DOCUMENTOS DE TRABAJO

Superintendencia de Bancos e Instituciones Financieras Chile

Interest Rate Caps, Consumer Protection and Credit Access: Evidence from Consumer Loans in Chile

José Ignacio Cuesta - Alberto Sepúlveda

Nº 01/19 - Enero 2019

Interest Rate Caps, Consumer Protection and Credit Access: Evidence from Consumer Loans in Chile

José Ignacio Cuesta - Alberto Sepúlveda

Superintendencia de Bancos e Instituciones Financieras - Chile

Superintendencia
de Bancos
e Instituciones
Financieras
Chile

The Working Papers series is a publication of the Chilean Superintendency of Banks and Financial Institutions (SBIF), whose purpose is to disseminate preliminary research in the finance area for discussion and comments. These works are carried out by professionals of the institution or entrusted by it to third parties.

The objective of the series is to contribute to the discussion and analysis of relevant topics for financial stability and related regulations. Although the Working Papers have the editorial revision of the SBIF, the analysis and conclusions contained therein are the sole responsibility of the authors.

La serie de Documentos de Trabajo es una publicación de la Superintendencia de Bancos e Instituciones Financieras de Chile (SBIF), cuyo objetivo es divulgar trabajos de investigación de carácter preliminar en el área financiera, para su discusión y comentarios. Estos trabajos son realizados por profesionales de esta institución o encargados por ella a terceros.

El objetivo de la serie es aportar a la discusión y análisis de temas relevantes para la estabilidad financiera y normativas relacionadas. Si bien los Documentos de Trabajo cuentan con la revisión editorial de la SBIF, los análisis y conclusiones en ellos contenidos son de exclusiva responsabilidad de sus autores.

Documentos de Trabajo de la Superintendencia de Bancos e Instituciones Financieras de Chile (SBIF)
Working Papers of the Superintendency of Banks and Financial Institutions of Chile
Moneda 1123, Santiago, Chile
Teléfono: (56) 22887 9200; Casilla 15-D

Documento de Trabajo Nº01/19
Working Paper Nº01/19

Interest Rate Caps, Consumer Protection and Credit Access: Evidence from Consumer Loans in Chile*

José Ignacio Cuesta§ - Alberto Sepúlveda‡
Dirección de Estudios
Superintendencia de Bancos e Instituciones Financieras

Enero, 2019

ABSTRACT

Price regulation on loans is a common policy in credit markets and usually takes the form of interest rate caps. Supporters of such policies argue that they protect borrowers from the exercise of market power by banks, while opponents argue that they may harm their access to the credit market. We study the implications of such regulation on pricing, credit volume, risk selection and loan performance. We exploit a policy change that strongly decreased interest rate caps in the Chilean retail credit market as an empirical application. Using administrative contract-level data, we find that stronger price regulation decreases loan interest rates, reduces the volume of credit and improves credit default risk of the borrower pool. Estimated effects are large: the number of loans in the market decreases by 19% within the regulated segment of the market, which combines a decrease in the number of loan applications with an increase in the number of rejections by banks. Importantly, our estimates show that the effects of stronger price regulation are concentrated among risky borrowers. We discuss the policy implications of our findings.

RESUMEN

La regulación de precios es una política común en el mercado del crédito y usualmente toma la forma de tasas de interés máximas. Los impulsores de tales políticas argumentan que ellas protegen a los consumidores del ejercicio de poder de mercado por parte de los bancos, mientras sus detractores argumentan que pueden perjudicar el acceso de los consumidores al mercado del crédito. En este trabajo, estudiamos las consecuencias de la fijación de tasas máximas sobre tasas de interés, volumen de crédito, selección de riesgo y comportamiento de pago. Utilizamos un cambio en esta política en el mercado de créditos de consumo de Chile como aplicación empírica. Usando datos administrativos a nivel individual, encontramos que una regulación de precios más estricta reduce las tasas de interés, reduce el volumen de crédito y mejora la composición de riesgo del mercado. Los efectos que estimamos son sustantivos: el número de créditos en el mercado se reduce en un 19% en el segmento del mercado afectado por la regulación, lo que combina una disminución en el número de solicitudes de crédito con un régimen de aprobación más estricto por parte de los bancos. Estos efectos están concentrados en el segmento de consumidores de mayor riesgo. Ofrecemos una discusión de las implicancias de política de nuestros resultados.

*This version: September, 2018. First version: August, 2017. We thank our discussants Robin Prager and Carmen Gloria Silva for their valuable suggestions. We also thank Michael Dinerstein, Andrew Gianou, Felipe González, Michael Greenstone, Ali Hortaçsu, Alejandro Hoyos, Agustín Hurtado, Gastón Illanes, Kyeongbae Kim, Neale Mahoney, Guillermo Marshall, Gregor Matvos, Víctor Medina, Christopher Neilson, Carlos Pulgar, Amanda Starc, Pietro Tebaldi, and seminar participants at the 3rd Conference on Banking Development, Stability, and Sustainability, the 16th International Industrial Organization Conference, the Superintendence of Banks and Financial Institutions of Chile, and the University of Chicago for helpful comments and suggestions. We gratefully acknowledge support from the Becker Friedman Institute IO Initiative at the University of Chicago Department of Economics. We thank the Superintendence of Banks and Financial Institutions for access to data and constant support for this research project, in particular Carolina Flores, Álvaro Yáñez, Nancy Silva and Luis Figueroa. The views expressed are those of the authors alone and do not necessarily reflect those of the Superintendence of Banks and Financial Institutions. Authors are solely responsible for all remaining errors. §Corresponding author. University of Chicago. Email: jicuesta@uchicago.edu. ‡Superintendence of Banks and Financial Institutions, Chile. Email: asepulveda@sbif.cl.

RESUMEN EJECUTIVO

En este documento se estudia la regulación de tasas de inter es en el mercado del crédito. A pesar de que esta regulación ha sido utilizada por siglos, la evidencia respecto a sus efectos es limitada. Adicionalmente, en la mayoría de los países que la utilizan, el diseño carece de mayor sofisticación, lo que contrasta con el comportamiento de los bancos que crecientemente deciden sus tasas de interés de forma más sofisticada. Dicho contraste podría derivar en consecuencias no deseables por parte de esta regulación. Este estudio examina una reforma importante a la regulación de tasas de interés en el mercado chileno de crédito de consumo, la Tasa Máxima Convencional (TMC), que afectó a los créditos por montos menores a 200 UF y que se implementó entre Diciembre de 2013 y Diciembre 2015. El estudio combina dicha reforma con diversas bases de datos administrativos para llevar a cabo un análisis enfocado en el mercado de crédito de consumo en cuotas, lo que permite considerar múltiples variables de resultados en el análisis y estudiar la heterogeneidad de los efectos sobre distintos grupos de deudores.

Los resultados sugieren que la reforma a la TMC tuvo efectos sustantivos en el mercado del crédito de consumo. Se encuentra que los precios de los préstamos disminuyeron considerablemente en respuesta a la misma. Sin embargo, el volumen de créditos también disminuye en respuesta a la política y parte de tal disminución se explica por una caída en el número de solicitudes de créditos por parte del público. Las estimaciones implican que la tasa de interés promedio en el mercado sujeto al cambio de política—créditos por montos menores a 200 UF—disminuyó en 9%, a la vez que la cantidad de contratos de crédito en dicho segmento del mercado cayó en 19%. Adicionalmente, el análisis arroja que los efectos de la reforma a la TMC se concentran entre los deudores de mayor riesgo. Este trabajo tiene diferencias metodológicas relevantes respecto a investigación previa sobre esta reforma (SBIF, 2017), lo que dificulta una comparación directa entre los resultados de ambos estudios. En el documento se discuten las diferencias con tal análisis en detalle.

Es importante destacar que los efectos de la regulación de tasas de interés son en principio ambiguos, por un lado pueden reducir los precios y beneficiar a los consumidores, mientras que por otro pueden restringir el acceso al crédito y perjudicar a los mismos. En esta línea, si bien este análisis encuentra que la reforma a la TMC disminuyó el volumen de crédito en el segmento del mercado directamente afectado, ello no implica que la misma política o variantes de la misma necesariamente disminuyan el volumen de crédito en otros contextos. Más bien, tal resultado dependerá de las condiciones subyacentes de la demanda y la oferta de esos escenarios.

Este documento proporciona evidencia de los efectos de los límites de las tasas de interés sobre una serie de variables de resultados y dialoga con un cuerpo de investigación relacionado. Sin embargo, cabe mencionar que este análisis no aborda otras variables que podrían eventualmente verse afectadas por esta regulación. En primer lugar, existe una preocupación entre los reguladores acerca del sobreendeudamiento de los hogares y la regulación de tasas de interés podría generar beneficios para los hogares en forma de endeudamiento controlado. En segundo lugar, los hogares podrían ser desplazados a otros oferentes de crédito, entre los cuales la principal preocupación son los prestamistas informales. Al respecto, estudios recientes basados en encuestas muestran discordancia en cuando a la evolución del mercado de crédito informal: ABIF (2018) presenta un aumento en los años recientes, mientras que la comparación de la EFH (2014) y EFH (2018) refleja una nula variación del crédito informal. La medición de los efectos de este cambio de política sobre estas variables proporcionaría una imagen más completa sobre los efectos de la regulación de precios en los mercados de crédito.

Varias preguntas relevantes permanecen abiertas para futuras investigaciones. Por ejemplo, una evaluación del efecto de la regulación de precios sobre el bienestar, el que combinaría el potencial de la regulación de beneficiar o perjudicar a los consumidores a través de menores precios y de menor acceso al mercado. Adicionalmente, no queda resuelta la pregunta si los deudores afectados por la política se mueven hacia otros segmentos del mercado. Para el caso Chileno en particular, esto consideraría estudiar los efectos en otros segmentos del crédito al consumo, incluyendo tarjetas de crédito y líneas de crédito. Se planea abordar estos aspectos en investigación futura.

1 Introduction

Interest rate caps have been employed to regulate credit markets for millenia (Temin and Voth, 2008). Multiple developed and developing countries have some form of interest rate regulation (Maimbo and Henríquez, 2014). Regulators often decide to implement price regulation in order to limit lender usury and market power. Opponents argue that interest rate caps harm access to credit for small firms and consumers and thus may harm growth and welfare. This raises concerns about the consequences of this class of regulation. However, research analyzing this regulation is somewhat limited and discussion about alternative designs for it that may deal with this tradeoff better is rare.

The main arguments present in the policy discussion of interest rate regulation can be captured by a simple conceptual framework in which banks compete for loan contracts with borrowers in an environment with imperfect competition. On the one hand, stronger price regulation limits the ability of banks to exercise market power and therefore provide protection to borrowers whenever the regulation is binding. On the other hand, stronger price regulation may deem segments of the market unprofitable for banks at regulates prices, thus harming credit access for such segments. The welfare implications of interest rate regulation are therefore heterogeneous along the distribution of borrower risk, whereas stronger regulation benefits protected borrowers while harming excluded borrowers. However, despite this trade-off, the simplicity of the design of price regulation for credit markets is striking: few countries have tried policy designs that go beyond having interest rate caps that vary across a few loan size and type brackets (Maimbo and Henríquez, 2014).[1] Moreover, there is no discussion of the role that the competitive environment may have in terms of potential effects of interest rate regulation.

In this paper, we study the equilibrium effects of price regulation in retail credit markets. We start by developing a model of demand and supply for consumer loans that accommodates imperfect competition and adverse selection. This model provides a rich albeit simple conceptual framework that allows for analyzing the effects of price regulation on a range of market outcomes on both the demand and supply sides of the market. We combine it with a convenient empirical application, a policy change in the Chilean credit market that made price regulation on consumer loans substantially stronger. We employ detailed data on loan applications and contracts along with policy variation across loan size and time to provide evidence for the effects of price regulation on different margins of interest, including loan prices and quantities, as well as loan performance.

We exploit the Chilean retail credit market as a setting for empirically studying the implications of interest rate caps, which combines useful policy variation on price regulation with rich administrative data. In particular, we focus on the case of consumer loans in this market, a product held by 15 percent of Chilean households. Price regulation in this market is similar to that in several countries; the regulator sets caps on

[1] 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 percent for payday loans. In Europe, many countries have designs that impose caps at a mark-up over the average interest rate in the market, including Germany and Italy. Other countries, such as Belgium and France, have more sophisticated designs that allow the cap to vary by loan type and size. In the case of Chile, the design imposes differentiated interest rate caps for a small number of loan size brackets.

interest rates to limit loan prices, which vary across a small number of loan size brackets. Interest rate caps were substantially strengthened between Dec 2013 and Dec 2015 in this market. Throughout that period, interest rate caps decreased by between 17 p.p and 24 p.p for loans below $2,000 and between $2,000 and $8,000 respectively, leaving larger loans unaffected. We exploit administrative data on loan applications and contracts signed in this market around this policy change in order to implement a thorough analysis of its equilibrium effects.

The analysis in this paper proceeds in two steps. First, we start by developing an equilibrium model of demand for and supply of consumer loans, where borrowers are heterogeneous in risk, which includes imperfect competition and adverse selection. The model illustrates the consequences of stronger interest rate regulation: relatively risky borrowers are excluded from the market if they become unprofitable under stronger regulation, some borrowers access credit at lower interest rates, and the ability of banks to exercise market power is limited. The magnitude of these effects depends crucially on the distribution of borrower risk and on the competitive environment. The model is useful for interpreting our findings of the effects of price regulation on the Chilean credit market and for discussing the implications that alternative competitive environments or designs of price regulation would have.

In the second part of the paper, we study the effects of stronger price regulation guided by this model. We find that price regulation has strong price and quantity effects when binding. Our analysis exploits variation across time and loan size in treatment intensity to identify policy effects. Our estimates show that stronger interest rate regulation reduced loan prices. The distribution of interest rates shifted to the left in response to stronger price regulation, displaying substantial bunching at the interest rate cap after the policy change. This pattern suggests that banks in our setting hold substantial market power since, under perfect competition, banks would choose not to offer loans exposed to price regulation at rates lower than the interest rate cap. At the same time, we find that stronger price regulation affected loan application behavior on average, with most of such effect being driven by a reduction in loan applications by risky borrowers. However, stronger price regulation did reduce equilibrium credit volume in the market. The magnitude of the full effects of the policy change on quantity is large: the number of loan originations decreased by 38.6 and 21.3 percent for loans of sizes below $2,000 and between $2,000 and $8,000 respectively as a result of stronger price regulation. Moreover, we find evidence of reduced borrower default rates, which decrease by 33 percent as a result stronger price regulation. Effects on all these dimensions are stronger for observably riskier borrowers, who were the most exposed to price regulation given banks engage in risk pricing in this market.

Our results suggest that, when binding, interest rates caps can have strong equilibrium effects on credit markets. The trade-off between consumer protection and credit access on which we focus becomes readily apparent in these results. On the one hand, our estimates imply that 153,037 loans were deterred by stronger price regulation, equivalent to $366 million U.S. dollars in loan contracts per year. On the other hand, average monthly payments decreased by $3.5 U.S. dollars, adding up to an aggregate reduction of $32.9 million U.S. dollars in present value. The fact that quantity effects are concentrated among risky borrowers suggest that the costs and benefits from this policy were unequally distributed and that most of the benefits of it in terms of consumer protection were captured by safer borrowers. We discuss the potential of risk based price regulation as an alternative design for this class of regulation that may deal better with such unequal distribution of cost and benefits along the distribution of borrower risk.

This paper complements ongoing work in which we adopt a structural approach to analyze the implications of price regulation in credit markets in Cuesta and Sepúlveda (2018). In that paper, we estimate our model of demand for and supply of loans using the same data sources we employ for this paper. We then use the estimated model to study equilibrium effects of price regulation along the different channels that are at play in this market and to estimate welfare effects using a revealed preferences approach. We proceed to exploit the estimated model to study counterfactual policy experiments and competitive environments, which include addressing the relevance of market power in determining the effects of price regulation, the effects of designs that feature size-dependent interest rate caps and the effects of risk based interest rate caps.

Related Literature. This paper contributes to two different branches of the literature. First, it contributes to a literature that focuses directly on the effects of price regulation on credit market outcomes. Papers in this line of research have found mixed effects from these policies on credit volume. The most recent research, however, has mostly found negative effects on that margin when the policy has been binding (Bodenhorn, 2007; Temin and Voth, 2008; Benmelech and Moskowitz, 2010; Zinman, 2010; Rigbi, 2013; Fekrazad, 2016; Melzer and Schroeder, 2017; Safavian and Zia, 2018). Many of these recent papers have focused on the market for payday loans in the U.S. In many cases, they focus on empirical applications based on a single lender or a single market. Meanwhile, this paper contributes to this literature by studying the role that interest rate regulation plays in determining equilibrium outcomes using rich contract level data for a large market. Moreover, we emphasize the role of two elements that are pervasive in credit markets, imperfect competition and borrower risk heterogeneity. The empirical application we focus on has also been previously studied by Hurtado (2015), SBIF (2017b) and Schmukler et al. (2018). To some extent, this paper also contributes to research that studies regulation on other margins of contract pricing in credit markets (Agarwal et al., 2015; Nelson, 2018; Benetton, 2018), and to a literature that focuses on the welfare implications of access to expensive credit, which has found mixed effects (Melzer, 2011; Morse, 2011; Skiba and Tobacman, 2015).

Second, this paper is broadly related to a recent literature that has focused on the empirical analysis of selection markets with imperfect competition. on adverse selection in credit markets. This literature has emphasized that the effects of different policies on selection markets depend on the extent of competition in them (Veiga and Weyl, 2016; Mahoney and Weyl, 2017; Lester et al., 2017). Recent papers have developed empirical models that allow for adverse selection, product differentiation, and imperfect competition explicitly. Relevant empirical applications along these lines for credit markets are those by Agarwal et al. (2017), Crawford et al. (2018), Allen et al. (2017) and Benetton (2018). Our paper embeds imperfect competition in cost heterogeneity across banks and studies equilibrium effects of price regulation on different margins of the market, including loan applications, prices, quantities and loan performance.

Outline. The remainder of the paper is organized as follows. In Section 2, we describe the setting where we implement our empirical application and describe the data we employ for it. In Section 3, we develop a conceptual framework for credit markets, which we employ to interpret our main findings for the effects of interest rate caps. In Section 4, we provide evidence for the policy effects of price regulation in our setting. In Section 5, we provide a discussion of the implications of our findings for the design of price regulation. Finally, Section 6 concludes.

2 The Chilean Credit Market

We exploit the Chilean credit market as an empirical application to study the effects of price regulation in credit markets. Within such setting, we focus on consumer loans. Consumer loan contracts can be characterized basically by their interest rate, term and amount. Banks require no collateral on these loans. Every year, around 1.5 million such contracts are signed, adding up to a credit volume higher than 12 billion U.S. dollars. There are 15 banks that offer consumer loans in the retail credit market, which also offer other credit products. The market is somehow concentrated, and the market share of the three largest banks adds up to more than 50 percent across the country.[2] 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). These products are under the same price regulation described in Section 2.1 below. Payday loans, a relevant source of expensive credit in other countries, have not developed at scale in Chile.

Regarding risk assessment by banks, there are no marketwide risk scores in this setting, different to the case of FICO scores in the U.S. Instead, there are three sources of information that banks use for this task, namely (ii) comprehensive hard information on consumer covariates covering demographic characteristics and credit history across all banks in the market that the regulator gathers and provides to banks; (ii) additional soft information banks may ask borrowers for when loans upon applications, and (iii) information from risk scoring services provided by private firms, which often include information on repayment of debt disclosed by department stores and non-bank lenders. As mentioned below, we have access to the first of these sources of information for our empirical application. We use this data to construct measures of predicted default risk.

Consumer debt has a high penetration in Chile. The 2014 Survey of Household Finance, conducted by the Central Bank of Chile, provides a picture of the relevance of consumer loans for household finance (EFH, 2014). As much as 63 percent of households hold some form of consumer debt and 15.4 percent hold consumer loans. Consumer loans are employed by households towards different objectives. The share of households holding consumer loans for different usages varies as follows: 54 percent for household durables, 30 percent for clothing, 22 percent for debt consolidation, 11 percent for vehicles, 9 percent for medical treatment, 9 percent for households improvement and 5 percent for vacations (EFH, 2014). Among

[2]Banks are not the only type of institution that provide consumer loans in our market. However, they account for as much 92 percent of the total credit volume of consumer loans, which make them the most relevant kind of institution in the market.

households holding consumer debt, the average debt to income ratio is around 5 and every month households allocate 24.9 percent to debt repayment (SBIF, 2017a).

2.1 Interest Rate Regulation

Price regulation in the Chilean credit market is not new. Since 1929, several versions of interest rate caps on credit products have been in place.[3] We focus on a policy change enacted by Law 20,715, which aimed at further protecting vulnerable borrowers and easing 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 covered 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 under study introduced changes both in the definition of loan size categories for interest rate caps and on the formulas designed for the calculation of them. Interest rate caps are always measured in terms of annualized interest rates. Loan size categories are defined in UF (*Unidades de Fomento*), an inflation adjusted monetary unit commonly used in Chile.[4]

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

$$\bar{p}_{st} = \phi \tilde{p}_{st-1} + \alpha_{st} \tag{1}$$

such that caps $\bar{p}_{st}$ are set as a combination of proportional and constant mark-ups over a reference rate $\tilde{p}_{st-1}$. Before the policy change, only two loan size categories where considered by the regulation, namely \$0-\$8,000 and \$8,000-\$200,000. For both categories, the regulation considered $\phi = 1.5$ and $\alpha_{st} = 0$. The reference rate $\tilde{p}_{st-1}$ was calculated as a weighted average of interest rates on loans of size s during the previous month.[5] 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 respectively for loans in the \$0-\$8,000 and \$8,000-\$200,000 size categories respectively.

The discussion towards a reduction in interest caps imposed by this policy started in 2011, which led to posterior legislation in Congress and introduction in December 2013. The views express by policymakers and economists in the press reflected the trade-off we highlighted in the introduction of the paper. On the one hand, there was a widespread view that interest rates on consumer loans in the market were high, in

[3]For further detail on the evolution of interest rate regulation in the Chilean credit market, see Flores et al. (2005), Hurtado (2015) and SBIF (2017b).

[4]According to the Central Bank of Chile, one UF was equivalent to 39.4 U.S. dollars on June, 30th, 2016, at the end of our sample period. 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 this two thresholds as \$2,000 and \$8,000 respectively for expositional simplicity. All analyses, however, are conducted without such approximation.

[5]Throughout the analysis, we ignore potential strategic incentives banks may have to increase loan interest rates in order to increase the reference rate $\tilde{p}_{st-1}$.

particular for poor households. For instance, this view is reflected by this quote by a Senator who played an active role in the discussion:

> "Price regulation aims at **supporting** highly indebted households, which are usually poor and in many case pay **usurious** interest rates"
>
> *Eugenio Tuma*
> *El Mostrador, 2011*

However, on the other hand, there was a view that the level of interest rate caps is crucial for its equilibrium consequences, with the potential for effects on credit access if its level is too low. This view is reflected by this quote by the former Governor of the Central Bank of Chile:

> "If the price cap is set too low, it may **exclude** borrowers from the system; whereas if it is set too high, it looses its potential for **consumer protection**"
>
> *José de Gregorio*
> *La Tercera, 2011*

The reform made four changes to the previous regulation. First, it divided the \$0-\$8,000 size category in two, namely \$0-\$2,000 and \$2,000-\$8,000.[6] Second, it set $\phi = 1$ for these categories and left $\phi = 1.5$ for the \$8,000-\$200,000 category. 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 category for all size categories. Thus, 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 categories, while leaving the regulation for loans in the \$8,000-\$200,000 category unchanged.

Had the policy been fully enacted by the Dec 2013, interest rate caps would have fallen instantaneously by 16.9 p.p and 23.9 p.p for loans in the \$0-\$2,000 and \$2,000-\$8,000 categories respectively (SBIF, 2017b). In order to avoid such sharp decrease, a smooth transition was included in the design of the policy. 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 α_{st}. Under such calendar, the policy was fully in place by Dec 2015. Figure 1 displays the evolution of interest rate caps after the reform. The reduction in caps for the \$0-\$2,000 and \$2,000-\$8,000 size categories is stark, and the difference between the former and the latter is of 7 p.p. In contrast, the cap on larger loans has remained roughly constant over the period of study. We exploit these features as identifying variation to study the effects of this regulation below.

[6]This component of the design is related to considerations about risky borrowers being potentially excluded from the credit market by these regulation. Exclusion was indeed a relevant part of the discussion around the approval of this policy. Allowing for a less strict regulation on the smaller loan size category aimed at reducing such concern.

2.2 Data

We exploit large administrative datasets collected by the regulator of the Chilean credit market, the Superintendence of Banks and Financial Institutions (*Superintendencia de Bancos e Instituciones Financieras*, SBIF). The data covers the period between Jan 2013 and Jun 2016, which subsumes the roll-out of the policy change in price regulation described in Section 2.1 above. Our analysis exploits two main administrative datasets: one that contains every loan contract signed and one that provides a large sample of loan applications by borrowers. In both cases, we are able to merge additional datasets describing the demographics and credit history of the population of borrowers.

2.2.1 Loan Contracts Dataset

The first dataset we employ is a registry of all consumer loan contracts signed in the Chilean credit market during the period of study. This dataset has several remarkable features. First, both borrower and bank identifiers are available for each loan contract in the data, along with relevant information on contract characteristics, including loan interest rate, amount and term.[7] Second, the dataset tracks the performance of each loan contract, which allows us to observe occurrence and date of loan default. Third, the dataset provides relevant borrower attributes, including age, gender, income and county of residence. Fourth, the dataset collects the full credit history of each borrower in the system. These variables includes amount of consumer and mortgage debt held, and amount of such debt that is under 90-day default. Importantly, this is the same information that is provided by the regulator to banks in order for the latter to assess borrower risk and covers the relationships between each borrower and all banks in the market. In the absence of marketwide risk scores in the Chilean credit market, we exploit this information in order to construct risk scores for our analysis in Section 2.2.3 below. The fact that banks employ this same information in assessing borrower risk reinforces our approach. For convenience, we measure all monetary variables in U.S. dollars and all interest rates in annualized terms.

2.2.2 Applications Dataset

The second dataset we utilize for our analysis covers consumer loan applications for the period of study. While the loan contracts dataset covers the whole market, the coverage of the application dataset is only partial, as reporting practices by banks for this data were not as rigorous. Applications can be linked to the loan contracts dataset using borrower identifiers. As a measure of the coverage of the applications dataset, we are able to match application events for 64.5 percent of loan contracts in the data. 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 and accepted by the borrower, we also observe the loan interest rate. For each application, we are able to merge the same borrower attributes and credit history available for the loan contracts dataset.

[7] All interest rates described and employed in analyses in the paper correspond to annualized interest rates.

We organize the applications dataset by constructing application events. We construct application clusters of a given borrower across potentially multiple banks in a short period of time. Concretely, we define an application cluster as a set of applications by a borrower such that no pair of applications are more than eight days apart. We then merge these application events to loan contracts using borrower and banks identifiers.

2.2.3 Measuring Credit Default Risk

We exploit the availability of data on loan performance, consumer covariates and credit history in order 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 has the form:

$$P(d_i = 1) = \Lambda(x_i'\gamma)$$

where d_i is an indicator for loan default over the term of loan i and x_i is a vector of borrower covariates determined before signing the reference loan contract. This is a standard risk scoring model (Ohlson, 1980). We consider different sets of variables in x_i. We start with borrower income and leverage, we then add borrower credit history variables, and end up adding basic demographics and macroeconomic controls. The set of features we use for prediction of default risk is similar to that employed by Liberman et al. (2018) in their recent paper, which also studies the Chilean consumer credit market.

Table A.1 displays estimates of different specifications of this model.[8] Overall, results point in the expected directions: borrowers with higher income and lower leverage default less frequently. Regarding credit history variables, 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 without mortgage debt under default are less likely to default. In terms of demographics, both older and female borrowers are less likely to default. The model has reasonable predictive accuracy: it predicts 69 percent of loan defaults correctly out of sample. We construct our measure of credit default risk as the fitted probabilities from this logit model. 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. Figure A.1 displays relationships between relevant market outcomes and our measures of predicted risk. Figures A.1-a and A.1-b display negative relationship between predicted risk and approvals; while Figures A.1-c and A.1-d display positive relationships between interest rates and predicted risk. Finally, Figures A.1-e and A.1-f displays positive relationships between realized and predicted default.

2.3 Descriptive Statistics

Using the previously described administrative data, we are able to describe the evolution of the consumer loans market in high detail. The dataset contains almost four million loan contracts. Table 1 displays

[8]For the rest of the paper, we utilize the results from a model that splits all continuous covariates in ventiles and includes indicators for such ventiles as regressors. The objective of using a more flexible model is to accommodate potential non-linearities in the relationship between covariates and default.

summary statistics for this dataset. There is substantial dispersion in loan characteristics in the data. Average annualized interest rates are around 20 percent, but more than ten percent of the loans in the dataset are provided at rates higher than 35 percent, which is partly what motivated the implementation of the regulation we study.[9] The average loan in the sample is of around $6,800 and 33 months long, with substantial variation in these attributes. The average monthly payment for loans in the sample is 268, with substantial dispersion driven by heterogeneity loan attributes.[10] In terms of loan performance, five percent of borrowers default on payments on the first year of the loan and nine percent of them do so through the loan term.[11] The average predicted default risk by our model in the previous Section is 0.11 for both the income and history models, with most of the predicted values being under 0.2.

There is substantial variation in the population of borrowers in this market. The average borrower has an annual income of $19,300 and is 44 years old. Moreover, 40 percent of borrowers are females. Most of loan contracts in our data are signed by consumers previously in the financial system, and 76 percent of them are signed with a bank of which the borrower was previously a customer. In terms of credit history, the average consumer holds around $7,080 in consumer loans and $12,600 in mortgage debt. Most of borrowers sign one consumer loan contract throughout our sample period, with a median of one and an average of 1.86. Finally, borrowers in the system hold relationships with multiple banks, and the average borrower is customer of almost three banks.

Our applications dataset collects almost 3.7 million application events. Loan amounts in the applications dataset are larger than those in the loan contracts dataset and loan terms are slightly larger as well. In terms of outcomes, as much as 72 percent of applications are approved and accepted by applicants, while 10 percent of applications are approved but actually not accepted by applicants and therefore do not lead to a loan contract. The remaining 18 percent of applications are rejected by banks.

Borrower risk plays an important role in this market. We provide descriptive evidence for such by studying correlations between a set of relevant borrower and bank choices and our measure of borrower risk. Table 2-A shows that riskier borrower are more likely to apply for loans. Among applicants, banks tend to reject riskier borrowers more often, as shown by Table 2-B. Conditional on loan approval, banks implement risk pricing, offering higher interest rates to riskier borrowers, as displayed in Table 2-C. Finally, Table 2-D shows that, conditional on signing a loan contract, riskier borrowers are more likely to default on loan repayments.

---

[9]Most of the price dispersion is cross-sectional. While there is variation in the funding cost of banks through time, only 1.2 percent of the variation in interest rates can be explained by monthly dummies. See Figure A.2 for the evolution of bank funding cost through our 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 percent of the variation.

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

[11]These default statistics include loans originated towards the end of the sample period, which shades the averages of the variables. Averages default on the first year and overall for loans originated at least a year before the end of our sample are five and ten percent.

3 Conceptual Framework

In this Section, we develop an equilibrium model of applications, pricing and default in the market for retail credit. Borrowers are heterogeneous in risk and only part of that risk is observable to banks. Borrowers start by choosing whether to apply for loans. If so, they then shop for loans across banks in the market. Upon signing a contract, they may default on repayment. The model allows for adverse selection, in that riskier consumers may value loans more. Banks offer loan contracts to borrowers and are able to negotiate different contracts across borrowers. If expected profits of a loan contract are negative, banks reject that loan application.

Equilibrium prices are modeled as the result of an English auction, were banks compete for borrowers who bargain with banks for lower loan prices. The benefit of this approach is that it provides a tractable model that accommodates price dispersion and imperfect competition. This approximation is isomorphic to modeling the market as a standard Bertrand game where firms with heterogeneous production costs sell homogeneous goods. This approach has been recently employed for modeling markets with bargained prices (Allen et al., 2017; Salz, 2017). Under this framework, the source of market power for banks in our model is cost heterogeneity, which translates in loan prices being set at a mark-up over expected costs, similar to the interpretation of market power in Petersen and Rajan (1995). In this version of the paper, we do not study in isolation other sources of market power such as search frictions, which have been the focus of recent research on credit markets (Woodward and Hall, 2012; Agarwal et al., 2017; Allen et al., 2017).

3.1 Motivating Facts

We start by documenting relevant facts of the market under study. With these facts in hand, we then propose a model of the characteristics outlined above.

Fact #1: There is substantial price dispersion. The setting we study displays substantial variation in loan prices, as displayed by Figure A.3. After residualizing interest rate margins of interacted month, bank, location, loan size, term and borrower risk fixed effects, as much as 26 percent of the variation in interest rate margins remains unexplained. There is thus substantial price dispersion even within remarkably narrow segments of the market where only similar loans for similar borrowers within a given month, bank and location are compared. The standard deviation of residualized interest rate margins remains high at 3.9 p.p, slightly less than a third of the unconditional standard deviation in interest rates margins. One potential source of price dispersion within observably similar contracts is discretion of banks' sales agents and bargaining over prices.

Fact #2: Banks reject applications from observably riskier borrowers. Loan approval by banks depends on previous relationships with borrowers. Table 2-B displays results from regressions of an indicator for loan application approval on different covariates. Applications from borrowers with higher predicted default risk are less often approved. This is also documented by Figure A.1-a.

Fact #3: Previous relationships are a shifter of bank choice. There is substantial variation in the number of relationships held by borrowers, and few contracts are signed by borrowers new to the system, as shown by Figure A.4-a. On the other hand, Figure A.4-b shows that the likelihood of signing a given loan contract with a related bank is high overall. Moreover, that likelihood increases with the number of previous relationships held by the borrower and decreases with borrower risk. Part of these patterns may operate through the fact that applications from previously related borrowers are more often approved, as documented in Table 2-B. These patterns suggest that previous relationships shift bank choices by borrowers.

Fact #4: Loan contracts with observably riskier borrowers are signed at higher prices. Banks engage in risk pricing in the market, through offering higher loan prices to observably riskier borrowers. Table 2-C displays results of regressions of interest rate margins over banks' funding cost on different sets of covariates. Loan interest rates are increasing in borrower predicted default risk. This is also documented by Figure A.1-b.

Fact #5: Loan contracts with previously related borrowers are signed at lower prices. Even after conditioning on loan attributes, borrower default risk and other covariates, previous relationships between borrowers and banks affect prices: borrowers that were previously related to a bank receive loan prices that are lower on average, as shown by Table 2-C.

The model we develop in this Section is consistent with many of these facts. Modeling the supply side of the market as an English auction leads to equilibrium price dispersion if there is cost heterogeneity across banks. We introduce cost heterogeneity in ways suggested by these facts, namely by (i) allowing for expected default cost to vary across borrowers according to borrower observables, by (ii) allowing bank cost for a given borrower to vary across banks, and by (iii) allowing bank cost to depend on previous relationships with borrowers, thus introducing the potential for incumbency advantages. Additionally, we allow for adverse selection on the demand side.

3.2 Setup

There are N consumers in the market, each denoted by $i \in \mathcal{I}$, where $\mathcal{I}$ is the set of consumers in the market. There are J banks in the market, denoted by $j \in \mathcal{J}_i$, where $\mathcal{J}_i$ is the set of banks in the market of borrower i. The model is static and we focus on a month in the span of a loan contract, such that borrowers' utility, loan prices and banks' cost are measured at the monthly level. In the model, borrowers demand loans and shop across banks for the lowest interest rate. We approximate banks' competition to sign a contract with the borrower as an English auction. Borrowers demand loans of a given amount and term (L_i, T_i), determined in a previous stage that we do not model, such that loan contracts are characterized only by the offered interest rate or corresponding monthly payment. The structure and timing of the model is summarized in

Figure A.5.

Borrowers. Borrowers are endowed with observable characteristics x_i and unobservable characteristics ε_i. Let $\theta_i = (x_i, \varepsilon_i)$ summarize borrower type. The vector x_i collects all publicly available information in the market, including risk scores, borrower income, borrower credit history, among others.

Borrowers decide whether to shop for loans or not. If they decide to shop for loans, they incur in an application cost κ_i and choose between the best contract offer they obtain from banks and their outside option. If they choose not to shop for loans, they obtain their outside option. Let the indirect utility from a loan contract and the outside option be:

$$\begin{aligned} u_{Li} &= v_L(\theta_i) - p_i \\ u_{Oi} &= v_O(\theta_i) \end{aligned}$$

where $v_L(\theta_i)$ is the monthly willingness to pay for a loan, p_i is the monthly payment offered by a given loan contract and $v_O(\theta_i)$ is monthly willingness to pay for the outside option. Borrowers then choose to apply for loans by comparing the expected value of both options, u_{Ai} and u_{NAi}, given by:

$$\begin{aligned} u_{Ai} &= P_{Li} \underbrace{\int \max\{u_{Li}, u_{Oi}\} f_{p|L}(p) dp}_{\text{Value of approval}} + (1 - P_{Li}) \underbrace{u_{Oi}}_{\text{Value of rejection}} - \kappa_i + \varepsilon_{Ai} \\ u_{NAi} &= u_{Oi} + \varepsilon_{NAi} \end{aligned}$$

where P_{Li} is the probability that the borrower receives an approval by some bank in the market and therefore a contract offer, and where the borrower integrates the value from a loan contracts over the density of loan prices they face conditional on obtaining an approval, which we assume is known to them and denote by $f_{p|L}(p)$. Both ε_{Ai} and ε_{NAi} are preference shocks to the value that borrowers obtain from applying or not applying for loans.

Given this structure, a borrower decides to apply for a loan whenever the expected utility of doing so is higher than the expected utility of remaining out of the credit market, which is:

$$P_{Ai} = P\left(P_{Li} \int \max\{u_{Li} - u_{Oi}, 0\} f_{p|L}(p) dp - \kappa_i + \varepsilon_{Ai} \geq \varepsilon_{NAi}\right)$$

from where it is clear that application decisions are driven by (i) the approval probability, (ii) the expected gains from a loan contract relative to the outside option, (iii) the distribution of loan prices conditional on approval, and (iv) any application costs that borrowers may face. Let $a_i = 1\{i \in \mathcal{A}\}$ indicate that borrower i applies for a loan, where $\mathcal{A}$ is defined as the set of loan applicants.

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

$$u_{Li} = \max_{j \in \mathcal{J}_i} \quad v_L(\theta_i) - p_{ij}$$

such that bank choice is driven solely by monthly payment, given there is no differentiation across banks in terms of the utility they provide to borrowers. As further detailed below, all differentiation is concentrated in banks' costs.

Loan Repayment. After signing a loan contract, a borrower may choose to default on loan payments. Let $s_i \in [0, 1]$ measure the share of payments made by borrower i relative to the total number of payments in the loan contract:

$$s_i = s(\theta_i, L_i, T_i)$$

which is a function of borrower characteristics and non-price contract terms. Loan price p_{ij} does not enter in the utility from default, which implies that endogenous default is not considered in this model. We depart on this aspect from recent work on credit markets, including Adams et al. (2009) and Holmstrom and Tirole (1997). While restrictive, this assumption simplifies greatly the analysis of pricing behavior by banks we develop below. Moreover, recent experimental evidence in Castellanos et al. (2018) suggests moral hazard might not a be first order concern in consumer credit markets. Finally, there is adverse selection in the model if the choices of applying for loans and the length of repayment are negatively correlated in a way unobservable to the bank, i.e. through ε_i.

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 may make it less costly for a bank to serve some borrowers than others, and (iii) repayment risk.[12] We combine the first two components in $c_{ij} = f_i - \omega_{ij}$. In terms of repayment risk, banks consider it when pricing loan contracts. Banks observe x_i and the pool of loan applicants $\mathcal{A}$, which they employ in order to estimate repayment risk.

Bank profits from a loan contract are calculated as the difference between a stream of monthly payments with repayment risk and a stream of monthly bank costs. Let $\rho(r_j, T_i) \equiv \frac{1}{r_j}(1 - \exp(-r_i T_i))$ be a present value operator that discount a stream of payments for T_i months at a discount rate r_j and, similarly, $\rho(r_j, S_i) \equiv \frac{1}{r_j}(1 - \exp(-r_i S_i))$ be a present value operator that discount a stream of payments for $S_i = s_i T_i$ months, where S_i is repayment length by borrower i. Given these assumptions and notation, the expected profit from a given loan contract at price p_{ij} is:

$$\pi_{ij} = E[\rho(r_j, S_i)] p_{ij} - \rho(r_j, T_i)(f_i - \omega_{ij})$$

where it is important to note that repayment risk and funding cost depend only on borrower-specific attributes, while match value ω_{ij} depends on bank-borrower attributes. The role of ω_{ij} is thus to introduce cost heterogeneity across banks and can be thought of as a term measuring the match value of a potential contract. The latter could capture bank-borrower relationships and bank convenience in local markets,

[12]Funding cost f_i is borrower-specific when computed at the monthly level, as it depends on loan amount and term. However, the funding rate is constant across borrower.

among others 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 chooses to offer a loan contract with a borrower whenever $E[\pi_{ij}|x_i] \geq 0$. Otherwise, the bank rejects the borrower. If the borrower repayment risk is increasing in observable risk, then expected profits are decreasing in risk at a given price and observably riskier borrowers are less likely to get a loan application approved by a given bank. Application choices by borrowers and approval decisions by banks are connected. Given banks observe x_i, borrowers reveal information about their unobservable type ε_i by applying for a loan. Banks incorporate that information in their approval decision by adjusting expected repayment risk accordingly.

Price Regulation. We introduce price regulation in the model in the form of a price cap. In particular, banks are not allowed to charge monthly payments higher than a cap set at $\bar{p}$.

3.3 Equilibrium

There are two elements that characterize equilibrium in this model, which are the pool of applicants and loan prices. In absence of price 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.[13] The solution to:

$$E[\pi_{i(2)}|x_i] = E[\rho(r_j, S_i)]b_{i(1)} - \rho(r_j, T_i)c_{i(2)} = 0$$

is thus the equilibrium unconstrained price:

$$p_i^u = \frac{\rho(r_j, T_i)}{E[\rho(r_j, S_i)]}(f_i - \omega_{i(2)})$$

which, as expected, is increasing in repayment risk and funding cost, and decreasing in match value of the closest competitor.[14] This price yields equilibrium expected profits $E[\pi_{i(1)}|x_i] = \rho(r_j, T_i)(c_{i(2)} - c_{i(1)}) = \rho(r_j, T_i)(\omega_{i(1)} - \omega_{i(2)})$, from where it becomes clear that the source of banks' market power in this model is given by cost advantages.

In presence of price regulation as described above, there are three potential outcomes for a borrower

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

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

$$p_i^u = \underbrace{\frac{\rho(r_j, T_i)}{E[\rho(r_j, S_i)]}}_{\text{Risk adjustment}}(\underbrace{f_i - \omega_{i(1)}}_{\text{Mg. Cost}} + \underbrace{\omega_{i(1)} - \omega_{i(2)}}_{\text{Mark-up}}) \quad (2)$$

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 contract relative to its closest competitor.

trying to get a loan in this market. If not binding, then the loan contract is signed at the unconstrained price. If price regulation is binding, however, the unconstrained price is higher than the price cap, $p_i^u > \bar{p}$.[15] In such case, the lowest cost bank will extend the loan at price equal to $p_i = \bar{p}$ as long as $E[\pi_{i(1)}|x_i] = \bar{p} - c_{i(1)} \geq 0$. If the cost of the lowest cost bank is high enough as to make lending at the price cap unprofitable, $c_{i(1)} > \bar{p}$, the bank will choose to reject the borrower. Equilibrium loan prices for borrowers that apply for a loan under price regulation are therefore:

$$p_i^* = \begin{cases} p_i^u & \text{if } p_i^u \leq \bar{p} \\ \bar{p} & \text{if } p_i^u > \bar{p}, \frac{\rho(r_j,T_i)}{E[\rho(r_j,S_i)]} c_{i(1)} \leq \bar{p} \\ \cdot & \text{if } \frac{\rho(r_j,T_i)}{E[\rho(r_j,S_i)]} c_{i(1)} > \bar{p} \end{cases} \tag{3}$$

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

3.4 Effects of Price Regulation

Application Behavior. What are the implications on the demand side? Stronger price regulation affects borrower application behavior through two mechanisms. On the one hand, it reduces the approval probability of a loan application. However, approved loan applications will be offered contracts at weakly lower prices. We denote these effects as access and protection effects, as they highlight the trade-off related to price regulation. These incentives jointly determine the effect of price regulation on borrower application behavior as follows:

$$\frac{du_{Ai}}{d\bar{p}_i} = \underbrace{\frac{\partial P_i^L}{\partial \bar{p}_i} E_{p|L}[\max\{u_{Li}, u_{Oi}\}]}_{\text{Access effect } (\geq 0)} + \underbrace{P_i^L \frac{\partial E_{p|L}[\max\{u_{Li}, u_{Oi}\}]}{\partial \bar{p}_i}}_{\text{Protection effect } (\leq 0)}$$

which is in principle ambiguous and will depend on which of the two incentives dominate. For instance, if the approval probability by banks decreases sharply in response to stronger price regulation but loan prices conditional on approval do not respond as strongly, then borrowers will likely choose to apply for loans less often. If the case is the opposite, and the effects on approval probability are small relative to those on expected prices,borrowers might choose to apply more often for loans. Finally, if regulation is not binding, then it should not affect the approval probability nor expected prices, and therefore should have no effect on applications. The overall effect of stronger application behavior is ambiguous and will depend on the relative value that consumers place on approval probability and expected prices, as well as on the determinants of those.

[15] Note that for price regulation to be binding, it must be the case that only one bank $j \in \mathcal{J}_i$ has a cost below the price cap. Otherwise, competition by other banks would drive price below the price cap, making the latter non-binding.

Banks' Lending. We consider now how price affects pricing and approval incentives for banks. The effect of stronger price regulation on bank expected profits will depend on whether price regulation is binding. For loan applicants that were already in the market, the effect on profits is given by:

$$\frac{dE[\pi_{ij}|x_i]}{d\bar{p}} = \begin{cases} E[\rho(r_j, S_i)] & \text{if } p_i^* = \bar{p} \\ 0 & \text{if } p_i^* = p_i^u \end{cases}$$

such that profits decrease in response to stronger price regulation whenever price regulation is binding and are unaffected whenever it is not binding. There are two possible scenarios for the former set of loan applicants. On the one hand, banks may choose to sign those contracts, as long as they yield non-negative profits. On the other hand, banks may choose not to approve those contracts if they yield negative profits at the lower price cap. Given borrower expected profitability is decreasing in observable risk at a given price, the probability that a bank decides to reject a loan application under stronger price regulation is increasing in observable risk x_i.

Heterogeneity across Consumers. Four sets of borrowers can be identified by the effects of price regulation on them. First, consumers who remain in the market under stronger price regulation are *protected* and increase their consumer surplus. That is, the policy translates in a transfer from banks to borrowers in the amount of the change in the price cap. Second, consumers who become *excluded* of the market either through them being discouraged to apply for loans or through them having their applications rejected under stronger price regulation experiment a loss of consumer surplus. Third, consumers who enter the market under stronger price regulation are *included*. These are consumers that experiment an improvement in their expected loan prices due to stronger price regulation without a strong enough decrease in their approval probability, such that they are induced to enter the market and apply for loans. Finally, consumers for which stronger price regulation does not change their approval probability nor their expected loan prices, are *unaffected*, for which consumer surplus does not change.

Welfare Effects. The ex-post effects of stronger price regulation on welfare will 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 in principle ambiguous.[16] Importantly, note that in a setting without market power, there would be no borrowers that are protected by the policy, as all marginal borrowers would become unprofitable for banks under stronger price regulation.

From an ex-ante perspective, the effects of stronger price regulation are ambiguous and determined by the same forces as the effects on application behavior, namely an access effect and a protection effect. For every borrower, the effect on application behavior will have the same sign as that on expected consumer surplus.

[16]Sorting into the market for loans on the basis of unobservable risk is relevant for welfare implications of price regulation, as welfare effects depend on borrower risk. If there is adverse selection and thus the value borrowers place on loans is correlated with their risk, then excluded borrowers will likely be ones with relatively high willingness to pay for loan contracts.

Loan Performance. Price regulation also has implications for borrower default. If stronger price regulation improves the risk of the borrower pool through rejecting marginally (observably) riskier borrowers, then the aggregate default rate in the market would decrease under stronger price regulation. In this model, where prices do not directly affect default, the effect of price regulation on aggregate loan performance is thus purely compositional. In a framework with such kind of moral hazard, price regulation would further improve loan performance through causally decreasing default rates by reducing loan interest rates.

4 The Effects of Price Regulation

In this Section, we provide evidence for the policy effects of price regulation policy in the Chilean credit market. As described in Section 2.1, this policy strongly decreased interest rate caps on loans, in magnitudes that differ according to loan size. Using different approaches, we provide evidence for price, quantity and risk composition effects. Throughout this Section, we emphasize heterogeneity in policy effects across borrower risk. In doing so, we split the sample according to the terciles of predicted default risk before the reform and estimate policy effects for low and high risk borrowers.

4.1 Descriptive Evidence

The regulation we study reduced the level of interest rate caps between Dec 2013 and Dec 2015 for loans smaller than $8,000. As a first piece of evidence, we visually inspect the evolution of loan interest rate for different loan size categories. Figures 2-a, 2-c and 2-e display the evolution of the distribution of prices in the market for loans in the $0-$2,000, $2,000-$8,000 and $8,000-$20,000 size categories respectively, along with the evolution of the interest rate cap for each of those, for months around the implementation of the reform. There are two relevant aspects to these figures. First, note that the interest rate caps were not binding for most of the loans in the treated size categories. Second, after Dec 2013, as interest rates caps were reduced, they became increasingly binding for these groups. Loans larger than $8,000, the extent to which interest rates were binding did not change noticeably over the period of study.

To further illustrate the price effects of interest rates caps in this setting, we compare the distribution of loan interest rates before the policy was implemented and after it was fully in place. Figures 2-b, 2-d and 2-f overlay the distribution of loan interest rates the month before the policy was implemented with the month exactly two years after that, when the policy was fully in place, for each relevant loan size category.[17] It is easy to note how for treated loan size categories, a substantial share of the distribution was displaced downwards by the policy, inducing substantial bunching of interest rates at the respective interest rate caps. For loans under $2,000, 42 percent of loans were exposed to the policy, while for loans in $2,000-$8,000 as much as 23 percent were so. In contrast, only 8 percent of loans in $8,000-$20,000 were exposed to the policy, and they were only marginally so. This response of interest rates suggests there is imperfect

[17] In particular, the period we utilize for this exercise covers the second half of November and the first half of December of 2013 and 2015 respectively. The pattern we find remains the same when utilizing longer time periods.

competition in this market. If there was perfect competition, then banks would have chosen not to keep offering *exposed* loans after the policy was in place, as those loans would not yield positive expected profits. Overall, these patterns suggest that banks in this market had market power, which allowed them to charge loan prices above expected costs.

Exposure also varies across borrower risk. Figure A.6 displays similar measures of exposure for three borrower risk groups within each policy size bracket. There is substantial heterogeneity in exposure across borrower risk groups, with the same pattern across policy size brackets. As much as half of high risk borrowers signing loan contracts in $0-$2,000 were exposed to the policy, whereas only less than 15 percent of low risk borrowers signing such contracts were exposed. Similarly, a third of high risk borrowers in the $2,000-$8,000 size bracket were exposed to the policy, while less than five percent of low risk borrowers in such bracket were exposed to it. Overall, these patterns suggest that exposure to price regulations was increasing in borrower risk. This is as expected in a context in which there is risk pricing.

This descriptive evidence suggests that as interest rate caps were strengthened, the equilibrium distribution of loan interest rates responded by bunching below the interest rate cap. Moreover, they suggest that riskier borrower were more strongly impacted by the policy. This is not surprising and simply implies that the regulation was enforced. However, the magnitude of the effects is large and understanding its effects on other equilibrium outcomes and the mechanisms behind those is relevant. We address these aspects in the rest of the paper.

4.2 Empirical Strategy

The design of the policy change we study provides different sources of variation that are useful to measure its effects. First, it provides variation across time in the magnitude of price regulation. Before Dec 2013, regulation was in place but was not binding in practice for loans smaller that $8,000. After that date, price regulation became increasingly binding until the policy was fully in place by Dec 2015. Second, it provides variation across loans of different sizes. Regulation became more binding for loans in the $0-$2,000 category than for those in the $2,000-$8,000 category, and so it it did for the latter than for those in the $8,000-$20,000 category, which remained basically untreated. We exploit these two sources of variation in order to measure equilibrium effects of the policy.

For our analysis, we aggregate the data in order to measure effects across the market. In doing so, we construct bins for loan attributes and aggregate the data at that level. Concretely, we define loan size bins in intervals of 50 UF ($2,000) and employ a clustering algorithm to classify loan term in eight bins. This adds up to 64 loan type bins, where type is defined as a combination of loan size and term. We then compute average or aggregate levels of the outcomes of interest for each product type and month in our sample. To study heterogeneous effects, we implement the same procedure but for each of the three borrower risk bins.

In order to exploit the time variation in the implementation of the policy and to ease the interpretation of the results, we define the following treatment intensity variable:

$$\Delta^{\bar{p}}_{s,t} \equiv (\bar{p}_{s,0} - \bar{p}_{s,t}) - (\bar{p}_{>8000,0} - \bar{p}_{>8000,t})$$

for each of treated size categories $s \in \{\$0\text{-}\$2{,}000, \$2{,}000\text{-}\$8{,}000\}$. The first term in $\Delta^{\bar{p}}_{s,t}$ measures the change in the interest rate cap between current month t and $t = 0$ at Dec 2013 for loan size bracket s. The second term in $\Delta^{\bar{p}}_{s,t}$ measures the change in the interest rate cap for the control group in this setting, loans larger than $8,000. Subtracting the second term cleans the treatment intensity variable from variation in conditions of the economy determining interest rate caps and isolates the policy variation we want to employ for this estimating policy effects. The evolution of these treatment intensity variables is displayed in Figure A.7.

Using these variables, we estimate effects of interest rate caps using the equation:

$$y_{pt} = \sum_{s} \beta_{s(p)} \Delta^{\bar{p}}_{s(p),t} + \delta_p + \phi_{pm(t)} + \gamma_t + \varepsilon_{pt} \tag{4}$$

where y_{pt} is the outcome of interest for product bin p at month t; δ_p is a set of fixed effects that controls for unobservable shocks specific to a loan size and term, but constant through time; $\phi_{pm(t)}$ is a set of fixed effects that controls for unobservable shocks specific to a product type and month of the year $m(t)$; and similarly γ_t is a set of fixed effects that controls for unobservable shocks specific to a month but constant across loan size and term. The coefficients of interest are β_{0-2000} and $\beta_{2000-8000}$. Given how the treatment variable $\Delta^{\bar{p}}_{s,t}$ 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. 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 such bins across time.

We study policy effects on four sets of outcomes. First, we measure effects on loan interest rates, focusing on maximum and average rates. Second, we study whether quantity outcomes responded to stronger price regulation by measuring effects on the number of applications, on credit volume and on the number of loan contracts signed. Third, we study whether stronger price regulation affected loan performance by measuring effects on loan default outcomes, namely the share of loans ever under 90-day default in its first year and the average number of months under 90-day default in its first year. Finally, we also measure effects on ex-ante risk of the borrower pool by using predicted default risk and borrower income as outcome variables. In each case, we estimate policy effects both across all borrowers and separately for low and high risk borrowers.

4.3 Main Results

4.3.1 Loan Interest Rates

Stronger interest rates caps reduced prices, consistent with descriptive evidence provided in Section 4.1. Table 3 displays estimates of equation (4) using maximum and average loan interest rates as dependent variable. Estimates for effects of reduction in price caps on maximum interest rates reveal that pass-through of the policy was high. Effects from a 1 p.p decrease in interest rate caps range from 0.84 p.p for low risk borrowers to 0.99 p.p for high risk borrowers for loans in \$0-\$2,000; and from 0.53 p.p for low risk borrowers to 0.79 p.p for high risk borrowers for loans in \$2,000-\$8,000. Full effects of the policy on maximum prices can be calculated by scaling up estimated coefficients by the full change in price caps. Full effects are indeed large and close to the total change in the interest rate cap, in particular for riskier borrowers. These results verify that the policy was indeed implemented and that it was more binding for smaller loans and riskier borrowers.

Our estimates for effects on average interest rates in Table 3-B imply that reducing interest rate caps by 1 p.p decreases average interest rates by 0.23 p.p and 0.07 for loans in \$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 0.07 p.p and 0.01 p.p, while those on high risk borrowers are much larger at 0.28 p.p and 0.13 p.p respectively. The full effects on average interest rates were 3.8 p.p and 1.7 p.p respectively after the policy was fully in place.[18]

4.3.2 Quantity Outcomes

Stronger price regulation may affect borrower choices of applying for loans. On the one hand, stronger price regulation may reduce loan prices upon approval and thus induce marginal borrowers to take more loans. On the other hand, banks may be less willing to approve loan applications from borrowers if they are constrained in terms of pricing, which may deter borrower applications. The latter should be particularly relevant for observably riskier borrowers, and we find evidence pointing in that direction. Table 4-A displays estimates of equation (4) using the number of applications as outcome of interest. We find no statistically significant effects both across borrower risk and for low risk borrowers. However, we find that risky borrower apply less often for loans under stronger price regulation. In particular, a 1 p.p decrease in interest rate caps reduced applications by that group by 1.3 percent for loans in \$0-\$2,000, with no significant effect for loans in \$2,000-\$8,000. These estimates imply that the full policy decreased applications by risky borrower for loans in \$0-\$2,000 by 18.5 percent.

[18] Note that these estimates measure the effect on the average interest rate, regardless of whether loans were exposed to the policy. The effect on loans not exposed to the policy should arguably be close to zero, which suggests that effects on exposed loans should be larger. In absence of quantity effects, we would expect perfect pass-through of changes 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 be equal to one. However, such calculation yields around 0.55 p.p and 0.32 p.p respectively for loans in \$0-\$2,000 and \$2,000-\$8,000, which readily suggests the policy had quantity effects.

How did this change in price regulation affect equilibrium quantities? Under stronger price regulation, the interest rate cap may not cover the cost of risky enough loans.[19] Tables 4-B and 4-C display estimates of equation (4) for two quantity outcomes, namely credit volume and number of loans. Our estimates imply that reducing interest rate caps by 1 p.p reduced credit volume by 1.8 percent and 0.6 percent respectively for loans in $0-$2,000 and $2,000-$8,000 respectively. Again, we find substantial heterogeneity across borrower risk. For low risk borrowers, we estimate decreases of 0.9 and 0.4 percent for loans in $0-$2,000 and $2,000-$8,000 respectively. Additionally, for high risk borrowers, we estimate effects almost three times larger, at 3 percent and 1 percent respectively. Results are similar quantitatively when using the number of loans as dependent variable. Full effects of the policy are large. We estimate that the policy reduced credit volume by 25.5 and 12.6 percent respectively for loans in $0-$2,000 and $2,000-$8,000. Effects are particularly large for high risk borrowers, for which we estimate decreases of 38.6 and 21.3 percent respectively. The fact that effects on credit volume and the number of loans are substantially larger than those on borrower loan applications suggest that a large share of the estimated reduction in quantity comes from bank rejections.

4.3.3 Loan Performance and Risk Selection

Changes in loan interest rates and quantities could in turn affect loan performance. On the one hand, lower loan prices may induce higher loan repayment through reduced moral hazard (Adams et al., 2009; Holmstrom and Tirole, 1997). On the other hand, if quantity decreases are related to better risk selection by banks, one would expect improved loan performance under stronger interest rate caps. Results in Table 5-A show that loan performance indeed improved as a result of the policy. On average across all borrowers, reducing interest rate caps by 1 p.p decreased the share of loans under 90-day default in their first year by 0.07 p.p and 0.02 p.p respectively for loans in $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 1.55 p.p and 0.89 p.p which is equivalent to 22.9 and 14.7 percent of their average baseline levels. Results are qualitatively similar when using months under 90-day default during the first year of loans as a dependent variable, as displayed in Table 5-A.

Finally, we study policy effects on risk selection. Finding effects on this dimension would suggest that at least part of the improvement in loan performance we found would be due to changes in the borrower pool risk. Table 5-B displays results from estimating equation (4) using predicted default risk as dependent variable. We find that the policy improved the borrower pool risk. A decrease of 1 p.p in the interest rate cap decreases the average borrower predicted default rate by between 9.4 p.p and 5.3 p.p for loans in $0-$2,000 and by between 0.04 and 0.02 p.p for loans in $2,000-$8,000, depending on the measure of predicted risk. The full policy reduced predicted default risk of borrowers by between 1.54 and 0.87 p.p for loans in $0-$2,000, and by between 0.81 and 0.35 p.p for loans in $2,000-$8,000. The fact that the magnitude of these estimates is close to those for loan performance suggests that most of the improvement in loan performance as a result of the policy was due to risk selection rather than to reduced moral hazard.

[19]The analysis of bank pricing and bank margins in SBIF (2017b) shows how stronger price regulation indeed reduced bank margins and deemed part of the market unprofitable, which is consistent with our findings.

4.3.4 Robustness Exercises

The research design we propose exploits variation in price regulation across loan size brackets and time to estimate the effects of price regulation on market outcomes. In order to assess the assumptions underlying this strategy, we develop a number of robustness exercises. We provide a summary of the main results from them in this section, and develop an extended discussion of them for Appendix A.

We start studying whether trends in outcomes of interest leading to the policy change in Dec 2013 differ across comparison groups, which would invalidate the differences in differences strategy we propose. Appendix A.1 shows evidence for well behaved pre trends. Second, and in a similar vein, we study whether placebo policies shifted in loan size space relative to the actual policy change could generate effects similar to our estimates. Appendix A.2 shows that effects from such placebo policy are generally smaller and close to zero. Third, we consider whether using different comparison groups affect our results. Appendix A.3 shows that our results hold for a range of comparison groups. Fourth, we study whether the distribution of loan size changed around the cutoffs imposed by the policy, which may lead to spillover effects across comparison groups and thus confound our estimates of the effects of price regulation. Appendix A.4 shows there are no such patterns in the data. 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 a sizable share of the banks in the market display effects in line with our results.

4.4 Discussion

The set of results we discuss in this section is consistent with the model proposed in Section 3. First, we find that stronger interest rate regulation shifts the distribution of interest rates below interest rate caps. Second, we find that the number of loans and credit volume in the market decreases under stronger price regulation, which is driven by a combination of decreased application by borrowers and increased rejections by banks. We interpret the decrease in application as that borrowers weight the decrease in approval probabilities by banks more than the decrease in expected prices conditional on approval. In terms of the effects described in Section 3.4, the latter implies that the credit access effect dominated the consumer protection effect, at least in this context. The fact that the reduction in quantity is stronger than the reduction in applications suggests that bank rejections increase. We interpret such increase as that the inability of banks to charge interest rates large enough as to cover expected costs of risky loans induce banks to reduce the amount of loans provided to the market. Third, we find that as a result of changes in the quantity margin, the borrower pool becomes safer in equilibrium and loan performance improves.

Our results are somewhat stronger than the finding of previous attempts at measuring the effects of this policy change in the Chilean market by Hurtado (2015) and SBIF (2017b). The differences might stem from differences in the methodology we employ relative to those papers, which makes her to compare quantitative results. In particular, our analysis differs from that in SBIF (2017b) along different dimensions, including: (i) we construct quantity outcomes differently, by using logged credit volume and number of loans instead of their levels; (ii) we adopt a different unit of analysis, by aggregating at the market level, across banks; (iii) we exploit the full policy variation provided by the evolution of interest rate caps, instead of implementing a comparison between the period before and after the policy change; (iv) we restrict our sample to include consumer loans only, instead of a river set of credit products; (v) we restrict the sample to January 2013 through December 2015; and (vi) we report results from regressions weighted by the number of loans in each loan type bin in order to account for heterogeneity in market size in our estimates. Moreover, In addition to our findings on credit access, we provide additional evidence on two important aspects not addressed before. First, we show that part of the decrease in the number of loan contracts signed in the market comes from a decrease in loan applications, which is consistent with consumers understanding that having loan applications approved under stronger price regulation is harder and therefore choosing not to apply for them. Second, we show that stronger price regulation led to improvements in loan performance.

Our estimates show that the effects of stronger price regulation are larger for observably risky borrowers. This is a pattern that persists across outcomes and is related to the fact that such group was more *exposed* to the policy change, as they were already being charged higher loan prices before the policy change due to their higher expected default cost. If the value riskier borrowers place on loans is higher than that of inframarginal, safer borrowers, then this pattern could be particularly relevant from a welfare point of view.

Overall, our estimates imply that 153,073 loan contracts per year were deterred by stronger price regulation, equivalent to $366.3 million in consumer loans.[20] Furthermore, our estimates of average price effects imply that the average decrease in monthly payments across originated loans was of $3.5 and that the present value of reduced monthly payments on loan contracts on loans originated after the policy was in place adds up to $32.9 million.[21] These two numbers provide a notion of the magnitudes involved in the trade-off between consumer protection and credit access that surrounds the design of price regulation in credit markets. They highlight that price regulation might have sizable unintended consequences which, given the patterns of heterogeneity in our estimates, are distributed unequally along the distribution of borrower risk.

This analysis leaves several relevant questions unanswered. The extent to which the policy harmed or benefitted consumer welfare along the risk distribution, the interaction of these effects with the degree of market power held by banks in the market, and the role that alternative designs of price regulation may have on market outcomes are aspects we address in Cuesta and Sepúlveda (2018). Moreover, the extent to which (potentially inferior) substitutes to regulated credit markets in the economy may develop in response to this type of regulation and the consequences of such should be a policy concern.

[20] This figure 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.

[21] 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 five percent and the term of each loan contract. Finally, we aggregate across loan contracts actually signed during the year before the policy change was implemented.

5 Implications for the Design of Interest Rate Caps

In the previous section, we have provided evidence for the effects of stronger price regulation on a range of market outcomes. Our results suggest this class of regulation may have sizable effects on credit markets, when binding. Moreover, they highlight how the trade-off between consumer protection and credit access is indeed relevant for the design of this policy: the policy seems to protect those who remain in the market, but credit access decreases. Gains and losses from price regulation are heterogeneous across the pool of potential borrowers and, in particular, decreases in credit access are particularly large for risky borrowers. While policymakers may want to protect more vulnerable populations the most, our results suggest that this policy does not in principle achieve such objective.

In this section, we discuss two policy implications of our results through the lens of our model in Section 3. First, we discuss how the amount of market power held by banks determines the effects of price regulation. Second, we discuss how a design of the regulation that takes into account borrower risk may improve its performance in terms of dealing with the trade-off between consumer protection and credit access. There are additional policy aspects related to our results that we do not address in this paper. For instance, the relevance of the effects of price regulation on loan performance for financial stability, from a macroeconomic perspective; or the extent to which consumers are indeed protected from their own behavioral biases by restricting their access to credit at high prices, from a behavioral economics perspective.

5.1 The Role of Market Power

The motivation of price regulation in credit markets has usually been to limit usury, which can be understood as limiting the exercise of market power by banks. In Section 4, we showed that stronger price regulation indeed reduces interest rates on signed contracts while simultaneously decreasing the number of loans in the market. It is worth considering how those results would vary under alternative competitive environments.

Our model in Section 3 provides a useful framework to implement this analysis. For unconstrained approvals, loan contracts are priced at a mark-up determined by the difference between the lending bank and its closest competitor, $\rho(r_j, T_i)(\omega_{i(1)} - \omega_{i(2)})$. As price regulation becomes stronger and eventually binding, such mark-up becomes constrained and equal to $E[\rho(r_j, S_i)]\bar{p}_i - \rho(r_j, T_i)(f_i - \omega_{i(1)})$, which as long as it remains positive does not deter the bank of signing the contract but redistributes rents from the bank to the borrower. That is the consumer protection aspect of price regulation. However, that outcome becomes less likely as banks hold less market power.[22] Consider for instance the extreme case in which there is no cost heterogeneity across banks, which is $\omega_{ij} = \bar{\omega}_i \, \forall j$. In such case, unconstrained approvals would already lead to zero profits and binding price regulation would discourage banks to sign loan contracts with those borrowers, leading to a decrease in credit access.

In competitive credit markets where banks do not have substantial market power, the trade-off between

[22] In fact, it can be shown that the share of constrained approvals decreases as the amount of market power decreases, which in our model would be measured as a reduction in the variance of ω_{ij}.

consumer protection and credit access becomes less appealing, as bank profit margins are already low and therefore the room for consumer protection is low. Thus, interest rate caps in such settings will have effects mostly in the form of decreases in credit access rather than consumer protection. Therefore, the competitive environment should be considered for the design of price regulation in credit markets.

5.2 Risk Based Interest Rate Caps

Despite the trade-off between consumer protection and credit access we have emphasized throughout the paper, innovation in the design of this policy has been scant as emphasized by Maimbo and Henríquez (2014). We argue that this trade-off is in fact partly caused by the design of interest rate caps that are set independently of borrower risk in a context of borrower risk heterogeneity and risk pricing by banks. A design that accounts for such elements of the market may be able to better deal with such trade-off. And while the role of risk pricing in selection markets has received substantial attention (e.g., Einav et al. 2013), few attention has been placed on the utilization of available information about risk in such markets for the design of regulation.

We consider in this discussion an alternative design for price regulation that sets interest rate caps differently across borrowers according to their attributes. Using the same notation as in equation (1), interest rate caps can be written as $\bar{p}_{pt} = \bar{p}^r(\tilde{p}_{pt-1}; \phi, \alpha_{pt})$, such that price regulation in its current design is a function of a reference interest rate of the economy, a parameter that operates as a multiplier on such rate and a parameter that operates as a mark-up. Risk based price regulation would instead specify $\bar{p}^r_{ipt} = \bar{p}(\tilde{p}_{pt-1}, x_{ipt}; \phi, \alpha_{pt})$, where x_{ipt} is some measure of risk. Whenever $\frac{\partial \bar{p}^r_{ipt}}{\partial x_{ipt}} > 0$, this design would set a higher interest rate cap for observably riskier borrowers than for observably safer borrowers.

What are the implications of risk based price regulation relative to constant interest rate caps? Under a constant interest rate cap, the extent to which the regulation constrains bank pricing increases with borrower risk, and therefore the likelihood that a loan applicant is rejected by a bank also increases with borrower risk. Thus, the extent of consumer protection and the risk of exclusion imposed by a constant interest rate cap are unequally distributed across borrowers. In contrast, risk based price regulation would reduce such inequality by setting interest rate caps higher for riskier (costlier) borrowers and lower for safer (cheaper) borrowers. With that, it would provide more consumer protection to borrowers of low risk and would limit potential adverse effects on credit access for borrowers of high risk.

Overall, this discussion suggests that risk based interest rate caps may be able to manage the trade-off between consumer protection and credit access better than common designs of interest rate caps. This comes from the fact that banks implement risk pricing in the credit market. In absence of risk pricing, adverse effects of risk based caps on safe borrowers may actually be larger than under common designs of the policy. This discussion highlights the importance that the availability of risk scores may have in a credit market, both for risk pricing but also for the design of regulation.

6 Conclusion

In this paper, we study the implications of price regulation in retail credit markets. Despite the fact that this class of regulation has been present in credit markets for centuries, the design of it in most countries lacks sophistication, which may lead to unintended consequences. We exploit the Chilean credit market as an empirical application, where we study the effects of a reform to interest rate caps that made price regulation on consumer loans stronger. We leverage rich administrative data for our analysis, which allows for including several outcomes in the analysis and to study heterogeneity in effects across borrower risk groups.

We find that loan prices decreased sharply in response to stronger price regulation. However, we also find that the volume of credit decreased in response to the policy. We show that this decrease in credit volume combines a reduction in the number of loan application by borrowers with an increase in rejections by banks. The magnitudes are large, as our estimates imply that the number of loan contracts signed in the market subject to this policy change decreased by 19 percent. Importantly, our estimates show that the effects of stronger price regulation are concentrated among risky borrowers. Thus, while the purpose of price regulation in credit markets is often to protect borrowers from the exercise of market power by banks, we find it may end up harming borrowers' access to credit. However, we emphasize that the effects of price regulation on application behavior are ambiguous. Therefore, our results from this setting do not necessarily imply that price regulation will decrease credit volume in other contexts. Rather, such outcome will depend on the particular underlying configurations of demand and supply of those settings.

We employ an equilibrium model of demand and supply for loans to provide a conceptual framework with which to interpret our findings. In complementary work in Cuesta and Sepúlveda (2018), we estimate this model in order to further understand the role of price regulation in credit markets. The motivation for this exercise is threefold: an estimated model allows for decomposing equilibrium effects and providing a better understanding of the mechanisms driving them; for calculating welfare effects of price regulation; and for studying quantitatively how effects of price regulation would differ under counterfactual competitive environments and policy designs. In particular, in that paper we provide quantitative assessments for the role of market power in terms of the effects of price regulation and for the role of risk based price regulation, both of which we discuss conceptually in Section 5.

This paper provides evidence for the effects of interest rate caps on a range of relevant market outcomes and builds on a body of literature on the topic. However, we emphasize that our analysis does not address other outcomes that could be eventually affected by price regulation. First, there is concern among regulators about overindebtedness by households, and price regulation might deliver benefits to households in the form of controlled indebtedness. Second, households might be displaced to inferior substitutes to satisfy their demand for credit. Among such substitutes, informal moneylenders are of primary concern. Recent reports using survey data to study the development of such segments in the Chilean credit market disagree on its evolution, with ABIF (2018) suggesting the informal sector has grown over recent years and a comparison between EFH (2014) and EFH (2018) suggesting the opposite. Measuring the effects of this policy change on these outcomes would provide a more complete picture for the effects of price regulation in credit markets.

Several relevant questions remain open for future research. For instance, an assessment of the welfare effects of interest rate caps would complement analyses that focus on market outcomes. Moreover, the extent to which borrowers harmed by price regulation obtain credit from potentially inferior substitutes also remain an unanswered question. For the Chilean case in particular, this would consider studying effects on other segments of consumer credit, including credit cards and credit lines. Furthermore, a better understanding of the relationship between interest rate caps and the extent of adverse selection in the market may be relevant for the design of this regulation. We plan to address these aspects in future research.

References

ABIF (2018). Evolución del endeudamiento de los hogares. Technical report, Asociación de Bancos e Instituciones Financieras.

Adams, W., Einav, L., and Levin, J. (2009). Liquidity Constraints and Imperfect Information in Subprime Lending. *American Economic Review*, 99(1):49–84.

Agarwal, S., Chomsisengphet, S., Mahoney, N., and Stroebel, J. (2015). Regulating Consumer Financial Products: Evidence from Credit Cards. *Quarterly Journal of Economics*, 130(1):111–164.

Agarwal, S., Grigsby, J., Hortaçsu, A., Matvos, G., Seru, A., and Yao, V. (2017). Search and Screening in Credit Markets: Evidence from the U.S. Mortgage Market. Manuscript.

Allen, J., Clark, R., and Houde, J. F. (2017). Search Frictions and Market Power in Negotiated Price Markets. Manuscript.

Benetton, M. (2018). Leverage Regulation and Market Structure: An Empirical Model of the UK Mortgage Market. Manuscript.

Benmelech, E. and Moskowitz, T. (2010). The Political Economy of Financial Regulation: Evidence from U.S. State Usury Laws in the 19th Century. *The Journal of Finance*, 65(3):1029–1073.

Bodenhorn, H. (2007). Usury Ceilings and Bank Lending Behavior: Evidence from Nineteenth Century New York. *Explorations in Economic History*, 44(2):179 – 202.

Castellanos, S., Jiménez-Hernández, D., Mahajan, A., and Seira, E. (2018). Financial Inclusion and Contract Terms: Experimental Evidence From Mexico. Working Paper 24849, National Bureau of Economic Research.

Crawford, G. S., Pavanini, N., and Schivardi, F. (2018). Asymmetric Information and Imperfect Competition in Lending Markets. *American Economic Review*, 108(7):1659–1701.

Cuesta, J. I. and Sepúlveda, A. (2018). Price Regulation in Credit Markets: Evidence from Consumer Loans. Manuscript.

EFH (2014). Encuesta Financiera de Hogares 2014: Principales Resultados. Technical report, Banco Central de Chile.

EFH (2018). Encuesta Financiera de Hogares 2018: Principales Resultados. Technical report, Banco Central de Chile.

Einav, L., Jenkins, M., and Levin, J. (2013). The impact of credit scoring on consumer lending. *The RAND Journal of Economics*, 44(2):249–274.

Fekrazad, A. (2016). Welfare Consequences of Payday Loan Regulation: Evidence from Rhode Island. Manuscript.

Flores, C., Morales, L., and Yánez, A. (2005). Interés Máximo Convencional: Origen, Evolución y Forma de Cálculo. Serie técnica de estudios, SBIF.

Holmstrom, B. and Tirole, J. (1997). Financial Intermediation, Loanable Funds, and the Real Sector. *The Quarterly Journal of Economics*, 112(3):663–691.

Hurtado, A. (2015). Efectos de la Regulación de Tasas de Interés en el Mercado de Crédito Bancario: Evidencia para Chile. Manuscript.

Lester, B., Shourideh, A., Venkateswaran, V., and Zetlin-Jones, A. (2017). Screening and Adverse Selection in Frictional Markets. Manuscript.

Liberman, A., Neilson, C., Opazo, L., and Zimmerman, S. (2018). The Equilibrium Effects of Information Deletion: Evidence from Consumer Credit Markets. Manuscript.

Mahoney, N. and Weyl, G. (2017). Imperfect Competition in Selection Markets. *Review of Economics and Statistics*.

Maimbo, S. and Henríquez, C. (2014). Interest Rate Caps around the World : Still Popular, but a Blunt Instrument. Policy Research Working Paper 7070, World Bank Group, Washington, DC.

Melzer, B. (2011). The Real Costs of Credit Access: Evidence from the Payday Lending Market. *Quarterly Journal of Economics*, 126(1):517–555.

Melzer, B. and Schroeder, A. (2017). Loan Contracting in the presence of Usury Limits: Evidence from Automobile Lending. Manuscript.

Morse, A. (2011). Payday Lenders: Heroes or Villains? *Journal of Financial Economics*, 102(1):28–44.

Nelson, S. (2018). Private Information and Price Regulation in the US Credit Card Market. Manuscript.

Ohlson, J. A. (1980). Financial Ratios and the Probabilistic Prediction of Bankruptcy. *Journal of Accounting Research*, 18(1):109–131.

Petersen, M. A. and Rajan, R. G. (1995). The Effect of Credit Market Competition on Lending Relationships. *The Quarterly Journal of Economics*, 110(2):407–443.

Rigbi, O. (2013). The Effects of Usury Laws: Evidence from the Online Loan Market. *Review of Economics and Statistics*, 95(4):1238–1248.

Safavian, M. and Zia, B. (2018). The Impact of Interest Rate Caps on the Financial Sector: Evidence from Commercial Banks in Kenya. Policy Research Working Paper 8393, World Bank.

Salz, T. (2017). Intermediation and Competition in Search Markets: An Empirical Case Study. Manuscript.

SBIF (2017a). Informe de Endeudamiento 2017. Serie Técnica de Estudios, Superintendencia de Bancos e Instituciones Financieras de Chile.

SBIF (2017b). Tercer informe anual sobre los efectos de la aplicación de la Ley 20715: Tasa Máxima Convencional. Serie Técnica de Estudios, Superintendencia de Bancos e Instituciones Financieras de Chile.

Schmukler, S., Tessada, J., Vasquez, C., and Vera, M. (2018). Winners and Losers from Interest Rate Ceilings: Quasi-experimental Evidence from Chile. Manuscript.

Skiba, P. M. and Tobacman, J. (2015). Do Payday Loans Cause Bankruptcy? Manuscript.

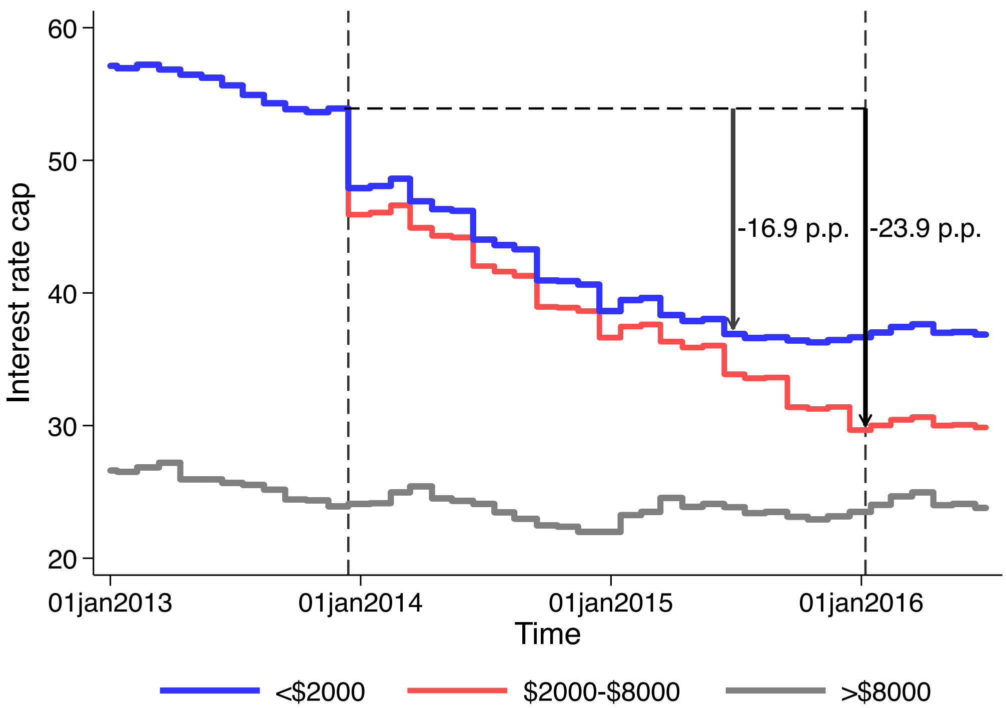
Temin, P. and Voth, H.-J. (2008). Interest Rate Restrictions in a Natural Experiment: Loan Allocation and the Change in the Usury Laws in 1714. *The Economic Journal*, 118(528):743–758.

Veiga, A. and Weyl, G. (2016). Product design in selection markets. *The Quarterly Journal of Economics*.

Woodward, S. E. and Hall, R. E. (2012). Diagnosing Consumer Confusion and Sub-optimal Shopping Effort: Theory and Mortgage-Market Evidence. *American Economic Review*, 102(7):3249–76.

Zinman, J. (2010). Restricting Consumer Credit Access: Household Survey Evidence on Effects around the Oregon Rate Cap. *Journal of Banking & Finance*, 34(3):546–556.

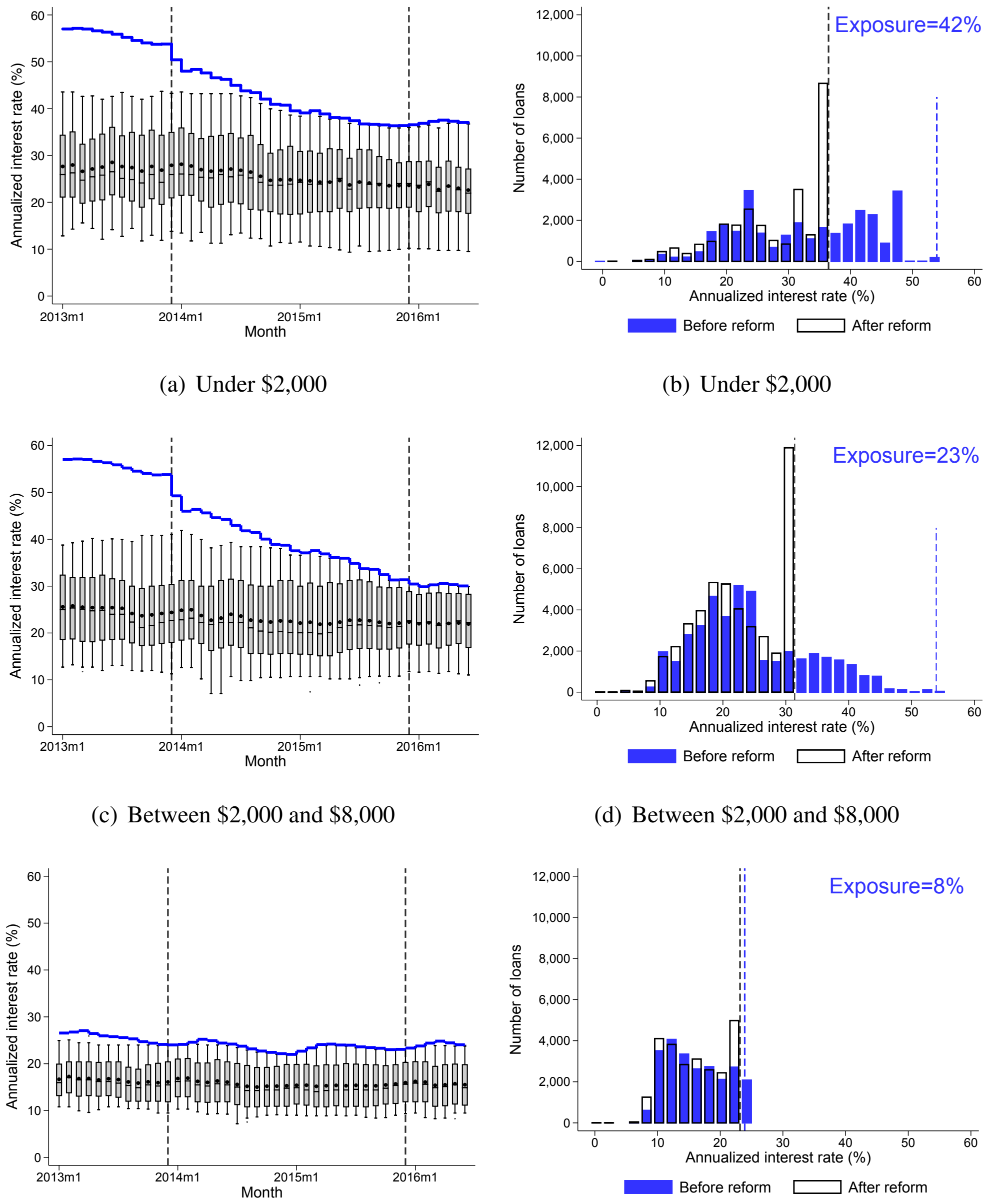
Figure 1: Evolution of interest rate caps



(a) Evolution of interest rate caps

Notes: These figure displays the evolution of the level of interest rate caps for different loan size categories. The dashed black line indicates the approval of Law 20,715, after which interest rate caps for all loans under $8,000 were reduced. The policy was fully implemented by Dec 2015.

Figure 2: Evolution of the distribution of interest rates



(a) Under $2,000

(b) Under $2,000

(c) Between $2,000 and $8,000

(d) Between $2,000 and $8,000

(e) Between $8,000 and $20,000

(f) Between $8,000 and $20,000

Notes: Panels (a), (c) and (d) in this figure display the evolution of the distribution of interest rates by loan size within each month. Each box displays the 25th, 50th and 75th percentiles of such distribution. Spikes display the 5th and 95th percentiles of it. Black dots indicate the mean of it. In each plot, the blue line displays the current interest rate cap relevant for the corresponding loan size interval. Panels (b), (d) and (f) in this figure display frequency histograms of interest rates for the month before the reform started, Dec 2013 (blue), and for the month in which it was fully in place, Dec 2015 (white). The blue dashed line indicates the level of the interest rate cap for each size category 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.

Table 1: Summary statistics

Variable	N	Mean	SD	p10	p50	p90
A - Loan attributes						
Loan interest rate	3,936,218	22.89	9.90	10.80	21.12	37.68
Loan amount	3,936,218	6,765.93	7,059.22	964.12	4,409.70	16,361.53
Loan term	3,936,218	33.06	16.22	12.20	36.17	51.37
Monthly payment	3,936,218	268.00	325.45	66.77	190.60	525.65
B - Loan performance						
Default during loan first year	3,936,218	0.05	0.21	0.00	0.00	0.00
Default during loan term	3,936,218	0.09	0.28	0.00	0.00	0.00
Amount of charge-off	3,936,218	283.86	1,784.06	0.00	0.00	0.00
Predicted default probability - Income	3,935,987	0.11	0.06	0.03	0.11	0.18
Predicted default probability - History	3,930,652	0.11	0.09	0.02	0.09	0.24
C - Borrower attributes						
Annual income	3,936,218	19,338.04	18,816.06	5,482.00	13,269.84	39,343.65
Age	3,930,883	43.67	13.27	28.00	42.00	63.00
Female	3,936,218	0.40	0.49	0.00	0.00	1.00
Consumer debt to income ratio	3,936,218	4.22	4.33	0.08	2.96	9.97
Mortgage debt to income ratio	3,936,218	5.50	12.33	0.00	0.00	23.53
Consumer debt	3,936,218	7,080.32	10,574.39	75.24	3,178.03	18,483.54
Consumer debt under default	3,936,218	42.79	598.30	0.00	0.00	0.00
Mortgage debt	3,936,218	12,598.35	31,542.56	0.00	0.00	48,774.83
Mortgage debt under default	3,936,218	12.02	691.92	0.00	0.00	0.00
Previously related to bank	3,936,218	0.76	0.43	0.00	1.00	1.00
Previously related to any bank	3,936,218	0.94	0.25	1.00	1.00	1.00
D - Borrowers through the dataset						
Number of loans	2,118,426	1.86	1.34	1.00	1.00	3.00
Amount in loans	2,118,426	12,571.67	15,706.10	1,577.01	7,159.14	30,374.05
Number of banks with loan contracts	2,118,426	1.23	0.50	1.00	1.00	2.00
Number of related banks	2,118,426	2.91	1.52	1.00	3.00	5.00
E - Application events						
Loan amount	3,659,274	7,177.85	7,345.34	1,038.11	4,834.77	17,141.88
Loan term	3,255,669	34.60	15.67	12.63	36.50	54.77
Approved, accepted by applicant	3,659,396	0.72	0.45	0.00	1.00	1.00
Approved, denied by applicant	3,659,396	0.10	0.30	0.00	0.00	0.00
Rejected	3,659,396	0.18	0.39	0.00	0.00	1.00

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.

Table 2: Borrower risk, behavior and outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Panel A: 1(Application)		Panel B: 1(Approval)			Panel C: log(Interest rate margin)			Panel C: 1(Default during 1Y)		
Predicted default risk	0.003***	0.004***	-0.438***	-0.281***	-0.202***	1.681***	0.809***	0.838***	0.472***	0.465***	0.462***
	(0.000)	(0.000)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.001)	(0.001)	(0.002)
log(Loan size)				0.057***	0.052***		-0.356***	-0.356***		-0.002***	-0.001***
				(0.000)	(0.000)		(0.000)	(0.000)		(0.000)	(0.000)
log(Loan term)				-0.114***	-0.109***		0.168***	0.167***		0.017***	0.016***
				(0.000)	(0.000)		(0.001)	(0.001)		(0.000)	(0.000)
Related to bank					0.114***			-0.005***			-0.023***
					(0.000)			(0.001)			(0.000)
Related to any bank		0.014***			-0.058***			0.033***			0.024***
		(0.000)			(0.001)			(0.001)			(0.001)
Observations	35,372,052	35,372,052	3,526,874	3,526,866	3,526,866	2,628,922	2,628,922	2,628,922	2,631,973	2,631,973	2,631,973
R-squared	0.001	0.001	0.053	0.074	0.090	0.367	0.612	0.612	0.051	0.053	0.054
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Bank FE	N	N	Y	Y	Y	Y	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Notes: Panel A in this table displays results from regressions of an indicator for loan application on borrower covariates; Panel B does so for an indicator for approval conditional on application; Panel C does so for regressions of loan interest rate margins; and Panel D does so for regressions of loan default on covariates. Interest rate margins are calculated as the difference between interest rate and the funding cost of banks. Standard errors are displayed in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3: Policy effects on loan interest rates

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Maximum interest rate			Panel B: Average interest rate		
Marginal effect						
Loans in $0 – $2000	-1.001***	-0.844***	-0.989***	-0.232***	-0.072***	-0.281***
	(0.011)	(0.049)	(0.017)	(0.035)	(0.015)	(0.043)
Loans in $2000 – $8000	-0.786***	-0.526***	-0.791***	-0.073***	-0.012*	-0.128***
	(0.030)	(0.054)	(0.037)	(0.020)	(0.007)	(0.029)
Full effect						
Loans in $0 – $2000	-16.376***	-13.802***	-16.179***	-3.803***	-1.175***	-4.595***
	(0.181)	(0.808)	(0.283)	(0.572)	(0.241)	(0.703)
Loans in $2000 – $8000	-18.336***	-12.264***	-18.448***	-1.715***	-0.284*	-2.986***
	(0.694)	(1.259)	(0.860)	(0.471)	(0.157)	(0.681)
Baseline mean						
Loans in $0 – $2000	55.14	52.82	55.32	33.05	25.10	35.68
Loans in $2000 – $8000	50.46	46.15	51.38	24.92	19.28	28.69
Borrower risk bin	All	Low	High	All	Low	High
Observations	2,880	2,880	2,572	2,880	2,880	2,572
R-squared	0.986	0.964	0.979	0.984	0.985	0.967
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 one p.p. Full effects are calculated by multiplying the marginal effect of the policy by 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 are displayed in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Policy effects on quantity outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Panel A: log(Applications)			Panel B: log(Credit volume)			Panel C: log(Number of loans)		
Marginal effect									
Loans in \$0 – \$2000	-0.004	0.003	-0.013***	-0.018***	-0.009***	-0.030***	-0.020***	-0.007***	-0.033***
	(0.004)	(0.003)	(0.004)	(0.004)	(0.002)	(0.005)	(0.003)	(0.002)	(0.005)
Loans in \$2000 – \$8000	-0.000	0.002	-0.005	-0.006*	-0.004**	-0.010**	-0.006*	-0.003	-0.011***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.004)	(0.003)	(0.002)	(0.004)
Full effect									
Loans in \$0 – \$2000	-0.069	0.057	-0.205***	-0.294***	-0.148***	-0.487***	-0.326***	-0.119***	-0.544***
	(0.060)	(0.055)	(0.065)	(0.062)	(0.039)	(0.079)	(0.056)	(0.037)	(0.075)
Loans in \$2000 – \$8000	-0.011	0.041	-0.120	-0.135*	-0.101**	-0.239**	-0.129*	-0.078	-0.263***
	(0.071)	(0.068)	(0.073)	(0.074)	(0.050)	(0.095)	(0.074)	(0.050)	(0.097)
Baseline mean									
Loans in \$0 – \$2000	1179.91	477.04	1070.46	2,202.06	741.91	2,000.89	1,806.72	556.85	1,685.99
Loans in \$2000 – \$8000	634.92	347.09	489.30	3,547.66	2,108.92	2,605.17	810.29	444.08	634.03
Borrower risk bin	All	Low	High	All	Low	High	All	Low	High
Observations	2,876	2,836	2,793	2,880	2,880	2,880	2,880	2,880	2,880
R-squared	0.993	0.986	0.992	0.985	0.984	0.971	0.992	0.985	0.987
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 one p.p. Full effects are calculated by multiplying the marginal effect of the policy by 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 displayed in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 5: Policy effects on loan performance and risk selection

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Panel A: Loan performance						Panel B: Risk selection		
	Share of loans under 1Y default			Months under 1Y default			Risk-Inc	Risk-Hist	log(Income)
Marginal effect									
Loans in \$0 – \$2000	-0.095***	-0.046***	-0.066	-0.003***	-0.002***	-0.002*	-0.094***	-0.053***	0.007***
	(0.018)	(0.007)	(0.042)	(0.001)	(0.000)	(0.001)	(0.006)	(0.014)	(0.001)
Loans in \$2000 – \$8000	-0.038***	-0.000	-0.044	-0.002***	-0.000	-0.002**	-0.035***	-0.015**	0.001
	(0.011)	(0.007)	(0.029)	(0.000)	(0.000)	(0.001)	(0.008)	(0.007)	(0.001)
Full effect									
Loans in \$0 – \$2000	-1.550***	-0.745***	-1.075	-0.056***	-0.027***	-0.035***	-1.543***	-0.868***	0.117***
	(0.299)	(0.109)	(0.693)	(0.010)	(0.004)	(0.020)	(0.106)	(0.225)	(0.018)
Loans in \$2000 – \$8000	-0.890***	-0.006	-1.036	-0.039***	-0.001	-0.046**	-0.805***	-0.348**	0.019
	(0.251)	(0.152)	(0.675)	(0.010)	(0.005)	(0.018)	(0.188)	(0.161)	(0.017)
Baseline mean									
Loans in \$0 – \$2000	6.78	2.31	9.27	0.27	0.07	0.32	13.95	13.72	12.19
Loans in \$2000 – \$8000	6.05	1.86	9.38	0.19	0.06	0.31	12.37	12.50	16.96
Borrower risk bin	All	Low	High	All	Low	High	All	All	All
Observations	2,880	2,880	2,572	2,880	2,880	2,572	2,880	2,880	2,880
R-squared	0.914	0.722	0.658	0.911	0.706	0.669	0.988	0.980	0.991
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 one p.p. Full effects are calculated by multiplying the marginal effect of the policy by the magnitude of the policy change once fully implemented for each policy loan size bracket. Estimating sample in Panel A is restricted to loans originated before Dec 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 bin-risk bin before the policy was implemented. Clustered standard errors at the product bin-risk bin level are displayed in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

A Robustness Exercises and Additional Results

The empirical strategy developed in Section 4.2 proposes to exploit policy variation across time and loan size brackets in order to measure the effect of interest rate caps on credit market outcomes. In this Section, we implement different robustness exercises that provide support to our empirical strategy. Moreover, we include some additional results.

A.1 Evolution of Policy Effects

As a first robustness exercise, we estimate differences in differences models that decompose policy effects though time. This addresses concerns related to trends in the outcomes of interest before the implementation of the policy that could be correlated with the policy itself. We start with a simple differences in differences model that exploits variation across time and policy size brackets, by estimating the equation:

$$y_{pt} = \sum_{s,\tau} L_{s(p)} \beta_{s(p)\tau} + \alpha_p + \delta_t + \varepsilon_{pt} \tag{5}$$

where the notation is the same as that in equation (4). In this case, however, we decompose policy effects and estimate coefficients specific to each treated loan size bracket and month, denoted as $\beta_{s(p)\tau}$. We denote months relative to policy implementation in Dec 2013 by τ. We normalize coefficients in the month before the policy is implemented to zero. $L_{s(p)}$ is an indicator for policy size brackets. The specification includes both product type bin and month fixed effects.[23]

Results from estimating equation (5) are displayed in Figure A.8 for our main price and quantity outcomes. Figures A.8-a through A.8-c display policy effects on maximum interest rates across borrower risk and then separately for different borrower risk groups, while Figures A.8-d through A.8-f do so for average interest rates. In all cases, results reveal flat trends before the policy is implemented and a steady decrease in interest rate once the policy is in place. Consistent with our estimates in Table 5, these results show stronger impacts on average interest rates among high risk borrowers than among low risk borrowers. Figures A.8-g through A.8-l display analogous estimates using credit volume and number of loans as dependent variables, for which trends before the policy change are also estimated to be flat. Moreover, estimated effects are larger for high risk borrowers than for low risk borrowers, and full effects are of similar magnitude as our baseline estimates in Table 4.

A.2 Effects of Placebo Policies

Our approach in Section 4.2 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. We study whether that is the case, by estimating equation (4) for placebo policies. Concretely, we use the same definition for

[23] 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 p before estimating equation (5).

the policy and the same policy intensity variables as in Section 4.2 to estimate effects of price regulation on different parts of the loan size distribution. In practice, we proceed by replacing the dependent variable y_{prt} to $y_{p+\Delta,rt}$, where Δ defines the placebo policy. We start by policy size brackets defined as being \$12,000 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 loan interest rates, and the results are stark: estimates from placebo policies are remarkably different from our estimates of policy effects 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.

A.3 Alternative Comparison Groups

Our analysis in section 4.2 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 estimates of policy effects 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.10, 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 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 reflect in tighter standard errors.

A.4 Effects on Loan Size

The approach proposed in Section 4.2 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 price 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 instance, one could argue that before the reform borrowers could have sought to get loans right above the \$8,000 threshold in order 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 4.3 would underestimate the effects of the reform.

In order to address this concern, 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, 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.[24]

In a more systematic attempt to address this concern, we repeat the analysis in Section 4.3 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 4.3, which is reassuring in terms of our empirical strategy. Finally, Figure A.12 repeats this exercise 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.

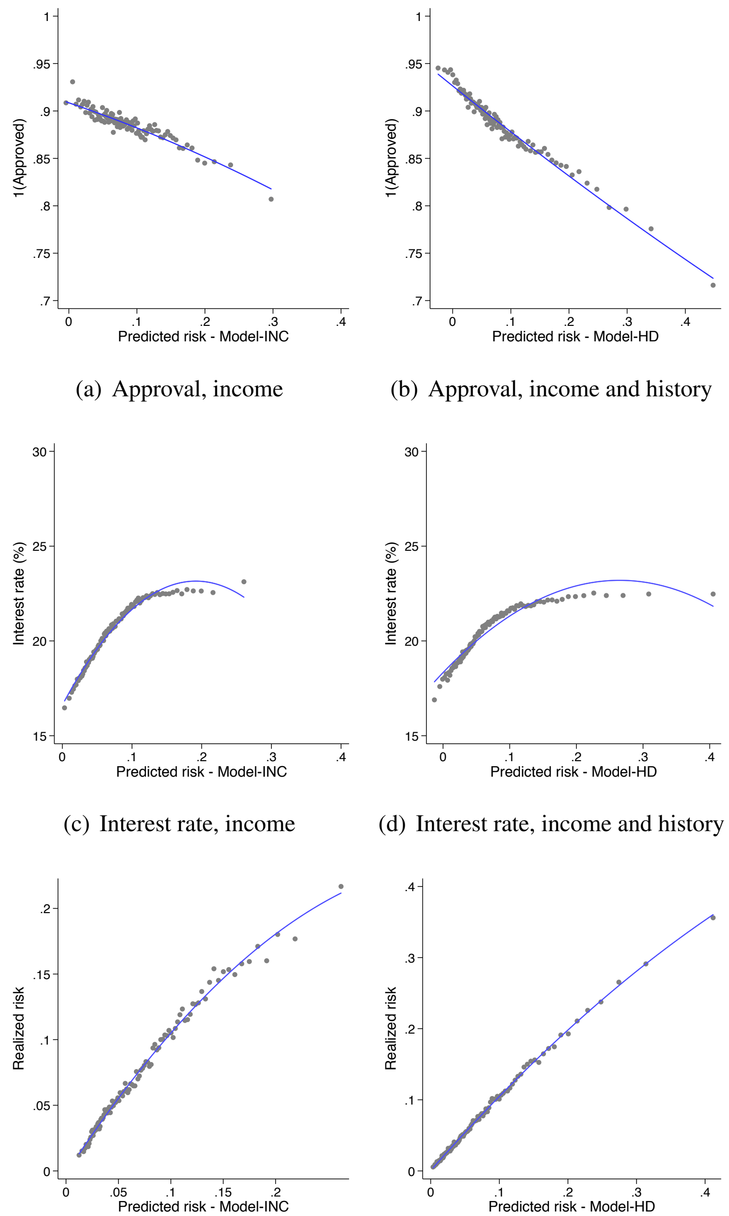
A.5 Heterogeneity across Banks

We have focused so far on the effects of price regulation at the market level. In this subsection, we provide results for heterogeneity in effects across banks. Figure A.13 provides results for marginal effects of price regulation on both number of lines and prices for each of the eight largest banks in the market. While there heterogeneity in magnitudes, our estimates suggest that the stronger price 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 one bank that displays a different behavior, by reducing average interest but simultaneously increasing credit volume as a result of stronger price 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.

[24]Figures A.11-c and A.11-d complement this analysis by showing that the distribution of prices shifts downwards after the policy, but that there is no discontinuity in average loan 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.

Figure A.1: Predicted and realized default



(a) Approval, income

(b) Approval, income and history

(c) Interest rate, income

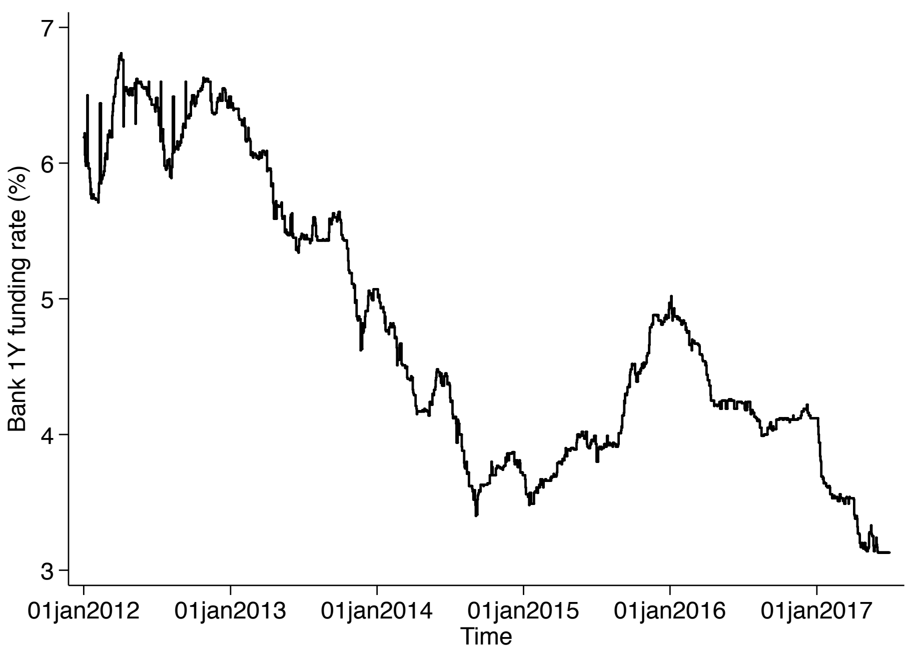
(d) Interest rate, income and history

(e) Realized default, income

(f) Realized default, income and history

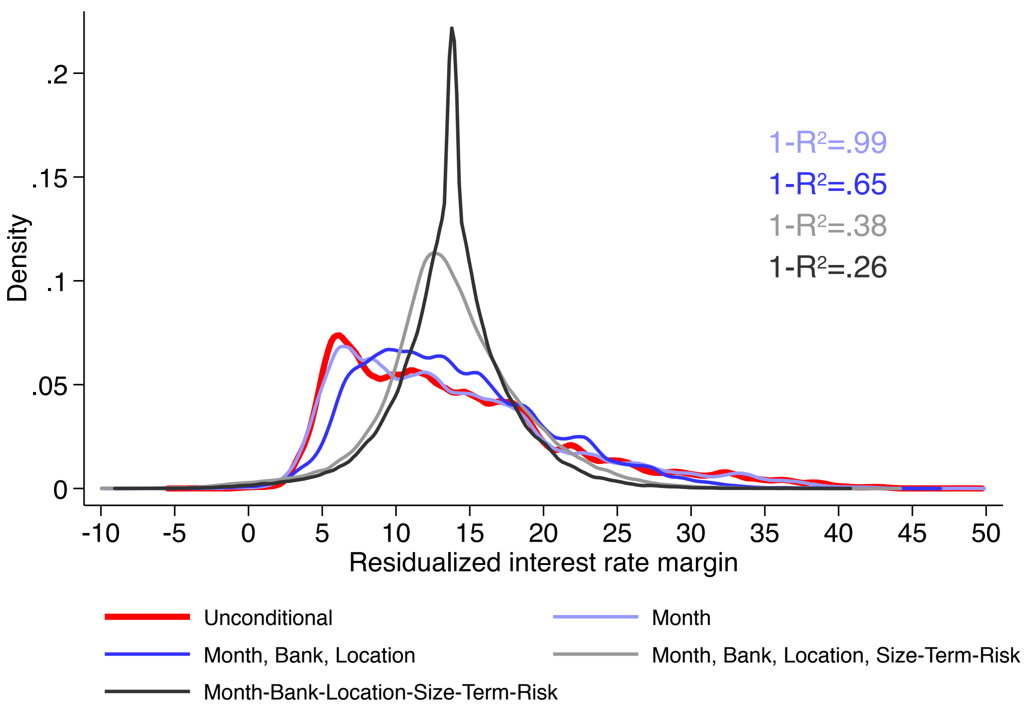
Notes: This figure displays binned scatterplot of predicted loan default probability as constructed using the model described in Section 2.2 and realized outcomes. The first column displays results using borrower income and loan to income ratios as main predictors of default, whole the second columns 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 loan 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.2: Evolution of the funding cost of banks



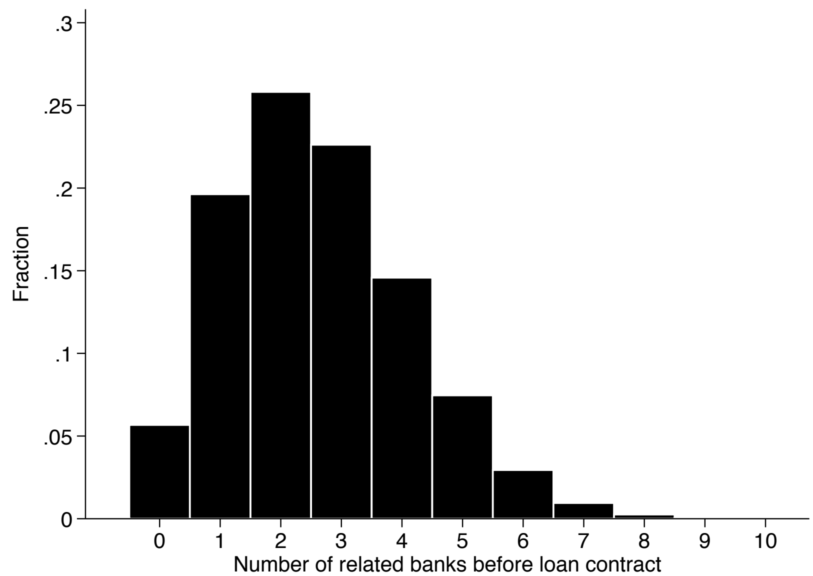
Notes: This figure displays the evolution of the funding cost of banks. This funding cost is calculated as a weighted average of banks deposit rates.

Figure A.3: Price dispersion in consumer loan contracts

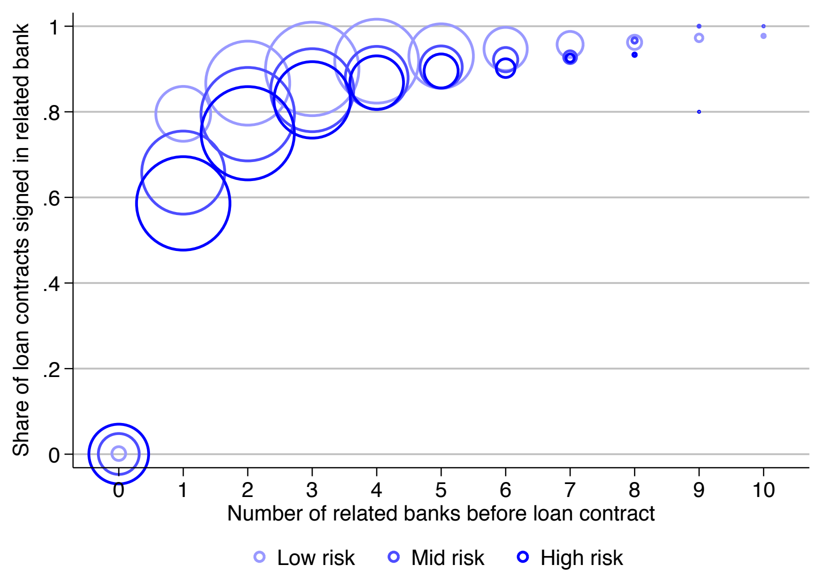


Notes: This figure displays interest rate margins. The red line displays the density of raw interest rate margins in the data. Each additional density displays margins residualized by a increasingly richer sets of covariates, from month FEs to month-bank-size-term-risk FEs.

Figure A.4: Relationship between borrowers and banks



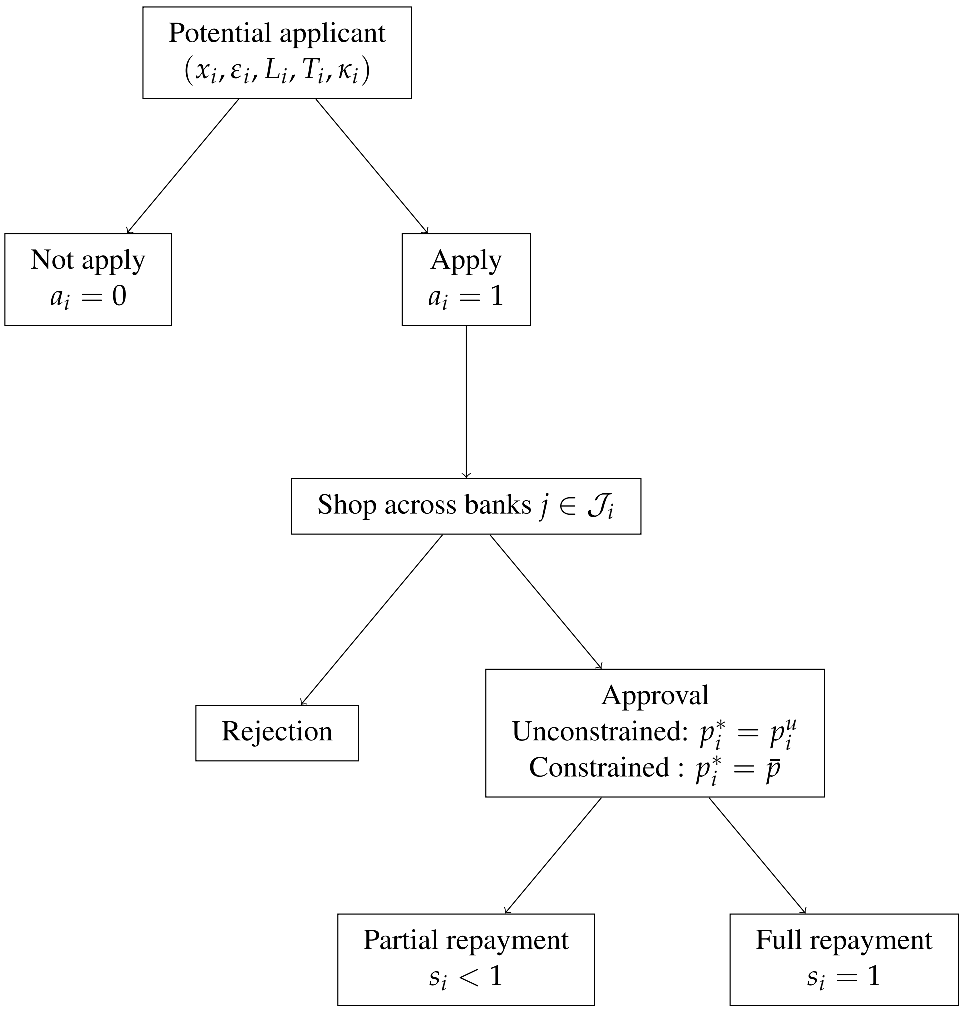
(a) Number of related banks



(b) Loans at related banks

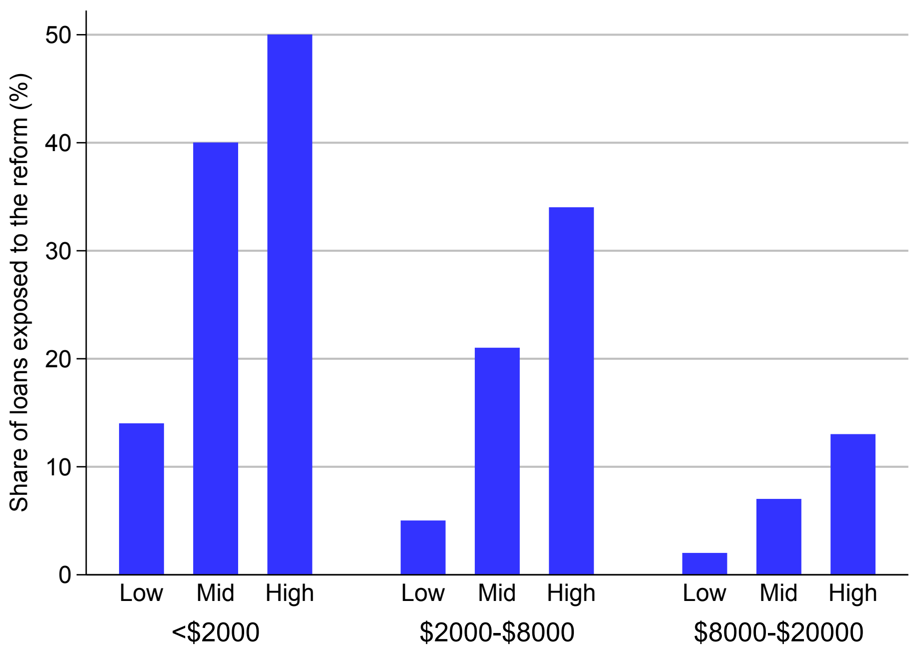
Notes: Panel (a) describes the share of previously related banks for each borrower in the dataset. Panel (b) describes the share of loan contracts signed with a previously related bank for each tercile of borrower risk and each number of previously related banks.

Figure A.5: Timing of the model



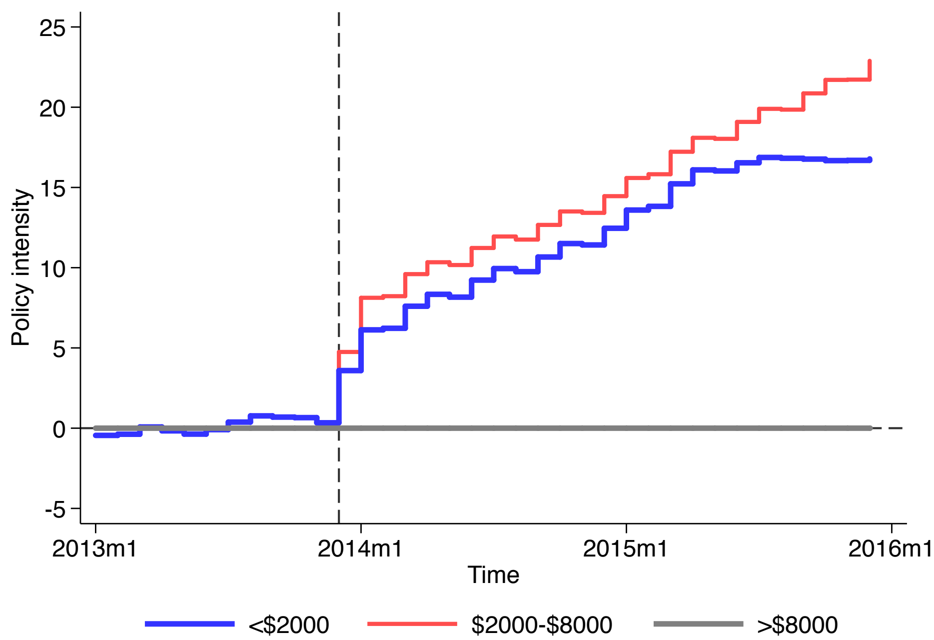
Notes: This figure displays the timing and structure of the model.

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



Notes: This figure displays measure of exposure to price 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 Nov 2013 at interest rates higher than the interest rate cap once the policy was fully in place in Dec 2015.

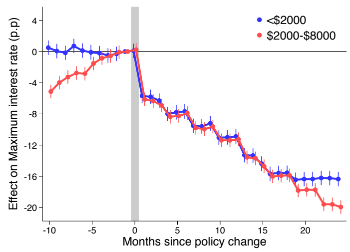
Figure A.7: Treatment intensity variable



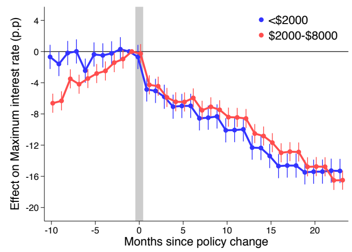
(a) Evolution of treatment

Notes: This figure displays the evolution of the treatment intensity variable defined in Section 4.2. This variable is defined as $\Delta^{\bar{p}}_{s,t} \equiv (\bar{p}_{s,0} - \bar{p}_{s,t}) - (\bar{p}_{>8000,0} - \bar{p}_{>8000,t})$ 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 $>$\$8,000.

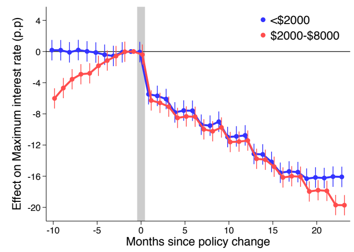
Figure A.8: Differences in differences policy effects through time on price outcomes



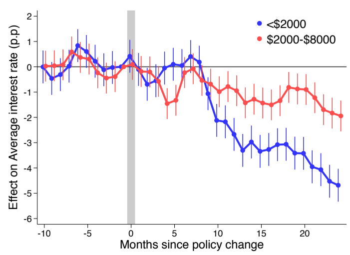
(a) Max interest rate, all



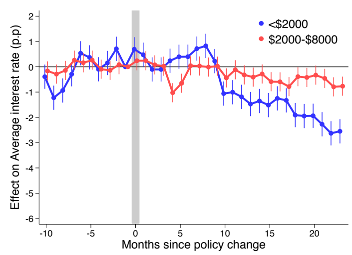
(b) Max interest rate, low risk



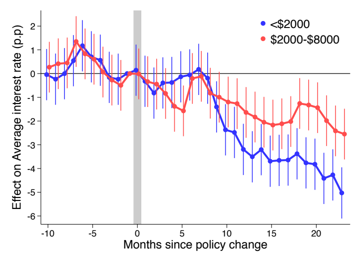
(c) Max interest rate, high risk



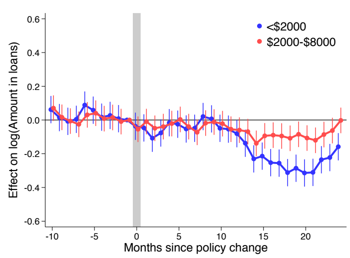
(d) Average interest rate, all



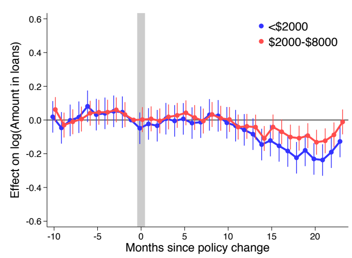
(e) Average interest rate, low risk



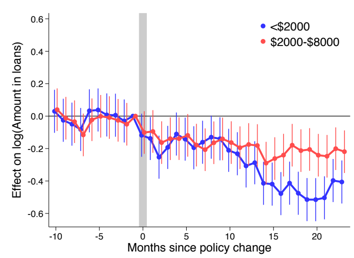
(f) Average interest rate, high risk



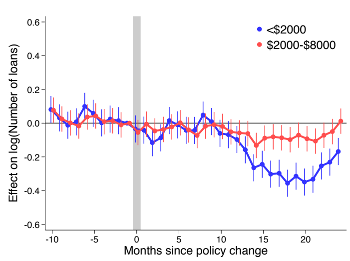
(g) Amount in loans, all



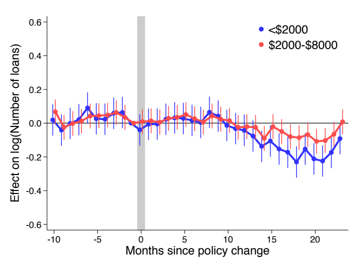
(h) Amount in loans, low risk



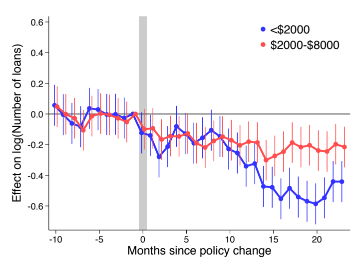
(i) Amount in loans, high risk



(j) Number of loans, all



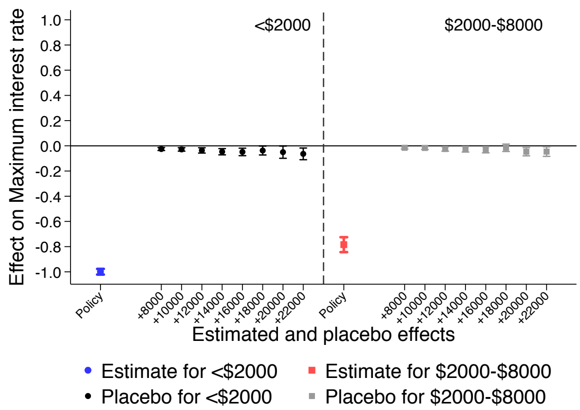
(k) Number of loans, low risk



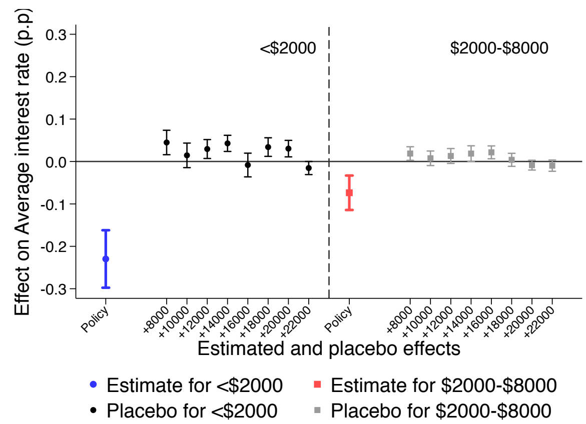
(l) Number of loans, high risk

Notes: These figures display results from estimating equation (5). Each row of figures displays results for a given outcome, while each column does so for a different borrower risk group. Within each plot, dots indicate estimated policy effects for a given month while dashed lines indicate standard errors. All regressions are weighted by the number of loans in the product-risk bin before the policy was implemented. Results for loans in \$0-\$2,000 and \$2,000-\$8,000 are displayed in blue and red respectively.

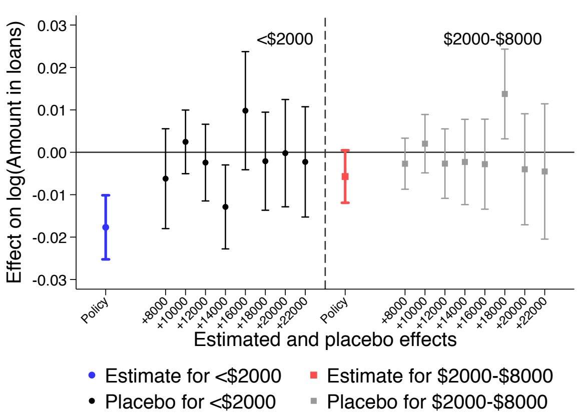
Figure A.9: Differences in differences effects of placebo policies



(a) Maximum interest rate

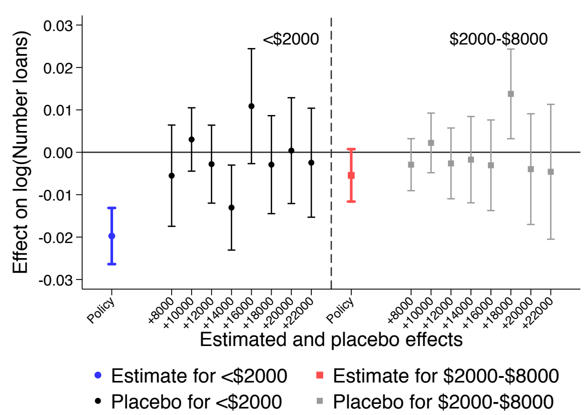


(b) Average interest rate



t

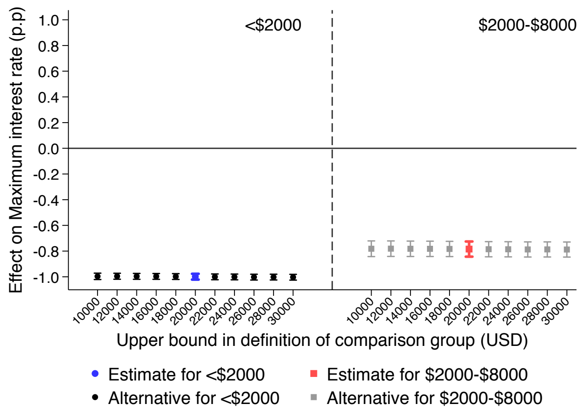
(c) Amount in loans



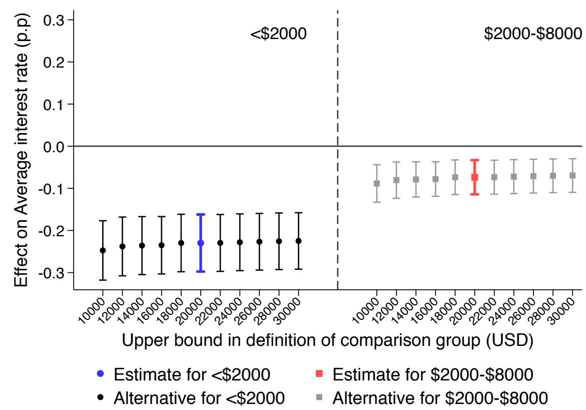
(d) Number of loans

Notes: This figure displays the contrast between our estimates of policy effects 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 in $0-$2,000 and the left panel displays results for loans in $2,000-$8,000. Placebo policies are constructed by using the same policy intensity variables to estimate effects of price regulation on different parts of the loan size distribution. The first placebo policy adds $12,000 to the actual policy definition, and subsequent placebo policies subsequently add $2,000. Each dot indicates the estimated coefficient, while 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.

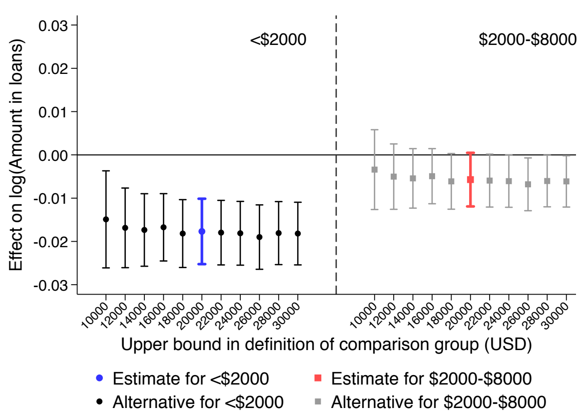
Figure A.10: Differences in differences effects under alternative comparison group size



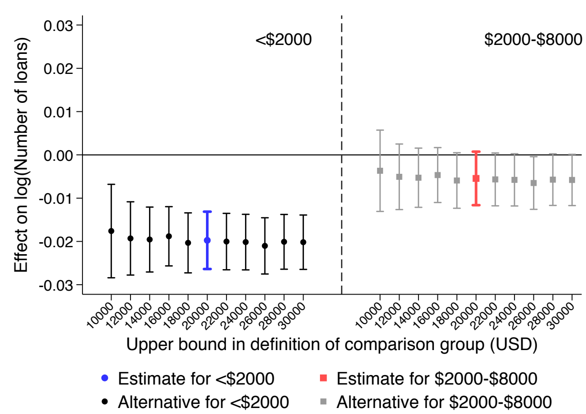
(a) Maximum interest rate



(b) Average interest rate



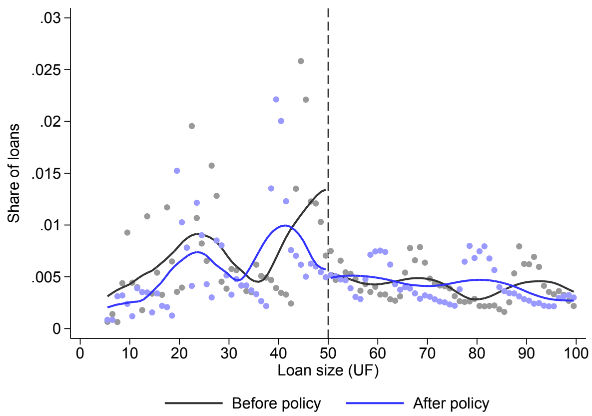
(c) Amount in loans



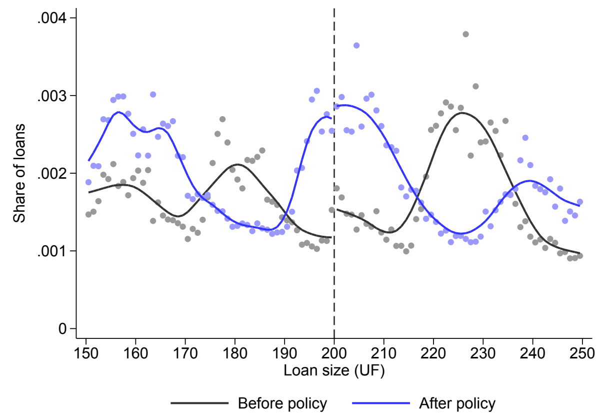
(d) Number of loans

Notes: This figure displays the contrast between our estimates of policy effects 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 in $0-$2,000 and the left panel displays results for loans in $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.

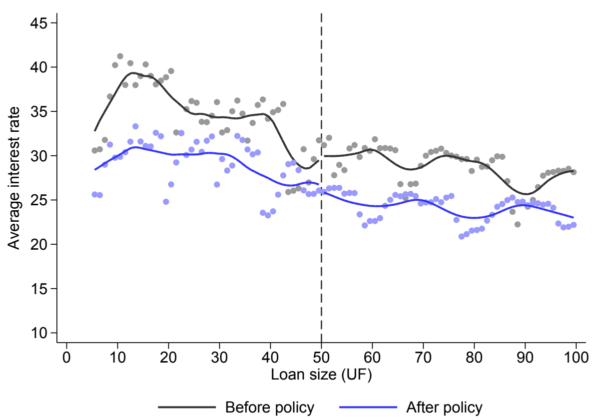
Figure A.11: Distribution of loan size around policy thresholds



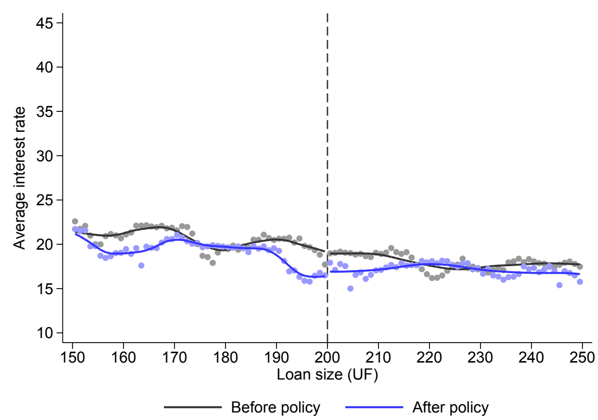
(a) Loan size around $2,000 threshold, before and after



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



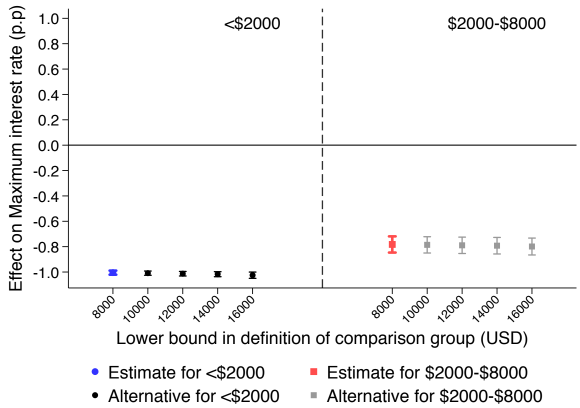
(c) Rates around $2,000 threshold, before and after



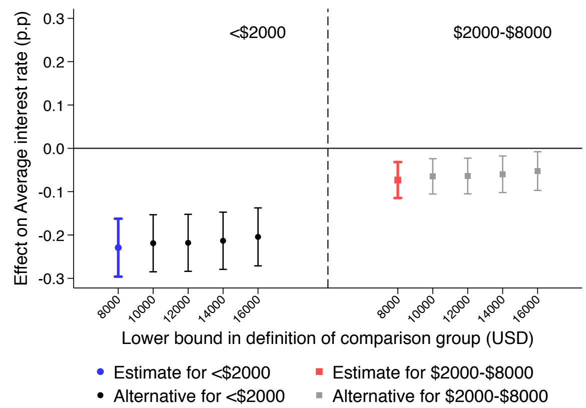
(d) Rates around $8,000 threshold, before and after

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 Jan 2013 and Nov 2013. Blue dots indicate loan originations in the last semester before the policy was fully in place, between Jan 2016 and Nov 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.

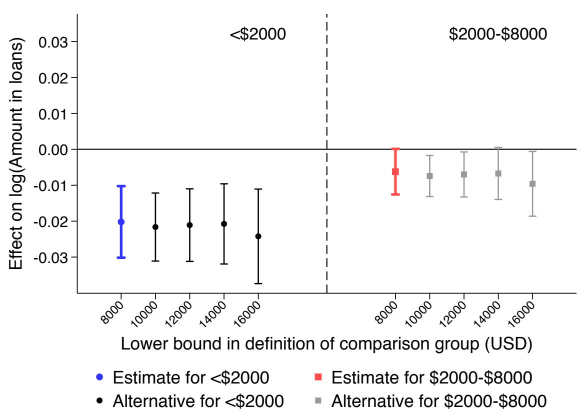
Figure A.12: Differences in differences effects under alternative comparison groups



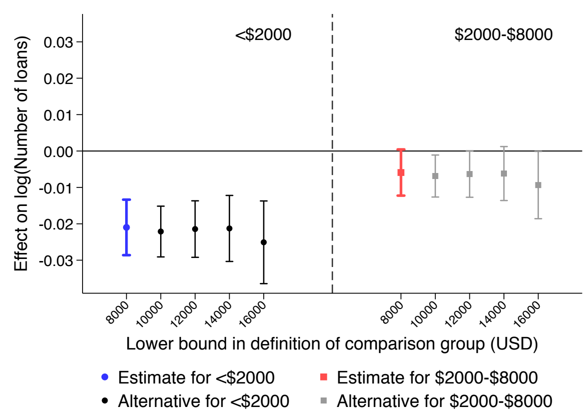
(a) Maximum interest rate



(b) Average interest rate



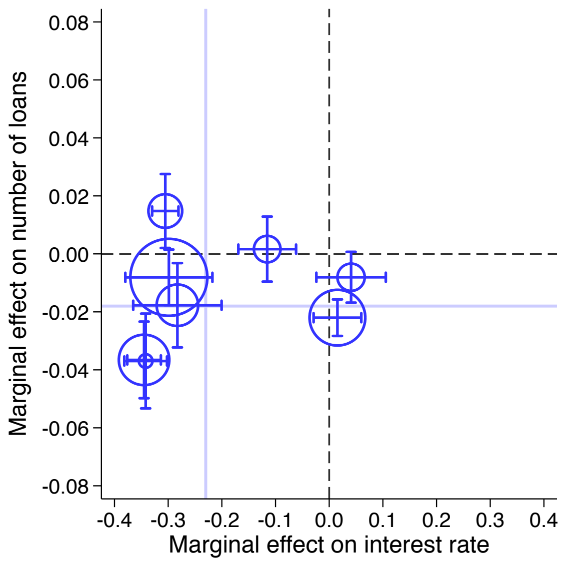
(c) Amount in loans



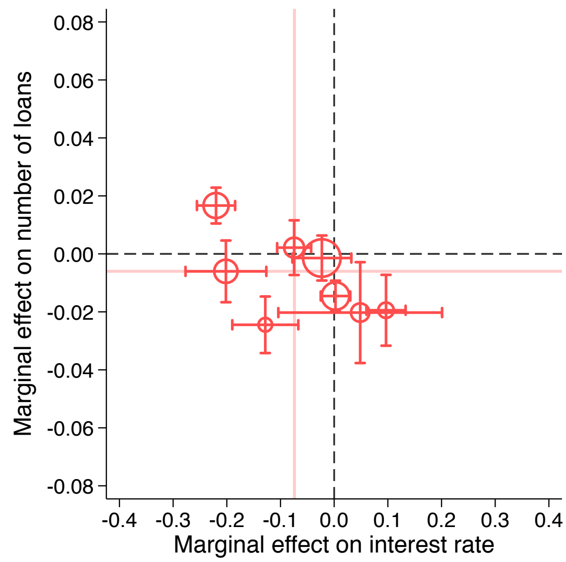
(d) Number of loans

Notes: This figure displays the contrast between our estimates of policy effects 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 in $0-$2,000 and the left panel displays results for loans in $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: Heterogeneity in effects across banks



(a) Loans in $0-$2,000



(b) Loans in $2,000-$8,000

Notes: These figures display marginal effects of price 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.

Table A.1: Determinants of loan performance

	(1)	(2)	(3)	(4)	(5)
	1{Default}				
log(Income)	-0.485***	-0.486***	-0.677***	-0.673***	-0.673***
	(0.004)	(0.006)	(0.006)	(0.006)	(0.006)
Consumer debt to income ratio	0.044***	-0.037***	0.065***	0.065***	0.065***
	(0.004)	(0.007)	(0.007)	(0.007)	(0.007)
Mortgage debt to income ratio	-0.200***	0.157***	0.114***	0.119***	0.119***
	(0.006)	(0.012)	(0.012)	(0.012)	(0.012)
log(Consumer debt)		0.245***	0.244***	0.240***	0.240***
		(0.014)	(0.014)	(0.014)	(0.014)
No consumer debt		1.362***	1.223***	1.062***	1.061***
		(0.036)	(0.036)	(0.038)	(0.038)
No consumer debt ≥90-day default		-0.515***	-0.385***	-0.385***	-0.385***
		(0.019)	(0.019)	(0.019)	(0.019)
No consumer debt <90-day default		-0.438***	-0.437***	-0.437***	-0.437***
		(0.009)	(0.009)	(0.009)	(0.009)
Consumer ≥90-day default to debt ratio		0.633***	0.676***	0.666***	0.666***
		(0.040)	(0.040)	(0.040)	(0.040)
Consumer <90-day default to debt ratio		0.543***	0.720***	0.691***	0.691***
		(0.105)	(0.106)	(0.107)	(0.107)
log(Mortgage debt)		-0.786***	-0.777***	-0.806***	-0.807***
		(0.046)	(0.049)	(0.048)	(0.048)
No mortgage debt		-0.760***	-0.859***	-0.932***	-0.933***
		(0.088)	(0.093)	(0.093)	(0.093)
No mortgage debt ≥90-day default		-0.383***	-0.409***	-0.427***	-0.427***
		(0.054)	(0.055)	(0.055)	(0.055)
No mortgage debt <90-day default		-0.619***	-0.605***	-0.609***	-0.609***
		(0.028)	(0.028)	(0.028)	(0.028)
Mortgage ≥90-day default to debt ratio		0.105	0.255*	0.267*	0.268*
		(0.145)	(0.145)	(0.145)	(0.145)
Mortgage <90-day default to debt ratio		-2.781***	-2.059***	-2.116***	-2.115***
		(0.476)	(0.442)	(0.443)	(0.443)
Change in consumer debt		0.251***	0.241***	0.241***	0.241***
		(0.006)	(0.006)	(0.006)	(0.006)
Change in consumer debt ≥90d default		0.009**	0.003	0.003	0.003
		(0.004)	(0.004)	(0.004)	(0.004)
Change in mortgage debt		-0.012***	-0.014***	-0.014***	-0.014***
		(0.003)	(0.003)	(0.003)	(0.003)
Change in mortgage debt ≥90d default		0.044***	0.030***	0.029***	0.029***
		(0.007)	(0.008)	(0.008)	(0.008)
Age			-0.438***	-0.435***	-0.435***
			(0.004)	(0.004)	(0.004)
Female			-0.390***	-0.391***	-0.391***
			(0.008)	(0.008)	(0.008)
Previously related to any bank				-0.236***	-0.236***
				(0.017)	(0.017)
Regional unemployment rate					0.020***
					(0.007)
Constant	-1.970***	0.132	0.182**	0.495***	0.500***
	(0.027)	(0.087)	(0.091)	(0.093)	(0.093)
Observations	738,142	738,142	737,686	737,686	737,686
Pseudo R-squared	0.034	0.058	0.087	0.087	0.087
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: Policy 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)		
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)
Borrower risk bin	All	Low	High	All	Low	High	All	Low	High	All	Low	High
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		
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)
Borrower risk bin	All	All	All	All	Low	High	All	Low	High	All	Low	High
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 Dec 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.

Superintendencia
de Bancos
e Instituciones
Financieras
Chile