Removing the Fine Print: Standardization, Disclosure, and Consumer Loan Outcomes*

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Abstract

Consumers face a choice when evaluating financial contracts: study the fine print and incur a cognitive cost or ignore it and risk costly surprises in the future. We use a pair of policy changes in Chile meant to reduce the costs of fine print in consumer decisions: the first improves disclosure and the second standardizes and regulates contract features. With administrative data from the banking regulator on consumer loans, we use a regression discontinuity design to estimate the causal effects of these regimes. Consumers offered standardized contracts experienced 40% (14.4 percentage points) less delinquency. Using a difference-in-differences design, we that sophisticated borrowers are helped most by increased disclosure, while unsophisticated borrowers benefit more from product standardization. Additionally, we show that only sophisticated borrowers—who benefit from the informational disclosure treatment—leave less "money on the table." We contextualize these results in a stylized model that predicts that financially sophisticated will benefit from disclosure while unsophisticated borrowers will benefit from standardization based on differences in the cost of studying.

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1 Introduction

Financial contracts often contain complex webs of fees and add-ons in the fine print. Consumers must therefore make a choice to study the fine print and incur a cognitive cost or ignore it and risk the possibility of costly surprises. Consider the case of loans: borrowers with limited attention and/or financial literacy may choose not to study pages of fine print. But doing so is risky. One may unknowingly purchase superfluous debt insurance, for example, and lack the funds to make a loan payment as a result.

Governments have frequently used two types of regulation to reduce information asymmetries that result from the fine print. One strategy is *standardization*: regulations such as the Durbin Amendment eliminate features of financial contracts that the government deems pernicious. Consumers cannot be surprised by fees that firms can't charge. Another strategy focuses on *increased disclosure*. Regulations such as the Truth in Lending Act and SEC's disclosure rules assume that consumers can make correct decisions so long as they can easily access the appropriate information. We ask two questions: Do standardization and increased disclosure lead to better loan outcomes? If so, does one size fit all or are different regulations helpful for different consumers?

To evaluate these questions, we use a unique natural experiment and an administrative data set with the entire consumer bank loan segment of Chile (roughly six million loans). Our primary identification strategy uses a regression discontinuity design that exploits features of disclosures in loan contracts. Borrowers who asked for loans less than a specified amount had to be provided with a standardized contract. This contract removed superfluous insurance add-ons that were commonly charged to borrowers and included improved disclosure, notably a breakdown of all loan fees and a total loan cost stated monthly. Comparing these borrowers' loan outcomes for those just about the cutoff that experienced no change in regulation, we find that the improvements in standardization and disclosure reduced delinquency by 14.4 percentage points (40%) and reduced default by 1.6 percentage points (94%). In order to separate the effects of standardization and disclosure, we take advantage of a law that was introduced a year later, which improved disclosure for all loans. Because borrowers who took out loans below the cutoff still had access to the standardized loan option, we can use this cutoff to estimate the effect of standardization alone, which we find to be statistically insignificant. We can therefore attribute the effects from the first law to increased

disclosure.

Our regression discontinuity design is able to overcome a common problem associated with any regression discontinuity that uses an endogenous variable such as loan amount as the running variable. For regression discontinuity design to capture a causal effect, borrowers and lenders must not manipulate the loan amount to fall on either side of the cutoff. This assumption is plausible in our context because of a unique feature of Chile's financial system: consumer loans and transactions are conducted in one currency, Chilean pesos, while the regulation applies at a cutoff in an second, inflation-adjusted currency, Unidad de Fomento or UFs. As consumers are likely to target their loan amount in pesos, they are unlikely to manipulate their loan amount in UF to be above or below the cutoff based on the daily exchange rate between the two currencies. Indeed, conducting a McCrary density test (2008), we find no bunching of loan volume above or below the cutoff. Additionally, we find no evidence for borrower selection on observables on either side of the cutoff, which is reassuring given that we can observe almost all of the objective borrower characteristics that banks use to make loan decisions (credit risk, income, age, neighborhood, and gender). Of course we cannot definitively rule out borrower selection on unobservables, where the lender can see borrower features that we cannot (e.g. whether the borrower sounds financially sophisticated in conversation), however, the lender would be expected to price such unobservables into the interest rate, which also does not change discontinuously around the cutoff.

Our regression discontinuity results suggest that improved disclosure is the main mechanism by which consumers default less. However, these borrowers are taking out very large loans and are arguably more financially sophisticated than borrowers that the regulation aimed to target. We therefore use a second identification strategy to assess the external validity of the regression discontinuity results across the rest of the population. Specifically, we use a difference-in-differences design to evaluate the impacts of financial regulation on sophisticated and unsophisticated borrowers. Although the assumptions necessary for this second identification strategy are stronger than those required for the regression discontinuity design, they provide a richer understanding of the impact of financial regulation on heterogeneous consumers. As a proxy for financial sophistication, we use borrowers from neighborhoods with three levels of average education: less than high-school, high-school (our median borrower), and university. By comparing these borrowers, we can evaluate how borrowers from neighbourhoods with differential financial sophistication are affected by these

regulations. Standardization reduced delinquency by ten percentage points for (unsophisticated) borrowers from neighbourhoods with on average less than a high school education, and five percentage points under improved disclosure. In contrast, sophisticated borrowers from neighbourhoods with on average more than a high school education are not delinquent significantly less under the standardization regime, but improved disclosure reduces delinquency 10 percentage points. These results suggest that improved disclosure helps more financially sophisticated borrowers avoid missed payments, but not less financially sophisticated borrowers. In order to reduce delinquency rates for financially unsophisticated borrowers, having a standardized contract is more effective.

Lastly, we try to determine whether these initial loan delinquency rates are caused by borrowers obtaining more favourable loan terms at origination, or if these results are better explained by repayment behaviour by the borrowers. To do so, we evaluate whether these regulations led borrowers receiving lower markups, or "money on the table". We conceptualize lower markups as a measure of the difference in price a borrower pays for a product as compared to an "ideal" price that they might have gotten had they had greater bargaining power or searched longer. We use dispersion in rates as a proxy for money on the table, as price dispersion is used as a sufficient statistic for search costs (Hong and Shum 2006). Across our price dispersion measures, we find that dispersion decreased for sophisticated (university educated) borrowers in both regulations, and that the reduction was larger in the disclosure period. In contrast, dispersion increased for less sophisticated borrowers.[1] This suggests that more sophisticated borrowers were able to negotiate better initial loan terms, which could explain their reduced delinquency rates, while less sophisticated borrowers did not receive such improvements and thus these regulations were more likely to influence their repayment behaviour.

We then develop a stylized model to explain why disclosure and standardization have heterogeneous impacts on educated and less educated consumers. We model a borrower's decision whether to study a loan contract and link this decision to delinquency probabilities. Intuitively, if a borrower has a very high cost of studying, they are unlikely to study loan contracts (even with improved disclosure) and will only be protected by regulations that discourage lenders from hiding penalties or extra fees in the fine print. If borrowers have lower costs of studying, improved

[1] While a full evaluation of the implications of this regulation for search costs is outside the scope of this paper, our companion paper (Truffa et al. 2018) uses a partial equilibrium model to evaluate disclosure's effect on search costs, banks' strategic behaviour, and welfare.

disclosure increases the probability that the borrower will study features of the loan and potentially obtain better rates. Sophisticated consumers do not necessarily benefit from standardization, since it encourages them not to study because they know their downside is limited. Our paper suggests that one-size financial regulation does not seem to fit all in either empirics or in theory.

Our paper is structured as follows. Section 2 provides a review of the literature. Section 3 describes aspects of the financial system and our regulatory interventions. Sections 4, 5, 6 present our regression discontinuity's identification strategy, data, and results. Sections 7 and 8 present our results on borrower heterogeneity and "money on the table", respectively. Section 9 presents a stylized model that contextualizes our empirical results. Section 10 concludes.

2 Literature Review

It is well-documented that consumers frequently make sub-optimal decisions about complex financial products. These include health insurance (Handel and Scwartzstein 2018, Handel 2013, Abaluck and Gruber 2011), index funds (Hortaçsu and Syverson 2004), pensions (Illanes 2016 and Luco 2013) and loans (Zaki 2018). These mistakes could be the result of information asymmetries between borrower and lender (Gabaix and Laibson 2006, Jin et al. 2018) that prevent borrowers from obtaining the required information to make optimal decisions. While a variety of consumer protection measures have been implemented to solve such market failures (e.g. anti-fraud legislation, fiduciary duties and licensing guidelines for financial professionals) we focus on two in this paper. The first is improved disclosure requirements and the second is for the government to explicitly regulate contract features. We also investigate the impact of such regulation on consumer heterogeneity.

There is a large body of research pertaining to the effects of disclosure, a full survey of which is beyond the scope of this paper. Instead, we limit our analysis to changes in disclosure for products targeted to consumers. Thus far, the literature has been mixed as to disclosure's effects on consumer financial outcomes. Disclosure has been shown to reduce loan take-up for payday loans (Bertrand and Morse 2011). Others have found no effect on interest rate disclosure for credit card take up and a minimal effect for payments (Ferman 2015, Bertrand et al. 2010, Seira et al. 2017 and Agarwal et al. 2014). Consumers are also insensitive to disclosure for savings accounts (Argyle et al. 2016).

However, Woodward and Hall 2010 shows that when consumers are presented with fees and interest rates bundled together, they pay less in fees. In our own setting, Montoya et al. 2017 find that more educated borrowers receive better rates under our same disclosure regulation.

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Our paper contributes to this literature by documenting a large and robust effect of disclosure. We believe we are able to do so for three reasons: first, we measure disclosure mandated by the regulator rather than provided voluntarily by lenders. This is important since past research (Argyle et al. 2016) has found that borrowers disregard disclosure from the lender since they assume it is self-interested. In contrast, borrowers may trust disclosure provided under the aegis of a regulator. Second, we have administrative data on all consumer loan borrowers in the banking sector rather than at a subset of lenders. This allows us, for example, to track borrowers who decide to patronize a different bank after viewing disclosure. This is not possible for many of the papers in the literature, who observe only what borrowers do at the particular lenders under study. Lastly, we observe bi-monthly payment updates on payment and default over the life of the loan. We find a large effect of disclosure on these repayment behaviours. In contrast, many papers in the literature either measure product take up or initial loan terms, on which they find small effects. Similarly, we find that disclosure has minimal effects on initial loan terms except for the most educated of borrowers, which suggests that payment behaviour may be more sensitive to disclosure than initial terms.

Unlike disclosure, we know of no empirical evidence about the impact of standardized financial contracts. Economists (Campbell et al. 2011) have proposed that consumers would benefit from loan product standardization and a theoretical model Heidhues and Kőszegi 2018 predicts that standardization would improve competition in the market, leading to lower interest rates.[2] We believe we are the first empirical study to evaluate these claims. We provide evidence that standardization can also improve consumer outcomes in the form of fewer missed payments by borrowers, particularly for those that are less financially sophisticated, and a companion paper (Truffa et al. 2018).

We are also uniquely able to measure heterogeneous impacts of standardization and disclosure

[2]Standardization is a "liberal paternalist" policy that encourages borrowers to choose the option that regulators assume most borrowers would want if they were fully informed and well-advised (Campbell et al. 2011). Liberal paternalism also underpins the literature on nudging interventions (Thaler 2008, David et al. 2006). Research on nudges generally finds that consumers make better retirement savings decisions and are no worse off on other savings metrics.

on different types of borrowers. Theory (Gabaix and Laibson 2006) suggests that savvy consumers should respond differently to disclosure than naïve ones. We are uniquely positioned to observe these heterogeneous effects, since a diverse population of borrowers in Chile take up the same consumer loan products even though they vary on characteristics like education. In contrast, products like payday loans target a narrower segment of the borrower population (Lawrence and Elliehausen 2008). We find that the impacts of regulations are heterogeneous on a number of dimensions: borrowers from more educated neighbourhoods benefit primarily from disclosure, whereas borrowers from less educated neighbourhoods benefit primarily from standardization. We then develop a stylized model to explain why differences in study costs lead to heterogeneous impacts of standardization and disclosure on delinquency. Empirical and theoretical evidence therefore both suggest that regulatory policies should not be "one size fits all".

We also provide suggestive evidence that disclosure reduces search costs for more educated borrowers.[3] Price dispersion is arguably a sufficient statistic for search costs(Hong and Shum 2006). Although search costs are sometimes taken to depend only on physical constraints like one's distance from a lender, Campbell et al. 2011 argue that search costs may be more correlated with cognitive ability or financial experience. Price dispersion may therefore be a sufficient statistic for whether consumers leave "money on the table" because of physical and cognitive search frictions. Consistent with this, we find that more educated borrowers obtain less disperse and cheaper loans as a result of both standardization and disclosure. Furthermore, these borrowers benefit much more from disclosure than less educated borrowers.

3 Institutional Details

For four reasons, Chile is an ideal laboratory in which to assess the effects of standardization and disclosure regulations. First, Chile's financial system and products generalize to those in developed economies such as the U.S. (section 3.1). Second, Chile has a unique pair of currencies that we exploit in our primary identification strategy (section 3.2). Third, Chile implemented two natural experiments in 2011 and 2012 that allow us to tease apart the effects of disclosure and

[3]Our companion paper Truffa et al. 2018 develops a structural model to estimate the disclosure regulation's affect on search costs and the ensuring partial equilibrium effects for welfare and the banking sector. We find that search costs decrease 10% in response to improved disclosure and that borrowers are 15% better off as a result due to improved competition.

standardization (section 3.3). Fourth, we have access to unusually comprehensive administrative data from Chile's financial regulator. The banking regulatory agency has been collecting detailed information on every loan transaction for the universe of loans, including on loan performance and borrower characteristics since 1982, giving us a window in which assess the effect of financial regulation on consumer outcomes (section 5).

3.1 Chilean Financial System and Products

Chile is the wealthiest country in South America, with a GDP of $24,013 USD per capita as of 2017 (OECD). Similar to the US economy, the Chilean banking system is concentrated in roughly five large national banks (figure 1).[4]

Our analysis focuses on consumer loans offered by Chilean banks. Roughly 15.4% of households carry such a loan and the average loan amount is $3,400 USD. According to a 2014 household finance survey by the Chilean central bank (Banco Central de Chile 2015), these loans are primarily used for home improvement, purchasing clothes, retiring more expensive debt, and occasionally for automobile purchases. Chilean consumer loans are unsecured and offered at fixed rates for a fixed maturity, and the full loan amount is disbursed at the time of borrowing. Although these loans do not have a direct analogue in the US, they fulfill a similar function to US personal unsecured lines of credit. We focus on these loans for two reasons: the first is that because they have relatively short maturities (usually less than two years), we can examine the effect the legislation had over the life of the loan. Secondly, since these loans are unsecured, they are sensitive to information asymmetries which are exacerbated by lenders potentially choosing to hide important information in the fine print.

Similar to the US, Chilean consumers can also use credit cards and lines of credit to fund consumption purchases (e.g. home improvement and clothes). Consumer credit (including consumer loans, credit cards, and lines of credit) is roughly as widespread in Chile as the US, where 63.4% and 56.9% of households respectively hold some form of consumer credit. Chile also offers loans specifically for automobiles, mortgages, and education, although they are less prominent in Chile than the US (table 1). Overall, these data suggest that consumer loans are a) an important source of debt for Chilean households and b) play a role analogous to consumer debt in developed

[4]One unique institution is BancoEstado, a state-backed bank that operates as a for-profit entity.

economies such as the US.

One notable difference between Chile and the US concerns financial literacy: roughly 41% of Chilean adults are financially literate, compared to 57% of those in the U.S. (Klapper et al. 2015). One might worry that disclosure regulations—which were explicitly enacted to help consumers better understand their products (section 3.3)—would have a larger effect in Chile than more financially literate countries. If so, our results might overestimate the effectiveness of financial regulation relative to likely effects elsewhere. Three factors mitigate this concern. First, our regression discontinuity design focuses on consumers who held large loans around a cutoff of approximately $40,000. These consumers are considerably wealthier and better educated than the average Chilean, and therefore most likely more financially literate. Second, Chile's overall financial literacy rate of 16% is comparable to US financial literacy rates in as younger, older, and less educated populations (Lusardi and Mitchell 2007). Results from our event studies, which examine the broader Chilean population, can therefore be generalized to at-risk US populations including the young, old, and less educated. Chile is therefore a representative country in which to study the effects of financial regulation for borrowers. Third, we find that disclosure benefits highly educated consumers more than less educated consumers. If anything, this suggests that we may have underestimated the effects of disclosure in countries with higher financial literacy rates.

3.2 Currency

Chile has a unique pair of currencies, which we exploit to identify the parameters of our regression discontinuity. One of the key identification conditions for a regression discontinuity design is that borrowers do not manipulate the running variable—in our case loan amount—to determine whether they are below or above the cutoff. Since borrowers endogenously choose their loan amount, it is challenging to preserve the necessary random variation around the cutoff.

We can overcome this challenge because Chile has two official currencies. Consumer purchases and loans are denominated in Chilean pesos, while the regulation is implemented in a different currency, Unidad de Fomentos or UFs. UFs were created in 1967 for use in international secured loans. They are primarily used for secured bank loans and mortgages, long-term credit where inflation risk that would normally be borne by the bank is now borne by the borrower. In contrast, consumer loans have a nominal rate and the contract is written in pesos (so the inflation risk during

the life of the loan is born by the bank). Crucially, the UF to peso exchange rate changes bi-weekly, is set at least a week in advance by the government (see table below), and is roughly equally variable in all periods around the regulation (figure 2). Borrowers choose loan amounts in pesos in order to purchase a specific item or service. But depending on exogenous changes to the peso–UF exchange rate, they will fall above or below the regulatory cutoff that is set at 1,000 UF. Despite borrowers endogenously controling their loan amounts in pesos, we still have plausibly exogenous variation in whether borrowers fall above or below the regulatory cutoff in UFs.

Chilean Currency Conversion Rates as of January 1st, 2018

	Peso	USD
USD	615	1
UF	26,795	43

3.3 Regulatory Changes

After the 2008 financial crisis, Chilean President Sebastián Piñera campaigned on and then enacted consumer financial protection measures. Specifically, Piñera's government enacted reforms that allowed the National Consumer Service (SERNAC) to intervene in consumer credit markets. SERNAC is the consumer finance advocate in Chile, the rough equivalent to the Consumer Financial Protection Bureau in the United States. One of SERNAC's central goals was to reduce information asymmetries and predatory contracts in consumer credit markets:

> Financial service providers have not always prioritized their duty to adequately inform consumers so that they can freely decide with whom they should contract. Financial institutions are not providing transparent information to allow consumers to effectively evaluate and compare the costs associated with a credit, like interest rate, commissions and exit costs associated with the termination of the contract.
> -Biblioteca del Congreso National de Chile 2010

Chile introduced two laws – Law 20.448 and 20.555 – that a) *standardized* what terms could appear in loan contracts and b) regulated how information was *disclosed* to consumers. We exploit the differences between Law 20.448 and 20.555 to identify and distinguish the effects of standardization and disclosure regulations on consumer loan outcomes.

3.4 Law 20.448

The first consumer financial regulatory change was announced in December 16, 2010 and implemented on October 24, 2011. The goal of this law was to standardize loan features and improve disclosure for a subset of the market.

The law created a new product known as Universal Credits that had a) standardized loan features and b) increased disclosure requirements. Certain features of Universal Credits are standardized: universal mortgage credits must have fire and earthquake insurance, for example, while universal consumer credits cannot have added insurances such as disability or life insurance. Prior to the legislation, banks often automatically added extra insurances to consumer credits, which could add approximately 5 percentage points per year (roughly 20% of the average interest rate). If the consumer desired to add features such as insurance to their Universal Credit, these features had to be explicitly contracted on and agreed to by both the lender and the consumer. While such features were standardized across lenders, banks could charge different interest rates and origination fees. While the consumer was not obligated to choose a Universal Credit loan, any consumer requesting a loan below certain loan size and maturity cutoffs—1,000 UF ($\sim$ \$40,000 USD) and three years for consumer credits—had to be offered a Universal Credit contract by the lender.

Universal Credits also had increased disclosure. Universal loan contracts had to be presented with an effective interest rate, which rolled the interest rate together with all fees associated with the credit. This effective interest rate, called annual charge indicator or "CAE", is equivalent to APR in the U.S and was not presented prior to the regulation.Additionally, Universal Credit contracts had to include the monthly payment, total cost, and fee breakdown of the loans. An example of a Universal Credit loan contract can be seen in Figure 3.

The introduction of Universal Credit contracts in October 2011 allows us to use a regression discontinuity design to estimate the effect of standardization and disclosure on consumer credit outcomes. Since Universal Credits are offered as an option for all loans between 1,000 UF and there is exogenous variation for whether a loan is valued at 1,000 UF (section 3.2), we compare loans just above and just below the cutoff between October 2011 and July 2012.[5] This comparison gives us a combined treatment effect for standardization and disclosure regulation.

[5]We conduct placebo tests for the pre-period and post July 2012 in Appendix A.

As the loans around the cutoff are for approximately $40,000, the borrowers in our regression discontinuity are likely to be wealthy and well-educated. We therefore claim that our RD results reveal the effects of standardization and disclosure for sophisticated borrowers. Our model (section 9) predicts that standardization will not significantly improve outcomes for these sophisticated borrowers. However, our model also predicts that improved disclosure will lower study costs and thus improve outcomes for sophisticated borrowers. We therefore predict that borrowers below the cutoff will show less default and delinquency than those above the cutoff. A caveat is in order here: while our model predicts different effects for standardization and disclosure regulations, we cannot isolate these effects by looking at Law 20.448 alone. To disentangle the effects of standardization and disclosure, we therefore compare the effects of Law 20.448 to those of Law 20.555, which introduced the same disclosure requirements to all loans (section 3.5).

3.5 Law 20.555

Chile's first regulation (Law 20.448) had two prongs: it standardized loan features and improved disclosure for Universal Credits. Improved disclosure was so popular that the incoming administration created a new law (20.555) to expand disclosure requirements to all consumer loans and mortgages. Yet to avoid excessive paternalism, SERNAC did not standardize features for any loans except Universal Credits. Law 20.555 was announced in March 14, 2012 and implemented July 31, 2012. Past this date, all loan contacts had to satisfy disclosure requirements (Figure 3): consumers were presented with CAE (the effective interest rate, equivalent to APR), as well as the monthly payment, total cost, and breakdown of non-contingent and contingent fees. Figure 3 shows the standardized disclosure guidelines specified in Law 20.555. This is similar to the disclosure required for Universal Credit contracts by Law 20.448. The explicit goal of this law was to improve disclosure, thus reducing informational asymmetries between borrowers and lenders. As the Ministry of Finance stated in the law,

> We have noted the existence of informational asymmetries in the financial services market for individuals, where the current attributions of the National Consumer Service (SERNAC) have not been sufficient to resolve them. Therefore, we consider it essential to strengthen the consumer protection of financial services, through the allocation of

greater powers and competencies to SERNAC, improving the delivery of information and carrying out studies that reduce information asymmetries.

-Biblioteca del Congreso National de Chile 2011

We exploit the differences between Laws 20.448 and 20.555 to separately identify the effects of standardization and disclosure. After the introduction of Law 20.555, all loans had roughly the same disclosure requirements. Yet Universal Credits – with standardized features such as the removal of disability and life insurance – were still offered to all borrowers who asked for a loan below $1000 UF in value and three years in maturity. Now the only difference between a borrower below and above the cutoff is that the former was offered a standardized Universal Credit contract (in addition to other contracts). We can therefore use a regression discontinuity to isolate the effect of standardization from the post July 2012 period after the second law (20.555) was implemented. We can then subtract this standardization coefficient from the regression discontinuity coefficient obtained from the first law (20.448) to isolate the effect of disclosure under the assumption that the effects of standardization and disclosure are additively separable. As mentioned previously, borrowers around the 1,000 UF (∼$40,000 USD) cutoff will tend to be sophisticated. Our model therefore predicts that standardization should have an insignificant effect on borrower outcomes, whereas disclosure should have a significant effect.

4 Estimation

Following Lee and Lemieux (2010), our regression discontinuity uses the following equation:

$$\begin{aligned} y_i =& \beta_1 Loansize_i + \beta_2 \mathbb{1}_{\{Loansize_i<1000\}} \\ &+ \beta_3 \mathbb{1}_{\{Loansize_{it}<1000\}} Loansize_i + \gamma_1 X_i + \epsilon_i \end{aligned} \tag{1}$$

y_i represents financial outcomes of interest, in particular whether the borrower ever is delinquent, defaults, or extends their loan. β_1 and β_3 represent the relationship between default, delinquency, and extensions below and above the 1,000 UF cutoff, and β_2 is our coefficient of interest, namely the discontinuity of being just below the loan-cutoff where banks were required to present a standardized option and increased disclosure. Loan size is centered around the cutoff amount of 1,000 UF. Loans at or above three-years maturity were not subject to the regulation, so our analysis focuses only

on loans below three years maturity. Lastly, X_i contains three types of controls: a) controls for the individual borrower—age, credit score, income, marital status, and gender; b) controls for loan characteristics—interest rate, maturity at issue, lender, and neighborhood in which the loan was issued; and c) macroeconomic controls for the interbank rate and the expected inflation rate[6]. We use the bandwidth selection procedure outlined in Calonico et al. 2014 and Calonico et al. 2018. We conduct additional sensitivity tests for bandwidth size and cutoff threshold in Appendix A.

4.1 Standardization versus Disclosure

Our regression discontinuity estimation strategy that exploits variation from law 20.448 does not disentangle the effects of standardization versus disclosure. For this, we exploit the fact that Chile implemented two different regulations at different times. We use implementation periods rather than announcement periods as consumers were plausibly inattentive to the regulatory announcements.

We consider January 2009-October 2011 the pre-period when no regulatory changes were implemented. From November 2011-July 2012, consumers requesting a loan below the cutoff were offered Universal Credits that had standardized loan features (e.g. no disability insurance) and improved disclosure. Conducting the regression discontinuity over loans taken out in this time period gives us an estimate of $\beta_2 = \psi_d + \psi_s$, where ψ_d is the effect of disclosure and ψ_s is the effect of standardization. However, from August 2012 onward, we have the implementation of Law 20.555 that introduced the same disclosure requirements for *all* loans. Lenders were still required to provide Universal Credits to borrowers who requested loans below the 1,000 UF cutoff, but now the only difference between Universal Credits and other loans is that the former had standardized loan terms (e.g. no insurance). In this post period, we therefore have $\beta_2' = \psi_s$, the effect of standardization alone. Thus, $\beta_2 - \beta_2' = \psi_d$ gives us the impact of disclosure alone (section 3.5).

[6]Expected inflation is defined as $(\frac{1+CLP}{1+UF} - 1) * 100$, where the Chilean peso rate is the rate at which Chilean banks borrow pesos between each other for the period of 2 years, and UF is the rate at which Chilean banks borrower from each other in UFs in the same horizon. As this is a swap rate between UF and pesos over a two year horizon, it reflects the expected inflation between pesos and UF as perceived by banks over a two year time horizon.

4.2 Identification Assumptions

Regression discontinuity estimates capture causal effects when individuals just above and below the threshold are similar in every aspect but their treatment status. To determine that our effects are causal, we must establish two identification assumptions. The first is that there should be no bunching in the distribution of loan size around the threshold to ensure that borrowers did not manipulate their treatment status. We verify this assumption in section 6.0.1 The second assumption is that borrowers are similar above and below the cutoff so that our effects are due to treatment rather than borrower selection. We affirm this assumption by evaluating the distribution of covariates around the cutoff in section 6.0.2.

5 Data

We use administrative data on the universe of consumer loans from the Chilean banking regulator, the Superintendencia de Bancos e Instituciones Financieras (SBIF).[7] We observe many of the objective borrower characteristics that banks use to assign loans: age, income, marital status, gender, and the bank's credit risk score for borrower. We see each loan's amount, rate, and maturity, as well the lender and location where that loan was issued. We then follow the loan in monthly intervals after its issuance, which is essential to evaluate borrower outcomes such as delinquency and default. To construct our sample, we start with an initial sample size of 7,655,263 unique consumer loans in Chile, representing roughly 95% of the population of consumer bank loans between January 1, 2009 and December 31, 2014. We drop all loans that do not go to Chilean citizens or that have missing observations for any of our control variables. This leaves us with a final sample of 5,097,802 unique loan observations. We then collapse the full history of the loan to one observation.

Table 3 presents our summary statistics. Roughly one quarter of our borrowers miss one payment or more ("ever delinquent"). One percent of our borrower sample is in default at some point in the life of their loan (default is defined as three missed payments and judicial proceedings initiated). The nominal interest rate that includes all fees grows over time from a mean of 19% to a

[7]The SBIF recently merged with the Commissión para el Mercado Financiero (CMF) on June 1st, 2019 and the merged entity is known as the CMF.

mean of almost 30% in 2013.[8] The average loan amount grows over time from 113 UF to an average of roughly 130 UF between November of 2011 and July of 2012, before falling again to roughly 100 UF for loans issued in 2013 (figure 5). Our demographic characteristics like the fraction of females, age, and the fraction married are stable over the sample period, with slightly less than half of borrowers being female with an average age of 44 and roughly 60-70% of borrowers are married. Most loans are roughly 24 months in maturity, which allows us to see the full history of the loan for most loans during our sample period. The credit risk measure is an indicator from zero to one that represents the fraction of each loan that is set aside by the bank as a loan reserve. Between 8-10% of the median loan is provisioned for future losses. The more a bank provisions against a customer, the riskier they are perceived to be. Annual income is roughly 500 UF, which translates to roughly $22,000 USD per year, though the standard deviation in income is large.

On average, borrowers take out six loans and have four loans outstanding at a time. The average borrower has roughly $5,600 USD in outstanding debt and will borrow roughly $10,000 USD more in future debt after we observe a loan.

5.1 Discontinuity Sample

Since our regulations apply to loans below three years maturity, we further restrict our sample to those loans. Using the bandwidth selection procedure outlined in Calonico et al. 2014 and Calonico et al. 2018, we then restrict our sample to loans 138.5 UF (roughly $5,000 USD) above and below the regulatory cutoff of 1,000 UF between November 2011 and July 31, 2012. With these restrictions, we obtain 1,088 observations. Table 4 presents detailed statistics on the discontinuity sample, while table 5 compares loan and borrower characteristics of the discontinuity sample and full sample. Compared to the full sample, loans in the discontinuity sample are less likely to be delinquent and default (though this difference is not statistically significant) but are significantly more likely to be extended or renegotiated. Loans around the discontinuity also have interest rates that are roughly half that of the full sample (25% vs 12%). As the loans in the discontinuity sample must be below three years to be offered a Universal Credit, the whole sample average maturity of

[8] While the average interest rate in our sample may seem high, it is consistent with, and even on the low end, of interest rates on consumer debt in other Latin American countries. For example, credit card interest rates in Mexico are between 35 and 700% APR and average credit card rates in Brazil are between 58 and 700%. Venezuela and Costa Rica have average rates of 29% and 32% respectively. For consumer credit, Panama has an average rate of 9.18%, while Argentina's is 34.5% APR.

25 months is mechanically larger than the discontinuity sample (by six months).

As loans around the discontinuity are much larger than other loans, we find a statistically significant difference in loan size between the two samples. Surprisingly, credit risk (fraction of loan amount provisioned by the bank) is slightly larger around the cutoff than the full sample (17% vs 12%). Though the borrower income is roughly three times higher (1,500 UF) in the discontinuity sample compared to the full sample, the difference is not statistically significant due to the large standard deviation in income. We do not find a statistically significant difference across samples in the average number of loans held by each individual (between 5 and 6). Lastly, borrowers in the discontinuity sample tend to live in neighbourhoods with higher levels of education for 30-50 year olds than those in the full sample.

From table 6, we are able to calculate switching behaviour for 2,286,552 borrowers. Of those borrowers in the full sample, 47% switch to take out a loan with a bank different than their previous bank, and 35% of borrowers switch to a bank they had never used before. In the discontinuity sample, 52% of borrowers take out a loan at a new bank they had not previously borrowed at, and the same percentage switch to a bank they had not used before as in the full sample.

6 Results

Our estimates for equation (1) are presented in table 7. We find that standardization and disclosure decreased the probability of being delinquent (ever missing a payment) by 14.4 percentage points. Given that the mean delinquency probability for loans just above the cutoffs is 34.1%, this represents a 41% reduction in the probability of a borrower ever missing a payment. Similarly, with a 1.6 percentage point decrease in defaults on a mean of 1.7%, standardization and improved disclosure reduced the probability of borrowers defaulting on loans by 94%. Since some loans in our sample have their maturity extended, the reductions in defaults and delinquencies could have been due to banks being more likely to renegotiate loans that may have otherwise been delinquent or defaulted. However, our results suggest that loans above and below the cutoff were not extended differentially, suggesting that these were true improvements rather than window dressing on the part of banks. Raw regression discontinuity results are presented in figure and table 8. We see that the discontinuity is significant at the 10% level without controls and at the 5% level after adding

controls for characteristics about the loans substantially reduces the noise around the cutoff. The global polynomial regression for if a loan ever becomes delinquent is presented in the Appendix.

As explained in section 4.1, these estimates give us only the combined effect of standardization and disclosure. We therefore compare the effects of Law 20.448 and Law 20.555 to disentangle the effects of each (section 4.1). Law 20.555 introduced the same disclosure requirements for *all loans* issued after August 2012 (not just Universal Credits). Yet consumers below the 1,000 UF cutoff were still presented with Universal Credits with standardized loan features (e.g. no disability insurance). We can therefore perform a regression discontinuity on loans issued after the August 2012 period to separately identify the effect of standardization. During this period, we find no significant decrease in default or delinquency for loans issued below versus above the cutoff (table 9). Given that the effects of standardization alone are insignificant, we conclude that borrowers were helped primarily by increased disclosure rather than standardization (section 4.1).

These results are consistent with the predictions of our model. Consumers who take out consumer loans of approximately $40,000 USD are likely to be more financially sophisticated. Our model predicts that sophisticated consumers like these will benefit more from increased disclosure—which helps them to make informed decisions—than standardization—which regulates their loan features. Given that the first regression discontinuity has a must greater effect than the second, the remainder of our analysis focuses on Law 20.448.

[9]

We explore other credit outcome variables in table 10. We find that borrowers below the cutoff miss half of a payment less and have missed payments reduced on average by 31 UF (equivalent to $1,200 USD). We also investigate the timing at which borrowers default. If a borrower misses a payment sooner, especially within the first year of the loan, this suggests that they may have misunderstood key payment features about their contract (e.g. the monthly payment amount). In

[9]While our model provides comparative statics for if *all* products are standardized, our estimation strategy will measure the effects of *introducing* a standardized product. Introducing another product can potentially have competitive affects for the lenders' other available options (see Hausman and Leonard 2002). Ideally, we would conduct such an evaluation on our data, we are unable to see if a given contract is a Universal Credit. Broadly speaking, the effect of adding an additional product can be decomposed into a variety effect of consumers valuing more choice in the market, and a price effect with ambiguous sign. In terms of the price effect, from figure 6 we see that the average price generally went up after the introduction of the law change. Additionally, in section 8, this is true even if we consider differing trends in the selection of borrowers or products. Thus we think most of the reductions in default we see are primarily due to the standardization effect of our regulation rather than the price competition of the introduction of a new product.

contrast, if a borrower becomes delinquent later in their loan tenure, this is more likely because of liquidity or income shocks. Our model therefore predicts that borrowers below the cutoff should become delinquent sooner than those above the cutoff. While we present regression discontinuity results for month of delinquency in column 1, our results are not statistically significant due to a small sample size (only 110 loans in total are delinquent in our regression discontinuity sample).

In order to use our full sample size to estimate how disclosure and default influence repayment behaviour, we use a Cox proportional hazard rate model to test if there are differences in delinquency rates over time for borrowers below and above the cutoff. Because the model estimates the cumulative probability of a loan ever being delinquent, rather than being restricted to the loans that actually are delinquent, we are able to obtain more precise estimates on the effect of the regulation on when the loan defaults.

Our results are presented in table 11 and figure **??**. We find that the improved transparency reduced the hazard ratio of delinquency by between 48 and 68% (including fixed effects). This translates to a 32-52% reduction in the cumulative probability of delinquency for loans around the cutoff. Multiplying this by the average rate of delinquency for loans around the cutoff (roughly 30%), this gives us a between 9.8 to 15.6 percentage point decrease in the delinquency of loans, consistent with our results from the regression discontinuity analysis. In addition, from figure **??**, we can see this comes from a rightward shift in the cumulative probability distribution, meaning that borrowers are defaulting later in the transparency regime as compared to the previous regime. Based on Haughwout et al. 2008, who argue that borrowers who default later in the life of the loan are more likely to do so due to income shocks rather than due to taking out a loan that ill-matches their financial situation, this shift provides suggestive evidence that borrowers understand their loan terms better with transparency than under the previous regime.

6.0.1 Manipulation of Loan Size

Given that Law 20.448 was common knowledge, one might worry that borrowers or lenders manipulated loan amounts to either receive or avoid increased disclosure. Lenders may have encouraged borrowers to take out slightly larger loans to avoid increased disclosure, for example, or borrowers may have withdrawn multiple smaller loans to receive it. Such endogenous selection would undermine our causal estimates of the effect of standardization and transparency. Chile's unique

currencies give us reasons to believe that such bunching does not occur (see section 3.2). All consumer loans and purchases in Chile are conducted in pesos while the regulatory cutoffs are set in a separate, inflation-adjusted currency, UFs. Since the UF to peso conversion rate changes every two weeks and is posted by the government, borrowers can at the same time endogenously choose their loan amount (in pesos) while being effectively randomly assigned by the exchange rate to either below or above the cutoff (in UFs). Indeed, figure 9 shows that loan sizes bunch around round numbers in pesos, while there is a much smoother distribution around round numbers in UFs.

Furthermore, aside from the disclosure laws, there is no regulatory reason for banks to treat 999 UF loans any differently than 1,001 UF loans. To confirm that these features eliminate bunching, we conduct a McCrary density test (2008) in figure 10. The percentage change in the log ditribution is measured at 22% with a standard deviation of 22%, showing an insignificant change in the mass of the distribution of loan size around the cutoff. These results suggest that borrowers and lenders did not sort themselves strategically on either side of the loan size cutoff.

6.0.2 Covariates

To check for imbalances on observed characteristics, we replicate our regression discontinuity design using the relevant covariates as outcome variables. We find no significant discontinuities in borrower characteristics (age, credit score, income, marital status, and gender) or loan characteristics (maturity at issue and rate) around the cutoff. This is reassuring for two reasons. First, the richness of our data allows us to rule out selection based on many of the borrower characteristics that banks use to assess credit risk. Second, while we cannot rule out unobservable differences, it is important to note that interest rates are not significantly different above and below the cutoff. If banks were sorting borrowers based on information that we cannot observe (e.g. whether a borrower sounds naïve in conversation), then we would expect to see a discontinuity in rate around the cutoff, which we do not. We do observe a significant discontinuity at the 10% level for expected inflation.[10]

Further robustness checks are described in Appendix A including bandwidth sensitivity, loan size cutoff sensitivity, and McCrary density tests for the pre-period and disclosure period.

[10]While we can't rule out that this is due to noise, we examine potential avenues that might mechanically cause this correlation. The expected inflation significance is not due to an increase in funding costs as the interbank rate is not significant around the cutoff (table 12). The significance is not the result of a relationship with the current exchange rate between UF and pesos as that is not significant either. Lastly, this does not seem to increase the interest rate above and below the cutoff as interest rate is also not discontinuous around the cutoff.

To summarize, we find that borrowers are 40% less likely to miss a payment on their loans, reduce default by 94%, and reduce missed payments by approximately $1,200 USD. While consumers who borrow large amounts, have strong incentives to study their loans even without disclosure and standardization, our results are consistent with the predictions of our model. Borrowers who take out loans in the right tail for size are likely to be financially sophisticated and see large benefits from increased disclosure. Since this result is a local estimate for borrowers with low studying costs, we cannot yet say whether standardization helps unsophisticated borrowers, as predicted by our model. This is a limitation, since Chile explicitly crafted standardization and disclosure regulations to help unsophisticated borrowers.

For this reason we conduct a difference-in-differences analysis which allows us to test whether standardization and increased disclosure have heterogeneous impacts on financially sophisticated and unsophisticated borrowers.

7 Heterogeneity: Difference-in-Differences

To determine the heterogeneous effects of financial sophistication on loan outcomes for borrowers, we would ideally have an individual measure of financial sophistication for each of our borrowers. While we do not have this ideal, Lusardi and Mitchell 2007 show that financial literacy is strongly related to education. Indeed, Montoya et al. 2017 use actual borrowers education and find that more educated borrowers do receive better interest rates as a result of the disclosure law change. We consider a average years of schooling by neighbourhood (comuna) to be a reasonable measure of borrower education. Even if the borrowers themselves have less than a high school education, their spouse, neighbour, family member, etc. may be more educated or financially experienced and can help guide them through the loan process. The measure is sufficiently granular to capture real differences in education as there are 346 comunas in Chile with an average population of 50,000 residents. Table 15 shows individual measures of years of schooling are highly correlated with our measure of average schooling, suggesting this measure is granular enough to reasonably proxy for a borrower's educational attainment.

However, using an average at the neighbourhood level is also correlated with other socio-economic status indicators such as wealth, familial connections. We believe unobservables are

likely correlated with how financially sophisticated a borrower is likely to be. While not the ideal measure of financial sophistication, we believe this neighbourhood proxy is a reaonsable proxy for a borrower's financial sophistication.

We divide our sample into borrowers between the ages of 30 and 59 as of 2016 that live in neighbourhoods where the average educational attainment is equal to or less than 11.5 years of education (or less than high school), more than 11.5 to 12 years of education (roughly high school completion), and lastly neighbourhoods with average educational attainment of more than 12 years of school (at least some university). Table 16 shows the number of loans in each of these groups across our sample period.

7.1 Estimation

As before, we collapse the history of each loan to one observation. We run the following regression separately for highly educated and less educated borrowers using the 11.5-12 year schooling group as a control:

$$y_{it} = \sum_{t(i)=-6}^{14} \left[\alpha_{\tau+t(i)} + \beta_{\tau+t(i)} \times \mathbb{1}_{\{EDU_i\}}\right] + \gamma X_{it} + \epsilon_{it} \tag{2}$$

The coefficients of interest are time dummies interacted with either the sophisticated or unsophisticated dummy variables, representing the treatment effect of being either a sophisticated or unsophisticated borrower by month. We use minimal controls in this specification (age, married, sex, expected inflation, interbank rate, and neighborhood fixed effects), as borrower characteristics like income and credit risk may be endogenously determined by selection into loans as a result of the regulations. Additionally, we consider initial loan characteristics like interest rate, loan size, and maturity to be endogenously determined, so we present their evolution as dependent variables as well. While these variables did not change for our regression discontinuity, we expect that they variables may change over time, and differentially for different types of borrowers.

For regression 2 to be identified, we require a parallel trends assumption for both groups against the control group, and that our control group of high-school educated borrowers does not respond to the regulations. The pre-trends in figure 12 show that delinquency rates for unsophisticated and

control borrowers trend slightly downward six months before the standardization and disclosure regulation is introduced in 2011 but are otherwise fairly flat. In figure 13, there are no discernable pre-trends between the control and sophisticated treatment group. Figure 14 shows the time trends for the control group delinquency rates. As these are time trends, there is no requirement that their coefficients be zero. We find there are no changes in sign directly around the regulatory changes, supporting our assumption that these borrowers were not affected by the regulatory changes.

7.2 Results

Figures 12 and 13 show the estimates of equation (2) for both sophisticated and unsophisticated borrowers. We find that unsophisticated borrowers experience a reduction in delinquency rates of ten percentage points after the introduction of the standardization legislation but are not less delinquent with the enactment of the disclosure legislation introduced in 2012. In contrast, more sophisticated borrowers do not seem to be less delinquent from the standardization of products. However, they experience a decrease of ten percentage points when the more complex disclosure was introduced. This is consistent with our model prediction that suggest borrowers with a high cost of studying or less financially sophisticated borrowers are less likely to be delinquent when standardized products are introduced rather than when easier disclosure is introduced for all products. Additionally, our model predicts that borrowers with a low cost of studying, or financially sophisticated borrowers are more apt to respond to improved disclosure rather than standardization.

8 Money on the Table

While our previous regression and difference-in-differences results suggest that these regulatory changes helped borrowers sort into more suitable loans, we have said relatively little on whether this means borrowers made better choices while shopping for loans. While our companion paper explicitly estimates the change in search costs as a results of these changes (Truffa et al. 2018), we provide suggestive evidence here that whether borrowers made better choices depended on their level of education as well.

We begin by creating categories of observably similar borrowers. While we could examine aggregate statistics on borrower choice, this would not tell us if these measures were changing

because of changes in the composition of borrowers and the products they demanded or if they were choosing the same products but receiving different initial loan terms. To create categories of similar borrowers, we discretize borrowers into buckets based on the following characteristics (similar to the methodology used in Argyle et al. 2017 and Atal 2016): the region the loan originates from, by gender (binary), by marital status (binary), income bins segmented at 622,850, 1,384,110, 2,306,850, 3,229,590, 4,152,330, and over 5,536,440 Chilean pesos per year in annual income based on the tax brackets (PWC, 2017). We also create ten year age bins starting at age 18.

To ensure we compare borrowers obtaining similar products, we cut the product space on two dimentions: maturity and loan size. We create maturity bins of 0-1 year loans, between 1 and 3 year, between 3 and 5 years, between 5 and 7 years, between 7 and 10 years, 10 to 15 year, 15 to 20 year loans, and loans larger than 20 years maturity. For loan size, we create half million peso loan bins up to 2 million pesos, 1 billion loan size bins from 2-7 million loans, a 7-10 million loan size bin, a 10-20 million loan size bin, and a bin for loans over 20 million pesos. This leaves us with a total of 96 product bins with roughly 55 observations per bin. This gives us a total of 3,637,586 loan observations across 96 product bins and 15,550 borrower bins. To ensure we have enough observations to calculate meaningful measures of dispersion, we drop any borrower$\times$product cells with less than 5 borrowers.

Table 17 presents summary statistics for our measure of interest rate dispersion, and therefore measure of quality of choices. We find that in the pre-period, the mean rate difference between the interest rates in the same bin and those of the 25th percentile interest rate in the same bin was 3.5 percentage points. The average difference between rate and the minimum rate in the bins was four times larger at 12 percentage points. The average standard deviation of rates was 7.8 percentage points. After the standardization and disclosure regulation was implemented between November 2011 and July 2012, the difference in rates rises to 6.2 percentage points and 16.8 points respectively and the standard deviation slightly increases to 8.6 percentage points. In the disclosure period, the rate distances are highest with a mean of 8.2 percentage points and 20.2 percentage points respectively and a standard deviation of 9 percentage points.

To evaluate the effect of the different regulations, we regress our measure of rate dispersion on our borrower characterstic variables (female, married, urban, income, credit risk, and age). With this strategy we hope to explain the variation of interest rates within rather than across buckets.

To control for time effects we add year fixed effects and controls for the interbank rate and expected inflation rate between UF and pesos.

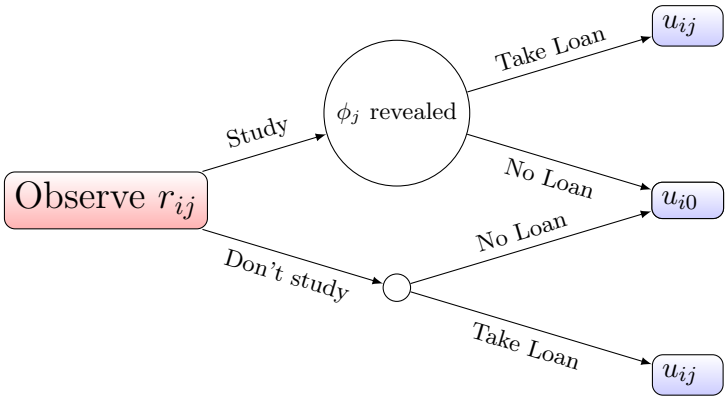
To explore the effects of borrower heterogeneity, we restrict our sample to borrowers that live in neighbourhoods where the average level of education is more than high school and borrowers with the average level of education is below high school, as in our difference-in-difference analysis. We find that across all measures coming from a more educated comuna reduces the dispersion measure by 0.5 percentage points in the case of the 25th percentile rate, by 5 percentage points in the distance to the minimum rate and reduces the standard deviation in a bin by 1.3 percentage points. We also find that while borrowers in less educated comunas received higher dispersion rates in both regulatory periods (1 percentage point versus 3 percentage points in the difference to the 25th percentile rate, 0.7 percentage points versus 4 percentage points in distance from minimum rate and 0.4 to 0.6 percentage points in standard deviation increase), the differences are negative for educated borrowers across the regulatory periods. For more educated borrowers, the difference in the 25th percentile rate decreased by almost 1.5 percentage points in the standardization and disclosure period and by almost 2.5 percentage points in the disclosure period. For the distance to the minimum rate, this was reduced by 2 percentage points in the standardization and disclosure period and almost 4 percentage points in the disclosure period. Lastly, educated borrowers experience a 0.4 percentage point decrease in standard deviation of interest rate in the standardization and disclosure period and a decrease of 1 percentage points after the disclosure regulation is enacted.

While our model only generates predictions for delinquency and default, we can intuitively see how its conclusions about borrower heterogeneity might extend to rate shopping behaviour. Less educated borrowers do not seem to incorporate the regulations into their rate shopping behaviour to obtain lower rates. However, through better searching or better bargaining position, educated borrowers are able to reduce the dispersion in their rates and get lower rates for similar products. We also see that the disclosure regulations were most impactful for more educated borrowers rather than the regulations that merely provided a standardized product.

9 Model

Borrowers randomly observe a loan l_{ij} interest rate quote from a lender j from $J \geq 2$ lenders at rate λ. These lenders offer loan contracts with headline interest rates r_{ij} and fine print conditions that the borrower must anticipate to avoid fees f_j. Once observing a rate, borrowers are faced with the decision to study a loan and subsequently whether to take out the loan. Studying comes at a cost $c(\gamma_i)$ that is a function of the borrower's sophistication γ_i, but eliminates the possibility of costly surprises during repayment, which can lead to default.[11]

Borrowers' financial sophistication is represented as γ_i which is a fixed type that represents their ability to avoid unwanted or unnecessary fees f_j described in the loan's fine print. The borrower knows γ_i, but not the lender. The level of sophistication required to avoid f_j is ϕ_j. Complex and opaque contracts have high ϕ_j, while simple products have a low ϕ_j. The borrower knows ϕ_j if and only if they study.[12]



If the borrower chooses not to take the loan, they receive u_{i0}. u_{i0} can reflect either the utility of the borrower not taking a loan at all, or the utility of taking a loan from a different lender.[13] The borrower chooses to take out the loan from lender j if expected utility of dong so is at least a good as the outside option, $E[u_{ij}] \geq u_{i0}$. If the borrower chooses to take out the loan, their utility is $u_{ij} = v_i - r_{ij} \times l_{ij} - \mathbb{1}[study_{ij}]c(\gamma_i) - \mathbb{1}[\phi_j > \gamma_i]f_j - P\big[m_i - r_{ij} \times l_{ij} - \mathbb{1}[\phi_j > \gamma_i]f_j < 0\big]d_i$. The

[11] Our model is created in the spirit of Heidhues et al. 2018, who model a borrower's decision about whether to study a single contract in detail or browse the headline rate of multiple contracts. In contrast, our model focuses on the decision whether to study, because our identification strategy can directly assess this decision. We return to browsing (that is, search) behavior in a companion paper (Truffa et al. 2018), where we use a structural model to assess how these regulations affect search costs.

[12] We assume that the distribution of ϕ_j is uniform. This allows us to obtain a closed form for our unconditional probabilities of default, though are unnecessary to compute our comparative statics.

[13] Although we do not model search costs here, search costs would increase u_{i0}, since lower search search costs will allow borrowers to search extensively and have better outside options, increasing u_{i0}.

"value" of the loan the borrower receives is v_i, for example, the value of using the loan to conduct home renovations (this value can depend on the loan size but is not required to).

A borrower can avoid all additional fees ($\gamma_i \geq \phi_j$) if and only if the borrower is sophisticated enough to do two things. First, before taking out the loan, the consumer is saavy enough to know *ex-ante* to ask the lender to remove unwanted fees. Second, during repayment, the consumer can avoid conditions that would incur contingent fees such as late or prepayment penalties. Otherwise, the borrower is not sophisticated enough (i.e. $\gamma_i < \phi_j$) and the borrower incurs fees f_j. Furthermore, f_j negatively impacts the consumer by increasing their probability of default, that is, the probability that their monthly income m_i is smaller than the fees associated with their loan ($P\big[m_i - r_{ij} \times l_{ij} - \mathbb{1}[\phi_j > \gamma_i] f_j < 0\big]$). If the borrower defaults, they suffer a default cost d_i.

If $\phi_j > \gamma_i$, then $u_{ij} = v_i - r_{ij} \times l_{ij} - f_j - P[m_i - r_{ij} \times l_{ij} - f_j < 0] d_i < u_{i0}$, i.e., the borrower would regret taking out loan j because they either would have preferred to not take out a loan or should have gone to another lender where they could have avoided additional fees. If a borrower studies the contract from lender j and learns that $\phi_j > \gamma_i$, they will not take out the loan. A borrower therefore chooses to study if and only if

$$E\Big[max\Big\{u_{i0}, v_i - r_{ij} \times l_{ij} - P[m_i - r_{ij} \times l_{ij} < 0] d_i\Big\}\Big] - max\Big\{u_{i0}, v_i - r_{ij} \times l_{ij} - P[\phi_j > \gamma_i] f_j - \Big[P[\phi_j > \gamma_i] P[m_i - r_{ij} \times l_{ij} - f_j < 0] + \big[1 - P[\phi_j > \gamma_i]\big] P[m_i - r_{ij} \times l_{ij} < 0]\Big] di\Big\} \geq c(\gamma_i) \tag{3}$$

We can now link the decision to study with the probability of default. As mentioned before, if a borrower chooses to study, the borrower will take out a loan if and only if $\gamma_i \geq \phi_j$. Therefore, $P[default | study_{ij}, loan] = P[m_i - r_{ij} \times l_{ij} < 0]$. If a borrower chooses not to study, then their probability of default is $P[default | \text{no study}_{ij}, loan] = P[m_i - r_j - \mathbb{1}[\phi_j > \gamma_i j] f_j < 0]$. Therefore, the probability of default conditional on the borrower taking a loan reduces to:

$$P[default_{ij} | loan] = P[\gamma_j > \gamma_i] P[\text{no study}_{ij}] P[0 < m_i - r_{ij} \times l_{ij} < f_j] + P(m_i - r_{ij} \times l_{ij} < 0) \tag{4}$$

Now that we have an expression for the probability of default, we can obtain predictions for how the probability of default will change for heterogeneous consumers depending on the regulations.

9.1 Predictions

We consider two sets of borrowers: unsophisticated ones with higher costs of studying (low γ_i) and sophisticated ones with lower costs of studying (high γ_i), though we still consider borrowers to have a spectrum of study costs within the sets of c_H and c_L. Call c_H and c_L the set of study costs for unsophisticated and sophisticated borrowers. We believe it is a reasonable assumption that the cost of studying a loan contract would be negatively related to a borrower's financial sophistication level.

9.1.1 Disclosure

Increased disclosure makes it easier for consumers to study features of the loan contract. For all borrowers, there is a new study cost function c^d such that, $c^d(\gamma_i) < c(\gamma_i) \forall i$.

Proposition 1. *c_L borrowers will default less under improved disclosure.*

Decreasing c will increase $P[study_{ij}]$, since the right and side of equation (3) is smaller.

Proposition 2. *c_H borrowers will experience no change in default rates under improved disclosure.*

Unsophisticated c_H borrowers have such high costs of studying that $c^d(\gamma_i)$ is still too high to satisfy equation (3). The only borrowers affected by a change in disclosure regulation are sophisticated c_L borrowers. Whether sophisticated borrowers took a loan or not under c_L, under $c^d(\gamma_i)$, they will choose to study and thus the marginal borrower will default at rate $P(mi - r_{ij} \times l_{ij} < 0)$.

9.1.2 Standardization

We interpret loan standardization as a shift in the fee to $f_j^s < f_j \forall j$: that is, borrowers pay smaller fees if $\phi_j > \gamma_i$, since standardized contracts no longer contain features such as costly insurances. So $P[0 < m_i - r_{ij} \times l_{ij} < f_j^s] < P[0 < m_i - r_{ij} \times l_{ij} < f_j] \forall i$, which lowers the probability of default.[14] More importantly, we follow Heidhues, Johnen and Kőszegi (2018) and assume $\phi_j = \phi^s \forall j$: that is, the level of sophisticated required for all loans is equal. We depart from Heidhues, Johnen and

[14] One might argue that unsophisticated borrowers tend to be less wealthy (that is, they have a lower m_i) than sophisticated borrowers. Our model does not rely on this assumption, but it would introduce another channel by which standardization helps unsophisticated borrowers more than sophisticated ones. For lower fees (f^s) should reduce $P[0 < m_i - r_{ij} \times l_{ij} < f_j^s]$ to a greater extent for lower income borrowers than higher-income ones.

Kőszegi (2018), who assume that $\phi^s = 0$, since the borrower may still require sophistication to avoid contingent fees or differential origination fees, but assume that standardization will lower the sophistication required for each contract, in that $\phi^s < \phi_j, \forall \phi_j$. This can decrease the probability that consumers are hit with unexpected surprises ($P(\phi^s > \phi_i)$), but the extent of this gain is heterogeneous across consumers as outlined in the propositions below.

Proposition 3. *The effect of standardization on c_L borrowers is ambiguous.*

Sophisticated c_L consumers already tend to avoid unexpected surprises on most contracts. $P(\phi^s > \gamma_i)$ is therefore only negligibly lower than $P(\phi_j > \gamma_i)$ for these consumers. Yet because $P(\phi^s > \gamma_i)$ and $P(0 < m_i - r_{ij} \times l_{ij} < f_j^s)$ are lower, $P[study = 0]$ increases because the left hand side of (3) is larger. Put informally, sophisticated borrowers are more likely to trust that the standardized contracts have removed contigent and unncessesary fees, which increases their probability of default. Our model therefore predicts that standardization will have a mixed effect on sophisticated borrowers, since it reduces two channels for default while increasing another.

Proposition 4. *c_H borrowers are less likely to default if contracts are standardized.*

Unsophisticated c_H consumers are more likely to be surprised with fees on many contracts, so $P(\gamma^s > \gamma_i)$ is therefore much lower than $P(\gamma_j > \gamma_i)$ for these consumers. Furthermore, these consumers have such high study costs that they study under neither the standardization nor the unregulated regimes (that is $P[study = 0] = 1$ for all regimes). Our model therefore predicts that standardization will substantially decrease the probability of default for unsophisticated borrowers, since it reduces the probability and cost of surprises, while leaving the probability that they study constant.

In sum, our model predicts that financial regulations should have heterogeneous affects across consumers. Sophisticated consumers should default less with increased disclosure, but be largely unaffected (or even worse off) from standardization. In contrast, unsophisticated consumers should default less under a standardized regime but see no benefit from increased disclosure.

10 Conclusion

We exploit two natural experiments in Chile to measure the impact of disclosure versus standardization on consumer loan outcomes. The first exploits a regression discontinuity design using administrative banking data on the universe of loans. We find that disclosure reduces delinquency (missing one loan payment) by 14.4 percentage points or 40% and reduces default by 1.6 percentage points or 94%. We find no significant effects for standardization. These results apply to predominantly wealthy, financially sophisticated population of borrowers around the legislative cutoff.

Sophisticated borrowers may benefit predominantly from improved disclosure that allows them to make informed decisions. But less sophisticated borrowers may benefit more from standardized loan contracts that remove problematic loan features entirely. Motivated by these hypotheses we broaden our sample and use difference-in-differences to compare borrowers in neighborhoods that vary in average levels of education (a proxy for financial sophistication). We find striking heterogeneity across borrowers. Consistent with our regression discontinuity, improved disclosure reduced delinquency by ten percentage points for borrowers from highly educated neighborhoods. In contrast, standardized contracts reduced delinquency by ten percentage points for borrowers from low-education neighborhoods.

While our primary focus is on loan outcomes, we also ask if borrowers (especially sophisticated ones) use information from increased disclosure to make better decisions about their loan terms. In particular, did disclosure help borrowers leave less "money on the table"? Following (Atal 2016), we use measures of interest rate dispersion amongst comparable borrowers and loans as a proxy for money on the table. We find that our regulations reduce dispersion for more educated borrowers but not less educated ones. Results across our identification strategies tell a coherent story about the impact of regulation on heterogeneous consumers. Financially sophisticated consumers benefit most from information, which they use to arrive at better outcomes and leave less money on the table.

Our results suggest that disclosure regulation is most effective at curbing delinquency and obtaining better rates for financially sophisticated and more educated consumers. However, these borrowers are not usually the borrowers regulators are hoping to help when enacting legislation to solve informational market failures between lenders and prospective borrowers. For less finan-

cially sophisticated borrowers, regulations that restrict pernicious loan features by standardizing contracts do improve delinquency rates but do not seem to appreciably decrease the prices or price dispersion for similar products for these borrowers. Further research is required to determine whether regulations targeting transactions between lenders and borrowers are more effective than effective delivery of financial education in improving borrower outcomes and prices.

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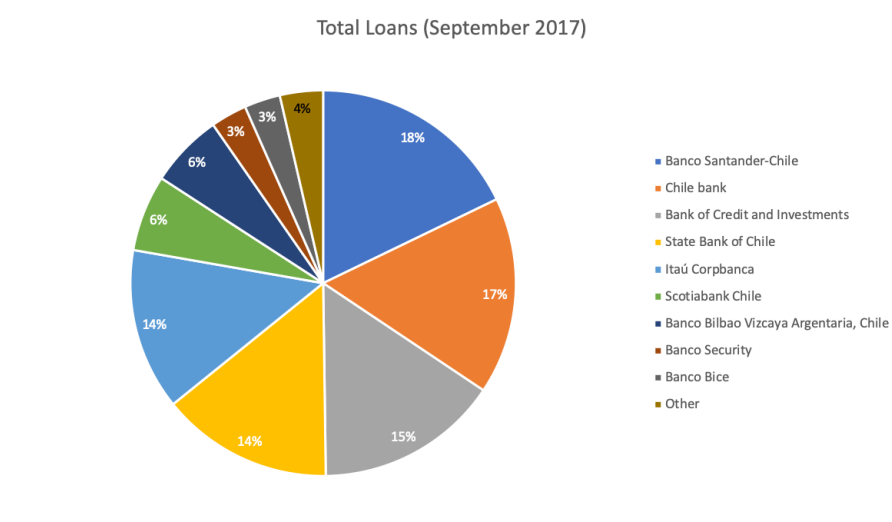
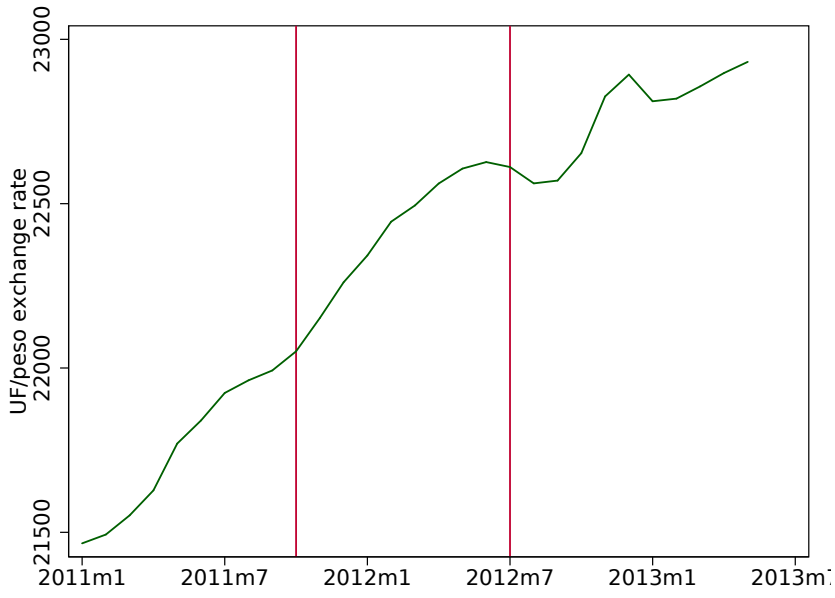
11 Figures and Tables

Figure 1: 2017 Chilean Bank Composition



Source: SBIF
Notes: This figure graphs the market share of total loans across banks in Chile. BancoEstado (State Bank of Chile in yellow) is a state-owned bank that is run as a for-profit entity.

Figure 2: UF to Peso Exchange Rate



Notes: This figure graphs mean monthly exchange rate of UF to pesos. The first red line is the implementation date of law 20.448 (the introduction of Universal Credit Contracts) and the second red line is the implementation date of law 20.555 (disclosure requirements for all loans).

Figure 3: Example of Law 20.448 Universal Credit Contract

CRÉDITO HIPOTECARIO - SIMULACIÓN

Fecha : 24 de Octubre de 2011
UF : $ 22.079,1

Antecedentes del Crédito Hipotecario

	Valores en UF	Valores en $		
Valor Propiedad	5.000,00	110.395.500	Producto	MUTUO UNIVERSAL
Monto Solicitado	3.000,00	66.237.300	Objetivo Préstamo	VIVIENDA
Pago contado	2.000,00	44.158.200	Destino	COMPRA CASA
Porc. Financiamiento	60,00%		Antiguedad	NUEVA
			Meses de gracia	---

Cálculo de dividendo

			Crédito hipotecario				
Plazo (Años)	Tasa Anual %	Dividendo sin seguro UF	Sin seguro de Incendio UF	Seguro Desgravamen UF	Dividendo Total UF	Dividendo Total $	Renta Mínima $
20	4,80	19,30	0,00	0,84	20,14	444.673	1.778.692

Gastos Operacionales

	Valores en UF	Valores en $
Tasacion	2,50	55.197
Legales	5,00	110.395
Notaria	3,00	66.237
Impuesto de Timbres y Estampillas	18,00	397.423
Conservador Bienes Raices	19,00	419.502
Total Gastos Operacionales	47,50	1.048.757

Seguros Involucrados

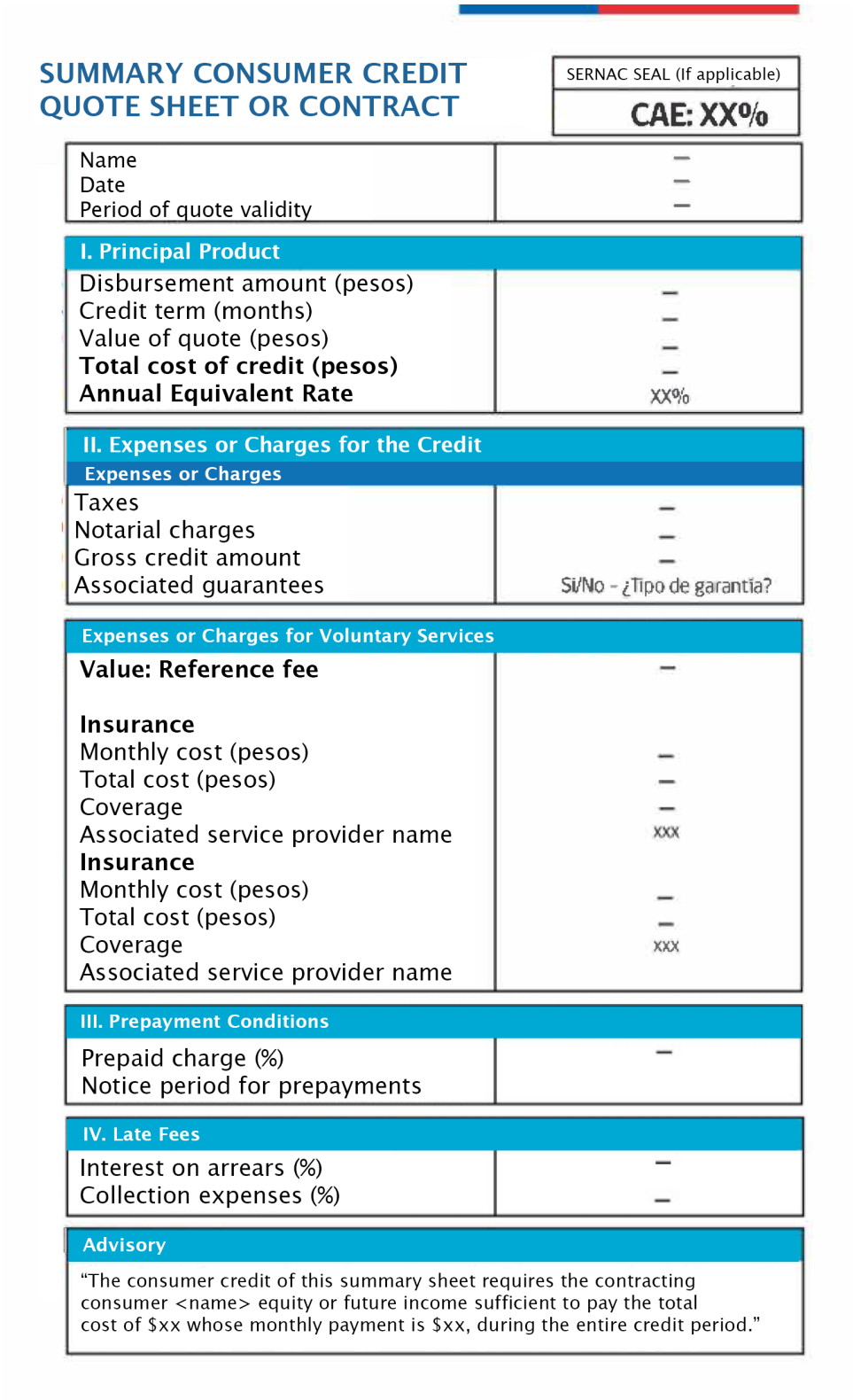
Seguro Desgravamen	1 Asegurado

CAE (**): **5,03%**
Costo Final de Crédito (***): **4.687,98**

(**) Carga Anual Equivalente (CAE) indicador que, expresado en forma de porcentaje, revela el costo de un crédito en un periodo anual, cualquiera que sea el plazo pactado para el pago de la obligación. Contempla el tipo de interés, todos los gastos asociados al crédito, el plazo de la operación; y se calcula sobre base anual.
(***) Costo Final de Crédito es un indicador que, expresado en una suma de dinero, da cuenta del monto total a pagar por el crédito solicitado, sumado lo adeudado por tasa de interés y los gastos asociados al crédito.

Notes: This is an example of a simulated Universal Credit contract outlined by law 20.448 from bank BCI. The main innovation of law 20.448 was the introduction of the middle table (starting with "Plazo"). The Universal Credit contract provided basic information about the credit such as term, annual rate, credit disbursement amount, and minimum monthly payment. The CAE (APR equivalent) is shown at the bottom of the page as well as the final cost of credit. This particular contract is a mortgage contract and not a consumer credit contract, so information on UF amounts is not present for consumer loans as they are denoted in pesos.

Figure 4: Example of Law 20.555 Disclosure Sheet (English translation)



SUMMARY CONSUMER CREDIT QUOTE SHEET OR CONTRACT

SERNAC SEAL (If applicable)

CAE: XX%

Name	–
Date	–
Period of quote validity	–

I. Principal Product

Disbursement amount (pesos)	–
Credit term (months)	–
Value of quote (pesos)	–
Total cost of credit (pesos)	–
Annual Equivalent Rate	XX%

II. Expenses or Charges for the Credit

Expenses or Charges	
Taxes	–
Notarial charges	–
Gross credit amount	–
Associated guarantees	Si/No - ¿Tipo de garantía?

Expenses or Charges for Voluntary Services

Value: Reference fee	–
Insurance	
Monthly cost (pesos)	–
Total cost (pesos)	–
Coverage	–
Associated service provider name	xxx
Insurance	
Monthly cost (pesos)	–
Total cost (pesos)	–
Coverage	xxx
Associated service provider name	

III. Prepayment Conditions

Prepaid charge (%)	–
Notice period for prepayments	

IV. Late Fees

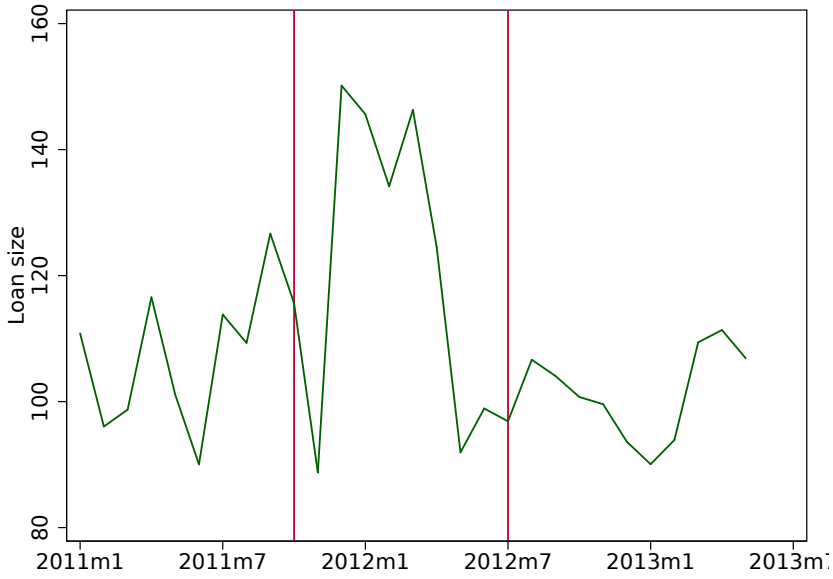
Interest on arrears (%)	–
Collection expenses (%)	–

Advisory

"The consumer credit of this summary sheet requires the contracting consumer <name> equity or future income sufficient to pay the total cost of $xx whose monthly payment is $xx, during the entire credit period."

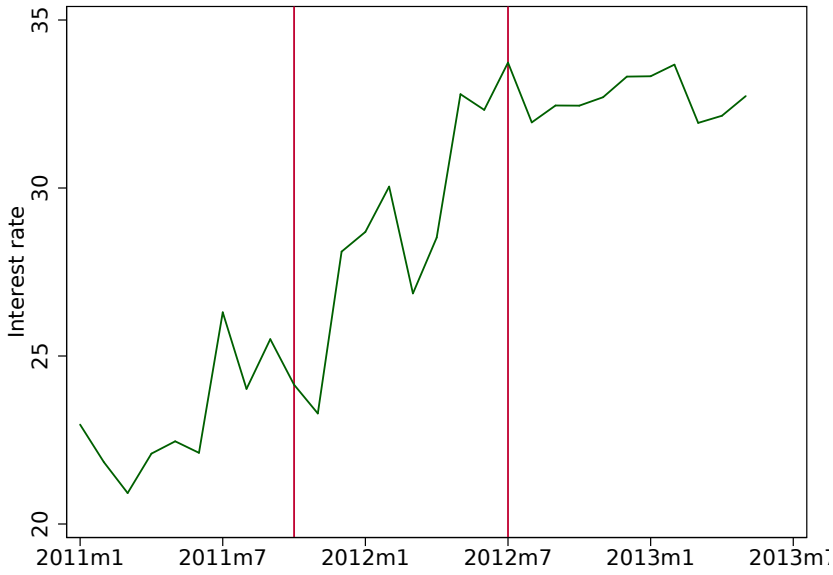
Notes: This an English translation of the guidance included in law 20.555 that applied to all loan contracts. The disclosure requirements are similar to those of Universal Credits outlined in law 20.448 (see figure 3).

Figure 5: Average Loan Size (UF)



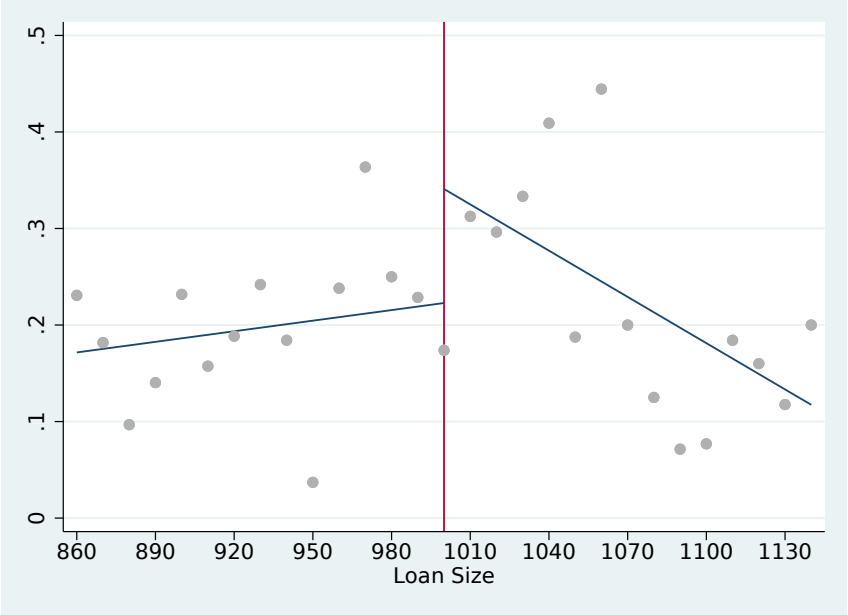
Notes: This figure graphs the unweighted average of loan sizes in UF of newly issued loans by issuance date. The first red line is the implementation date of law 20.448 (the introduction of Universal Credit Contracts) and the second red line is the implementation date of law 20.555 (disclosure requirements for all loans).

Figure 6: Average Nominal Interest Rate



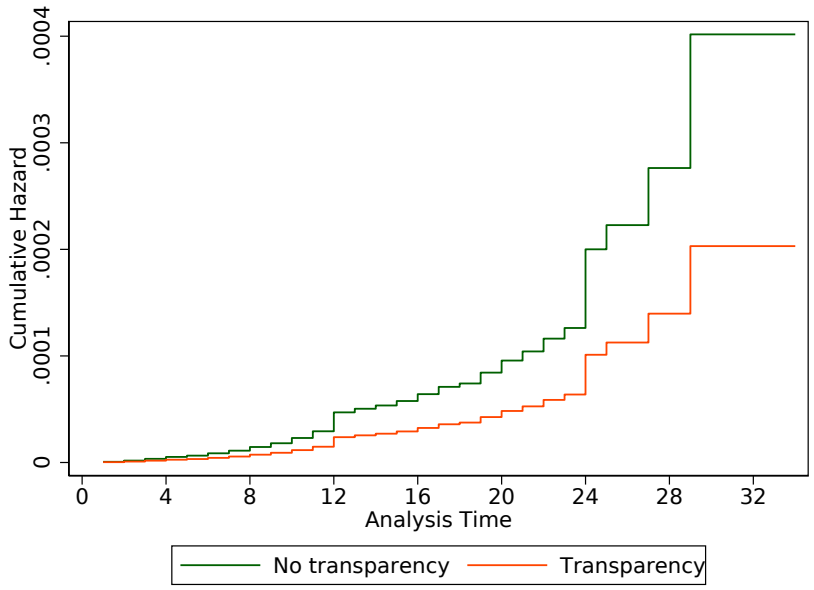
Notes: This figure graphs the unweighted average of nominal interest rates of newly issued loans by issuance date. This rate includes all fees and insurance charges associated with the loan and is equivalent to APR. The first red line is the implementation date of law 20.448 (the introduction of Universal Credit Contracts) and the second red line is the implementation date of law 20.555 (disclosure requirements for all loans).

Figure 7: Raw Regression Discontinuity - Ever Delinquent



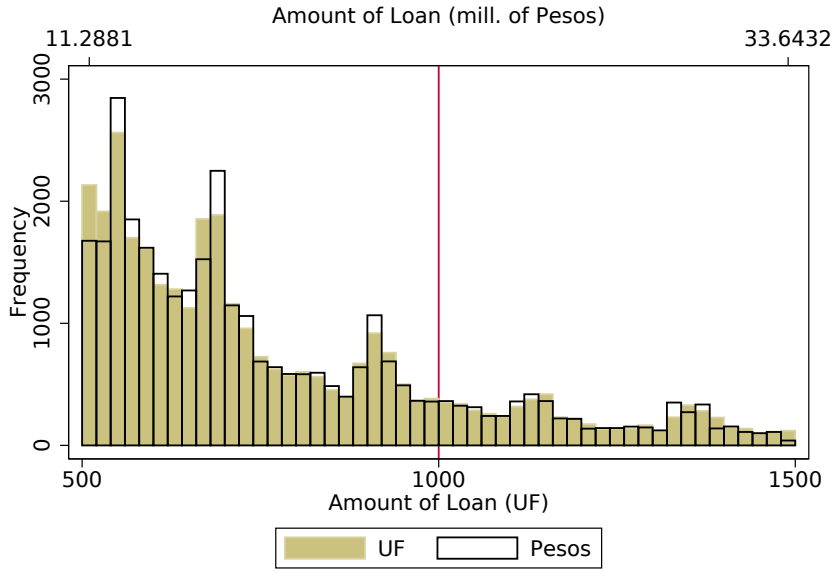
Notes: This figure graphs the linear fit of the raw regression discontinuity of the dependent variable of the borrower ever being delinquent (missing a payment) in equation (1) with no controls. The red line marks the loan cutoff of 1,000 UF. Confidence intervals are shown at the 95% significance level The pre-period version of this graph is shown in figure A.8.

Figure 8: Cox Proportional Hazard Rate Model



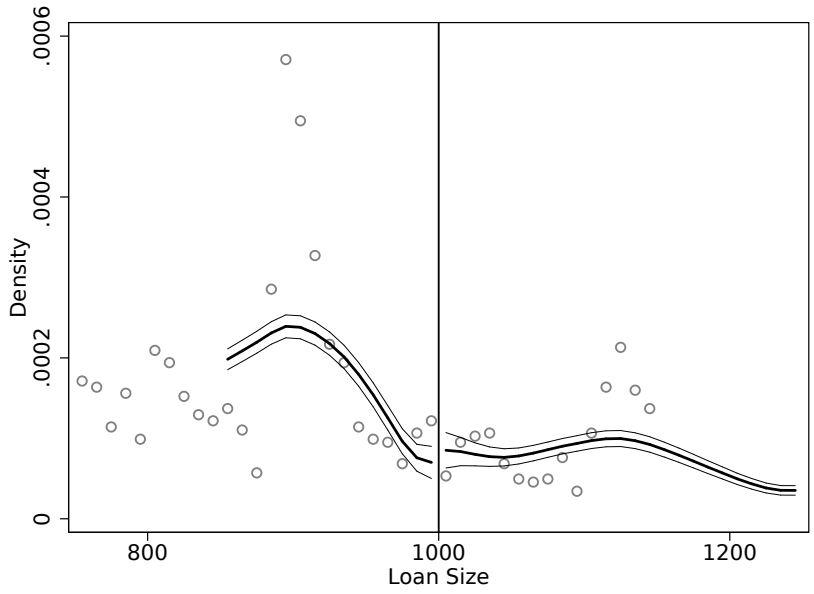
Notes: This figure plots the cumulative probability of being delinquent for borrowers around the regression discontinuity cutoff. All covariates included in the regression discontinuity regression are included at set at the mean of the regression discontinuity sample, except for the loan size which is set at the cutoff amount. Fixed effects for lender and comuna are also included.

Figure 9: Histogram of Loan Amounts in Pesos and UF



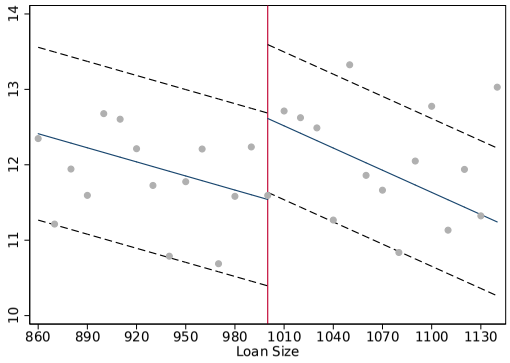
Notes: This figure graphs distribution of loan amounts in pesos and UF around the 1,000 UF cutoff between November 2011 and July 2012. The coloured bars are the loan amounts in UF (bottom x-axis) while the clear bars are the corresponding peso amounts (top x-axis). The red line corresponds to the 1,000 UF cutoff. Below the cutoff, customers were presented with the Universal Credit contracts with standardized features and improved disclosure outlined in law 20.448.

Figure 10: McCrary Density Plot (Law 20.448)

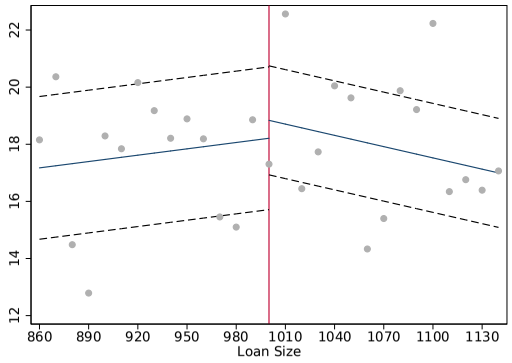


Notes: This figure shows the McCrary density test for loan amounts in pesos and UF around the 1,000 UF cutoff between November 2011 and July 2012. The vertical black line is for the 1,000 UF cutoff amount in law 20.448. Confidence intervals are shown at the 95% significance level.

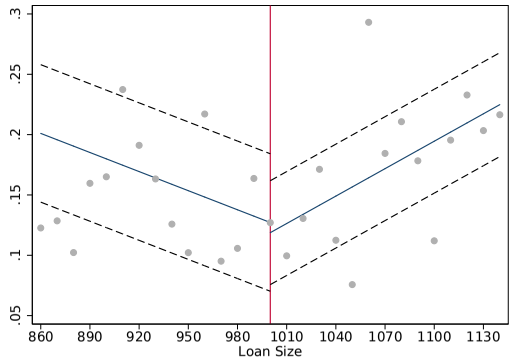
Figure 11: Covariate Balancing Tests



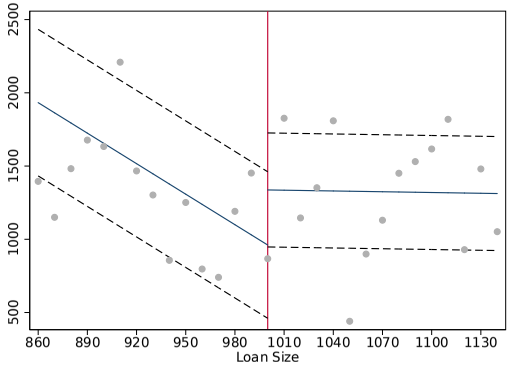
(a) Interest Rate



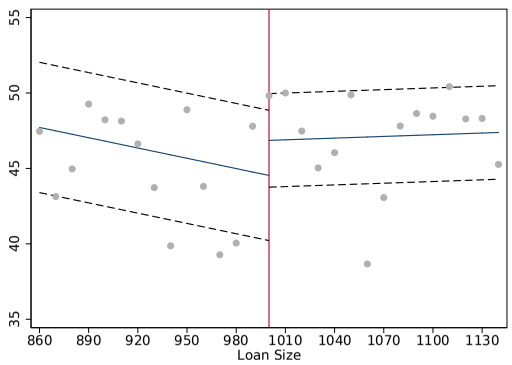
(b) Maturity at Issuance



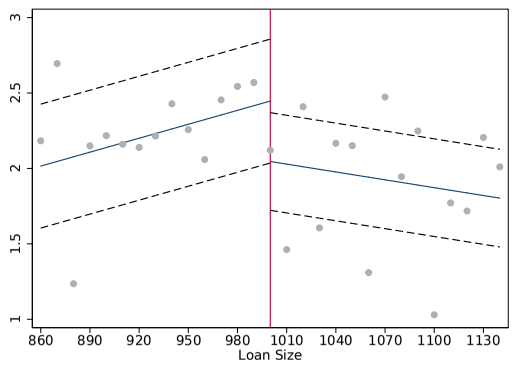
(c) Credit Risk



(d) Annual Income (UF)



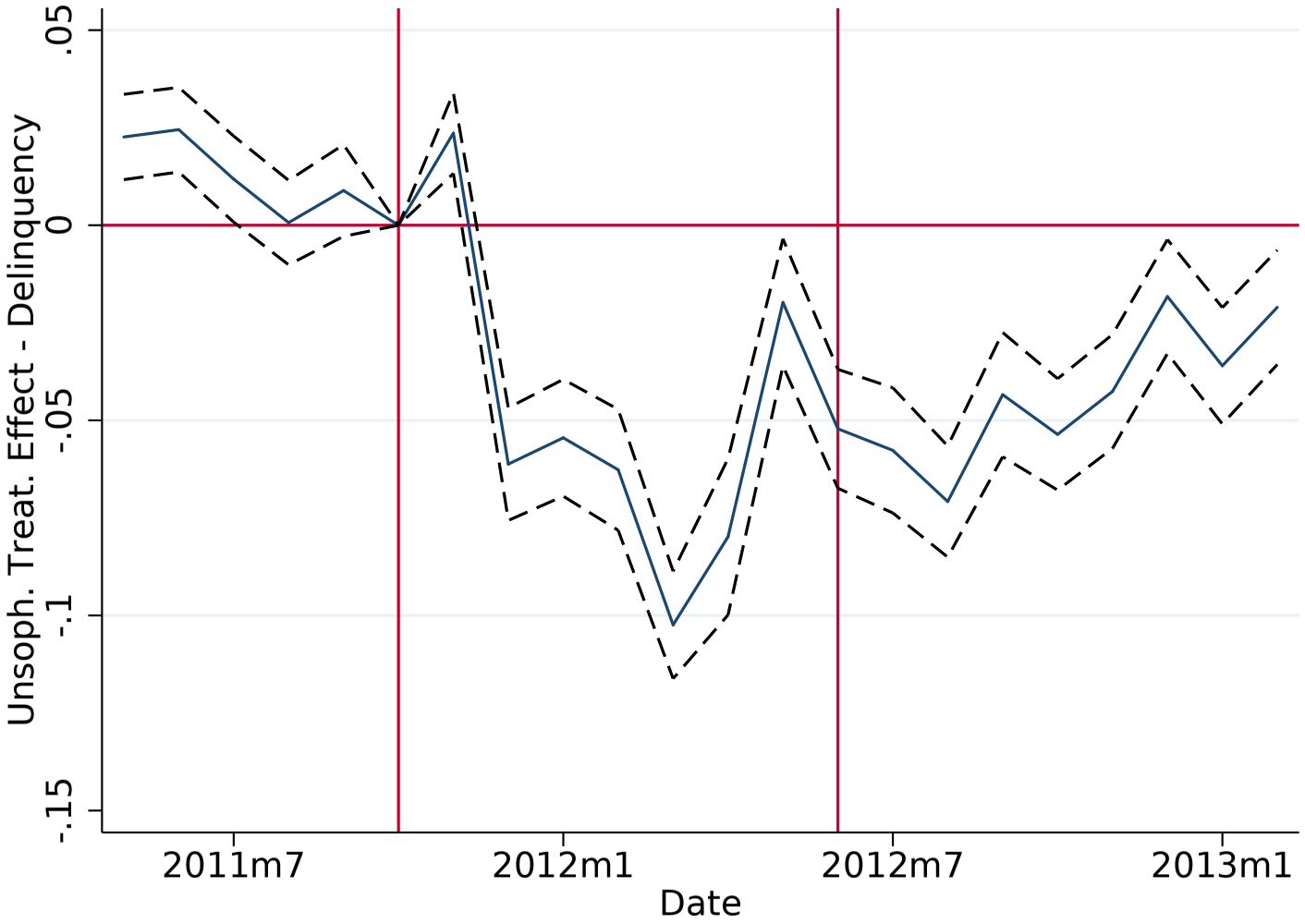
(e) Age



(f) Expected Inflation

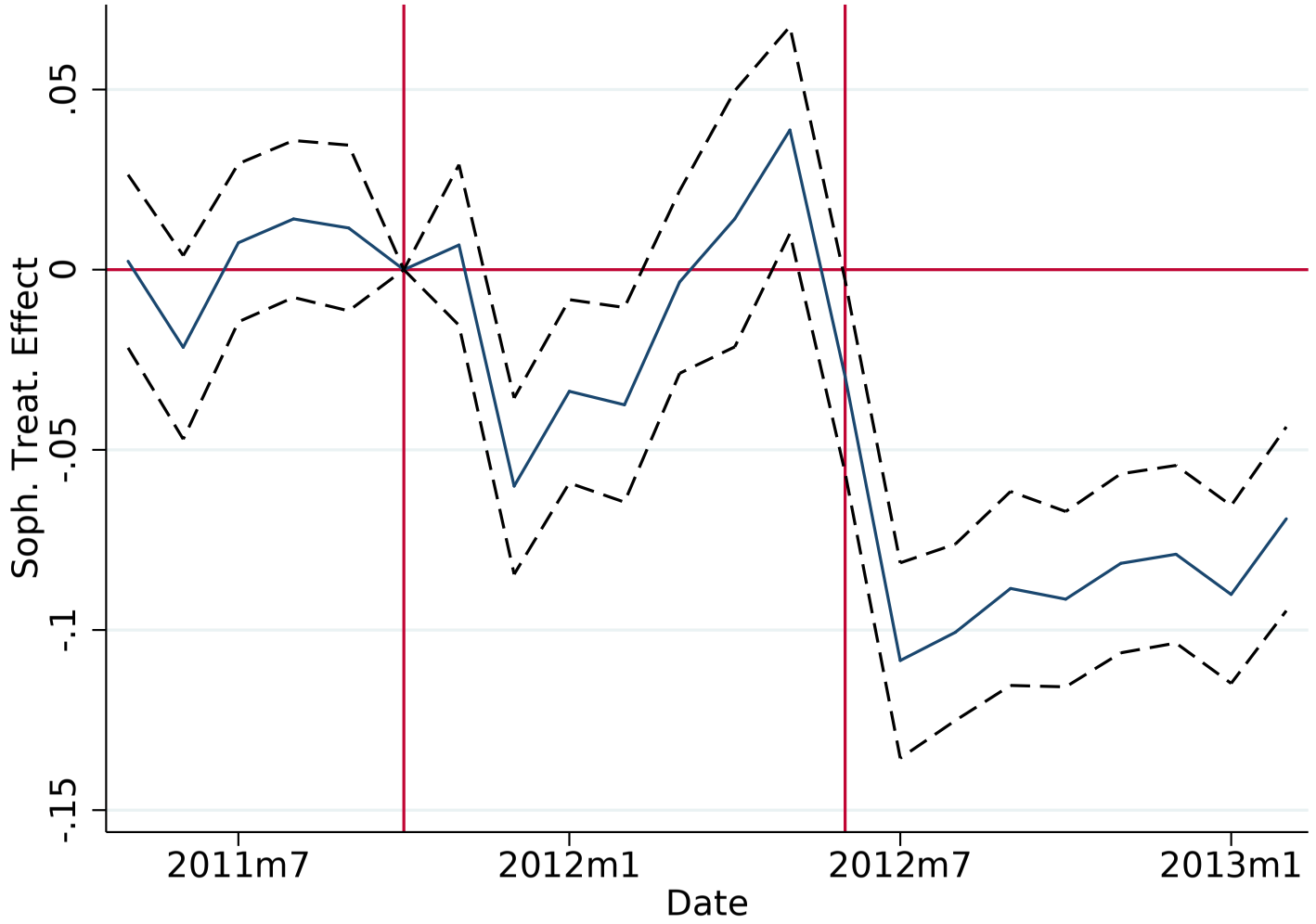
Notes: These figures graph the regression discontinuities in equation (1) for the control variables used in our specification presented in table 7. Observations are between November 2011 and July 2012. Corresponding regression tables for these figures can be found in table 12. The red lines show the 1,000 UF cutoff amount in law 20.448. Confidence intervals are shown at the 95% significance level. Interest rate (panel 11a) is defined as the total interest rate plus associated fees and insurances of the loan (equivalent to APR). Maturity (panel 11b) is defined as the term of the loan in months. Credit Risk (panel 11c) is defined as the fraction of the loan the bank sets as provisions in case of loss aside for across all loans at all Chilean banks (higher numbers correspond to higher credit risk). Income (panel 11d) is defined as the annual amount in UF that a borrower earns. Age is defined as the age in years of the borrower. Expected inflation (panel 11f) is defined as $(\frac{1+CLP}{1+UF} - 1) * 100$, where the Chilean peso rate is the rate at which Chilean banks borrow pesos between each other for the period of 2 years, and UF is the rate at which Chilean banks borrower from each other in UFs in the same horizon. As this is a swap rate between UF and pesos over a two year horizon, it reflects the expected inflation between pesos and UF as perceived by banks over a two year time horizon.

Figure 12: Delinquency: Unsophisticated borrowers versus control



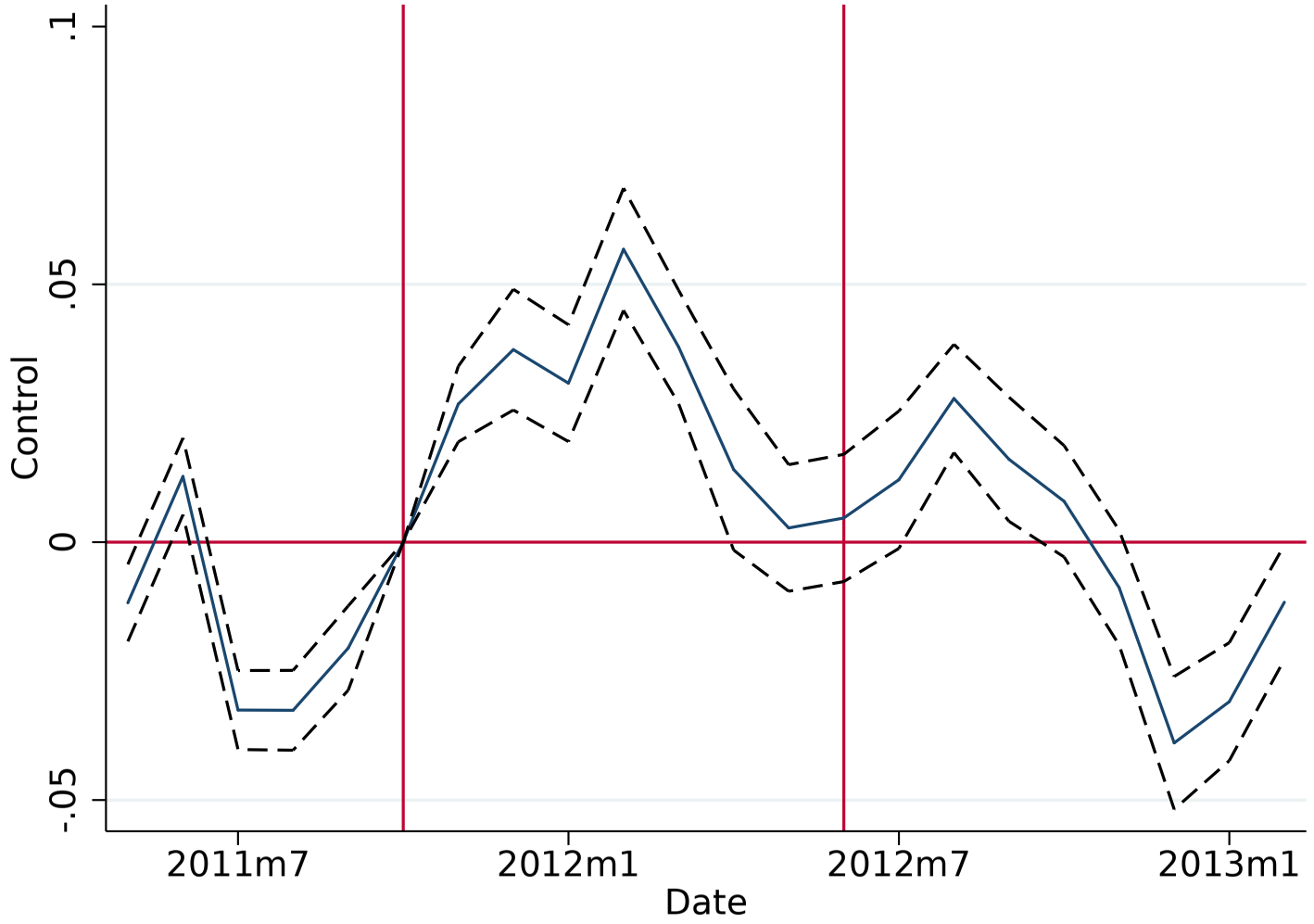
Notes: Estimates of βs from equation(2) for borrowers in neighbourhoods with the average education below 11.5 years of schooling ("unsophisticated") as compared to the control group (11.5 to 12 years of schooling). Loans are collapsed to one data point per observation, and all loans are 2 years maturity or less and under 1,000 UF in loan amount. The first vertical red line marks the implementation of law 20.448 (introduction of Universal Credit contracts with standardized features and improved disclosure) and the second vertical red line marks the implementation of law 20.555 which introduced improved disclosure to all loan contracts.

Figure 13: Delinquency: Sophisticated borrowers versus control



Notes: Estimates of βs from equation(2) for borrowers in neighbourhoods with the average education at or above 12 years of schooling ("sophisticated") as compared to the control group (11.5 to 12 years of schooling). Loans are collapsed to one data point per observation, and all loans are 2 years maturity or less and under 1,000 UF in loan amount. The first vertical red line marks the implementation of law 20.448 (introduction of Universal Credit contracts with standardized features and improved disclosure) and the second vertical red line marks the implementation of law 20.555 which introduced improved disclosure to all loan contracts.

Figure 14: Delinquency: Control Group Time Trends



Notes: Estimates of αs from equation(2) for borrowers in neighbourhoods with the average education between 11.5 and less than 12 years of schooling, or the control group. These coefficients are equivalent to time trends in delinquency in the absence of treatment. Loans are collapsed to one data point per observation, and all loans are 2 years maturity or less and under 1,000 UF in loan amount. The first vertical red line marks the implementation of law 20.448 (introduction of Universal Credit contracts with standardized features and improved disclosure) and the second vertical red line marks the implementation of law 20.555 which introduced improved disclosure to all loan contracts.

Table 1: Chilean Household Debt Breakdown

Debt Type	Total	Consumption	Mortgage	Automotive	Educational	Other
Chile (2014)						
% of households	72.6	63.4	18.9	3.0	8.2	7.2
Average $ USD		1,000	30,000	4,000	3,500	300
U.S. (2017)						
% of households	77.1	56.9	47.5	33.8	22.4	5.4
Average $ USD	123,400	8,570	158,040	17,200	34,200	26,800

Source: Banco Central de Chile 2015, Bricker et al. 2017.
Notes: This table shows the breakdown by type of debt by households in both the U.S. and Chile. Rows show the percentage of households with different types of debt, and the average balances of households with this debt. Consumption credit in the United States is defined as the combination of credit card, unsecured lines of credit, and other installment credit. Chilean numbers are from the Central Bank of Chile as of 2014 and the U.S. numbers are as of 2014 from the Federal Reserve's Survey of Consumer Finances.

Table 2: Chilean Consumer Credit Breakdown

Type	Total	Credit Cards	Lines of Credit	Consumer Credit	Total	Credit Cards	Loans & Advances	CyC Loans
Lender	Banks				Department Stores			CyC
% of households	30.2	19.3	7.8	15.4	48.4	46.6	7.0	11.4
Average $ USD	1,800	900	500	3,400	400	350	500	700

Source: Banco Central de Chile 2015
Notes: This table shows the breakdown of consumer credit in Chile as of 2014. There are three main sources of consumer credit in Chile: banks, department stores, and CyCs (cajas de compensacion y cooperativas), which are small non-profit funds and cooperative credit organizations that generally provide credit services to a community similar to a credit union. Numbers are from the Central Bank of Chile's household finance survey as of 2014

Table 3: Summary Statistics - Full Sample

	p25	mean	p50	p75	sd
Ever Delinquent	0.00	0.25	0.00	0.00	0.43
Ever Defaulted	0.00	0.01	0.00	0.00	0.08
Ever Extended	0.00	0.01	0.00	0.00	0.10
Rate	13.29	25.24	20.84	36.39	14.15
Maturity at Issue	12.00	24.69	25.00	37.00	17.25
Loan Size (UF)	18.23	110.32	50.08	134.72	165.15
Female	0.00	0.43	0.00	1.00	0.50
Age	33.00	44.45	43.00	54.00	13.57
Credit Risk	0.02	0.12	0.08	0.13	0.16
Income (UF)	10.54	554.52	81.45	336.36	22,0750.29
Married	0.00	0.64	1.00	1.00	0.48
Total Number of Loans	2.00	5.67	4.00	7.00	6.89
Number of Outstanding Loans	1.00	3.57	2.00	4.00	4.49
Outstanding Debt (UF)	24.88	137.48	64.96	163.96	204.29
Future Debt (UF)	0.00	210.71	36.48	207.26	481.43
Mean Neigh. Years of Sch. (age 30-50)	10.80	11.29	11.50	11.80	0.88
Observations					5,098,890

Notes: To construct our sample, we start with an initial sample size of 7,655,263 unique consumer loans across the sample period. We drop all loans that do not go to Chilean citizens or that have missing observations for any of our control variables. This leaves us with a final sample of 5,097,802 unique loan observations. We then collapse the full history of the loan to one observation. Ever delinquent is defined as missing one or more payments over the life of the loan. Ever defaulted is missing three or more payments and having judicial proceedings enacted against the borrower. Ever extended is defined as the maturity of the loan being extended after the loan has been issued. The rate is the interest rate inclusive of all fees and insurance. Loan size is presented in UF. Credit risk is denoted as the percentage of provisions all banks have allocated against losses for an individual's loans (higher scores denote riskier borrowers) and is between zero and one. Income is defined as a borrower's annual income in UF. Outstanding debt is constructed by taking all loan terms and determining what the monthly payment would be and then determining the outstanding balances the borrower owes across all banks. If the borrower has missed any payments, we simply add those payments to the balance but do not add any additional amounts for fees. Future debt is the amount of debt the borrower subsequently takes out after the issuance of each loan observation. Neighbourhood years of schooling was obtained from the Chilean census data for the year 2016.

Table 4: Summary Statistics - Discontinuity Sample

	p25	mean	p50	p75	sd
Ever Delinquent	0.00	0.20	0.00	0.00	0.40
Ever Defaulted	0.00	0.00	0.00	0.00	0.07
Ever Extended	0.00	0.02	0.00	0.00	0.14
Rate	10.30	12.00	11.61	13.56	3.34
Maturity at Issue	12.00	17.48	16.00	25.00	7.82
Loan Size (UF)	900.63	968.77	930.91	1031.20	83.87
Female	0.00	0.21	0.00	0.00	0.41
Age	39.00	46.89	47.00	56.00	12.84
Credit Score	0.03	0.17	0.09	0.21	0.21
Income (UF)	11.13	1458.51	1040.42	1983.77	2,344.99
Married	1.00	0.76	1.00	1.00	0.43
Total Number of Loans	3.00	5.46	4.00	7.00	4.33
Number of Outstanding Loans	2.00	3.97	3.00	5.00	3.15
Outstanding Debt (UF)	906.17	1089.02	981.55	1118.05	341.32
Future Debt (UF)	0.00	908.24	403.77	1328.70	1364.68
Mean Neigh. Years of Sch. (age 30-50)	10.60	11.56	11.40	13.00	1.25
Observations					1,088

Notes: Using the bandwidth selection procedure outlined in Calonico et al. 2014 and Calonico et al. 2018, we then restrict our sample to loans 138.5 UF (roughly $5,000 USD) above and below the regulatory cutoff of 1,000 UF between November 2011 and July 31, 2012. We also exclude any loans at or above three years in maturity. Definitions for variables are presented in table 3.

Table 5: Sample Comparison

	Full sample mean/(sd)	RD sample mean/(sd)	Difference [p-value]
Ever Delinquent	0.25	0.20	0.05
	(0.43)	(0.40)	[0.00]
Ever Defaulted	0.01	0.00	0.00
	(0.08)	(0.07)	[0.46]
Ever Extended	0.01	0.02	-0.01
	(0.10)	(0.14)	[0.00]
Rate	25.24	12.00	13.25
	(14.15)	(3.34)	[0.00]
Maturity at Issue	24.69	17.48	7.20
	(17.25)	(7.82)	[0.00]
Loan Size (UF)	110.14	968.77	-858.63
	(164.68)	(83.87)	[0.00]
Credit Score	0.12	0.21	-0.05
	(0.16)	(0.17)	[0.00]
Income (UF)	554.33	1,458.51	-904.17
	(220,773.85)	(2,344.99)	[0.89]
Total Number of Loans	5.67	5.46	0.21
	(6.89)	(4.33)	[0.32]
Mean Neighbourhood Years of Sch. (age 30-50)	11.28	11.56	-0.28
	(0.88)	(1.25)	[0.00]

Notes: This table compares our relevant control and other variables of the full sample and our regression discontinuity sample chosen by the bandwidth procedure outlined in Calonico et al. 2014 and Calonico et al. 2018. Definitions for variables are presented in table 3.

Table 6: Summary Statistics - Bank Switching

	mean	sd
Full Sample		
Switched Banks	0.48	0.50
Switched to New Bank	0.36	0.48
Observations	2,286,020	
Discontinuity Sample		
Switched Banks	0.52	0.50
Switched to New Bank	0.35	0.48
Observations	532	

Notes: From our full sample, we restrict our sample further to loans where we can identify the borrower and where the borrower takes out more than one loan. We end up with 2,286,020 observations over the full sample and 532 observations within our discontinuity sample.

Table 7: Regression Discontinuity: Borrower Outcomes

	(1) Ever Delinquent	(2) Ever Defaulted	(3) Ever Extended
Transparency	-0.144**	-0.0161**	0.00413
	(0.0711)	(0.00809)	(0.0311)
Loan Size	-0.148**	-0.00604	-0.000818
	(0.0623)	(0.00796)	(0.0328)
Transparency X Loan Size	0.163*	-0.00175	0.0189
	(0.0861)	(0.00943)	(0.0389)
Comuna Fixed Effects	Y	Y	Y
Lender Fixed Effects	Y	Y	Y
Controls	Y	Y	Y
Bandwidth	138	153	131
Kernel	Tri	Tri	Tri
Mean	.341	.017	.034
N	1088	1183	1033

Robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Table 7 shows the estimates of equation 1 for law 20.448's impact on borrowers taking out loans from the period of November 2011 to July 2012 with a maturity of less than three years and loans within our bandwidth selected by procedures outlined in Calonico et al. 2014 and Calonico et al. 2018. All estimates are based on regressions that include fixed effects for comunas (neighbourhoods), and lender, as well as controls for the credit risk, income, age, sex, and marital status of the borrower. Expected inflation (future UF to peso inflation rate) and inter-bank rate are included as controls for aggregate economic conditions. Loan amount is centered around the cutoff amount of 1,000 UF. We use the bandwidth selection procedure outlined in Calonico et al. 2014 and Calonico et al. 2018.

Table 8: Raw Regression Discontinuity

	(1) Ever Delinquent	(2) Ever Defaulted	(3) Ever Extended
Transparency	-0.118*	-0.0194	-0.0118
	(0.0706)	(0.0141)	(0.0275)
Loan Size	-0.160**	-0.0107	-0.00983
	(0.0662)	(0.0141)	(0.0307)
Transparency X Loan Size	0.196**	0.00587	0.0184
	(0.0841)	(0.0145)	(0.0360)
Comuna Fixed Effects	N	N	N
Lender Fixed Effects	N	N	N
Bandwidth	138	153	131
Kernel	Tri	Tri	Tri
Mean	.341	.017	.034
N	1088	1183	1033

Robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Table 8 shows the estimates of equation 1 for law 20.448's impact on borrowers taking out loans from the period of November 2011 to July 2012 with a maturity of less than three years and loans within our bandwidth selected by procedures outlined in Calonico et al. 2014 and Calonico et al. 2018. The dependent variables are if a borrower is ever delinquent (1), defaults (2), or has their loan maturity extended (3). Ever delinquent is defined as missing a loan payment in less than 90 days), ever defaulted is defined as missing loan payments for over 90 days and judicial proceedings having been initiated against the borrower by the bank. Ever extended is defined as the borrower having their loan maturity extended after the loan is taken out. No controls are included.

Table 9: Regression Discontinuity, Post-period

	(1) Ever Delinquent	(2) Ever Defaulted	(3) Ever Extended
Transparency	-0.0272	-0.00364	0.00143
	(0.0201)	(0.00356)	(0.0102)
Loan Size	0.0256	0.00141	0.0122
	(0.0234)	(0.00520)	(0.0115)
Transparency X Loan Size	-0.0593*	-0.00573	-0.0222
	(0.0309)	(0.00606)	(0.0141)
Comuna Fixed Effects	Y	Y	Y
Lender Fixed Effects	Y	Y	Y
Bandwidth	138	153	131
Kernel	Tri	Tri	Tri
Mean	.081	.002	.015
N	4241	4680	4007

Robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Table 9 shows the estimates of equation 1 for law 20.555's impact on borrowers taking out loans from the period of August 2012 to December 2014 with a maturity of less than three years and loans within our bandwidth selected by procedures outlined in Calonico et al. 2014 and Calonico et al. 2018. Transparency then gives us the sole effect of standardization on loan outcomes. All estimates are based on regressions that include fixed effects for comunas (neighbourhoods), and lender, as well as controls for the credit risk, income, age, sex, and marital status of the borrower. Expected inflation (future UF to peso inflation rate) and inter-bank rate are included as controls for aggregate economic conditions.

Table 10: Regression Discontinuity - Other Loan Outcomes

	(1) Month Default	(2) # Miss. Pmnts	(3) $ Miss. Pmnts	(4) Future debt
Transparency	0.419	-0.413**	-31.70**	284.0
	(4.584)	(0.196)	(15.61)	(212.1)
Loan Size	2.907	-0.335**	-25.77	356.2
	(9.208)	(0.153)	(17.70)	(245.2)
Trans. X Loan Size	-1.162	0.294	24.73	-289.6
	(10.17)	(0.191)	(20.06)	(316.3)
Comuna FE	Y	Y	Y	Y
Lender FE	Y	Y	Y	Y
Bandwidth	87	187	132	127
Kernel	Tri	Tri	Tri	Tri
Mean	7.141	.795	55.365	652.741
N	110	1369	1038	1005

Robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Table 10 shows the estimates of equation 1 for law 20.448's impact on borrowers taking out loans from the period of August 2012 to December 2014 with a maturity of less than three years and loans within our bandwidth selected by procedures outlined in Calonico et al. 2014 and Calonico et al. 2018. Dependent variables are the number of loans from issuance before the loan defaults, the number of missed payments, and the amount of future debt the borrower subsequently takes out. All estimates are based on regressions that include fixed effects for comunas (neighbourhoods), and lender, as well as controls for the credit risk, income, age, sex, and marital status of the borrower. Expected inflation (future UF to peso inflation rate) and inter-bank rate are included as controls for aggregate economic conditions.

Table 11: Cox Proportional Hazard Rate Model

	(1) Delinquency	(2) Delinquency
Transparency	-0.480**	-0.682***
	(0.241)	(0.265)
Maturity	-0.123***	-0.146***
	(0.00767)	(0.0101)
Loan Size	-0.00203	-0.00346**
	(0.00137)	(0.00150)
Female	0.186	0.187
	(0.116)	(0.123)
Age	-0.0153***	-0.0136**
	(0.00509)	(0.00564)
Credit Risk	0.182	0.0647
	(0.218)	(0.232)
Monthly Income	-0.0000643**	-0.0000761***
	(0.0000262)	(0.0000243)
Married	-0.137	0.00412
	(0.134)	(0.152)
Loan Interest Rate	0.0560***	0.0577***
	(0.0136)	(0.0174)
Inflation	0.0167	0.0517
	(0.0419)	(0.0453)
Bank Funding Rate	0.310***	0.159
	(0.116)	(0.128)
Comuna Fixed Effects	N	Y
Lender Fixed Effects	N	Y
N	13266	13266

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Table 11 shows regression results for a Cox Proportional Hazard Rate model. The Transparency coefficient represents law 20.448's impact on borrowers' cumulative probability of delinquency. The loans are the same as the regression discontinuity analysis but are now represented as a monthly panel of loan statuses. Control variables include fixed effects for comunas (neighbourhoods), and lender, as well as controls for the credit risk, income, age, sex, and marital status of the borrower. Expected inflation (future UF to peso inflation rate) and interbank rate are included as controls for aggregate economic conditions.

Table 12: Covariate Balancing Tests

	(1) Interest Rate	(2) Maturity	(3) Credit Risk	(4) Income	(5) Age	(6) Expected Inflation	(7) Inter-bank Rate	(8) UF/peso exchange rate
Transparency	-0.759	-1.292	0.000430	-326.2	-3.096	0.368*	-0.0718	-15.81
	(0.508)	(1.228)	(0.0311)	(241.5)	(2.143)	(0.217)	(0.0811)	(28.10)
Loan Size	-0.367	-1.586	0.0769**	1.744	0.661	-0.195	0.0675	34.49
	(0.464)	(1.195)	(0.0310)	(232.7)	(1.789)	(0.206)	(0.0748)	(28.02)
Trans. X L. Size	-0.264	2.289	-0.141***	-623.8*	-4.004	0.469*	-0.174*	-81.26**
	(0.618)	(1.526)	(0.0400)	(342.1)	(2.513)	(0.262)	(0.0924)	(35.95)
Comuna FE	Y	Y	Y	Y	Y	Y	Y	Y
Lender FE	Y	Y	Y	Y	Y	Y	Y	Y
Bandwidth	138	138	138	138	138	138	138	138
Kernel	Tri	Tri	Tri	Tri	Tri	Tri	Tri	Tri
Mean	12.614	18.837	.119	1,336.922	46.859	2.046	5.793	22,396.383
N	1,088	1,088	1,088	1,088	1,088	1,088	1,088	1,088

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Table 12 shows the estimates of equation 1 for law 20.555's impact on borrowers taking out loans from the period of August 2012 to December 2014 with a maturity of less than three years and loans within our bandwidth selected by procedures outlined in Calonico et al. 2014 and Calonico et al. 2018. The dependent variables are our pre-selected controls: interest rate including all fees and insurances, maturity at the issuance date of the loan in months, credit risk (percentage of total loans provisioned for across all banks for the individual), annual income in UF, age, expected inflation, and inter-bank borrowing rate.

Table 13: CIA Test

	Law 20.448 Implementation		Law 20.555 Implementation	
	D=0	D=1	D=0	D=1
	(1)	(2)	(3)	(4)
Loan Size (000s)	-0.000601	-0.0000757	0.0000189	-0.0000479
	(0.000472)	(0.000297)	(0.000124)	(0.0000864)
Interest Rate	-0.0109	0.00487	0.0229***	0.0192***
	(0.00951)	(0.00459)	(0.00385)	(0.00246)
Maturity at Issue	-0.00134	0.000816	0.0000535	-0.000978
	(0.00307)	(0.00184)	(0.000882)	(0.000604)
Female	-0.0487	0.0759*	-0.00848	0.00859
	(0.0579)	(0.0404)	(0.0189)	(0.0125)
Age	-0.00350	-0.00350**	-0.00139**	-0.00134***
	(0.00243)	(0.00136)	(0.000687)	(0.000481)
Credit Score	-0.189*	-0.121	-0.0277	-0.0326
	(0.105)	(0.0756)	(0.0354)	(0.0236)
Income (UF)	0.00000194	-0.00000339	-0.00000483	-1.10e-09
	(0.00000581)	(0.00000454)	(0.00000326)	(0.000000301)
Married	-0.0567	-0.0996**	0.00874	-0.0191
	(0.0646)	(0.0419)	(0.0210)	(0.0144)
Expected Inflation	0.00221	0.0199*	0.00271	-0.00272
	(0.0197)	(0.0112)	(0.00603)	(0.00414)
Interbank Rate	-0.0159	0.0375	0.0294***	0.0104*
	(0.0513)	(0.0290)	(0.00983)	(0.00616)
Comuna Fixed Effects	Y	Y	Y	Y
Lender Fixed Effects	Y	Y	Y	Y
N	447	996	2236	4195

Notes: Table 13 replicates table 2 from Angrist and Rokkanen 2015. These regressions test that the running variable is uncorrelated with the relevant outcome variable (ever delinquent) both 100 UF above and below the cutoff point of the running variable. Robust standard errors are reported in the parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 14: CIA-based Estimates

	Law 20.448 Implementation		Law 20.555 Implementation	
	(1)	(2)	(3)	(4)
$\beta_{CIA} - \beta_{RD}$	-0.00235	-0.0217	-0.0208*	-0.0156*
	(0.0369)	(0.0271)	(0.0112)	(0.00885)
Weighting Method	Linear	Propensity score	Linear	Propensity Score
N Untreated	447	429	2236	2211
N Treated	996	884	4195	4077
t-statistic	1.273	0.950	-1.719	-1.622

Notes: Table 14 replicates table 3 from Angrist and Rokkanen 2015. Bootstrapped standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 15: Correlation: Individual Census Years of Schooling versus Comuna Averages

	(1)
	Years of Schooling (2002 Census)
Comuna Average Years of Schooling Completed	0.937***
	(0.000504)
Observations	583,954

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Table 15 shows the correlation between individually-measured years of schooling for individuals residing in a comuna between 30 and 59 years of age (dependent variable) and aggregate schooling by comuna in 2016. The individual data comes from the 2002 Chilean Census obtained through IPUMS.

Table 16: Number of Observations by Education Category

Sophistication	Frequency	Delinquency Rate
≥12 years school	43,495	18.8%
>11.5 to <12 years school	338,876	26.6%
≤11.5 years school	356,946	25.3%
Total	739,317	

Notes: Summary statistics for difference-in-differences analysis. Loans are collapsed to observation per loan, and all loans are 2 years maturity or less and under 1,000 UF in loan amount. Education is determined by average education completed by all residents in the comuna. Information on comunas was collected from the Chilean Census.

Table 17: Money on the Table: Summary Statistics

	mean	sd
Pre-period		
Rate-25th pctile rate	3.5	8.4
Rate-minimum rate	12.3	12.0
Rate standard deviation	7.8	4.1
Law 20.448 Implementation Period		
Rate-25th pctile rate	6.2	10.2
Rate-minimum rate	16.8	14.2
Rate standard deviation	8.6	3.8
Law 20.555 Implementation Period		
Rate-25th pctile rate	8.2	10.3
Rate-minimum rate	20.2	13.7
Rate standard deviation	9.0	3.4
Observations	3,637,586	

Notes: Cells of similar borrowers and products were created (see section 8 for details). Dispersion is measured by the difference in interest rate from the 25th percentile rate in the borrower $\times$ product bin, the difference in the minimum rate and the standard deviation of rates.

Table 18: Money on the Table: Regression Results

	(1)	(2)	(3)
	Rate-25th pctile rate	Rate-minimum rate	Rate standard deviation
Standardization	0.852***	0.764***	0.442***
	(0.0247)	(0.0337)	(0.00880)
Disclosure	3.140***	4.133***	0.620***
	(0.0320)	(0.0418)	(0.0109)
Sophisticated	-0.495***	-5.690***	-1.282***
	(0.0169)	(0.0230)	(0.00700)
Sophisticated x Std.	-1.495***	-2.025***	-0.412***
	(0.0394)	(0.0527)	(0.0149)
Sophisticated x Disc.	-2.478***	-3.816***	-1.031***
	(0.0290)	(0.0383)	(0.0100)
Controls	Y	Y	Y
Year Fixed Effects	Y	Y	Y
N	3637586	3637586	3561743

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents our results related to measures of interest rate dispersion. Dispersion is measured by the difference in interest rate from the the lowest available rate (25th percentile rate, the minimum rate) and the standard deviation of rates for similar borrowers. Cells of similar borrowers and products were created using the criteria outlined in section 8. Controls include loan maturity, credit risk, income, sex, if married, whether province, age, inter-bank rate, and expected inflation.

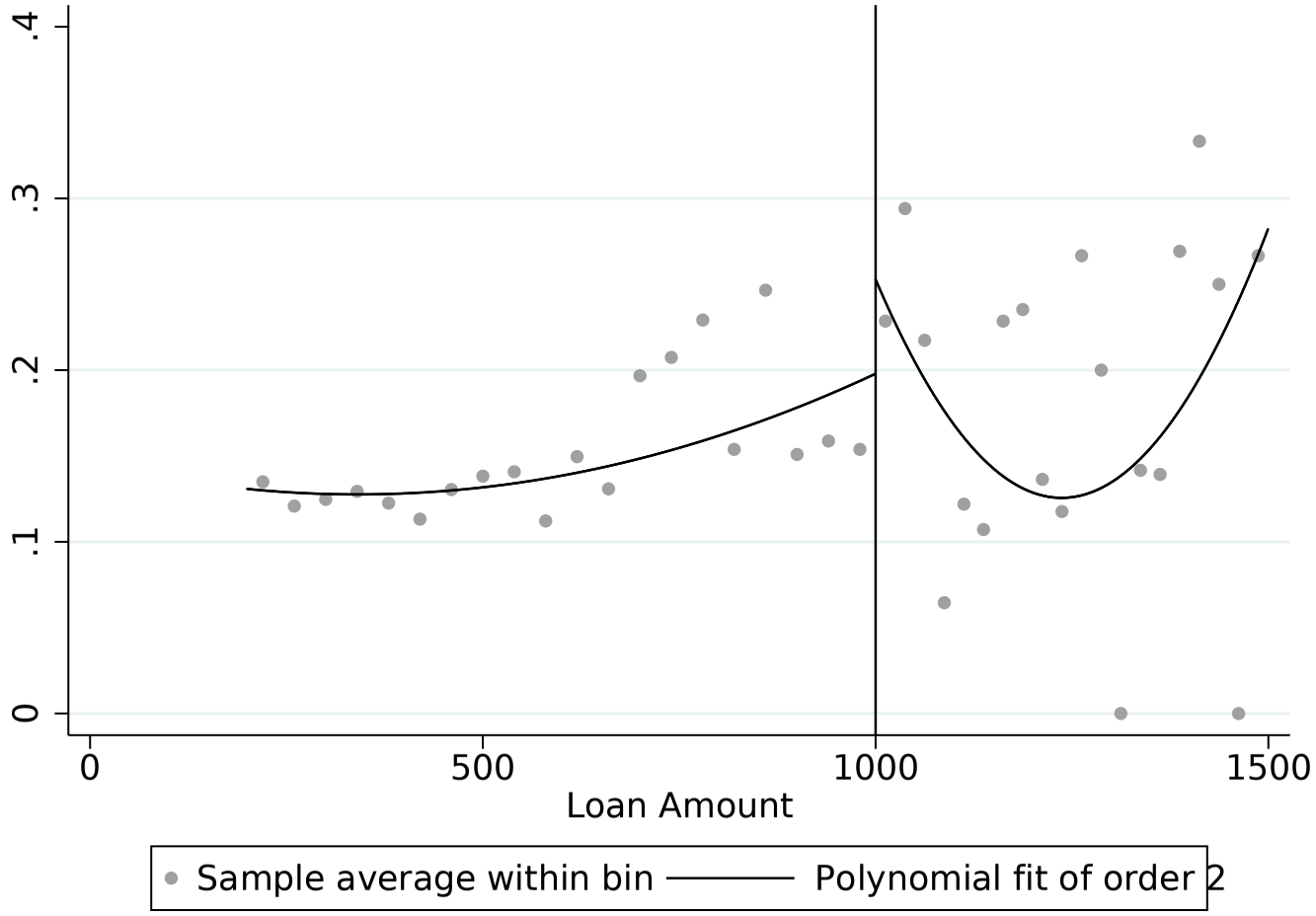
Appendix A Robustness Checks

Figures A.2-A.3 shows the global polynomial for delinquency, default, and loan extensions. Table A.1 adds controls for outstanding debt, number of outstanding loans, and leverage (debt to income ratio) and shows the magnitude of our coefficient increases from 14.4 percentage points to 16.9 percentage points with the addition of these controls. In table A.2 we show there is a significant negative effect on delinquency in the pre-period, this effect is roughly a third of the size of our main effect. We suspect this is a result of bunching in loan amounts as shown in the McCrary density test for the pre-period in figure A.8. Specifically, there is bunching to the left of cutoff, likely due to a round peso amount close to the cutoff. Banks may regularly use different interest rate pricing rules for loans on either side of a round number, which could explain the slight effect around the discontinuity. There is no regulatory or otherwise advantageous reason for borrowers to be on either side of the cutoff in the pre-period. In the disclosure period, all loans have the same disclosure requirements as specified by law 20.448, so it is unlikely banks or borrowers are sorting to avoid informational disclosures. As standardized products should offer the same or lower rates than loan contracts above the cutoff, suggesting we should see bunching on the other side of the cutoff if it were due to borrower manipulation of loan size.

Figures A.4 and A.5 show the results of bandwidth sensitivity on the RD jump coefficient. We plot the regression discontinuity coefficient in intervals of 10 UF starting from an initial bandwidth of 50 UF. We find that the coefficient is stable and significant for bandwidths larger than the MSE-optimized bandwidth choice of 138.5 for both default and delinquency. For delinquency, the coefficient then remains stable (though becomes insignificant) for bandwidths as small as 110 UF. Lastly, we conduct placebo cutoff tests at 10 UF intervals between 900 UF and 1,100 UF in figures A.6 and A.7. We find that the RD coefficient is not significant below 1,000 UF. As expected, the coefficient then becomes negative and significant at and slightly above the actual cutoff (until 1,020 UF). For larger cutoffs, the coefficient is then either insignificant or positive. For defaults, the coefficient is significant only around the 1,000 UF cutoff.

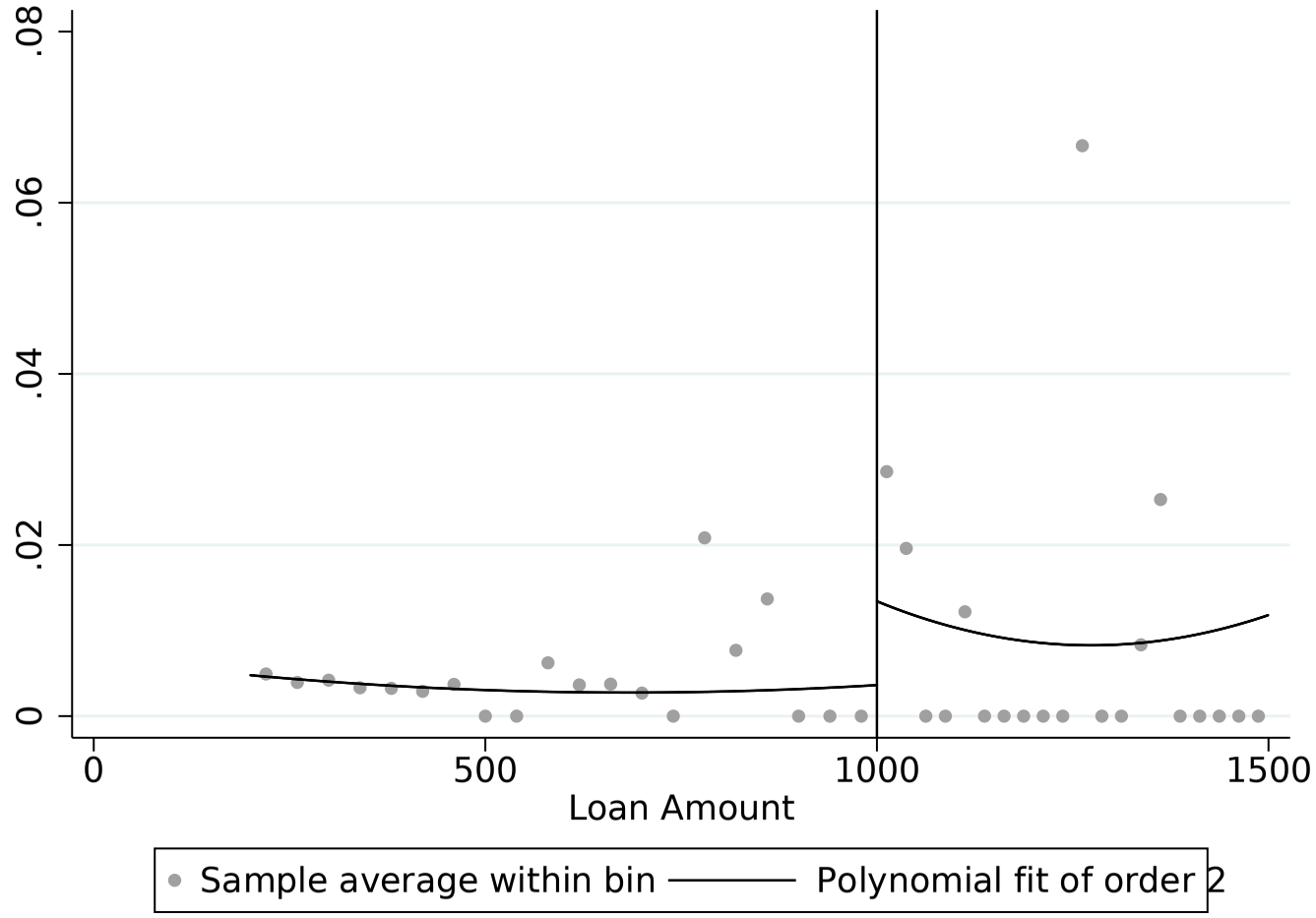
Lastly, we run the regression discontinuity restricting the loan size slope coefficients to zero in figure A.10 and table A.3. We still find that the discontinuity is significant at the five percent level, though the coefficient decreases to 8 percentage points from from 14.4.

Figure A.1: Global Polynomial - Ever Delinquent



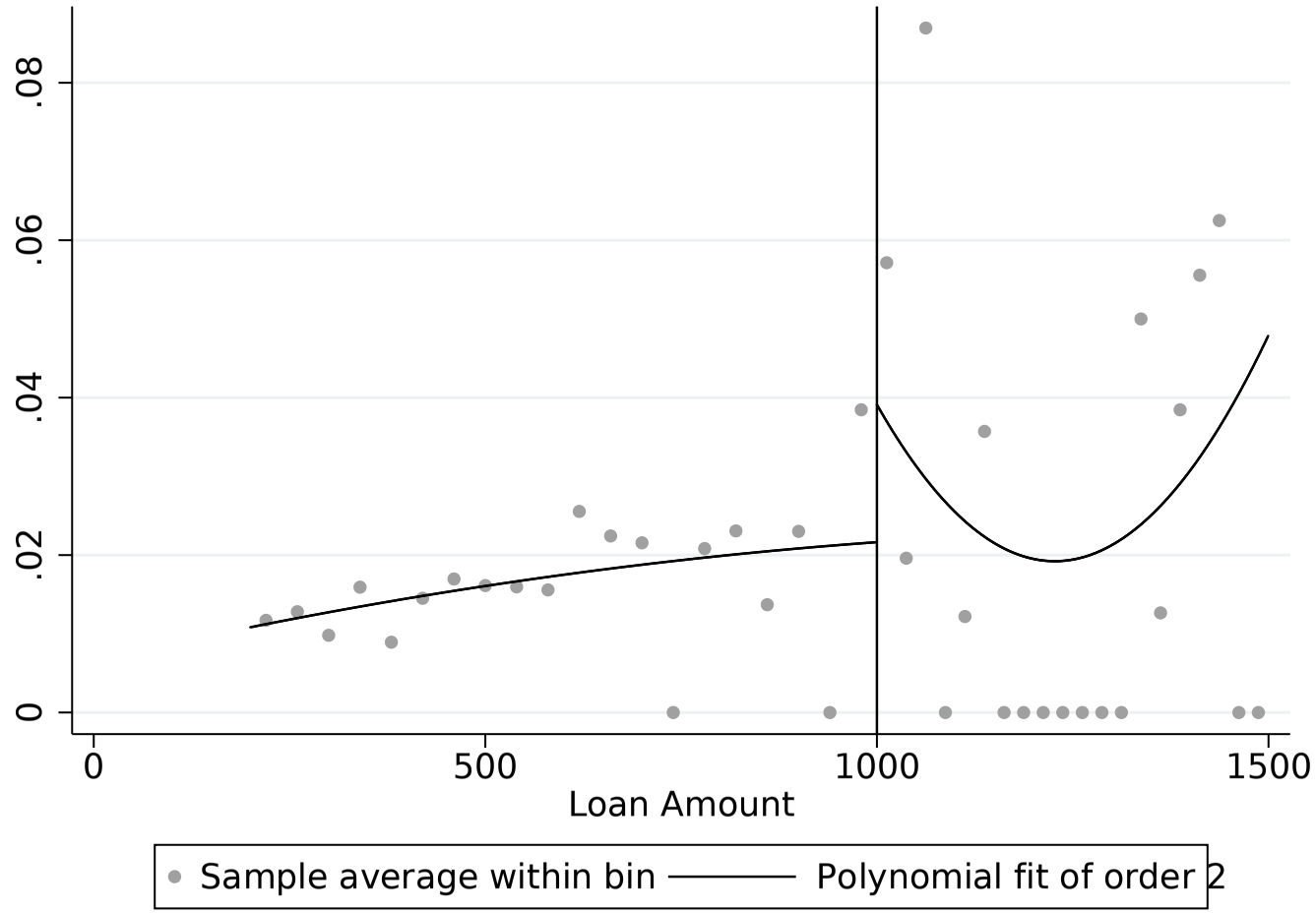
Notes: Global second-degree polynomials drawn to best fit ever delinquent rate on each side of the 1,000 UF loan cutoff (black vertical line).

Figure A.2: Global Polynomial - Ever Default



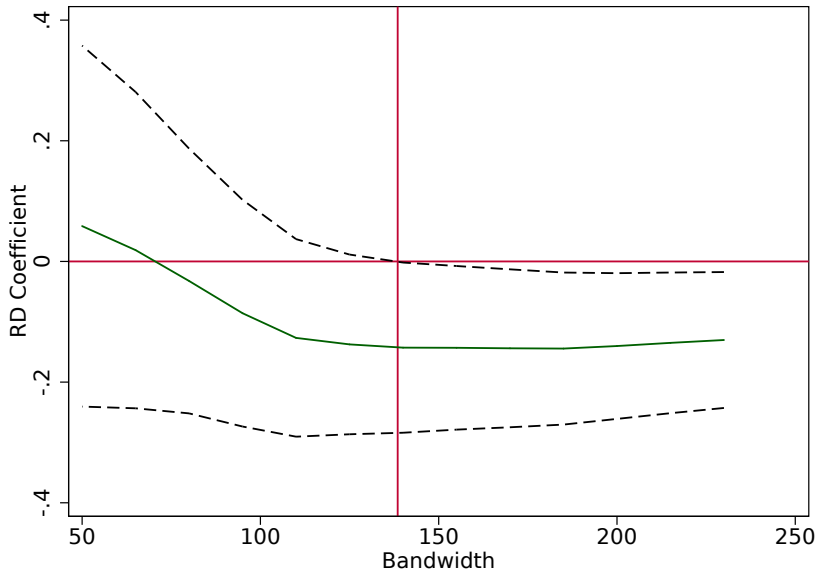
Notes: Global second-degree polynomials drawn to best fit ever default rate on each side of the 1,000 UF loan cutoff (black vertical line).

Figure A.3: Global Polynomial - Ever Extended



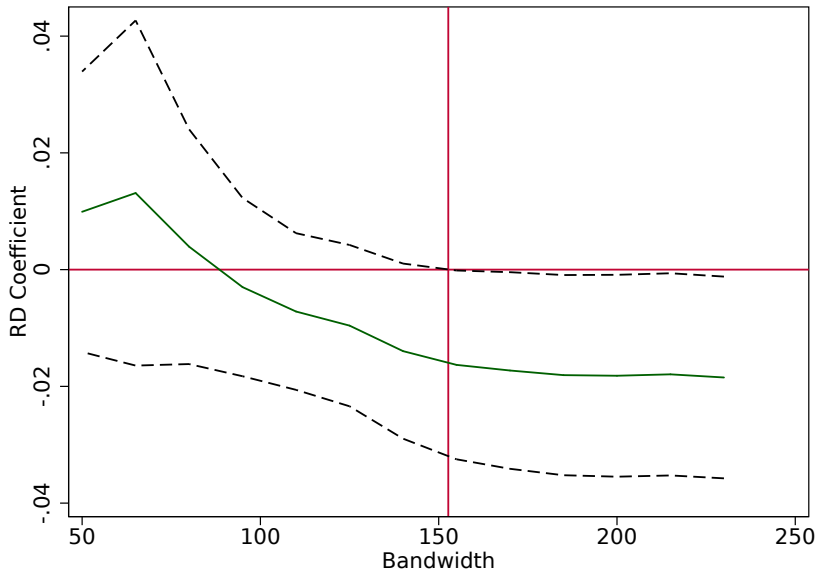
Notes: Global second-degree polynomials drawn to best fit the probability that the maturity was ever extended on each side of the 1,000 UF loan cutoff (black vertical line).

Figure A.4: Regression Discontinuity Bandwith Sensitivity: Delinquency



Notes: Regression discontinuity coefficient estimates of equation 1 with 95% confidence intervals for varying levels of bandwidths. We vary the bandwidth in intervals of 10 UF and graph the corresponding coefficients and confidence intervals. The vertical red line corresponds with the optimal bandwidth chosen by the procedure outlined in Calonico et al. 2014 and Calonico et al. 2018.

Figure A.5: Regression Discontinuity Bandwith Sensitivity: Default



Notes: This figure graphs the regression discontinuity coefficient estimates of equation 1 with 95% confidence intervals for varying levels of bandwidths. We vary the bandwidth in intervals of 15 UF between 50 UF and 230 UF and graph the corresponding coefficients and confidence intervals. The vertical red line corresponds with the optimal bandwidth chosen by the procedure outlined in Calonico et al. 2014 and Calonico et al. 2018.

Table A.1: Regression Discontinuity with Additional Controls

	(1) Ever Defaulted	(2) Ever Delinquent	(3) Ever Extended
Transparency	-0.169**	-0.0203**	-0.0000357
	(0.0768)	(0.0103)	(0.0318)
Loan Size	-0.173***	-0.00991	-0.0118
	(0.0595)	(0.00948)	(0.0234)
Transparency X Loan Size	0.159*	0.00435	0.0290
	(0.0859)	(0.0121)	(0.0296)
Comuna Fixed Effects	Y	Y	Y
Lender Fixed Effects	Y	Y	Y
Bandwidth	150	174	201
Kernel	Tri	Tri	Tri
Mean	.298	.024	.048
N	957	1,045	1,157

Robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.1 gives the estimated effect of the presentation of a standardized contract and increased disclsoure (Transparency) on default, delinquency, and maturity extensions using additional controls. All estimates are based on regressions that include fixed effects for comunas (neighbourhoods), and lender, as well as controls for the credit risk, income, age, sex, and marital status of the borrower. Expected inflation (future UF to peso inflation rate) and interbank rate are included as controls for aggregate economic conditions. Additional controls presented in this table are outstanding debt, number of outstanding loans, and leverage (debt to income ratio). We use the bandwidth selection procedure outlined in Calonico et al. 2014 and Calonico et al. 2018.

Table A.2: Regression Discontinuity, Pre-period

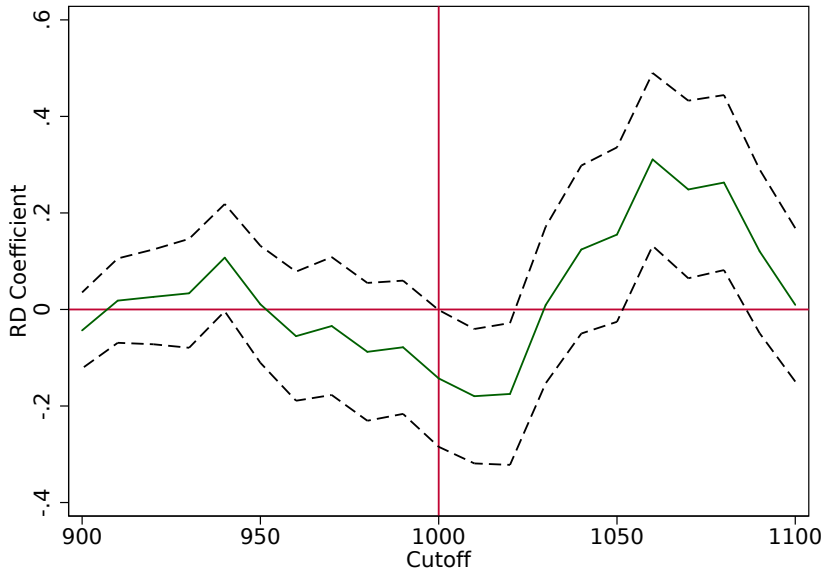
	(1) Ever Defaulted	(2) Ever Delinquent	(3) Ever Extended
Loan Size < 1,000 UF	-0.0502*	0.00630**	0.0102
	(0.0275)	(0.00272)	(0.0158)
Loan Size	-0.0277	0.00934	0.00758
	(0.0397)	(0.00634)	(0.0237)
Transparency X Loan Size	-0.0386	-0.00321	0.00477
	(0.0477)	(0.00728)	(0.0299)
Comuna Fixed Effects	Y	Y	Y
Lender Fixed Effects	Y	Y	Y
Bandwidth	138	153	131
Kernel	Tri	Tri	Tri
Mean	.128	-.002	.047
N	3,283	3,535	3,142

Robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

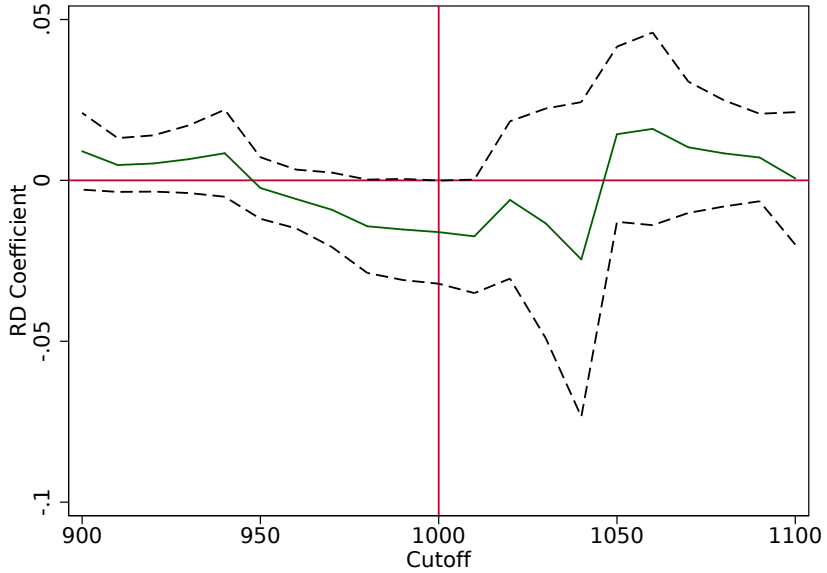
Table A.2 gives the estimated effect of having a loan smaller than 1,000 UF on delinquency, default, and maturity extensions before the regulation was announced (January 2009-October 2011). All estimates are based on regressions that include fixed effects for comunas (neighbourhoods), and lender, as well as controls for the credit risk, income, age, sex, and marital status of the borrower. Expected inflation (future UF to peso inflation rate) and interbank rate are included as controls for aggregate economic conditions. We use the bandwidth selection procedure outlined in Calonico et al. 2014 and Calonico et al. 2018.

Figure A.6: Regression Discontinuity Cutoff Sensitivity: Delinquency



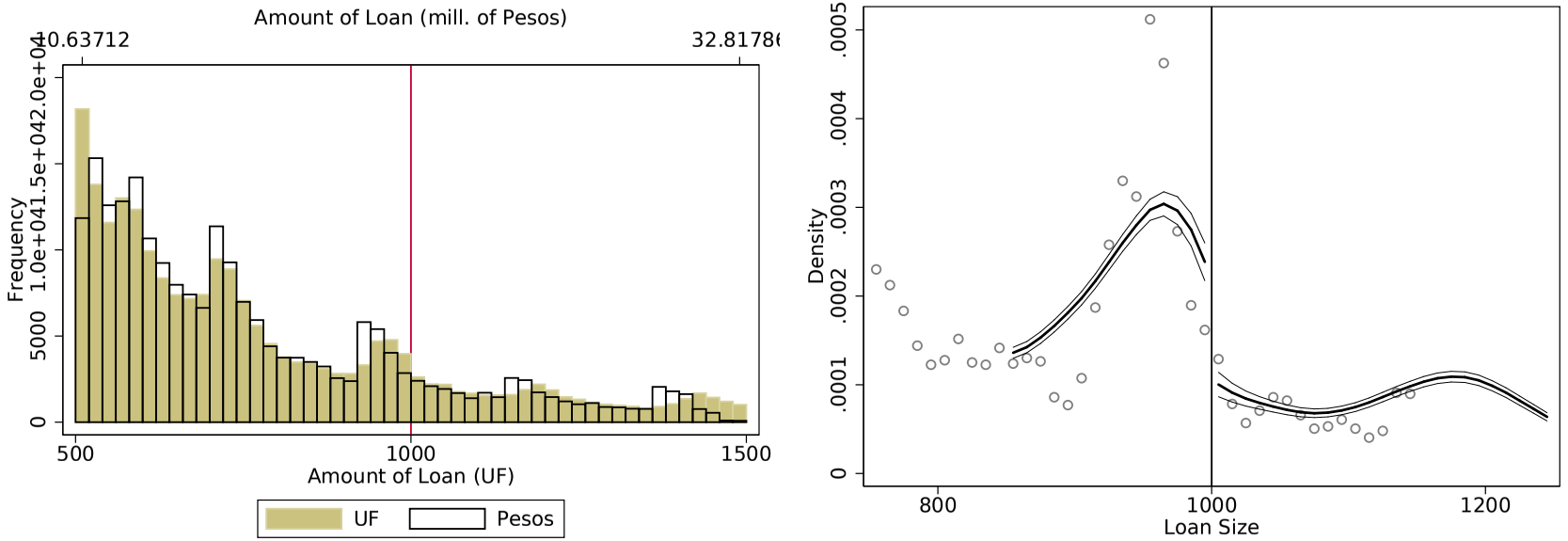
Notes: This figure graphs the regression discontinuity coefficient estimates of equation 1 with 95% confidence intervals for varying cutoffs around loan size. We vary the cutoffs by 10 UF between 900 and 1,100 UF. The vertical red line corresponds with the 1,000 UF bandwidth specified by law 20.448.

Figure A.7: Regression Discontinuity Cutoff Sensitivity: Default



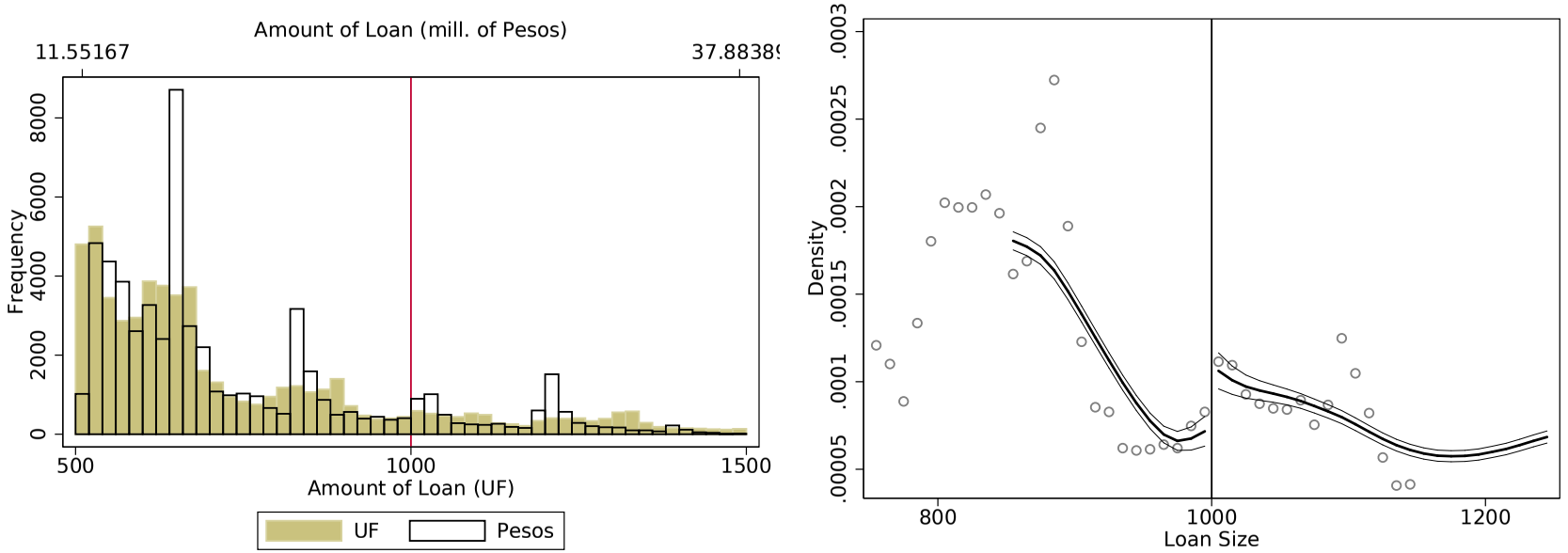
Notes: This figure graphs the regression discontinuity coefficient estimates of equation 1 with 95% confidence intervals for varying cutoffs around loan size. We vary the cutoffs by 10 UF between 900 and 1,100 UF. The vertical red line corresponds with the 1,000 UF cutoff specified by law 20.448.

Figure A.8: Loan Size Density - Pre-period



Notes: The left figure graphs distribution of loan amounts in pesos and UF around the 1,000 UF cutoff between January 2009 and October 2011 (prior to any regulatory changes). The coloured bars are the loan amounts in UF (bottom x-axis) while the clear bars are the corresponding peso amounts (top x-axis). The red line corresponds to the 1,000 UF cutoff. The right figure shows the McCrary density test for loan amounts in UF around the 1,000 UF cutoff between January 2009 and October 2011. The vertical black line is the placebo for the 1,000 UF cutoff outlined in law 20.448. Confidence intervals are shown at the 95% significance level.

Figure A.9: Loan Size Density - Disclosure



Notes: The left figure graphs distribution of loan amounts in pesos and UF around the 1,000 UF cutoff between August 2012 to December 2015 (after the introduction of law 20.555). The coloured bars are the loan amounts in UF (bottom x-axis) while the clear bars are the corresponding peso amounts (top x-axis). The red line corresponds to the 1,000 UF cutoff. The right figure shows the McCrary density test for loan amounts in UF around the 1,000 UF cutoff between August 2012 and December 2015. The vertical black line is for the 1,000 UF cutoff specified by law 20.448. Confidence intervals are shown at the 95% significance level.

Table A.3: Regression Discontinuity, No Slope

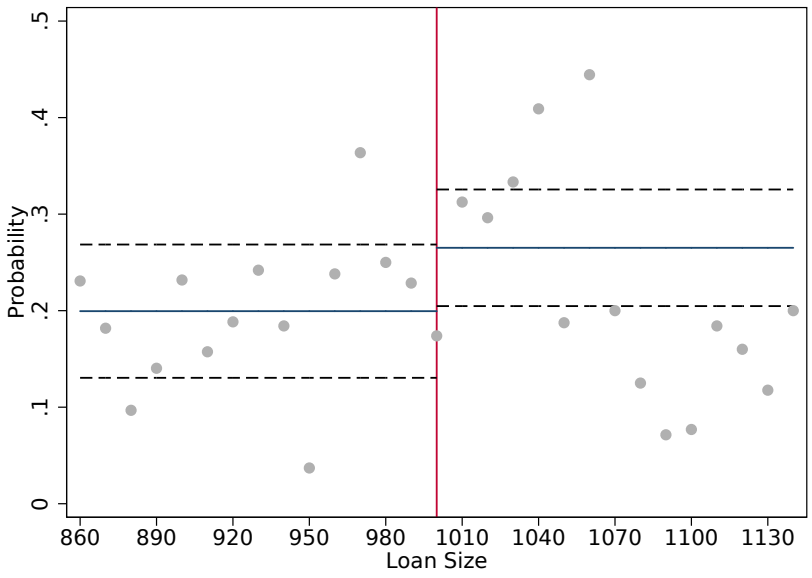
	(1) Ever Defaulted	(2) Ever Delinquent	(3) Ever Extended
Transparency	-0.0802**	-0.00714	-0.00691
	(0.0342)	(0.00512)	(0.0153)
Comuna Fixed Effects	Y	Y	Y
Lender Fixed Effects	Y	Y	Y
Controls	Y	Y	Y
Bandwidth	138	153	131
Kernel	Tri	Tri	Tri
Mean	.265	.011	.03
N	1,088	1,183	1,033

Robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.3 gives the estimated effect of a standardized contract and increased disclsoure (Transparency) on default, delinquency, and maturity extensions using additional controls. Loan size controls are not included. All estimates are based on regressions that include fixed effects for comunas (neighbourhoods), and lender, as well as controls for the credit risk, income, age, sex, and marital status of the borrower. Expected inflation (future UF to peso inflation rate) and interbank rate are included as controls for aggregate economic conditions. We use the bandwidth selection procedure outlined in Calonico et al. 2014 and Calonico et al. 2018.

Figure A.10: Ever Delinquent Regression Discontinuity - no slope



Notes: This figure gives a visual representation to the results presented in table A.3 of the estimates for equation 1. Loan size controls are not included. All estimates are based on regressions that include fixed effects for comunas (neighbourhoods), and lender, as well as controls for the credit risk, income, age, sex, and marital status of the borrower. Expected inflation (future UF to peso inflation rate) and interbank rate are included as controls for aggregate economic conditions. We use the bandwidth selection procedure outlined in Calonico et al. 2014 and Calonico et al. 2018.

Appendix B Difference-in-differences: Other concurrent regulations

We see from figures 12 and 14 that a change may have occurred in the consumer loan market around March of 2012. Indeed, Liberman et al. 2018 document that the Chilean government introduced another policy change in February of 2012. As a result of the 2010 earthquake that caused financial strain to borrowers, the government declared that any borrowers with cumulative defaults of less than 2.5 million pesos (about $4,000 USD or 200 UF) as of December 2011 would have their default records removed from the credit registry. Going forward, defaults and delinquencies would still be recorded, but this would be a one-time credit score "holiday" for roughly 21 percent of borrowers.

In Chile there are two different credit registries. The first is a record of the number, amount, and delinquency record of bank loans. This registry is shared between banks by the SBIF and was unaffected by this regulation. The second is a registry of delinquencies for nonbank and bank lenders, which did experience this default holiday. The effect was that nonbank lenders no longer had access to any external credit information and banks lost access to nonbank delinquency information. We provide evidence for how this law change may have affected our results and find it does not materially change our conclusions.

Looking at the evolution of aggregate credit, March 2012 shows a clear restriction in the total amount of credit loaned (figure B.1). However, the restriction in credit access did not substantially change the distribution of credit across education level (figure B.3). Given that banks did not relatively increase their provisions against new loans for either group (figure B.2), we believe the primary risk management strategy enacted by banks was through borrower selection rather than to maintain normal lending relations and provision more for these loans. Thus, we explore how borrower selection be lenders may have affected our estimates, first for less sophisticated borrowers and then separately for more sophisticated borrowers.

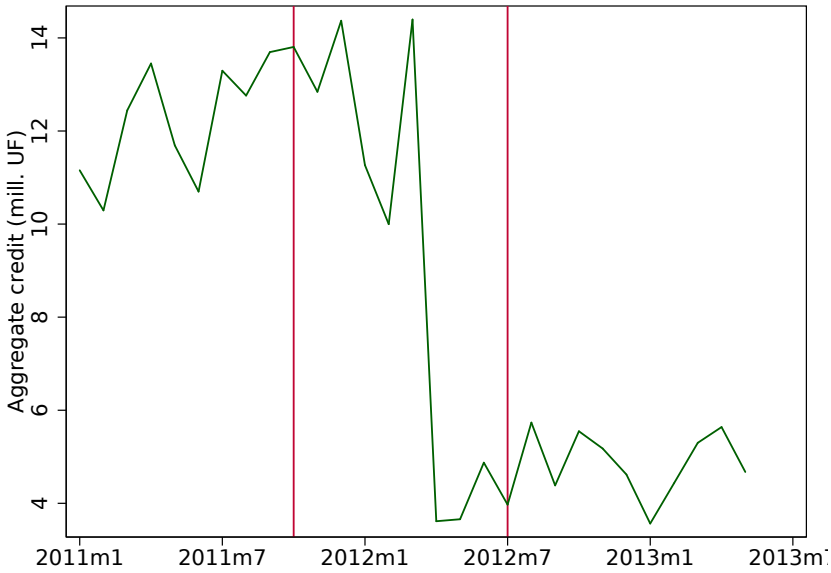
As less sophisticated borrowers are most at risk for being selected against (as they are the most exposed to a rise in expected credit costs as documented in Liberman et al. 2018), we can indeed see from figure B.4-B.6 that around March 2012 they had to have much lower credit risk, lower interest rates, and smaller debt amounts in order to take out a loan. This means that they were a relatively better quality borrower than the control group, leading our lower than high school borrowers to

show a downward spike in default around the same time in figure 12. Thus it seems reasonable to examine our estimates in light of a permanent increase in the quality of less than high school borrowers in relation to the control group. If this is the case, then our estimates for the relative effect of delinquency should be downwardly biased (i.e. less than high school educated borrowers should default at a lower rate than our control group). This seems likely to be the case as our model suggests we should find a minimal to null effect of disclosure regulation on these borrowers while the data suggests a persistent positive effect (less likely to be delinquent). Thus it is possible that this regulation indeed affects our results and biases us against finding the null effect we would have predicted.

For the borrowers with a more than high school education, the spike in delinquencies around March 2012 might suggest that the borrower quality of the control group had improved relative to that of the sophisticated borrowers. This makes sense as the more educated borrowers were more likely to use bank loans rather than non-bank credit (Liberman et al. 2018) and thus experience fewer information asymmetries. Therefore it seems likely that maintaining the same selection standards for the borrowers with more than high school education while raising them for the control group would indeed suggest the pattern we see in delinquencies for both groups around March 2012. However, figures B.4-B.9 show that around this time more sophisticated borrowers actually improved their credit risk,interest rates, and lowered their debt amounts despite higher delinquencies at the same time relative to our control group. Further, substantial changes in both delinquency and credit risk after the introduction of the disclosure policy suggest that our findings for sophisticated borrowers are not affected by borrower selection due to credit registry deletions.

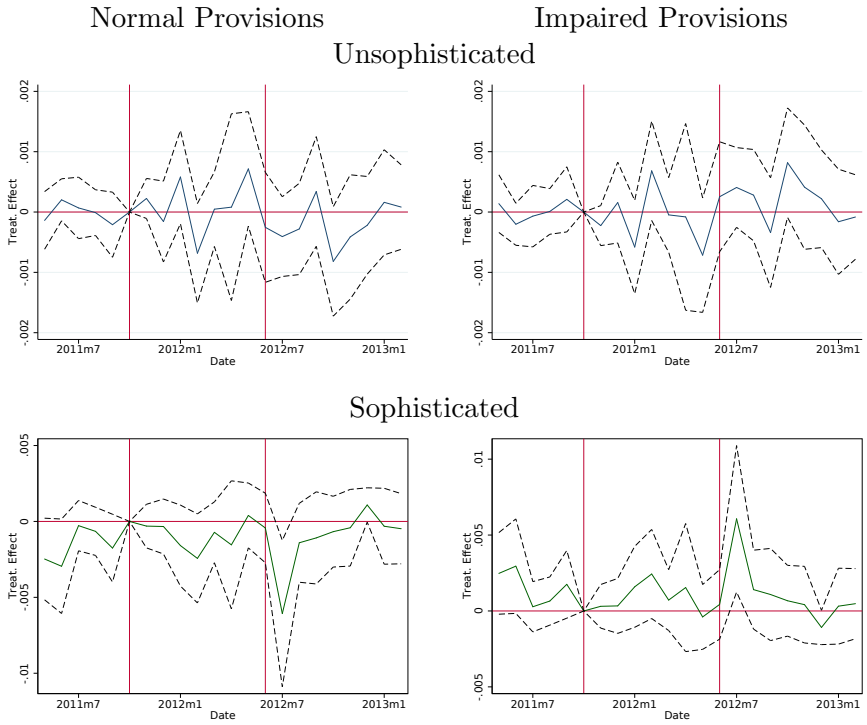
We provide additional difference-in-differences results in figure B.10-B.11 for other relevant borrowing characteristics such as income (generally increases for both borrower types), default (no effect for either group), outstanding loans (increased after the standardization regulation for both groups), maturity (reduced after standardization for both groups), loan size (decreased for unsophisticated, increased for sophisticated), and switching behaviour (both groups less likely to switch banks).

Figure B.1: Aggregate Credit



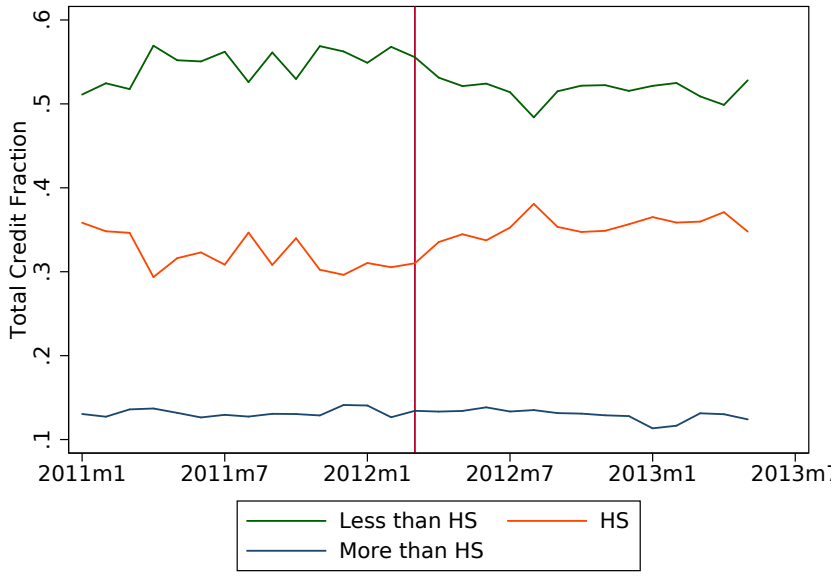
Notes: This figure graphs the sum of all loan amounts (in millions of UF) in UF of newly issued loans by issuance date. The first red line is the implementation date of law 20.448 (the introduction of Universal Credit Contracts) and the second red line is the implementation date of law 20.555 (disclosure requirements for all loans).

Figure B.2: Credit Provisions



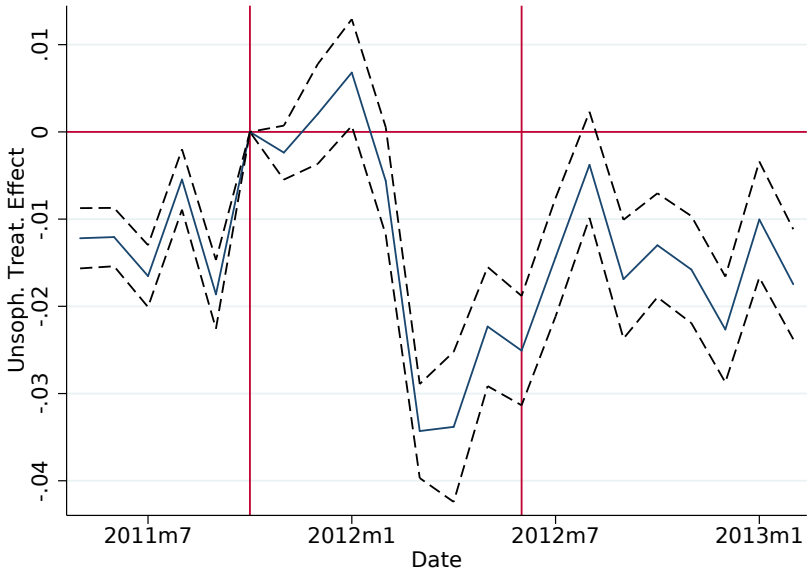
Notes: Estimates of βs from equation(2) for borrowers in neighbourhoods with the average education below 11.5 years of schooling ("unsophisticated") as compared to the control group (11.5 to 12 years of schooling). The dependent variable is normal provisions for figures on the left (provisions against loans in good standing) and impaired provisions (provisions against loans that are impaired). Loans are collapsed to one data point per observation, and all loans are 2 years maturity or less and under 1,000 UF in loan amount. The first vertical red line marks the implementation of law 20.448 (introduction of Universal Credit contracts with standardized features and improved disclosure) and the second vertical red line marks the implementation of law 20.555 which introduced improved disclosure to all loan contracts.

Figure B.3: Borrower Composition



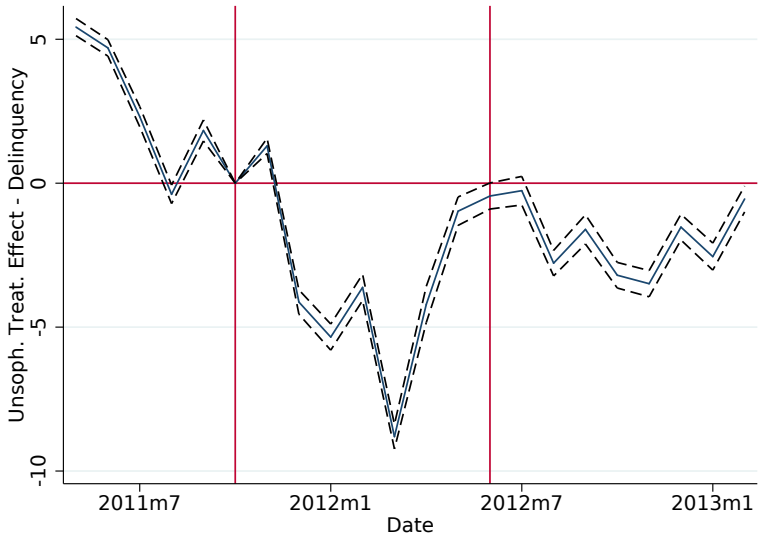
Notes: Fraction of total credit by loan size disbursed to each level of neighbourhood education average. Our education levels below 11.5 years of schooling for less than high school, between 11.5 and less than 12 years of schooling for high school educated, and above 12 years of schooling for more than high school educated. The red vertical line denotes March of 2012 when the non-bank credit registry was not available to banks making lending decisions.

Figure B.4: Credit Risk: Unsophisticated borrowers versus control



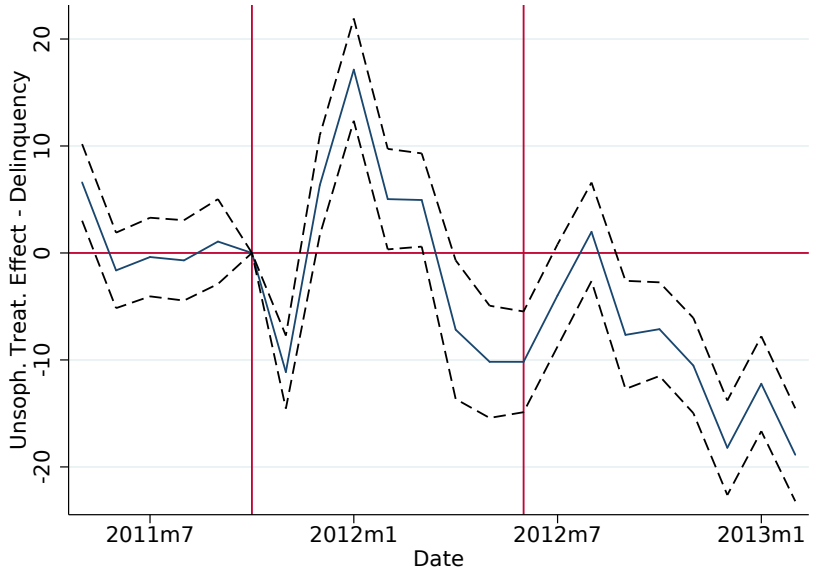
Notes: Estimates of βs from equation(2) for borrowers in neighbourhoods with the average education below 11.5 years of schooling ("unsophisticated") as compared to the control group (11.5 to 12 years of schooling). The dependent variable is credit risk, orthe amount of loans provisioned across all bank loans for an individual. Loans are collapsed to one data point per observation, and all loans are 2 years maturity or less and under 1,000 UF in loan amount. Confidence intervals are shown at the 95% significance level. The first vertical red line marks the implementation of law 20.448 (introduction of Universal Credit contracts with standardized features and improved disclosure) and the second vertical red line marks the implementation of law 20.555 which introduced improved disclosure to all loan contracts.

Figure B.5: Interest Rate: Unsophisticated borrowers versus control



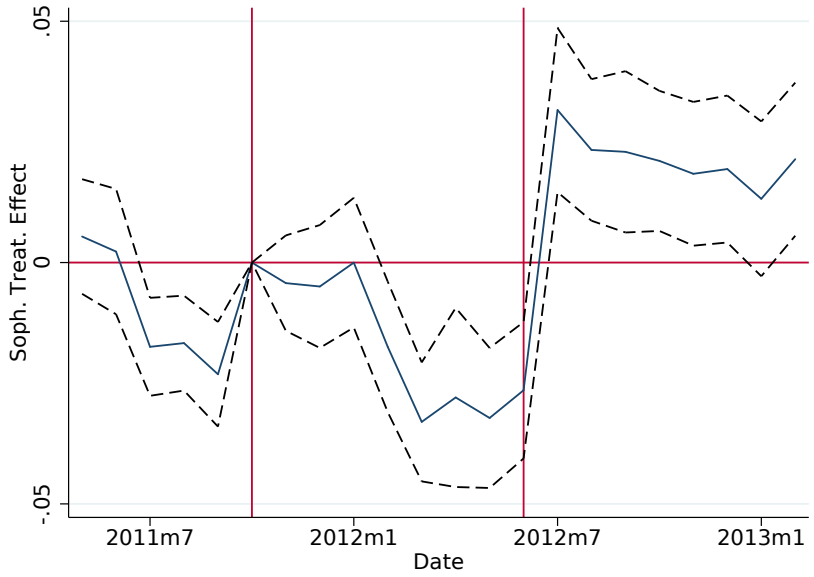
Notes: Estimates of βs from equation(2) for borrowers in neighbourhoods with the average education below 11.5 years of schooling ("unsophisticated") as compared to the control group (11.5 to 12 years of schooling). The dependent variable is interest rate inclusive of all fees and insurances. Loans are collapsed to one data point per observation, and all loans are 2 years maturity or less and under 1,000 UF in loan amount. Confidence intervals are shown at the 95% significance level. The first vertical red line marks the implementation of law 20.448 (introduction of Universal Credit contracts with standardized features and improved disclosure) and the second vertical red line marks the implementation of law 20.555 which introduced improved disclosure to all loan contracts.

Figure B.6: Outstanding Debt: Unsophisticated borrowers versus control



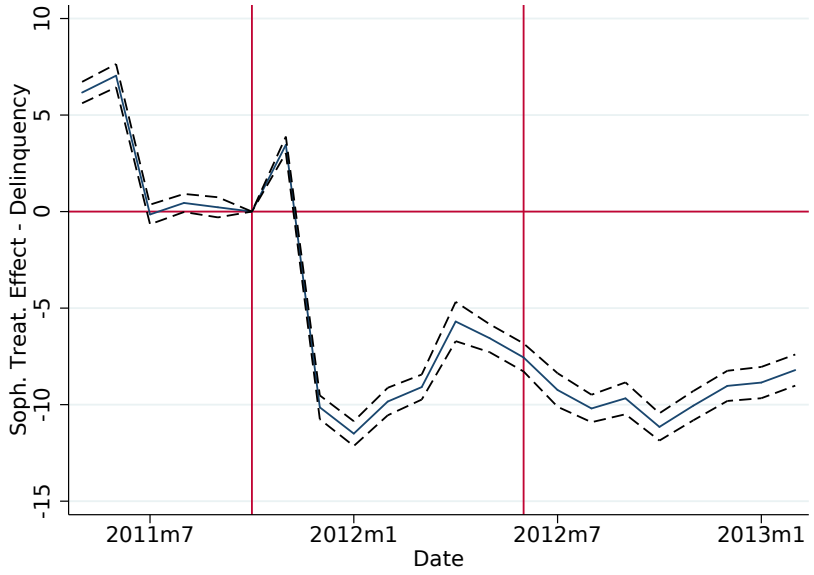
Notes: Estimates of βs from equation(2) for borrowers in neighbourhoods with the average education below 11.5 years of schooling ("unsophisticated") as compared to the control group (11.5 to 12 years of schooling). The dependent variable is outstanding debt (debt balance as imputed by monthly fixed payments). Loans are collapsed to one data point per observation, and all loans are 2 years maturity or less and under 1,000 UF in loan amount. Confidence intervals are shown at the 95% significance level. The first vertical red line marks the implementation of law 20.448 (introduction of Universal Credit contracts with standardized features and improved disclosure) and the second vertical red line marks the implementation of law 20.555 which introduced improved disclosure to all loan contracts.

Figure B.7: Credit Risk: Sophisticated borrowers versus control



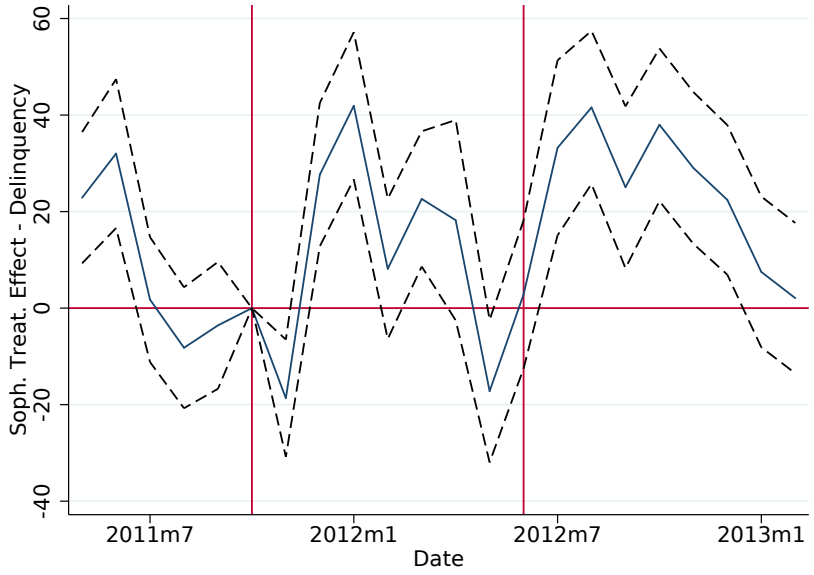
Notes: Estimates of βs from equation(2) for borrowers in neighbourhoods with the average education at or above 12 years of schooling ("sophisticated") as compared to the control group (11.5 to 12 years of schooling). The dependent variable is credit risk, orthe amount of loans provisioned across all bank loans for an individual. Loans are collapsed to one data point per observation, and all loans are 2 years maturity or less and under 1,000 UF in loan amount. Confidence intervals are shown at the 95% significance level. The first vertical red line marks the implementation of law 20.448 (introduction of Universal Credit contracts with standardized features and improved disclosure) and the second vertical red line marks the implementation of law 20.555 which introduced improved disclosure to all loan contracts.

Figure B.8: Interest Rate: Sophisticated borrowers versus control



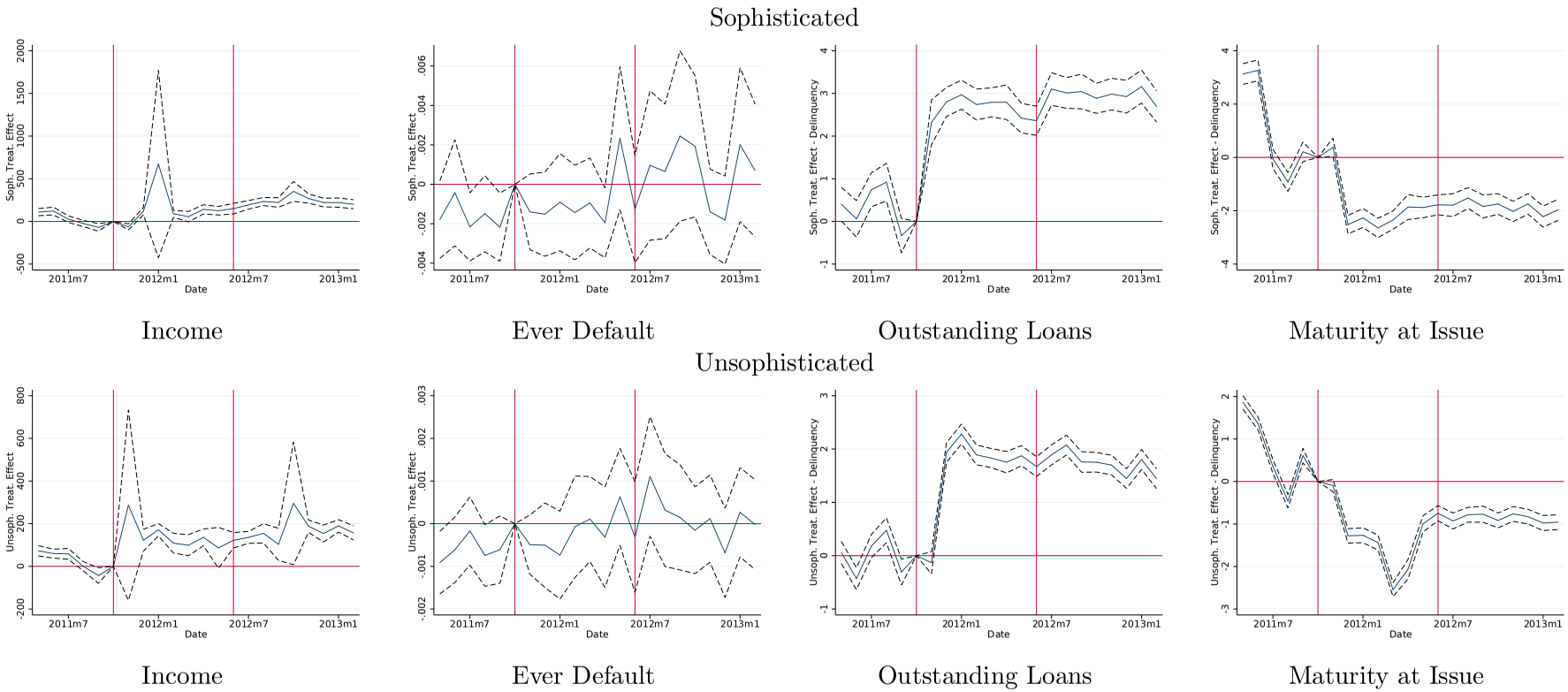
Notes: Estimates of βs from equation(2) for borrowers in neighbourhoods with the average education at or above 12 years of schooling ("sophisticated") as compared to the control group (11.5 to 12 years of schooling). The dependent variable is interest rate inclusive of all fees and insurances. Loans are collapsed to one data point per observation, and all loans are 2 years maturity or less and under 1,000 UF in loan amount. Confidence intervals are shown at the 95% significance level. The first vertical red line marks the implementation of law 20.448 (introduction of Universal Credit contracts with standardized features and improved disclosure) and the second vertical red line marks the implementation of law 20.555 which introduced improved disclosure to all loan contracts.

Figure B.9: Outstanding debt: Sophisticated borrowers versus control



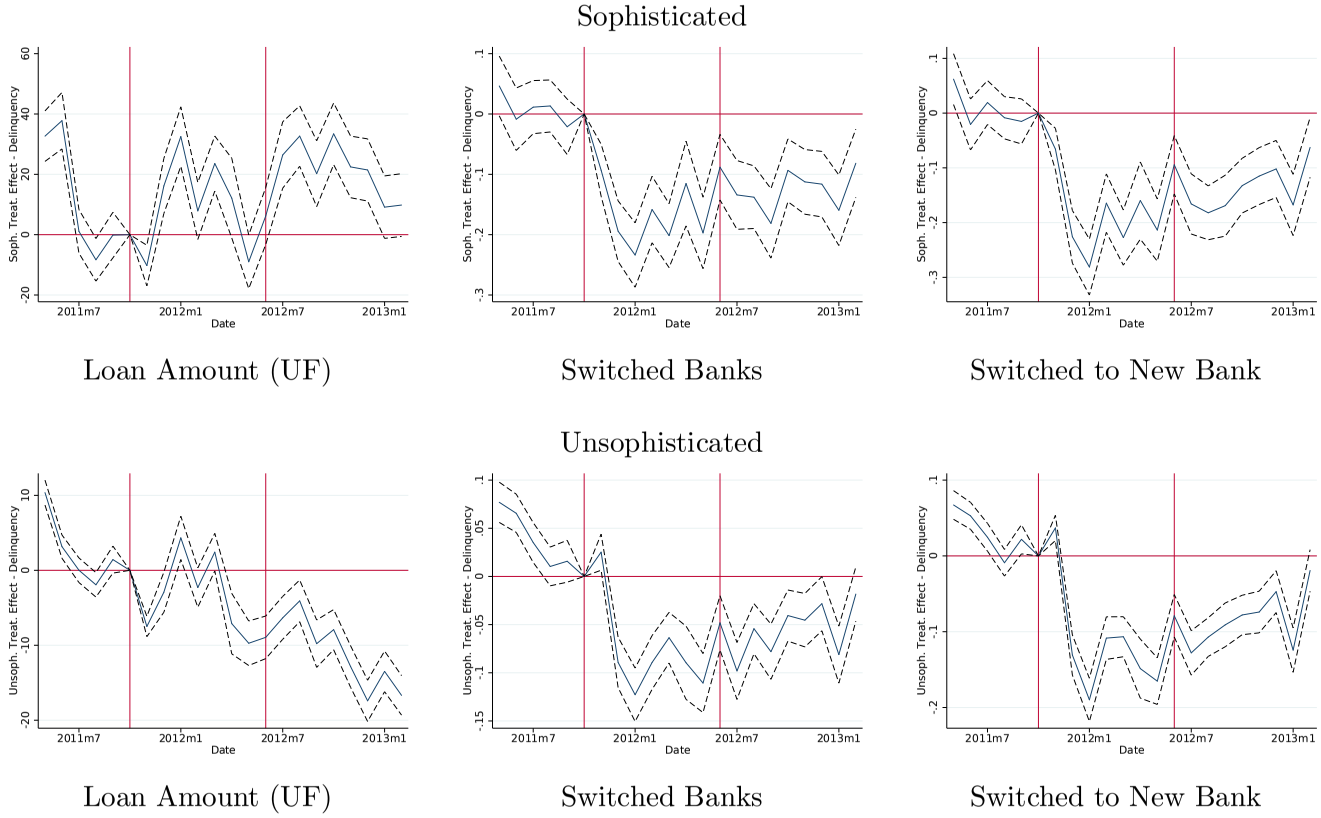
Notes: Estimates of βs from equation(2) for borrowers in neighbourhoods with the average education at or above 12 years of schooling ("sophisticated") as compared to the control group (11.5 to 12 years of schooling). The dependent variable is outstanding debt (debt balance as imputed by monthly fixed payments). Loans are collapsed to one data point per observation, and all loans are 2 years maturity or less and under 1,000 UF in loan amount. Confidence intervals are shown at the 95% significance level. The first vertical red line marks the implementation of law 20.448 (introduction of Universal Credit contracts with standardized features and improved disclosure) and the second vertical red line marks the implementation of law 20.555 which introduced improved disclosure to all loan contracts.

Figure B.10: Other Characteristics - I



Notes: Estimates of βs from equation(2) for borrowers in neighbourhoods with the average education at or above 12 years of schooling ("sophisticated") as compared to the control group (11.5 to 12 years of schooling) in the first row. The second row shows estimates of βs from equation(2) for borrowers in neighbourhoods with the average education below 11.5 years of schooling ("unsophisticated") as compared to the control group (11.5 to 12 years of schooling). Loans are collapsed to one data point per observation, and all loans are 2 years maturity or less and under 1,000 UF in loan amount. Confidence intervals are shown at the 95% significance level. The first vertical red line marks the implementation of law 20.448 (introduction of Universal Credit contracts with standardized features and improved disclosure) and the second vertical red line marks the implementation of law 20.555 which introduced improved disclosure to all loan contracts. Income is the total annual income for the borrower in UF, ever default is an indicator if a loan payment has not been made in 90 days and judicial proceedings have been initiated against the borrower. Outstanding loans are the total number of loans the borrower has at the time of origination, and maturity at issue is the maturity of the loan in months at the date of loan issuance.

Figure B.11: Other Characteristics - II



Notes: Estimates of βs from equation(2) for borrowers in neighbourhoods with the average education at or above 12 years of schooling ("sophisticated") as compared to the control group (11.5 to 12 years of schooling) in the first row. The second row shows estimates of βs from equation(2) for borrowers in neighbourhoods with the average education below 11.5 years of schooling ("unsophisticated") as compared to the control group (11.5 to 12 years of schooling). Loans are collapsed to one data point per observation, and all loans are 2 years maturity or less and under 1,000 UF in loan amount. Confidence intervals are shown at the 95% significance level. The first vertical red line marks the implementation of law 20.448 (introduction of Universal Credit contracts with standardized features and improved disclosure) and the second vertical red line marks the implementation of law 20.555 which introduced improved disclosure to all loan contracts. Loan size is the size of the loan in UF. Switch banks is an indicator for if the borrower took out this loan at a bank different than the bank they previously took a loan at. Switch to new bank is an indicator for if the borrower had not previously taken out a loan at this bank.