac{1}{\hat{\delta}_p} \int \max\{P_{Ci}(\bar{p}_i) \int u_L(x_i, L_i, T_i, p; \hat{\delta}) f_{p|C}(p; \bar{p}_i) dp - z_i'\kappa + \varepsilon_A, 0\} f_{\varepsilon_A}(\varepsilon_A) d\varepsilon_A$$

where interest rate regulation enters through both the approval probability and the density of prices conditional on approval. We use our model to construct all components on the right hand side of this expression, and calculate the effect of a change in interest rate regulation from $\bar{p}_i^0$ to $\bar{p}_i^1$ on expected consumer surplus as $\Delta E[CS_i] = E[CS_i(\bar{p}_i^1)] - E[CS_i(\bar{p}_i^0)]$. This change in expected consumer surplus is measured from an expected utility perspective, and thus reflects how credit market conditions change for potential applicants in terms of approval probability and expected prices, regardless of whether ex-post those applicants are approved and sign contracts at lower prices or are rejected.

We find that expected consumer surplus decreases by an average and median of \$82.47 and \$40.24 per month respectively, which is equivalent to 3.5% and 1.7% of average monthly income. There is substantial heterogeneity in estimated effects on expected consumer surplus, as displayed in Figure 5-a. Moreover, the distribution of estimated effects on expected consumer surplus is skewed: 66% of potential borrowers display changes in expected consumer surplus smaller than average, and less than 29% of them display decreases in expected consumer surplus of more than \$100 per month. On the other hand, bank monthly profits decrease by \$2.41 per potential borrower in the market under stronger interest rate regulation, which adds up to 18% of total profits in the market. The combination of decreases in consumer surplus and profits implies that average welfare per consumer in the market decreases.

Interest rate regulation has heterogeneous effects across consumers. Overall, we find that adverse effects dominate positive effects. In fact, our simulation implies that expected consumer surplus decreases for 82.3% of consumers, remains unchanged for 1.5%, and increases for 16.2% However, the average loss for the former is \$100.01, whereas the average gain for the latter is only \$0.32. Therefore, the effect of a decreased approval probability dominates that of a decreased expected monthly payment in terms expected surplus. These effects are positively correlated, as displayed in Figure 5-b, where the lack of borrowers in the upper-left area explains the small share of borrowers that benefits from stronger regulation. Few borrowers receive large decreases in expected prices without large decreases in approval probability.

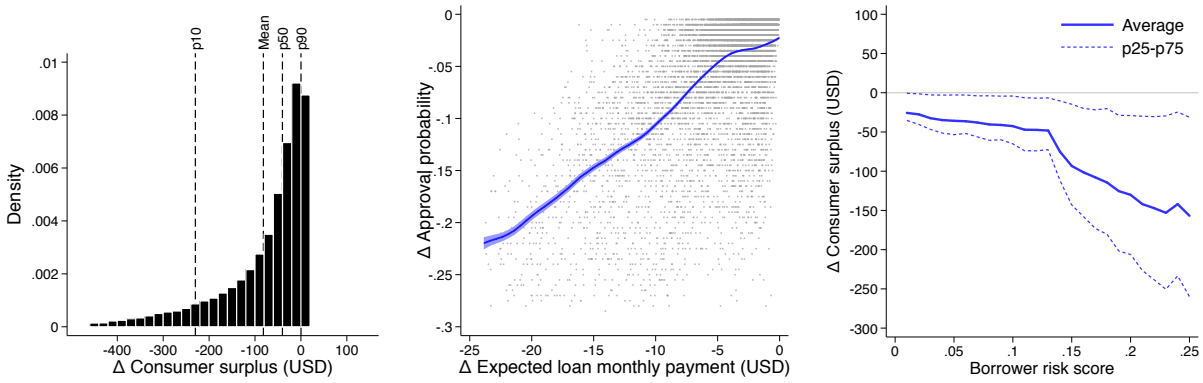
Risky borrowers are the most affected by interest rate regulation in terms of expected consumer surplus, as displayed in Figure 5-c. The average decrease in expected consumer surplus for low- and high-risk borrowers is \$38.70 and \$130.29 per month respectively. This pattern of heterogeneity is driven by three forces: risky borrowers were charged higher prices at baseline and therefore more exposed to stronger regulation; display a stronger preference for loans; and are less sensitive to expected monthly payments.

We decompose changes in borrower expected consumer surplus to further quantify the trade-off between credit access and consumer protection, as follows:

$$\Delta E[CS_i] = \underbrace{(E[CS_i(P_{Ci}^1, p_i^1)] - E[CS_i(P_{Ci}^0, p_i^1)])}_{\text{Credit access}} - \underbrace{(E[CS_i(P_{Ci}^0, p_i^1)] - E[CS_i(P_{Ci}^0, p_i^0)])}_{\text{Consumer protection}}$$

where the first term isolates the effect of lower approval probabilities, and the second term isolates the effect of lower prices, such that the overall effect combines these two effects. We estimate that the average effects

Figure 5: Heterogeneity in welfare effects of interest rate regulation



(a) Effects on consumer surplus (b) Effects on drivers of applications (c) Consumer surplus and risk

Notes: These figures display heterogeneity in effects of interest rate regulation across borrowers. All figures compare outcomes under full regulation by November 2015 with outcomes under baseline regulation by November 2013. Panel (a) displays the correlation between effects o expected approval probability and expected loan monthly payment. Panel (b) displays changes in consumer surplus across borrowers. Panel (c) displays the average, and 25th and 75th percentiles of changes in consumer surplus across borrower risk.

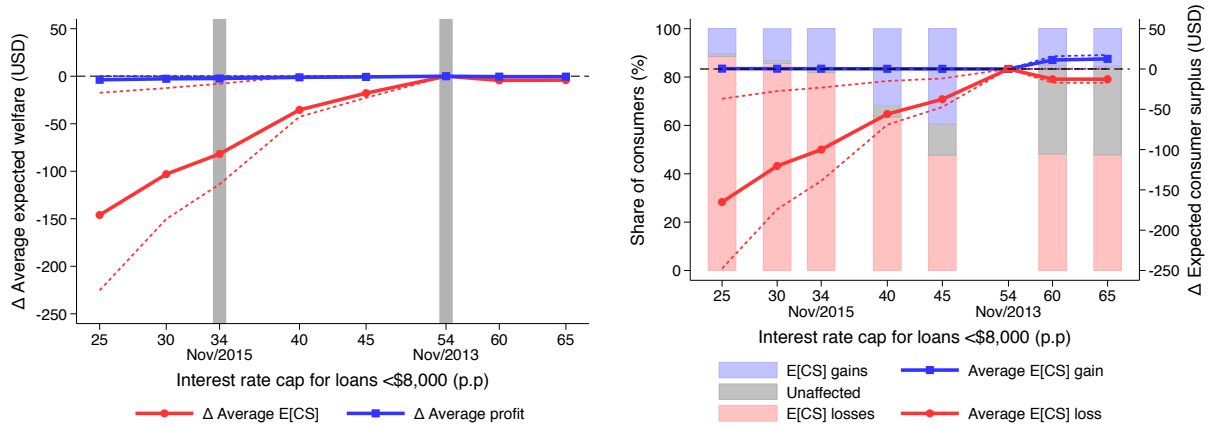
of decreased credit access and increased consumer protection on expected consumer surplus are -$82.63 and $0.85, respectively. This pattern reflects that the value borrowers place on expected reduced credit access under stronger regulation is substantially higher that value they place on the expected price decrease they would obtain in the market.

Finally, we study the welfare effects of a range of interest rate caps. Figure 6-a shows that stronger interest rate regulation beyond that in December 2015 would only further reduce expected consumer surplus. In particular, setting interest rate caps for loans in $0-$8,000 would decrease expected consumer surplus by almost $150. On the other hand, there would not be gains in terms of expected consumer surplus from setting interest rate caps higher than those in December 2013, when regulation was essentially not binding, which implies that any small benefits from increased approval probabilities would be compensated by increased expected prices. Figure 6-b shows that the share of consumers that benefit from changes in interest rate regulation is larger for moderate decreases in interest rate caps relative to those in December 2013, but remain below 20% otherwise. However, average gains in expected consumer surplus remain small across the range of regulations we study, which is consistent with the gains from the consumer protection component of interest rate regulation being low relative to the losses due to decreased credit access.

6.3 Survey Evidence for the Effects of Reduced Credit Access

In Section 3.2.2, we estimated that stronger interest rate regulation decreased the number of loans. Moreover, in the previous section we adopted a revealed preferences approach to estimate that stronger interest rate regulation decreased the average consumer surplus in the market. In this section, we exploit our survey to provide suggestive evidence about potential channels for how reduced credit access could decrease

Figure 6: Welfare effects of interest rate regulation



(a) Welfare effects

(b) Expected consumer surplus gains and losses

Notes: These figures display welfare effects of interest rate regulation across borrowers. Both figures compare outcomes for a range of regulation scenarios relative to that in November 2013. Panel (a) displays the change in average expected consumer surplus and average profit per consumer. Panel (b) displays average changes in expected consumer surplus for consumers that increase and decrease their expected consumer surplus relative to baseline, along with the share of consumers that experience experience consumer surplus gains, losses or none of them. Solid lines indicate averages and dotted lines indicate the 25th and 75th percentiles.

consumer surplus in our setting.[46]

We study how the effects of economic hardships for household vary depending on whether they deal with them using bank credit. In particular, we exploit information on whether households experienced economic hardships during the last five years, how they dealt with them, and how it affected consumption and financial outcomes for them.[47] We compare outcomes of households that did not experience any shocks with those that experienced shocks and financed them by either (i) obtaining bank credit, (ii) liquidating savings or assets, or (iii) using some other source, including informal sources of credit or increased labor supply. We estimate the following specification:

$$y_i = \alpha + \beta_c credit_i + \beta_s savings_i + \beta_o other_i + x_i'\gamma + \varepsilon_i \tag{15}$$

where y_i is the outcome of interest, x_i is a vector of control variables that includes household income, vulnerability and age of survey respondent, as well as loan approval probability, estimated using administrative data. The coefficients of interest are β_c, β_s and β_o, which measure the difference between outcomes for households that experienced no negative shock relative to those that experienced a negative shock and financed it with either credit, savings or other, respectively.

First, we study whether credit access in the event of shocks is associated with household consumption. In particular, we collect information on whether households cut expenditure on relevant items (e.g., trans-

[46] We provide some summary statistics for our household survey in Table A.3. The sample of survey respondents is riskier and of lower income than the average borrower in the market. Households are quite experienced in the credit market and most of them hold checking accounts, credit cards and have held consumer loans.

[47] We define economic hardship in the survey as a sustained period of time over which household expenses were higher than its income. In practice, 58.6% of survey participants answered they were under such situation over the previous five years. The questions related to how this shock was dealt are expressively linked to the shock itself, rather than general questions about credit access.

portation, education, health, travel, among others) due to economic hardships experienced over the last five years. Figure 7-a shows that households that experienced these shocks did cut expenses in several items, but that those effects are smaller for households that obtained bank credit upon those shocks. In particular, households that dealt with shocks using bank credit cut expenses for an average of 7.5 p.p less items than households that dealt with economic hardships using other means. These results suggests that reduced credit access might harm consumption smoothing, similar to findings in Morse (2011).

Additionally, we study whether credit access in the event of shocks affects the ability of households to repay their financial commitments. In particular, we focus on whether households stopped paying bills (e.g., health bills, rent, mortgage payments, credit card and consumer loans payments, among others). Figure 7-b shows that households that obtained credit access upon economic hardships do not display any differential behavior relative to households that did not experience economic hardships. However, households that did not access credit are significantly more likely to have unpaid bills than the latter. In particular, households that dealt with economic hardships using bank credit are 36 p.p less likely to have any unpaid bill than those that dealt with economic hardships using other means. This suggests that credit access might provide liquidity to avoid financial distress episodes associated with debt repayment, as in Zinman (2010).

These results provide suggestive evidence that credit access serves as a means for consumption smoothing and alleviation of financial distress upon economic hardships, although we do not claim that our estimates describe a causal relationship between them. We interpret this evidence as complementary to our estimates of effects on consumer surplus. These results are in contrast with research finding adverse effects of access to payday loans on financial distress (e.g., Melzer 2011; Gathergood et al. 2018; Skiba and Tobacman 2018). This contrast might be driven by the fact that interest rates in the market we study are substantially lower than those charged on payday loans—which are often the setting for those studies—, and therefore access to this type of credit is less likely to lead to financial distress, as in Morse (2011).

7 Counterfactual Analysis of Interest Rate Regulation

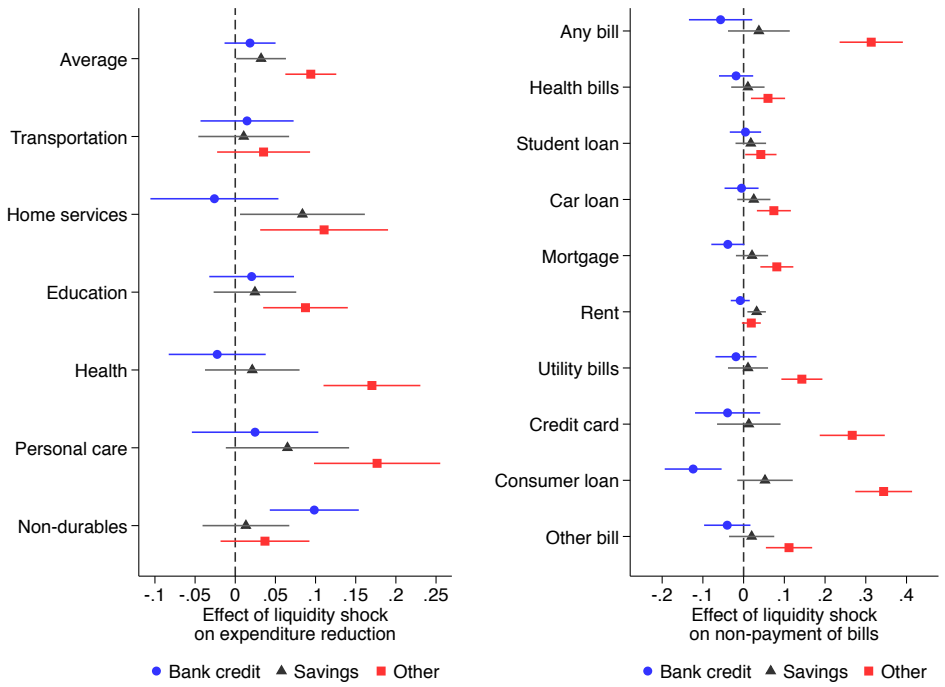
7.1 The Interaction between Market Power and Interest Rate Regulation

The usual motivation for implementing interest rate regulation is to limit usurious behavior, which we define as limiting the exercise of market power by banks. In Section 6.1, we showed that stronger interest rate regulation indeed reduced average bank profit margins while simultaneously increasing rejections and reducing the number of loans and overall welfare in the market. In this section, we study how those results vary under alternative competitive environments.

We study the role of the competitive environment by sequentially merging banks in the market, starting from the baseline market structure with 9 banks until all banks are consolidated into a monopoly.[48] For each such market structures, we simulate equilibrium outcomes for interest rate regulation at November 2013 and November 2015, compute the effects of the policy change, and analyze how those effects change across market structures.

[48] In order to isolate the effect of the number of banks in the market—and to make the ordering of mergers inconsequential for our results—, we remove part of the cost heterogeneity across banks: we set bank fixed effects τ_j and incumbency advantages γr_{ij} to the average across banks.

Figure 7: Survey evidence for effects of reduced credit access on household outcomes



(a) Credit access and household expenses

(b) Credit access and unpaid bills

Notes: This figure displays results from estimating equation (15) for outcomes related to household consumption and financial distress on indicators for whether a household suffered economic hardships and dealt with the using bank credit (blue), liquidating savings or assets (gray), or in some other way (red). Panel (a) display results reductions in household expenditure. Panel (b) display results for unpaid bills. Markers indicate coefficients. Lines indicate 95% confidence intervals.

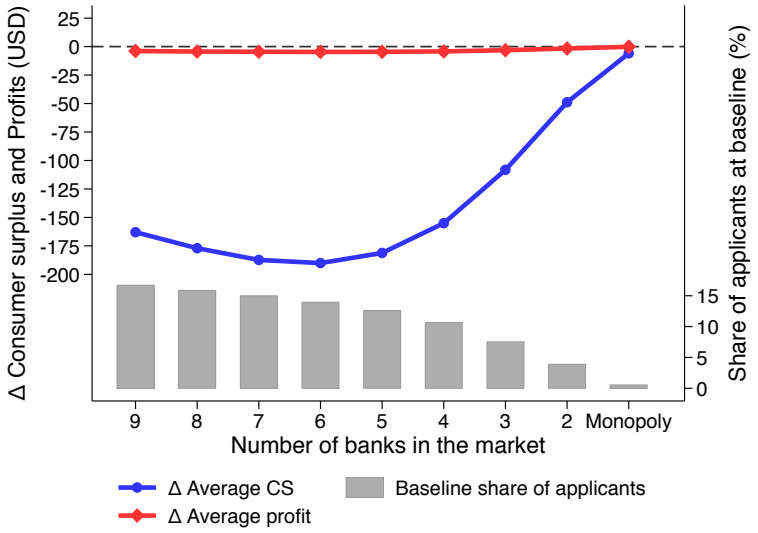
Market power plays a relevant role in determining the equilibrium effects of interest rate regulation. The main results from this analysis are displayed in Figure 8, which shows the effect of market concentration on equilibrium outcomes for a given interest rate regulation level.[49] We find that as the number of banks decreases and the credit market becomes more concentrated, the effect of stronger interest rate regulation on expected consumer surplus decreases. This result suggests that when banks have more market power and therefore can charge higher prices conditional on borrower cost, interest rate regulation might be able to play a role in constraining the exercise of such market power by banks, shifting rents from banks to borrowers. However, our results show that even under a market structure with a monopoly in the market, both expected consumer surplus and bank profits decrease under stronger interest rate regulation, such that there would not be any efficiency grounds for such a change in interest rate regulation in this market.

In competitive credit markets where banks do not have substantial market power, the trade-off between exclusion and protection becomes less appealing, as profit margins are already low. In such settings, interest rate caps will mainly have credit access rather than consumer protection effects. In contrast, welfare losses introduced by interest rate caps will be lower in less competitive environments. In fact, considering that our

[49]For reference, we display equilibrium outcomes for relevant variables under baseline interest rate regulation in Figure A.24 As expected, quantities decrease as the market becomes more concentrated.

theoretical predictions regarding effects of interest rate regulation on applications and expected consumer surplus are ambiguous, it might be the case that interest rate regulation can deliver welfare increases in other settings, and that may be more likely whenever banks hold substantial market power.

Figure 8: The effects of interest rate regulation under different market structures



Notes: This figure display the effects of interest rate regulation on average consumer surplus (blue) and average profits (red) under different market structures, as measured by the left y-axis. We start with the baseline market structure of 9 banks, and sequentially merge banks until a scenario in which the market is served by a monopoly, as indicated by the x-axis. Each line displays the effect of the full policy on the outcome, for each market structure. Additionally, gray bars display the share of applicants under each market structure, as measured by the right y-axis.

7.2 Risk-Based Interest Rate Caps

Despite the trade-off between consumer protection and credit access, innovation in the design of interest rate regulation has been scant. We argue that the cause of such a trade-off is partly in the mismatch between unsophisticated regulation in the form of constant interest rate caps and sophisticated risk pricing by banks. Risk-based interest rate regulation intends to account for borrower risk heterogeneity and risk pricing.

We consider a counterfactual design that sets interest rate caps differently according to borrowers' attributes. Perfect risk-based interest rate regulation would involve setting interest caps at the cost of each borrower for the most efficient bank. Such regulation would yield efficient market outcomes by fully constraining the exercise of market power by banks. In particular, average welfare would be $164 higher than under the regulation in place in November 2015, which would stem from the average monthly payment being 17% lower and the number of loans in the market being 65% higher. However, this policy design is hardly feasible, as it would require perfect knowledge of cost by the regulator. Instead, we work on a feasible version of risk-based interest rate regulation, with the broad structure of the current design.

Using the notation of equation (1), interest rate caps can be written as $\bar{\iota}_{\ell t} = \bar{\iota}^r(\tilde{\iota}_{\ell t-1}; \psi, \alpha_{\ell t})$, i.e. as a function of a reference interest rate, a parameter that operates as a multiplier on that rate (ψ) and a parameter that

operates as a mark-up ($\alpha_{\ell t}$). We consider a case in which instead $\bar{\iota}^r_{\ell t} = \bar{\iota}(\bar{\iota}_{\ell t-1}, x_{i\ell t}; \psi, \alpha_{\ell t}, \phi)$. If $x_{i\ell t}$ is some measure of risk and $\frac{\partial \bar{\iota}^r}{\partial x_{i\ell t}} > 0$, then this design sets a higher interest rate cap to observably riskier borrowers. We adopt a simple linear example of it and measure its performance relative to the design currently in place. In particular, let:

$$\bar{\iota}^r_{\ell t} = \bar{\iota}_{\ell t} + f(x_{i\ell t}), \quad f(x_{i\ell t}) = \phi \frac{x_{i\ell t} - \bar{x}_{\ell t}}{\bar{x}_{\ell t}}$$

be the risk-based interest rate cap for borrower i with risk score $x_{i\ell t}$, where $\bar{x}_{\ell t}$ is the average risk score and ϕ controls the incidence of risk in interest rate caps. Note that the average level of regulation in the market is the same as under the baseline design for each loan-size bracket p, given $E[f(x_{i\ell t})] = 0$, but the interest rate cap is higher (lower) for riskier (safer) borrowers.

Risk-based interest rate caps recover part of the losses in credit access and welfare imposed by constant interest rate caps. We set November 2015 as the reference level of regulation, once the policy change is fully in place. We simulate outcomes for a range of values for ϕ between 0 and 14. Figure 9-a shows that there is a range of values of ϕ for which risk-based interest rate caps increase the number of loans in the market relative to constant interest rate caps. Figure 9-b shows a similar pattern for expected consumer surplus and welfare. At its best specification, risk-based interest rate caps increase the number of loans and average expected consumer surplus in the market by almost 2% and \$22 respectively, while average profits per consumer remain constant.[50]

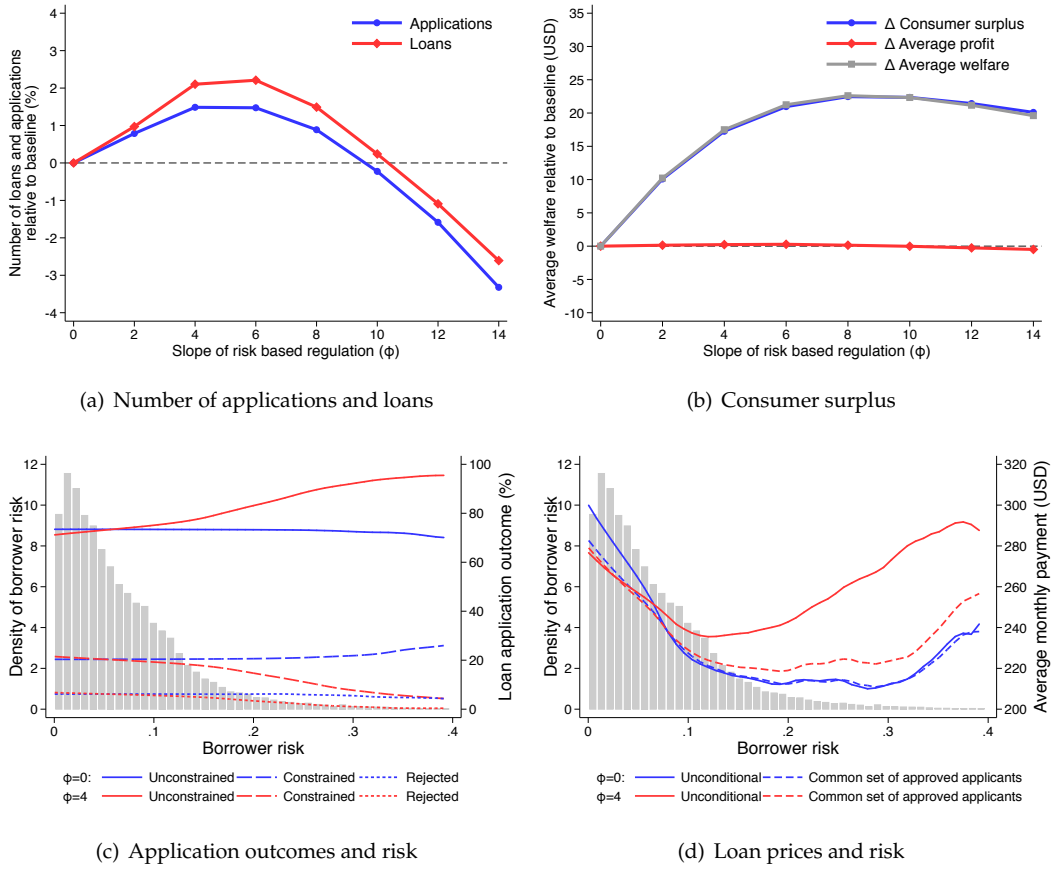
From a welfare perspective, these results suggest that risk-based interest rate caps may manage the trade-off between consumer protection and credit access better than constant interest rate caps. This result stems from the fact that banks implement risk pricing. In absence of risk pricing, adverse effects of risk-based caps on safe borrowers may actually be larger than under constant caps. The case we analyze here is, of course, an example. Other variants of risk-based interest rate regulation could further improve market outcomes relative to designs that do not account for risk.

8 Conclusion

Interest rate regulation is widespread in consumer credit markets and has been utilized for a long time, but there is disagreement about its effects. Moreover, its design often lacks sophistication, which may lead to unintended consequences. In this paper, we provide evidence of the effects of interest rate caps on market outcomes and welfare, using the Chilean credit market as a setting. We find that the trade-off between consumer protection and credit access exists, but that adverse effects on credit access dominate consumer protection effects. Thus, while the objective of interest rate regulation is often to protect borrowers from bank market power, we find it ends up mostly harming borrowers' access to credit.

[50] To further illustrate the effects of risk-based interest rate caps, we study patterns of heterogeneity across borrower risk it induces. We compare the case of $\phi = 4$ with the baseline case of $\phi = 0$. Figure 9-c shows that it affects application outcomes by increasing approval rates for risky borrowers, while slightly decreasing approval rates for safe borrowers. On the other hand, Figure 9-d shows that average monthly payments increase (decrease) for risky (safe) borrowers as they face relatively weaker (stronger) interest rate regulation. How this heterogeneity across borrower risk aggregates depends on the joint distribution of borrower demand, risk and cost. The fact that our estimates imply that risky borrowers value loans more than safe borrowers explains that the relative benefits from risk-based interest rates are stronger in terms of expected consumer surplus than in terms of loans: as interest rate caps become more aggressive, the benefits in terms of limiting losses in credit access diminish, but the fact that the policy increases the share of risky borrowers in the market implies that it still increases average expected consumer surplus.

Figure 9: Risk-based interest rate caps



(a) Number of applications and loans

(b) Consumer surplus

(c) Application outcomes and risk

(d) Loan prices and risk

Notes: These figures display simulated market outcomes under different levels of risk-based interest rate regulation, as characterized by the parameter ϕ. Baseline outcomes for $\phi = 0$ correspond to simulated equilibrium outcomes for November 2015 under the baseline regulation design. See Section 7.2 for details. Panel (a) displays changes in number of loans relative to the baseline level, while Panel (b) displays changes in consumer surplus. Panel (c) displays a local polynomial fit of application outcomes over borrower risk, along with a histogram of borrower risk in the background. Panel (d) displays a local polynomial fit of loan monthly payments over borrower risk, along with a histogram of borrower risk in the background. Solid lines provide results for approved loans under each regulation, whereas dashed lines provide results for the common set of approved loans under both regulation regimes.

We develop and estimate a model of demand and supply for consumer loans, which we exploit in a variety of ways. First, we use it to estimate welfare effects of interest rate regulation and find that welfare mostly decreased in our setting. Second, we use the model to show that the adverse effects of interest rate regulation are smaller in more concentrated markets as the consumer protection motive becomes more relevant, but that welfare decreases even under a monopoly. Finally, we explore how equilibrium outcomes differ under risk-based interest rate caps, and find that such design reduces adverse effects of interest rate regulation and recovers at least part of the losses in terms of credit access and consumer welfare, without increasing bank profits. This result suggests that this design may perform better in terms of providing consumer protection without harming credit access.

Our welfare analysis follows a revealed preferences approach, and does not account for any behavioral biases that might take place in consumer credit markets (Zinman, 2015; Beshears et al., 2018). In our

approach, we exploit consumer application and repayment behavior to estimate our model and estimate welfare effects. We acknowledge that such behavioral biases might affect our conclusions regarding the welfare implications of interest rate regulation, and consider it a relevant line for future research. However, evidence from our survey suggests that households that access bank credit upon economic hardships display a higher degree of consumption smoothing and a lower degree of financial distress. This complementary evidence provides support to our findings that does not rely on revealed preferences.

Our analysis shows how a combination of a theoretical framework and data can inform the design of regulation for consumer credit markets, by identifying relevant economic forces at work, and by measuring its implications and their relationship to relevant features of credit markets. Importantly, while our findings show mostly adverse effects of interest rate regulation in our setting, the theoretical predictions of our model regarding its welfare effects are ambiguous. This implies that interest rate regulation might improve market outcomes in other settings with different underlying market and demand structures. However, the fact that most of the related literature points towards adverse or non-existent effects of interest rate regulation on market outcomes suggests such a setting might be uncommon.

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Price Regulation in Credit Markets: A Trade-off between Consumer Protection and Credit Access

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A Robustness Exercises and Additional Results

The empirical strategy developed in Section 3.2.2 exploits policy variation across time and loan-size brackets to measure the effect of interest rate caps on credit market outcomes. The main concern with this strategy is the potential for equilibrium effects on loans larger than $8,000, which are not directly treated by the policy change, but might be affected through some form of substitution along that margin. In this section, we implement different robustness exercises that provide support to our empirical strategy. Moreover, we include some additional results.

A.1 Effects of Placebo Policies

Our approach in Section 3.2.2 relies on a comparison across treated and untreated loan-size brackets. One should not expect to find the same estimated effects across different comparison groups within untreated loan size brackets. We study whether that is the case, by estimating equation (4) for placebo policies. Concretely, we use the same definition for the policy and the same policy intensity variables as in Section 3.2.2 to estimate effects of interest rate regulation on different parts of the loan size distribution. In practice, we proceed by replacing the dependent variable y_{krt} to $y_{k+\Delta,rt}$, where Δ defines the placebo policy. We start by policy size brackets defined as being $8,000 higher than actual ones, and then sequentially increase them by $2,000 to generate a range of placebo policies.

Figure A.9 displays the results from this exercise for price and quantity outcomes. Each figure displays our main estimates from Table 3 and Table 4, along with estimates for a range of placebo policies. Figures A.9-a and A.9-b display results for maximum and average interest rates, and the results are stark: estimates from placebo policies are remarkably different from our estimates and close to zero. Figures A.9-c and A.9-d display results for quantity outcomes, for which placebo estimates are noisier but offer a similar pattern: most of point estimates are close to zero and not statistically different to zero. These results provide evidence against particular patterns of substitution from untreated loan size brackets to treated loan size brackets.

A.2 Effects on Distribution of Application Loan Size and Term

An additional robustness exercise we implement is to study the evolution of the distribution of application amount and term. If there substitution across policy size brackets, that should reflected in a change in the distribution of application loan amount. Figure A.10-a shows the evolution of relevant statistics of the distribution of application loan amount separately for loans smaller than $8,000 and loans larger than $8,000. We find no evidence of changes in this distribution around the date of the policy change we study. Moreover, the same is true for application loan term, as displayed by Figure A.10-b.

A.3 Effects around Policy Thresholds for Loan Size

The approach proposed in Section 3.2.2 exploits loans larger than $8,000 as a control group for evaluating the effect of the policy. One concern regarding that is that, in response to the change in relative interest rate regulation between loans under and above that threshold, there could be equilibrium effects affecting loans

larger than $8,000 despite them not being directly treated by the reform we study. For example, one could argue that, before the reform, borrowers could have sought to get loans right above the $8,000 threshold to benefit from lower interest rate caps on loans in that loan-size bracket than on loans of amounts marginally below such threshold. That incentive would be reduced by the policy, given interest rate caps for both groups were brought closer by it. In such case, we should observe bunching at that threshold from above before the policy, and a decrease in such behavior after it. That in turn would imply that our results in Section 3.2.2 would underestimate the effects of the reform. On the other hand, banks may have opposite incentives to induce borrowers to take marginally smaller loans on the left side of the policy threshold or to take multiple small loan instead of a large one, for which we already provided evidence against in Figure A.8. This incentive would also decrease under stronger regulation.

In order to address this concern about substitution around the threshold, we study the distribution of the number of loans around relevant policy thresholds. Figure A.11 displays the number of loan originations at loan sizes around relevant policy thresholds, before and after the policy. As displayed by Figure A.11-a, The relationship between the density of loan size below the $2,000 threshold is remarkably noisy—this pattern is driven by mass points in the loan size distribution that are observed at certain round number for loan size—, which makes it difficult to conclude anything. However, above the $2,000 threshold, there is no noticeable change in such density before and after the policy. A similar comparison is displayed by Figure A.11-b for the $8,000 policy threshold. While the distribution of the number of loans shows more mass around the policy threshold after the policy, that behavior is similar on both sides of threshold.[1]

In a more systematic attempt to address this concern, we repeat the analysis in Section 3.2.2 dropping loans around the $8,000 policy threshold. Table A.2 displays results from estimating equation (4) excluding loans of size between $6,000 and $10,000 from the sample. Estimates are quantitatively similar to those obtained with the full sample in Section 3.2.2, which is reassuring in terms of our empirical strategy. Finally, Figure A.12 repeats this exercise for price and quantity outcomes for a variety of comparison groups which differ in their lower bound, and provides evidence in the same direction: estimates for the effects of the policy do not change substantially when excluding loans close to the policy threshold from the comparison group.

A.4 Alternative Comparison Groups

Our analysis in Section 3.2.2 exploits loans between $8,000 and $20,000 as a comparison group for those directly affected by the policy change. In this subsection, we assess how would our estimates change under alternative definitions of compares groups. In particular, we estimate the same specification as in (4) but for variety of comparison groups, starting with loans between $8,000 and $10,000, and then increasing sequential by $2,000 until a group covering loans between $8,000 and $30,000.

Results from this exercise are displayed in Figure A.13, and show results for price and quantity outcomes. Each figure displays our main estimates from Table 3 and Table 4, along with estimates for a range of

[1]Figures A.11-c and A.11-d complement this analysis by showing that average interest rates shift downwards after the policy, but that there is no discontinuity in average interest rates around policy thresholds. We should mention that when looking at high enough percentiles in the distribution of loan interest, discontinuities at the policy thresholds become evident, which is consistent with bunching at the interest rate cap displayed in Figure 2.

alternative comparison groups. Overall, the main conclusions of our main analysis are unaffected, as point estimates do not change substantially across comparison groups. On the other hand, there are some efficiency gains from using larger comparison groups, which reflects in tighter standard errors.

A.5 Heterogeneity across Banks

We have focused so far on the effects of interest rate regulation at the market level. In this subsection, we provide results for heterogeneity in effects across banks. Figure A.14 provides results for marginal effects of interest rate regulation on both number of lines and prices for each of the 8 largest banks in the market. While there heterogeneity in magnitudes, our estimates suggest that the stronger interest rate regulation affects most banks in the same direction, by inducing them to sign less loan contracts and to do so at lower interest rates. This is consistent with our estimates for average effects and with the interpretation we give to them.

Interestingly, there is 1 bank that displays a different behavior, by reducing average interest but simultaneously increasing credit volume as a result of stronger interest rate regulation. Those estimates suggest that either borrowers substituted towards that bank which perhaps had a more lenient screening process or that the bank changed its screening process as a result of the policy change.

B Estimation Details

B.1 Preliminary Steps in Estimation of Application Equation

We discuss joint estimation of the application and repayment equations in (11) and (12) in Section 5.3. Estimation proceeds in three steps, of which the first two are related to estimation of components of the application equation in equation (11) that are not observed for every consumer in the sample, and that we then use as inputs in estimation of the key parameters in that equation by maximum likelihood. We provide further detail about those steps in this section.

In the first step, we estimate the conditional distribution of loan amount and term (L_i, T_i) using data from applicants and then draw from that distribution for non-applicants. In the first stage, we estimate a probit model for applications on a rich vector of borrower covariates x_i that includes the level and change of consumer and mortgage debt and default, income, consumer and mortgage debt to income ratio, age and gender; and application shifters z_i that include the total number of branches across banks in the consumer local market and the lagged number of related banks:

$$P(a_i = 1) = \Phi(x_i'\beta_a + z_i'\gamma_a)$$

which we estimate this model on data for the period before the policy change in December 2013.

In the second step, we estimate a regression of loan amount on the same vector of borrower covariates, and include fitted propensity scores $\hat{P}(x_i, z_i)$ as a control function to account for selection into application. This procedure is based on Das et al. (2003) and also used by Attanasio et al. (2008) for studying loan

demand. In particular, we estimate:

$$\ln(L_i) = x_i'\beta_L + \lambda(\hat{P}(x_i, z_i)) + \epsilon_i$$

where λ is the control function.

In the third step, we estimate an ordered logit model for loan term monthly bins on borrower covariates and loan size:

$$\begin{aligned} P(T_i = j) &= P(\alpha_{j-1} < x_i'\beta_T + \delta_T L_i \leq \alpha_j) \\ &= \Lambda(\alpha_j - (x_i'\beta_T + \delta_T L_i)) - \Lambda(\alpha_j - (x_i'\beta_T + \delta_T L_i)) \end{aligned}$$

where α_{j-1} and α_j are the cutoffs that define loan term monthly bin j. The advantage of using an ordered logit model in this step is that it accommodates the fact that the empirical distribution of loan term features noticeable spikes at multiples of semesters. Using estimates from these regressions, we first take draws of loan amount for consumers that did not apply for loans $\tilde{L}_i$, conditional on x_i; and then take draws of loan term for consumers that did not apply for loans $\tilde{T}_i$, conditional on x_i and L_i.

In the second step, we estimate the approval probability P_C and the density of loan prices conditional on approval $f_{p|C}$. We estimate P_C using a probit model for an indicator of application approval on a vector of borrower covariates, previous relationship variables, as well as application amount and term from the first step and month fixed and market effects:

$$P(C_i = 1) = \Phi(x_i'\eta + \zeta_t + \tau_m)$$

which we estimate separately for low- and high-risk borrower to allow for flexibility. We include time and market fixed effects in order to accommodate the possibility that approval probabilities change over time and across markets due to the policy change. We compute expected approval probabilities $\hat{P}_{Ci}$ as the fitted values from this equation for each consumer in the sample. Then, we estimate the density of loan monthly payments conditional on approval $f_{p|C}$, using a kernel density estimator after conditioning on the same vector of variables. We then take draws from this estimated conditional density for estimation of borrower preferences by maximum likelihood below.

B.2 Likelihood Function for Application and Repayment

The parameters of interest in the application equation are those in v_{Ci}, δ_p and κ_i, whereas the parameters of interest in the repayment equation are α_S. Moreover, we are interested in the parameters in the joint distribution of application and repayment shocks, namely ρ and σ_S. Recall that σ_A is normalized to 1.

We start by the likelihoods of application choices. Given the normality assumption we impose on ε_{Ai},

the probabilities that a potential borrower chooses to apply and not to apply are:

$$\begin{aligned}
P_{a_i=1} &= \Pr\left(P_{Ci}\int(x_i'\delta_X+\delta_L L_i+\delta_T T_i-\delta_p p)f_{p|C}(p)dp-z_i'\kappa+\varepsilon_{Ai}>0\right)\\
&= \Phi\left(P_{Ci}\int(x_i'\delta_X+\delta_L L_i+\delta_T T_i-\delta_p p)f_{p|C}(p)dp-z_i'\kappa\right)\\
P_{a_i=0} &= \Pr\left(P_{Ci}\int(x_i'\delta_X+\delta_L L_i+\delta_T T_i-\delta_p p)f_{p|C}(p)dp-z_i'\kappa+\varepsilon_{Ai}<0\right)\\
&= \Phi\left(-P_{Ci}\int(x_i'\delta_X+\delta_L L_i+\delta_T T_i-\delta_p p)f_{p|C}(p)dp+z_i'\kappa\right)
\end{aligned}$$

where Φ is the standard normal cdf. We approximate the integral through simulation by taking $N_A^S = 100$ Halton draws from the estimated conditional density of loan monthly payments conditional on approval, which is:

$$\int(x_i'\delta_X+\delta_L L_i+\delta_T T_i-\delta_p p)f_{p|C}(p)dp \approx \frac{\sum_{s=1}^{N_A^S}(x_i'\delta_X+\delta_L L_i+\delta_T T_i-\delta_p p_i^{(s)})1\{L_i^{(s)}=1\}}{\sum_{s=1}^{N_A^S}1\{L_i^{(s)}=1\}}$$

where $L_i^{(s)}$ is an indicator for whether a loan application by borrower i in simulation draw s was approved.

We now derive the likelihoods of repayment outcomes. The likelihood of observing a given repayment behavior can be written in terms of the distribution of ε_{Si} conditional on application, for which we exploit the properties of conditional normal distributions. There are three cases of interest, one in which borrower i fully repays, one in which borrower i partially repays, and one in which borrower i does not repay at all. The probabilities for these three cases are:

$$\begin{aligned}
P_{S=0|a_i=1} &= \Pr(\exp(x_i'\alpha_S+\varepsilon_{Si})\leq\chi|a_i=1)\\
&= \int_{-P_{Ci}\int(x_i'\delta_X+\delta_L L_i+\delta_T T_i-\delta_p p)f_{p|C}(p)dp+z_i'\kappa}^{\infty}\Phi\left(\frac{\ln(\chi)-x_i'\alpha_S-\mu_{S|Ai}}{\sigma_{S|A}}\right)f_{\varepsilon_A}(\varepsilon_A)d\varepsilon_A\\
P_{S=s_i|a_i=1} &= \Pr(\exp(x_i'\alpha_S+\varepsilon_{Si})=s_i|a_i=1)\\
&= \int_{-P_{Ci}\int(x_i'\delta_X+\delta_L L_i+\delta_T T_i-\delta_p p)f_{p|C}(p)dp+z_i'\kappa}^{\infty}\frac{1}{\sigma_{S|A}}\phi\left(\frac{\ln(s_i)-x_i'\alpha_S-\mu_{S|Ai}}{\sigma_{S|A}}\right)f_{\varepsilon_A}(\varepsilon_A)d\varepsilon_A\\
P_{S=1|a_i=1} &= \Pr(\exp(x_i'\alpha_S+\varepsilon_{Si})\geq 1|a_i=1)\\
&= \int_{-P_{Ci}\int(x_i'\delta_X+\delta_L L_i+\delta_T T_i-\delta_p p)f_{p|C}(p)dp+z_i'\kappa}^{\infty}\Phi\left(\frac{x_i'\alpha_S+\mu_{S|Ai}}{\sigma_{S|A}}\right)f_{\varepsilon_A}(\varepsilon_A)d\varepsilon_A
\end{aligned}$$

where $\chi=\frac{1}{T_i}\approx 0$ is the repayment share achieved after the first payment on the contract, and ϕ is the standard normal pdf. Given the joint normality assumption of $(\varepsilon_{Ai},\varepsilon_{Si})$, the conditional distribution of ε_S is given by:

$$\varepsilon_S|\varepsilon_A \sim N\left(\frac{\rho\sigma_S}{\sigma_A}\varepsilon_A,\sigma_S^2(1-\rho^2)\right)$$

which we use for computing the integral in each likelihood by simulation. In particular, we take 100 Halton draws $\varepsilon_{Ai}^{(s)}$ from its truncated marginal distribution, and then compute the conditional mean of ε_{Si} for each draw, $\mu_{S|Ai}^{(s)}=\frac{\rho\sigma_S}{\sigma_A}\varepsilon_{Ai}^{(s)}$.

The likelihood of an observation varies across five observed combinations of application and repayment

events, which we indicate using I_i^1 through I_i^5. The log likelihood of the data is:

$$\begin{aligned}\log \mathcal{L}^D &= \frac{1}{N}\sum_i I_{1i}\log P_{a_i=0} + I_{2i}\log P_{a_i=1} + I_{3i}[\log P_{a_i=1} + \log P_{S=0|a_i=1}] \\ &\quad + I_{4i}[\log P_{a_i=1} + \log P_{S=s_i|a_i=1}] + I_{5i}[\log P_{a_i=1} + \log P_{S=1|a_i=1}]\end{aligned}$$

where I_{1i} indicates non-applicates, I_{2i} indicates rejected applicants, I_{3i} indicates applicants that do not repay at all, I_{4i} indicates applicants that only partially repay, and finally I_{5i} indicates applicants that fully repay. This log likelihood can can be written as:

$$\begin{aligned}\log \mathcal{L}^D &= \frac{1}{N}\sum_i I_{1i}\log P_{a_i=0} + (I_{2i}+I_{3i}+I_{4i}+I_{5i})\log P_{a_i=1} \\ &\quad + I_{3i}\left[\log \int_{-P_{Ci}\int(x_i'\delta_X+\delta_L L_i+\delta_T T_i-\delta_p p)f_{p|C}(p)dp+z_i'\kappa}^{\infty} F_{\varepsilon_D|\varepsilon_A}(\ln(\chi)-x_i'\alpha_S|\varepsilon_A)f_{\varepsilon_A}(\varepsilon_A)d\varepsilon_A\right] \\ &\quad + I_{4i}\left[\log \int_{-P_{Ci}\int(x_i'\delta_X+\delta_L L_i+\delta_T T_i-\delta_p p)f_{p|C}(p)dp+z_i'\kappa}^{\infty} f_{\varepsilon_D|\varepsilon_A}(\ln(s_i)-x_i'\alpha_S|\varepsilon_A)f_{\varepsilon_A}(\varepsilon_A)d\varepsilon_A\right] \\ &\quad + I_{5i}\left[\log \int_{-P_{Ci}\int(x_i'\delta_X+\delta_L L_i+\delta_T T_i-\delta_p p)f_{p|C}(p)dp+z_i'\kappa}^{\infty} F_{\varepsilon_D|\varepsilon_A}(x_i'\alpha_S|\varepsilon_A)f_{\varepsilon_A}(\varepsilon_A)d\varepsilon_A\right]\end{aligned}$$

We maximize this log-likelihood to estimate the parameters of interest in the application and repayment equations.

B.3 Likelihood Function for Banks' Costs

The parameters of interest on the supply side are $\{\tau, \gamma, \sigma_\omega\}$. The likelihood function for prices and loan application outcomes can be obtained using results for the distributions of order statistics of the T1EV distribution assumed for ω_{ij}, as detailed in Appendix B.4. We work separately on the corresponding likelihood for each of the three potential outcomes generated by the model and stated in equation (9). In terms of notation, we employ uppercase letters for random variables and lowercase variables for data.

Likelihood for Unconstrained Loans. When regulation is not binding, loan price is the optimal unconstrained price for the lowest cost bank. The likelihood of a contract in such situation, signed at price $p_i \leq \bar{p}_i$ with bank b_i is:

$$\begin{aligned}P(P_i = p_i, P_i < \bar{p}, B_i = b_i|\mathcal{J}_i) &= P(P_i^u = p_i, P_i^u < \bar{p}_i, B_i = b_i|\mathcal{J}_i) \\ &= \left(g_{i(1)}\left(\frac{f_i - \frac{E_\varepsilon[\varphi(S_i)]}{\varphi(T_i)}p_i}{l_i}|\mathcal{J}_{i\setminus j}\right)\right. \\ &\quad \left. + (\rho_{ib_i}(\mathcal{J}_i)-1)g_{i(1)}\left(\frac{f_i - \frac{E_\varepsilon[\varphi(S_i)]}{\varphi(T_i)}p_i}{l_i}|\mathcal{J}_i\right)\right)1(P_i < \bar{p})\end{aligned}$$

where $g_{i(1)}(\omega)$ is the density of the first order statistic of match-value and $\rho_{ib_i}(\mathcal{J}_i)$ is the choice probability of the bank chosen by borrower i.

Likelihood for Constrained Loans. When regulation is binding and a loan is approved, loan price equals the price cap and the lowest cost bank makes profits from the loan. In such a scenario, loan price is less than the optimal unconstrained price but higher than the cost of the lowest cost bank. The likelihood of a contract in such situation, signed at price $p_i = \bar{p}_i$ with bank b_i is:

$$\begin{aligned} P(P_i = \bar{p}_i, B_i = b_i|\mathcal{J}_i) &= P\left(P_i^u > \bar{p}, \frac{\varphi(T_i)}{E_\varepsilon[\varphi(S_i)]}(f_i - l_i\omega_{i(1)}) \leq \bar{p}_i, B_i = b_i|\mathcal{J}_i\right) \\ &= \left(G_{i(1)}\left(\frac{f_i - \frac{E_\varepsilon[\varphi(S_i)]}{\varphi(T_i)}\bar{p}_i}{l_i}|\mathcal{J}_{i\setminus j}\right) + (\rho_{ib_i}(\mathcal{J}_i) - 1)G_{i(1)}\left(\frac{f_i - \frac{E_\varepsilon[\varphi(S_i)]}{\varphi(T_i)}\bar{p}_i}{l_i}|\mathcal{J}_i\right)\right) \\ &\quad \times\left(1 - G_{i(1)}\left(\frac{f_i - \frac{E_\varepsilon[\varphi(S_i)]}{\varphi(T_i)}\bar{p}_i}{l_i}|B_i = b_i, \mathcal{J}_i\right)\right) \end{aligned}$$

where $G_{i(1)}(\omega)$ is the distribution of the first order statistic of match-value.

Likelihood for Rejected Loans. When a loan is rejected, the lowest cost bank does not make any profit out of it. Therefore, the cost of such bank is higher than the price cap on the loan. The likelihood of a contract in such situation is:

$$\begin{aligned} P(P_i = \cdot, B_i = \cdot|\mathcal{J}_i, r_i) &= P\left(\frac{\varphi(T_i)}{E_\varepsilon[\varphi(S_i)]}(f_i - l_i\omega_{i(1)}) > \bar{p}_i|\mathcal{J}_i\right) \\ &= G_{i(1)}\left(\frac{f_i - \frac{E_\varepsilon[\varphi(S_i)]}{\varphi(T_i)}\bar{p}_i}{l_i}|\mathcal{J}_i\right) \end{aligned}$$

Likelihood Function. The likelihood of the data combines the individual likelihoods for these three cases. Let I_i^u, I_i^c and I_i^r indicate that the outcome for application by i was an unconstrained approved loan, a constrained approved loan or a rejected application respectively. The full log-likelihood function for the data is:

$$\begin{aligned} log\mathcal{L}^S &= \sum_{i\in\mathcal{A}} I_i^u \log P(P_i = p_i, P_i < \bar{p}, B_i = b_i|\mathcal{J}_i, x_i) \\ &\quad + I_i^c \log P(P_i = \bar{p}, B_i = b_i|\mathcal{J}_i, x_i) + I_i^r \log P(P_i = \cdot, B_i = \cdot|\mathcal{J}_i, x_i) \end{aligned}$$

We estimate the parameters related to banks' costs by maximizing this log-likelihood function.

B.4 Useful Properties of T1EV Distribution

In this Appendix, we summarize useful properties of the T1EV distribution, which we use in the derivation of the likelihood function. Proofs for these results are available in Froeb et al. (1998). First, it can be shown using the properties of extreme value distributions, that the cdf of the highest utility across banks for a borrower is given by:

$$G_{(1)}(\omega; \mathcal{J}_i) = G(\omega; (\omega_{i,max}, \sigma_\omega), \mathcal{J}_i)$$

where:

$$\omega_{i,max} = \sigma_\omega \log \sum_{j \in \mathcal{J}} \exp\left(\frac{\delta_{ij}}{\sigma_\omega}\right)$$

is the location parameter in the distribution.

Moreover, the probability that j is the bank with the lowest cost for i among those in $\mathcal{J}_i$ is given by the usual logit formula:

$$\rho_{ij} \equiv P(u_{ij} = \max_{k \in \mathcal{J}_i} u_{ik}; \mathcal{J}_i) = \frac{\exp\left(\frac{\delta_{ij}}{\sigma_\omega}\right)}{\sum_{k \in \mathcal{J}} \exp\left(\frac{\delta_{ik}}{\sigma_\omega}\right)}$$

Finally, we can also derive an analytical expression for the distribution of the second order statistic of ω_{ij} in terms of that of the first order statistic. Conditional on j being the choice of borrower i, such distribution would be:

$$G_{(2)}(\omega | u_{ij} = \max_{k \in \mathcal{J}_i} u_{ik}; \mathcal{J}_i) = \frac{1}{\rho_{ij}} G_{(1)}(\omega; \mathcal{J}_{i \setminus j}) + \frac{\rho_{ij} - 1}{\rho_{ij}} G_{(1)}(\omega; \mathcal{J}_i)$$

which by integrating over j to recover the unconditional distribution of the second order statistic yields:

$$\begin{aligned} G_{(2)}(\omega; \mathcal{J}_i) &= \sum_{j \in \mathcal{J}_i} \rho_{ij} G_{(2)}(\omega | u_{ij} = \max_{k \in \mathcal{J}} u_{ik}; \mathcal{J}_i) \\ &= \sum_{j \in \mathcal{J}_i} G_{(1)}(\omega; \mathcal{J}_{i \setminus j}) + G_{(1)}(\omega; \mathcal{J}_i)(1 - |\mathcal{J}_i|) \end{aligned}$$

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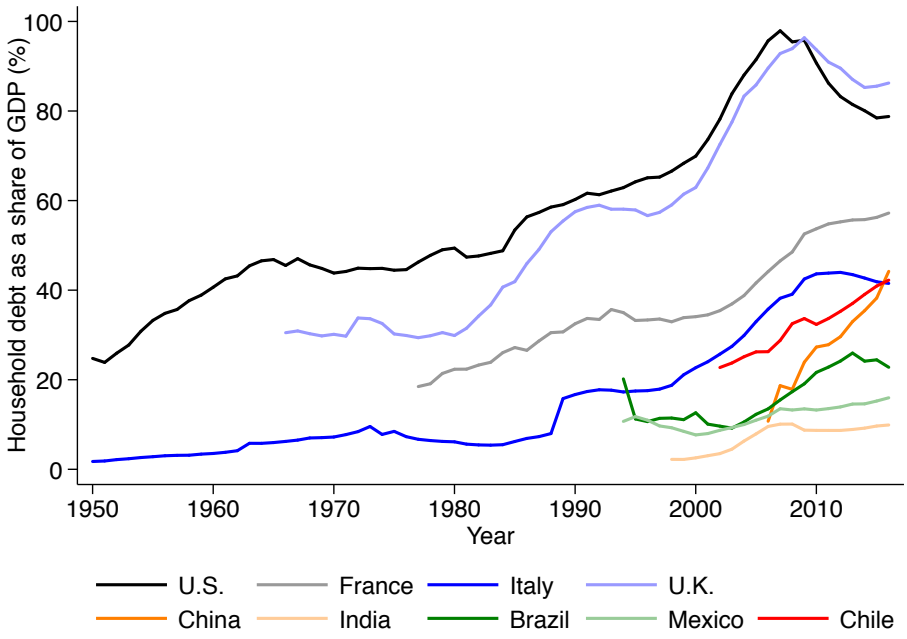
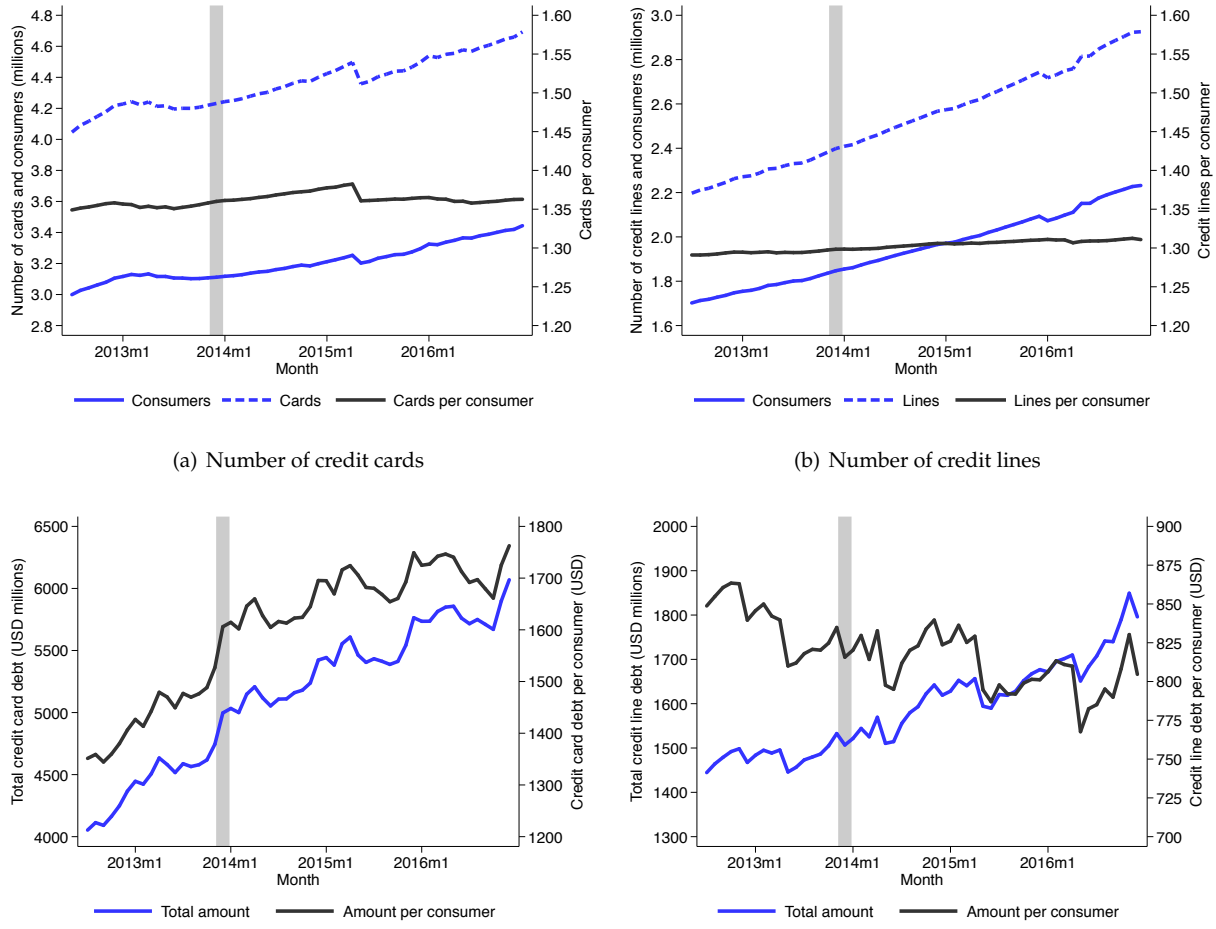
Figure A.1: Evolution of household debt as a share of GDP across time and countries



Notes: This figure displays the evolution of household debt as a share of GDP for a sample of countries. Authors' calculation based on based on data from the Global Debt Database by the International Monetary Fund (Mbaye et al., 2018). The length of each series is determined by the availability of data for each country. There is substantial cross sectional variation. For 2016, household debt as a share of GDP ranges from 0.6% for Afghanistan to 126.3% for Switzerland, with an average of 39.7%.

Figure A.2: Evolution of credit card and credit line debt



(a) Number of credit cards

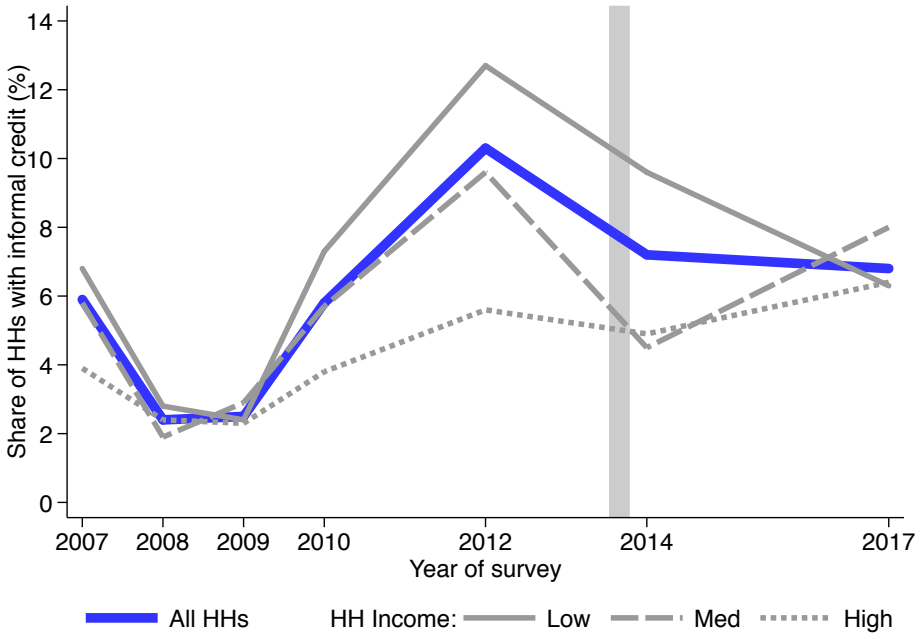
(b) Number of credit lines

(c) Amount of credit card debt

(d) Amount of credit line debt

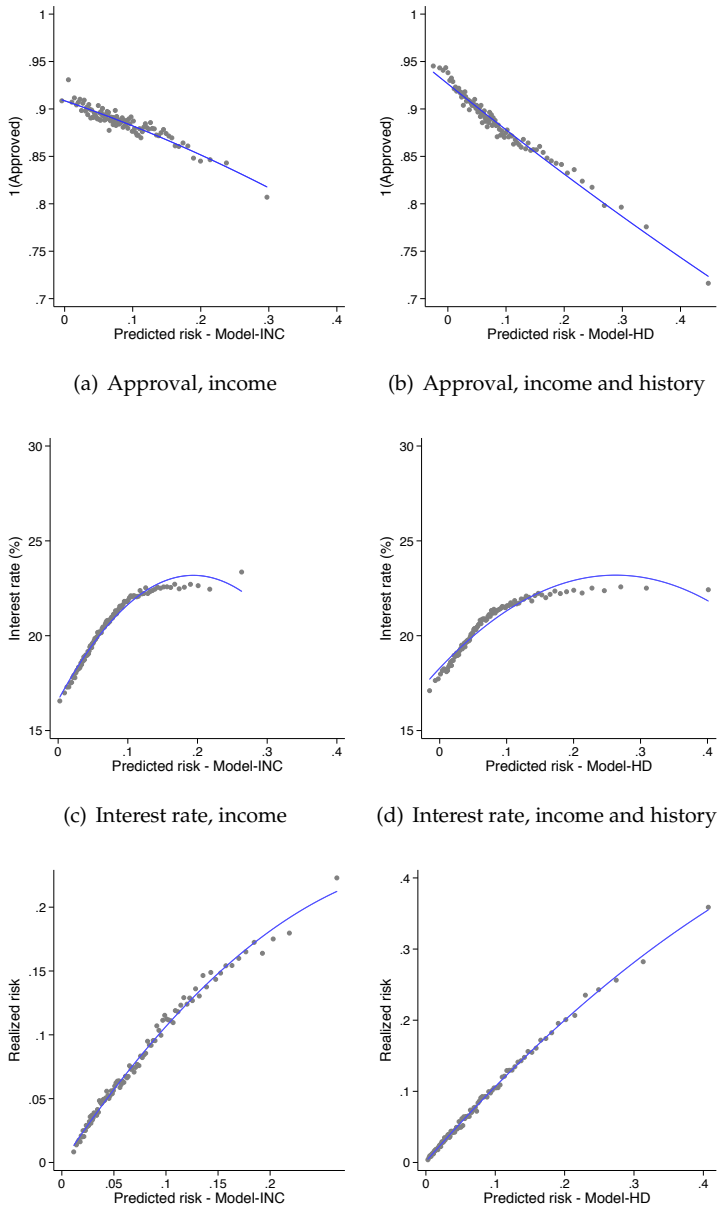
Notes: This figure displays the evolution of the the markets for credit cards and credit lines in the Chilean market using administrative data from CMF. Panels (a) and (b) display the number of consumers and cards/lines (blue) as well as the number of cards/lines per consumer in the market (black). Panels (c) and (d) display the total amount of credit card/line debt in the market (blue) and per consumer in the market (black). The vertical gray line indicates the policy change.

Figure A.3: Evolution of informal debt penetration



Notes: This figure displays the evolution of the share of households in the Chilean market that hold some kind of informal debt, as measured by the nationally representative Survey of Consumer Finance (EFH, 2018). This statistic covers several sources of informal credit, including family and friends, informal lenders, pawn shops, among others. Data is only available for selected years displayed in the x-axis, which are the ones in which the survey has been implemented. The blue line measures the overall share, whereas gray lines measure shares by terciles of household income. The vertical gray line indicates the policy change.

Figure A.4: Predicted and realized default



(a) Approval, income

(b) Approval, income and history

(c) Interest rate, income

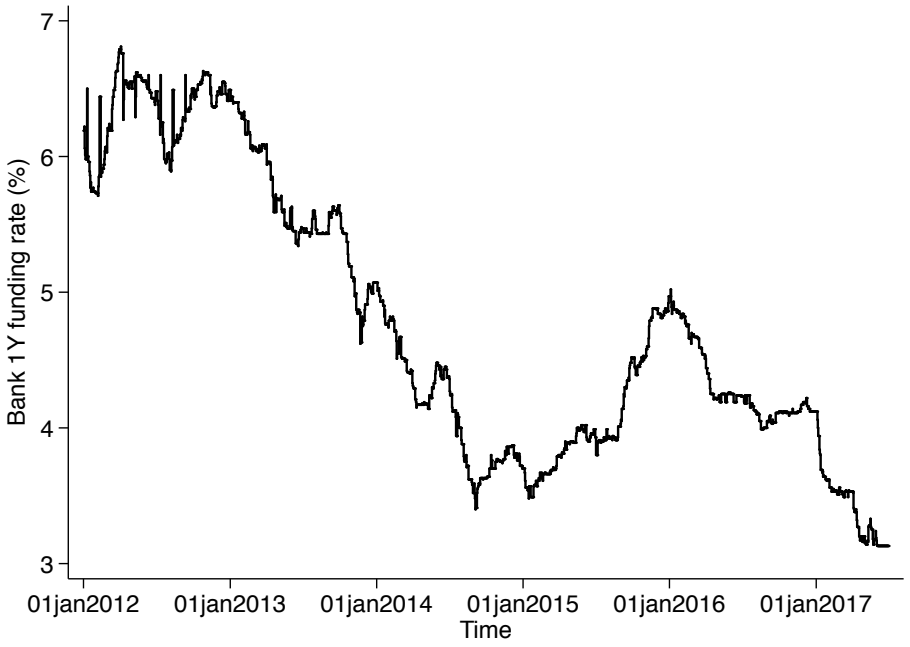
(d) Interest rate, income and history

(e) Realized default, income

(f) Realized default, income and history

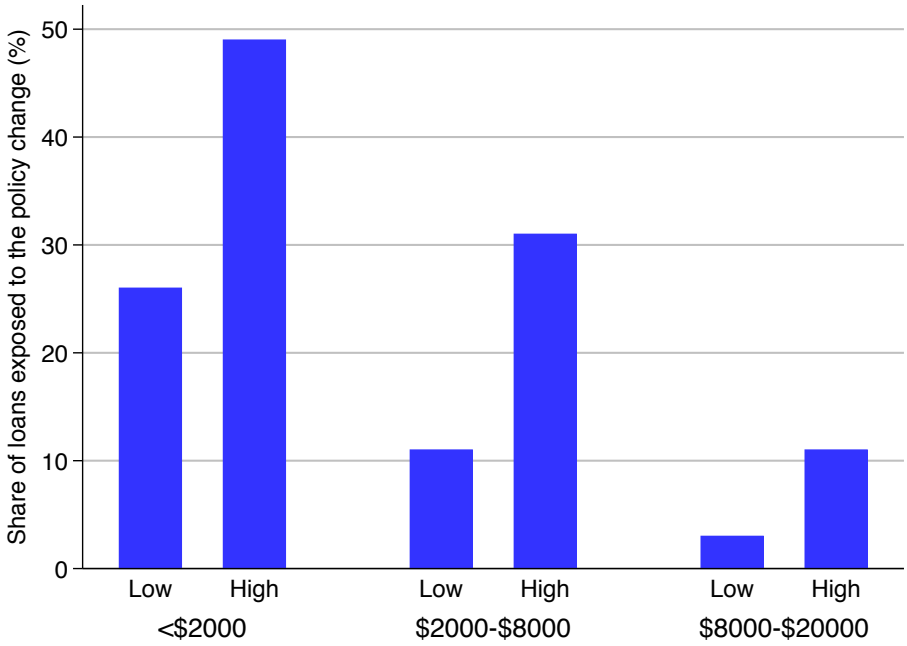
Notes: This figure displays binned scatterplot of predicted loan default probability as constructed using the model described in Section 2.2 and realized outcomes. The left column displays results using borrower income and loan to income ratios as main predictors of default, while the right columns adds a long vector of credit history covariates. Panels (a) and (b) display the relationship between predicted default and loan application approval; Panels (c) and (d) display the relationship between predicted default and interest rate; while Panels (e) and (f) display the relationship between predicted default and realized default,. Each dot measures average realized default for loans in each of 100 quantiles of predicted default. The blue line is a quadratic fit of the relationship between both variables.

Figure A.5: Evolution of the funding cost of banks



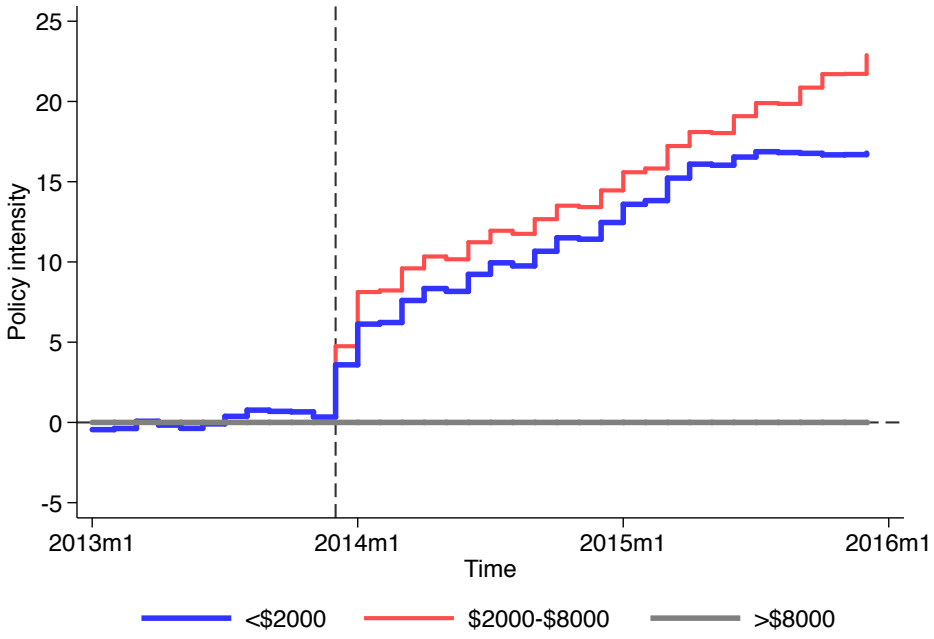
Notes: This figure displays the evolution of the funding cost of banks. This funding cost is calculated as a weighted average of banks deposit rates.

Figure A.6: Exposure to interest rate regulation by loan size and borrower risk



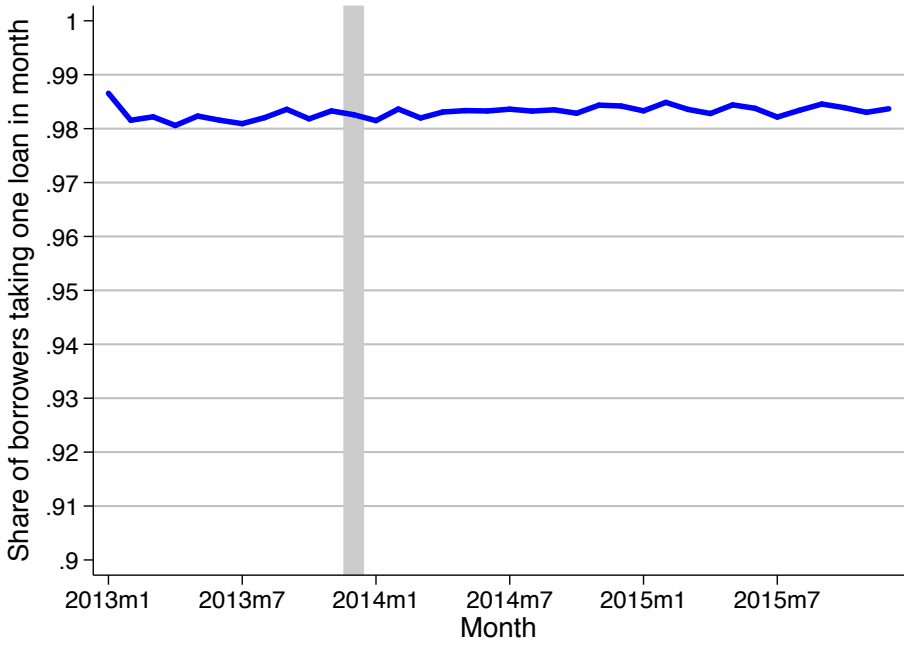
Notes: This figure displays a measure of exposure to interest rate regulation across loan size and borrower risk. Exposure to the policy is calculated as the share of loans that were signed before the policy was implemented in December 2013, at interest rates higher than the interest rate cap once the policy was fully in place in December 2015.

Figure A.7: Treatment intensity variable



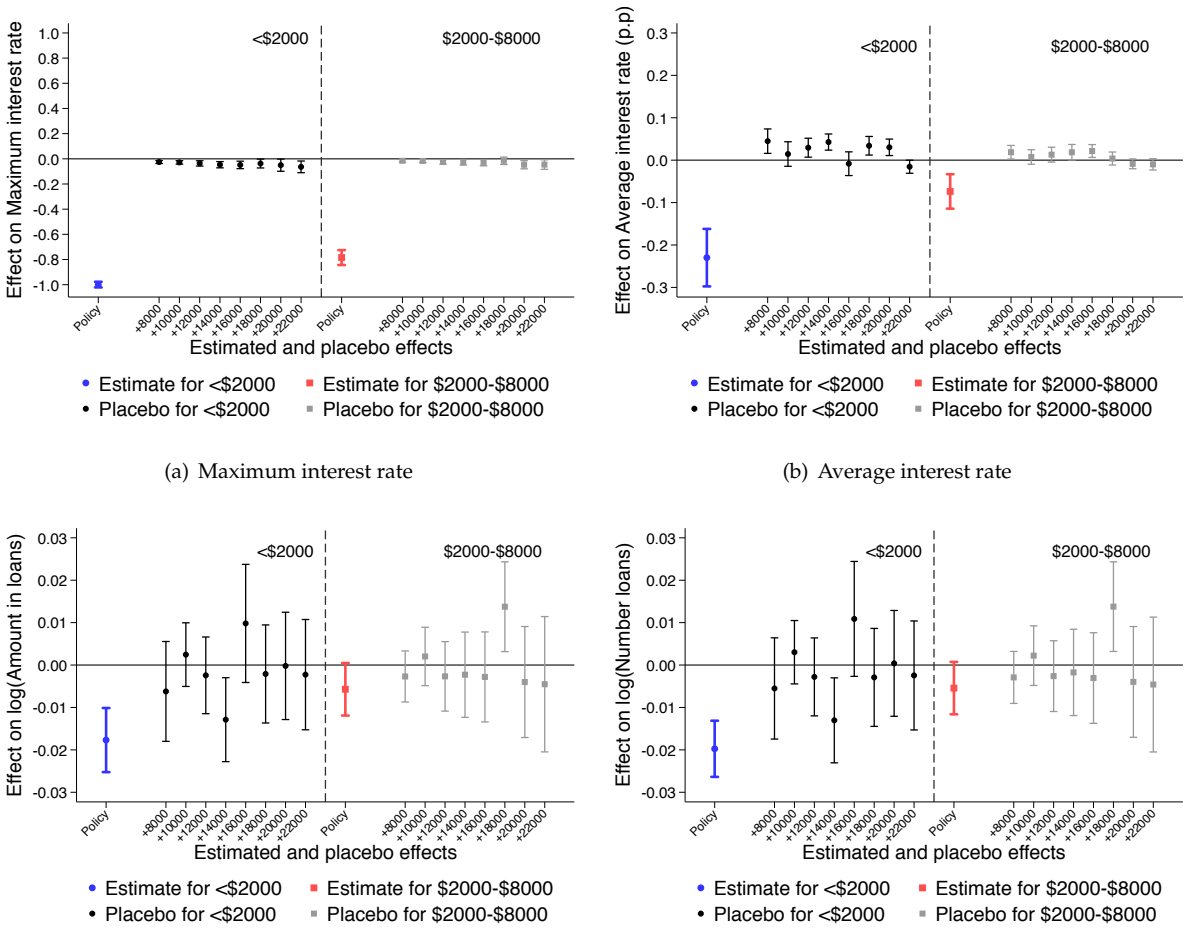
Notes: This figure displays the evolution of the treatment intensity variable defined in Section 3.2.2. This variable is defined as $\Delta^{\bar{\iota}}_{\ell,t} \equiv (\bar{\iota}_{\ell,0} - \bar{\iota}_{\ell,t}) - (\bar{\iota}_{>8000,0} - \bar{\iota}_{>8000,t})$ and measures the decrease in the interest rate cap for a treated loan-size bracket net of the decrease in the interest rate cap for the untreated loan-size bracket of loans in \$0-\$8,000.

Figure A.8: Number of loans per borrower and month, conditional on borrower



Notes: This figure displays the evolution of the share of borrowers taking only one loan in a month in which they borrow. The gray vertical line indicates the policy change.

Figure A.9: Differences-in-differences effects of placebo policies



(a) Maximum interest rate

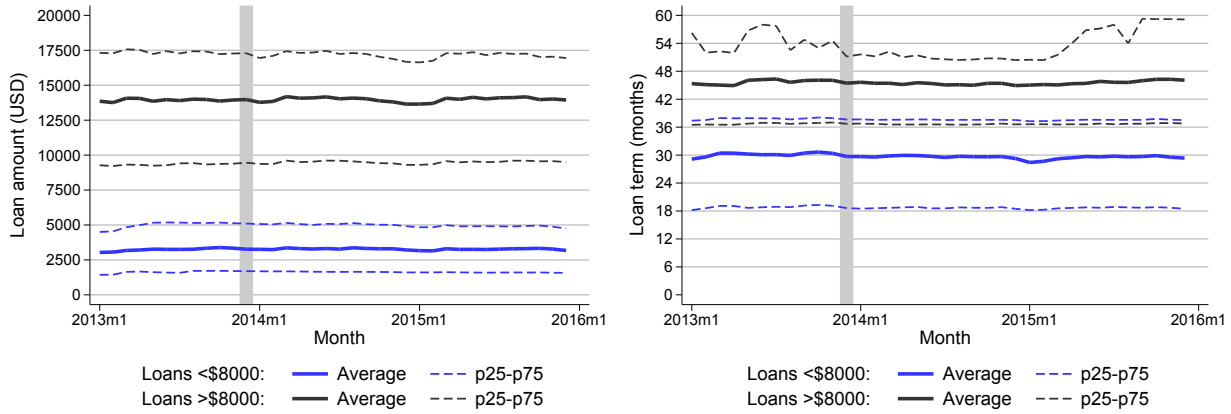
(b) Average interest rate

(c) Amount in loans

(d) Number of loans

Notes: This figure displays the contrast between our estimates from Table 3 and Table 4 (blue and red) with estimates for a range of placebo policies (black and gray). In each figure, the left panel displays results for loans of $0-$2,000 and the left panel displays results for loans of $2,000-$8,000. Placebo policies are constructed by using the same policy intensity variables to estimate effects of interest rate regulation on different parts of the loan size distribution. The first placebo policy adds $8,000 to the actual policy definition, and subsequent placebo policies subsequently add $2,000. Each dot indicates the estimated coefficient, while spikes indicate 95% confidence intervals clustered at the risk bin-product bin level. All regressions are weighted by the number of loans in the product type bin-risk bin before the policy was implemented.

Figure A.10: Evolution of distribution of application loan amount and term

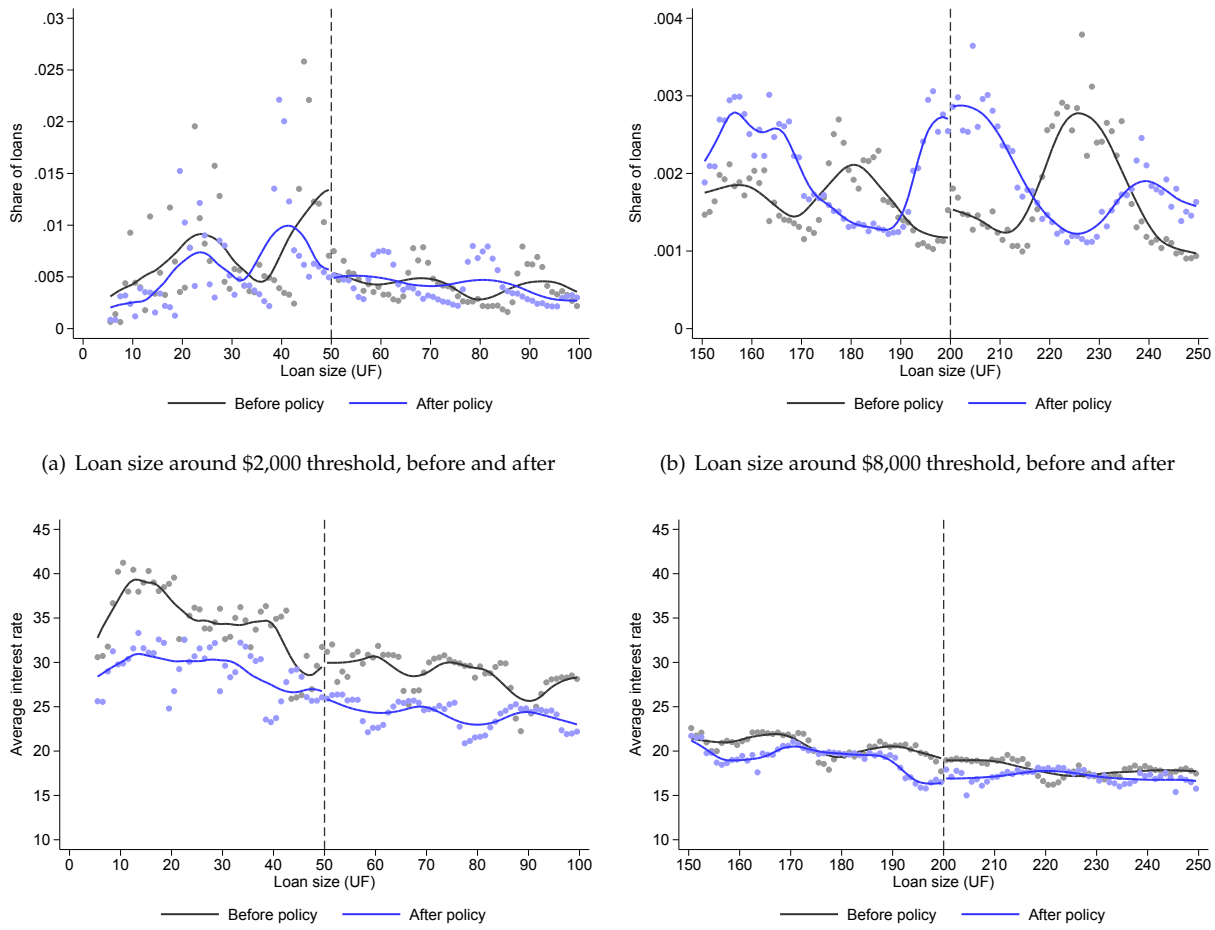


(a) Application loan amount

(b) Application loan term

Notes: This figure displays the evolution of the distribution of application loan amount and term separately for loans of $0-$8,000 and loans of $8,000-$20,000. In particular, each panel displays the average and the 25th and 75th percentiles of each variable for each month.

Figure A.11: Distribution of loan size around policy thresholds



(a) Loan size around $2,000 threshold, before and after

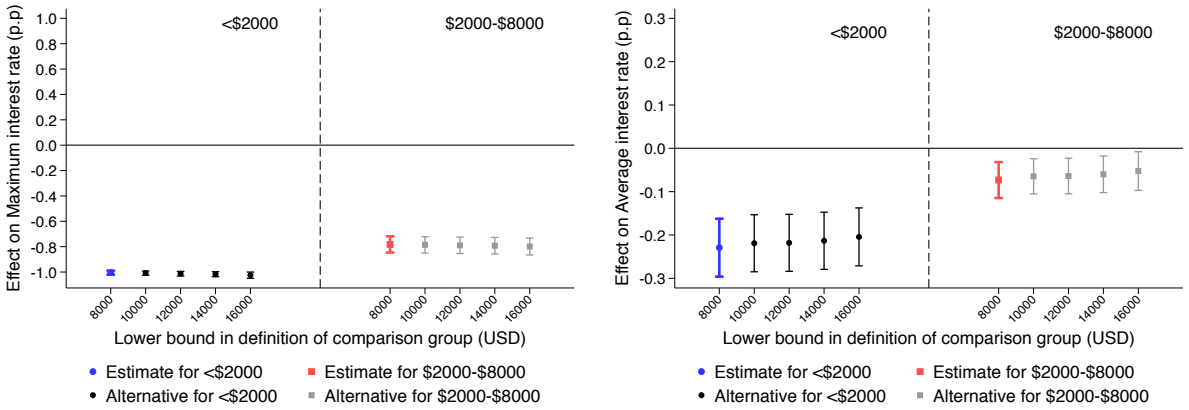
(b) Loan size around $8,000 threshold, before and after

(c) Rates around $2,000 threshold, before and after

(d) Rates around $8,000 threshold, before and after

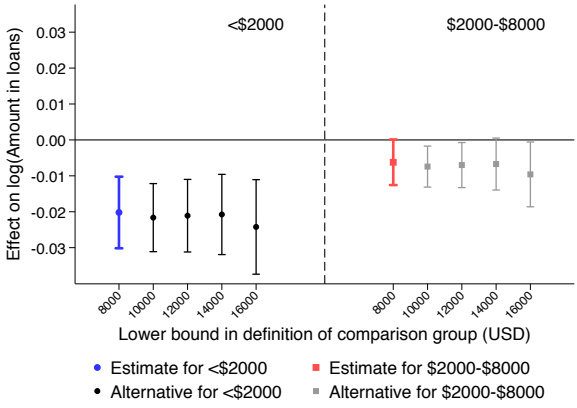
Notes: This figure displays shares of loans and average interest rates by loan size around policy size thresholds at 50UF ($2,000) and 200UF ($8,000). The data is binned in bins of 1UF ($40). For each bin, dots indicate the share of loans originated and average interest rates. Shares are computed across the $0-$20,000 interval. Gray dots indicate loan originations in the semester before the policy was implemented, between January 2013 and November 2013. Blue dots indicate loan originations in the last semester before the policy was fully in place, between January 2016 and November 2016. Gray and blue lines are local polynomial fits of the relationship between number of loans and loan size in Panels (a) and (b) and between interest rates and loan size in Panels (c) and (d), allowed to differ at both sides of the relevant policy threshold.

Figure A.12: Differences-in-differences effects under alternative comparison groups

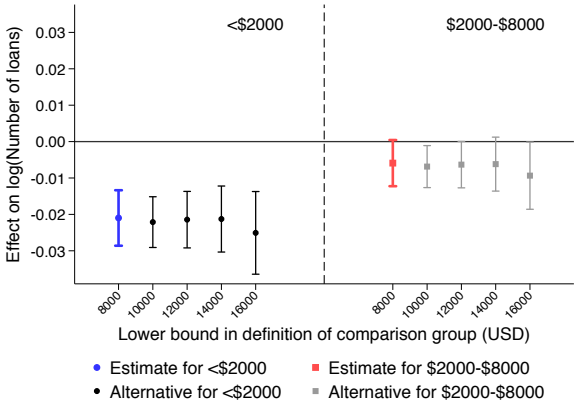


(a) Maximum interest rate

(b) Average interest rate



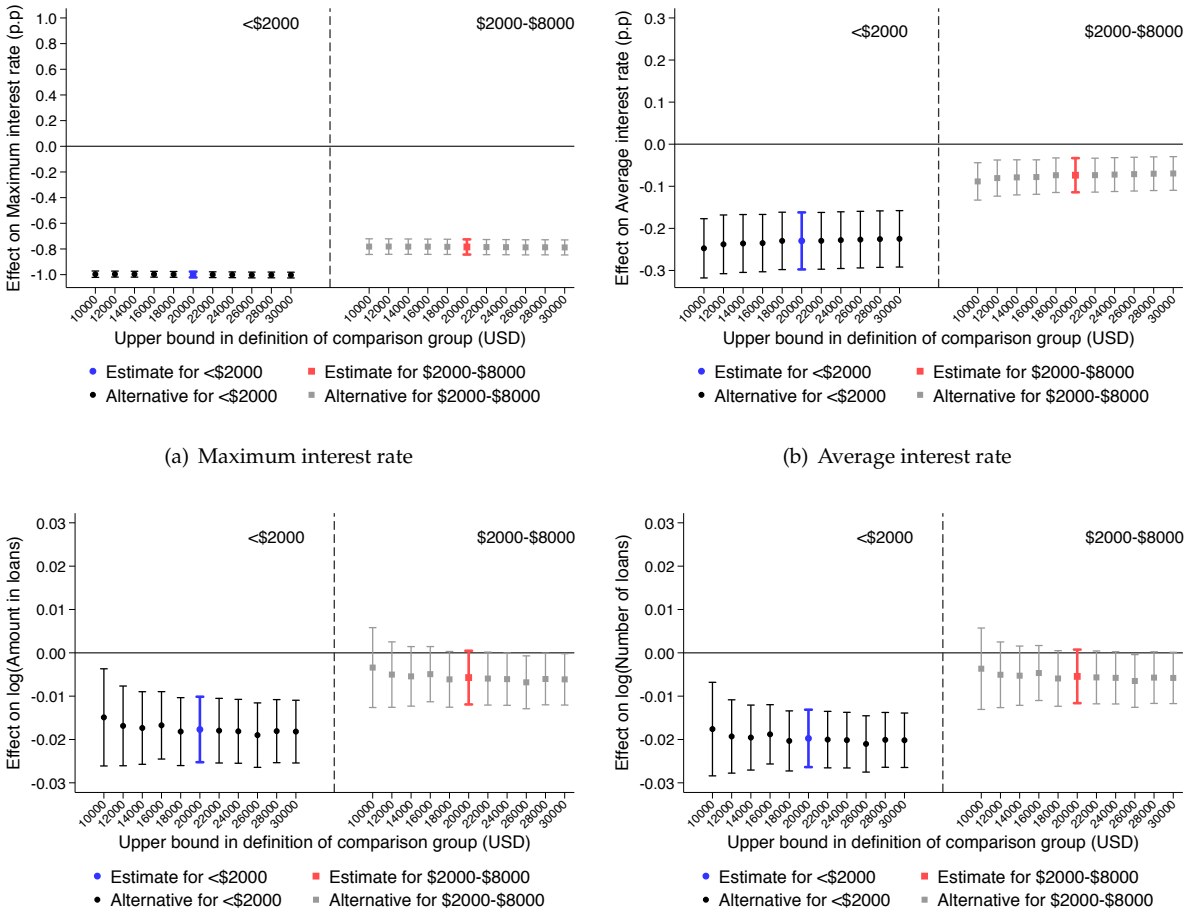
(c) Amount in loans



(d) Number of loans

Notes: This figure displays the contrast between our estimates from Table 3 and Table 4 (blue and red) with estimates for a range of alternative comparison groups (black and gray). In each figure, the left panel displays results for loans of $0-$2,000 and the left panel displays results for loans of $2,000-$8,000. Alternative comparison groups are constructed by shifting the lower bound in the definition of the comparison group, so as to vary the inclusion criterion in terms of the distance to the cutoff set by the policy at $8,000. The first comparison group sets such upper bound at $8,000 as in our baseline results, and subsequent alternative comparison groups include loans $2,000 larger in size. Each dot indicates the estimated coefficient, while spikes indicate 95% confidence intervals clustered at the risk bin-product bin level. All regressions are weighted by the number of loans in the product type bin-risk bin before the policy was implemented.

Figure A.13: Differences-in-differences effects under alternative comparison group size



(a) Maximum interest rate

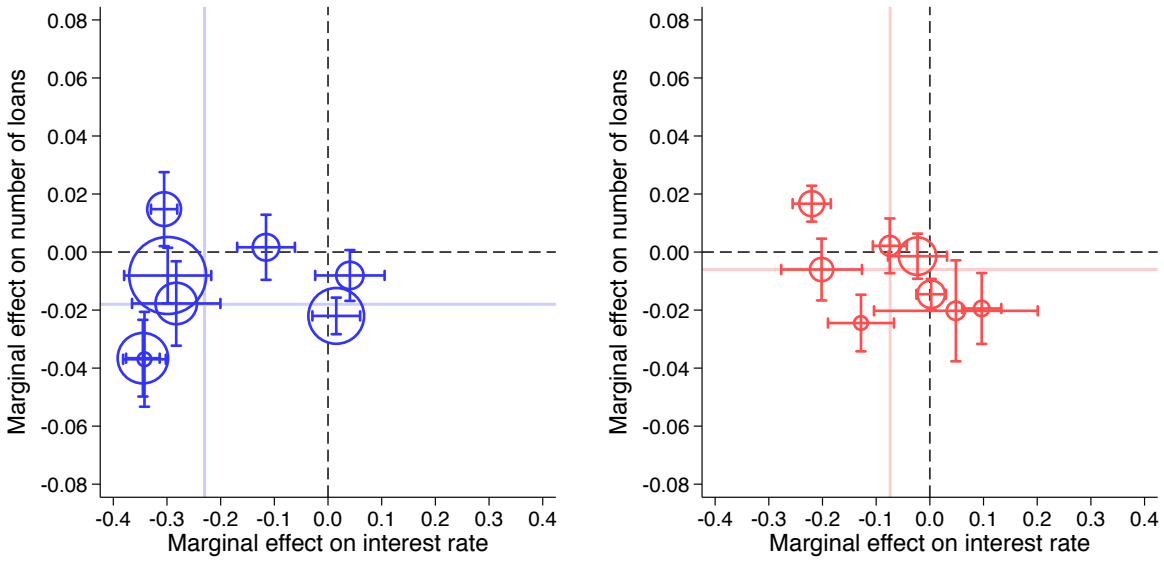
(b) Average interest rate

(c) Amount in loans

(d) Number of loans

Notes: This figure displays the contrast between our estimates from Table 3 and Table 4 (blue and red) with estimates for a range of alternative comparison groups (black and gray). In each figure, the left panel displays results for loans of $0-$2,000 and the left panel displays results for loans of $2,000-$8,000. Alternative comparison groups are constructed by shifting the upper bound in the definition of the comparison group, so as to vary the inclusion criterion in terms of the distance to the cutoff set by the policy at $8,000. The first comparison group sets such upper bound at $10,000, and subsequent alternative comparison groups include loans $2,000 larger in size. Each dot indicates the estimated coefficient, while spikes indicate 95% confidence intervals clustered at the risk bin-product bin level. All regressions are weighted by the number of loans in the product type bin-risk bin before the policy was implemented.

Figure A.14: Heterogeneity in effects across banks

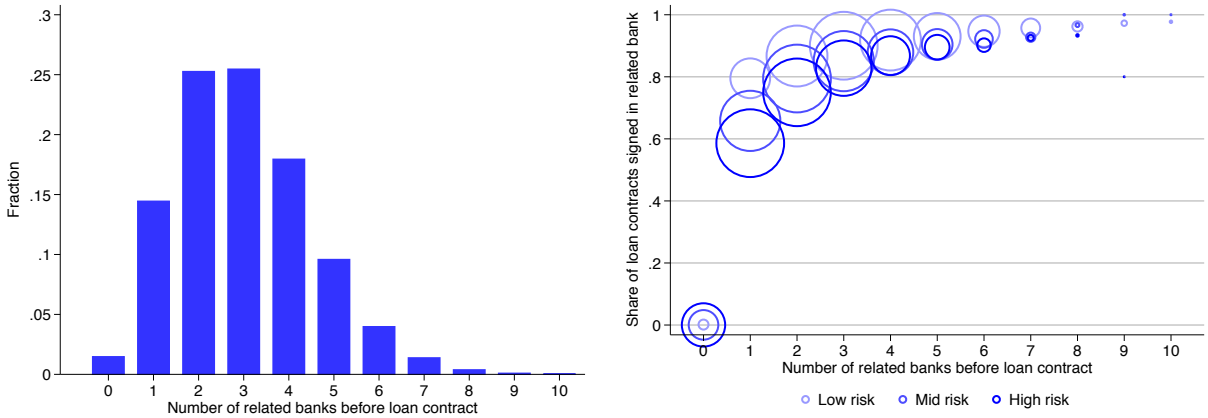


(a) Loans of \$0-\$2,000

(b) Loans of \$2,000-\$8,000

Notes: These figures display marginal effects of interest rate regulation on both number of loans and prices across banks. In each panel, circles indicate estimates for effect on prices on the x-axis and on number of loans on the y-axis the size of the circle is given by the market share of the bank; and spikes indicate standard errors clustered at the risk bin-product bin level. All regressions are weighted by the number of loans in the product type bin-risk bin before the policy was implemented. Solid lines indicate marginal effects estimated across banks, as displayed in Table 3 and Table 4.

Figure A.15: Relationship between borrowers and banks

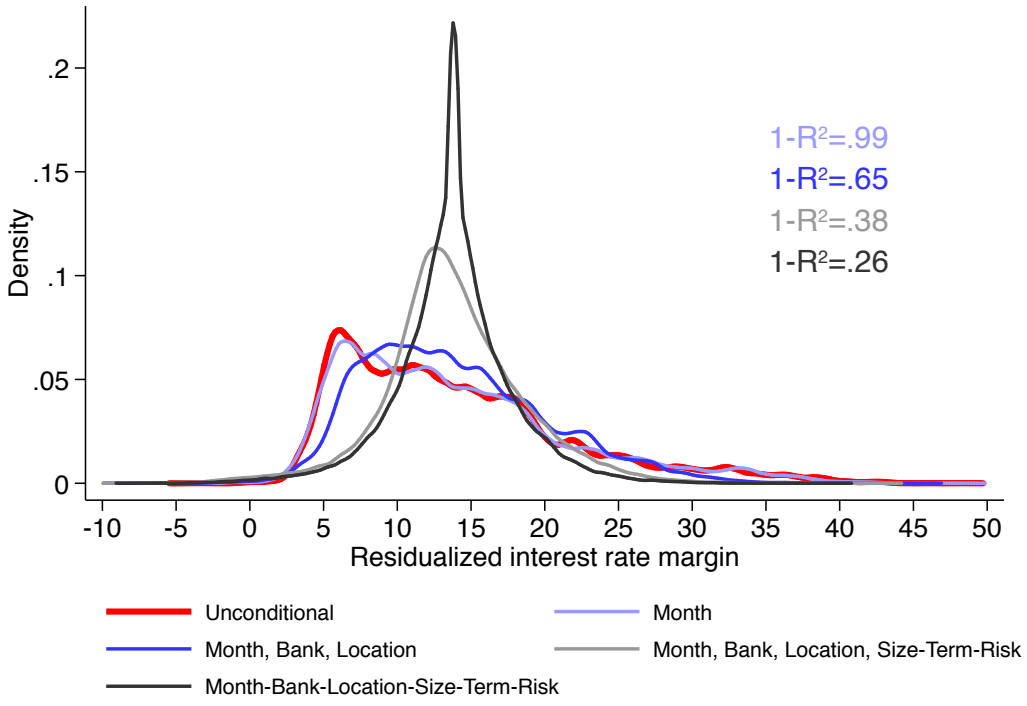


(a) Number of related banks

(b) Loans at related banks

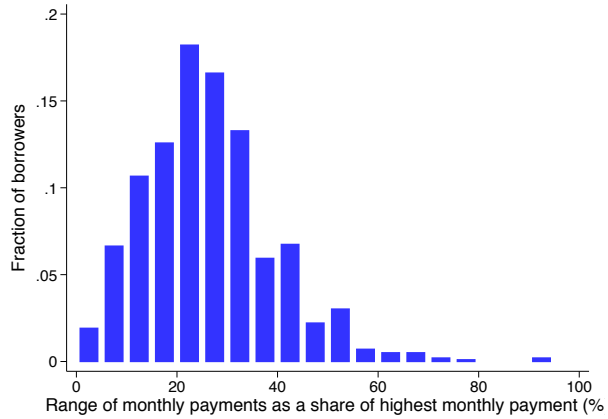
Notes: Panel (a) describes the share of previously related banks for each borrower in the dataset. Panel (b) describes the share of loan contracts signed with a previously related bank for each tercile of borrower risk and each number of previously related banks.

Figure A.16: Price dispersion in consumer loan contracts

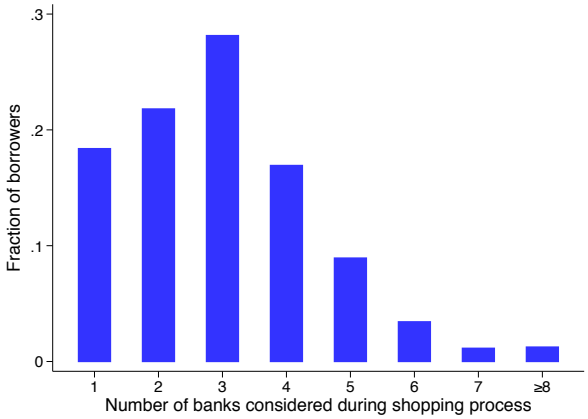


Notes: This figure displays interest rate margins. The red line displays the density of raw interest rate margins in the data. Each additional density displays margins residualized by a increasingly richer sets of covariates, from month FEs to month-bank-size-term-risk FEs.

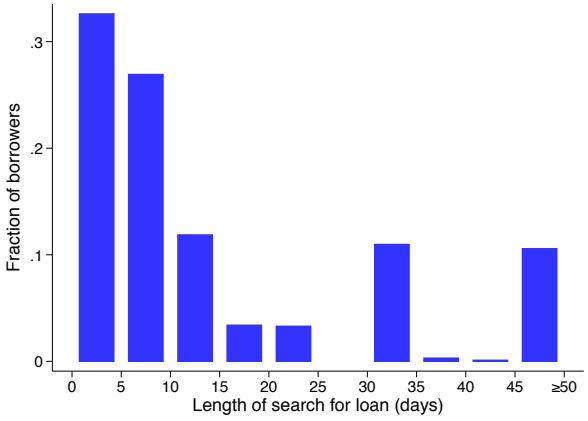
Figure A.17: Survey evidence supporting modeling choices



(a) Perceived price dispersion



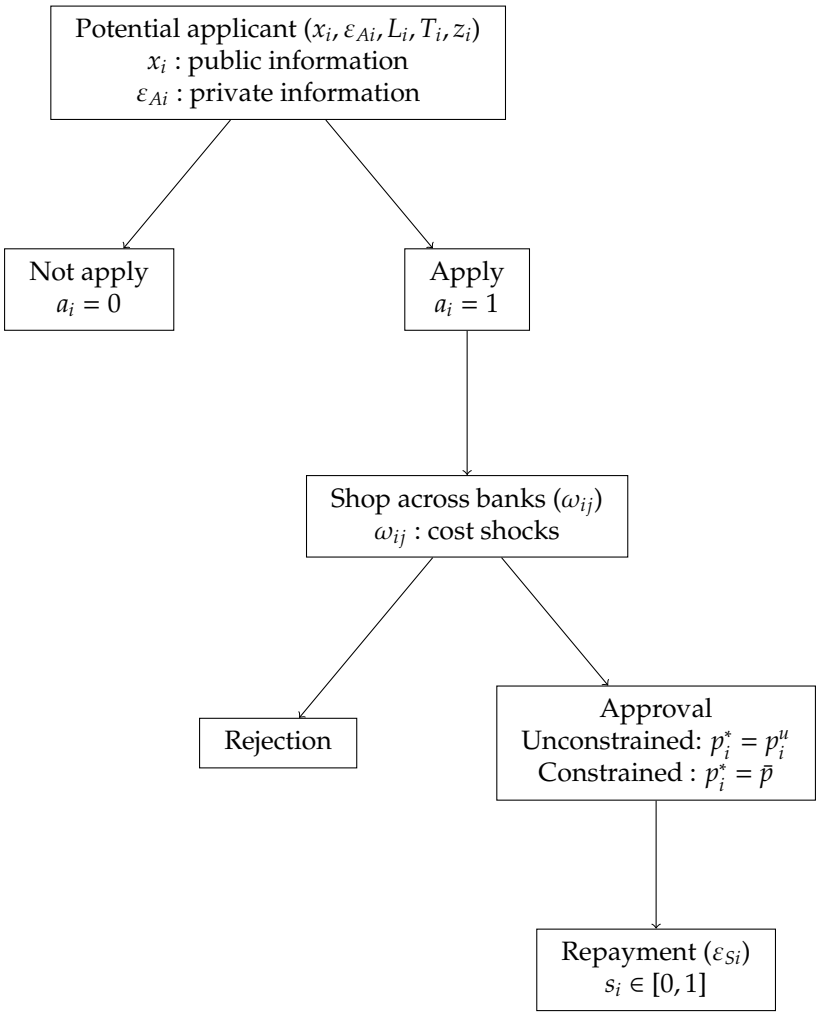
(b) Number of banks considered in shopping process



(c) Length of shopping process

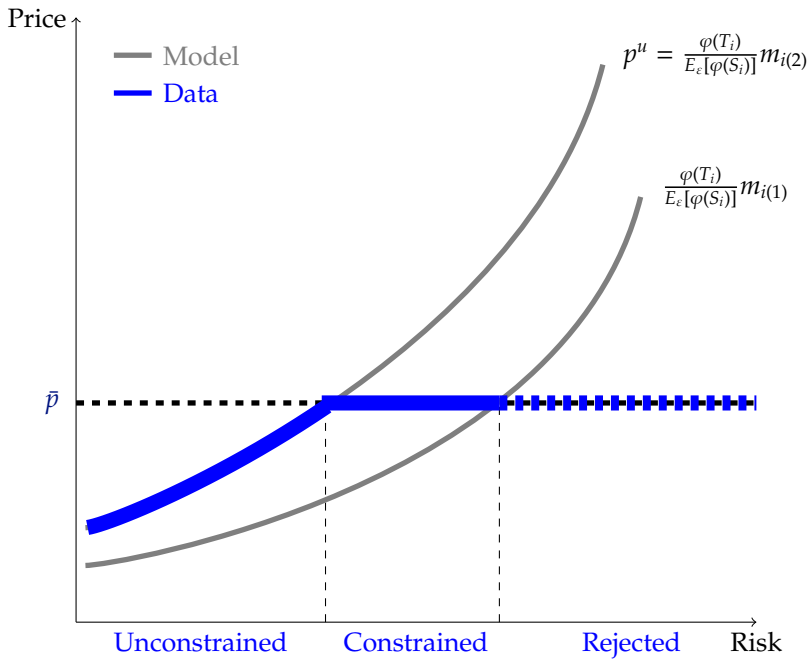
Notes: These figures display survey results related to modeling choices. Panel (a) shows a histogram of the number of banks that borrowers reported to consider during the shopping process for loans. Panel (b) shows a histogram of the length of the search process measured in days between beginning of their search to the end of it, regardless of the approval or rejection of their applications. Panel (c) shows a histogram of perceived price dispersion in the market, as measured by the ratio of the range between the lowest and the highest monthly payment in the market to the highest monthly payment in the market, $\frac{p_H - p_L}{p_H}$.

Figure A.18: Timing of the model



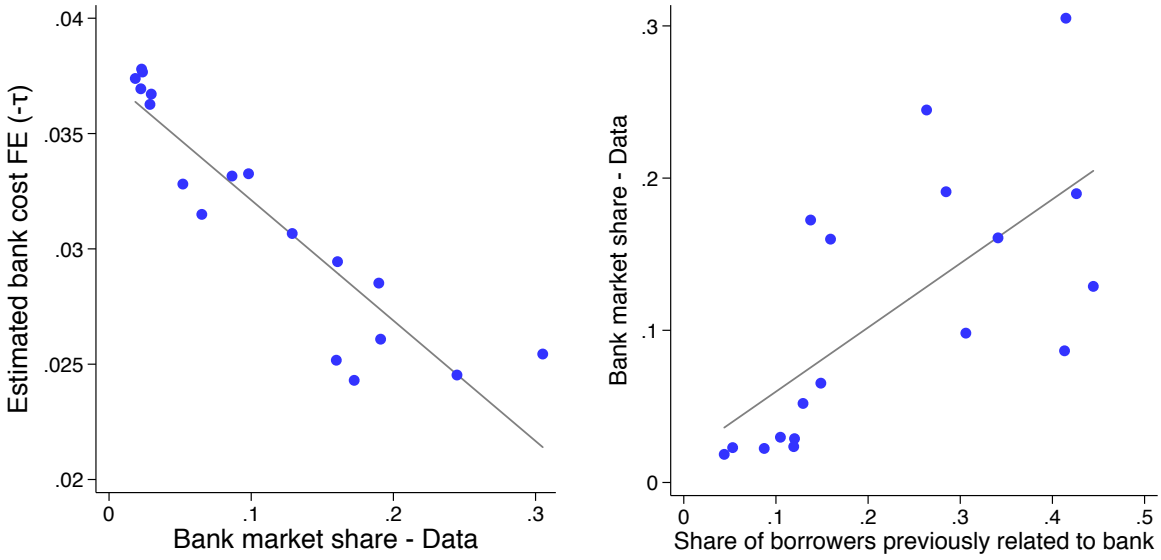
Notes: This figure displays the timing and structure of the model. Exogenous observables characterizing the consumer are covariates x_i, loan amount and term (L_i, T_i), and application cost shifters z_i. Consumer unobservables are $(\varepsilon_{Ai}, \varepsilon_{Si})$, whereas bank unobservables are cost shocks ω_{ij}. Finally, endogenous variables are application choices a_i, approval and pricing choices p_i, and repayment s_i.

Figure A.19: Intuition for identification of bank cost



Notes: This figure provides an illustration of how observed outcomes inform the identification of bank cost. The blue line combines observed application outcomes with observed prices, while the gray lines represent the first order statistic of cost and optimal price for the bank with the lowest cost in the market.

Figure A.20: Relationship between cost estimates and data

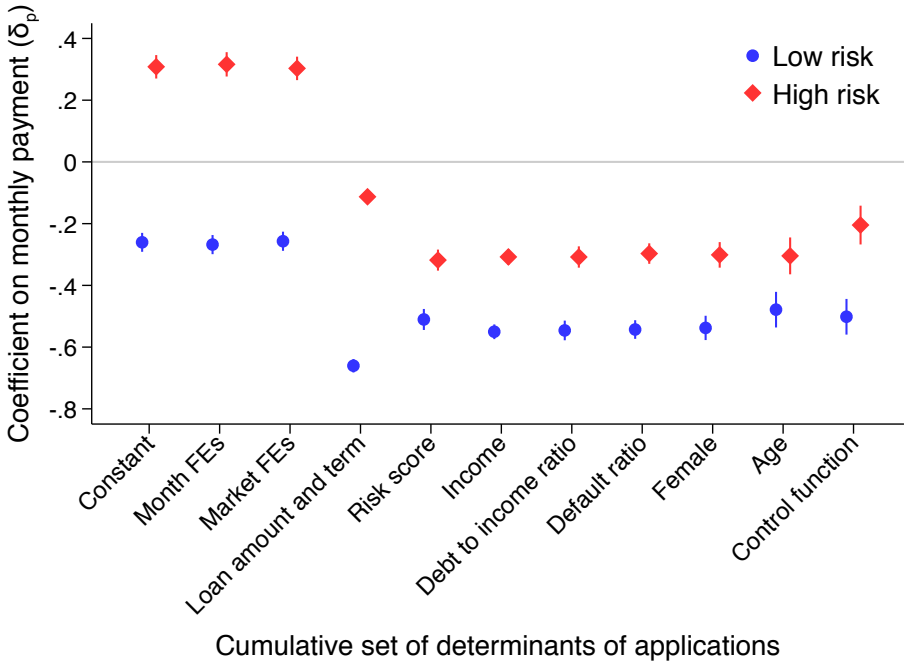


(a) Market shares and cost FEs

(b) Relationships and shares

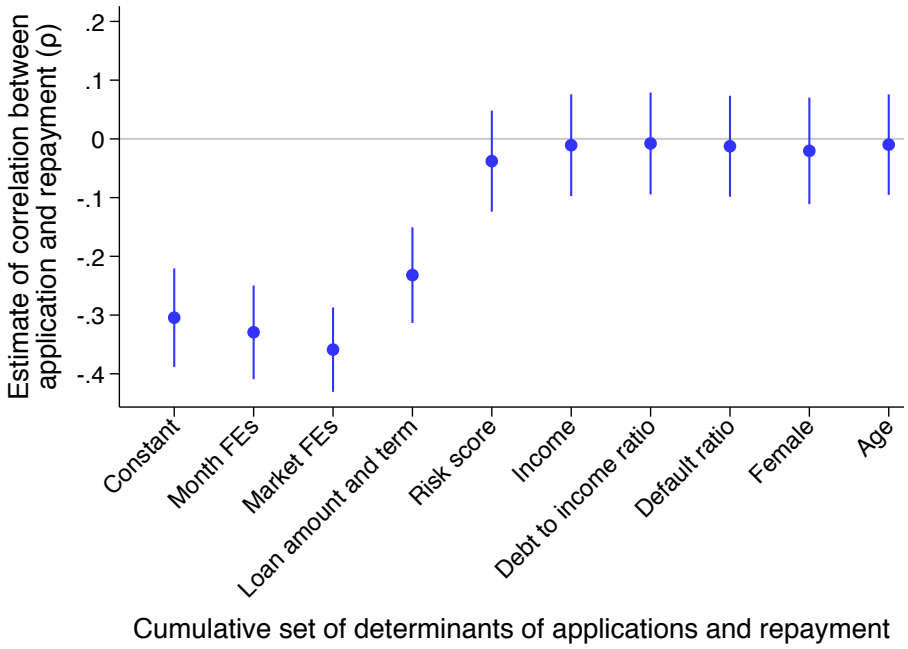
Notes: Panel (a) in this figure displays observed market shares and estimates for bank fixed effects τ_j in ω_{ij}. Panel (b) in this figure displays the correlation between observed shares of previously related borrowers and observed bank market shares.

Figure A.21: Price sensitivity estimates under different specifications



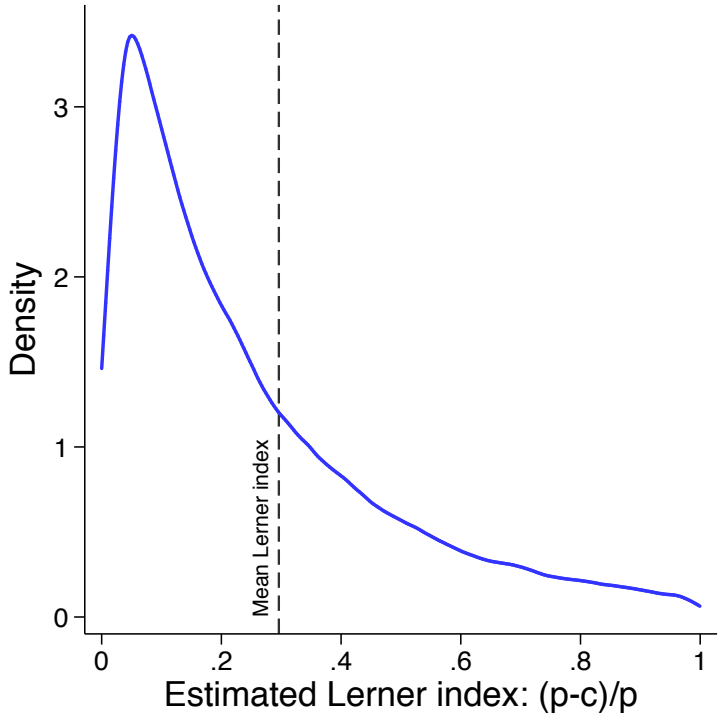
Notes: This figure displays estimates for the coefficient on expected monthly payment in the application equation (δ_p) under different specifications. In particular, we estimate that equation using an increasingly rich set of borrower covariates to assess the role of unobservables related to both applications and bank pricing in terms of driving our estimates of price sensitivity, in line with Altonji et al. (2005). The last estimates include a control function as an additional covariate in estimation, following Petrin and Train (2010). The figure displays estimates for low-risk (blue) and high-risk (red) borrowers. Dots indicate estimates of δ_p. Lines indicate 95% confidence intervals.The first specification we consider includes a constant, and subsequent specifications add more covariates sequentially. Our preferred specification for the analysis in the paper is that with the full vector of covariates.

Figure A.22: Selection estimate under different specifications



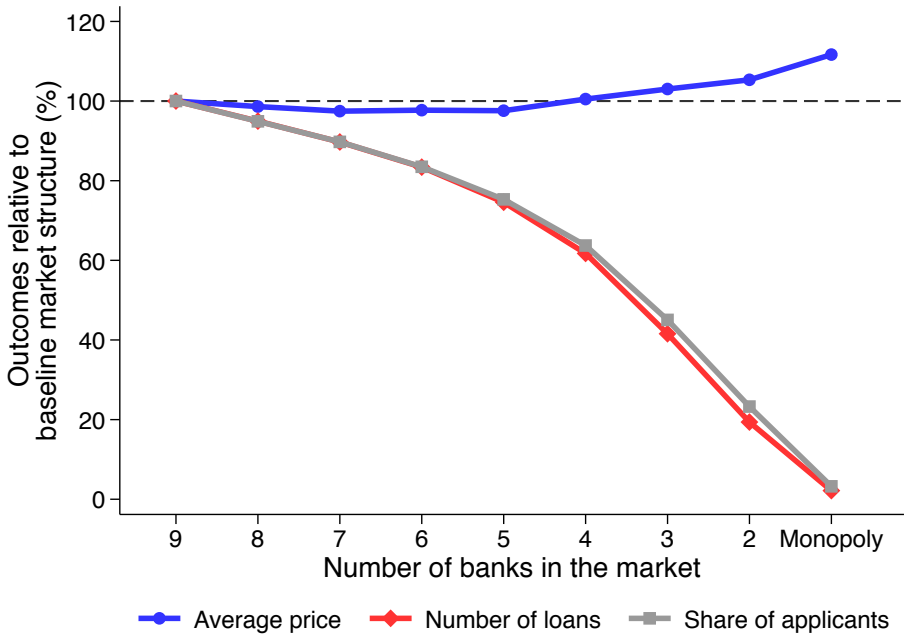
Notes: This figure displays estimates for the coefficient measuring the correlation between shocks to application and repayment (ρ) under different specifications. In particular, we estimate the application and repayment equations using an increasingly rich set of borrower covariates for *both* of them to assess the extent to which available observables are able to capture patterns of risk selection into the market. Dots indicate estimates of ρ. Lines indicate 95% confidence intervals. The first specification we consider includes a constant, and subsequent specifications add more covariates sequentially. Our preferred specification for the analysis in the paper is that with the full vector of covariates.

Figure A.23: Simulated bank profit margins



Notes: This figure displays results for the distribution of the predicted Lerner index using model estimates.

Figure A.24: The effects of interest rate regulation under different market structures: baseline outcomes



Notes: This figure display baseline outcomes under baseline interest rate regulation under different market structures. We start with the baseline market structure of 9 banks, and sequentially merge banks until a scenario in which the market is served by a monopoly.

Table A.1: Determinants of loan performance

	(1)	(2)	(3)	(4)	(5)
	1{Default}				
log(Income)	-0.421***	-0.376***	-0.501***	-0.498***	-0.498***
	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)
Consumer debt to income ratio	0.013***	-0.020***	0.054***	0.054***	0.054***
	(0.004)	(0.006)	(0.006)	(0.006)	(0.006)
Mortgage debt to income ratio	-0.217***	0.136***	0.101***	0.104***	0.104***
	(0.005)	(0.010)	(0.010)	(0.010)	(0.010)
log(Consumer debt)		0.108***	0.111***	0.109***	0.108***
		(0.011)	(0.011)	(0.011)	(0.011)
No consumer debt		1.059***	0.934***	0.747***	0.746***
		(0.029)	(0.029)	(0.031)	(0.031)
No consumer debt ≥90-day default		-0.531***	-0.414***	-0.414***	-0.415***
		(0.017)	(0.017)	(0.017)	(0.017)
No consumer debt ≥90-day default		-0.458***	-0.467***	-0.468***	-0.468***
		(0.008)	(0.008)	(0.008)	(0.008)
Consumer ≥90-day default to debt ratio		0.591***	0.630***	0.618***	0.619***
		(0.035)	(0.036)	(0.036)	(0.036)
Consumer <90-day default to debt ratio		0.425***	0.554***	0.519***	0.518***
		(0.093)	(0.094)	(0.094)	(0.094)
log(Mortgage debt)		-0.746***	-0.794***	-0.821***	-0.822***
		(0.040)	(0.041)	(0.041)	(0.041)
No mortgage debt		-0.663***	-0.838***	-0.911***	-0.912***
		(0.077)	(0.080)	(0.080)	(0.080)
No mortgage debt ≥90-day default		-0.308***	-0.339***	-0.360***	-0.360***
		(0.049)	(0.050)	(0.050)	(0.050)
No mortgage debt <90-day default		-0.626***	-0.620***	-0.625***	-0.625***
		(0.025)	(0.025)	(0.025)	(0.025)
Mortgage ≥90-day default to debt ratio		0.052	0.178	0.195	0.195
		(0.136)	(0.136)	(0.136)	(0.136)
Mortgage <90-day default to debt ratio		-2.557***	-2.063***	-2.116***	-2.117***
		(0.400)	(0.376)	(0.376)	(0.376)
Change in consumer debt		0.226***	0.201***	0.201***	0.201***
		(0.005)	(0.005)	(0.005)	(0.005)
Change in consumer debt ≥90d default		0.010***	0.006*	0.006*	0.006*
		(0.004)	(0.004)	(0.004)	(0.004)
Change in mortgage debt		-0.011***	-0.012***	-0.012***	-0.012***
		(0.003)	(0.003)	(0.003)	(0.003)
Change in mortgage debt ≥90d default		0.042***	0.027***	0.025***	0.025***
		(0.006)	(0.007)	(0.007)	(0.007)
Age			-0.411***	-0.407***	-0.407***
			(0.003)	(0.003)	(0.003)
Female			-0.308***	-0.310***	-0.309***
			(0.007)	(0.007)	(0.007)
Previously related to any bank				-0.283***	-0.283***
				(0.016)	(0.016)
Local unemployment rate					0.015**
					(0.007)
Constant	-1.907***	0.113	0.234***	0.599***	0.603***
	(0.024)	(0.078)	(0.080)	(0.082)	(0.082)
Observations	916,934	916,934	916,436	916,436	916,436
Pseudo R-squared	0.034	0.054	0.079	0.080	0.080
Market FE	Y	Y	Y	Y	Y

Notes: All columns display results from logit regressions of individual loan default outcomes on borrower covariates. All covariates are standardized. Credit history variables are computed as average over the year previous to each loan. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.2: Effects excluding loans around the $8,000 threshold

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Maximum interest rate			Average interest rate			log(Applications)			log(Credit volume)		
	All	Low-risk	High-risk	All	Low-risk	High-risk	All	Low-risk	High-risk	All	Low-risk	High-risk
Loans in $0 – $2000	-1.003***	-0.907***	-0.996***	-0.220***	-0.108***	-0.253***	-0.005	0.002	-0.013*	-0.019***	-0.013***	-0.029***
	(0.012)	(0.062)	(0.018)	(0.034)	(0.015)	(0.042)	(0.004)	(0.003)	(0.007)	(0.003)	(0.003)	(0.004)
Loans in $2000 – $8000	-0.838***	-0.662***	-0.831***	-0.075***	-0.032***	-0.120***	-0.001	0.001	-0.007	-0.008**	-0.007**	-0.014***
	(0.020)	(0.028)	(0.027)	(0.024)	(0.011)	(0.028)	(0.004)	(0.003)	(0.006)	(0.003)	(0.003)	(0.003)
Observations	2,304	2,304	2,145	2,304	2,304	2,145	2,304	2,304	2,034	2,304	2,304	2,304
R-squared	0.991	0.979	0.985	0.983	0.986	0.964	0.995	0.990	0.994	0.984	0.984	0.966
	log(Number of loans)			Risk-I	Risk-H	log(Income)	Share of loans under 1Y default			Months under 1Y default		
	All	Low-risk	High-risk	All	All	All	All	Low-risk	High-risk	All	Low-risk	High-risk
Loans in $0 – $2000	-0.021***	-0.012***	-0.032***	-0.082***	-0.034***	0.004***	-0.097***	-0.066***	-0.112***	-0.004***	-0.002***	-0.003***
	(0.003)	(0.003)	(0.003)	(0.005)	(0.011)	(0.001)	(0.018)	(0.007)	(0.036)	(0.001)	(0.000)	(0.001)
Loans in $2000 – $8000	-0.008**	-0.006*	-0.015***	-0.027***	-0.009	0.000	-0.050***	-0.023***	-0.080***	-0.002***	-0.001***	-0.003***
	(0.003)	(0.003)	(0.003)	(0.006)	(0.008)	(0.001)	(0.012)	(0.007)	(0.025)	(0.000)	(0.000)	(0.001)
Observations	2,304	2,304	2,304	2,304	2,304	2,304	2,304	2,304	2,145	2,304	2,304	2,145
R-squared	0.992	0.988	0.987	0.988	0.978	0.989	0.909	0.720	0.696	0.907	0.698	0.700
Product bin FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Product bin-month of year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Notes: This table display estimated coefficients from equation (4) using a sample that excludes loans of size between $6,000 and $10,000. For each outcome, the regression is estimated across borrower risk bins and separately by borrower risk bin. All regressions include risk bin-product bin fixed effects and risk bin-month fixed effects. For default outcomes, the estimating sample is restricted to loans originated before December 2015, so as to allow for a year long period after origination in which the outcomes are measured. Share of loans under default in first year is computed in a 0-100 scale. Predicted risk is computed as described in Section 2.2.3 and measured in a 0-100 scale. Income is measured in thousands of U.S. dollars. All regressions are weighted by the number of loans in the product type bin-risk bin before the policy was implemented. Clustered standard errors at the risk bin-product bin level are displayed in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.3: Summary statistics for household survey

Variable	N	Mean	SD	p10	p50	p90
A - Experience in credit market						
Debit account	1,003	0.84	0.36	0.00	1.00	1.00
Checking account	1,003	0.89	0.31	0.00	1.00	1.00
Debit card	1,003	0.97	0.17	1.00	1.00	1.00
Credit card	1,003	0.98	0.15	1.00	1.00	1.00
Credit line	1,003	0.88	0.32	0.00	1.00	1.00
Consumer loan	1,003	0.98	0.14	1.00	1.00	1.00
Car loan	1,003	0.35	0.48	0.00	0.00	1.00
Mortgage	1,003	0.50	0.50	0.00	0.00	1.00
College loan	1,003	0.34	3.15	0.00	0.00	1.00
B - Shopping behavior						
Number of considered banks	963	2.99	1.65	1.00	3.00	5.00
Number of applications	963	1.35	0.80	1.00	1.00	2.00
Duration of search period in days	963	15.15	28.65	1.00	7.00	30.00
Offline shopping	963	0.81	0.39	0.00	1.00	1.00
Perceived range of prices (%)	994	25.70	12.64	10.53	24.81	41.18
C - Economic hardships						
Experienced economic hardship	1,003	0.59	0.49	0.00	1.00	1.00
Financed economic hardship with formal credit	1,003	0.55	0.50	0.00	1.00	1.00
Financed economic hardship with savings/assets	1,003	0.27	0.44	0.00	0.00	1.00
Financed economic hardship with other	1,003	0.35	0.48	0.00	0.00	1.00
Stopped paying consumer loan	1,003	0.28	0.45	0.00	0.00	1.00
Stopped paying credit card	1,003	0.43	0.50	0.00	0.00	1.00
Stopped paying utility bills	1,003	0.11	0.31	0.00	0.00	1.00
Stopped paying rent	1,003	0.02	0.14	0.00	0.00	0.00
Stopped paying mortgage	1,003	0.06	0.24	0.00	0.00	0.00
Stopped paying car loan	1,003	0.07	0.25	0.00	0.00	0.00
Stopped paying student loan	1,003	0.06	0.23	0.00	0.00	0.00
Stopped paying health bills	1,003	0.07	0.25	0.00	0.00	0.00
Stopped paying other bills	1,003	0.13	0.34	0.00	0.00	1.00
Cut expenditure on non-durables	1,003	0.87	0.34	0.00	1.00	1.00
Cut expenditure on personal care	1,003	0.37	0.48	0.00	0.00	1.00
Cut expenditure on health	1,003	0.18	0.38	0.00	0.00	1.00
Cut expenditure on education	1,003	0.12	0.32	0.00	0.00	1.00
Cut expenditure on home services	1,003	0.36	0.48	0.00	0.00	1.00
Cut expenditure on transportation	1,003	0.15	0.35	0.00	0.00	1.00
D - Borrower attributes						
Age	1,003	42.18	8.80	32.00	41.00	55.00
Female	1,003	0.39	0.49	0.00	0.00	1.00
Approval probability	981	0.59	0.07	0.49	0.60	0.67
Annual income	981	17,557.88	7,803.02	7,772.14	16,853.41	28,616.13
Financial literacy score (1-3)	1,003	1.75	0.81	1.00	2.00	3.00

Notes: This table displays summary statistics for our household survey.