Removing the Fine Print: Standardized Contracts, 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 cost or ignore it and risk surprises in the future. Two policy changes can be used to reduce the importance of fine print: improved disclosure and the option of a standardized contract. We hypothesize that sophisticated borrowers with low studying costs are less likely to be delinquent with disclosure regulations that help them understand their loans more. Conversely, standardized contracts help unsophisticated borrowers avoid delinquency because they are protected from unexpected fees. Using a regression discontinuity, we find that borrowers offered increased disclosure experienced a 40% (14.4 percentage points) reduction in delinquency rates. Because the regulation is applied in a different currency than that denominated in loan contracts, we observe no bunching on either loan amounts or observable borrower characteristics. Using a difference-in-differences design, with education as a proxy for financial sophistication, we find that sophisticated borrowers are delinquent 10 percentage points less under increased disclosure., while unsophisticated borrowers receive a similar benefit from product standardization. These results suggest heterogeneous borrowers might benefit from a variety of financial regulations.

Keywords: Contract design, complexity, consumer financial protection, disclosure, standardization
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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 cost or ignore it and risk the possibility of expensive 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 insurance, for example, and subsequently increase the probability of lacking sufficient funds to pay their bills.

Governments have frequently used two types of regulation to reduce information asymmetries that result from fine print in financial contracts. One strategy is *standardized contract features*: regulations such as the Durbin Amendment eliminate features of financial contracts that are deemed pernicious. Consumers cannot be surprised by fees that firms can't charge. Second is *increased disclosure*. Regulations such as the Truth in Lending Act and the Securities Exchange Commission'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 standardized contracts and increased disclosure lead to better loan outcomes? If so, does one size fit all or are different regulations helpful for different consumers?

We propose and test a set of hypotheses based on Heidhues et al. (2018). We show that delinquency is influenced by the cost of studying financial contracts, which depend on one's level of financial sophistication. If a borrower has unexpectedly high fees after signing a contract, they may not have enough to make their loan payment. In contrast, if the borrower is aware of the total monthly cost, they can either choose not to take out a loan, take out a loan with a better ability to budget for monthly costs, or search for a more affordable loan from a different lender if there is a risk they couldn't afford the loan contract under consideration.

Our framework suggests different policies can help different borrowers avoid delinquency. In particular, borrowers with a high cost of studying (financially unsophisticated) will benefit from standardized contracts. These contracts can reduce unexpected fees without the borrower having to carefully study, or even understand complex financial terminology. Disclosure regulations make salient important elements of the financial contract, which help borrowers that are already fairly financially sophisticated ("low cost") borrowers that may not want to read the fine print, but would understand it if they did.

Focusing on Chile, we use a unique combination of a change in regulation, administrative data on the population of consumer bank loans, and a pricing quirk to casually estimate the impact of standardization and disclosure on consumer outcomes. We compare borrowers who had loan amounts slightly below an administrative cutoff that were provided with the option of a standardized contract (removed superfluous add-on insurance) with improved disclosure (APR equivalent and a breakdown of loan fees). 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%).

Our regression discontinuity design is able to overcome the use of a traditionally endogenous variable, loan amount, as the running variable. This is due to 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. We also find no evidence for borrower selection on observables including loan interest rate on either side of the cutoff.We cannot, of course, rule out selection on unobservables. If there were important borrower characteristics observable to lenders but unobservable to us, the econometrician, however, we would expect them to be reflected in prices, i.e. loan interest rates which shows no discontinuity.

We separately identify the role of standardization versus disclosure in improving consumer outcomes in the Chilean loan market. One year after the banking regulator imposed new standardization and disclosure policies on smaller loans, the regulator imposed the same disclosure policy for all loans. Crucially for our identification strategy, the standardization policies continued to only be applied to loans below the cutoff. We find that standardized contracts do not have a statistically significant effect on delinquency. However, given that the regulatory loan-size cutoff is relatively large (1,000 UF), we are also interested in borrower behavior away from the cutoff. We believe we are the first to apply the methodology proposed by Angrist and Rokkanen (2015) to estimate the effects of standardized contracts away from the cutoff. We find that standardized contracts decrease delinquency as loan sizes decrease.

To determine how heterogeneous borrowers react to the different interventions, we follow Ru and Schoar (2017), among others, and use years of schooling as a reasonable proxy for financial sophistication. Using a difference-in-differences strategy, we empirically confirm our hypothesis: sophisticated borrowers benefit primarily from disclosure, whereas less sophisticated borrowers benefit primarily from standardization. Specifically, financially sophisticated borrowers reduce their delinquency rates by 10 percentage points relative to control borrowers under the disclosure regime. Financially unsophisticated borrowers reduce their delinquency rates by a similar margin when they have access to standardized contracts. Financially sophisticated borrowers do not seem to benefit from standardized contracts and financially unsophisticated borrowers do not seem to benefit from disclosure. The fact that borrower selection provides an incomplete explanation of our difference-in-differences results suggests that these regulations did not only discourage people from taking out loans, but may have helped people understand or bargain for better loan terms.

To test if borrowers obtained better initial loan terms or had an improved understanding of their terms, we use a "money on the table" approach similar to that of Argyle et al. (2017) and Atal (2016). We find that financially sophisticated borrowers left less "money on the table" on average and across both regulatory regimes, but especially so under the disclosure regime. In contrast, less financially sophisticated borrowers left more money on the table in all periods, even those when their delinquency rates fell. This suggests that financially sophisticated borrowers may have lowered their delinquency rates by searching across or bargaining more aggressively with lenders to achieve better initial loan terms. In contrast, less sophisticated consumers were not delinquent less due to improved initial loan terms, but because of their improved comprehension of their terms. Our results suggest that one-size financial regulation does not seem to fit all, particularly if regulation is limited to disclosure.

Our paper is structured as follows. Section 2 provides a review of the literature. Section 3 presents our testable hypotheses in a theoretical framework. Section 4 describes aspects of the financial system and our regulatory interventions. Sections 5, 6, and 7 present our regression discontinuity's identification strategy, data, and regression discontinuity results. Section 8 concludes.

2 Literature Review

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) that prevent borrowers from obtaining the required information to make optimal decisions. However, a growing literature (Célérier and Vallée 2017, Carvalho and Silverman 2019, Gao et al. 2020, Jin et al. 2018, and Ru and Schoar 2017 among others) has shown that complexity is used to shroud undesirable contract features from consumers. 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 use quasi-experimental variation and rich administrative data to identify the effects of disclosure and explicit regulation of contract features.

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), and reduce loan size and more responsible repayment behaviour (Padi 2018). 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 (Adams et al. 2019). 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.

In contrast to this literature, we document a large, robust, and heterogeneous effect of disclosure. Our ability to document such a significant effect stems from three features of our setting: first, 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. Second, we have administrative data on all consumer loan borrowers in the banking sector rather than at a subset of lenders. This allows us to track borrowers who decide to patronize a different bank after treatment, ensuring our sample experiences no attrition based on choice of lender. 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 measure disclosure mandated by the regulator rather than provided voluntarily by lenders. This is important since past research (Adams et al. 2019) has found that borrowers disregard disclosure from the lender in part because they assume it is self-interested. In contrast, borrowers may trust disclosure provided under the aegis of a regulator.

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.[1] 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 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

[1]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.

from less educated neighbourhoods benefit primarily from standardization. We 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.[2] 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 standardized contracts and disclosure.

3 Theoretical Framework

We develop a framework based on Heidhues et al. (2018) presented in appendix A. For brevity, we outline our predictions and their intuition here.

There are multiple potential borrowers naturally endowed with a particular level of financial sophistication prior to taking out a loan contract. This level of financial sophistication is unobserved by lenders and also influences a borrower's cost of studying (in that higher financial sophistication requires a lower cost of studying).[3] Multiple lenders offer loan contracts with rate, maturity, size, and additional fees. Borrowers observe these loans and choose either to study or not study the contract and then can subsequently choose to take or leave the loan. If they choose not to take the loan, they can either continue searching for a loan contract at a different lender or they can choose not to take a loan at all. A borrower can choose to study and learn the additional fees associated with the loan exactly, but if a borrower does not study, then they learn the additional fees associated with their loan only after they have chosen to sign the contract.

[2] 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.

[3] This is a simplifying assumption. The predictions of our model do not change if lenders have a signal of the financial sophistication of the borrower as in Ru and Schoar (2017).

The probability of default of a borrower can be decomposed into two parts: the probability that they study their loan and the probability that they will default on the loan contract they sign. We assume that a borrower will regret taking out a contract where the actual fees are larger than the borrower's expectation of the fees.[4] This is because they either could have not taken out a loan at all, or kept searching for a loan across other lenders. As long as the cost of studying is low enough, this assures that the benefits of studying one's loan contract are higher than the costs. Since borrowers with high levels of financial sophistication are more likely to study contracts, they are also less likely to become delinquent as they will not take out contracts they will regret (consistent with table 12, which shows that delinquency rates for more educated borrowers are lower than those of less educated borrowers). Unexpected fees increase the probability of default once a loan is signed. If the fees or unexpected costs are sufficiently large, the borrower may not have enough income to cover the loan payments with additional fees. While this unexpected shock may not be consequential if it is a small underestimation, SERNAC, Chile's consumer finance agency estimated that fees for credit insurance (which were removed in standardized contracts and are not mandatory for loans) over the life of the loan ranged between less than one and six percent of the cost of the loan (roughly 2% of an average monthly Chilean income) (2012).

Turning to our regulations, increased disclosure makes it easier for consumers to study features of the loan contract, and so decreases the cost of studying for all borrowers. Standardized contracts reduce the benefits to studying as there is now a cap on unexpected additional fees. However, standardized contracts also reduce the risk of default for precisely the same reason. While we have a direct analogy between increased disclosure and Law 20.555, only one contract was standardized under Law 20.448, so these estimates may be a lower bound on the effect of standardized contracts on consumers. Based on this framework, we make four predictions:

Prediction 1: *Improved disclosure will reduce the delinquency rates of borrowers with a low cost of studying.*

Disclosure reduces the costs of studying. Sophisticated consumers who did not study under the fine print regime now have a low enough study cost that they will study the new disclosure. Studying reduces their probability of delinquency, because they will not take out a contract where the fees

[4] Zinman (2014) review evidence that consumers have imperfect expectations of fees.

are larger than expected.[5]

Prediction 2: *Improved disclosure will not affect the delinquency rates of borrowers with a high cost of studying.*

Less sophisticated consumers start with a high cost of studying. Therefore, even if disclosure reduces this cost marginally, the cost may remain so high that they still do not study. Thus disclosure may not reduce study costs enough to affect their probability of delinquency.

Prediction 3: *Standardized contracts will reduce delinquency rates of borrowers with a high cost of studying.*

Since borrowers with a high cost of studying do not study their contracts before they sign them, they are more likely to be surprised with fees or higher than expected monthly payments on their loans. This is because they take out loans where they did not accurately estimate the monthly cost of the loan, or were unable to avoid contingent fees associated with the loan contract. Since standardization lowers the complexity of loan contracts, high study cost borrowers are less likely to incur contingent fees and/or an upper bound is placed on their monthly payment estimation mistakes. This should reduce the probability of delinquency.

Prediction 4: *Standardized contracts have a theoretically ambiguous effect on delinquency rates of borrowers with a low cost of studying.*

Sophisticated consumers already tend to avoid unexpected surprises on most contracts because they are more likely to study them before they sign them. However, because standardization lowers the expected benefit of studying (your unexpected surprises are capped), fewer low cost borrowers may study and subsequently default more. Yet because unexpected loan expenses are capped, fewer borrowers are at risk of default. Our model therefore predicts that standardization has two opposing effects on sophisticated borrowers, since it reduces the probability of delinquency by capping possible expenses while increasing the probability of delinquency by reducing studying.

This framework also provides an important ancillary prediction: these regulations can can

[5]While the simplified model implies that financially sophisticated borrowers may select out of a particular financial contract, they may select into a better contract rather than deciding not to take out a loan. As such, our model is consistent with the fact that there is no selection on aggregate loan volume.

reduce delinquency *even if there is no observed borrower selection.* This is because borrowers may still take out the loans they had planned for and either understand their terms better, or searched across lenders until they received terms they were satisfied with.

Our results are consistent with all four of our predictions. Sophisticated consumers are delinquent less under disclosure (prediction 1), but not standardization (prediction 3). In contrast, unsophisticated consumers default less under standardization (prediction 2), but not disclosure (prediction 4). Neither group benefits from the other regulation. This stylized framework explains why our regression discontinuity results showed disclosure was the primary regulation that helped borrowers avoid delinquency. 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. (because they tended to be more educated than the average borrower) and why the impact of standardization increased as we moved further away from the cutoff.

While our explicit predictions concern delinquency, we can intuitively see how they cohere with our results about money on the table. Unsophisticated borrowers do not seem to incorporate the regulations into their rate shopping behaviour to obtain lower rates. This is consistent with our prediction that unsophisticated borrowers benefit primarily from standardization (which helps them avoid costly mistakes) rather than disclosure (which they cannot practically use, because their study costs are still too high). In contrast, we predict that sophisticated borrowers will use disclosure to better understand their loans. This likely improves their search and bargaining position and thus reduces price dispersion. Consistent with this, the benefits of financial sophistication were greatest under the regulation that expanded disclosure to all products, rather than the regulation that created disclosure for one standardized product.

4 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 4.1). Second, Chile has a unique pair of currencies that we exploit in our primary identification strategy (section 4.2). Third, Chile implemented two natural experiments in 2011 and 2012 that allow us to tease apart the effects of disclosure and standardization (section 4.3). 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 6).

4.1 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 B.1).[6]

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

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

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 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 4.3)—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.

4.2 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 B.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 controlling 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

4.3 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.

4.4 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. We conceive contract standardization to be the absence of unnecessary insurance in the fine print. 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. There were no additional regulations applied to how the lender introduced the universal credit contract, only that they had to provide the Universal Credit contract as an option. This could mean that lenders could price that contract more disadvantageously to the borrower, show within a larger menu of contract choices, steer borrowers into other contracts or employ other methods to make the Universal Credit contracts less appealing.

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. While the listed figures could be included in loan contracts prior to law 20.448, they were not mandatory to state. Prior to the regulation, interest rate would have been available to potential borrowers, but an APR equivalent had not been standardized by the regulator. An example of a Universal Credit loan contract can be seen in Figure 3.

4.5 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.
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> -Biblioteca del Congreso National de Chile 2011

5 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[7]. 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.

5.1 Standardized Contracts versus Disclosure

We conduct a regression discontinuity in three periods: the pre-period before regulations were introduced or announced, the implementation period for law 20.448 to estimate the combined effect of standardization and disclosure, and the implementation period for law 20.555 to estimate the effect of standardization alone.

Additionally, we use the "difference-in-discontinuities" method from Grembi et al. (2016) to attempt to estimate the precise effect of disclosure alone. Specifically, we estimate the following

[7]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.

equation:

$$y_{it} = \beta_1 L_i + S_i^*(\beta_2 + \beta_3 L_i) + T_t[\alpha_0 + \alpha_1 L_i + S_i^*(\alpha_2 + \alpha_3 L_i)] + \gamma_1 X_i + \epsilon_i \tag{2}$$

where S_i^* is our indicator variable for whether the loan had an amount above our UF cutoff, L_i is loan size, and T_t is an indicator variable for if the loan was issued during the Law 20.555 implementation period. This new regression can give us the effect of standardization (α_3), and disclosure ($\beta_2 - \alpha_3$ directly.

5.2 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 7.1.1 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 7.1.2.

6 Data

We use administrative data on the universe of consumer loans from the Chilean banking regulator, the Superintendencia de Bancos e Instituciones Financieras (SBIF).[8] 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

[8]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.

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 mean of almost 30% in 2013.[9] 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 B.3). 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. Figure 1 plots key indicators of the nominal interest rate distribution for consumer loans over time. Over the sample, rates appear to increase and grow more disperse. Figure 2 plots the change in the Chilean consumer price index during the same period, suggesting roughly 20% (6 percentage points) of nominal rates are composed of inflation.

[9]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.

6.1 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 B.1 presents detailed statistics on the discontinuity sample, while table 4 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 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 B.2, 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.

7 Results

7.1 Regression Discontinuity

Our estimates for equation (1) are presented in table 5. As a result of the first regulation, law 20.448, 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 "window dressing": that is, banks may have renegotiated loans that would otherwise default or become delinquent. However, our results suggest that loans above and below the cutoff were not extended differentially, which suggests that these were true improvements rather than window dressing. Raw regression discontinuity results are presented in figure and table B.3. 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, which substantially reduces the noise around the cutoff. The global polynomial regression for if a loan ever becomes delinquent is presented in the Appendix in figures C.1 and C.2.

During the period where disclosure was applied to all loans, we find no significant decrease in default or delinquency for loans issued below versus above the cutoff (table 6). In addition, using the "difference-in-discontinuities" method from Grembi et al. (2016), we find that the effect of the standardized contract remains insignificant, while the coefficient of the combined effect is negative and statistically significant at the 10% level.

While we attribute our results to the effect of standardization and disclosure, it is possible that introducing another product can potentially have competitive affects for the lenders' other available options (see Hausman and Leonard 2002). One might therefore worry that our results are driven by competition. We cannot directly evaluate this hypothesis, since our data does not indicate whether given contract is a Universal Credit contract. Yet various indirect considerations suggests that competition cannot account for our results. 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 B.4 we see that the average price generally went up after the introduction of the law change. Additionally, in section 7.3.1 we find that 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 transparency effects of our regulation rather than the competitive effects of introducing a new product.

We next exploit our discontinuity to examine the timing at which borrowers default, which provides evidence about the mechanisms that drive our results. Haughwout et al. (2008) argue that 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 contrast, if a borrower becomes delinquent later in their loan tenure, this is more likely because of liquidity or income shocks. We therefore predict that borrowers below the cutoff should become delinquent later than those above the cutoff. We first use a regression discontinuity to evaluate whether loans that become delinquent below the cutoff do so earlier than loans that become delinquent above the cutoff. Column 1 of table 8 indicates that there is no significant difference, which is unsurprising, given that this regression is restricted to the 110 loans around the cutoff that become delinquent.

To avoid this problem, we use a Cox proportional hazard model that allows us to include the full regression discontinuity sample of borrowers. This allows us to exploit the richness of our bi-monthly payment data in order to obtain more precise estimates of the timing of delinquency before 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 9 and figure 7. 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 7, 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. Recall that borrowers who default early in the life of the loan typically do so because the loan ill-matched their financial situation (citehaughwout2008juvenile). Given that the reduction in delinquency rates happens within the first year of the loan term, this suggests that Law 20.448 helps borrows to understand and better match with their loan terms.

7.1.1 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. The standard way to test for selection around the discontinuity is to examine whether there is bunching in the distribution of loan size around the cutoff. Chile's unique currencies give us reasons to believe that such bunching does not occur (see section 4.2). 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 8 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 8e. The percentage change in the log distribution is measured at 55% with a standard deviation of 23%, 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.

7.1.2 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.

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 show that even this population benefits from these measures. However, borrowers who take out loans in the right tail for size are likely to be financially sophisticated. We therefore cannot use a standard regression discontinuity method to determine whether standardization helps less sophisticated borrowers. We will use two other methods to address those borrowers. First, we use a recent econometric technique to estimate treatment effects away from a regression discontinuity cutoff (section 7.2). Second, we conduct a difference-in-differences analysis to test whether standardization and increased disclosure have heterogeneous impacts on financially sophisticated and unsophisticated borrowers (section 7.3).

[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 10). 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.

7.2 Measuring Treatment Effects Away from the Cutoff

We use a recent method from Angrist and Rokkanen (2015) to identify treatment effects that are not localized around our cutoff. This paper develops a method called "conditional independence estimation" to measure the effect of a treatment on agents further away from the original regression discontinuity cutoff. The method posits that if the running variable is conditionally independent from the outcome variable above and below the cutoff, then its only relevance to the outcome is its assignment of treatment status. We can then either re-weight or propensity match observations further above and below the cutoff based on observables to get a less local treatment effect for borrowers.

To illustrate, we use the example presented in Abdulkadiroğlu et al. (2014). Abdulkadiroğlu et al. (2014) estimate the effect of elite exam schools on students that are just above and below the cutoffs for the exams that determine admission to these schools. They find that these students do not benefit from access to these elite schools in either 10th grade test scores or postsecondary outcomes. However, that does not necessarily imply that these students do not add value to students. Indeed, this is what Angrist and Rokkanen (2015) aim to show with their modified estimation method. While a student just below the cutoff is still a fairly elite student, a student *further away* from the exam cutoff may benefit from a selective school. However, in order to preserve the identifying assumptions for a regression discontinuity, these students who are further away from the cutoff must be similar in their observables (i.e. prior test scores in grades 6 and 7) to the inframarginal *qualified* students whose exam scores are just below the cutoff. By matching students just below the cutoff based on exam scores with those further below the cutoff *with the same observables* and comparing them to students above the cutoff, one can obtain an estimate of the effect of elite schools on these inframarginal but unqualified applicants.

In order to use this method, we have to confirm that the running variable (loan size) is conditionally independent of default above and below the regression discontinuity cutoff (i.e. loan size only matters to default *beyond* other covariates only because it determines treatment status). We confirm the conditional independence assumption, i.e. that delinquency status is uncorrelated with loan size separately above and below the 1,000 UF cutoff in table B.4. The assumption holds in both the period of implementation for law 20.448 (where we measure the joint effect of standardization and disclosure) and in the implementation period of law 20.555 (when we can measure standardization separately). Next, we implement the mechanism to determine the 'CIA' estimate. Effectively, we propensity match borrowers just below the cutoff with borrowers that have a loan size at least 100 UF smaller than that of the cutoff with the same observables (i.e. interest rate, maturity, credit risk, income, and age). Table 11 shows the difference between the beta estimated by the conditional independence procedure and our regression discontinuity coefficient. From columns one and two we see that the regression discontinuity coefficient of the combined effect of standardization and disclosure is just as large over 100 UF away from the cutoff as within our bandwidth. This suggests that the effect of disclosure is not localized around the cutoff.

However, in columns three and four, we see that the effect of standardization is larger away from the cutoff. While these results do not confirm or deny any of our specific empirical predictions, it does suggest that standardization of loan contracts may be more important than what the pure regression discontinuity results suggest. For this reason, we attempt to provide a further analysis of the effects of the regulations for different borrowers in the next section.

7.3 Financial Sophistication: Results

We now investigate the heterogeneous impact of standardization and disclosure regulations on borrowers with different levels of financial sophistication. Our administrative data does not contain questionnaire-based measures of financial sophistication. However, Ru and Schoar (2017) and Lusardi and Mitchell (2007) show that financial literacy is strongly related to education. Furthermore, Montoya et al. (2017) find that more educated borrowers do receive better interest rates as a result of Law 20.555. We therefore use average years of schooling by neighbourhood (comuna) as a proxy for financial sophistication. This allows us to capture spillover effects of education: even if the borrowers themselves are less financially sophisticated, their spouse, neighbour, family member, etc. may be more financially experienced and can help guide them through the loan process.[11] Furthermore, table B.5 shows that average comuna education is a reasonable proxy for individual-level education for a sample of roughly 600,000 individuals. This is likely because average comuna education is sufficiently granular to capture individual differences in education as there are 346

[11]Average comuna education is also correlated with other socioeconomic status indicators such as wealth and familial connections. We believe unobservables are likely correlated with how financially sophisticated a borrower is likely to be, thus enhancing the spillover effects of neighborhood on financial sophistication.

comunas in Chile with a median population of 16,676 residents.

Our sample contains all consumer loans with less than three years maturity and less than 1,000 UF between 2009 and 2012 (for a total of 739,317 loans). We merge this sample with census information on average years of schooling of people between the ages of 30 and 59 as of 2016. Using this data, we divide our sample into loans from 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 more than 12 years of school (at least some university). Table 12 shows the number of loans in each of these groups across our sample period.

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

$$y_{it} = \sum_{t(i)=-6}^{14} \left[\alpha_{\tau+t(i)} + \beta_{\tau+t(i)} \times \mathbb{1}_{\{EDU_i\}} \right] + \gamma X_{it} + \epsilon_{it} \tag{3}$$

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 and loan characteristics could change endogenously as a result of these regulations. We therefore evaluate borrower and loan characteristics separately to determine whether there is selection on these variables. While we found no evidence of selection on observable characteristics in our regression discontinuity sample, that could be because our discontinuity sample is relatively small and composed of highly sophisticated consumers.

For the parameters in regression 3 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 10 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 11, there are no discernible pre-trends between the control and sophisticated treatment group. Figure B.5 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.

Figures 10 and 11 show the estimates of equation (3) 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 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.

Figures 12, B.7, and B.8 document borrower and loan characteristics during the two implementation periods. While we do see changes in these variables that could be due to selection of borrowers, it is unclear if borrower selection on observables accounts for our results on default and delinquency. For the case of unsophisticated borrowers, their credit risk and outstanding debt provide evidence against borrower selection, while their received interest rates provide supportive evidence that borrower selection or the reduction in rate could explain the reduced rates in delinquency. For sophisticated borrowers, credit risk, interest rate risk, outstanding debt all provide evidence *against* borrower selection explaining the effect of each of the regulations. While we cannot rule out selection on unobservables to explain our results, the patterns in borrower and loan characteristics suggest that something other than selection on whether a borrower takes out a loan may be at play to reconcile the results we see for both populations. It is for this reason that we suspect the regulations may have protected borrowers on loans they were going to take out regardless rather than influenced whether or not they took out a loan.

7.3.1 Money on the Table

While our previous regression and difference-in-differences results suggest that standardization and disclosure help borrowers sort into more suitable loans, we have not yet said whether this means that borrowers made better choices while shopping for loans. Our companion paper answers this question by estimating the change in search costs as a result of disclosure (Truffa et al. 2018).

Here, we provide suggestive evidence that whether borrowers made better choices depended on their financial sophistication.

To assess whether borrowers leave less "money on the table", we compare observably similar borrowers. We do not examine aggregate statistics on borrower choice, which cannot distinguish cases that change the composition of borrowers and products from cases where similar borrowers make better choices. We instead keep borrower and product characteristics constant, and use dispersion in rates as a proxy for money on the table. We can do this because price dispersion is a sufficient statistic for search costs (Hong and Shum 2006). One can conceptualize our dispersion measures as estimates of distance between the borrower's actual interest rate and the "ideal" rate they might have received if they had greater bargaining power or searched longer.

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, gender (binary), marital status (binary), and income bins based on tax brackets (622,850, 1,384,110, 2,306,850, 3,229,590, 4,152,330, and over 5,536,440 Chilean pesos) (PWC, 2017). We also create ten year age bins starting at age 18.

To ensure that we compare borrowers obtaining similar products, we cut the product space on two dimensions: 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 million 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×product cells with less than 5 borrowers.

Table 13 presents aggregate summary statistics on income rate dispersion in the pre-period and under laws 20.448 and 20.555. We use three measures of interest rate dispersion: the actual rate minus the 25th percentile rate, the actual rate minus the minimum rate, and the standard deviation within a bin. In aggregate, all three measures seem to increase over time. However, based on our difference-in-differences results, we predict that price dispersion may be heterogeneous across financially sophisticated and unsophisticated consumers.

We regress our measure of rate dispersion on financial sophistication. We control for borrower characteristics (female, married, urban, income, credit risk, and age), macroeconomic variables (interbank rate and expected inflation rate between UF and pesos), and includes year fixed effects. With this strategy we hope to understand the effect of financial sophistication within borrower×product cells rather than across cells. We then restrict our sample to financially sophisticated and unsophisticated borrowers, as detailed in our difference-in-difference analyses (section 7.3).

Across all periods, financial sophistication reduced distance from the 25th percentile rate by 0.5 percentage points, distance from the minimum rate by 5.7 percentage points, and standard deviation within a bin by 1.3 percentage points. However, the advantages of financial sophistication increased in both regulations. After consumers were presented with one standardized product with increased disclosure, financial sophistication reduced distance from the 25th percentile rate by an additional 1.5 percentage points, distance from the minimum rate by an additional 2 percentage points, and standard deviation within a bin by an additional 0.4 percentage points. After disclosure was applied to all loans, financial sophistication reduced distance from the 25th percentile rate by an additional 2.5 percentage points, distance from the minimum rate by an additional 3.8 percentage points, and standard deviation within a bin by an additional 1.0 percentage points. In contrast, less sophisticated borrowers actually received *higher* dispersion rates in both regulatory periods: distance from the 25th percentile rate increased by 0.9 and 3.1 percentage points, distance from the minimum rate increased by 0.8 percentage points and 4.1 percentage points, and dispersion increased by 0.4 and 0.6. In summary, our results suggest that both regulations helped financially sophisticated borrowers leave less money on the table, even though unsophisticated borrowers left more.

8 Conclusion

We propose a theoretical framework based on Heidhues et al. (2018) to explain how standardized contracts and disclosure affect different types of borrowers. All consumers must pay a cost to study financial contracts: doing so takes time, effort, and training. Yet this cost differs depending on one's level of financial sophistication. Financially sophisticated consumers have relatively low study costs. Even so, the cost of studying pages of fine print may be so high that they choose not to study at all. Disclosure reduces study costs, which can lead financially sophisticated consumers to study their contracts and make better choices. Even after disclosure, however, the cost of studying unfamiliar technical material may be too high for unsophisticated borrowers. Instead, less sophisticated borrowers are likely to benefit from standardized contracts, which put a cap on costly surprises.

We find that the introduction of standardized contracts and disclosure regulation 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 using "differences-in-discontinuities" (Grembi et al. 2016). We find that standardized contracts did not have a statistically significant effect around the cutoff, but using the methodology of Angrist and Rokkanen (2015), find that the effects of standardized contracts grow the smaller the loans are.

Using a difference-in-differences we find that financially sophisticated borrowers reduce their delinquency rates by 10 percentage points relative to control borrowers under the disclosure regime. Financially unsophisticated borrowers reduce their delinquency rates by a similar margin when they have access to standardized contracts. Financially sophisticated borrowers do not seem to benefit from standardization regulations and financially unsophisticated borrowers do not seem to benefit from disclosure regulations. The fact that borrower selection provides an incomplete explanation of our difference-in-differences results suggests that these regulations helped match borrowers with more appropriate loans. Using a "money on the table" approach similar to that of Argyle et al. (2017) and Atal (2016), we find that financially sophisticated borrowers left less money on the table. This is true on average and across both regulatory regimes, but especially so under the disclosure regime. In contrast, less financially sophisticated borrowers left more money on the table in all periods, even those when their delinquency rates fell. This suggests that financially sophisticated borrowers may have lowered their delinquency rates by searching across or bargaining more aggressively with lenders to achieve better initial loan terms. In contrast, less sophisticated consumers were delinquent less due to their improved comprehension of loan terms rather than better initial loan conditions.

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 who regulators hope to help when enacting legislation to solve informational market failures between lenders and prospective borrowers. For less financially sophisticated borrowers, regulations that restrict pernicious loan features by standardizing contracts reduce delinquency but do not seem to appreciably decrease the prices or price dispersion for similar products for these borrowers. This paper suggests that one-size financial regulation does not seem to fit all borrowers in either empirics or in theory.

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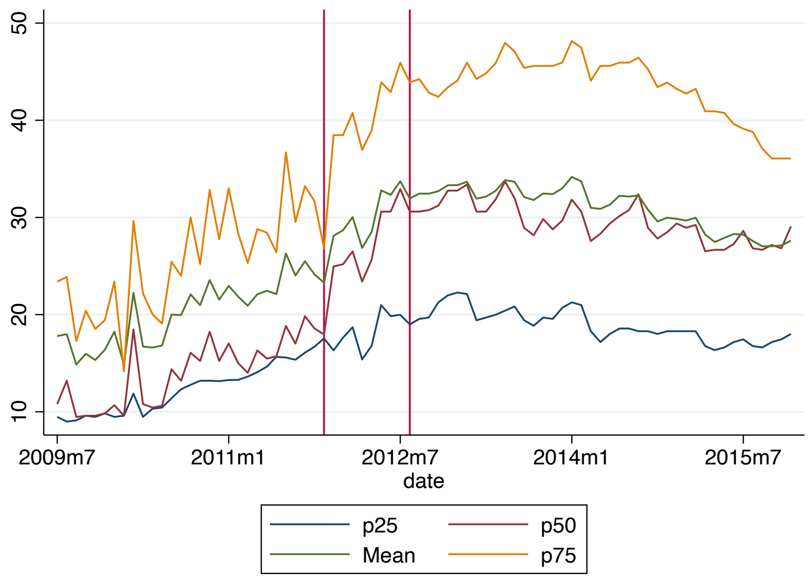
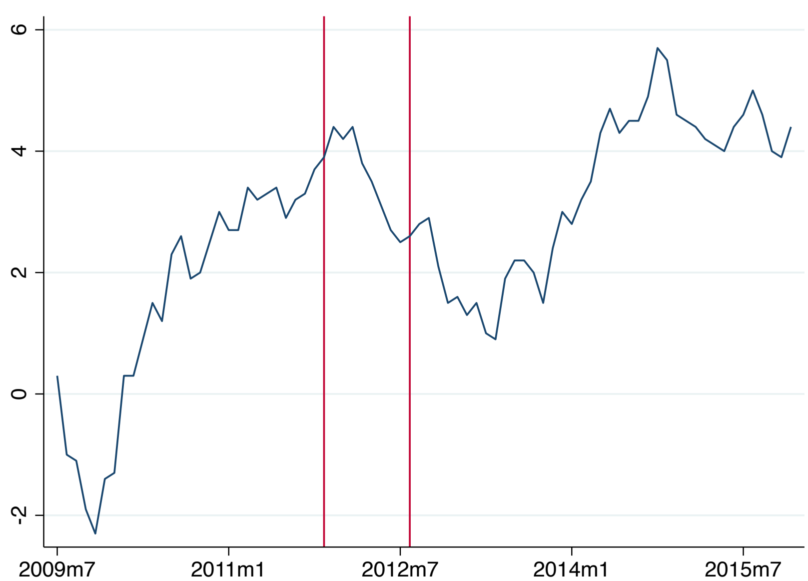
9 Figures and Tables

Figure 1: Consumer Credit Interest Rates 2009-2015



Notes: Nominal interest rate distributional statistics over the sample period. The first red line marks the implementation of law 20.448 in November of 2011 and the second marks the implementation of law 20.555 in August of 2012.

Figure 2: Historical Inflation 2009-2015



Notes: Monthly change in the consumer price index (IPC) from the Banco Central de Chile. The first red line marks the implementation of law 20.448 in November of 2011 and the second marks the implementation of law 20.555 in August of 2012.

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%		Antigüedad	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 Minima $
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	

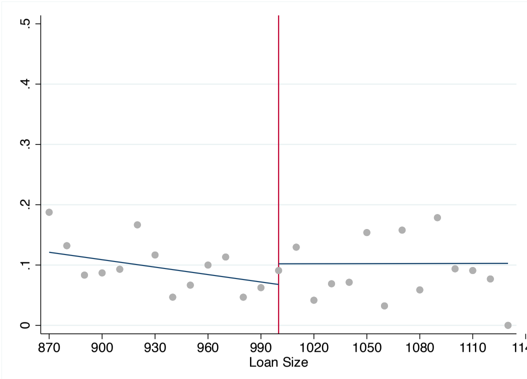
III. Prepayment Conditions	
Prepaid charge (%)	–
Notice period for prepayments	

IV. Late Fees	
Interest on arrears (%)	–
Collection expenses (%)	–

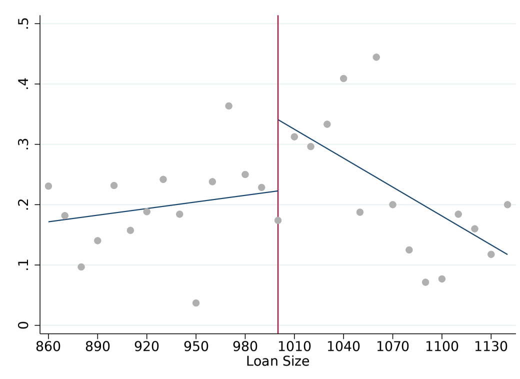
Advisory
"The consumer credit of this summary sheet requires the contracting consumer <name> equity or future income sufficient to pay the total cost of $xx whose monthly payment is $xx, during the entire credit period."

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).

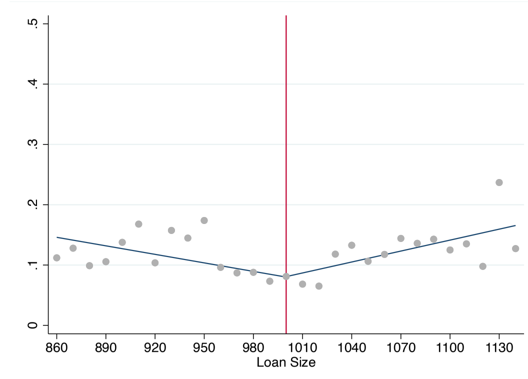
Figure 5: Raw Regression Discontinuties



(a) Pre-period



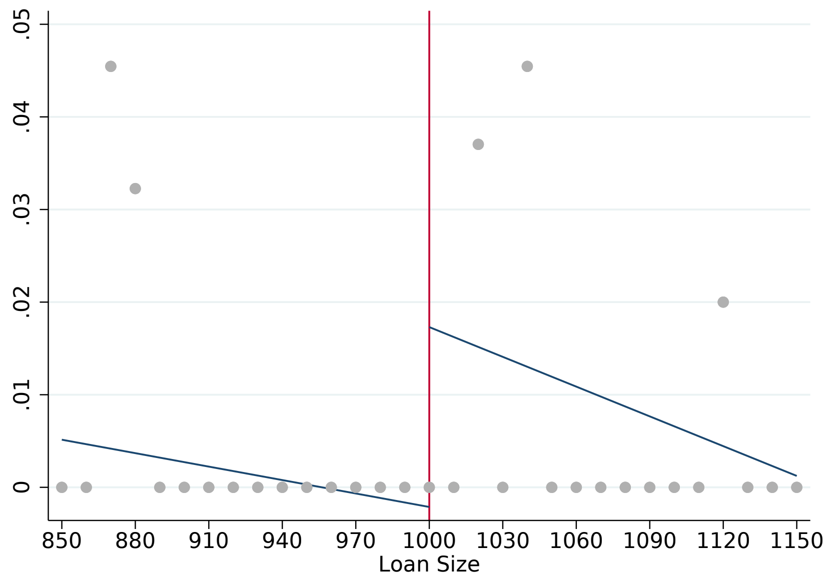
(b) Law 20.448 Implemented



(c) Law 20.55 Implemented

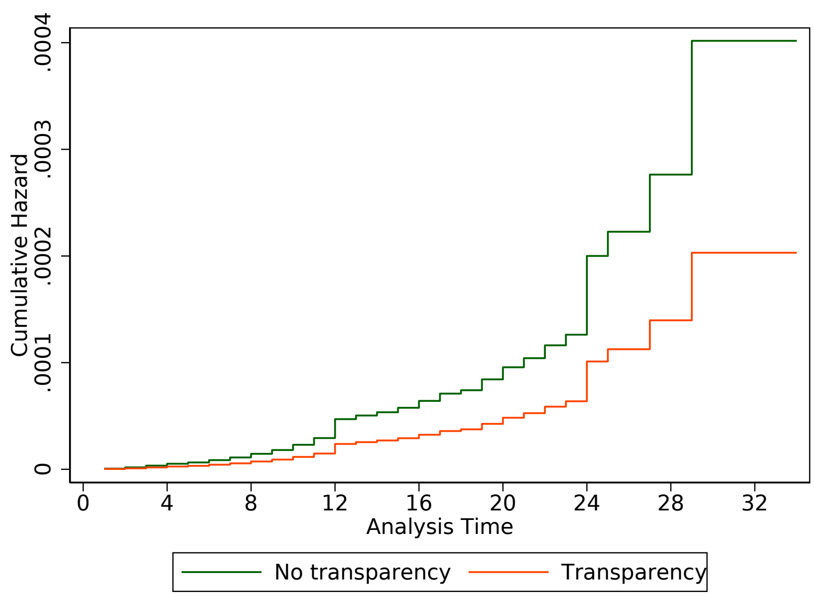
Notes: This figure graphs the linear fit of the raw regression discontinuity of the dependent variable of the borrower ever being delinquent (missing a payment) for the pre-period before any regulatory announcements (January 2009 and December 2010), the regulatory implementation period of law 20.448 (standardized contract and disclosure on one side of the cutoff (November 2011 and July 2012), and the post period between August 2012 to December 2015 (after the introduction of law 20.555). These graphs include no controls. The red line marks the loan cutoff of 1,000 UF.

Figure 6: Raw Regression Discontinuity - Ever Default



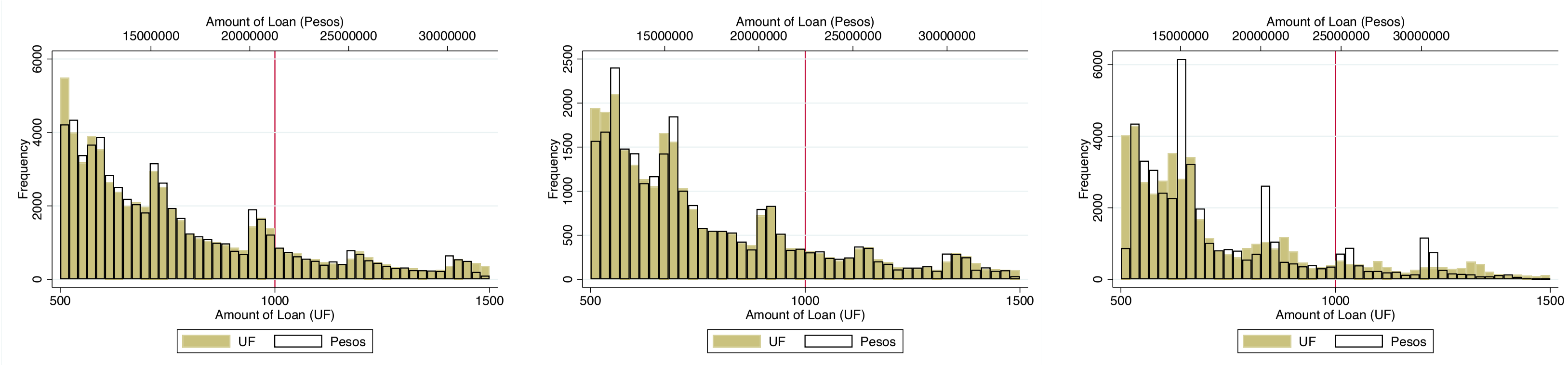
Notes: This figure graphs the linear fit of the raw regression discontinuity of the dependent variable of the borrower ever defaulting (missed three payments and judicial proceedings initiated) in equation (1) with no controls. The red line marks the loan cutoff of 1,000 UF. Confidence intervals are shown at the 95% significance level.

Figure 7: Cox Proportional Hazard Rate Model

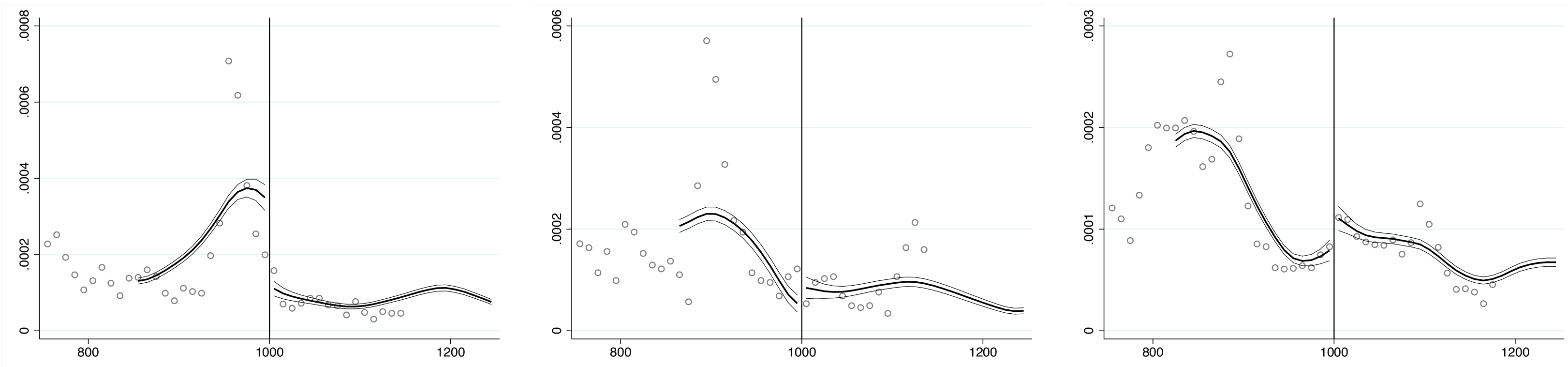


Notes: These figure plots the cumulative probability of being delinquent for borrowers around the regression discontinuity cutoff for the period of implementation for law 20.448. All covariates included in the regression discontinuity regression are included at set at the mean of the regression discontinuity sample, except for the loan size which is set at the cutoff amount. Fixed effects for lender and comuna are also included.

Figure 8: Loan Size Density



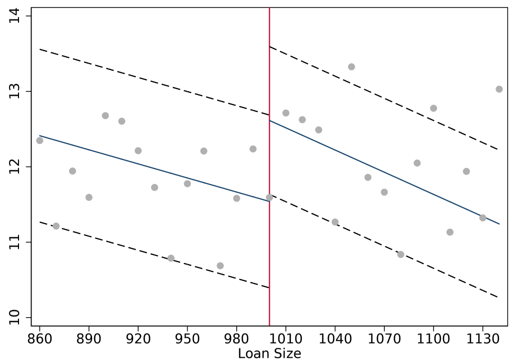
(a) Pre-period (b) Law 20.448 (c) Law 20.555



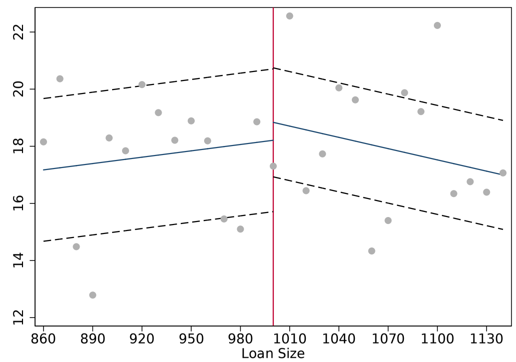
(d) McCrary Density Test: Pre-period (e) McCrary Density Test: Law 20.448 (f) McCrary Density Test: Law 20.555

Notes: The top figures graph the distribution of loan amounts in pesos and UF around the 1,000 UF cutoff in the pre-period before any regulatory announcements (January 2009 and December 2010), the regulatory implementation period of law 20.448 (standardized contract and disclosure on one side of the cutoff (November 2011 and July 2012), and the post period 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 bottom figures show the McCrary density test for loan amounts in UF around the 1,000 UF cutoff for the same periods. The vertical black line is for the 1,000 UF cutoff. Confidence intervals are shown at the 95% significance level.

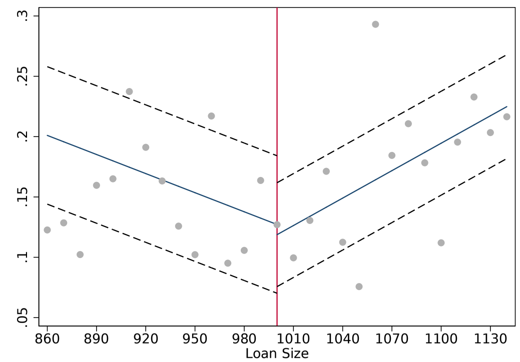
Figure 9: Covariate Balancing Tests



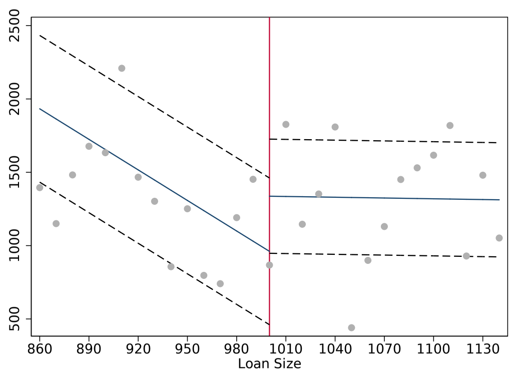
(a) Interest Rate



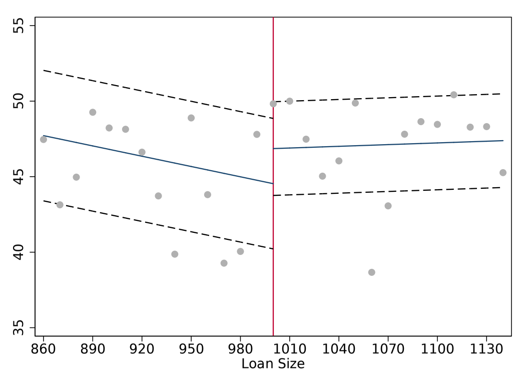
(b) Maturity at Issuance



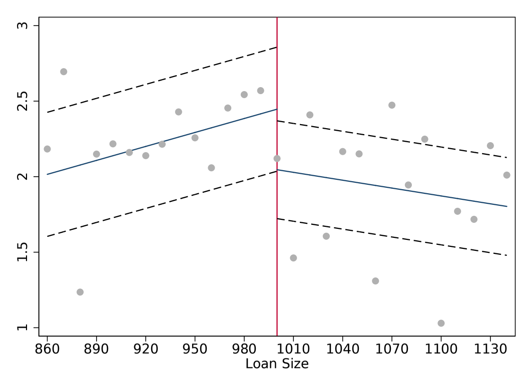
(c) Credit Risk



(d) Annual Income (UF)



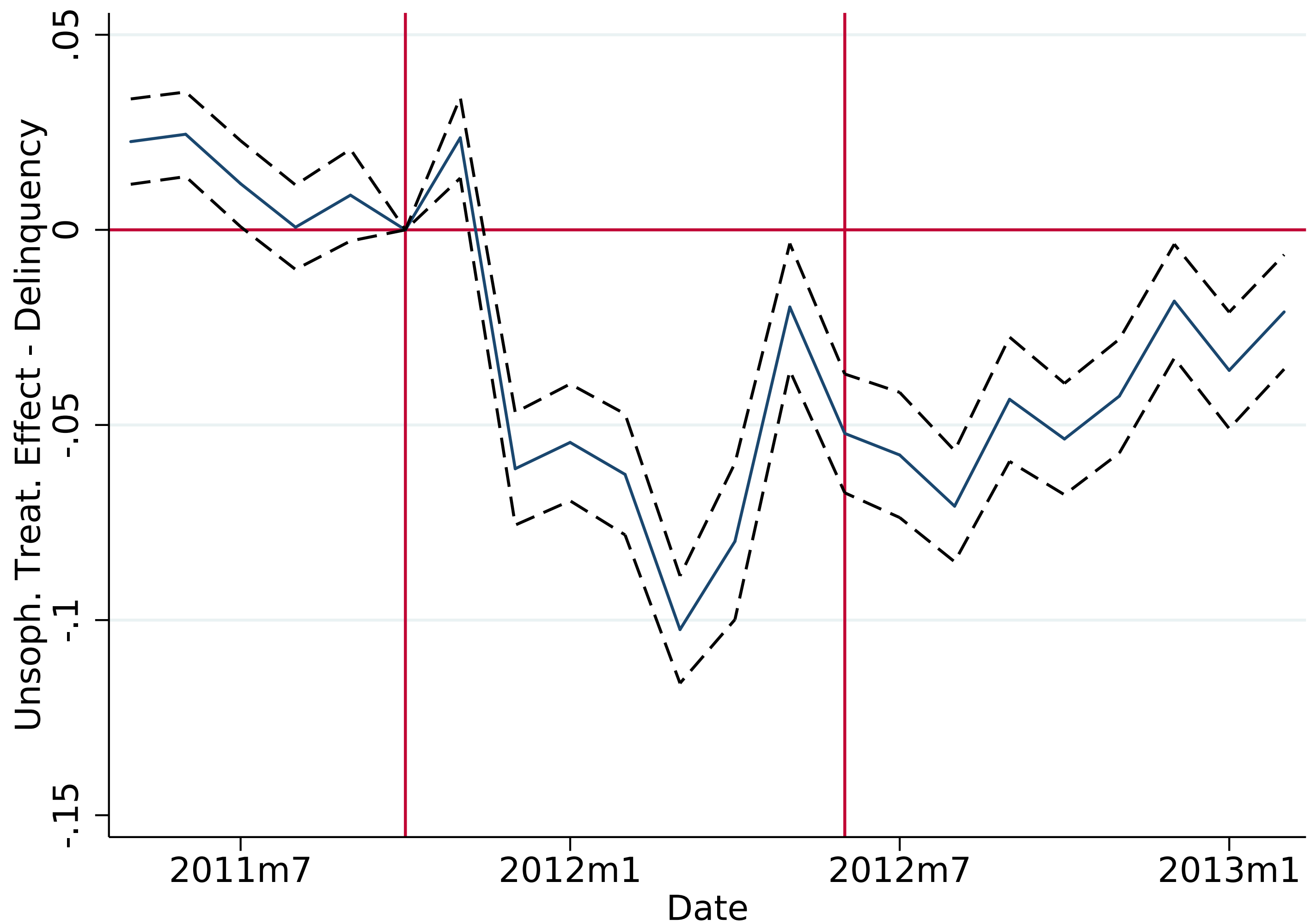
(e) Age



(f) Expected Inflation

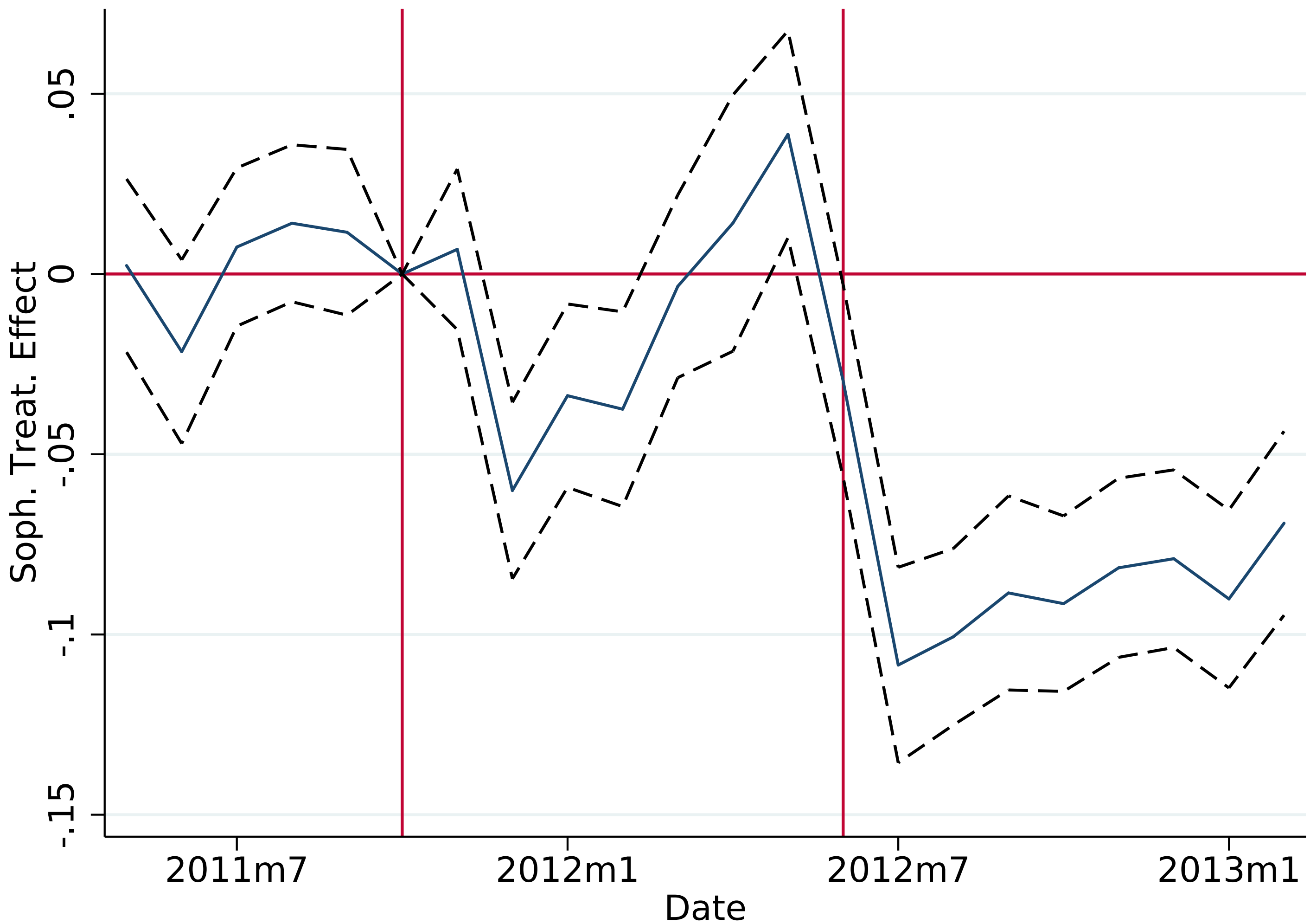
Notes: These figures graph the regression discontinuities in equation (1) for the control variables used in our specification presented in table 5. Observations are between November 2011 and July 2012. Corresponding regression tables for these figures can be found in table 10. The red lines show the 1,000 UF cutoff amount in law 20.448. Confidence intervals are shown at the 95% significance level. Interest rate (panel 9a) is defined as the total interest rate plus associated fees and insurances of the loan (equivalent to APR). Maturity (panel 9b) is defined as the term of the loan in months. Credit Risk (panel 9c) 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 9d) 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 9f) 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 10: Delinquency: Unsophisticated borrowers versus control



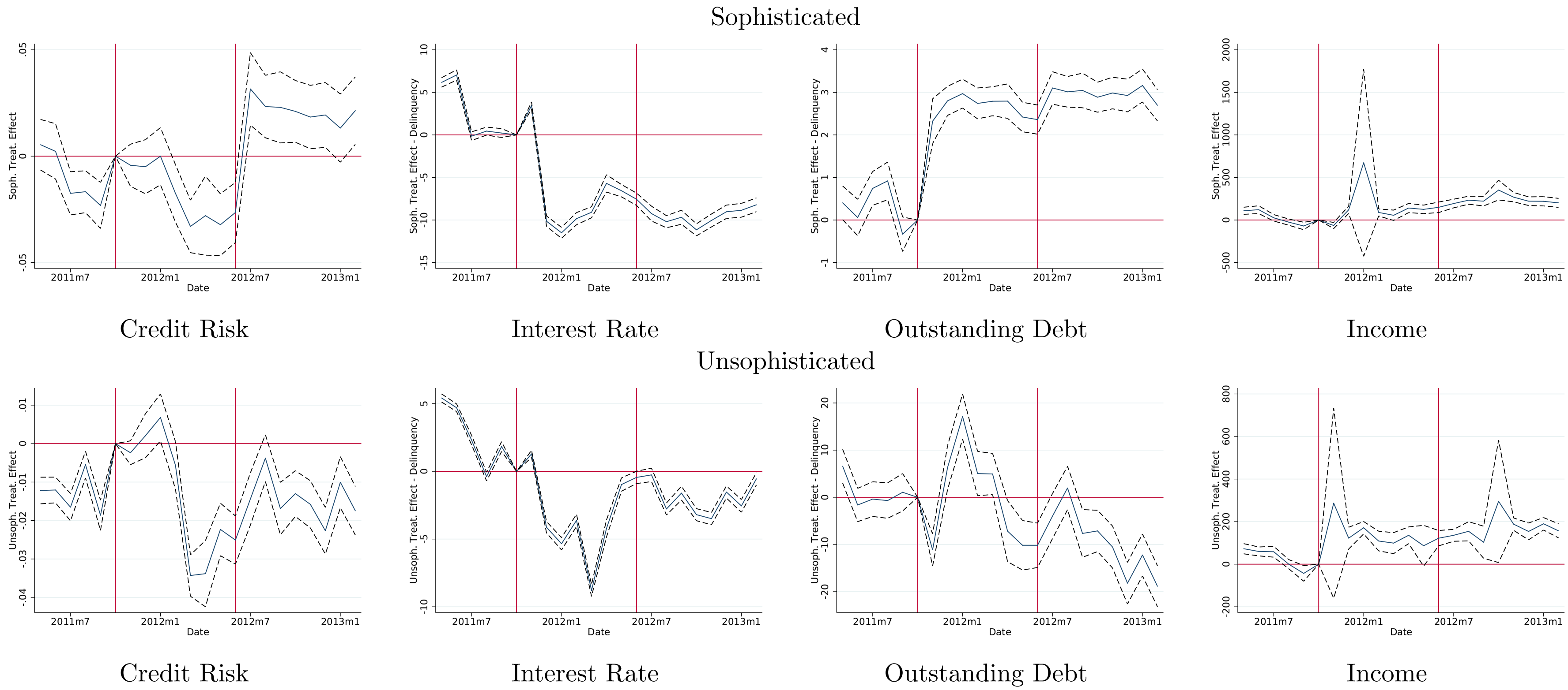
Notes: Estimates of βs from equation(3) for borrowers in neighbourhoods with the average education below 11.5 years of schooling ("unsophisticated") as compared to the control group (11.5 to 12 years of schooling). Loans are collapsed to one data point per observation, and all loans are 2 years maturity 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 11: Delinquency: Sophisticated borrowers versus control



Notes: Estimates of βs from equation(3) 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 12: Borrower Credit Risk



Notes: Estimates of βs from equation(3) 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(3) 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.

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: 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 5: 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 5 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 6: 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 6 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 7: Regression Difference-in-Discontinuity

	(1)
	Ever Defaulted
Standardized Contract and Disclosure	-0.0623*
	(0.0365)
Standarized Contract Only	0.0367
	(0.0415)
Comuna Fixed Effects	Y
Lender Fixed Effects	Y
Bandwidth	172
Kernel	Tri
N	8300

Standard errors in parentheses

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

Notes: Table 7 shows the estimates of equation 2 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). We 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 8: 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 8 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 9: 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 9 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 10: Covariate Balancing Tests

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Interest Rate	Maturity	Credit Risk	Income	Age	Expected Inflation	Inter-bank Rate	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 10 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 11: Conditional Independence Estimates

	Law 20.448 Implementation (1)	(2)	Law 20.555 Implementation (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 11 follows table 3 from Angrist and Rokkanen (2015). Bootstrapped standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 12: 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 13: 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 7.3.1 for details). Dispersion is measured by the difference in interest rate from the 25th percentile rate in the borrower × product bin, the difference in the minimum rate and the standard deviation of rates.

Table 14: Money on the Table: Regression Results

	(1) Rate-25th pctile rate	(2) Rate-minimum rate	(3) 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

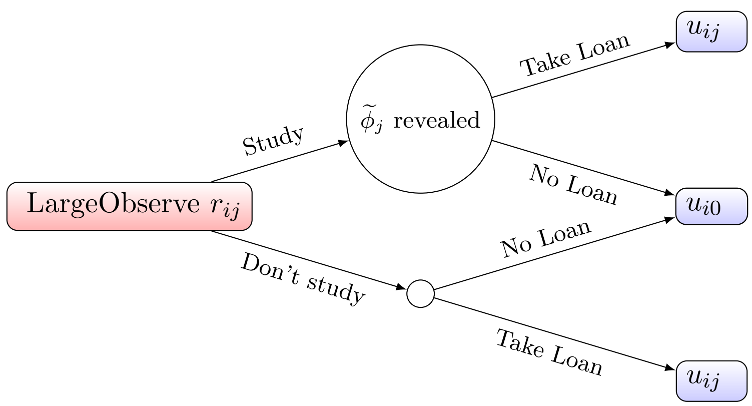
Standard errors in parentheses

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

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 7.3.1. Controls include loan maturity, credit risk, income, sex, if married, whether province, age, inter-bank rate, and expected inflation.

Appendix A 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 extra expenses $\widetilde{\phi}_j$ $[\bar{\phi}, \underline{\phi}]$. 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.[12] The borrower knows γ_i, but not the lender. The borrower knows ϕ_j if and only if they study, but may have an expectation of $\widetilde{\phi}_j$, $E[\widetilde{\phi}_j]$.



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

$u_{ij} = v_i - r_{ij} \times l_{ij} - \mathbb{1}[study_{ij}]c(\gamma_i) - \widetilde{\phi}_j - P\big[m_i - r_{ij} \times l_{ij} - \widetilde{\phi}_j < 0\big]d_i$. The "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).

The fees associated with the fine print affect the borrower in two ways. The first is that it decreases their utility directly because there is an additional term subtracted from the value

[12]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.

[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}.

the borrower obtains from the loan. The second term affects the borrower's utility indirectly through increasing the probability that the borrower will default on their loan payment, that is, the probability that their monthly income m_i is smaller than the fees associated with their loan ($P\big[m_i - r_{ij} \times l_{ij} - \widetilde{\phi}_j < 0\big]$). If the borrower defaults, they suffer a delinquency cost d_i.

If a borrower studies the contract from lender j and learns that $\phi_j > E[\widetilde{\phi}_j]$, we assume that $u_{ij} < u_{i0}$, i.e. they would have preferred not to take out the loan. A borrower therefore chooses to study if and only if the expected value of studying is greater than the expected value of not studying, i.e.

$$\begin{aligned} &E\Big[max\Big\{u_{i0}, v_i - r_{ij} \times l_{ij} - \phi_j - P[m_i - r_{ij} \times l_{ij} - \phi_j < 0]d_i\Big\}\Big] - \\ &max\Big\{u_{i0}, v_i - r_{ij} \times l_{ij} - E[\widetilde{\phi}_j] - P[m_i - r_{ij} \times l_{ij} - E[\widetilde{\phi}_j] < 0]di\Big\} \geq c(\gamma_i) \end{aligned} \tag{4}$$

If we restrict ourselves to cases where borrowers take out loans, equation (4) simplifies to

$$(\phi_j - E[\widetilde{\phi}_j] + P[E[\widetilde{\phi}_j] < m_i - r_{ij} \times l_{ij} < \phi_j]d_i > c(\gamma_i)$$

This means that as long as the costs of not studying (i.e. "surprises" in the amount of unexpected fees) are larger than the costs of studying, borrowers will study their loans.

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

$$P[delinquent|loan] = P[delinquent|nostudy_{ij}, loan] \times P[E[\widetilde{\phi}_j] < m - r_{ij} \times \_ij < \phi_j] \tag{5}$$

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.

A.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.

A.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 (4) 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 (4). 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 become delinquent at rate $P(mi - r_{ij} \times l_{ij} < 0)$.

A.1.2 Standardization

We interpret loan standardization as a truncation of the fee distribution, specifically, $\widetilde{\phi}_j < \phi^S < \bar{\phi} \ \forall j$. While standardizing contract features doesn't eliminate all fees, prohibiting particular clauses in the contracts such as costly insurance lowers the upper bound on what consumers can be charged. We depart from Heidhues, Johnen and Kőszegi (2018), who assume that $\phi^S = 0$, since the borrower may still require sophistication to avoid contingent fees or differential origination fees.

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 because they are more likely to study contracts. Yet because $P(0 < m_i - r_{ij} \times l_{ij} < \widetilde{\phi^s}_j)$ are lower, $P[study = 0]$ increases because the left hand side of (4) is larger. Put informally, sophisticated borrowers are more likely to trust that the standardized contracts have removed contingent and unnecessary fees, which increases their probability of delinquency. Our model therefore predicts that standardization will have an ambiguous effect on sophisticated borrowers, since it reduces the delinquency channel but also reduces the probability that borrowers will study.

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

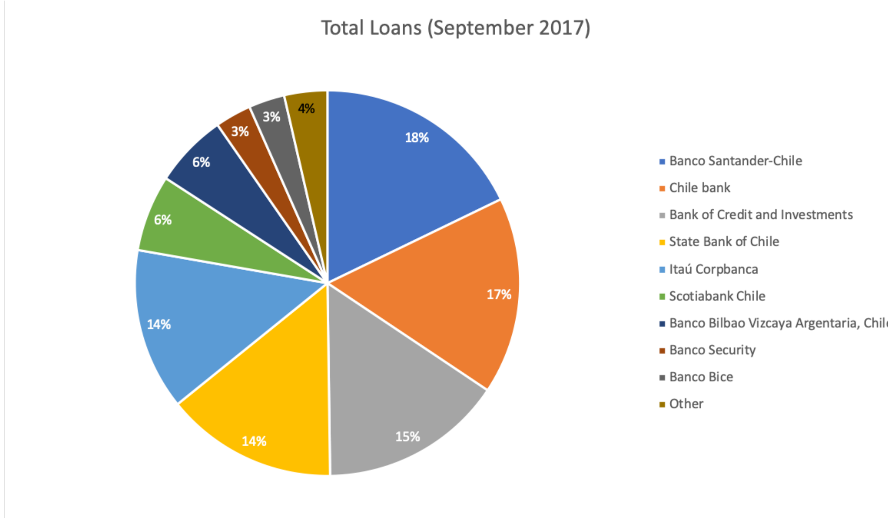
Unsophisticated c_H consumers are more likely to be surprised with fees on many contracts, so if the unexpected fees are capped, they are less likely to default. Furthermore, these consumers have such high study costs that they study under neither the standardization nor the unregulated regimes (that 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 roughly constant. 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.

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.

Appendix B Additional Figures and Tables

B.1 Figures

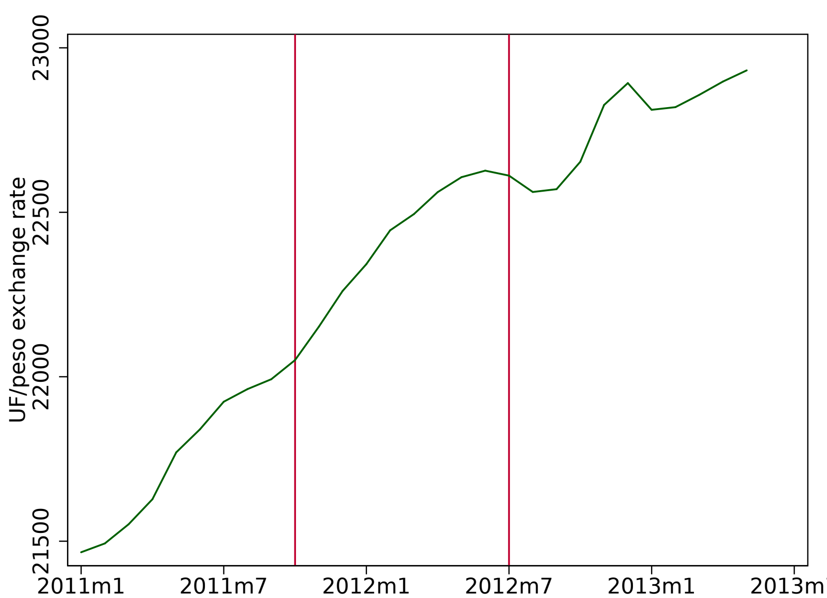
Figure B.1: 2017 Chilean Bank Composition



Source: SBIF

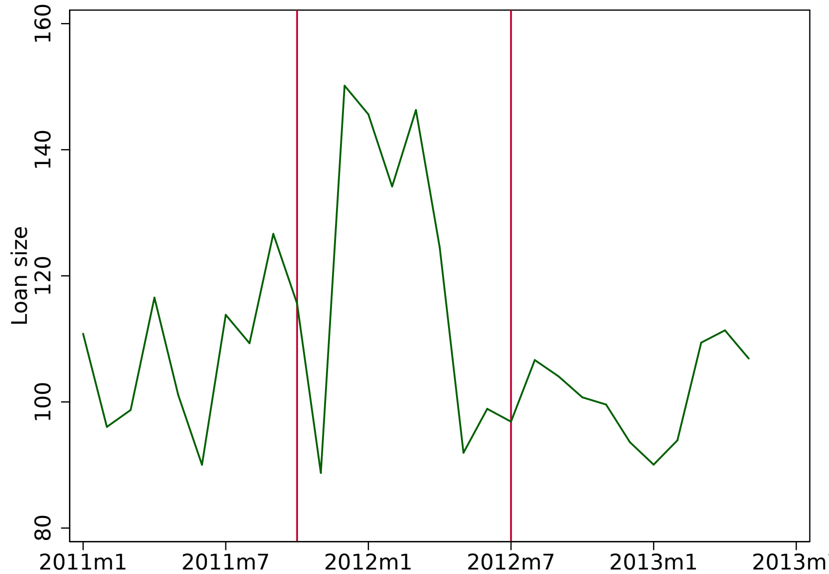
Notes: This figure graphs the market share of total loans across banks in Chile. BancoEstado (State Bank of Chile in yellow) is a state-owned bank that is run as a for-profit entity.

Figure B.2: UF to Peso Exchange Rate



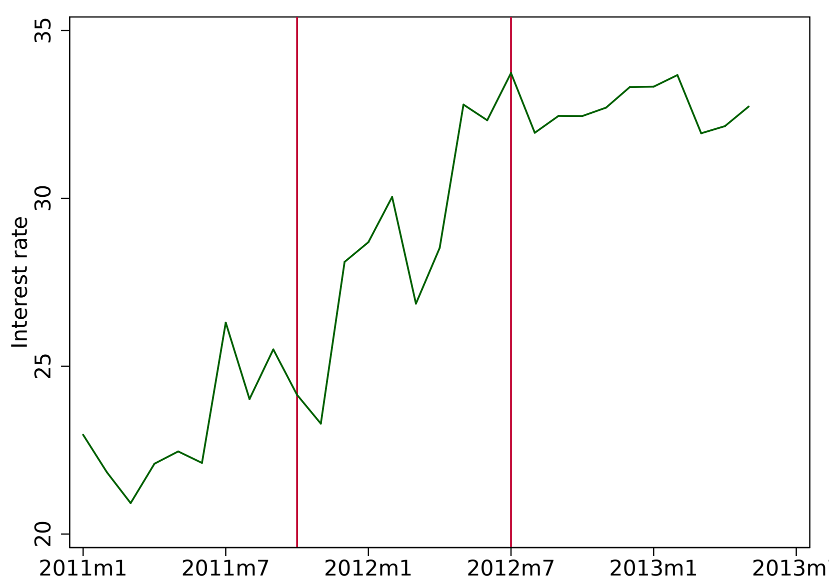
Notes: This figure graphs mean monthly exchange rate of UF to pesos. The first red line is the implementation date of law 20.448 (the introduction of Universal Credit Contracts) and the second red line is the implementation date of law 20.555 (disclosure requirements for all loans).

Figure B.3: Average Loan Size (UF)



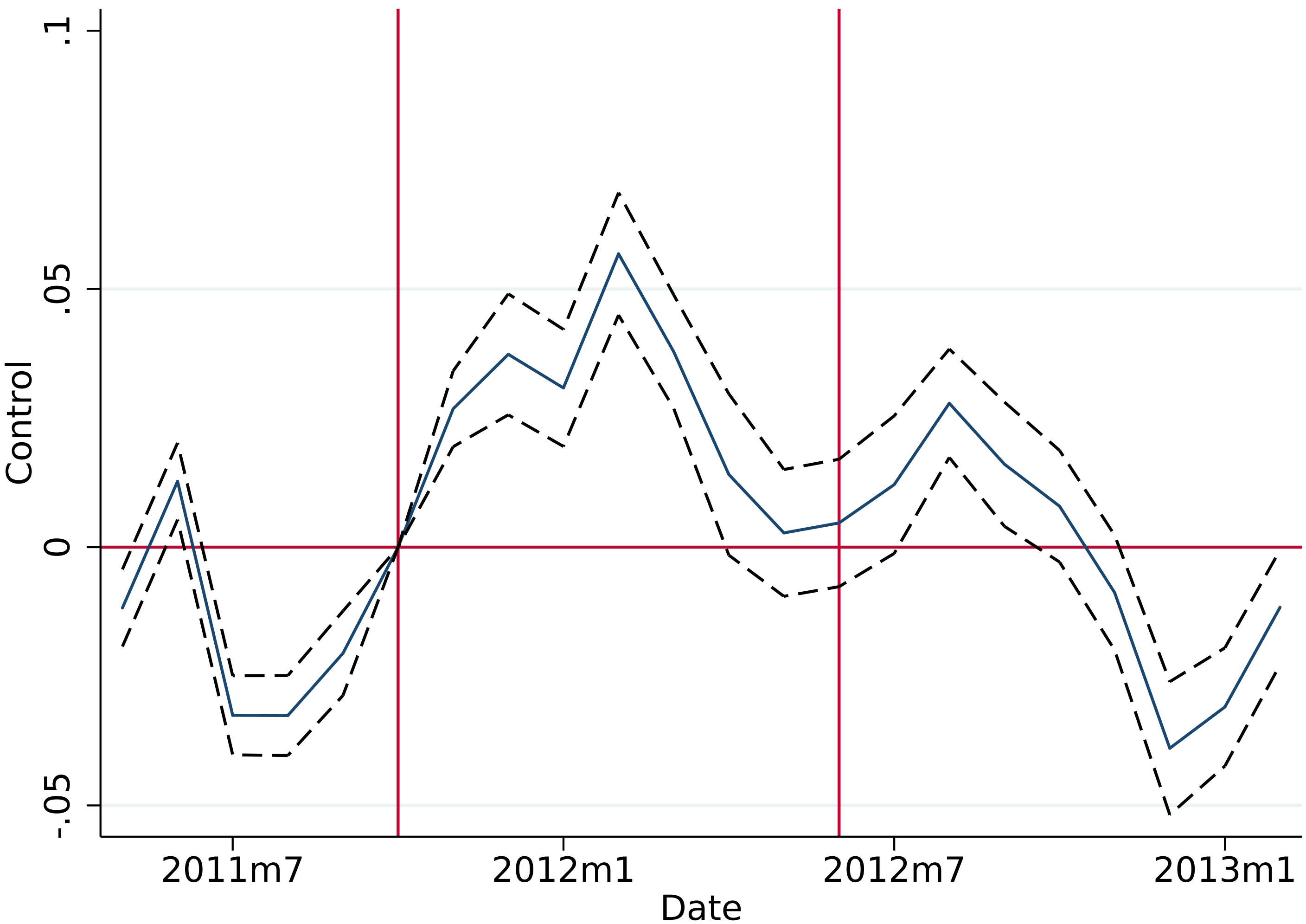
Notes: This figure graphs the unweighted average of loan sizes in UF of newly issued loans by issuance date. The first red line is the implementation date of law 20.448 (the introduction of Universal Credit Contracts) and the second red line is the implementation date of law 20.555 (disclosure requirements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

Figure B.4: Average Nominal Interest Rate



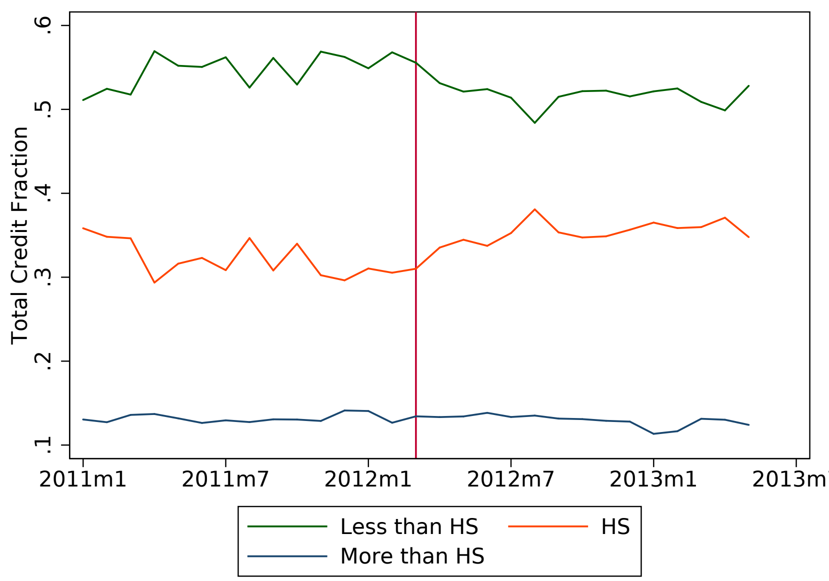
Notes: This figure graphs the unweighted average of nominal interest rates of newly issued loans by issuance date. This rate includes all fees and insurance charges associated with the loan and is equivalent to APR. The first red line is the implementation date of law 20.448 (the introduction of Universal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

Figure B.5: Delinquency: Control Group Time Trends



Notes: Estimates of αs from equation(3) 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.

Figure B.6: 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.7: Other Characteristics - I

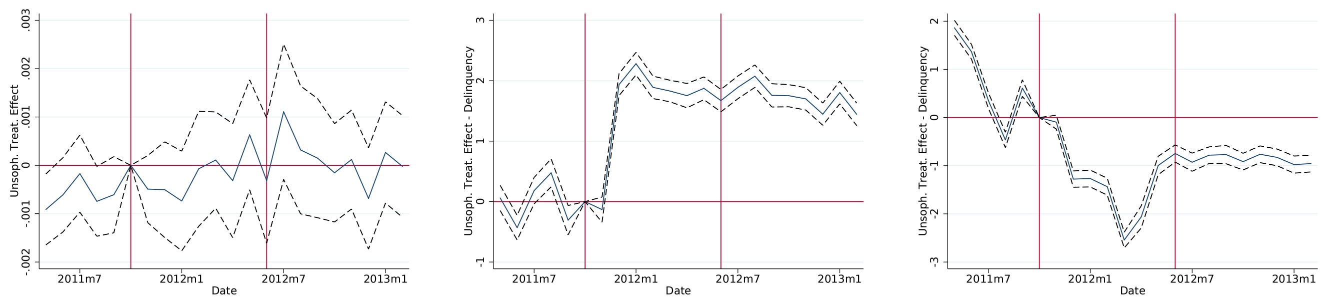
Sophisticated

Ever Default

Outstanding Loans

Maturity at Issue

Unsophisticated



Ever Default

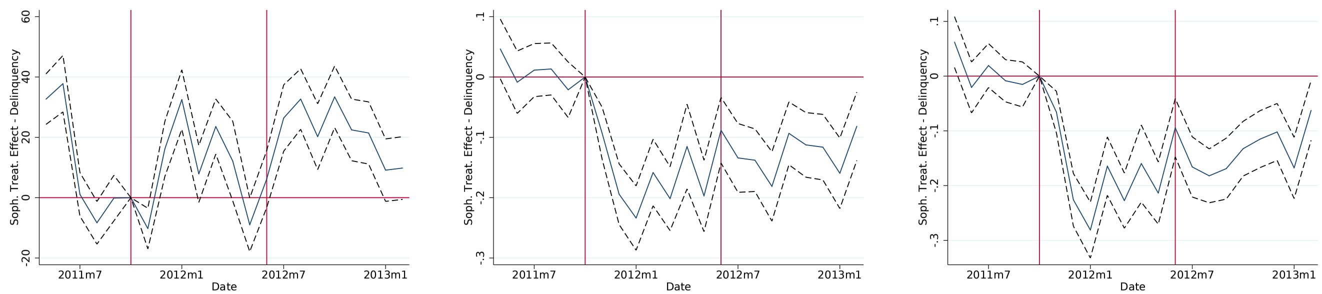
Outstanding Loans

Maturity at Issue

Notes: Estimates of βs from equation(3) 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(3) 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.

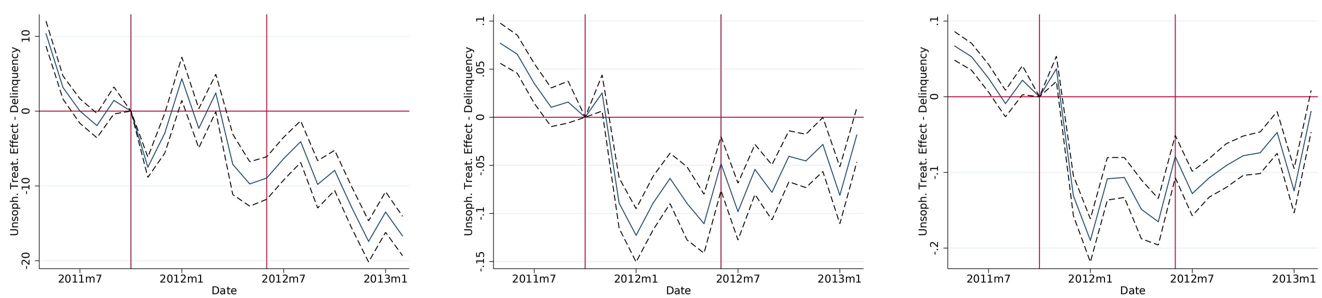
Figure B.8: Other Characteristics - II

Sophisticated



Loan Amount (UF) Switched Banks Switched to New Bank

Unsophisticated



Loan Amount (UF) Switched Banks Switched to New Bank

Notes: Estimates of βs from equation(3) 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(3) 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.

B.2 Tables

Appendix C Robustness Checks

Figures C.2-C.1 shows the global polynomial for delinquency, default, and loan extensions. Table C.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 C.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 8d. 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 C.3 and C.4 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 C.5 and C.6. 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

Table B.1: 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 B.2: 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 B.3: 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 B.3 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 B.4: Conditional Independence Assumption 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 B.4 follows 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 B.5: Correlation: Individual Census Years of Schooling versus Comuna Averages

	(1) Ind. Years of Schooling
Comuna Average Years of Schooling	1.527***
	(0.00521)
Constant	-6.426***
	(0.0562)
F Statistic	85753.1
N	583954

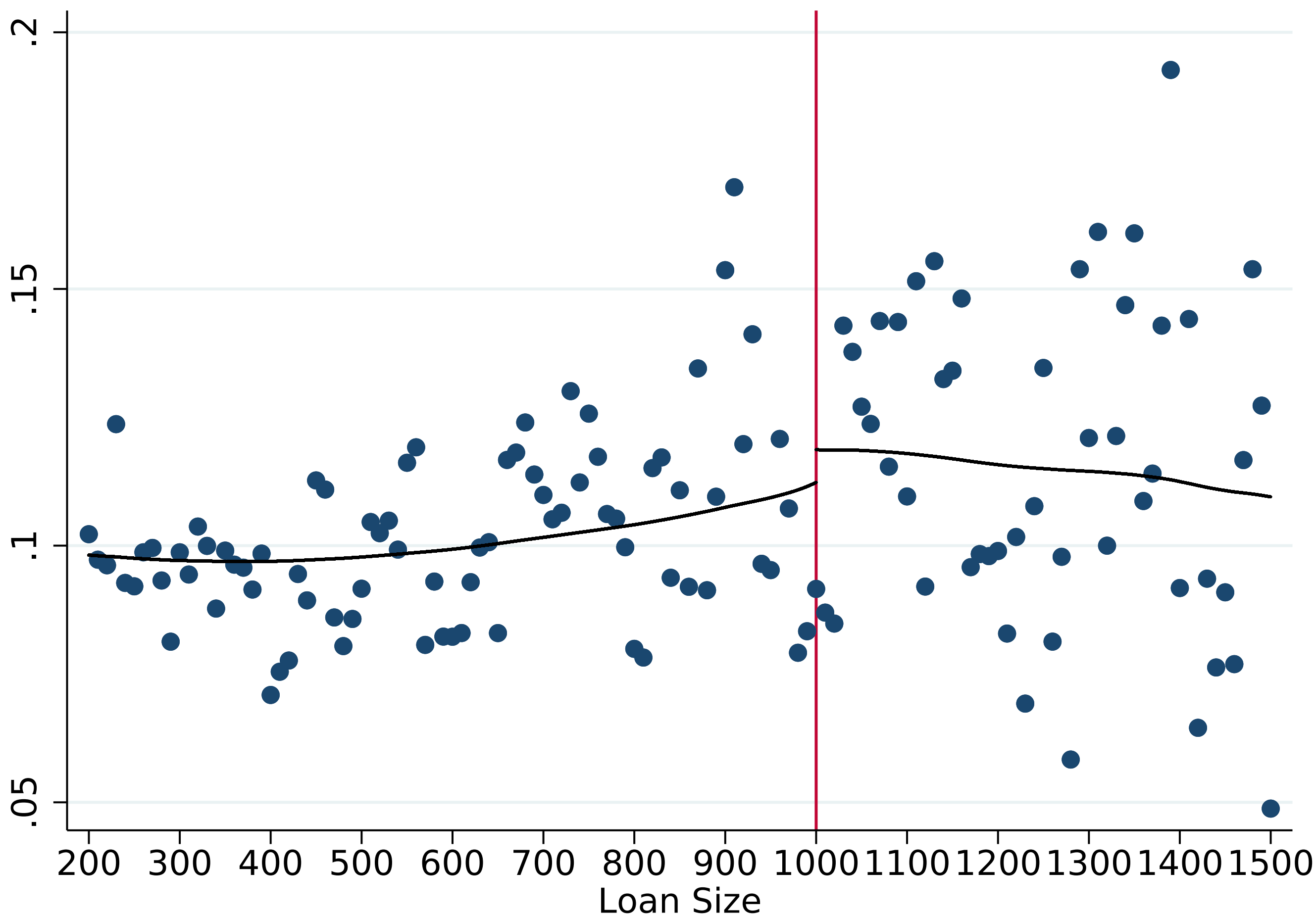
Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Robust standard errors in parentheses. Table B.5 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.

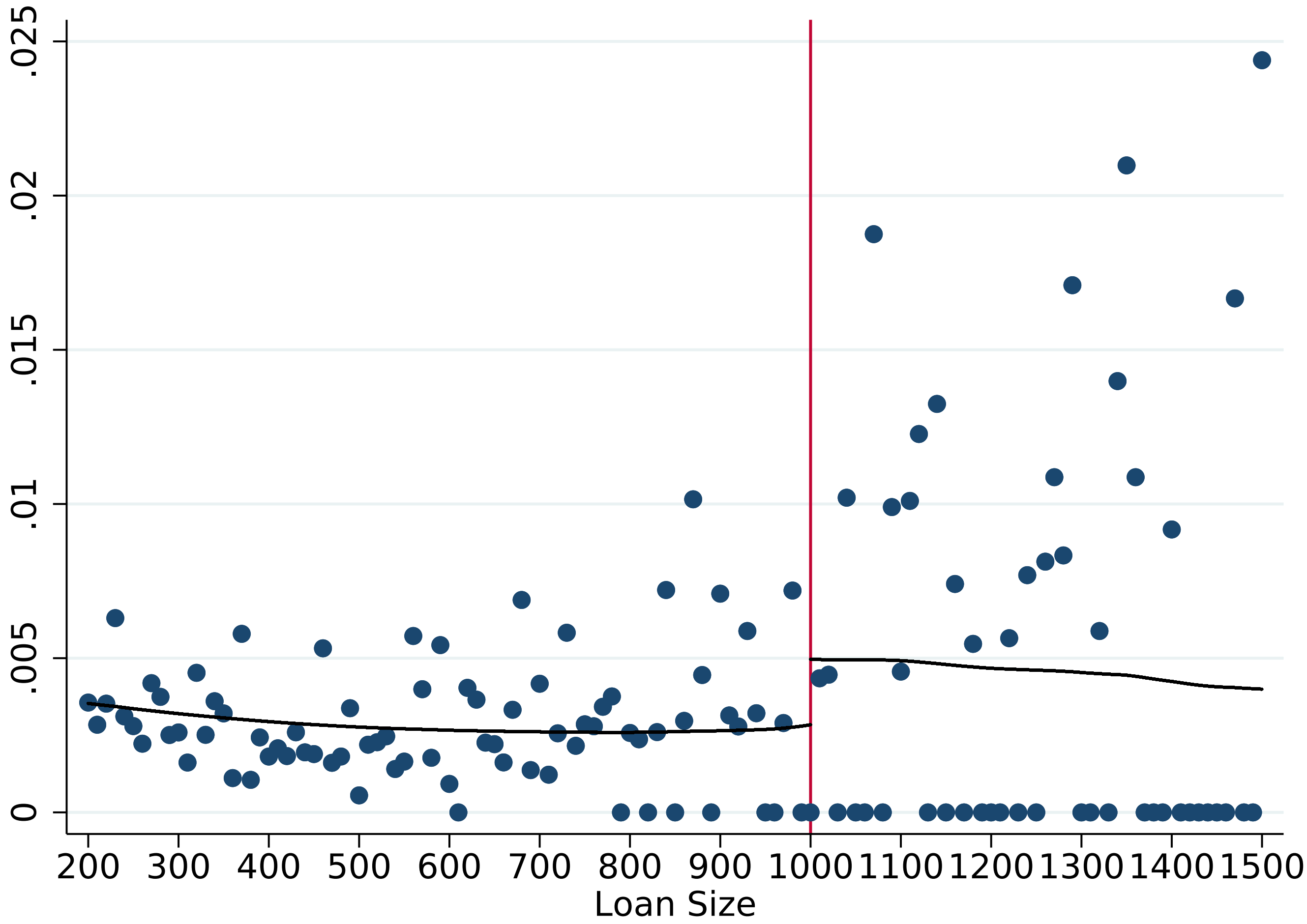
figure C.7 and table C.3. We still find that the discontinuity is significant at the five percent level, though the coefficient decreases to 8 percentage points from from 14.4.

Figure C.1: Global Polynomial - Ever Delinquent



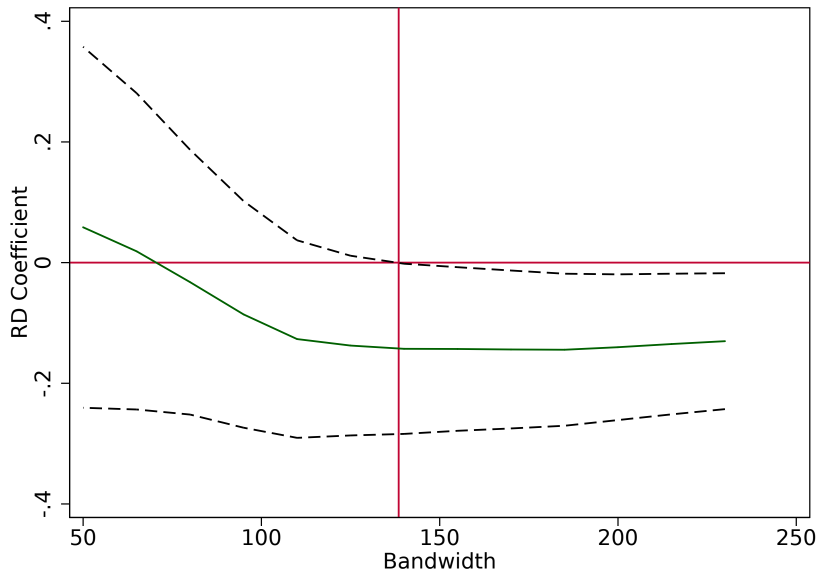
Notes: Global polynomial drawn to best fit ever delinquent rate on each side of the 1,000 UF loan cutoff (red vertical line).

Figure C.2: Global Polynomial - Ever Default



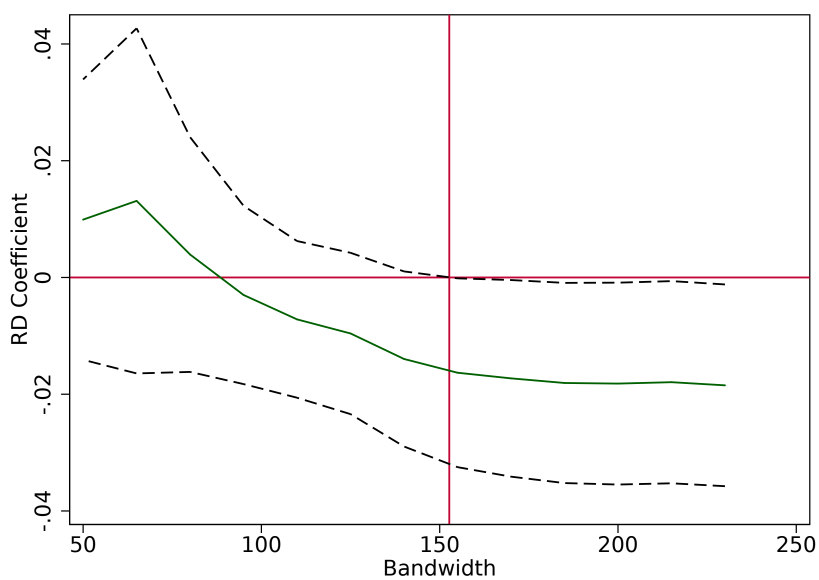
Notes: Global polynomial drawn to best fit ever default rate on each side of the 1,000 UF loan cutoff (red vertical line).

Figure C.3: 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 C.4: 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 C.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 C.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 C.2: Regression Discontinuity, Pre-period

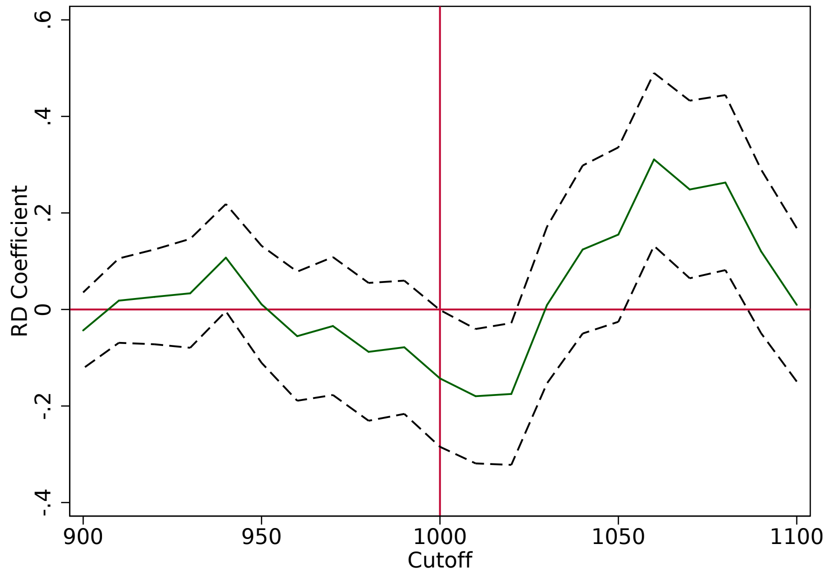
	(1) Ever Defaulted	(2) Ever Delinquent	(3) Ever Extended
Loan Size < 1,000 UF	-0.0502*	0.00630**	0.0102
	(0.0275)	(0.00272)	(0.0158)
Loan Size	-0.0277	0.00934	0.00758
	(0.0397)	(0.00634)	(0.0237)
Transparency X Loan Size	-0.0386	-0.00321	0.00477
	(0.0477)	(0.00728)	(0.0299)
Comuna Fixed Effects	Y	Y	Y
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

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

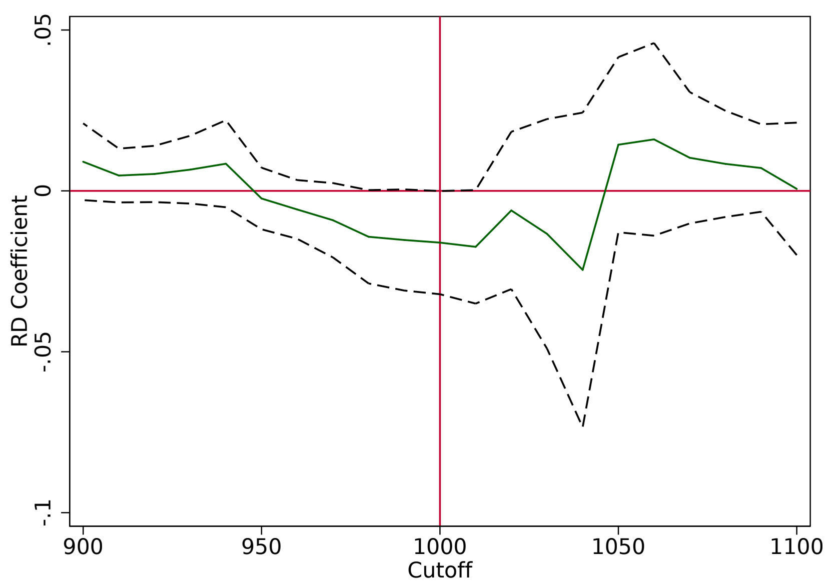
Table C.2 gives the estimated effect of having a loan smaller than 1,000 UF on delinquency, default, and maturity extensions before the regulation was announced (January 2009-October 2011). All estimates are based on regressions that include fixed effects for comunas (neighbourhoods), and lender, as 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 C.5: 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 C.6: 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.

Table C.3: Regression Discontinuity, No Slope

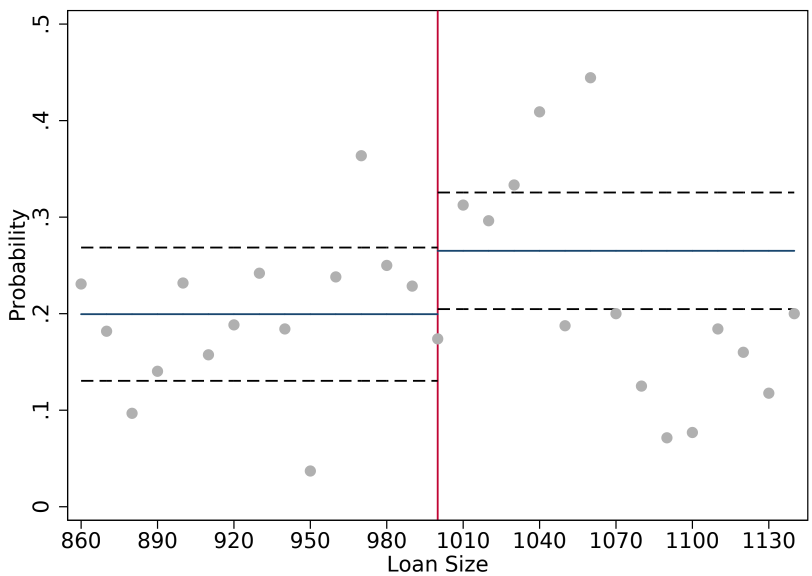
	(1) Ever Defaulted	(2) Ever Delinquent	(3) Ever Extended
Transparency	-0.0802**	-0.00714	-0.00691
	(0.0342)	(0.00512)	(0.0153)
Comuna Fixed Effects	Y	Y	Y
Lender Fixed Effects	Y	Y	Y
Controls	Y	Y	Y
Bandwidth	138	153	131
Kernel	Tri	Tri	Tri
Mean	.265	.011	.03
N	1,088	1,183	1,033

Robust standard errors in parentheses

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

Table C.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 C.7: Ever Delinquent Regression Discontinuity - no slope



Notes: This figure gives a visual representation to the results presented in table C.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 D Difference-in-differences: Other concurrent regulations

We see from figures 10 and B.5 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.

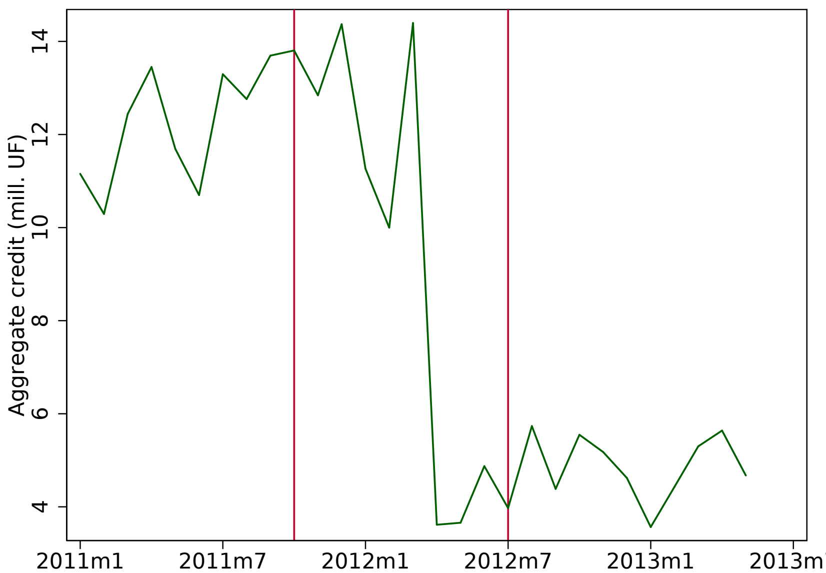
Looking at the evolution of aggregate credit, March 2012 shows a clear restriction in the total amount of credit loaned (figure D.1). However, the restriction in credit access did not substantially change the distribution of credit across education level (figure B.6). Given that banks did not relatively increase their provisions against new loans for either group (figure D.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 12 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 10. 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, figure 12 also shows 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.

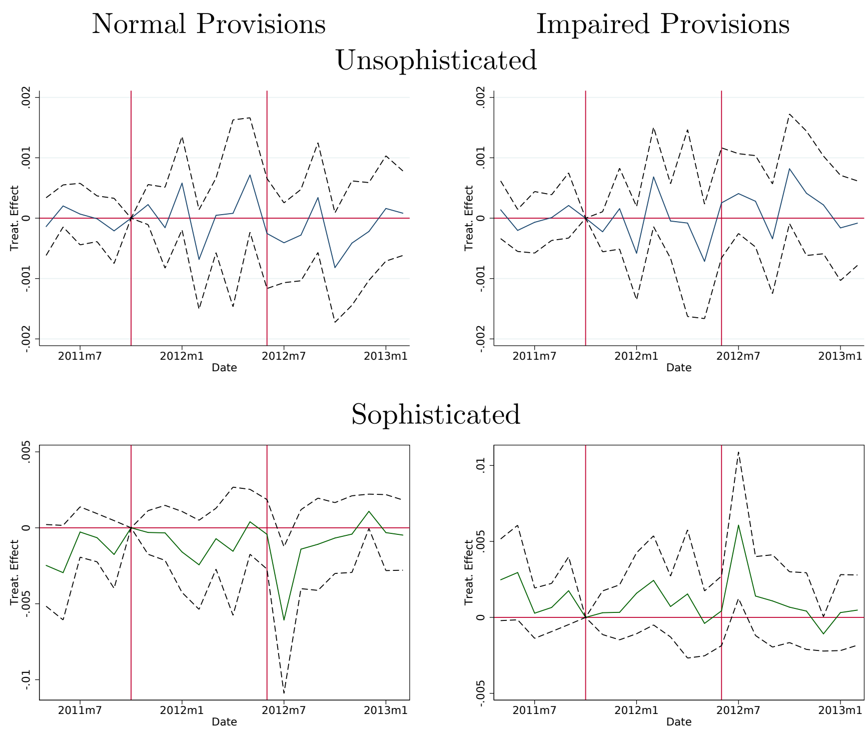
We provide additional difference-in-differences results in figure B.7-B.8 for other relevant borrowing characteristics such as income (generally increases for both borrower types), default (no effect for either group), outstanding loans (increased after the standardization regulation for both groups), 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 D.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 D.2: Credit Provisions



Notes: Estimates of βs from equation(3) 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.