Removing the Fine Print: Standardized Products, Disclosure, and Consumer Outcomes*

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Abstract

Prospective borrowers must study the fine print of loan contracts or risk surprises. To mitigate fine print, regulators have historically (a) improved disclosure or (b) standardized products. We use Chilean administrative data for a multistage natural experiment to separately identify the effects of disclosure and standardized products on borrower outcomes. We do this using Chile's unique dual-currency system, which generates exogenous variation around regulatory cutoffs. For financially sophisticated borrowers in our discontinuity sample disclosure reduces delinquencies by 13.7 percentage points or approximately 40%. We therefore use a difference-in-differences analysis to show that only standardized products benefit less sophisticated borrowers.

Keywords: Contract design, complexity, consumer financial protection, disclosure, standardization

JEL codes: D12, D14, D18, D83, D86, G21, G41, K12, L15, L51

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1 Introduction

At least 85 percent of Americans have encountered an unexpected or hidden fee over the past two years and two-thirds of them say they are paying more now in surprise charges than they were five years ago (Consumer Reports 2019). These surprises can be costly and often result from information asymmetries between borrowers and lenders that leave borrowers without the necessary information to make optimal decisions (Gabaix and Laibson 2006). Furthermore, 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) shows that these asymmetries may be intentional: companies use complexity to shroud undesirable contract features from consumers.

Regulators have used two kinds of initiatives to combat complexity. The first are relatively "paternalistic" policies: policy makers initially determine which product is optimal for most consumers and then use interventions like nudges (e.g., Thaler 2008, David et al. 2006) to encourage consumers to use this product. The second are disclosure policies: policy makers mandate that lenders disclose certain features of a product (e.g., interest rates and fees) and/or make those features salient. In theory, disclosure regulations should allow consumers to easily access the relevant information and therefore make better financial decisions. Unlike paternalists, policy makers in the disclosure camp assume that consumers will make (close to) optimal financial decisions if they can easily access the relevant information.

We directly compare the effects of product standardization versus disclosure regulations on consumers. Additionally, we test predictions about whether paternalistic and disclosure regulations have different effects across borrowers with different levels of financial sophistication. Campbell et al. (2011) have proposed that consumers would benefit from loan product standardization and Heidhues and Kőszegi (2018) predict, in theory, that standardization improves competition. However, we are the first to provide empirical estimates of the effects of standardized products on borrowers' repayment. Our results show that one size does not fit all when it comes to financial regulations; some consumers benefit from product

standardization, whereas others benefit from improved information disclosure.

On the one hand, standardization may be most helpful than disclosure for financially unsophisticated borrowers. Campbell et al. (2011) argue that search costs are correlated with cognitive ability and financial experience. Similarly, borrowers with less ability or experience likely pay a (much) higher cost to study financial contracts such as loan documents for multiple reasons. For example, it will likely take them (much) more time to study loan documents than their sophisticated peers because they have to learn many of the relevant financial concepts from scratch and are unfamiliar with technical, legal writing. It is unclear whether disclosure will defray those costs enough to make it viable for unsophisticated borrowers to study financial contracts. In contrast, borrowers needn't study to benefit from standardized products, which remove provisions that are harmful to the average borrower.

On the other hand, disclosure regulations may be best suited for borrowers who are financially sophisticated and pay a relatively low cognitive cost to study contracts. They may not wish to read the fine print but would understand the financial implications for their out-of-pocket costs if they did. Disclosure allows these borrowers study and therefore make better predictions about the cost of borrowing and what to budget for monthly payments. In contrast, standardized products do not help financially sophisticated borrowers as they are able to avoid the mistakes and pitfalls that less financially sophisticated borrowers succumb to.

In order to compare the magnitudes of different regulatory effects, we must be able to separately identify the effects of standardization and disclosure. To do so, we exploit a unique pair of regulatory changes in Chile. In 2011, Chile introduced "Universal Credit Contracts" that were both standardized and had improved disclosure. These contracts had to be shown to everyone that took out a loan below a certain cutoff amount, but the prospective borrower could choose a loan that was not a universal credit contract. In 2012, due to the popularity of the disclosure features of the universal credit contracts, they were subsequently applied to all loans. We exploit the different timing of the regulations to separately identify the effects

of standardized products and disclosure.

To estimate the effects of the different regulations, we compare borrowers just above and below the regulatory loan cutoff. This cutoff is plausibly exogenous due to a unique feature of Chilean currencies. In Chile, consumer loans and transactions are conducted in one currency–Chilean pesos–while the regulation applies at a cutoff in a 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 of borrower selection on observables, including the loan interest rate on either side of the cutoff. Because of this dual-currency system, prospective borrowers near the cutoff would in effect be randomly assigned to seeing Universal Credit contracts when they took out a loan, where borrowers above the cutoff would not be provided them as a loan option.

Our main specification finds that improvements in standardization and disclosure reduced delinquency by 14.4 percentage points (40% from an average of 34 percentage points) and reduced default by 1.6 percentage points. Our main effect is robust to other specifications, which find reduced delinquency between 8.0 and 16.9 percentage points and reduced default between 0.7 and 2.0 percentage points. Exploiting the differential timing of regulatory interventions, we then use a difference-in-discontinuity approach to separately identify the effect of standardization and disclosure. We find that disclosure alone accounts for almost all of the reduction in default (13.7 percentage points). We can thus attribute the effects from the first law to increased disclosure. However, given that the regulatory loan-size cutoff is relatively large (1,000 UF or $40,000 USD), our discontinuity sample includes relatively wealthy and sophisticated consumers. As we are also interested in the effect of regulations on less sophisticated consumers, we must also examine borrower behavior away from the cutoff. We apply the methodology proposed by Angrist and Rokkanen (2015) to estimate the effects of

standardized products away from the cutoff. As predicted by our theoretical framework in Section 2, we find that standardized products have a stronger effect on reducing delinquency as loan size decreases and consumers, presumably, become less sophisticated.

We follow Ru and Schoar (2017), among others, who use years of schooling in local neighbourhoods as a reasonable proxy for financial sophistication. Using a difference-in-differences strategy, we find that financially sophisticated borrowers reduce their delinquency rates by 10 percentage points relative to control borrowers under the disclosure regime. In contrast, financially unsophisticated borrowers reduce their delinquency rates by a similar amount with the introduction of standardized products. Moreover, financially sophisticated borrowers do not seem to default less with standardized products and financially unsophisticated borrowers do not seem to default less with increased disclosure.

Previous literature has shown mixed effects of consumer financial disclosure on financial outcomes. Disclosure has been shown to reduce loan take-up for payday loans (Bertrand and Morse 2011, Wang and Burke 2021) and reduce loan size and more responsible repayment behavior (Padi 2018). Others have found no effect of interest rate disclosure for credit card take-up and only 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. 2021). However, Woodward and Hall (2010) show that when consumers are presented with fees and interest rates bundled together, they pay less in fees.

Compared to this literature, we document a large, robust, and heterogeneous effect of disclosure. Our ability to document such a significant effect stems from four features of our setting. First, we observe the effects of disclosure on heterogeneous populations because a diverse population of borrowers in Chile take-up the same consumer loan products. In contrast, products like payday loans target a narrower segment of the population (Lawrence and Elliehausen 2008). Second, we observe administrative bimonthly payment updates on payment and default over the life of the population of consumer loans in Chile. Using this data, we find a large effect of disclosure on repayment behaviour over the life of the loan. In

contrast, many papers in the literature either measure product take-up or initial loan terms, for which they find small effects. Similarly, we find that disclosure has minimal effects on initial loan terms, except for among the most educated of borrowers, which suggests that payment behaviour may be more sensitive to disclosure than initial terms or take-up. Third, we have administrative data on all borrowers who take out a consumer loan from any lender. 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 studies in the literature, which 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 because past research (Adams et al. 2021) found that borrowers disregard disclosure from the lender in part because they (reasonably) assume the lender will benefit from informing consumers. However, borrowers may trust disclosure provided under the aegis of a regulator, which may explain our stronger results.

Our paper is structured as follows. Section 2 presents our testable hypotheses in a theoretical framework. Section 3 describes aspects of the financial system and our regulatory interventions. Sections 4, 5, and 6 present our estimation strategies, data, and results. Section 7 concludes.

2 Theoretical Framework

We develop a framework based on Heidhues et al. (2018) in appendix A. For brevity, we intuitively motivate our predictions here.

Borrowers have different levels of financial sophistication. Sophistication depends on multiple factors—cognitive ability, education, experience with financial products—that reduce the cost of studying financial contracts. We follow models like Gabaix and Laibson (2006) and make a simplifying assumption that there are two types of consumers: sophisticated consumers with low study costs and unsophisticated consumers with high study costs.

Potential borrowers know a loan's interest rate regardless of whether they study. Borrowers who study also learn about any additional fees such as origination costs, late charges, and insurance.

Studying reduces the probability that a borrower will default for two reasons. First, the borrower knows about fees in advance and can choose to either not take out a loan or continue searching if the fees are too high. Second, borrowers who know the fees can budget for them. In contrast, borrowers who take out a loan without studying learn about fees after they have already signed a contract. If those fees are higher than expected, borrowers may not have enough income to service the loan and therefore become delinquent. This is especially likely in the Chilean context, where fees can make up a large portion of the cost of a loan. For example, Chile's consumer finance agency SERNAC estimates that fees for credit insurance (which were removed in standardized products and are not mandatory for loans) range between less than 1 and 6 percent of the lifetime cost of the loan (roughly 2% of an average monthly Chilean income) (2012).

Despite the advantages of studying, consumers may not do so if the costs of studying are too high. Financially sophisticated consumers have lower study costs than unsophisticated consumers. Therefore, financially sophisticated consumers have a higher probability of studying and a lower probability of default.

Default also depends on factors other than studying, such as shocks to income and expenses. As such, the overall probability of default can be decomposed into two parts: the probability of studying and the probability of unexpected shocks.

With this framework in hand, we can turn to the effects of disclosure and standardized products. Our first two predictions concern default:

> **Prediction 1**: *Improved disclosure reduces delinquency rates of borrowers with a low cost of studying.*
>
> **Prediction 2**: *Improved disclosure does not affect delinquency rates of borrowers with a high cost of studying.*

Disclosure reduces study costs but does not eliminate them. Consider the disclosure sheet in Chile: it contains 22 lines of quantitative financial information. Many of those lines include technical terms such as Carga Anual Equivalente (CAE), which denotes the effective annual interest rate (the equivalent of US APR). But not all consumers understand those terms. For example, 28.4% of Chilean borrowers did not understand that a lower CAE (i.e. a lower interest rate) is preferable to a higher CAE (Berwart et al. 2021). One possible reason for this is that the acronym 'CAE' has multiple uses in Chile's financial sector: for example, 'CAE' also denotes the student loan program Programa de Crédito con Aval del Estado, under which it is beneficial to receive a higher loan.

Chilean borrowers are not the only ones to misunderstand simplified loan terms such as effective annual interest rate. For example, experimental evidence suggests that 80% of American participants were unable to calculate the number of months required to pay off a loan when it was quoted in APR. Furthermore, 12% of participants purchase goods when credit is quoted in interest rate that they would not purchase when credit is quoted in dollar terms (Zaki 2018). In both Chile and America, therefore, it requires financial sophistication to understand terms like APR.

Financially sophisticated consumers typically have enough ability, education, and experience to understand disclosure sheets. And those sheets are still *far* easier to study than contracts that hide information in fine print. 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 regime. Studying reduces their probability of delinquency because they will not take out a contract where the fees are larger than expected.[1]

Unsophisticated consumers will often lack the ability, education, or expertise to understand disclosure sheets. Disclosure sheets will marginally reduce the costs of studying for those consumers (it's better than fine print). But study costs will remain high enough that

[1]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.

those consumers still do not completely understand the loan before taking out a financial contract. On our framework, this counts as "not studying", because borrowers may still take out a contract where the fees are larger than expected. Such borrowers can still be hit with costly surprises, therefore, which can lead to default. In sum, our framework predicts that disclosure does not reduce study costs for unsophisticated consumers enough to decrease their probability of delinquency.

Our framework therefore predicts that disclosure will reduce delinquency for sophisticated borrowers but not unsophisticated borrowers. Standardization will have the opposite effect:

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

Unsophisticated borrowers do not study (i.e. do not fully understand) loan contracts before they sign them, because they have high study costs. Such borrowers can therefore be surprised by higher-than-expected fees or monthly payments, which can lead to delinquency. Standardized products do not include these fees and so will reduce the probability of costly surprises. As such, the product standardization should unambiguously reduce the probability of delinquency for unsophisticated borrowers. This is not the case, however, for sophisticated borrowers:

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

Standardization has a theoretically ambiguous effect on delinquency rates for sophisticated consumers, since it harms them in one way and helps them in another. Standardized products *lower* the probability of default for sophisticated consumers because they cap the costs of fees (e.g. by removing insurance).

But by capping fees, standardized products also lower the expected benefit of studying. Therefore, fewer sophisticated borrowers will study standardized products as compared to

regular products that do not cap hidden fees. But studying *any product* reduces the probability of default, since it ensures that borrowers will avoid unexpected surprises about contract terms such as origination fees and late fees. In this way, standardization will *increase* the probability of default for sophisticated consumers, by reducing the probability that they study.

Our framework thus predicts that standardization has two opposing effects on sophisticated borrowers: it reduces the probability of delinquency by capping possible expenses while increasing the probability of delinquency by reducing studying. Depending on the magnitude of these effects, standardization could either be helpful, harmful, or neutral for borrowers with low study costs.

Our framework also provides an important ancillary prediction: these regulations can reduce delinquency *even if there is no observed borrower selection.* Borrowers may take out the same loans under disclosure and standardization regimes, since many borrowers stop searching once they are approved for a loan (Agarwal et al. 2020). Indeed, 81% of Chilean borrowers shopping for consumer loans took out the first loan they were offered (Berwart et al. 2021). Even so, disclosure still causes sophisticated consumers to study more and standardization still protects unsophisticated consumers from unexpected fees. Both effects will help borrowers better plan for monthly payments, avoiding late fees, missed payments, overdraft fees on other accounts, etc. Even absent borrower selection, our model predicts that this improved financial planning will reduce default.

3 Institutional Details

For four reasons, Chile is an ideal laboratory in which to assess the effects of regulations that standardize products and increase disclosure. First, Chile implemented two natural experiments in 2011 and 2012 that allow us to tease apart the effects of disclosure and standardized products (Section 3.3). Second, Chile has a unique pair of currencies that we exploit in our

primary identification strategy (Section 3.2). Third, we have access to unusually comprehensive administrative data from Chile's financial regulator. Specifically, the banking regulatory agency has been collecting detailed information on every loan transaction for the universe of loans since 1982, including on loan performance and borrower characteristics. This gives us a window in which assess the effect of financial regulation on consumer outcomes (Section 5). Finally, Chile's financial system and products generalize to those in developed economies such as the United States (Section 3.1).

3.1 Chilean Financial System and Products

Chile is the wealthiest country in South America, with a GDP of $24,013 USD per capita as of 2017 (OECD). Similar to the Canadian and Continental European economies, the Chilean banking system is concentrated in roughly five large national banks (figure B.1).[2]

Our analysis focuses on consumer loans offered by Chilean banks with descriptive statistics presented in table 1. 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 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 are 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 that are exacerbated by lenders potentially choosing to hide important information in the fine print.

Chilean consumers can also use credit cards and lines of credit to fund consumption

[2]One unique institution is Banco Estado, a state-backed bank that operates as a for-profit entity.

purchases (table 2. Consumer credit (including consumer loans, credit cards, and lines of credit) is roughly as widespread in Chile as the US, where 63.4% and 56.9% of households respectively hold some form of consumer credit. Chile also offers loans specifically for automobiles, mortgages, and education, although they are less prominent in Chile than the US. 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 US (Klapper et al. 2015). One might argue that disclosure regulations—which were explicitly enacted to help consumers better understand their products (Section 3.3)—would have a larger effect in Chile than more financially literate countries. However, Chile's overall financial literacy rate is comparable to financial literacy rates in younger, older, and less-educated populations in the United States (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. Thus Chile is a representative country in which to study the effects of financial regulation for vulnerable borrowers.

3.2 Currency

Chile has a unique pair of currencies, which we exploit to identify the parameters of our regression discontinuity and difference-in-discontinuity. One of the key identification conditions for a regression discontinuity design is that borrowers do not manipulate the running variable—in our case the loan amount—to determine whether they are below or above the cutoff. As 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 are an inflation-adjusted currency that was created in 1967. Long-term loans such as mortgages are issued in UFs, because this allows banks to shift inflation risk onto consumers. 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 for our identification strategy, the UF to peso exchange rate changes biweekly, 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. Depending on exogenous changes to the peso–UF exchange rate, however, 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 plausibly have exogenous variation in whether borrowers fall above or below the regulatory cutoff in UFs. We verify this in section 6.1.1.

Chilean Currency Conversion Rates as of January 1, 2018

	Peso	USD
USD	615	1
UF	26,795	43

3.3 Regulatory Changes

After the 2008 financial crisis, Chilean President Sebastián Piñera campaigned for, and 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. 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 loan,

such as 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 standardized products and disclosure on consumer loan outcomes.

3.4 Law 20.448: Standardized Products and Disclosure

The first consumer financial regulatory change was announced on December 16, 2010 and implemented on October 24, 2011. The goal of this law is 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 added 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 of product standardization as 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 means that lenders could use various strategies to make Universal Credit contracts unappealing, such as pricing the Universal Credit contract disadvantageously to the borrower, presenting it within a larger menu of contract choices, and steering borrowers into other contracts.

Also, Universal Credits 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 (CAE) is equivalent to APR in the US 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 these listed figures could be included in loan contracts prior to law 20.448, they were not mandatory. Prior to the regulation, the 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 1.

3.5 Law 20.555: Disclosure

Chile's first regulation (law 20.448) had two prongs: it standardized product features and improved disclosure for Universal Credits. After its implementation, 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. Law 20.555 was announced on March 14, 2012 and implemented on July 31, 2012. Past this date, all loan contacts had to satisfy disclosure requirements (Figure 2): consumers were presented with an effective rate, CAE, as well as the monthly payment, total cost, and breakdown of non-contingent and contingent fees. The explicit goal of this law was to reduce informational asymmetries between borrowers and lenders, as the Ministry of Finance stated in the law:

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

through the allocation of greater powers and competencies to SERNAC, improving the delivery of information and carrying out studies that reduce information asymmetries.
-Biblioteca del Congreso National de Chile 2011

Law 20.448 was still in effect, so banks also had to offer Universal Credit contracts to consumers below the aforementioned regulatory cutoffs. As such, from July 31, 2012 onward, the only difference between consumers below and above the cutoff is that the former had access to standardized products. All consumers—below and above the cutoff—had access to increased disclosure on all loans due to law 20.555.

4 Estimation

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

$$y_i = \beta_1 Loansize_i + \beta_2 \mathbb{1}_{\{Loansize_i<1000\}} + \beta_3 \mathbb{1}_{\{Loansize_{it}<1000\}} Loansize_i + \gamma_1 X_i + \varepsilon_i, \quad (1)$$

where y_i represents financial outcomes of interest, in particular whether the borrower is ever 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. 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[3]. We use the bandwidth

[3]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

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. Our main specification is linear as Gelman and Imbens (2019) show higher-order polynomial estimations to be problematic. We include estimates for a global polynomial in Figures C.1 and C.2 in appendix C. We use a triangular kernel, shown to be optimal for regression discontinuity estimation by Fan et al. (1996).

4.1 Standardized Products versus Disclosure

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

The separate regression discontinuities do not allow us to separately identify the effect of each. In order to do so, we use the "difference-in-discontinuities" strategy developed in Grembi et al. (2016). They use a temporal change in regulations around an existing discontinuity to isolate the effect of the regulation separately from other effects related to the cutoff. We have an advantage over Grembi et al. (2016), as we do not have discontinuities in running variables or covariates. However, we can repurpose their method to pool both regulatory time periods (when universal credit contracts were implemented in 2011, and when disclosure was implemented for all contracts in 2012) and interact the slope and intercept of the regression discontinuity coefficients with the different temporal periods as below:

$$y_{it} = \delta_0 + \delta_1 Loansize_i + \mathbb{1}_{\{Loansize_i<1000\}}(\gamma_0 + \gamma_1 Loansize_i) + \mathbb{1}_{\{t\geq aug2012\}}[\alpha_0 + \alpha_1 Loansize_i + \mathbb{1}_{\{Loansize_i<1000\}}(\beta_0 + \beta_1 Loansize_i)] + u_i \quad (2)$$

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.

This regression allows us to separate the importance of disclosure (γ_0) and standardized products (β_0).

4.2 Identification Assumptions

Regression discontinuity estimates capture causal effects when individuals just above and below the threshold are similar in every aspect but their treatment status. To determine that our effects are causal, we must establish two identification assumptions. The first is that there should be no bunching in the distribution of loan size around the threshold to ensure that borrowers did not manipulate their treatment status. We verify this assumption in Section 6.1.1. 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 6.1.2.

5 Data

We use administrative data on the universe of consumer loans from the Chilean banking regulator, the Superintendencia de Bancos e Instituciones Financieras (SBIF).[4] We observe almost all the objective borrower characteristics that banks use to assign loans: age, income, marital status, gender, and the bank's credit risk score for the borrower. We see each loan's amount, rate, and maturity, as well the lender and location where that loan was issued. We then follow the loan in monthly intervals after its issuance, which is essential to evaluate borrower outcomes such as delinquency and default. To construct our sample, we start with an initial sample size of 7,655,263 unique consumer loans in Chile, representing roughly 95% of the population of consumer bank loans between January 1, 2009 and December 31, 2014. We drop all loans that do not go to Chilean citizens or that have missing observations for any of our control variables. This leaves us with a final sample of 6,330,428 unique loan

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

observations. We 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 grows over time from a mean of 19% in 2009 to a mean of almost 30% in 2013.[5] 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 all 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 3 plots the nominal interest rate distribution for consumer loans over time. Over the sample, rates appear to increase and grow more disperse. Figure B.4 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.

[5]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.

From Table B.1, we are able to calculate switching behaviour for 2,286,020 borrowers. Of those borrowers in the full sample, 48% switch to take out a loan with a bank different than their previous bank, and 36% 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 35% switch to a bank they had not used before as in the full sample.

5.1 Discontinuity Sample

As our regulations apply to loans below three years maturity, we further restrict our sample to only these 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 December 1, 2011 and July 31, 2012. With these restrictions, we obtain 1,088 observations. Table 3 compares loan and borrower characteristics of the discontinuity sample and full sample. Table B.2 compares loan and borrower characteristics of the difference-in-discontinuity sample and full sample. Compared to the full sample, loans in the discontinuity sample are less likely to be default (though this difference is not statistically significant) and are significantly less likely to be delinquent or extended. Loans around the discontinuity also have interest rates that are roughly half that of the full sample (25% versus 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 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% versus 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 find a statistically significant

difference across samples in the average number of loans held by each individual (between 5 and 6), though it is economically small. Lastly, borrowers in the discontinuity sample tend to live in neighborhoods with higher levels of education among 30-59 year-olds as compared to than those in the full sample.

6 Results

6.1 Regression Discontinuity

Our estimates for equation (1) are presented in table 4 and figure 4. As a result of the first regulation, law 20.448, we find that standardized products 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%, standardized products 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 Figures B.5-B.6 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 (Table 4). The global polynomial regression for if a loan ever becomes delinquent is presented in the Appendix C in Figures C.1 and C.2.

While we attribute our results to the effect of product standardization and disclosure, it is possible that introducing another product can potentially have competitive effects 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 because our data do not indicate whether a given contract is a Universal Credit contract. Yet various indirect considerations suggest 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 an ambiguous sign. In terms of the price effect, Figure B.7 plots the average interest rate. Interest rates generally went up after the introduction of the law change. Additionally, in Section 6.3.1 we find that this is true even if we consider differing trends in the selection of borrowers or products. Thus, we believe 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.

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 5). Using the "differences-in-discontinuities" method, we estimate equation (2). The results are presented in Table 6. We find that disclosure is responsible for a reduction in delinquency rates of 13.7 percentage points and is significant at the 5% level. In contrast, product standardization has a point estimate of 2 percentage points, and is statistically insignificant.

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 because borrowers below the cutoff should be better informed and thus less likely to be surprised. 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 7 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 bimonthly 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 of how the regulations effect when loans default.

Our results are presented in Table 8 and Figure 5. 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, which is consistent with our results from the regression discontinuity analysis. In addition, from Figure 5, 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. 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.

6.1.1 Manipulation of Loan Size

Given that law 20.448 is 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 disclosure. 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 3.2). All consumer loans and purchases in Chile are conducted in pesos while the regulatory cutoffs are set in a separate, inflation-adjusted currency (UFs). Because 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 6 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 (2008) density tests in Figure 6. 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 during the implementation period of December 2010-August 2012. However, we do see discontinuities in the UF distribution in the pre-period and post-period. These breaks are related to bunching around 20 million peso loan amounts as can be seen in Figure 7. The distributional breaks in the peso distributions are at least twice as large as those in the UF distributions and visually the peso distributions match breaks in the UF distributions. To account for bunching in the peso distribution, we include an indicator for loans of 20 million pesos (plus 0.1% to account for origination fees) in our regressions with additional controls (Table C.1) and find it does not affect our point estimate.

6.1.2 Covariates

To check for imbalances on observed characteristics, we replicate our regression discontinuity design using the relevant covariates as outcome variables. These results are presented in figure 8 and table 9 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 allow 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 (column 1). 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. However, this may be due to noise, since macroeconomic variables including bank funding costs and current UF–peso exchange rates are not significant around the cutoff. Regardless, if inflation expectations were driving our main effects, we should find that interest rate is discontinuous around the cutoff, which it is not. Further robustness checks are described in appendix C, including additional controls (table C.1) bandwidth sensitivity (figures C.3 and C.4), loan size cutoff sensitivity (figures C.5 and C.6), and with no slope controls (table C.2 and figure C.7).

To summarize, we find that borrowers below versus above the cutoff 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 when terms are opaque, our results show that even this population benefits from disclosure.

However, borrowers in our discontinuity sample are likely to be financially sophisticated, since they took out large loans that were in the right tail for loan size. Therefore, we cannot use a standard regression discontinuity method to determine whether standardized products

help less sophisticated borrowers. We instead 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 6.2). Second, we conduct a difference-in-differences analysis to test whether product standardization and increased disclosure have heterogeneous impacts on financially sophisticated and unsophisticated borrowers (Section 6.3).

6.2 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 how this method works, consider the example from Angrist and Rokkanen (2015). Abdulkadiroğlu et al. (2014) estimate the effect of elite schools on students just above and below the cutoffs for entrance exams that determine admission to these schools. Using a standard regression discontinuity, they find that students just above the cutoff did not have higher 10th grade test scores or postsecondary outcomes. They conclude that elite schools do not add value. However, Angrist and Rokkanen (2015) use their modified estimation method to question this result. 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 the default above and below the regression discontinuity cutoff (i.e., loan size matters to default *beyond* other covariates only because it determines treatment status). We confirm this conditional independence assumption: that is, delinquency status is uncorrelated with loan size separately above and below the 1,000 UF cutoff (Table B.4). The assumption holds in both the period of implementation for law 20.448 (where we measure the joint effect of standardized products and disclosure) and in the implementation period of law 20.555 (when we can measure product 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 10 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 standardized products 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 standardized products is larger away from the cutoff. This provides evidence for predictions 3 and 4, which imply that standardized products should add more value to consumers away from the cutoff (who tend to be less sophisticated) than consumers close to the cutoff (who tend to be more sophisticated). We next use an independent method to test this prediction using a difference-in-differences design.

6.3 Financial Sophistication: Results

We now investigate the heterogeneous impact of standardized products and disclosure 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. 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, neighbor, family member, etc. may be more financially experienced and can help guide them through the loan process.[6] 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 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 in that comuna. Using this data, we divide our sample into loans from neighborhoods 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 11 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:

[6] 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.

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

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.

Figures 9a and 9b 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 standardized products 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.

For the parameters in equation 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 9a show that delinquency rates for unsophisticated and control borrowers are flat six months before the standardization and disclosure regulation is introduced in 2011 but are otherwise fairly flat. In Figure 9b, there are no discernible pre-trends between the control and sophisticated treatment group. Figure B.8 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, B.10, and B.11 document borrower and loan characteristics during the two implementation periods. For three reasons, the timing of changes in these characteristics suggest that borrower selection does not explain the reduction in default for either sophisticated or unsophisticated borrowers. First, selection on income and outstanding debt cannot explain decreases in delinquency because they do not move in the same direction. Specifically, income remains flat during the period whereas outstanding debt grows while delinquency decreases. Second, credit risk grows when delinquency drops for sophisticated borrowers. Third, although both credit risk and delinquency drop for unsophisticated borrowers under the first law, the decrease in credit risk occurs approximately six months *after* the decrease in delinquency. As such, it is unlikely that the former caused the latter. It is possible that unobserved characteristics are driving the decrease in delinquency for unsophisticated borrowers, since interest rates seem to drop around the same time as the decrease in delinquency. But interest rates also drop for sophisticated borrowers at the same time period, even though delinquency remains unchanged. Although we cannot rule out the possibility that borrower selection explains some of the variance in delinquency, selection does not seem to be the only mechanism at play. Rather, a more parsimonious explanation is that standardized products protected unsophisticated borrowers whereas disclosure helped sophisticated ones. This explanation is consistent with our preceding results around the discontinuity. Next, we will see that the explanation is consistent with evidence about shopping behavior.

6.3.1 Rate Dispersion

While our previous regression and difference-in-differences results suggest that standardized products 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. Although

this is not the primary question of our paper, we provide suggestive evidence that financial sophistication affects whether borrowers make better choices due to standardized products and disclosure.

To assess whether borrowers make better choices, 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. Instead, we keep borrower and product characteristics constant and use dispersion in rates as a proxy for whether consumers are choosing optimal products. 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 sort 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 (Chilean peso cutoffs of 622,850, 1,384,110, 2,306,850, 3,229,590, 4,152,330, and over 5,536,440) (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 and loans between 1 and 3 year, 3 and 5 years, 5 and 7 years, 7 and 10 years, 10 to 15 year and 15 to 20 year loans, as well as 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 12 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 the aggregate, all three measures of dispersion 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 as proxied by average comuna educational attainment. We control for borrower characteristics (female, married, urban, income, credit risk, and age) and macroeconomic variables (interbank rate and expected inflation rate between UF and pesos), as well as include year fixed effects. This strategy allows us to identify the effect of financial sophistication within borrower×product cells (similar borrowers selecting similar products) rather than across cells (borrowers selecting different types of products).

We restrict our sample to financially sophisticated and unsophisticated borrowers, for our results in table 13. 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 point.

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 make better choices, whereas unsophisticated borrowers did not (since they showed greater dispersion). This is again consistent with our model, which predicts that sophisticated consumers use disclosure to better understand their loans. Using disclosure should both reduce default and improve initial terms. In contrast, we predict that less sophisticated consumers benefit from standardized products, which remove loan features that lead to costly surprises without improving understanding (and thus initial loan terms).

7 Conclusion

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 products, which put a cap on costly surprises.

We find that the introduction of standardized products and disclosure regulation reduced delinquency by 14.4 percentage points (40%) and reduced default by 1.6 percentage points (94%). We then exploit the differential timing of regulatory interventions, and use a difference-in-discontinuity approach parencitegrembi2016fiscal to separately identify the effect of standardized products and disclosure. Using this approach, we find that disclosure

alone accounts for almost all of the reduction in default (13.7 percentage points). In contrast, standardized products had no effect around the cutoff, which is consistent with our model because borrowers around the cutoff took out large loans and tended to be financially sophisticated. In contrast, using the methodology of Angrist and Rokkanen (2015), we find that standardized products have greater effects for smaller loans away from the cutoff. Again, this is consistent with our prediction that less sophisticated borrowers benefit from product standardization.

Using 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 products. Financially sophisticated borrowers do not seem to benefit from standardized product 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. Consistent with this, we use a rate dispersion approach similar to that of Argyle et al. (2017) and Atal (2016) and find that financially sophisticated borrowers made better choices (as indicated by lower dispersion). This is true on average and across both regulatory regimes, but it is especially so under the disclosure regime. In contrast, less financially sophisticated borrowers showed greater dispersion in all periods, even in periods when their delinquency rates fell. This suggests that financially sophisticated borrowers may have lowered their delinquency rates by searching across lenders, comparing multiple loans from the same lender, or bargaining more aggressively with lenders to achieve better initial loan terms. In contrast, less sophisticated consumers experienced less delinquency because they took out simpler, standardized products. Such consumers may become delinquent less because it is relatively easy comprehend the terms of simple, standardized loans, even if those loans do not offer better terms.

All our methods converge on the same conclusion: sophisticated borrowers benefit from improved disclosure, whereas unsophisticated borrowers benefit from product standardization. This is worrying because disclosure is by far the most prevalent regulatory response to fine print. Our research suggests that those regulations likely help sophisticated consumers. But that is not the stated target for most financial regulation. Unsophisticated consumers are most vulnerable and, tragically, least likely to benefit from standard regulations. One-size financial regulation does not seem to fit all borrowers in either empirics or in theory.

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8 Figures and Tables

8.1 Figures

Figure 1: Example of Law 20.448 Universal Credit Contract

CRÉDITO HIPOTECARIO - SIMULACIÓN

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

Antecedentes del Crédito Hipotecario

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

Cálculo de dividendo

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

Gastos Operacionales

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

Seguros Involucrados

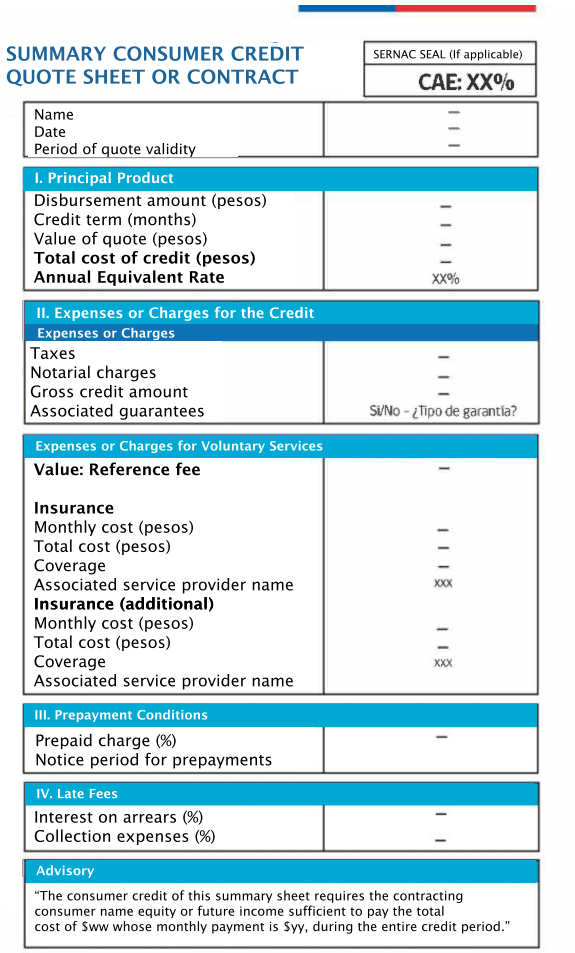
Seguro Desgravamen	1 Asegurado

CAE (**): 5.03%
Costo Final de Crédito (***): 4.687,98

(**) Carga Anual Equivalente (CAE) indicador que, expresado en forma de porcentaje, revela el costo de un crédito en un período 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 a Chilean bank, BCI. The main innovation of law 20.448 was to introduce the middle table (starting with "Plazo"). The Universal Credit contract provides 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, which were subject to the same requirements under law 20.448 but with different cutoffs (5,000 UF rather than 1,000 UF). Additionally, information on UF amounts is not present for consumer loans as they are denoted in pesos.

Figure 2: Example of Law 20.555 Disclosure Sheet (English translation)



SUMMARY CONSUMER CREDIT QUOTE SHEET OR CONTRACT

SERNAC SEAL (If applicable)

CAE: XX%

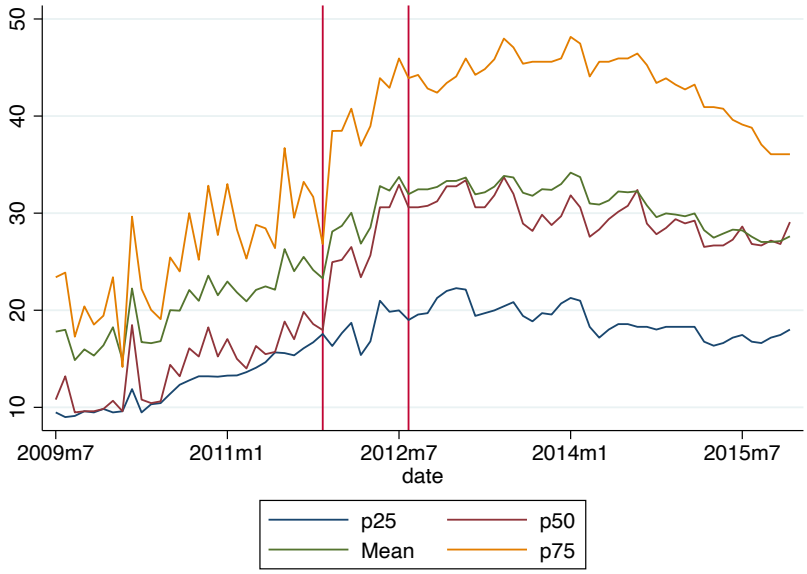
Name	–
Date	–
Period of quote validity	–
I. Principal Product	
Disbursement amount (pesos)	–
Credit term (months)	–
Value of quote (pesos)	–
Total cost of credit (pesos)	–
Annual Equivalent Rate	XX%
II. Expenses or Charges for the Credit	
Expenses or Charges	
Taxes	–
Notarial charges	–
Gross credit amount	–
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 (additional)	
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 $ww whose monthly payment is $yy, during the entire credit period."

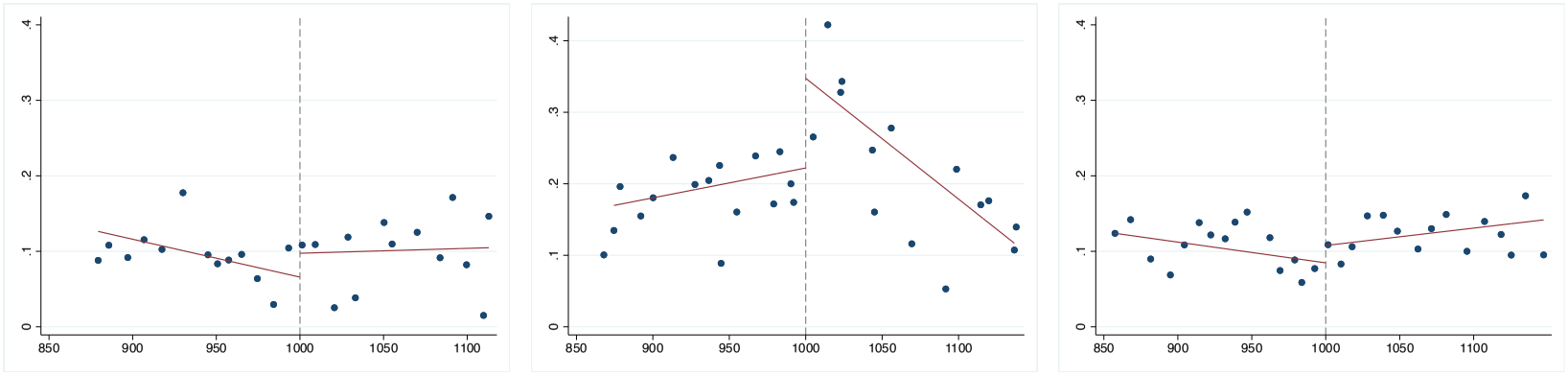
Notes: This is an English translation of the guidance included in law 20.555 that applies to all loan contracts. The disclosure requirements are similar to those of Universal Credits outlined in law 20.448 (see Figure 1).

Figure 3: Consumer Credit Interest Rates 2009-2015



Notes: Distribution of nominal interest rates over the sample period. The first red line marks the implementation of law 20.448 in November 2011 and the second marks the implementation of law 20.555 in August 2012.

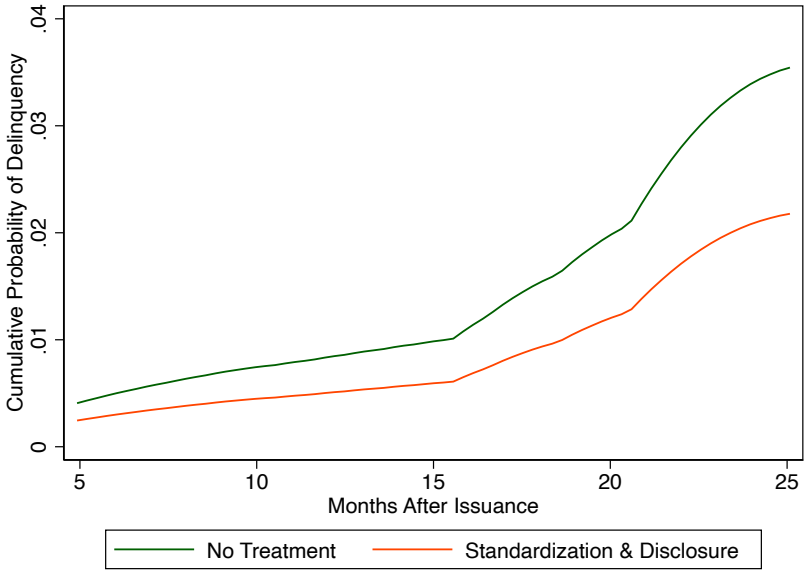
Figure 4: Regression Discontinuties



(a) Pre-period (b) Law 20.448 Implemented (c) Law 20.55 Implemented

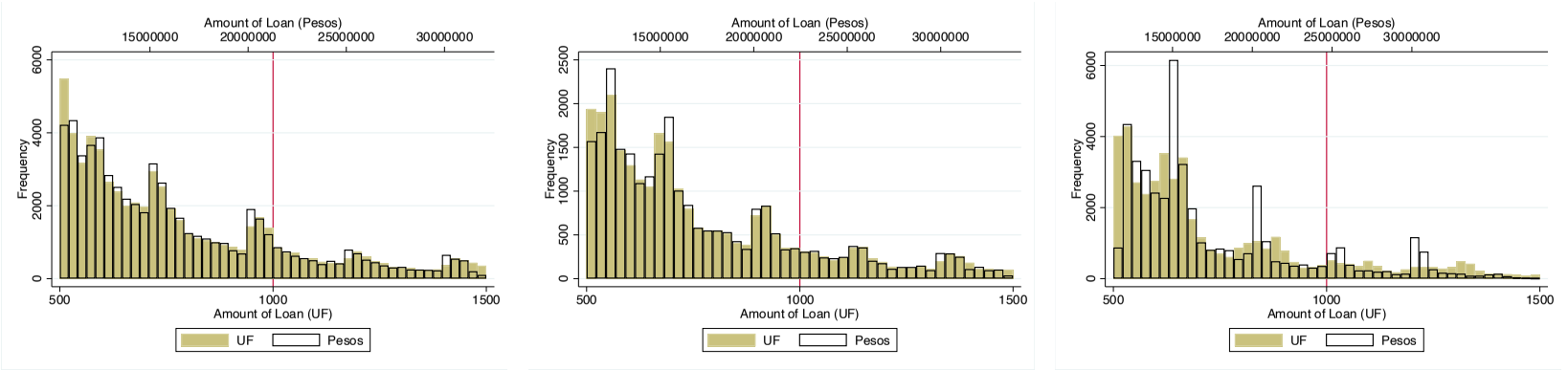
Notes: This figure graphs the linear fit of the regression discontinuity of the dependent variable of the borrower ever being delinquent (missing a payment) for the pre-period before any regulatory announcements (January 2009 and December 2010), the regulatory implementation period of law 20.448 (from November 2011 and July 2012, where standardized products with disclosure were offered on one side of the cutoff), and the post period between August 2012 to December 2015 (after the introduction of law 20.555). These graphs include controls for borrower characteristics (age, gender, married, credit risk, income), macroeconomic controls (bank funding rate, expected inflation), and comuna and lender fixed effects. The red line marks the loan cutoff of 1,000 UF.

Figure 5: Cox Proportional Hazard Rate Model

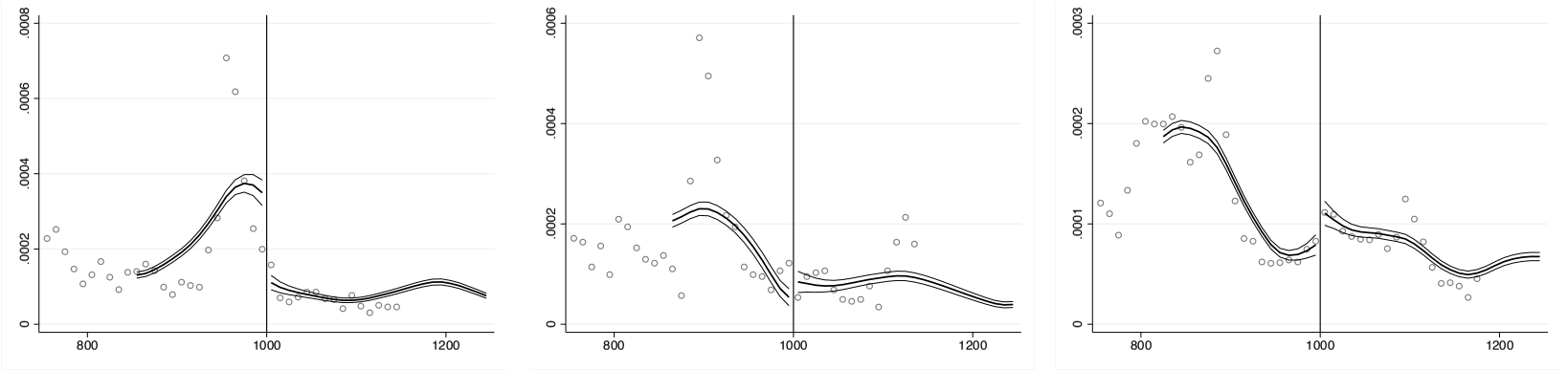


Notes: These figure plots the smoothed cumulative probability of being delinquent for borrowers around the regression discontinuity cutoff for the period of implementation for law 20.448. All covariates included in the discontinuity regression are included and set at the mean of the regression discontinuity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

Figure 6: Loan Size Density



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

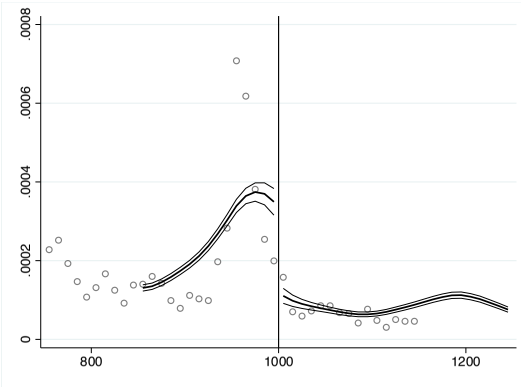


(d) McCrary Density Test: Pre-period (e) McCrary Density Test: Law 20.448 (f) McCrary Density Test: Law 20.555

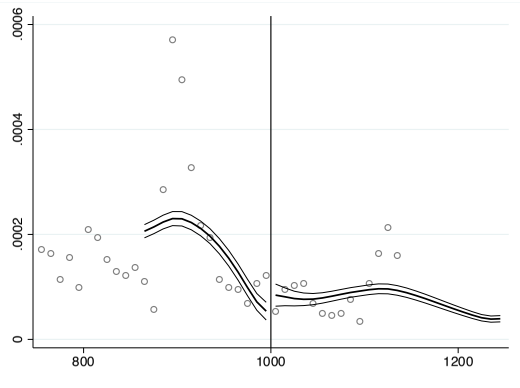
Notes: The top figures graph the distribution of loan amounts in pesos and UF around the 1,000 UF cutoff in the pre-period before any regulatory announcements (January 2009 and December 2010), the regulatory implementation period of law 20.448 (from November 2011 and July 2012, where standardized products with disclosure were offered on one side of the cutoff), and the post-period between August 2012 to December 2015 (after the introduction of law 20.555). The colored 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 7: UF versus Peso Loan Size Density

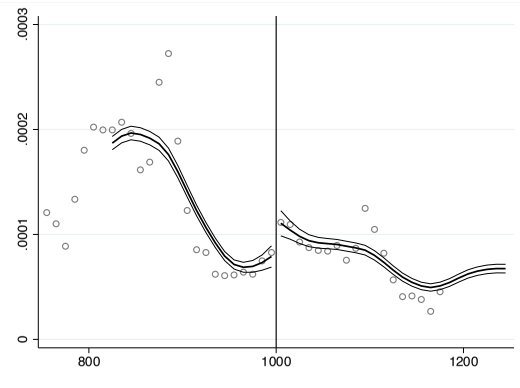
Unidad de Fomento cutoff (1,000)



(a) McCrary Density Test: Pre-period
Discontinuity Estimate = -1.32
Standard error: 0.093

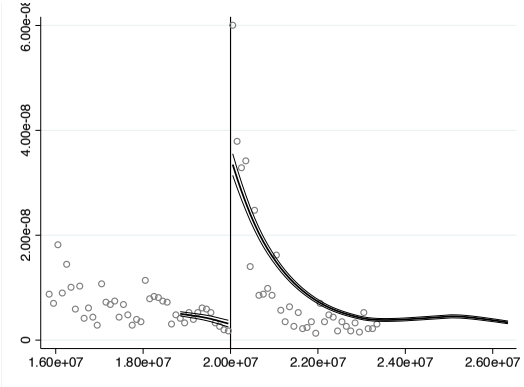


(b) McCrary Density Test: Law 20.448
Discontinuity Estimate = 0.43
Standard error: 0.22

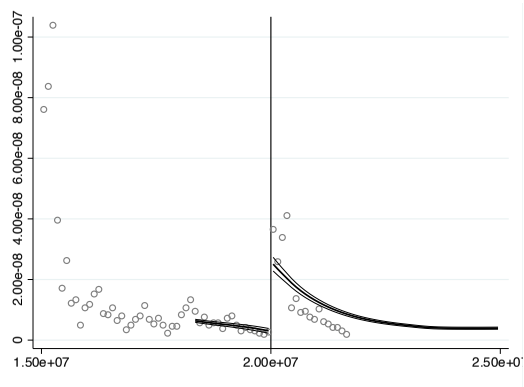


(c) McCrary Density Test: Law 20.555
Discontinuity Estimate = 0.98
Standard error: 0.074

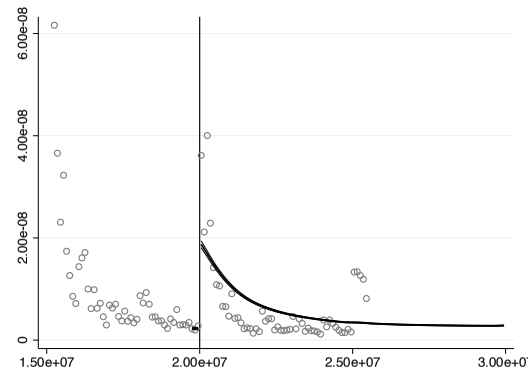
Peso cutoff (20,000,000)



(d) McCrary Density Test: Pre-period
Discontinuity Estimate = 2.43
Standard error: 0.11



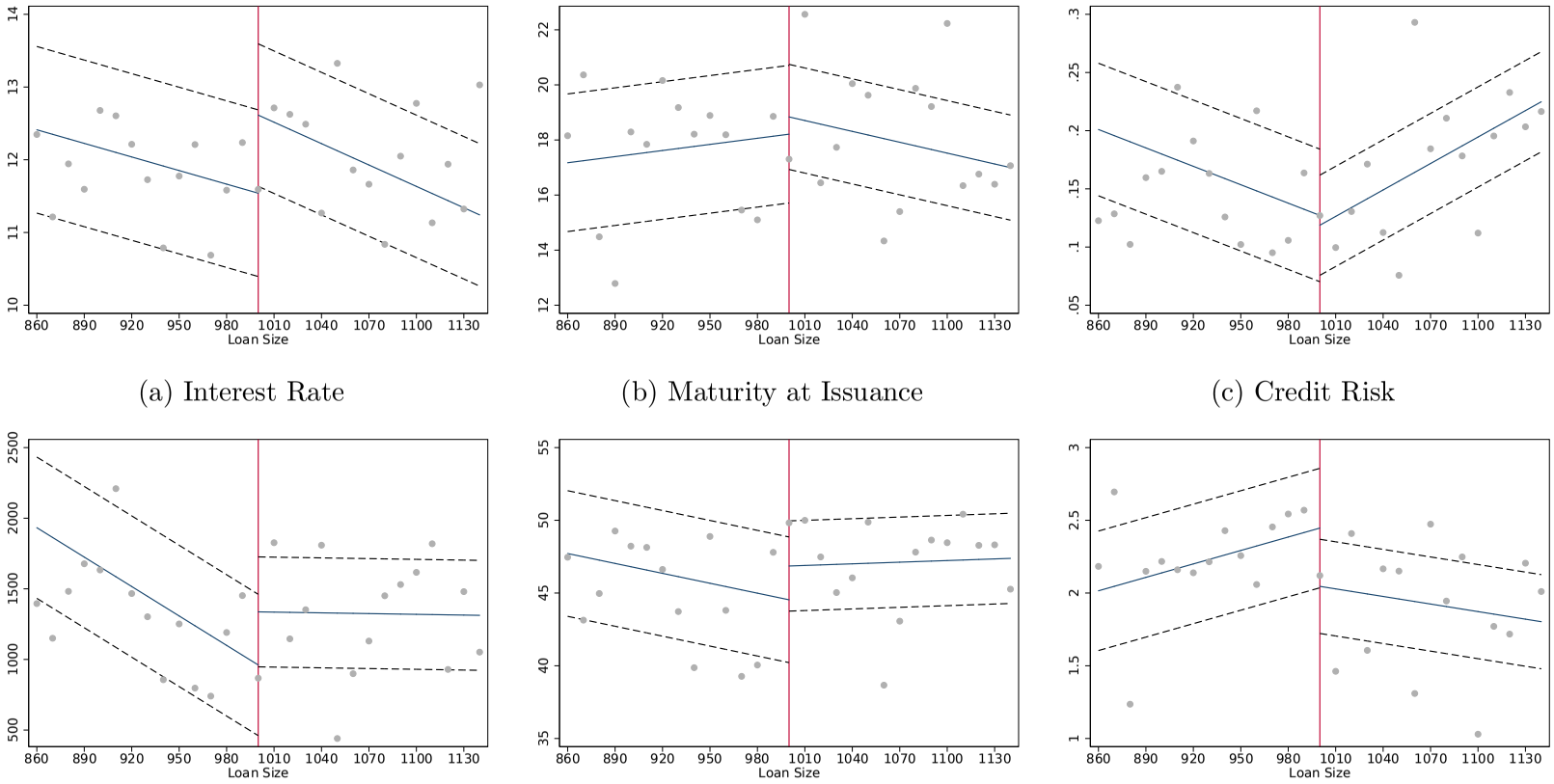
(e) McCrary Density Test: Law 20.448
Discontinuity Estimate = 2.18
Standard error: 0.15



(f) McCrary Density Test: Law 20.555
Discontinuity Estimate = 2.17
Standard error: 0.06

Notes: The top figures show the McCrary density test for loan amounts in UF around the 1,000 UF cutoff for the pre-period before any regulatory announcements (January 2009 and December 2010) and the regulatory implementation period of law 20.448 (from November 2011 and July 2012, where standardized products and disclosure were offered on one side of the cutoff). These are the same figures as in Figure 6. The bottom figures show the McCrary density test for loan amounts in pesos around the 20 million peso cutoff for the same periods. Confidence intervals are shown at the 95% significance level. The discontinuity (log difference in height) with standard errors are presented below the density graphs. The discontinuity estimates around 20 million pesos are roughly twice as large as those around the UF cutoff amounts, and indeed the bunching shapes of the 20 million peso amounts can be seen in scatter points of the UF density graphs.

Figure 8: Covariate Balancing Tests

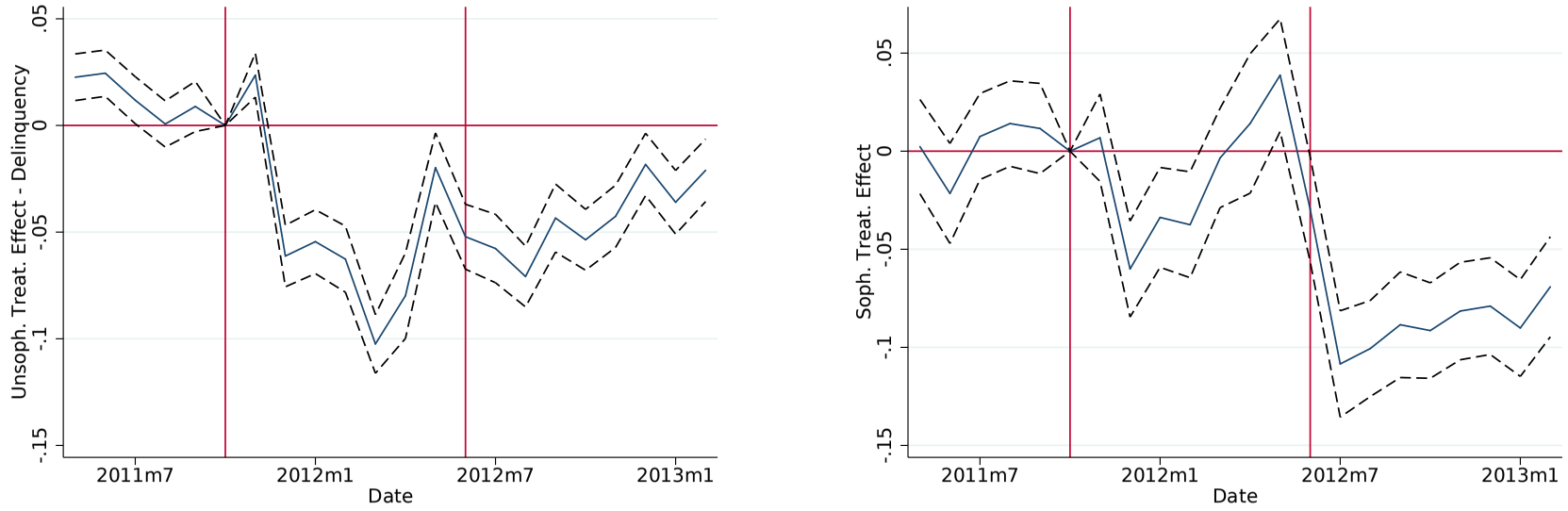


(a) Interest Rate (b) Maturity at Issuance (c) Credit Risk

(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 4. Observations are between November 2011 and July 2012. Corresponding regression tables for these figures can be found in Table 9. 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 8a) is defined as the total interest rate plus associated fees and insurances of the loan (equivalent to APR). Maturity (panel 8b) is defined as the term of the loan in months. Credit Risk (panel 8c) is defined as the average loan fraction the lenders sets as provisions in case of loss (higher numbers correspond to higher credit risk). Income (panel 8d) 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 8f) 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 9: Delinquency: Unsophisticated/Sophisticated Borrowers versus Control

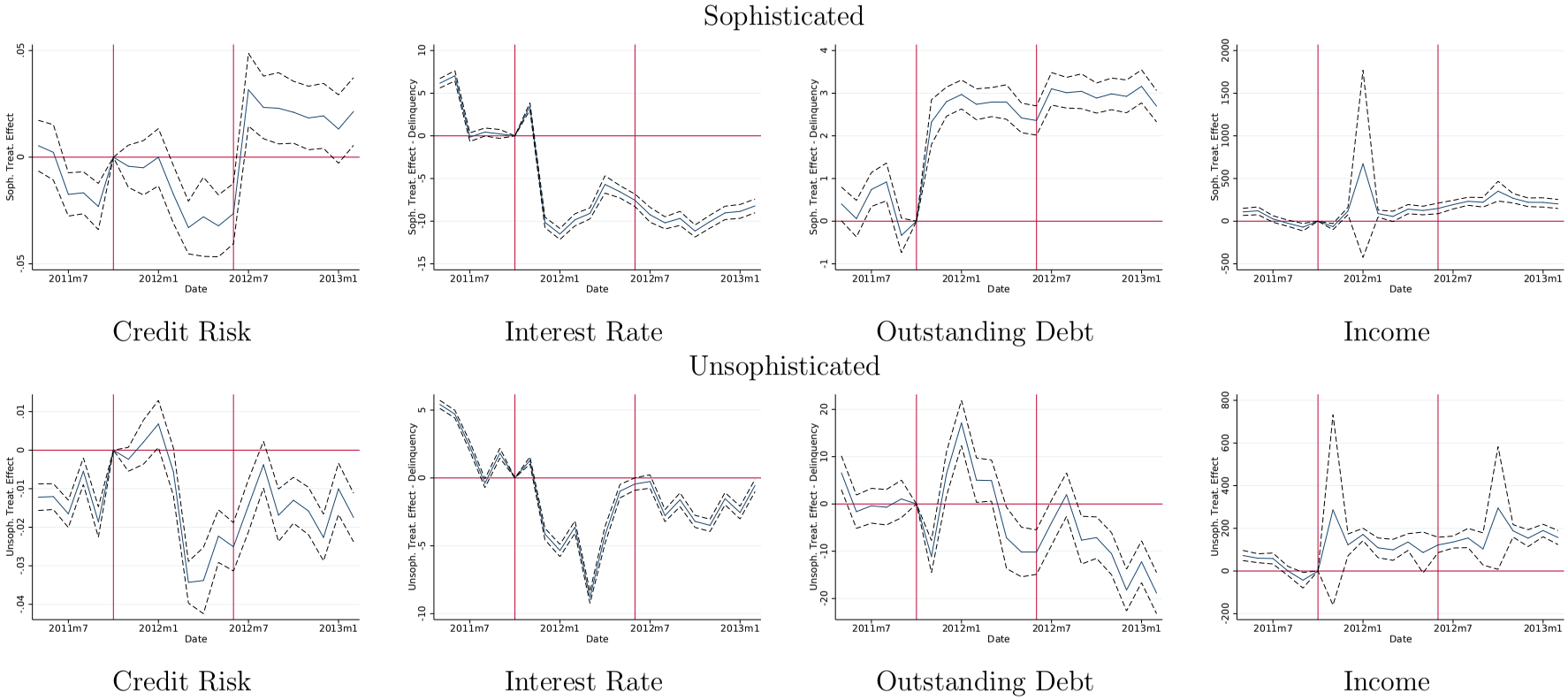


(a) Unsophisticated

(b) Sophisticated

Notes: Figure 9 graphs estimates of βs from equation(3) for borrowers in neighborhoods with the average education below 11.5 years of schooling ("unsophisticated") (left figure) and at or above 12 years of schooling ("sophisticated") (right figure) 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 two years maturity or less and under 1,000 UF in loan amount. Confidence intervals are shown at the 95% significance level. The first vertical red line marks the implementation of law 20.448 (introduction of Universal Credit contracts with standardized features and improved disclosure) and the second vertical red line marks the implementation of law 20.555, which introduced improved disclosure to all loan contracts.

Figure 10: Borrower Credit Risk



Notes: Figure 10 graphs 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 neighborhoods 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 two 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, and 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 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 nonprofit 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 2: 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 US 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 US 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 2014m and the US numbers are as of 2014 from the Federal Reserve's Survey of Consumer Finances.

Table 3: Sample Comparison

	Total Mean	Discontinuity Mean	Difference
Ever Delinquent	0.260	0.197	-0.063***
	(0.439)	(0.398)	(0.012)
Ever Defaulted	0.007	0.004	-0.002
	(0.081)	(0.067)	(0.002)
Ever Extended	0.203	0.021	-0.181***
	(0.402)	(0.145)	(0.004)
Rate	24.578	11.974	-12.604***
	(13.860)	(3.363)	(0.101)
Maturity at Issue	24.687	17.478	-7.209***
	(17.249)	(7.785)	(0.233)
Loan Size (UF)	117.256	970.966	853.710***
	(170.538)	(86.444)	(2.586)
Female	0.425	0.208	-0.217***
	(0.494)	(0.406)	(0.012)
Age	44.341	46.850	2.510***
	(13.508)	(12.901)	(0.386)
Credit Score	0.125	0.168	0.043***
	(0.164)	(0.210)	(0.006)
Total Num. Loans	5.716	5.437	-0.279**
	(6.837)	(4.294)	(0.128)
Num. Loans Outstanding	3.461	3.951	0.489***
	(4.331)	(3.120)	(0.093)
Outstanding Debt (UF)	139.193	1,085.726	946.534***
	(205.236)	(330.862)	(9.896)
Future Debt (UF)	236.349	903.401	667.052***
	(516.266)	(1,358.976)	(40.644)
Observations	6,330,428	1,117	6,331,545

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). 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. 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 nominal interest rate of the loan. 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. Neighborhood years of schooling was obtained from the Chilean census data for the year 2016.

Table 4: Regression Discontinuity: Borrower Outcomes

	(1) Ever Delinquent	(2) Ever Defaulted	(3) Ever Extended
Standardization & Disclosure	-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)
Stdn & Disc. 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 4 shows the estimates of equation 1 for law 20.448's impact on borrowers taking out loans from 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 (neighborhoods) and the 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 the interbank 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 5: Ever Delinquent Regression Discontinuity Across Time

	(1) Pre-change	(2) Implementation	(3) Post change
Standardization & Disclosure	-0.0349	-0.144**	-0.0253
	(0.0333)	(0.0701)	(0.0197)
Loan Size	0.0183	-0.150**	0.0262
	(0.0518)	(0.0585)	(0.0223)
Stdn & Disc. X Loan Size	-0.0831	0.166**	-0.0569*
	(0.0609)	(0.0824)	(0.0291)
Comuna Fixed Effects	Y	Y	Y
Lender Fixed Effects	Y	Y	Y
Bandwidth	128	143	145
Kernel	Tri	Tri	Tri
Mean	.102	.341	.081
N	1884	1117	4440

Standard errors in parentheses

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

Notes: Table 5 shows the estimates of equation 1 for the pre-period before any regulatory announcements (January 2009 and December 2010; column 1), the regulatory implementation period of law 20.448 (from November 2011 to July 2012, where standardized products with disclosure were offered on one side of the cutoff; column 2), and the post period between August 2012 to December 2015 (after the introduction of law 20.555; column 3). Loans with a maturity of less than three years and within our bandwidth are selected by procedures outlined in Calonico et al. (2014) and Calonico et al. (2018). Transparency corresponds to both product standardization and disclosure in the implementation period and to just product standardization in the post-period. All estimates are based on regressions that include fixed effects for comunas (neighborhoods) and the 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 the interbank rate are included as controls for aggregate economic conditions.

Table 6: Regression Difference-in-Discontinuity

	(1) Ever Delinquent
Disclosure	-0.137**
	(0.0668)
Standarized Contract	0.0204
	(0.0990)
Comuna Fixed Effects	Y
Lender Fixed Effects	Y
Bandwidth	153
Kernel	Tri
N	2466

Standard errors in parentheses

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

Notes: Table 6 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 (neighborhoods) and the 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 the interbank rate are included as controls for aggregate economic conditions. We allow for flexible borrower characteristics across the time period (interacting the borrower characteristics with a post-control dummy), and restrict the post sample to an equal time window before and after the implementation of law 20.555 that applied disclosure to all loans (i.e., October 2010-May 2013).

Table 7: Regression Discontinuity - Other Loan Outcomes

	(1) Month Default	(2) # Miss. Pmnts	(3) $ Miss. Pmnts	(4) Future debt
Standardization & Disclosure	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)
Stdn & Disc. 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 7 shows the estimates of equation 1 for law 20.448's impact on borrowers taking out loans for the period 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 (neighborhoods) and the 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 the interbank rate are included as controls for aggregate economic conditions.

Table 8: Cox Proportional Hazard Rate Model

	(1) Delinquency	(2) Delinquency
Standardization & Disclosure	-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 8 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 (neighborhoods) and the 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 the interbank rate are included as controls for aggregate economic conditions.

Table 9: Covariate Balancing Tests

	(1) Interest Rate	(2) Maturity	(3) Credit Risk	(4) Income	(5) Age	(6) Expected Inflation
Standardization & Disclosure	-0.774	-1.277	0.00256	-321.4	-3.159	0.368*
	(0.514)	(1.232)	(0.0314)	(241.7)	(2.161)	(0.219)
Loan Size	-0.424	-1.626	0.0754**	8.365	0.413	-0.176
	(0.451)	(1.150)	(0.0301)	(223.6)	(1.730)	(0.199)
Stdn & Disc. X Loan Size	-0.244	2.268	-0.136***	-620.5*	-3.723	0.438*
	(0.609)	(1.491)	(0.0393)	(334.2)	(2.471)	(0.257)
Comuna FE	Y	Y	Y	Y	Y	Y
Lender FE	Y	Y	Y	Y	Y	Y
Bandwidth	143	143	143	143	143	143
Kernel	Tri	Tri	Tri	Tri	Tri	Tri
Mean	13	19	0	1338	47	2
N	1117	1117	1117	1117	1117	1117

Standard errors in parentheses

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

Notes: Table 9 shows the estimates of equation 1 for law 20.555's impact on borrowers taking out loans from 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 10: Conditional Independence Estimates

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

Notes: Table 10 follows Table 3 from Angrist and Rokkanen (2015). Bootstrapped standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 11: 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: Table 11 presents summary statistics for difference-in-differences analysis sample. Loans are collapsed to observation per loan, and all loans are two 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 12: Rate Dispersion Summary Statistics

	Mean	Standard Deviation
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 using the procedure detailed in section 6.3.1. Dispersion is measured by the difference in the interest rate from the 25th percentile rate in the borrower × product bin, the difference in the minimum rate, and the standard deviation of rates.

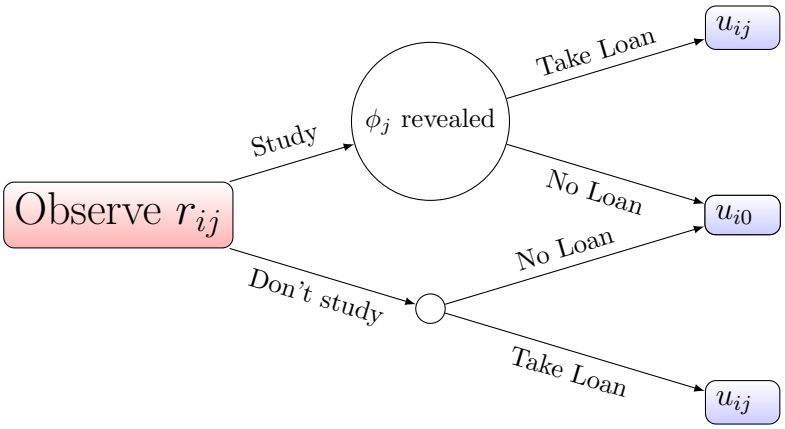
Table 13: Rate Dispersion Regression Results

	(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

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. This table presents our results related to measures of interest rate dispersion. Dispersion is measured by the difference in the 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 6.3.1. Controls include loan maturity, credit risk, income, sex, if married, comuna, age, interbank rate, and expected inflation. Robust standard errors in parentheses.

Appendix A Model (In Text Appendix)

Borrowers randomly observe a loan l_{ij} from a lender $j \in J \geq 2$ lenders and perfectly know its headline interest rate r_{ij}. The loan also has fine print conditions that the borrower must anticipate to avoid extra expenses that can be modeled as a random variable $\widetilde{\phi}_j \in [\underline{\phi}, \bar{\phi}]$. Once observing a rate, borrowers are faced with two decisions: whether to study the 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.[7] The borrower knows γ_i but the lender does not. Whereas who do not study have only an expectation about the fees $E[\widetilde{\phi}_j]$, borrowers who study know the actual fees ϕ_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 in J.[8] The borrower chooses to take out the loan from lender j if expected utility of doing so is at least as 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 u_{ij} in two ways. First, fees decreases the borrower's utility directly because there is an additional term $\widetilde{\phi}_j$ subtracted from the value the borrower obtains from the loan. Second, fees affect the borrower's utility indirectly by increasing the probability that the borrower will default on their loan payment because their monthly income m_i is smaller than the fees associated with their loan. The probability of

[7] 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.

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

default is represented by $P\big[m_i - r_{ij} \times l_{ij} - \widetilde{\phi}_j < 0\big]$. 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 $u_{ij} < u_{i0}$, that is, 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: that is,

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

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 borrowers will study if and only if the cost of studying is smaller than the cost of not studying (i.e. the cost of being "surprised" by unexpected fees).

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 make the simplifying assumption that there are two types of borrowers: unsophisticated ones with higher costs of studying (low γ_i) and sophisticated ones with lower costs of studying (high γ_i). We assume that borrowers within these types have a spectrum of study costs: c_H and c_L for unsophisticated and sophisticated borrowers, respectively.

A.1.1 Disclosure

Increased disclosure lowers the cost of studying a loan contract. That said, disclosure does not reduce study costs to 0 (see 2 for evidence to motivate this assumption.) For all borrowers, there is a new study cost function c^d such that, $0 < c^d(\gamma_i) < c(\gamma_i) \forall i$.

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

Since $c^d(\gamma_i) < c(\gamma_i) \in c_L$, the right hand side of (4) is smaller under the improved disclosure regime than the fine print regime. As such, some sophisticated borrowers who

wouldn't study under the fine print regime will now study under the improved disclosure regime.

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

Unsophisticated consumers start with a very high study cost $c(\gamma_i) \in c_H$. Although disclosure marginally lowers those study costs such that $c^d(\gamma_i) < c(\gamma_i)$, $c^d(\gamma_i)$ is still too high to satisfy equation (4) (see see 2 evidence to motivate this hypothesis.)

In sum, 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 Standardized Products

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 does not 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 standardized products 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 products have no contingent and unnecessary fees, which increases their probability of delinquency. Our model therefore predicts that product standardization will have an ambiguous effect on sophisticated borrowers because 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 product standardization nor the unregulated regimes (that is $P[study = 0] = 1$ for all regimes). Our model therefore predicts that standardized products will substantially decrease the probability of default for unsophisticated borrowers because 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 standardized products help unsophisticated borrowers more than sophisticated ones.

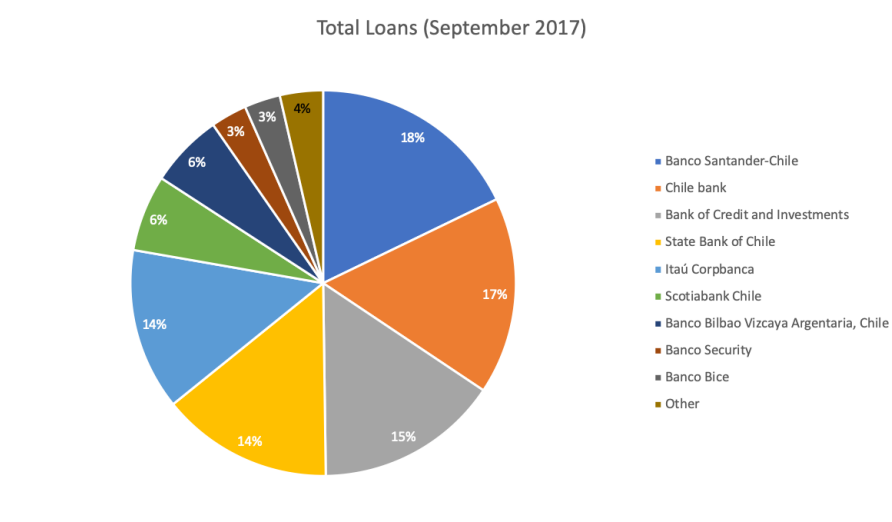
In sum, our model predicts that financial regulations should have heterogeneous affects across consumers. Sophisticated consumers should default less with increased disclosure, but be largely unaffected (or even worse off) from standardized products. In contrast, unsophisticated consumers should default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

Online Appendices

Appendix B Additional Figures and Tables

B.1 Figures

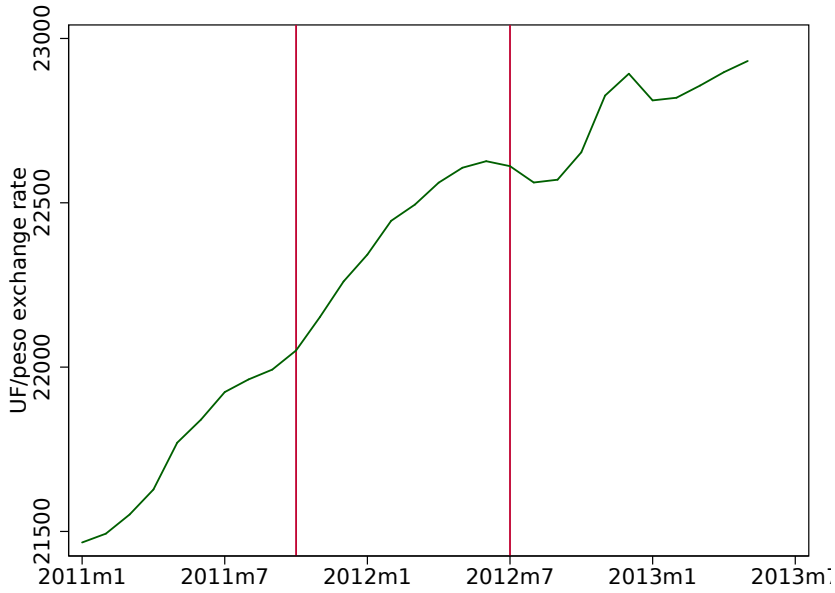
Figure B.1: 2017 Chilean Bank Composition



Source: SBIF

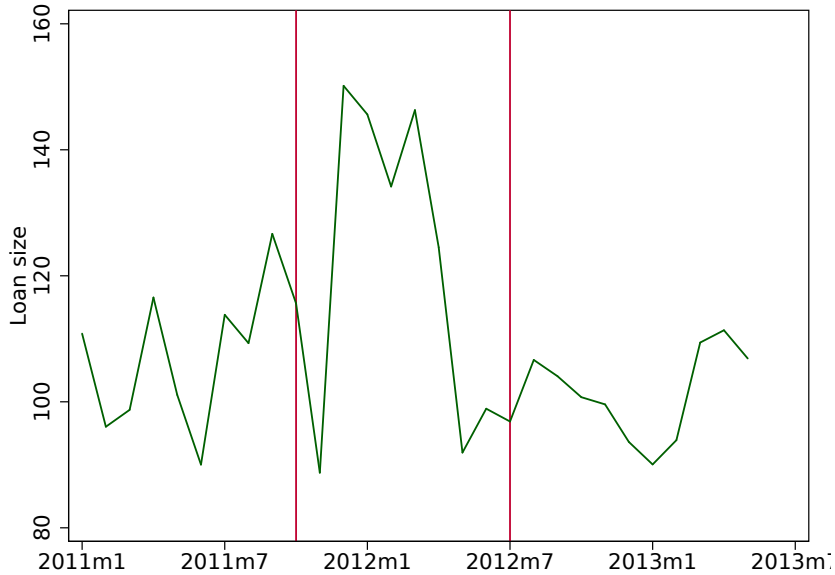
Notes: This figure graphs the market share of total loans across banks in Chile. Banco Estado (State Bank of Chile in yellow) is a state-owned bank that is run as a for-profit entity.

Figure B.2: UF-to-Peso Exchange Rate



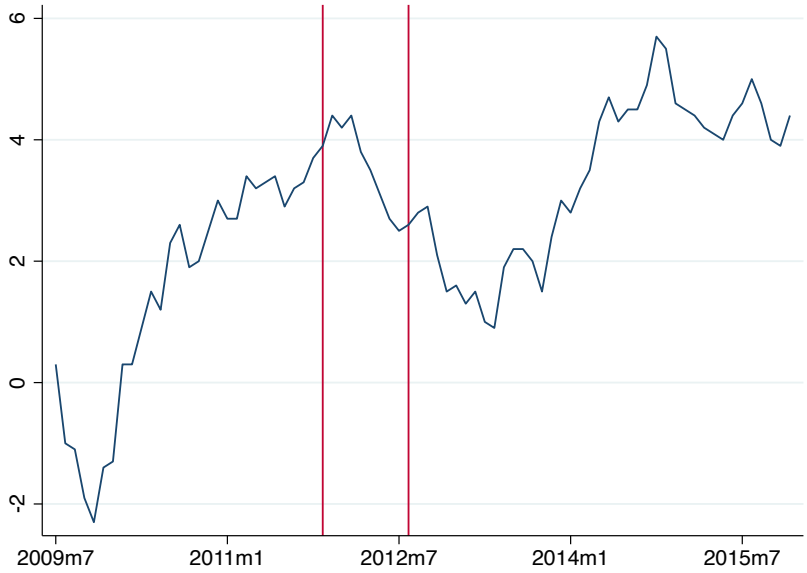
Notes: This figure graphs the mean monthly exchange rate of UF to pesos. The first red line is the implementation date of law 20.448 (the introduction of Universal Credit contracts) and the second red line is the implementation date of law 20.555 (disclosure requirements for all loans).

Figure B.3: Average Loan Size (UF)



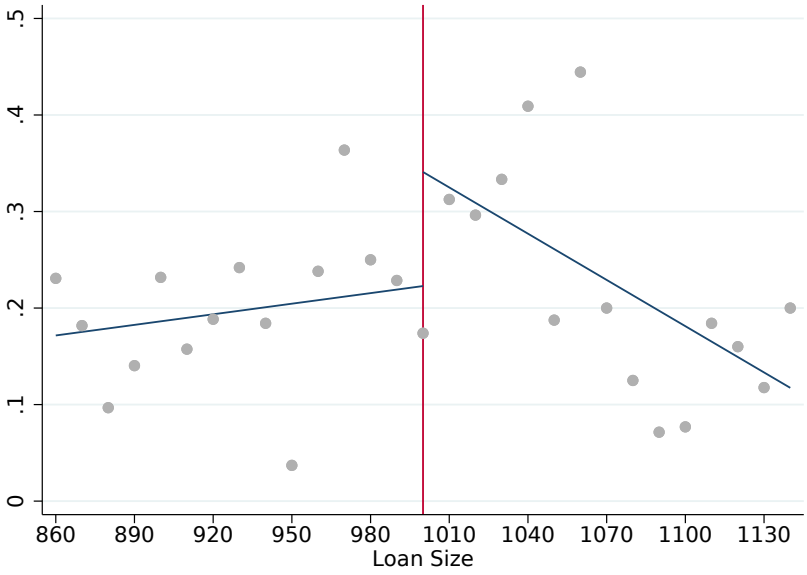
Notes: This figure graphs the unweighted average of loan sizes in UF of newly issued loans by issuance date. The first red line is the implementation date of law 20.448 (the introduction of Universal Credit contracts) and the second red line is the implementation date of law 20.555 (disclosure requirements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

Figure B.4: Historical Inflation 2009-2015



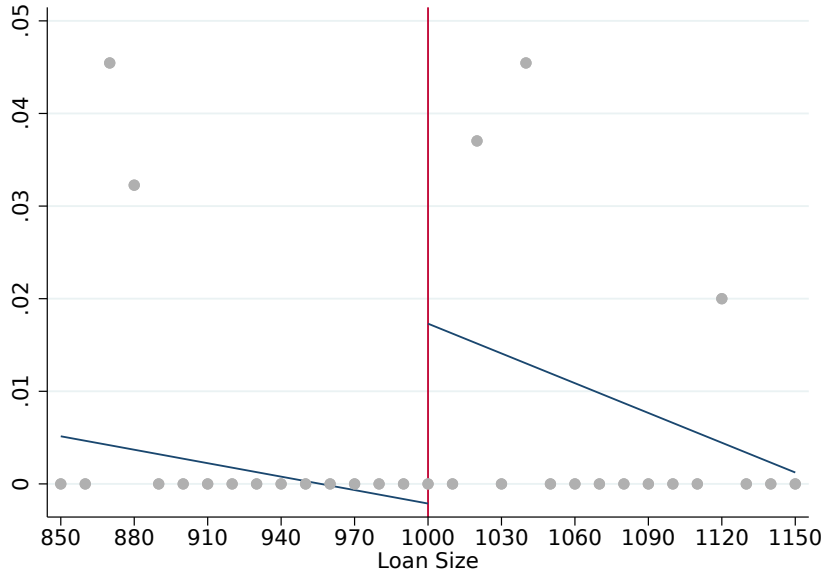
Notes: Monthly change in the consumer price index (IPC) from the Banco Central de Chile. The first red line marks the implementation of law 20.448 in November of 2011 and the second marks the implementation of law 20.555 in August of 2012.

Figure B.5: Raw Regression Discontinuity - Ever Delinquent



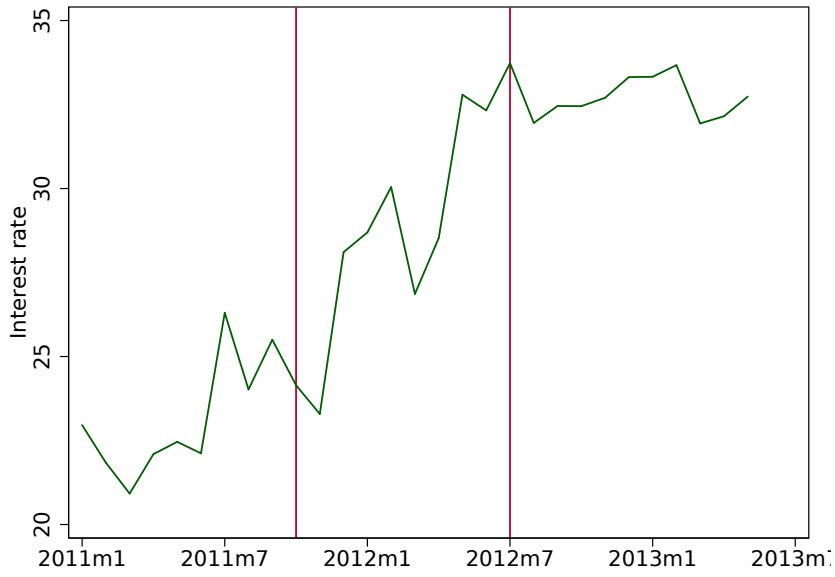
Notes: This figure graphs the linear fit of the raw regression discontinuity of the dependent variable of the borrower ever becoming delinquent (missing one or more payments) in equation (1) with no controls. The red line marks the loan cutoff of 1,000 UF.

Figure B.6: Raw Regression Discontinuity - Ever Default



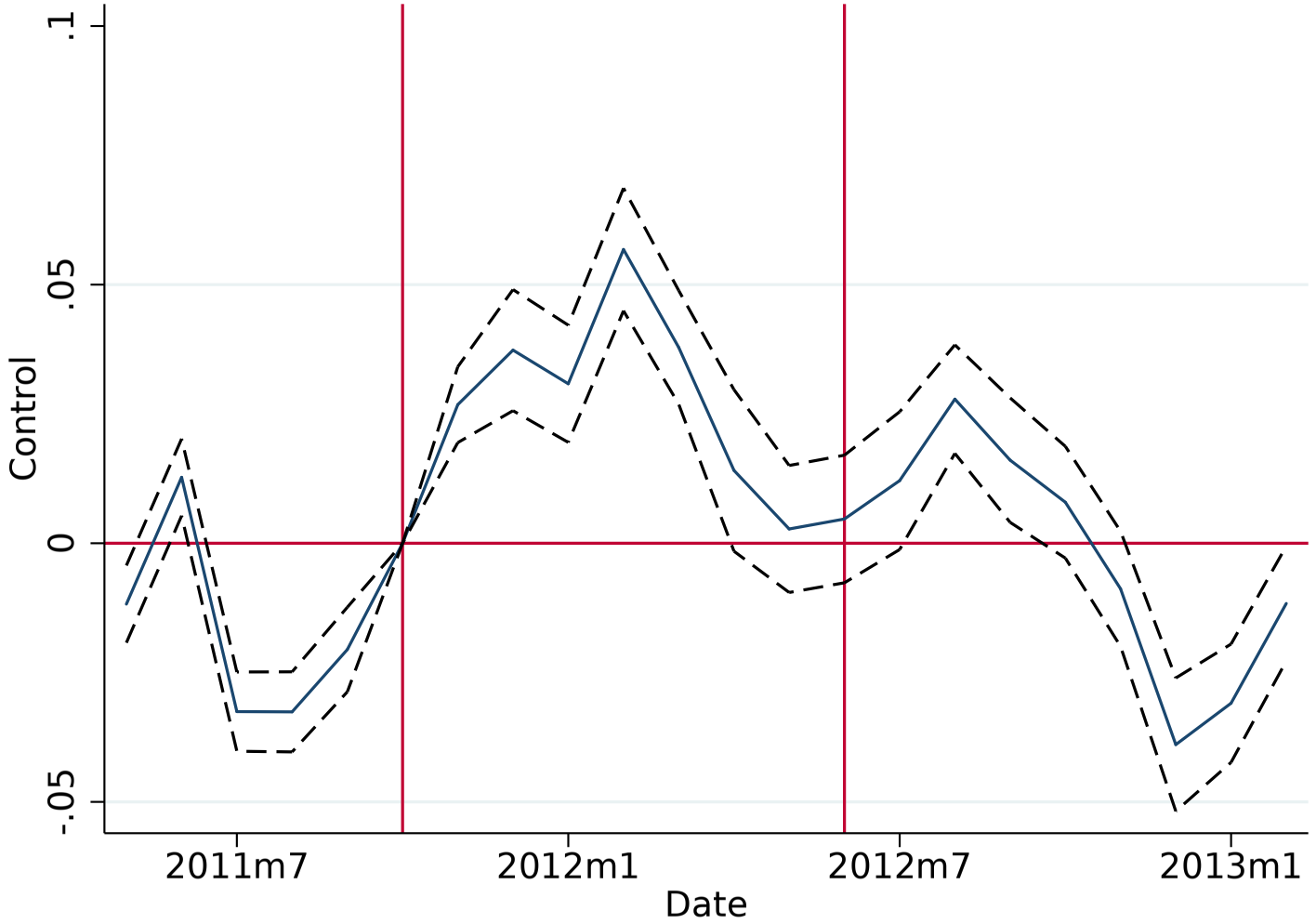
Notes: This figure graphs the linear fit of the raw regression discontinuity of the dependent variable of the borrower ever defaulting (missed three payments and judicial proceedings initiated) in equation (1) with no controls. The red line marks the loan cutoff of 1,000 UF.

Figure B.7: Average Nominal Interest Rate



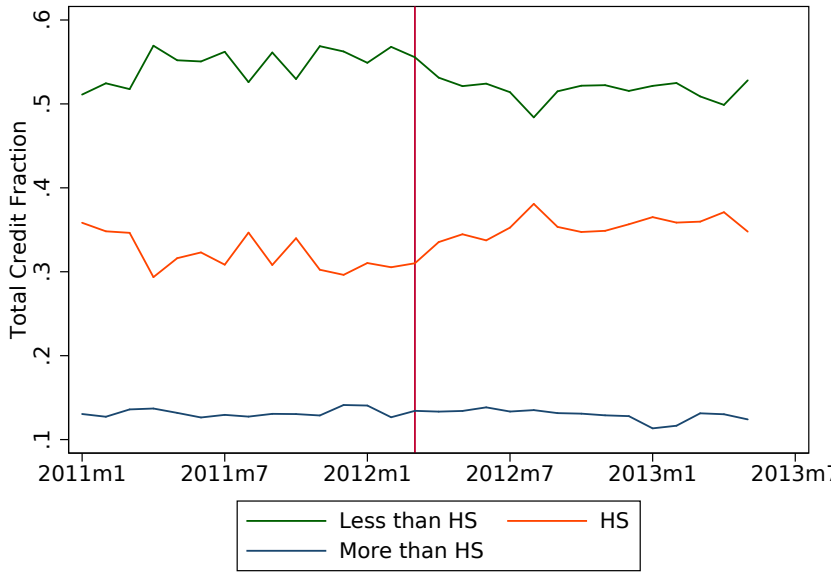
Notes: This figure graphs the unweighted average of nominal interest rates of newly issued loans by issuance date. This rate includes all fees and insurance charges associated with the loan and is equivalent to APR. The first red line is the implementation date of law 20.448 (the introduction of Universal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

Figure B.8: Delinquency: Control Group Time Trends



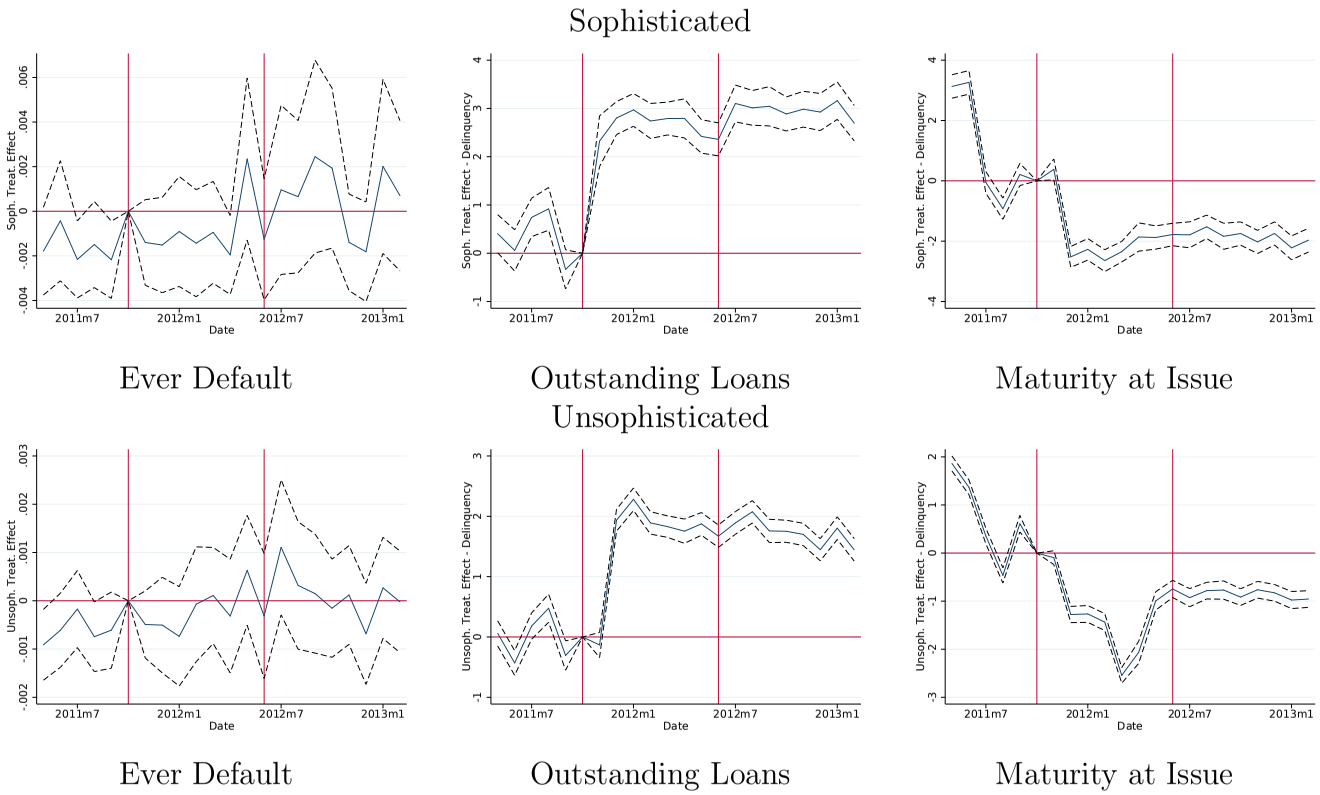
Notes: Estimates of αs from equation(3) for borrowers in neighborhoods with the average education between 11.5 and less than 12 years of schooling, or the control group. These coefficients are equivalent to time trends in delinquency in the absence of treatment. Loans are collapsed to one data point per observation, and all loans are two 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.9: Borrower Composition



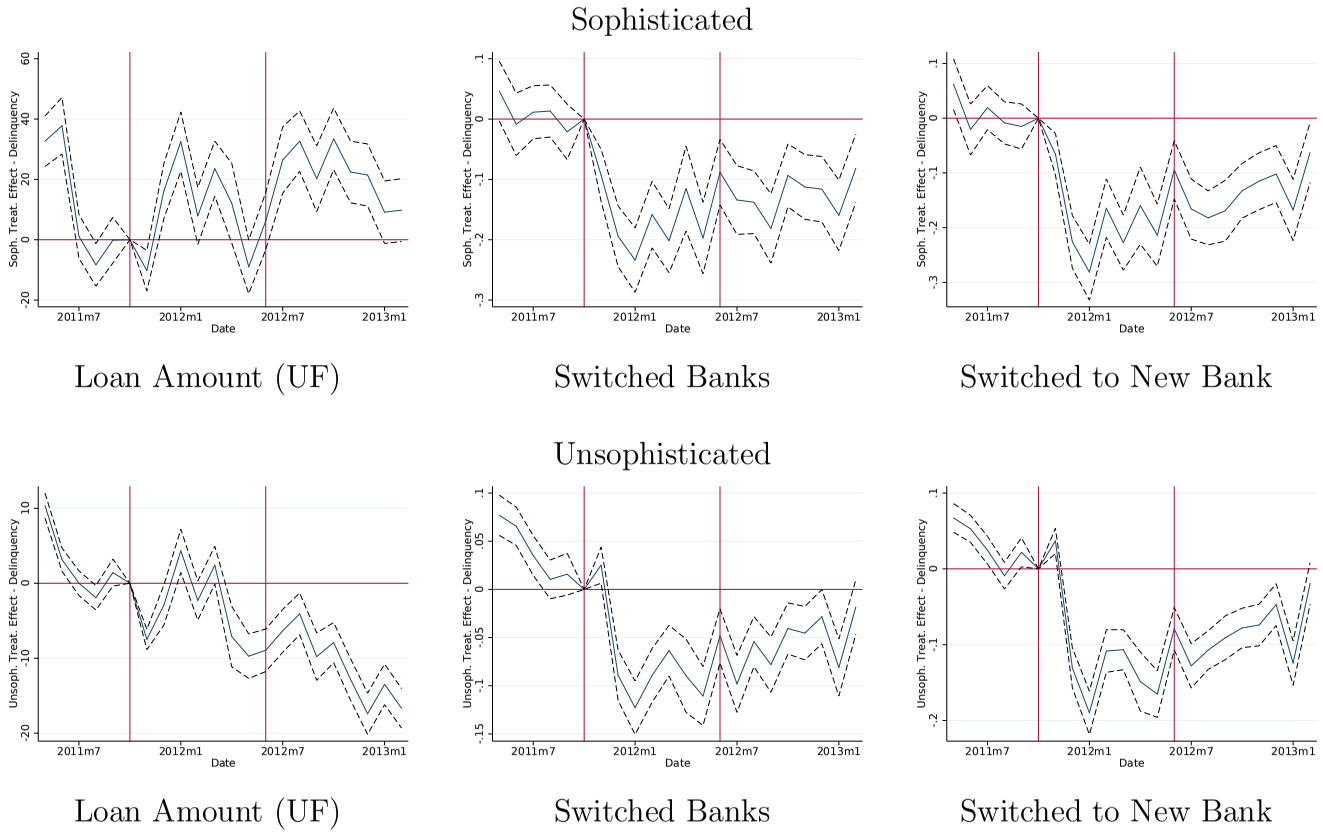
Notes: Figure B.9 shows the fraction of total credit by loan size disbursed to each level of neighborhood education average. Our education are levels below 11.5 years of schooling for less than high school, between 11.5 and less than 12 years of schooling for high school educated, and above 12 years of schooling for more than high school educated. The red vertical line denotes March 2012 when the nonbank credit registry was not available to banks making lending decisions.

Figure B.10: Other Characteristics - I



Notes: Estimates of βs from equation(3) for borrowers in neighborhoods 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 neighborhoods 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 two years maturity or less and under 1,000 UF in loan amount. Confidence intervals are shown at the 95% significance level. The first vertical red line marks the implementation of law 20.448 (introduction of Universal Credit contracts with standardized features and improved disclosure) and the second vertical red line marks the implementation of law 20.555, which introduced improved disclosure to all loan contracts. Income is the total annual income for the borrower in UF, ever default is an indicator if a loan payment has not been made in 90 days and judicial proceedings have been initiated against the borrower. Outstanding loans are the total number of loans the borrower has at the time of origination, and maturity at issue is the maturity of the loan in months at the date of loan issuance.

Figure B.11: Other Characteristics - II



Notes: Figure B.11 graphs estimates of βs from equation(3) for borrowers in neighborhoods 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 neighborhoods 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 two 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 out from. "Switch to new bank" is an indicator for if the borrower had not previously taken out a loan at this bank.

B.2 Tables

Table B.1: 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.2: Sample Comparison - Difference in Discontinuities

	Total Mean	Discontinuity Mean	Difference
Ever Delinquent	0.260	0.178	-0.083***
	(0.439)	(0.382)	(0.008)
Ever Defaulted	0.007	0.004	-0.002
	(0.081)	(0.067)	(0.001)
Ever Extended	0.203	0.026	-0.177***
	(0.402)	(0.158)	(0.003)
Rate	24.581	12.165	-12.416***
	(13.860)	(3.198)	(0.065)
Maturity at Issue	24.689	17.475	-7.213***
	(17.250)	(8.049)	(0.162)
Loan Size (UF)	117.074	970.619	853.544***
	(170.095)	(93.474)	(1.883)
Female	0.425	0.192	-0.233***
	(0.494)	(0.394)	(0.008)
Age	44.340	48.001	3.661***
	(13.509)	(12.071)	(0.243)
Credit Score	0.125	0.175	0.050***
	(0.164)	(0.209)	(0.004)
Total Num. Loans	5.716	5.446	-0.270***
	(6.838)	(4.326)	(0.087)
Num. Loans Outstanding	3.461	4.011	0.549***
	(4.331)	(3.315)	(0.067)
Outstanding Debt (UF)	138.989	1,091.461	952.472***
	(204.728)	(325.937)	(6.563)
Future Debt (UF)	236.223	862.741	626.518***
	(515.918)	(1,308.153)	(26.338)
Observations	6,329,079	2,466	6,331,545

Notes: This table compares our relevant control and other variables of the full sample and our difference-in-discontinuities sample (Panel B) chosen by the bandwidth procedure outlined in Calonico et al. (2014) and Calonico et al. (2018). 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. 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 nominal interest rate of the loan. 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. Neighborhood years of schooling was obtained from the Chilean census data for the year 2016.

Table B.3: Raw Regression Discontinuity

	(1) Ever Delinquent	(2) Ever Defaulted	(3) Ever Extended
Standardization & Disclosure	-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)
Stdn & Disc. 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 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 have 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) who use the variable D to denote treatment status (0 is untreated 1 is treated). Following 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 are 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 come from the 2002 Chilean Census obtained through IPUMS.

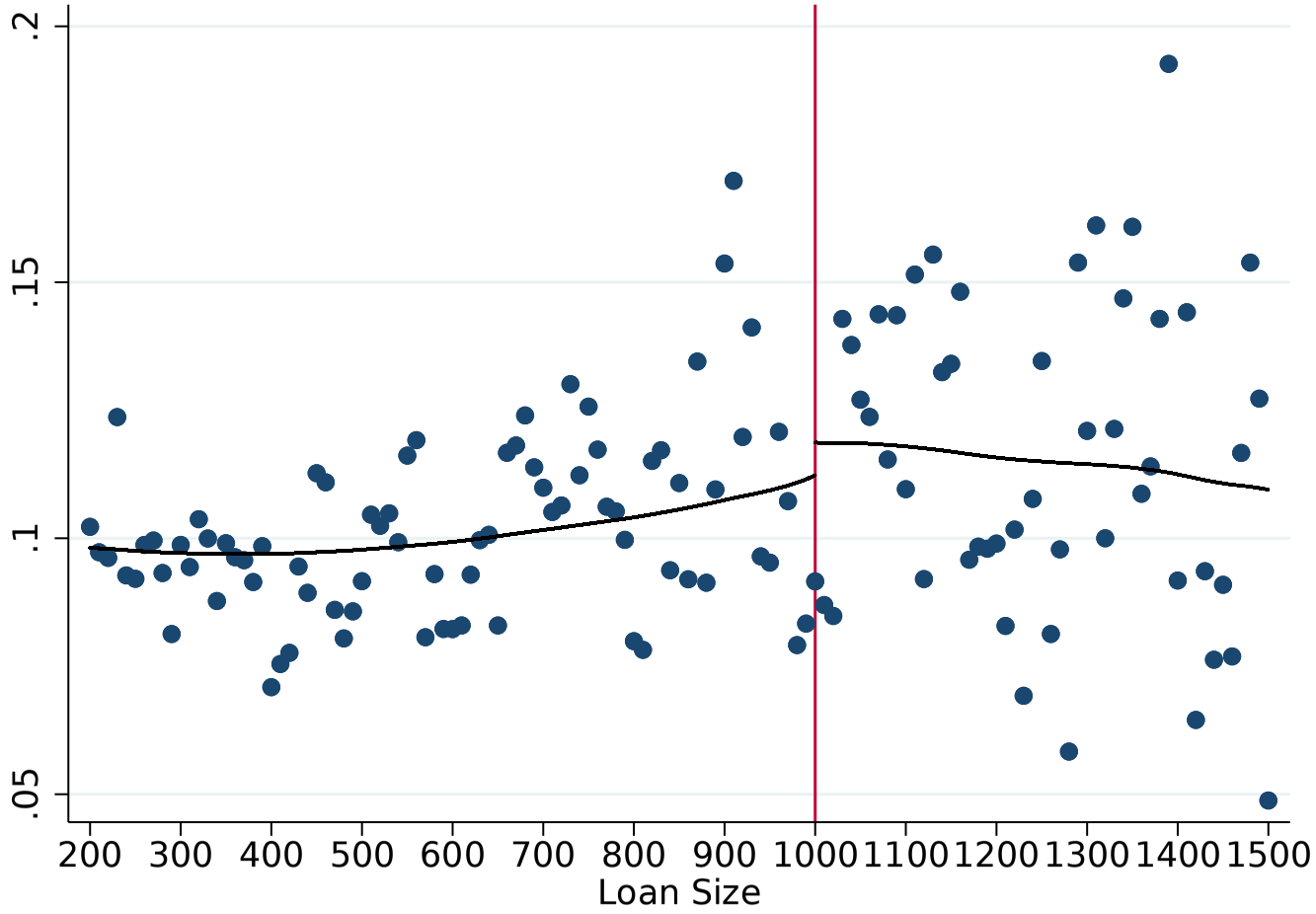
Appendix C Robustness Checks

FiguresC.1-C.2 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 a dummy variable for 20 million peso loans (plus 0.1% to account for fees). Also, the table shows the magnitude of our coefficient increases from 14.4 percentage points to 16.9 percentage points with the addition of these controls.

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.

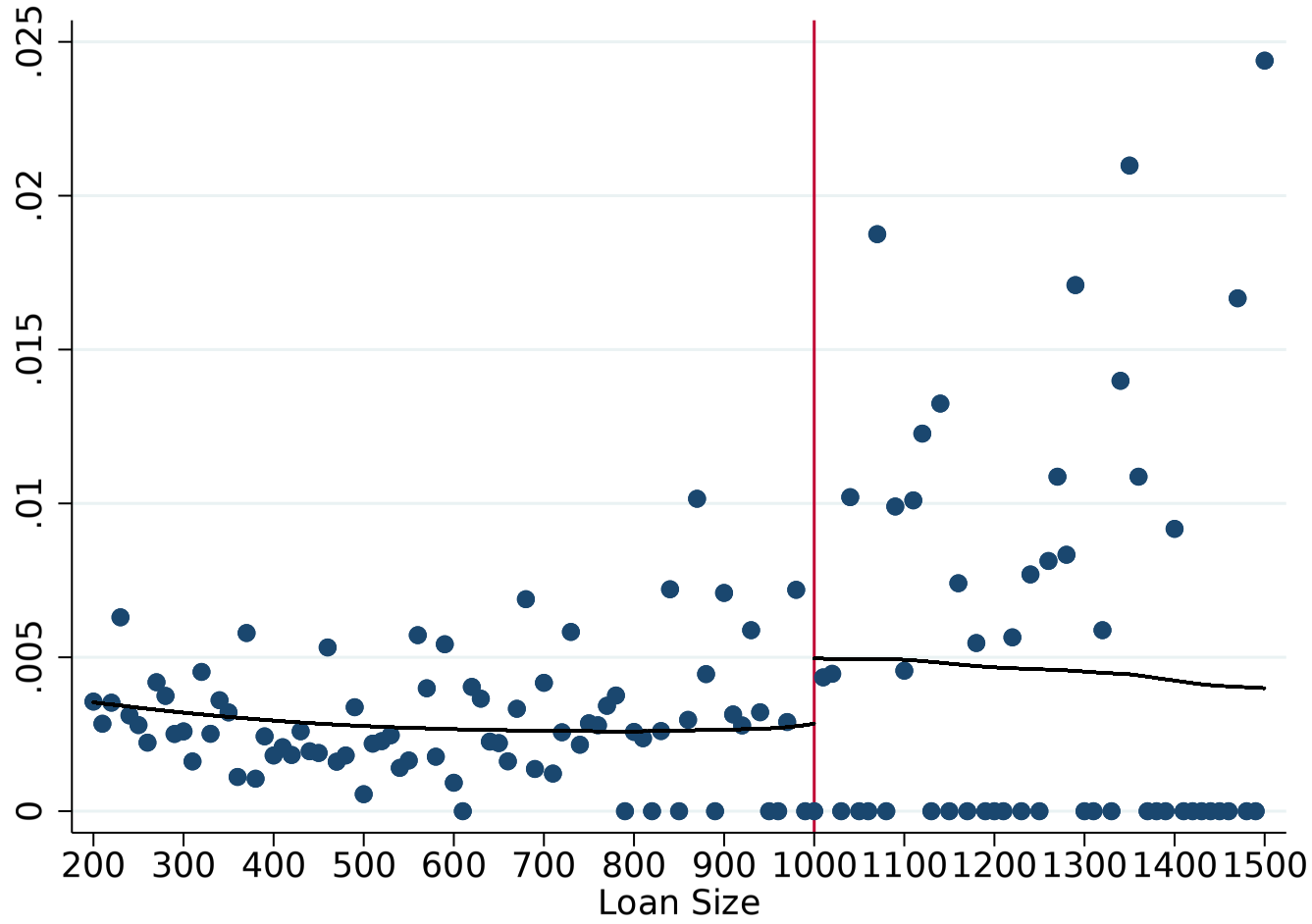
Lastly, we run the regression discontinuity restricting the loan size slope coefficients to zero in Figure C.7 and Table C.2. We still find that the discontinuity is significant at the 5% level, though the coefficient decreases to 8 percentage points from from 14.4.

Figure C.1: Global Polynomial - Ever Delinquent



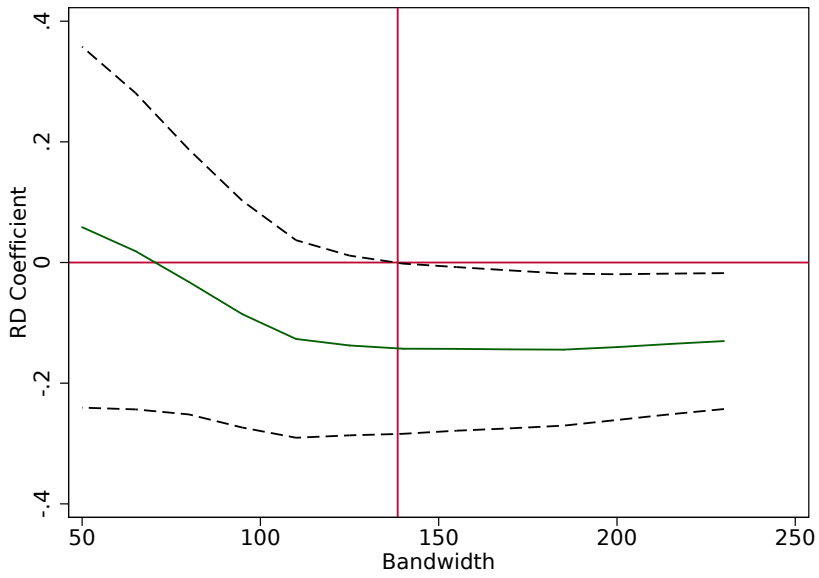
Notes: Global polynomial drawn to best fit ever delinquent rate on each side of the 1,000 UF loan cutoff (red vertical line).

Figure C.2: Global Polynomial - Ever Default



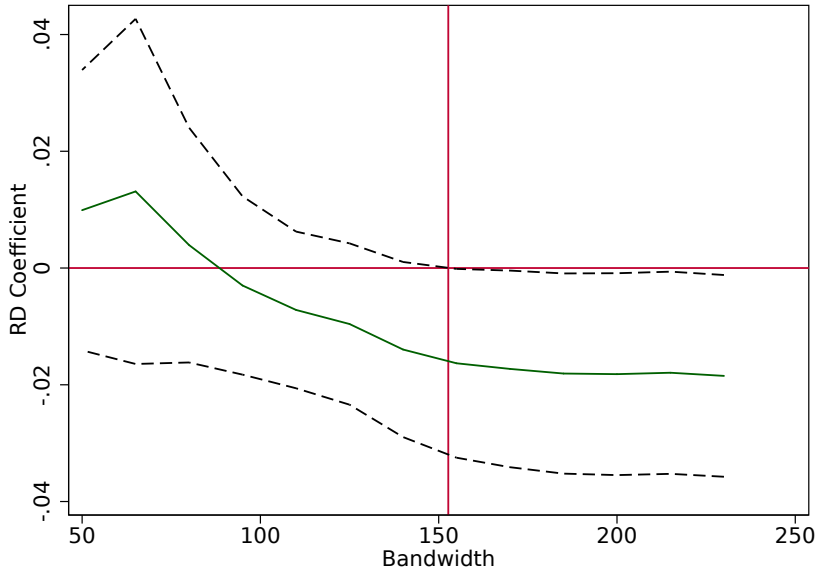
Notes: Global polynomial drawn to best fit "ever default rate" on each side of the 1,000 UF loan cutoff (red vertical line).

Figure C.3: Regression Discontinuity Bandwidth Sensitivity: Delinquency



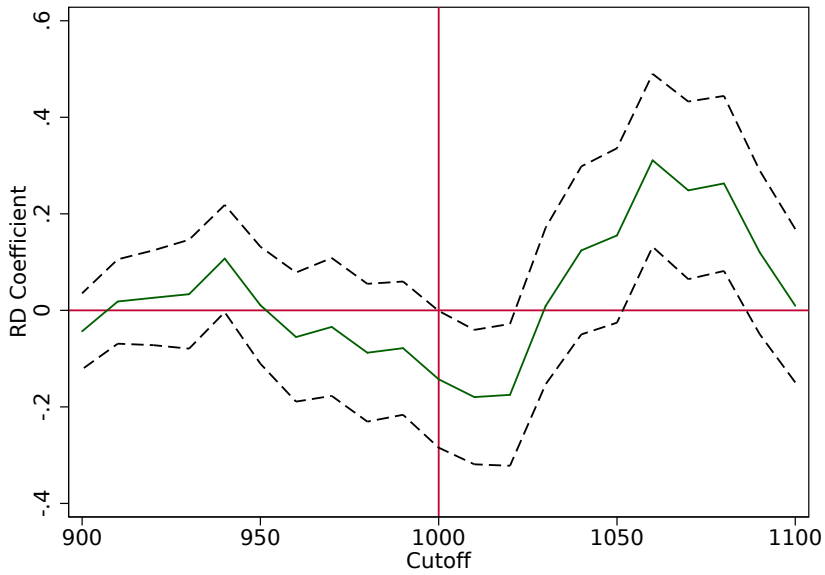
Notes: Figure C.3 graphs the regression discontinuity coefficient estimates of equation 1 with 95% confidence intervals for varying levels of bandwidths. We vary the bandwidth in intervals of 10 UF and graph the corresponding coefficients and confidence intervals. The vertical red line corresponds with the optimal bandwidth chosen by the procedure outlined in Calonico et al. (2014) and Calonico et al. (2018).

Figure C.4: Regression Discontinuity Bandwidth Sensitivity: Default



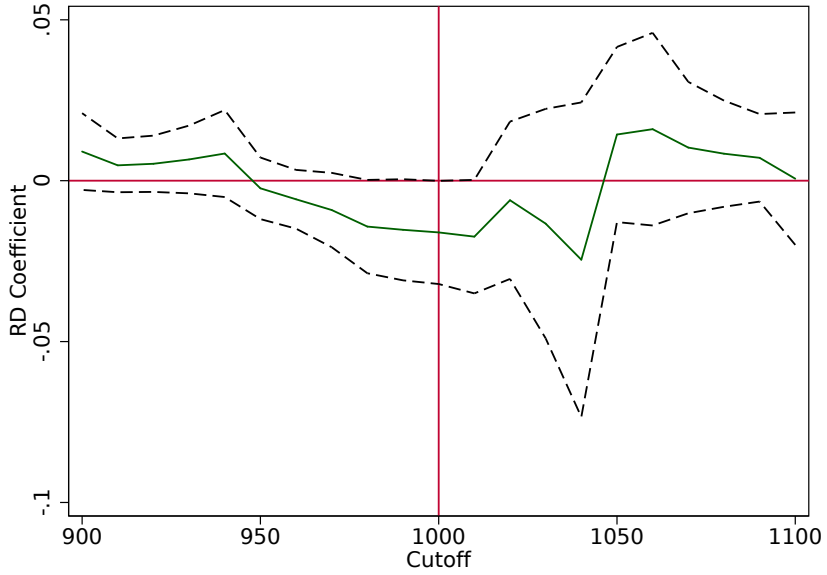
Notes: This figure graphs the regression discontinuity coefficient estimates of equation 1 with 95% confidence intervals for varying levels of bandwidths. We vary the bandwidth in intervals of 15 UF between 50 UF and 230 UF and graph the corresponding coefficients and confidence intervals. The vertical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

Figure C.5: Regression Discontinuity Cutoff Sensitivity: Delinquency



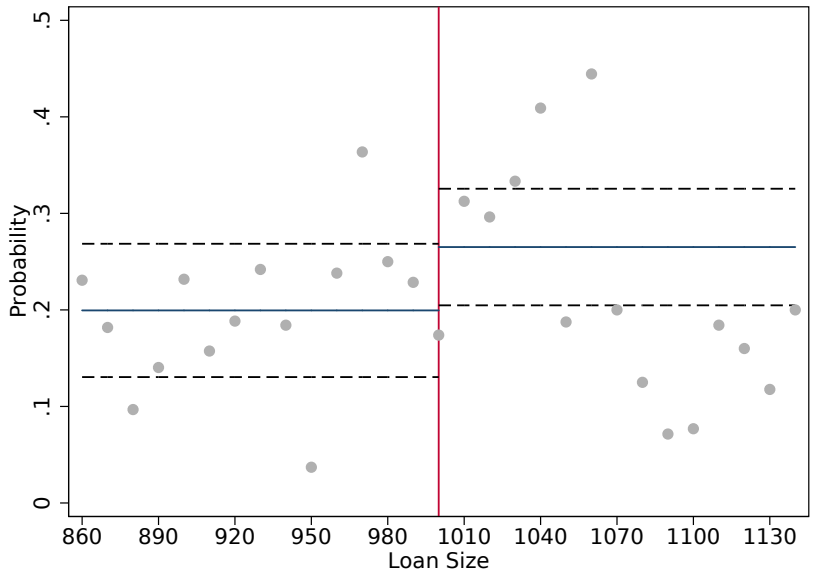
Notes: This figure graphs the regression discontinuity coefficient estimates of equation 1 with 95% confidence intervals for varying cutoffs around loan size. We vary the cutoffs by 10 UF between 900 and 1,100 UF. The vertical red line corresponds with the 1,000 UF bandwidth specified by law 20.448.

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.

Figure C.7: "Ever Delinquent" Regression Discontinuity - no slope



Notes: This figure gives a visual representation to the results presented in Table C.2 of the estimates for equation 1. Loan size controls are not included. All estimates are based on regressions that include fixed effects for comunas (neighborhoods) 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 the 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).

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 product and increased disclosure (transparency) on default, delinquency, and maturity extensions using additional controls. All estimates are based on regressions that include fixed effects for comunas (neighborhoods) 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 the interbank rate are included as controls for aggregate economic conditions. Additional controls presented in this table are outstanding debt, number of outstanding loans, leverage (debt-to -income ratio), and an indicator for loans around 20 million pesos (plus 0.1% to account for fees) to account for bunching around peso values. We use the bandwidth selection procedure outlined in Calonico et al. (2014) and Calonico et al. (2018).

Table C.2: 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.2 gives the estimated effect of a standardized product and increased disclosure (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 (neighborhoods) 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 the 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 9a and B.8 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 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.

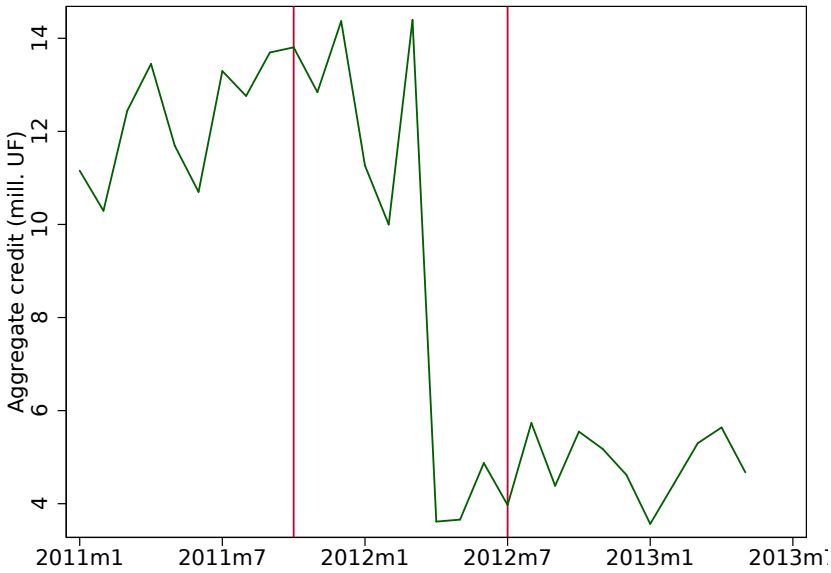
In Chile, there are two different credit registries. The first is a record of the number, amount, and delinquency record of bank loans. This registry is shared between banks by the SBIF and was unaffected by this regulation. The second is a registry of delinquencies for nonbank and bank lenders that 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.9). Given that banks did not increase their relative 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 by 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 10 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 as shown in Figure 9a. 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 suggest 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 im-

Figure D.1: Aggregate Credit

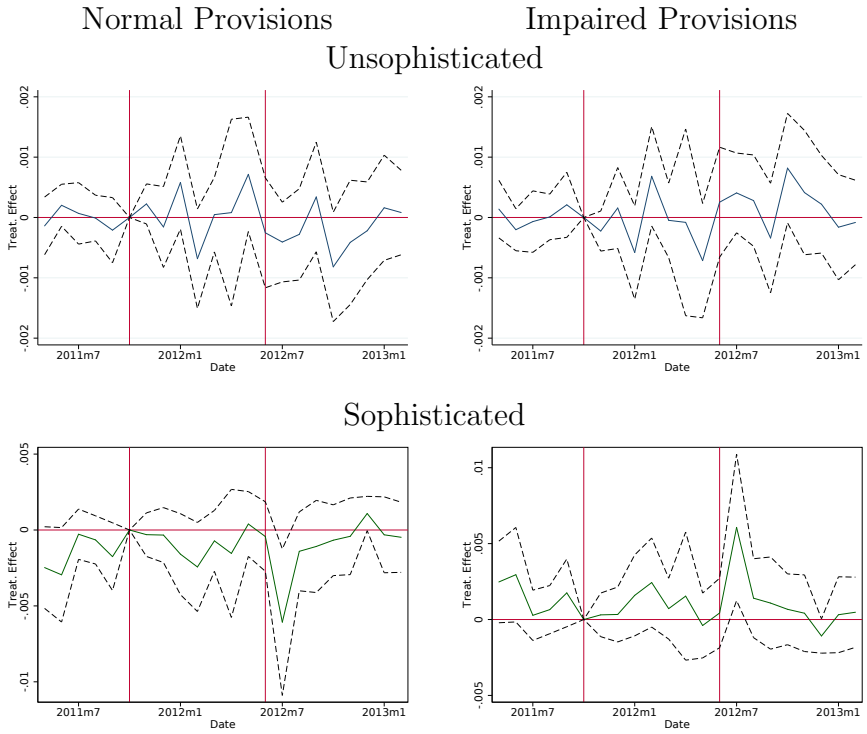


Notes: This figure graphs the sum of all loan amounts (in millions of UF) of newly issued loans by issuance date. The first red line is the implementation date of law 20.448 (the introduction of Universal Credit contracts) and the second red line is the implementation date of law 20.555 (disclosure requirements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

proved relative to that of the sophisticated borrowers. This makes sense as the more educated borrowers were more likely to use bank loans rather than nonbank credit (Liberman et al. 2018) and were thus more likely to experience fewer information asymmetries. Therefore, our patterns in delinquencies around March 2012 seem more consistent with lenders maintaining the same selection standards for the borrowers with more than high school education while raising them for the control group. However, Figure 10 also shows that around this time more sophisticated borrowers actually improved their credit risk, received lower interest rates, and lowered their debt amounts despite higher delinquencies relative to our control group. Further, substantial changes in both delinquency and credit risk after the introduction of the disclosure policy suggest that our findings for sophisticated borrowers are not affected by borrower selection due to credit registry deletions.

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

Figure D.2: Credit Provisions



Notes: Estimates of βs from equation(3) for borrowers in neighborhoods 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 two 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.