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Evidence from the Credit Card Market

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## Are Information Disclosure Mandates Effective? Evidence from the Credit Card Market\*

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**Abstract:** Consumer protection in financial markets in the form of information disclosure is high on governments agendas, despite the fact that the empirical evidence on its effectiveness is scarce. To measure the impact of Truth-in-Lending-Act-type disclosures on default and indebtedness, as well as of debiasing warning messages and social comparison information, we implement a randomized control trial in the credit card market for a large population of indebted cardholders. We find that providing salient interest rate disclosures has no effect, while social comparisons and debiasing messages have only a modest effect. Other types of disclosures discussed in the paper could have larger effects.

**Keywords:** Credit cards, information disclosure, truth in lending, Mexico.

**JEL Classification:** D12 , D14, D83, G02, G21, G28.

**Resumen:** La protección al consumidor en los mercados financieros, en forma de divulgación de información, ha cobrado importancia en la política de los gobiernos a pesar de que es escasa la evidencia empírica sobre su efectividad. Con el fin de medir el impacto sobre incumplimiento y endeudamiento de divulgaciones de información en el estilo del "Truth in Lending Act", así como de mensajes de advertencia sobre sesgo en creencias y comparaciones sociales, se implementó un experimento aleatorio en el mercado de tarjetas de crédito para una población grande de tarjetahabientes endeudados. Se encuentra que mostrar de forma llamativa la tasa de interés no tiene efecto, mientras que las comparaciones sociales y advertencias tienen efectos modestos. Otros tipos de divulgación de información discutidos en el artículo pudieran tener efectos más grandes.

**Palabras Clave:** Tarjetas de crédito, provisión de información, protección al consumidor, México.

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## Introduction

The recent financial crisis and the advent of behavioral economics have placed renewed focus on consumer protection in the financial sector. The US recently created the Consumer Protection Bureau and mandated new information disclosures in the Credit Card Accountability Responsibility and Disclosure Act (known as the Credit CARD Act) of 2009. Many countries have followed suit, requiring financial institutions to report more information.<sup>1</sup> However, important questions remain unanswered; for instance, how effective these disclosures are, which are better, and for which types of consumers.<sup>2</sup>

In order to be able to talk about effectiveness, one needs to understand what these disclosure laws are trying to achieve. The precursor to these laws, the Truth in Lending Act (TILA) of 1968, was motivated by a desire to standardize how the price of a loan was quoted. It was thought that facilitating price comparisons would “protect the consumer against inaccurate and unfair credit billing and *credit card* practices,”<sup>3</sup> “[enhance] economic stabilization... by the informed use of credit,” as well as “[strengthen] competition among the various financial institutions.” Recent modifications have basically the same objectives. Academic literature also provides motivation for price disclosures. For example, Stango and Zinman (2011a) find that similar consumers pay substantially different interest rates for credit cards, while Ponce et al. (2012) find that Mexican cardholder’s debt allocation is insensitive to

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<sup>1</sup>In the US, Title X of the Dodd-Frank Act created the Consumer Protection Bureau, the agency responsible for promoting fairness and transparency for financial products and services. In 2009, the Obama Administration produced the Credit CARD Act which, among various requirements, mandates more information disclosure, in monthly statements in particular. Among a wide range of disclosures, it requires card companies to specify the time it would take to holders to pay off existing debt if only the minimum required payment is made. The publication of APRs and interest rates has been a requirement since 1968. The Office of Information and Regulatory Affairs (OIRA) recently emphasized the importance of ‘smart’ information disclosure (see “Disclosure and Simplification as Regulatory Tools” (June 18, 2010), downloaded in May 2012 from [http://www.whitehouse.gov/sites/default/files/omb/assets/inforeg/disclosure\\_principles.pdf](http://www.whitehouse.gov/sites/default/files/omb/assets/inforeg/disclosure_principles.pdf))

<sup>2</sup>Referring to Truth-in-Lending-Act (TILA) disclosures, Durkin and Elliehausen (2011) recently wrote “The degree to which such disclosures can protect consumers is still a matter of debate, and it deserves careful consideration.” On the one hand Julie Willams, acting Comptroller of the Currency in 2005, believed financial disclosure policy had not worked well for consumers and had unnecessarily burdened banks (see her 2005 speech “Remarks before Women in Housing and Finance and The Exchequer Club”). In contrast, former Chairman of the Federal Reserve System’s Committee on Supervision and Regulation of Banking Institutions, R. Kroszner, was optimistic on their effectiveness (see his 2007 speech at George Washington University).

<sup>3</sup>Quoted from the first paragraph of the TILA. The use of italics is the authors’ own to emphasize that credit cards were the only type of credit mentioned explicitly in the Act’s stated purpose.

differences in the interest rates of the cards they hold, a fact which costs them dearly. Both findings could be explained by unawareness of interest rates resulting from high search or attention cost.

This paper seeks to provide a rigorous answer to two questions. First, do the most common disclosures in TILA-type regulations have an effect on the risk, indebtedness, and switching of credit card holders? How large are these effects? Second, are nonstandard disclosures, such as warnings and social comparisons, more effective at inducing changes in behavior? The answers to these questions are important since disclosure requirements are often mandated and amended without evidence, imposing costs on financial institutions and diverting the attention of policymakers.

We were particularly interested in running a horse race between TILA-type disclosures and more innovative disclosures, such as warnings and peer comparisons. The Mexican Banking Commission (CNBV) encouraged us to pursue this agenda, since it was contemplating sending personalized warnings triggered by risky consumer behavior and acknowledged that rigorous evidence was needed. To this avail, the CNBV paired us with a large Mexican bank to test several such messages. Together with the bank's marketing department, we designed seven of these. The first two were inspired by laws such as the TILA: one included a personalized interest rate and the other a measure of time to pay, reported as the number of months it would take the client to pay off his or her debt if he/she paid only the minimum amount due (MTP). Both of these disclosures feature prominently in the US disclosure mandates, the latter being added in the Credit CARD Act in 2009.

A second set of messages, not present in TILA-type regulations, were inspired by the psychology literature on peer comparisons<sup>4</sup> and by a recent paper in economics by Chen et al. (2010) showing that individuals care about being below or above average in terms of performance on a task. We try to use these findings to induce lower debt levels. To this aim we designed four "social comparison" messages, two of which inform the client that his or her credit card debt is above the mean for similar clients, one of these provides broad advice while the other does not. The other two comparison messages told the consumer whether he or she has a high risk or a low risk of default, respectively, compared to similar clients. A final, nonconventional message gave an explicit warning against overconfidence in paying down debt. It was inspired by current labeling on food, tobacco, and drug products, but also by Ausubel (1991)'s conjecture of the existence of a large fraction of overconfident

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<sup>4</sup>For an early account see Festinger (1954) and the vast literature after this paper.

consumers. Initially, the bank’s personnel believed that none of the messages would have any impact.

To test client responses to these messages, we implemented an extensive blind field experiment in conjunction with the bank. We selected a random sample of 167,190 clients whose credit card payments were not more than 89 days past due in September 2010 from the upper tercile of the bank’s risk distribution<sup>5</sup>. These costumers turned out to be highly indebted, unaware of their interest rate, and overconfident as to their ability to pay down their existing debt as we document below. As will be described later in the paper, the messages were randomized to treatment and control groups so that causal inference could be made more straightforwardly. We measured the effect of these messages on (a) account closures by the client (as a proxy for switching cards),<sup>6</sup> (b) interest paying debt (as a proxy for demand sensitivity), and (c) delinquency (as a proxy for risk, the opposite of stability), measured as a dummy variable that turns on if the client has an overdue payment of 30, 60 or 90 days.

In light of the strong policy emphasis on disclosures, some of the findings are surprising. We find that even when disclosed saliently, the interest rate *does not* change levels of debt, delinquency or the probability of the account being closed by the cardholder. This zero effect is quite precise and robust across subsamples. We also estimate little treatment response heterogeneity. This result is particularly striking given the low awareness environment among our sample. The other TILA-type message the MTP actually *decreased* payments by about 10 percent of its mean and *increased* delinquency by 8.6 percent of the mean for the population that pays interest regularly.

On the positive side, we find that non-TILA messages *are* effective, even when the information provided is quite coarse. The “high risk” message was particularly useful in decreasing delinquency, with an effect of 5.6 percent on mean delinquency, and was the only one that had any effect on account closures by clients. The “low risk” message actually *increased* delinquency by 65 percent. The “high debt” peer comparisons induced a reduction of around 1 percent in mean debt but had no effect on delinquency. We found no evidence that providing a call to specific actions in the form of general advice increases the efficacy of the message. Finally, the warning message also reduced debt by 1 percent and had no incidence on delinquency. All

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<sup>5</sup>Mexican Law mandates that banks hold reserves as a function of the probability of default based on an official formula described later in the paper.

<sup>6</sup>We did not measure switching cards, but did measure voluntary client closures, which could be interpreted as switching if the client has another card or will have one in the future.

effects were short-lived, lasting only one or two months. We show that the failure to detect effects is not due to low statistical power.

Despite their small effects, some messages are cost effective even if we consider the effects on a bank's balance sheets for only two months: the cost of sending a message is around 2.5 pesos, whereas the benefit is in the order of 40 pesos in expected loss reductions, along with an increase of around 20 pesos in profits. To this we would have to add any benefit to the consumers. The bank's personnel were surprised at these results and said they will begin sending the effective messages to their population at risk of default. We do not evaluate the effect of this information on consumer welfare but, given the smallness of the responses induced, it is likely to be small.

That responses to our messages were relatively small *does not* imply that the same would be true of other messages. We believe that for information disclosures to have larger impacts they have to be made easier to understand and actionable. For instance presenting aggregate of interest and fees incurred in the previous 6 months in peso terms, not percentages. Or presenting savings in pesos from switching to cheaper banks based on calculations of clients with similar credit scores. Furthermore, to enable the consumer to respond to information it should be easy to switch banks, ideally "a click away".

There is a growing literature that studies the effect of information provision on choices in many settings and which finds significant impacts. For example, Jensen (2010) provides information on the returns to a college degree; Dupas (2011) on the relative risk of HIV infection by partner's age; Hastings and Weinstein (2008) on school test scores; Jin and Leslie (2003) on restaurant hygiene grades; Bollinger et al. (2011) on food calories, and Bertrand and Morse (2011) on payday lending. Surprisingly, few studies have been carried out on disclosures in the credit card market, despite the fact that credit cards command considerable attention in policy circles and disclosure regulation.

Most studies on credit card information disclosure look at the effect of different mock statements on an individual's understanding or awareness, rather than on their actual behavior.<sup>7</sup> Ferman (2011) and Stango and Zinman (2011b) are two important exceptions. Ferman (2011) randomizes interest rate disclosure and the interest rate itself in credit card fixed-repayment plan offers in Brazil. In line with

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<sup>7</sup>See for example Soll et al. (forthcoming). Early studies: Shay and Schober (1973), Day and Brandt (1974), and Durkin (1975) look at the effect of the TILA itself on awareness, though do not include a control group.

our results, he finds small effects of information on payment plan take-up and take-up/interest elasticities. Stango and Zinman (2011b) study the effects of the TILA itself on interest rates. Overall, they find no effects on average interest rates, but do find lower interest rates for borrowers who underestimate APRs.<sup>8</sup> Another strand of literature studies consumer responses to information that is not standard in TILA disclosures. However, neither of these studies the credit card market. One highlight is Bertrand and Morse (2011) which studies payday loans in the US and shows that providing APR comparisons has no effect on subsequent borrowing –in line with our results– but that providing information on cumulative dollar cost does reduce by 5.4 percentage points the likelihood of payday borrowing in subsequent cycles.<sup>9</sup>

We were able to capitalize on the above literature and, in several respects, go further. First, by having a unified multi-arm experiment, we were able to run an internally valid horse race between TILA-type messages and other types of information. Second, we complement Bertrand and Morse (2011) and Ferman (2011) by focusing on the intensive margin, whereas they focus on loan take-up. This is relevant since large segments of the population already have credit cards and laws such as the TILA emphasize disclosures in monthly statements (i.e., for people that already have cards). Third, we study how information affects outcomes such as indebtedness, default, and account closures, all of which have been unstudied in the literature and are closely related to the outcomes that TILA-like regulations seek to affect. Fourth, we examine the credit card market, an extensive market that has been blamed for the increases in US bankruptcy filings in the late 90s (White (2007)) and one that has been a primary focus of TILA disclosures. Finally, we complement experimental evidence with a policy-induced variation of an actual and recent TILA-like regulation in Mexico for which we have a well-defined control group.

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<sup>8</sup>Using a triple-difference design around a 1981 regulatory change that decreased TILA enforcement for financial companies as compared to banks, Stango and Zinman (2011b) find that, after the regulatory change, borrowers who underestimate APRs are more likely to pay more on instalment loans taken out with financial companies than those who taken out with banks.

<sup>9</sup>In a public good context relating to movie ratings online, Chen et al. (2010) show that telling users that they rate fewer movies than the average user leads to a fivefold increase in the number of movies they rate subsequently. Allcott (2011) finds that, in the US, sending letters comparing the energy use of households to that of similar neighbors leads to a fall of 1.1 percent to 2.8 percent in energy consumption.



## I. Context and Data

Before we describe the field experiment, we outline the context in which it was carried out. The CNBV was thinking about issuing rules that mandated banks to send personalized warnings based on the risk profile of clients. At the same time our partner bank was seeking ways to reduce delinquency in their credit card portfolio, but had a healthy skepticism about the effectiveness of messages. They agreed to work with us to design and send information messages to measure their impact, but only for their riskier clients. We focused on such population by drawing a random sample from the upper tercile of the risk distribution of clients, based on the CNBV methodology to predict the probability of default. This methodology assigns a predicted probability of default (PD) in the next 12 months to each credit card based on card use according to a logistic function with five regressors: number of consecutive months delinquent (CD), number of total months delinquent in the last six months (D), tenure of the card (T), last month’s payment as a proportion of the minimum payment due (MP), and last month’s percentage of credit line used (LU).<sup>10</sup> When drawn in September 2010 the PD ranged from 9 percent to 100 percent with a mean of 26 percent. As described in Section II, a treatment group of this sample received messages in February 2011.

Obviously this sample is not representative of the bank’s entire clientele, but we argue that it is the type of population at which consumer protection laws are directed. Indeed, the patterns we found in the two surveys we implemented in this sample display low contract-terms awareness and high indebtedness. Section V reports additional results for representative populations based on samples from two other banks.

### A. Administrative Data

The data available to us consists of monthly information on the variables that appeared in the monthly statements for the selected 167,190 credit cards in the period from September 2010 to June 2011. These variables include interest-paying debt, account closings by clients, delinquency (30, 60 or 90 days overdue indicators), payments, purchases, interest rate, credit limit, fees, etc. We have limited demographic information as the bank does not collect it. We used administrative data to follow

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<sup>10</sup>The exact formula is given by  $\frac{1}{1+\exp(-2.9704+0.6730CD+0.4696D-0.0075T-1.0217MP+1-1513LU)}$ . The model fit and predictive power are high: ROC curves of 86 percent and prediction error in back-testing of less than 5 percent of mean default.

client behavior as this has the virtue of containing virtually no measurement error and is cheap to collect.

Table 1 shows that indeed clients are highly leveraged and risky. It also shows that interest rates are high and clients somewhat new. Average interest-paying debt (our main outcome variable) was around 18,000 pesos while mean income according to our survey was close to 9,000 pesos, so clients seemed highly leveraged. Mean card utilization was 70 percent of the credit line. Clients were also risky: the estimated ex-ante probability of their defaulting in the subsequent 12 months was 26 percent and default (more than 90 days past due) was already 9 percent by February 2011. Figure 1 plots a histogram of this probability measured in September 2010, when the sample was taken, and in January 2011, just before the messages were sent.<sup>11</sup> The expected loss per account as calculated by the bank proprietary formula was 2,721 pesos on average in September 2010, which explains why the bank wanted to induce a behavioral change in these clients.

We created a dummy variable called “Delinquent” which takes the value of one in a month if there are payments that are 30, 60 or 90 days overdue. This is our main measure of ex-post risk realization and one of our three main outcome measures. In an average month 11 percent of accounts are classified as delinquent by this standard. Clients do not close their accounts often. Only 2.6 percent of accounts had been closed by the client five months after the sample was selected. Counting also the closing of accounts by the bank the number increases to 4.4 percent. By April 2011 we have an attrition rate of just above 9 percent but we do not think it is a serious problem to our analysis since it is balanced across treatment and control groups (see Table 9 in Appendix).

Interest rates are high and stable at 44 percent per year. In fact, our bank is persistently among the top 5 in terms of highest interest rate charged. Moreover, the interest rate was not highly correlated with risk, which means that clients with similar risk profiles were paying very different interest rates even within this same bank. A regression of interest rates against deciles of the internal probability of default and months dummies yielded an R-square of 0.01. Finally, there was little variation in interest rates over time. The within-card standard deviation of the yearly interest rate was 7 percent, and in only 4 percent of observations did the interest rate change by more than 0.4 monthly percentage points (5 pp in terms of yearly rates)

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<sup>11</sup>We wish to emphasize that we are covering a broad domain of default probability and that this study is not just about clients with an extreme likelihood of default. In fact, some clients had zero default probability in January 2011.

from one month to the next. This means that being informed as regards interest rates and trying to obtain a lower rate (even within this bank) may pay off.

Lastly, we want to note that the average number of months to pay current debt with no further purchases and making the minimum payment due was 27. As we will show below, this quantity is much greater than what the people in our sample expected. Clients in the experiment were 42 years old on average and that they had less than 4 years tenure of their card.

### *B. Survey Data*

In December 2010, before sending the messages, we conducted a telephone survey among a random sample of around 800 clients included in our administrative data. The main purpose of the survey was to measure the clients knowledge of interest rates and elicit their beliefs about the months they would take to pay off their debt if they paid the minimum payment and had no further purchases. We also asked whether they read the monthly statement, how satisfied they were with current information disclosures, and their predictions as regards their paying down their debt in next 1, 2 and 3 months.<sup>12</sup> The questions and mean responses are tabulated in the Appendix.

We wish to highlight three lessons learned from the survey. First, these clients were uninformed as regards their interest rate: only 3 percent of clients claimed to know their exact interest rate and 37 percent claimed to know it approximately within 5pp. This happens in spite of Mexican Law mandating interest rate disclosure in the bank statements.<sup>13</sup> Second, clients underestimated the number of months needed to pay off their debt, on average believing it to be 11 months rather than the 27 months indicated by the real data. This overconfidence is also captured more directly: 35 percent of the consumers surveyed claimed to have overestimated their ability to pay down their debt in the previous six months.<sup>14</sup> Third, 92 percent of clients claimed

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<sup>12</sup>We also conducted an ex-post survey of around 2,800 clients in the last quarter of 2011. Since the bank did the survey and the attention span of respondents in phone surveys is short, the questionnaires were kept brief. Unfortunately, we were unable to include a question on what they thought their interest rate was that would later enable us to contrast this with the true rate in the administrative data.

<sup>13</sup>See monthly statement in the Appendix.

<sup>14</sup>Ausubel (1991) conjectured that people care little about interest rates in the credit card market because they wrongly believe they will not incur any interest. To test his conjecture we asked clients to make a prediction on whether or not they would have more or less debt in the following two months compared to their present debt and verified whether the prediction was ex-post correct using the administrative data. Around half got it wrong; interestingly, about 3/4 of these erred on the side of overconfidence, thinking that their debt would decrease when in fact it increased,

to read their monthly statement carefully, which is somewhat surprising given how uninformed they were as regards interest rates. Part of the explanation may be that the information is hard to read. For instance, 42 percent said they would prefer a clearer statement and 38 percent claimed default happens because people do not realize how fast they are accumulating debt, and not because of strategic default or due to unforeseen shocks.<sup>15</sup>

Overall, a substantial proportion of the clients were unaware of interest rates, unsatisfied with the clarity of their monthly statement, and displayed signs of overconfidence regarding their paying down their debt.

## II. Experiment Design and Model Specification

### *A. Experiment Design*

The aim of the field experiment was first to test whether information and warning messages indeed induced a change in behavior, and secondly to test which message was more effective in inducing behavioral change. We compared TILA-type disclosures to more innovative disclosures, such as warnings and peer comparisons. As we have previously stated, we teamed up with a bank to design and send seven messages.<sup>16</sup>

The first two messages were inspired by the disclosures that are typically mandated by laws such as the TILA. In particular, we sent a message disclosing the personalized interest rate very saliently, and a second message displaying the number of months it would take a consumer to pay off his or her debt if making only the minimum payment due without further purchases. Let us call these messages the “interest rate message” and the “months-to-pay message” (MTP), respectively. Both of these disclosures feature prominently in the US law.

The second set of messages was inspired by literature that stresses that people are influenced by what their peers do, either through the signal that this behavior

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lending support to Ausubel.

<sup>15</sup>In this paper we do not attempt to quantify consumer welfare, but through the survey we tried to capture the client’s opinion about their indebtedness and about the net benefit of defaulting. Over 4/5 said they would like to decrease their debt even taking into account the sacrifices this would imply, and over 9/10 said that defaulting would decrease their welfare taking the benefits of defaulting into account.

<sup>16</sup>Working with the bank offered the advantage that it enabled us to use its experience in marketing, though we had to adhere to the bank’s communication protocols.

provides, such as in rational herding models, or through a conformity channel. We designed four of these peer-comparison messages: two of these informed the client as to whether his or her card's debt was above the mean of clients of the same gender, similar credit limit (as a proxy for income), age, and risk: their "peer group". These two "high debt" messages differed only in terms of whether broad advice was provided or not. The other two comparison messages told the client their relative risk of default, again compared to similar clients. One message was sent to clients with a high probability of default warning them about it and the other message to clients with a low probability of default as a congratulatory note; we call these "high risk" and "low risk" messages respectively.

Finally, we included a message which did not contain any direct comparison but rather an explicit warning against overconfidence in paying down debt. We call this the "warning message". We thought this message was interesting because the clients in our survey seemed overconfident as regards paying down their debt and because these types of warning messages are common in health disclosures (e.g., "smoking kills") but have been understudied. This message could increase attention even when no hard information is provided. In a recent paper, Stango and Zinman (2013) show that surveying people about their card overdraft fees seems to cause them to pay less in fees, even when the survey does not contain much information in this regard. They interpret this as evidence of inattention.

Figures 5, 6 and 7 show some of these messages, the rest can be found in the Appendix. At this point, we wish to highlight two facts: first, with the help of the marketing department at the bank we made these messages very salient. Font sizes of around 50 points were used for the relevant amounts and the language used was as simple as bank communication protocols would allow. We believe the salience of the messages is an upper bound on the salience that TILA-like laws typically mandate. Second, unlike the "interest rate" and "months-to-pay" messages, the peer comparisons were coarse in the sense that they were not tailored to particular individuals. We could have told each individual exactly where he or she was in the distribution of debt for example. We did not do this as it was simpler for the bank, but as we shall see we still found impacts for this coarse information. We discuss our hypothesized effects of these messages in subsection B below.

The allocation of messages to clients was random within their cells. The randomization design was done as follows: since some messages involved comparisons among similar clients, we had to create an operational definition of what it meant to be

similar. To this end, we stratified the sample into cells by crossing four variables –gender, quintiles of age, quintiles of credit limit, and terciles of predicted default probability– to produce 150 cells in total. Next, within each cell we identified clients who had debt that was above the cell mean. These were candidates for receiving the “high debt” message. When we take into account the high-debt stratification, we effectively have 300 strata. Clients within a stratum constitute a peer group. Randomization into some treatment (77,175 messages) vs control (90,015 no-messages) was performed within each stratum to provide us with an appropriate control group.

We started by allocating the “high debt” message to about 85 clients in each of the 150 high debt strata for each of the two high debt messages. This meant we had 12,850 clients for each of these messages.<sup>17</sup> Next within each of the 300 strata we identified which unassigned clients had above (below) the median predicted probability of default and randomly allocated these to the “high (low) risk” message: 6,444 to the high risk message and 6,456 to the low risk one. The remaining unassigned treatment clients were randomly allocated within strata as follows: 12,900 for the interest rate and the debiasing warning message, respectively, and 12,825 for the months-to-pay message. Table 1 in the Appendix shows that randomization worked to balance the variables across groups.

The timing of the experiment (shown in Figure 8) was as follows: the selection of the sample and the randomization into treatments was carried out in September 2010. Messages were printed and sent out in February 2011, using administrative information from January 2011 for personalized messages. From the outside, the envelope was indistinguishable from a monthly statement, but inside it contained only our message, no monthly statement. We were told that the delivery service of the bank is of a very high quality and that more than 95 percent of the clients should have received the message. Although we cannot be sure that they did actually read the message, in the survey 78.3 percent claimed to read their monthly statements and 67 percent claimed to read them very carefully.

### *B. Model Specification*

Since messages were conditionally randomized, we can estimate the average causal effect from the difference in conditional means. We estimate the average treatment effect of message  $T_j$  on outcome variable  $Y$  in month  $t$  by estimating  $E[Y_{it}|T_j =$

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<sup>17</sup>Note that this does not exhaust all the high-debt clients but does leave fewer high-debt clients for the remaining messages and control group. To take this into account, all regressions included a high-debt strata dummy.

$1, S_k] - E[Y_{it}|T_j = 0, S_k]$ , where  $S_k$  are the stratification indicators for the  $k$  strata described above. Since the sample size is large we decided to estimate specifications separately for each month  $t$  and treatment  $j$  as reflected in equation (1), this afford more flexibility in parameters.

$$Y_{ijt} = \alpha_t + \beta_{jt}T_{ij} + \delta X_{i,t-k} + \gamma_t S_k + \epsilon_{ijt} \quad (1)$$

$\alpha_t$  estimates the mean on the control group of the respective message in month  $t$  and  $\beta_{jt}$  is the average treatment effect of message  $j$  in month  $t$ . This is exactly what we did. For specifications involving debt as a dependent variable we included debt measured in January 2011 as a baseline covariate,  $X_{i,t-k}$  to improve precision. We also estimated two related models: one regression with all treatments jointly (as they were randomized orthogonally), and a differences-in-differences specification. The results were similar and are not reported here.

The main outcome variables are interest-paying debt, a delinquency dummy variable, and a dummy for the client closing the account, described in the data section. We focus on these variables because we believe they are important on their own, but also because they are close to the outcomes that the TILA sought to influence. Closing the account is a proxy for switching while changes in debt are a proxy for demand responsiveness, both related to competition. Delinquency is related to the stability that the TILA mentions.

Given that the clients in our sample were highly indebted and at high risk of default, we hypothesized that the “high debt” message, the “high risk” message, and the warning message would reduce debt and default. We had no strong prior expectation as to their effect on account closures. Regarding the salient interest rate message, since this bank has one of the highest interest rates in the market, we expected that revealing the interest rate would decrease debt and increase account closures. The problem with this conjecture is that the client may not know the market interest rate to which he or she can access. A finer test would be to “shock” the client with a lower than expected interest message and see if it induces the client to carry more debt. We implement a test that approximates this scenario below. Finally, we expected that the MTP message would make clients realize they were underestimating their months to pay and induce larger payments, lower debt, and less delinquency on their part. We test these hypotheses in Section III.

Before we proceed to the results, it is important to state that we are not necessarily estimating the effect of *reading* the information –since we do not know if the clients

did actually read it— but we are estimating the effect of sending the information, which is what TILA-type laws mandate. However, we believe that the information did reach a significant part of the sample; note that some messages did have an effect. Furthermore, we are unable to make statements regarding consumer welfare, a characteristic we share with all of the papers we have cited.

### III. Legally Mandated Disclosures:

This section examines the average treatment effect of some of the disclosures mandated by the TILA and the recent Credit CARD Act (though these are shared by laws in many countries), namely, the interest rate and the MTP. We analyze each in turn.

#### *A. Personalized Interest Rate Information*

Probably the most prominent disclosure in TILA-type laws is the price of credit, as reflected in either the interest rate or the APR. In spite of its importance, to our knowledge there are no randomized control trials that measure the impact of increasing the salience of this information on the use of credit cards and their risk of default. In an interesting study, Malmendier and Lee (2011) found that online auction bidders pay, on average, prices above the posted price for the same good. They find, however, that the extent to which this happens is inversely related to the salience of the posted price. Chetty and Kroft (2004) find that tax elasticities are dependent on how salient taxes are. Low salience of the interest rate could rationalize the low interest rate awareness we found in the survey, and presumably a its salient disclosure could remedy it.

Before we proceed to the analysis of the effects of the message, it is useful to provide a brief discussion on how much money is potentially at stake. On average, our clients paid 7,752 pesos a year in interest, which is slightly less than their average monthly reported income. Ideally one would like to compare consumer utility with and without knowledge of interest rates, or at least compare financing cost under these counterfactual scenarios. Unfortunately we have no way of doing this. Instead we report some statistics that suggest (but in no way shows) some clients may be leaving money on the table.

First we note that 3,000 clients in the data have two cards at the bank and these cards do not typically have the same interest rate. Interestingly they carry debt on



the expensive card even when the cheaper one is not maxed out. Figure 3 plots the share of total debt allocated to the cheaper card for months when the rates for the credit cards were different and in which the consumer did incur financing costs. For 44 percent of clients it is the case that more than half the time they could save on interest by reallocating debt from the expensive to the cheaper card.<sup>18</sup> This means that they ended up paying 18 percent more.<sup>19</sup> Second, another interesting statistic is that the yearly interest rate on credit cards at our bank is almost 10pp higher than that of the cheapest of Mexico’s five largest banks; hence the mean consumer could probably save around 1,800 pesos per year from having this debt in the cheapest of these five banks. This amount is greater than the savings reported in Kling et al. (2012) and arguably not insignificant.

So our individuals seem not only unaware of interest rates but could also potentially profit from knowing them. Our conjecture was that reporting saliently the personalized interest rate would lead to a decrease in debt and an associated decrease in delinquency, perhaps through the clients’ substitution towards cheaper cards, for instance, or from just decreasing their total debt. The message might also have caused an increase in closures as clients switch to cheaper cards. But does it? To measure the impact of making the interest rate salient we sent the interest rate message displayed in Figure 5 (a) to a randomized treatment group of 12,825 clients, as described in Section II. We estimate its impact using the specification in equation (1) for the months of March and April 2011 separately, for our three dependent variables. We do not show any results for the months of May and June 2011 in this paper as these were economically small and not different from zero for any of the variables or treatments. Each cell in Table 2 represents an estimate of  $\beta_{jt}$  from equation (1). Dependent variables are displayed in columns and subsamples in rows.

From the first column, we can see that the effect of the interest rate message on debt in March is -36 pesos only. This is around 0.2 percent of mean debt and is not statistically different from zero. The effect on delinquency and closures is also not different from zero and is economically minuscule. One potential explanation for the finding is that the message may not have reached the clients. This is unlikely as the

<sup>18</sup>On average, the yearly difference in interest rates across the cards held by any one client is 4pp.

<sup>19</sup>We do not claim that this figure demonstrates that behavior is suboptimal since there may be other costs and benefits of reallocating debt. We present this evidence where as a teaser, but Ponce et al. (2012) provide an in-depth analysis of this issue using another dataset and give arguments in favor of a sub-optimality interpretation.

bank followed the same mailing procedure used when it sends monthly statements, though more importantly because we *do* find effects for other messages. Another possibility is that the stakes are low or that many clients do not revolve debt. To address these possibilities, the second row considers clients who revolved debt for 10 consecutive months before receiving the message; rows 3 and 4 consider populations that have above-median interest and above-median debt, respectively. We again find a zero effect. A third alternative is that clients have limited ways to decrease or substitute this debt. Row 7 considers clients that had other credit cards in any other bank, hoping to find larger debt elasticities.<sup>20</sup> Again, there is no effect.

We consider a further potential explanation for the zero effect. We have already documented that 97 percent of the clients in our sample claimed not to know their precise interest rate, and 67 percent not even approximately. However, we do not observe what they think the interest rate is. It may be the case that the treatment effect we estimate combines responses to good and bad news that cancel each other out. To investigate this, we first estimate equation (1) in a subsample for whom the message was most likely good news. In a second test, we classify any change—either positive or negative—as a positive change by using the absolute value of the change in debt as a dependent variable, therefore avoiding such cancelations when averaging. Section V presents results when we allow for a random coefficient in the treatment dummy.

For the first test, we exploit the fact that banks in Mexico often temporarily reduce the interest rate for subsets of consumers (see Ponce et al. (2012)) to induce them to spend. In our data, 6 percent of accounts had reductions of more than 10pp in January 2011. If we assume that the client’s expectation of the level of his interest rate is equal to an average of the recent interest rates, receipt of message which incorporates the low interest offer is likely good news. We estimate a specification in which the response to treatment is interacted with a dummy that indicates whether or not the client received a low interest offer in January 2011 when the message was printed.<sup>21</sup> We find that the interaction effect is statistically zero (not reported in the table). The second test is reported in Row 8. When  $|Debt_t - Debt_{jan2011}|$  is used as the dependent variable we again find a zero effect. We conclude that effects are

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<sup>20</sup>We obtained this information from the Credit Bureau. Unfortunately, we do not observe which card is more expensive, although our bank is in the top 5 in terms of highest credit card interest rates. We do not observe how full the other cards are either.

<sup>21</sup>The equation is as follows:  $Y_{it} = \alpha_t + \beta_{1t}T_i + \beta_{2t}T_i LowR_{Jan} + \theta LowR_{Jan} + \delta X_{i,t-z} + \gamma S_k + \epsilon_{it}$ . We are interested in  $\beta_{2t}$ .

not canceling each other out, instead there is really no response. This is consistent with our findings in Section V.

Why does the salient disclosure of the interest rate not cause any change in behavior then? Our preferred interpretation is that the interest rate is an abstract concept that does not help clients decide how much debt to have. For example, Lusardi and Tufano (2009) show that people score badly on questions relating to compound interest, while Bertrand and Morse (2011) show that providing APR comparisons has no effect on subsequent payday loan borrowing but that providing information on cumulative dollar cost does. Ideally one would like to test if reporting money costs generate larger impacts, but the bank did not want to try these out.

### B. Months to pay outstanding debt

For many consumers, paying their card debt is not an easy task and many pay close to their required minimum.<sup>22</sup> Given that such minimum payments are approximately 5 percent of debt, this implies that clients take a long time to pay. In our data, for 12.9 percent of clients, making the minimum payment due implies never paying off their debt, even if they make no further purchases. For the remaining observations, the mean number of months to pay (MTP) is 27 and the 99th percentile is 83 months.<sup>23</sup> Such long payment periods are worrisome since there is evidence that actual payment anchors on the minimum payments (e.g. Stewart (2009)). Figure 2 plots a histogram of actual MTP calculated from the administrative data vs MTP as reported by the clients themselves. Clearly many clients are grossly underestimating the amount of months.

Due to these types of concerns, the policymakers that enacted the Credit CARD Act (2009) required card companies to disclose the *number of months consumers will take to pay off debt if they stop purchasing and only pay the minimum amount due* (MTP). The logic was that giving consumers information on the time burden for paying their debt would make them more debt-conscious and lead to faster debt decreases. We expected that when consumers were made aware of their overconfidence, they would indeed decrease their debt and pay more.

To measure the impact of this disclosure, we randomly sent 12,900 messages with the personalized number of months to pay. The exact design is shown Figure 5 (b).

<sup>22</sup>In our sample, 6 percent of those clients that pay above the required minimum pay within 1 percent of the minimum and 20 percent pay within 10 percent of the minimum.

<sup>23</sup>The number of months to pay off the current debt balance if the minimum is paid and if no further purchases are made is given by the following formula 
$$N = -\frac{\ln(1 - \frac{Debt * MonthlyInterestrate}{MinPay})}{\ln(1 + MonthlyInterestrate)}$$
.

It informed cardholders of their personalized MTP, explicitly advising them to pay more than the minimum amount due. The results are reported in panel B of Table 2. Contrary to our expectation, the message had null effects, except that it caused delinquency to *increase* for those that often paid interest or had high debt. For these, the effect was an increase in delinquency of 0.0109 points in April, equivalent to 8.6 percent of its mean value. The amount of payments actually went down by about 10 percent of its mean (unreported in the table). This response was not what we expected and, we suspect, contrary to the expectations of policymakers. Although the bank lost money from this, we have no way of telling how it affected consumer welfare. There was no detectable effect on debt or account closures in the main sample or for the subsample of clients who had above the median MTP (fourth row), above median debt (third row), or for those that typically pay interest (second row).

Since the message does not affect income or debt, we believe that the delinquency it induces must be strategic in the sense of not being forced by circumstances. One interpretation of the increase in delinquency is that some clients are discouraged by finding out that there are still too many months in which to pay interest, which may seem unfair or unfeasible, and therefore decide to stop paying or pay less today.

#### IV. Non-Legally Mandated (Behavioral) Disclosures

While TILA-type laws have concentrated on disclosing information on contract terms, in this section we measure the effect of messages directly related to actual consumer behavior and comparisons with the behavior of their similar others. Peer comparisons have been shown to be effective in many contexts: inducing participation in elections, encouraging contributions to online public goods, and increasing savings on energy. We test their potential reducing debt.

Besides comparisons, we also test a warning message aimed at “debiasing” clients with respect to their ability to decrease debt. This is pertinent given that as documented above our consumers seem to display overconfidence. The only papers we are aware of that measure response to warnings against biases are those by Cummings and Taylor (1999) and List (2001). They show that debiasing individuals by warning them of the bias in answers to hypothetical valuation questions can help individuals to approximate true valuations.

##### *A. Peer-Group Comparisons*

We are agnostic about which are the channels through which peer comparisons induce behavioral change. It may reflect sophisticated rational behavior. One could imagine an environment in which individuals with similar preferences are subject to common but only partially observable income shocks, where each peer observes a signal of the shock. In such a context, observing the actions of others would convey information about the state of the shock and push the individual toward performing a similar actions than his peers. Alternatively, there may exist a tendency toward conformity that is present directly in preferences. We do not attempt to distinguish between these forces.

In a recent paper, Chen et al. (2010) showed that movie raters respond sharply to peer comparisons. When individuals were told that they rated fewer movies than the median rater, the number of movies they rated increased fivefold. Inspired by these results, we sent a similar message informing clients if they had above-average debt (or were high-risk). Since our sample of clients was highly indebted and risky to start with, we conjectured that these messages would decrease debt and delinquency. We also tested a message that congratulated the client for his or her low risk, displaying a thermometer indicating a “low risk” reading.

The messages are admittedly coarse, yet in spite of this we still find significant effects. We considered telling clients what their exact location in the distribution of debt and risk was or how much debt other clients in their strata had, but it was simpler for the bank to implement the message in Figure 7 as it not personalized.

Figures 6 and 7 display three of the messages sent, the other two can be found as Figures 2 and 3 in the Appendix. The main lines of the “High Debt + Advice” message said “with respect to this group, your debt is HIGHER than the average of people similar to you.” A footnote explained that the group was composed of people of a similar age, income, and the same gender, but no further details were provided. It then gave three broad pieces of advice: analyze your ability to pay, pay at least twice the minimum payment due, and decrease your debt. We also sent another message identical to this one except that we omitted the explicit advice in order to enable us to measure the effect of the advice per se. There was no deception in the experiment; cardholders that received the “High Debt” message really were above the mean debt of clients in their cell. We measure the treatment response against a control group of clients in the same cell who also had above-mean debt by including the strata dummies.

Table 3 reports the results for all nonstandard treatments. Row 4 shows that the

“high debt” message reduced debt by 152 pesos, which amounts to 0.6 percent of its respective mean, whereas the message that included the broad advice in Row 3 had no effect on debt.<sup>24</sup> So if anything the advice was detrimental in this case, although both magnitudes are economically small. Neither of the two messages influenced delinquency or account closures.

Presenting information on the probability of default was harder, the marketing department of the bank argued that their average client would not grasp the concept. Therefore, it decided to present the information graphically in the form of a thermometer. The thermometer was in the high temperatures when the client was above the median probability of default. The bank also decided to congratulate clients who were below the median probability of default and show the thermometer in the low temperatures. Row 1 reports the average impact of the “high risk” message and Row 2 the respective estimate for the “low risk” message. As expected, the effects across these two messages move in opposite directions and give more confidence to our causal interpretation. The “high risk” message caused a decrease in debt of 176 pesos and a decrease in delinquency of 1pp, about 6 percent of mean delinquency. The “low risk” message increased delinquency by 65 percent of its respective mean in both March and April. It is interesting to note that only in those messages that refer to risk do we see an impact on delinquency.

Clients that received the “high risk” message were more likely to voluntarily close the account (with respect to their control group obviously), with an increase of 0.8pp (26 percent of mean value!). Only for this message is there any effect in account closings. It could be that they found the message offensive. This may explain why banks are extremely prudent in their communications with clients.

### *B. Debiasing Warning Message*

Our message is shown in Figure 6 (b). Our intention with this message was to increase awareness that paying down debt is hard and to de-bias consumers by explicitly telling them that people are typically overconfident in their ability to pay down debt. The Mexican Law on disclosure actually talks about warning messages (leyendas precautorias), though the authorities have yet to define what these are.

The bank sent this message to a randomized treatment group of 12,900 consumers. Results in row 5 of Table 3 show that the message did decrease debt, although again,

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<sup>24</sup>We estimated regressions comparing the probability of paying close to twice the minimum amount comparing messages with and without the advice; there was no evidence that the advice was followed.

to a very limited degree, -132 pesos on average on March and -154 on April. Effects of delinquency have negative signs but are not statistically significant at conventional levels.

All in all, the nonstandard disclosures were more effective at reducing delinquency than TILA-type disclosures. However, their average effects are still small and in our opinion unlikely to have any major impact on consumer interest payments, financial sector stability, or competition.

## V. Heterogeneity, Specification Checks and Power

### A. Response Heterogeneity

Section III reported zero average effects for the interest rate disclosure. We performed some checks that suggested this was not driven by responses canceling each other out in the average (the good news vs bad news interpretation). In this section we explore treatment effect heterogeneity explicitly by allowing the response to depend on  $i$ . We estimate the following equation with random intercept and random treatment coefficient using data from March to June for each consumer<sup>25</sup>:

$$Debt_{it} = \alpha + \nu_{1i} + (\beta + \nu_{2i})T_j + \gamma S_k + \lambda_t + \epsilon_{it} \quad (2)$$

The results are reported in the Appendix (Table 2). One highlight is that the mean of  $\beta_i$  is -36 and its 95 percent confidence interval is (-486,414).  $\nu_{2i}$  has a small standard deviation, its 95 percent confidence interval is [5.35e-24,1.86e+18]. This means that there is very little heterogeneity in the response to treatment, casting doubt on the good news vs bad news interpretation for the small estimated average effect. Figure 4 shows graphically the extent of heterogeneity by plotting plots the empirical Bayes prediction of the random coefficients for the treatment  $\nu_{2i}$ . The distribution has most of its mass on the negative part of the support, suggesting that most clients decrease their debt as a response to the treatment, but the whole support of the distribution is economically tiny and concentrated around zero.

Tables 3, 4 and 5 in the Appendix focus on heterogeneity by splitting samples across some pre-specified dimensions: number of products with the bank (as a proxy

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<sup>25</sup>Month dummies ( $\lambda_t$ ) were included to control for omitted time effects.  $S_k$  represent the stratification controls. We used Stata xtmixed command which assumes joint normality of the error terms  $\nu_{1i}$  and  $\nu_{2i}$  below. Individuals  $i$  (second level) group monthly observations  $t$ , which run from 1 to 5 (first level).

for loyalty), categories of income as reported on the card’s application, and risk (proxied by delinquency in 2010). Here we just want to highlight some results. First, note that the effect on closings when the “high risk” message is received is not present when the card holder has several products with the bank, maybe owing to higher switching costs (Table 3, Appendix). Second, income also seems to matter: high income individuals reduce debt more strongly (4 percent of mean Debt) when receiving the “high debt” message (perhaps because they can afford to do this without foregoing much consumption); while low income individuals respond mainly through less default, with no detectable effect in debt (Table 4).

Finally, we found that previous delinquency does mediate the effects (Table 5). Clients with 2010 delinquencies significantly increased their debt after receiving the congratulating message of low risk, and increased delinquency with the MTP disclosure, while those with no 2010 delinquencies did not. For these later individuals we find larger *risk mitigation* responses to messages. In particular the warning message causes larger debt reductions for this subgroup. Note also that the effect on account closing of the high risk message is concentrated on those with good credit histories (no delinquency in 2010), which is reasonable as it is easier for them to obtain a card at another bank. We obtained the same result in an unreported estimation of equation 1 that interacted a dummy for having a poor Credit Bureau report.

We perform one last exercise here, raised by one important policy question that the CNBV had. The CNBV wanted to issue rules that mandate the use of predicted risk as a trigger of messages. That is, send messages only to those most likely to default in the near future. The CNBV expected larger responses for these individuals, but is it really the case that the message has a larger effect for the more risky clients?

We estimated a version of equation 1 with delinquency as the outcome variable, where we additionally include quintiles of CNBV’s predicted probability of default (PD) and interacted them with the treatment messages. Figure 10 plots the coefficients in the interactions<sup>26</sup>. It turns out that all the messages had statistically significant effects for the highest PD quintile, which supports CNBV’s conjecture. Interestingly (although hard to rationalize) the “high debt” messages appear to be highly effective to decrease risk for low risk individuals while they increased risk for high PD clients.

Putting the magnitudes of the coefficients into perspective, we interpret that there

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<sup>26</sup>For the “high risk” and “low risk” message we only plot the last three quintiles and the first three quintiles respectively because these messages were sent to these subpopulations.



is little heterogeneity, except perhaps at the very top of the risk distribution. We therefore think that the main message of the paper is robust across a very wide subset of the population.

### *B. Power, Confidence and Specification checks*

Statistical power is crucial in studies that cannot reject the null hypothesis of a zero effect. We simulated placebo treatments of different sizes for January 2011 (i.e., just before treatment) and used the regression specification in equation (1) so that we had the same sample and the same methodology as that employed with the real treatments, just two months before treatment<sup>27</sup>. Figure 11 shows that the design/sample has substantial power: we can detect an effect size of 455 pesos in debt (1.6 percent of mean debt) and 0.006 of delinquency (4 percent of its mean) with 80 percent power. We believe these are relatively small effects, as does the bank.

We also ran an specification test by estimating equation (1) in the same partition of control and treatment cards for the months of September and October 2010, that is before treatment. If the equation was misspecified, we would expect more than 95 percent of coefficients to be significant at the 5 percent level. Table 5 shows this was not the case: only one coefficient out of 28 was significant at the 5 percent level. This increases our confidence that the significant coefficients we found in Tables 2 and 3 are not due to sampling variance.

Since we are estimating the effects for several treatments and several dependent variables, we may be finding spurious statistically significant effects by failing to account for multiple testing. This seems unlikely as we did not find effects in Table 2 for the Interest Rate message or in Table 4 for the months before treatment. Furthermore, the effects that are statistically significant have the expected sign and are present only one or two months after the treatment and not later. Underestimation of p-values are not a major concern for the paper as we are arguing that the effects are zero or very small and inconsistent with policymakers' large emphasis on the importance of disclosures.

### *C. External validity*

In the introduction we mentioned the fact that that the main motivation behind the TILA was to enable interest rates to be compared more easily. One could ar-

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<sup>27</sup>Other months worked similarly.

gue that the information provided in our interest rate message caused no response because this information was not useful as it gave no benchmark for comparing the interest rate to that of other banks. Indeed Kling et al. (2012) show that small comparison frictions can have significant effects. Another argument against our previous findings is that external validity is limited in three ways. First, we studied only one bank. Second, we considered only a risky population. Finally, and potentially more importantly, we sent the message only once. A higher frequency of messages could have had a greater impact.

External validity questions are hard to address since by definition they are beyond the scope of the study sample. However one of the authors was able to run more experiments at other banks for another paper (Negrín and Seira (2014)). In this section we report some of those results. The experiment reported in this subsection goes out of sample in many ways that address the concerns above: it was carried out at two different banks from the one referenced in this paper and was representative of all their clients, not just the risky<sup>28</sup>; the price message was more aggressive as it involved direct price comparisons across bank; and the frequency of the message was varied randomly.

The experiment was motivated by the TILA's emphasis on price comparison and a new disclosure mandate from Mexico's central bank. In 2011, the Central Bank mandated disclosing the interest rates and APRs of *competitor banks* for similar cards in monthly statements every 6 months, separately for classic, gold and platinum cards. Figure 9 contains an example of the price comparison printed with the monthly statement. One could argue that this is a very aggressive mandate, it is uncommon to force companies to advertise the prices of their competitors when many of these prices are actually lower. Actually we know of no other country which requires this. In the Appendix we estimate the effect of the first time consumers received this message in April 2011, using consumers in our main sample and quasi-experimental methods. We found no effects.

Since we were interested in varying the frequency of the message one of the authors designed an experiment in conjunction with the central bank. One group of clients in each of two banks received the price comparison of Figure 9 in April 2012 once, another group (the frequent treatment group) received it *monthly* 7 times from April 2012 to October 2012, while a third group acted as a control group and received

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<sup>28</sup>With these two additional banks the population of this paper would easily include more than half of Mexico's total cardholders.

no messages at all during all 2012. Group size was approximately 30,000 clients per arm, representative of the banks' overall population of cardholders (no selection on risk was made).

To analyze the resulting behavior we estimated equation (1) by ordinary least squares (there were no strata). We estimated the regression separately for each bank and month. Figure 12 plots the estimated  $\beta$ 's, scaled down by the average of the dependent variable, for ease of interpretation. Panel A reports the results when debt is the dependent variable and Panel B when it is delinquency. As can be seen, the effects are very small economically speaking for all banks, all messages and all periods, i.e., always less than 5 percent of the mean and, more often than not, less than 1 percent of the mean of the dependent variable. None of the coefficients were statistically different from zero at the 10 percent level. Importantly for us, we tested and found that message frequency made no difference. These results are a powerful demonstration that our main results seem to be more general than just a specific interest message in a specific bank sent once.

## VI. Conclusion

This paper shows that currently mandated disclosures are likely to have zero effect and that alternative messages that include peer comparisons and warnings are probably more effective, though even so, only to a small degree.

Small effects do not imply money-losing effects. We estimated the same specification with the expected loss in pesos (calculated by the bank) as the dependent variable for the "high risk" message. The treatment group had a 40 pesos lower realized losses in March and April 2011 compared to its corresponding control group. Given that printing and sending the letter cost 2.5 pesos, this message was clearly beneficial to the bank. A sign of the usefulness of the message is that the bank now intends to use it and the authorities aim to mandate such a disclosure.

This study certainly does not rule out the possibility that other messages could have greater effects. If we could repeat the experiment, we would try using a message in which the client is told how much clients with the same credit score and type of card as them are paying at the cheapest bank and an estimate of how much savings *in pesos* they would make in a year by switching to that bank. Bertrand and Morse (2011) show that adding up several months and putting quantities in money terms has worked in other contexts, while Kling et al. (2012) show that direct and

personalized comparisons among providers have been effective. Kling et al. (2012) also show that minor frictions can have large effects. Indeed, optimization frictions à la Chetty (2012) could also potentially explain the small sensitivities we estimate. If this is the case, then facilitating direct comparisons and easing procedures to switch banks are important complementary policies to information disclosure.

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VII. Tables and Figures

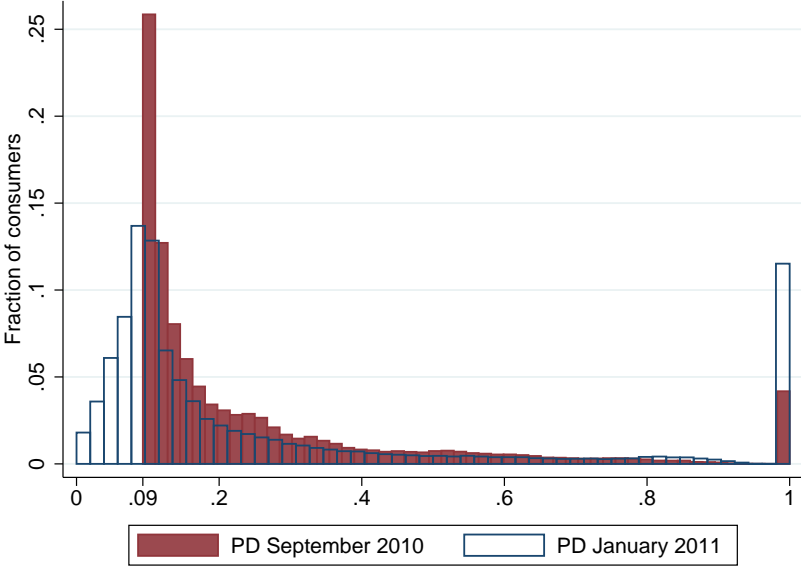


Figure 1: CNBV Probability of Default

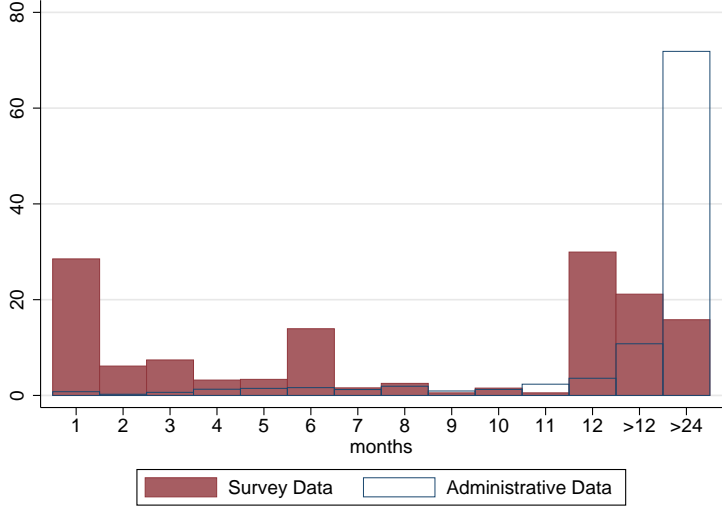


Figure 2: Beliefs on months to pay debt against actual administrative data

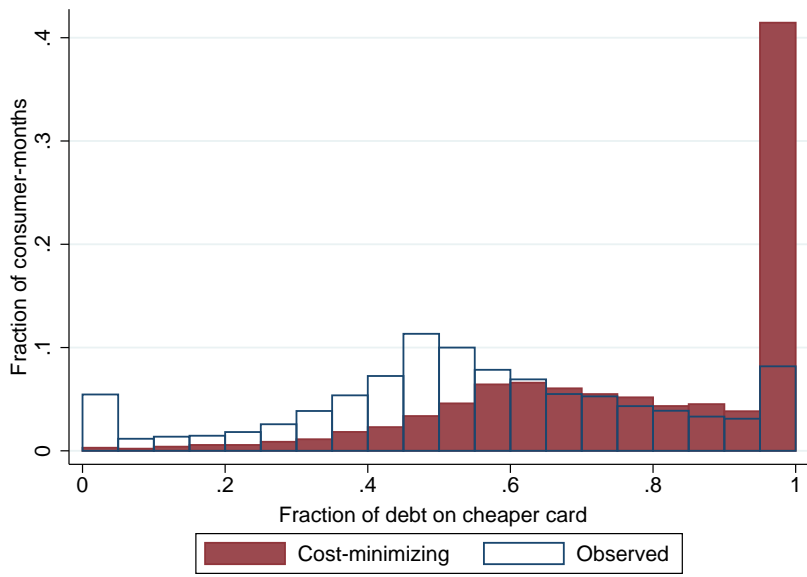


Figure 3: Debt Allocation

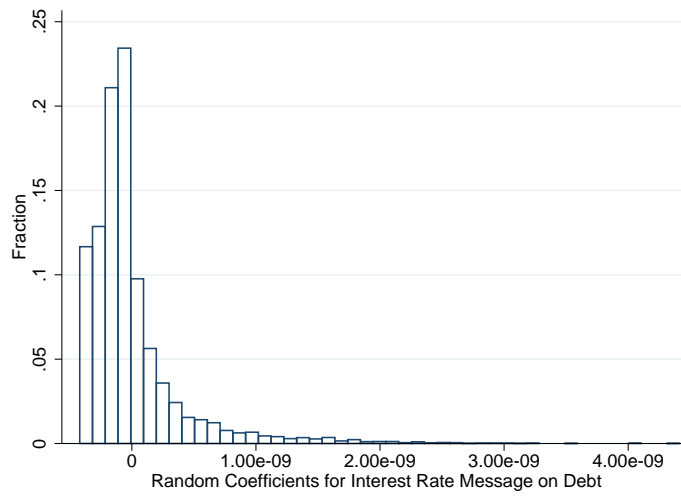


Figure 4: Empirical Bayes Prediction for Interest Rate Message



Dear XXXXX,

We want to help you keep your finances healthy. Your interest rate plays a crucial role in interest generation and in increasing debt.

Pay careful attention to your card's interest rate.

INFORMATION AT STATEMENT DATE			
Credit Limit	\$0,000.00	Annual Personalized Rate	XXXX%
Available Credit	\$0,000.00	Annual Rate	XXXX%
Average Daily Balance	\$0,000.00	Annual Investment Rate	0.00%
Annual Penalty Rate	00.00%	Statement Date	27 MAY 2010
Total Annual Cost (w.o. tax)	00.00%	Days in Cycle	30

XXXX%

You can find this information in your bank statement.

(a) Interest Rate Disclosure

Dear XXXXX,

We want to help you keep your finances healthy. That's why, in your bank statement, we tell you the number of months it will take you to pay off your debt if you only pay the minimum amount due.

SUMMARY OF CTIVITY		
DETAILED TRANSACTIONS OF JANE DOE 0000 0000 0000		
Date	Description	Amount
14 MAY	PAYMENT	\$0,000.00
27 MAY	INTEREST SUBJECT TO TAX	\$0,000.00
04 MAY	ANNUAL FEE	\$0,000.00
27 MAY	TAX ON INTEREST AND FEES	\$0,000.00

The time needed to pay of your debt by making only the minimum payment due is: XX months. The amount due in 12 months if you are up to date with your payments is: \$000.00. This does not consider purchases, interests, cash advances or fees incurred after the present statement date.

XX months

If you want to take less time, consider paying more than the minimum.

(b) Months-To-Pay-Off-Debt Disclosure

Figure 5: Salient Legally Mandated Messages

The figures present an English version of the messages sent in the experiment. This is the precise format used, except that the originals were in Spanish.

Dear XXXXX,

We want our clients to have healthy finances. That's why we have analyzed the credit behavior of a group of cardholders.

With respect to this group **your debt** is:

**HIGHER**  
than the average of  
people similar to  
yourself\*

To reduce this risk, we recommend you do the following:

Analyze your ability to pay and budget your monthly expenses.

Pay at least twice the minimum amount due in order to reduce the time it will take you to pay off your debt.

Maintain your debt well below your credit limit.

(a) High Debt + Advice

Dear XXXXX,

We want to help you keep your finances healthy.

**Don't get confident**  
**Paying off a debt is**  
**not that easy**

Many studies have found out  
that consumers overestimate their  
ability to pay and  
fail to service their debts.

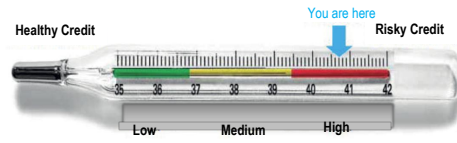
**Don't let it happen to you!**

(b) Warning

Figure 6: High Debt and Warning Messages

Dear XXXXX,

Based on your credit behavior, we have detected that your credit card has the following **probability of default**:



To reduce this risk, we recommend you do the following:

- Analyze your ability to pay and budget your monthly expenses.
- Pay at least twice the minimum amount due in order to reduce the time it will take you to pay off your debt.
- Maintain your debt well below your credit limit.

Figure 7: High Risk Message

The “low risk” message is analogous but the arrow is placed over the lower legend on the thermometer and the client is congratulated. See the Online Appendix.

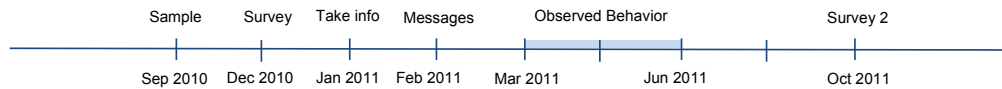


Figure 8: Experiment Timeline

Institution	Product	CAT (%)	Weighted Average Effective Rate (%)	Annual Fee (pesos)	Credit Limit (median value in pesos)*
Santander	Santander Light	31.4	24.4	430	17,821
BBVA Bancomer	Azul Bancomer	34.9	26.8	460	11,500
Banco Inbursa	Clásica Inbursa	41.4	35.1	0	4,300
Scotiabank	Tasa Baja Clásica	44.2	34.0	395	15,000
Banamex	Clásica Internacional	44.7	33.6	500	44,000
HSBC	Clásica HSBC	45.4	34.3	480	13,300
Banorte	Clásica	46.6	35.5	430	15,000
BNP Paribas	Comercial Mexicana	78.3	57.1	250	6,500
BanCoppel	BanCoppel	88.3	65.0	0	4,200
Products that account for less than 0.5% but more than 0.1% of the total number of "classic" cards					
Banco Walmart	Super Tarjeta de Crédito	56.3	43.9	200	3,200
American Express	Blue	56.4	41.8	459	12,000
SF Soriana	Soriana - Banamex	56.7	42.4	420	16,800
Ixe Tarjetas	Ixe Clásica	64.5	47.2	440	5,000
Globalcard	Globalcard	90.5	60.4	684	7,500

Figure 9: APR Comparison Message

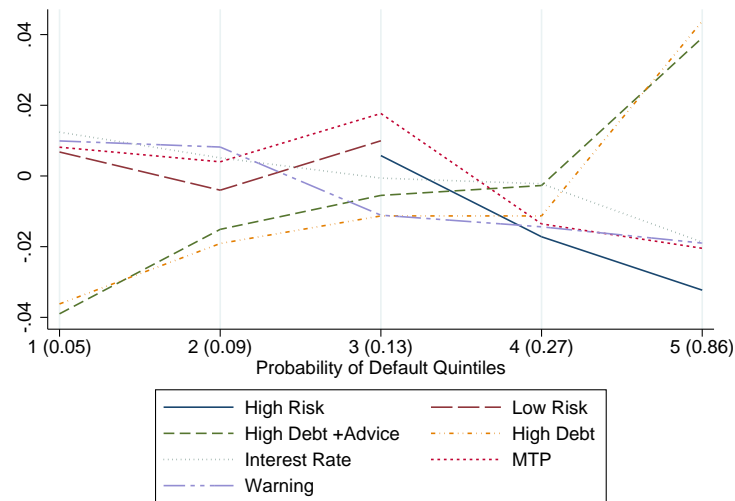
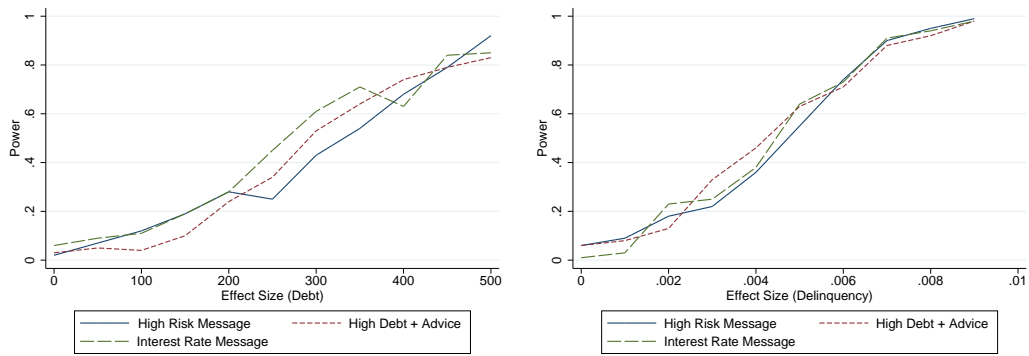


Figure 10: Effects on Delinquency by Probability of Default

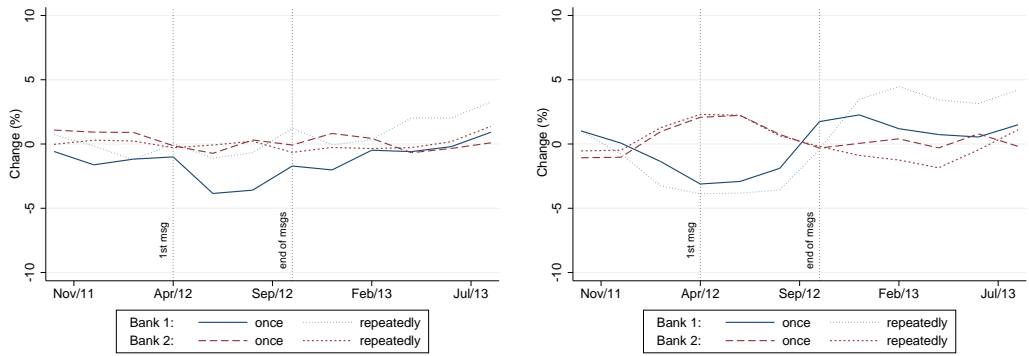
The average probability of default of each quintile is expressed in parenthesis on the x axis.



(a) Debt

(b) Delinquency

Figure 11: Statistical Power



(a) Debt

(b) Number of Delinquent Months in the Previous 6 Months

Figure 12: Effects at Two Different Banks

Table 1: Summary Statistics

	Mean	St. Deviation
<i>Dependent Variables</i>		
Delinquent (%)	11	(31)
Debt (MXN)	17800	(25297)
Debt <sup>a</sup> (MXN)	18415	(25410)
Closed <sup>b</sup> (%)	3	(16)
<i>Other Risk Measures</i>		
Probability of Default <sup>c</sup> (%)	26	(16)
Default <sup>b</sup> (%)	9	(29)
Expected Loss <sup>c</sup> (MXN)	2721	(5841)
Expected Loss <sup>ac</sup> (MXN)	2767	(5844)
<i>Credit Terms and Use</i>		
Credit Limit (MXN)	27502	(35831)
Annual Interest Rate (%)	44	(10)
Monthly Interest (MXN)	646	(896)
Months to Pay	27	(17)
Minimum Payment (MXN)	1490	(3538)
Utilization	70	(38)
Purchases (MXN)	1082	(4365)
Payments (MXN)	1925	(5659)
<i>Demographics</i>		
Age (Years)	42	(12)
Tenure (Months)	43	(26)
Male (%)	57	(49)
Income <sup>d</sup>	8563	(7444)
Observations	3343800	

Credit card variables are expressed in monthly terms.

<sup>a</sup> Conditional on being positive.

<sup>b</sup> Measured in February 2011.

<sup>c</sup> Measured in September 2010.

<sup>d</sup> Proxied by expenditures. Self-reported in the survey. After trimming the top 5 percent

Table 2: Legally Mandated Disclosures

	<i>Dependent Variables</i>					Observations
	Debt		Delinquent		Closed Account	
	March	April	March	April	June	
<i>Panel A: Interest Rate Disclosure</i>						
Full Sample	-36 (62)	12 (80)	-0.0002 (0.0035)	-0.0001 (0.0037)	0.0008 (0.0020)	93652
Paid Interest <sup>a</sup>	-19 (70)	10 (96)	-0.0025 (0.0047)	0.0004 (0.0050)	0.0003 (0.0017)	52257
Interest Rate <sup>b</sup>	-32 (65)	22 (89)	0.0027 (0.0046)	0.0032 (0.0047)	0.0010 (0.0026)	60947
Debt <sup>b</sup>	4 (118)	161 (152)	-0.0078 (0.0058)	-0.0053 (0.0060)	0.0011 (0.0015)	45744
January Rate Offer <sup>c</sup>	-51 (157)	-128 (194)	-0.0079 (0.0079)	-0.0039 (0.0082)	0.0026 (0.0041)	12036
No January Rate Offer <sup>c</sup>	-32 (68)	35 (88)	0.0009 (0.0039)	0.0004 (0.0040)	0.0005 (0.0022)	80653
Another Credit Card <sup>d</sup>	-46 (77)	49 (99)	-0.0022 (0.0042)	0.0002 (0.0043)	0.0021 (0.0023)	70710
Change in Absolute Value	29 (58)	-106 (76)	-0.0005 (0.0041)	-0.0048 (0.0042)		78480
<i>Panel B: Months to Pay Off Debt Disclosure</i>						
Full Sample	37 (95)	87 (105)	-0.0002 (0.0035)	0.0058 (0.0037)	-0.0019 (0.0037)	92621
Paid Interest <sup>a</sup>	-28 (102)	-61 (120)	0.0008 (0.0047)	0.0109** (0.0050)	0.0001 (0.0050)	52436
Debt <sup>b</sup>	87 (174)	185 (195)	0.0022 (0.0058)	0.0115* (0.0060)	-0.0019 (0.0060)	45287
MTP <sup>b</sup>	26 (134)	105 (141)	0.0009 (0.0035)	0.0055 (0.0038)	-0.0030 (0.0038)	46312

Significance level: \* 10 percent \*\* 5 percent \*\*\* 1 percent. Standard errors in parenthesis.

<sup>a</sup> Paid interest in the 10 months prior to March 2011.

<sup>b</sup> Above median in January 2011.

<sup>c</sup> Temporary low interest rate offer in January 2011.

<sup>d</sup> Cardholder has a card at another bank.

Table 3: Non-Legally Mandated (behavioral) Disclosures

	<i>Dependent Variables</i>				
	Debt		Delinquent		Closed Account
	March	April	March	April	June
High Risk	-176** (88)	-137 (116)	-0.0139*** (0.0052)	-0.00553 (0.0053)	0.00776*** (0.0029)
Low Risk	50 (82)	361 (310)	0.0098* (0.0051)	0.0088* (0.0052)	0.0016 (0.0028)
High Debt + Advice	-37 (62)	-129 (82)	0.0008 (0.00379)	0.0048 (0.00390)	-0.0032 (0.00200)
High Debt	-152** (62)	-31 (82)	-0.0037 (0.0038)	0.0000212 (0.0039)	0.0004 (0.002)
Warning	-132** (60)	-154* (79)	-0.0045 (0.0036)	-0.0041 (0.0037)	-0.0019 (0.0019)

Significance level: \* 10 percent \*\* 5 percent \*\*\* 1 percent. Standard errors in parenthesis.  
Each regression uses more than 78,166 observations.

Table 4: Placebo Effects

	<i>Dependent Variables</i>			
	Debt		Delinquent	
	September	October	September	October
High Risk	-307 (323)	-338 (326)	-0.0033 (0.0043)	-0.0032 (0.0046)
Low Risk	381 (322)	421 (323)	0.00967** (0.0041)	0.0046 (0.0044)
High Debt + Advice	1 (244)	26 (245)	-0.0004 (0.0031)	0.0030 (0.0033)
High Debt	-37 (243)	-78 (244)	0.0015 (0.0031)	0.0021 (0.0033)
Rate	60 (228)	6 (229)	0.0004 (0.0030)	-0.0027 (0.0032)
MTP	17 (228)	16 (230)	0.0006 (0.0030)	-0.0026 (0.0032)
Warning	16 (227)	46 (229)	-0.0014 (0.0030)	-0.0013 (0.0032)

Significance level: \* 10 percent \*\* 5 percent \*\*\* 1 percent. Standard errors in parenthesis.

Each regression uses more than 94,000 observations.



# Appendix

## A. Treatment/control balance

Table 1. Treatments and control balance: September 2010 final sample<sup>a</sup>

	All	High Risk	Low Risk	High Debt + Advice	High Debt	Rate	MTP	Warning	F test <sup>b</sup>
Delinquent	0.1348 (0.3415)	0.2449 (0.4300)	0.0150 (0.1217)	0.1515 (0.3586)	0.1537 (0.3607)	0.1264 (0.3324)	0.1265 (0.3325)	0.1256 (0.3314)	0.9573
Probability of default	0.2551 (0.2261)	0.4052 (0.2604)	0.1143 (0.0169)	0.2473 (0.2095)	0.2455 (0.2073)	0.2591 (0.2341)	0.2589 (0.2342)	0.2587 (0.2329)	0.9451
Debt (MXN)	18919 (25800)	15638 (23776)	17118 (24053)	24960 (29099)	24922 (28636)	16245 (23959)	16196 (23632)	16311 (23643)	0.9807
Tenure with Card (months)	43 (26)	46 (27)	41 (25)	42 (24)	42 (25)	44 (26)	44 (26)	43 (26)	0.1603
Credit Limit	27287 (35165)	27050 (34159)	26692 (35280)	28059 (34987)	27932 (34390)	26864 (35358)	27029 (35152)	26998 (35238)	0.8274
Age (years)	42 (12)	42 (12)	41 (12)	42 (11)	41 (11)	42 (12)	42 (12)	42 (12)	0.1979
Male ( percent)	0.5695 (0.4951)	0.5795 (0.4937)	0.5618 (0.4962)	0.5733 (0.4946)	0.5590 (0.4965)	0.5777 (0.4939)	0.5757 (0.4943)	0.5685 (0.4953)	0.1177
Closed Account <sup>c</sup>	0.0264 (0.1602)	0.0394 (0.1946)	0.0277 (0.1642)	0.0127 (0.1120)	0.0156 (0.1239)	0.0303 (0.1714)	0.0323 (0.1768)	0.0307 (0.1725)	0.2762
Observations	167190	6444	6456	12825	12825	12900	12825	12900	

Standard deviations are given in parenthesis.

<sup>a</sup> Final sample refers to the sample that were actually sent messages, after the bank removed premier cards. All stats refer to September 2010 before the treatments.

<sup>b</sup> Ftest of coefficients of all treatments being jointly equal to zero (p-values).

<sup>c</sup> As of February 2011.

Table 1 shows means and standard deviations of selected variables for the 7 treatments. To implement a formal test of balance we regress the variables in the first column of Table 1 against the seven treatment dummies while controlling for the stratification dummies. We report the p-values of an F-test of the hypothesis that all the coefficients on the treatment dummies are zero. We cannot reject the null that they are zero for any of the variables. That is the treatment and control groups are balanced. Recall that some messages were sent to high risk or high debt clients and therefore that one cannot directly compare means across treatment.

## B. Treatment Effect Heterogeneity

Table 2. Mixed Model

	<i>Fixed Effects</i>	<i>Random Effects</i>
Interest Rate Message	-36 (230)	0.00315 (0.0770)
Constant	8341*** (153)	23086 (54)
Observations	443143	
Groups	94515	

Significance level: \* 10 percent \*\* 5 percent \*\*\* 1 percent. Standard errors in parenthesis.

Table 2 summarizes the result of the mixed model we estimate in Section V and which explores treatment effect heterogeneity explicitly by allowing the response to depend on  $i$ . We estimated the following equation with random intercept and random treatment coefficient using data from March to June for each consumer. Month dummies ( $\lambda_t$ ) were included to control for time effects.  $S_k$  represent the stratification controls. We used Stata's xtmixed command which assumes joint normality of the error terms  $\nu_{1i}$  and  $\nu_{2i}$  below. For ease of convergence we assumed independence between  $\nu_{2i}$  and  $\nu_{1i}$ . Clients are indexed by  $i$  (second level) while months are indexed by  $t$ , which runs from 1 to 5 (first level):

$$Debt_{it} = \alpha + \nu_{1i} + (\beta + \nu_{2i})T_j + \gamma S_k + \lambda_t + \epsilon_{it} \quad (3)$$

The main effect ( $\beta$  is equal to -36 pesos, while the associated variance of  $\nu_{2i}$  is 0.003 pesos, with a standard error or 0.007. So the main conclusion is that treatment effect heterogeneity is almost null. There is however substantial variance in the intercept component.

Table 3. Conditional on Amount of Products with the Bank

	Dependent Variables				
	Debt		Delinquent		Closed Account
	March	April	March	April	June
<i>Panel A: One product with the bank</i>					
High Risk	-388.5*** (121.4)	-302.9* (165.0)	-0.0182** (0.00813)	-0.00718 (0.00834)	0.0123*** (0.00447)
Low Risk	-99.20 (117.9)	11.17 (159.4)	0.00390 (0.00837)	0.00235 (0.00860)	0.00211 (0.00464)
High Debt + Advice	-35.12 (87.99)	-226.7* (119.5)	-0.00259 (0.00608)	0.00292 (0.00624)	-0.00225 (0.00321)
High Debt	-9.174 (87.77)	74.40 (118.6)	0.000865 (0.00607)	0.00528 (0.00623)	0.000574 (0.00322)
Rate	7.376 (86.56)	15.83 (115.1)	-0.00311 (0.00578)	-0.00453 (0.00594)	-0.000547 (0.00319)
MTP	68.23 (86.35)	183.2 (117.1)	0.00147 (0.00578)	0.00880 (0.00595)	-0.00244 (0.00318)
Warning	-135.4 (85.06)	-231.0** (115.0)	-0.00180 (0.00591)	-0.00113 (0.00607)	-0.00433 (0.00325)
<i>Panel B: At least three products with the bank</i>					
High Risk	27.00 (173.2)	-16.70 (216.9)	-0.0137* (0.00817)	-0.00905 (0.00843)	0.00233 (0.00451)
Low Risk	229.2 (151.1)	138.2 (186.7)	0.0151** (0.00731)	0.00272 (0.00753)	0.00121 (0.00409)
High Debt + Advice	-44.96 (116.1)	-74.47 (145.5)	0.00208 (0.00557)	0.00375 (0.00575)	-0.000956 (0.00296)
High Debt	-262.5** (116.6)	-106.0 (146.2)	-0.0180*** (0.00555)	-0.00891 (0.00575)	0.00109 (0.00299)
Rate	26.34 (118.8)	61.44 (145.8)	-0.00325 (0.00517)	0.00000305 (0.00534)	0.00476* (0.00287)
MTP	-17.19 (116.9)	6.453 (143.9)	-0.00199 (0.00515)	0.00211 (0.00532)	0.000710 (0.00284)
Warning	-109.6 (114.2)	-99.68 (141.8)	-0.0102* (0.00535)	-0.0102* (0.00553)	-0.000165 (0.00300)

Significance level: \* 10 percent \*\* 5 percent \*\*\* 1 percent. Standard errors in parenthesis.

Each regression uses more than 34,000 individuals in Panel A and more than 27,800 in Panel B.

Table 3 estimates the treatment effect for all 7 treatments splitting the sample across clients with only this card with the bank, and those with at least three products with the bank. The idea is to explore if those more tied with the bank close the account less as a result of treatment, and have different responses in general. We do find some evidence for the this, however responses are still small.

Table 4. **Conditional on Income Level**

	Dependent Variables				
	Debt		Delinquent		Closed Account
	March	April	March	April	June
<i>Panel A: Low Income</i>					
High Risk	-2 (139)	-24 (187)	-0.0269*** (0.0104)	-0.0330*** (0.0107)	0.0096 (0.00642)
Low Risk	-133 (127)	-70 (168)	0.0012 (0.0099)	-0.0057 (0.0102)	0.004 (0.0062)
High Debt + Advice	104 (97)	8 (131)	-0.0023 (0.0075)	0.004 (0.0077)	0.0013 (0.0044)
High Debt	-122 (95)	-180 (131)	-0.0034 (0.0074)	0.0031 (0.0076)	0.0008 (0.0044)
Rate	111 (95)	267** (126)	0.0032 (0.0071)	0.0100 (0.0073)	-0.0027 (0.0043)
MTP	144 (95)	168 (128)	0.0028 (0.0069)	0.0064 (0.0072)	0.00002 (0.0043)
Warning	-101.9 (94)	-37.57 (123)	-0.0132* (0.0071)	-0.00820 (0.0073)	0.0045 (0.0044)
<i>Panel B: High Income</i>					
High Risk	-538 (928)	-1188 (1241)	0.0052 (0.0332)	0.0412 (0.0342)	0.0199 (0.0155)
Low Risk	1209 (883)	2557** (1137)	0.0117 (0.0334)	0.0051 (0.0343)	0.0019 (0.0155)
High Debt + Advice	-1491** (637)	-919 (873)	-0.0032 (0.0245)	-0.0061 (0.0251)	-0.0079 (0.0106)
High Debt	-1505** (654)	-417 (846)	-0.0263 (0.0242)	0.0105 (0.0250)	-0.0081 (0.0105)
Rate	-538 (649)	304 (841)	-0.0022 (0.0214)	0.0213 (0.0220)	-0.0021 (0.0095)
MTP	-6 (631)	351 (816)	0.0235 (0.0209)	0.0032 (0.0214)	-0.0032 (0.0092)
Warning	-871 (619)	-1452* (824)	-0.0391* (0.0233)	-0.0309 (0.024)	0.0004 (0.011)

Significance level: \* 10 percent \*\* 5 percent \*\*\* 1 percent. Standard errors in parenthesis.  
 Each regression uses more than 17,800 individuals in Panel A and more than 2,200 in Panel B.

Table 4 estimates the treatment effect for all 7 treatments splitting the sample across clients with low and high Income. Income information was obtained through the application form and given to us aggregated and splitted in 5 categories: A,B,C,D,E. Panel A estimates the regressions for category D and Panel B for category A. There is some heterogeneity across income groups: in particular high income groups have much greater debt responses to messages (even in percentage terms), while low income individuals respond mainly trough less delinquency. The interest rate message does have a positive and significant (at 10 percent confidence) coefficient.

Table 5. **Effects Conditional on Past Behavior**

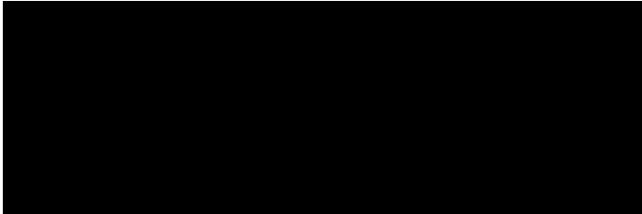
	Dependent Variables				
	Debt		Delinquent		Closed Account
	March	April	March	April	June
<i>Panel A: Delinquent in 2010</i>					
High Risk	-190 (122)	-129 (189)	-0.0072 (0.00895)	-0.0005 (0.0090)	0.0052 (0.0036)
Low Risk	333* (179)	570** (274)	0.0071 (0.0146)	-0.0040 (0.0147)	0.0051 (0.0058)
High Debt + Advice	52 (101)	-51 (157)	-0.0027 (0.0075)	0.0056 (0.0075)	-0.0036 (0.0028)
High Debt	-178* (101)	-67 (158)	-0.0017 (0.0075)	0.0042 (0.0075)	0.0008 (0.0029)
Rate	-1 (106)	273* (159)	0.0018 (0.0075)	0.0077 (0.0075)	0.0031 (0.0030)
MTP	289*** (105)	436*** (160)	0.0080 (0.0075)	0.0138* (0.0076)	-0.0017 (0.0030)
Warning	2 (103)	144 (159)	-0.0011 (0.0077)	-0.0031 (0.0078)	0.0015 (0.0031)
<i>Panel B: Non-delinquent in 2010</i>					
High Risk	-166 (123)	-167 (149)	-0.0111** (0.0049)	-0.0013 (0.0054)	0.0096** (0.0044)
Low Risk	-17 (93)	23 (113)	-0.0001 (0.0036)	0.0020 (0.0040)	0.0016 (0.0033)
High Debt + Advice	-85 (78)	-167* (95)	0.0011 (0.0032)	0.0023 (0.0035)	-0.0023 (0.0027)
High Debt	-136* (79)	-17 (95)	-0.0056* (0.0031)	-0.0035 (0.0035)	0.00020 (0.0027)
Rate	-50 (77)	-103 (92)	-0.0021 (0.0028)	-0.0054* (0.0031)	-0.0005 (0.0026)
MTP	-70 (76)	-65 (92)	-0.0040 (0.0028)	0.0020 (0.0031)	-0.0019 (0.0025)
Warning	-194*** (74)	-285*** (89)	-0.0036 (0.0029)	-0.0018 (0.0032)	-0.0037 (0.0026)

Significance level: \* 10 percent \*\* 5 percent \*\*\* 1 percent. Standard errors in parenthesis.  
Each regression uses more than 25,000 individuals in Panel A and more than 53,000 in Panel B.

Table 5 estimates the treatment effect for all 7 treatments splitting the sample across clients with no delinquency in 2010 and those with some delinquency in 2010. The rationale for doing so is twofold: first the CNBV conjectured that responses would be greater for more risky clients (those with previous delinquency (although this does not have to be so) and propose to send messages only to these. Second, we believed that switching cards could be limited by a bad credit history. We do find account closings is concentrated among clients with no delinquency in 2010. We also find that the adverse response to the MTP message is concentrated in the more risky clients.

C. Bank Statement and Messages

Estado de Cuenta Tarjeta de Crédito



NUMERO DE CUENTA

RFC

PERIODO  
22 Abr - 21 May 13

RESUMEN DE MOVIMIENTOS	
▶ Fecha Límite de pago	10 Jun 13
▶ Pago Mínimo	\$ 825.00
▶ Pago para no generar intereses	\$ 33,644.95
▶ Saldo Anterior	\$ 46,436.65
▶ Compras	+41,830.67
▶ Disposiciones en efectivo	+1,000.00
▶ Pagos y Créditos	- 55,639.77
▶ Intereses	+0.00
▶ Comisiones	+ 15.00
▶ IVA	+2.40
▶ Saldo Actual	\$ 33,644.95
Tasa Anual Personalizada	0.00%
<b>Tasa Anual</b>	<b>22.99%</b>
Costo Anual Total sin IVA	0.0%
<b>Tasa Moratoria Anual</b>	<b>0.00%</b>

El tiempo necesario para liquidar su saldo deudor realizando sólo el pago mínimo es de: 41 meses. El monto para pagar su saldo deudor si se encuentra al día en sus pagos en 12 meses es de: \$ 2,803.75. No considera compras, intereses, disposiciones ni comisiones posteriores a la presente fecha de corte.

INFORMACIÓN CORTE			
Límite de Crédito	\$ 66,000.00	Días del Periodo	30
Saldo Promedio Diario*	\$ 39,391.69	Tasa Anual de Inversión	0.00%
Fecha de Corte	21 MAY 2013		

Emitido por:

Hard information is located in the bottom. Annual interest rate is called TASA ANUAL.

Figure 13: Bank Statement

Figure 1 above is a real credit card monthly statement from our cooperating bank. As can be seen the interest rate and the MTP are displayed, but not too saliently.

Dear XXXXX,

Based on your credit behavior, we have detected that your credit card has the following **probability of default**:



## Congratulations!

You form part of our group of clients with very good payment behavior.

Continue to enjoy the benefits from your credit card by keeping your finances healthy.

Figure 14: Low Risk Message.

Dear **XXXXX**,

We want our clients to have healthy finances. That's why we have analyzed the credit behavior of a group of cardholders.

With respect to this group **your debt** is:

**HIGHER**  
than the average of  
people similar to  
yourself\*

Figure 15: High Debt Message.



## D. Survey

Table 6. Survey Self Selection

	Baseline Survey		P-value	Endline Survey		P-value
	Refused to Answer	Answered		Refused to Answer	Answered	
Debt (MXN)	17858 (24277)	14320 (18773)	0.000	17007 (21279)	15494 (21254)	0.003
Delinquent ( percent)	10.89 (16.38)	4.30 (9.65)	0.000	7.11 (11.38)	3.24 (7.75)	0.000
Closed Card by June ( percent)	4.38 (20.47)	4.47 (20.68)	0.904	0.67 (8.18)	0.9 (2.93)	0.000
Purchases (MXN)	1038 (2382)	952 (1816)	0.187	757 (1565)	934 (1827)	0.000
Payments (MXN)	1911 (2915)	1658 (2077)	0.001	1724 (2171)	1766 (2313)	0.434
Probability of Default ( percent)	22.41 (15.48)	17.83 (12.93)	0.000	22.08 (14.12)	16.68 (11.64)	0.000
Credit Limit (MXN)	27332 (34486)	22995 (29109)	0.000	23481 (27991)	25695 (33109)	0.004
Tenure (Months)	43 (26)	38 (23)	0.000	41 (21)	42 (25)	0.461
Age (Years)	42 (12)	44 (12)	0.000	41 (11)	45 (12)	0.000
Male ( percent)	57.01 (49.51)	44.83 (49.76)	0.000	58.22 (49.32)	51.80 (49.98)	0.000
Observations	166407	783		7271	2328	

For the endline survey we have information on every contacted individual whether the subject answered or not.

For the baseline we can only identify the ones that answered and thus we compared them with the whole population.

In the main text we used surveys within our sample to understand the context of the experiment. The most important result was that clients in our sample were highly leveraged and risky, while at the same time unaware of their interest rates and MTP. The surveys were conducted over the phone for cost reasons. Phone surveys usually do not have a high response rate, in our case they had close to 25 percent response rate.<sup>29</sup> One may worry that these low response rates generate substantial self-selection. Table 6 presents means and standard deviations for clients who answered and did not answer the survey and p-values of the difference in means. Indeed there is selection: those that answered are less risky and somewhat less indebted. Since this selection seems to go against our main survey finding and we still find large leverage and risk we do not think it is particularly worrisome for the purposes of this paper. Note also that we only use the survey to motivate the messages and our results are not dependent on it.

<sup>29</sup>We could not have access to the account level responses from survey 2 (the bank conducted them).

Table 7. Some Survey Results

	Yes ( %)	No ( %)	N/A ( %)
<b>Panel A. Monthly Statement</b>			
Receives bank statement monthly	78.3	21.3	0.4
Reads the statement	92	7.9	0.1
Would prefer a clearer statement	48	50.9	1.1
Believes a clearer statement would help reduce delinquencies	55.2	42.3	2.45
<b>Panel B. Knowledge</b>			
Claims to know interest rate of her CC	34.2	62.2	3.6
Claims to know exactly the interest rate of her CC*	3	97	
Claims to know which CC is cheaper <sup>a</sup>	36.3	36.7	27
Claims to know the statement date	34.2	62.2	3.6
Claims to know debt at statement date	68.7	20.1	11.2
Gives an accurate estimation of her previous debt <sup>b</sup>	54.3	29.5	
Knows how much interest she has paid during that year*	60.3	39.7	
<b>Panel C. Awareness</b>			
Had to paid interest at previous statement date (claimed)	50	43.9	6.1
Correctly answered previous question <sup>†</sup>	56	37.9 <sup>c</sup>	
Paid the minimum on time at previous statement date (claimed)	76.7	22.1	1.2
Correctly answered previous question <sup>†</sup>	70.8	28	
Believes to be at most as risky (interms of default) as her peers <sup>d *</sup>	81.1	8.9	10
Believes that unawareness of debt acumulation is leading to delinquency	38.3	61.7	
Incurred in overdraft fee at previous statement date (claimed)	18.4	78.9	2.7
Could very likely find a cheaper credit card <sup>e</sup> in the market* (claimed)	75.5	21.3	3.2
<b>Panel D. Prediction</b>			
	Wrong ( %)	Overconfident <sup>f</sup> ( %)	
Has over-estimated payment capability in previous 6 months (claimed)	35.7	62.2	2.1
Expectation of interest to be paid in January	47.4	44.7	
Expectation of interest to be paid in February	61.5	77.9	
Expectation of interest to be paid in March	52.6	75.4	
<b>Panel E. “Welfare” auto-evaluation and other claims</b>			
	A lot ( %)	Not much ( %)	Nothing ( %)
Believes that debt reduction improves welfare*	83.7	14.4	1.9
Defaulting credit card decreases people’s welfare*	92.9	4.8	2.3
<b>Panel F. Other</b>			
	Mean	St. Deviation	
Total monthly expenditures* (MXN)	8563	(7444)	
Education* (Years)	15.5	(3.8)	

\* These results correspond to a different survey realized ex-post to 2,772 individuals. We are grouping questions by topic.

† Obtained by comparing responses against administrative data.

<sup>a</sup> If the individual has more than one credit card.

<sup>b</sup> Percentage of people that correctly recalled the amount of debt at previous statement date. We obtained this after comparing responses against administrative data and allowed for a 10 percent error.

<sup>c</sup> Of those answering incorrectly, 83.5 percent say they did not incur in interests when they actually did.

<sup>d</sup> People of the same age, sex and credit limit.

<sup>e</sup> Compared to the one she has.

<sup>f</sup> Of those answering incorrectly, these individuals expected to pay less interest than what they actually end up paying.

**Questions in the ex-ante survey:** (not all questions are tabulated in Table 7 above)

1. In the registered address, do you receive your bank statement every month? (Yes/No)
2. Do you read attentively your bank statement? (Yes/No)
3. Would you like that your bank statement were clearer? (Yes/No)
3. Do you think that a clearer bank statement would help to reduce delinquency? (Yes/No)
4. Do you know, even if its only very approximately (within 5 points), the annual interest rate of your credit card? (Yes/No)
5. If you have more than one credit card, do you know which one is cheaper this month? (Yes/No)
6. Do you know, even if its only very approximately (within 5 days), the statement date of your card? (Yes/No)
7. Do you know, even if its only very approximately, the amount of money you owe? (Yes/No) How much?
8. Did you incur in overdraft fees for the last statement date? (Yes/No)
9. Did you have to pay interest for the last statement date? (Yes/No)
10. Did you pay the minimum on time for the last statement date? (Yes/No)
11. In the last 6 months, have you over-estimated the amount you can pay and end up paying less of what you had planned? (Yes/No)
12. Why do you think people incur in delinquencies? (They are unaware of the fact that they are accumulating debt very quickly/ They are aware of the situation but have no alternatives/ They just don’t care to incur in delinquencies)
13. Even if you are not completely sure, how much interest do you think you will pay for January, February and March? (zero/

more than zero but less of what you are paying today/ more than zero and more than what you are paying today)

**Questions used from the ex-post survey:**

1. Do you know the monthly interest rate of our credit card? (Yes/No)
2. How much do you think you have paid of interest during this year?
3. Relative to people of the same age, sex and credit limit, do you think you are more, less or, equally likely to default your credit card? (more likely/ less likely/ equally likely)
4. How likely do you think it is that you could find in the market a cheaper credit card than the one you currently have? (Very likely/Impossible)
5. With which of the following phrases would you be more likely to agree: "Reducing my debt, and what it implies in sacrifice, would improve my welfare"; "Reducing my debt, and what it implies in sacrifice, would not affect my welfare" or "Reducing my debt, and what it implies in sacrifice, would worsen my welfare"?
6. How much do you think the welfare of people is affected by defaulting their credit card (taking all the benefits and costs into account? (A lot/Not much/Nothing)
7. How much do you spend in an average month (include all expenses: housing, interest payments, food, clothing, etc.?)
8. How many months do you think it would take you to pay your current debt if you make no further purchases and only pay the minimum each month?

## E. Quasi-experimental evaluation of first time price comparisons

This section evaluates with quasi-experimental matching methods the effects of sending price comparisons. Although the main text of the paper presented results of an experiment, here we have the advantage of evaluating the effect when this information was sent *for the first time*. This may be important if the reader believes that failure to have an effect is due to the fact that clients already have the information.

The Central Bank of Mexico mandated disclosing the interest rates and APRs of *competitor banks* for similar cards –defined as classic, gold or platinum – in monthly statements starting April 2011. The comparison table was standardized and designed at the Central Bank, Figure 9 in the paper shows the one for classic cards. This is clearly a strong disclosure. Banks resisted this direct comparison in their own monthly statements since it surely reduces comparison frictions and has the potential to create competition, switching and reallocation of debt to cheaper cards. We expected a large response since our bank was in the top 5 most expensive banks and since this was *new* information.

Because we do not have a randomized control group to measure causal impacts, we rely on propensity score matching methods. Fortunately for us, our bank did not send the comparison price table to their top notch (TN) clients –3,581 cards in our sample–, so we use them as a control group.<sup>30</sup> TN clients are more wealthy and may have different spending and payment patterns. Figure 4 shows they have about twice as much debt but have similar time trends, which suggest the use of a differences in differences (DID) strategy.

To measure impacts non-experimentally we use two empirical strategies: a propensity score matching and a DID kernel matching strategy. As it is well known the latter controls for time invariant unobservable differences across treatment and control groups. For ease of computation, we matched 10,000 randomly selected treatment accounts with the 3,581 control accounts who did not receive the comparison table.<sup>31</sup>

Table 8 presents our results. Column 1 and 3 present falsification tests where we measure “impacts” in the pretreatment period; both show zero effects giving us confidence that we have a correct specification. Column 2 shows the propensity score matching impact estimates, where we compare the average debt on May and June 2011 of non-premier vs their matches in the premier control group for the same period. The effects are economically small –around 90 pesos for debt– and statistically not different from zero for both payments and debt<sup>32</sup>. Column 4 reports results for the

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<sup>30</sup>The TN client could have compared interest rates herself if she wanted. Kling et al. (2012) have shown however that making information slightly easier to access may have significant effects.

<sup>31</sup>We estimated a logit propensity score which includes debt, payments, purchases, credit limit, behavior score, late payments, number of purchases, number of cash withdrawals, and some quadratic and cubic terms of this variables as covariates. The specification successfully balances observed covariates (unreported). We use one neighbor with replacement and trimming on common support at 95 percent.

<sup>32</sup>We also estimated the model for purchases as dependent variables, however we could not find an specification of the propensity score that balanced the observable pretreatment variables, and therefore we are not confident to present results as causal.

matching diff-in-diff strategy. Again effects are negligible.

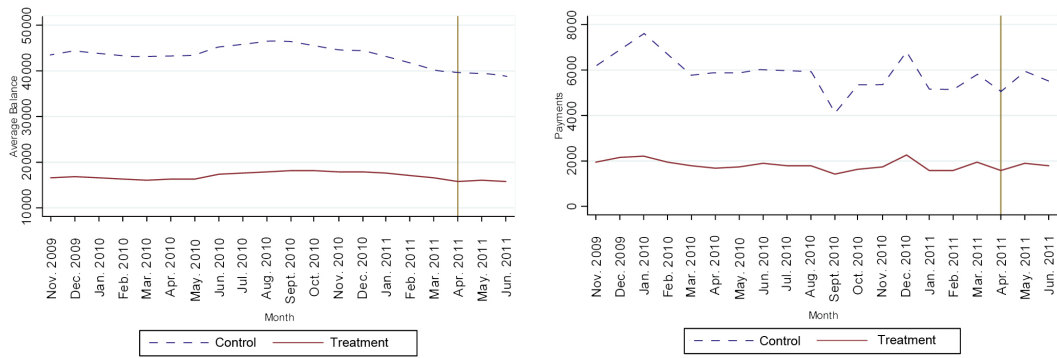


Figure 16: Debt and payment trends TN (control) vs not TN (treatment) clients.

Table 8. Propensity Score

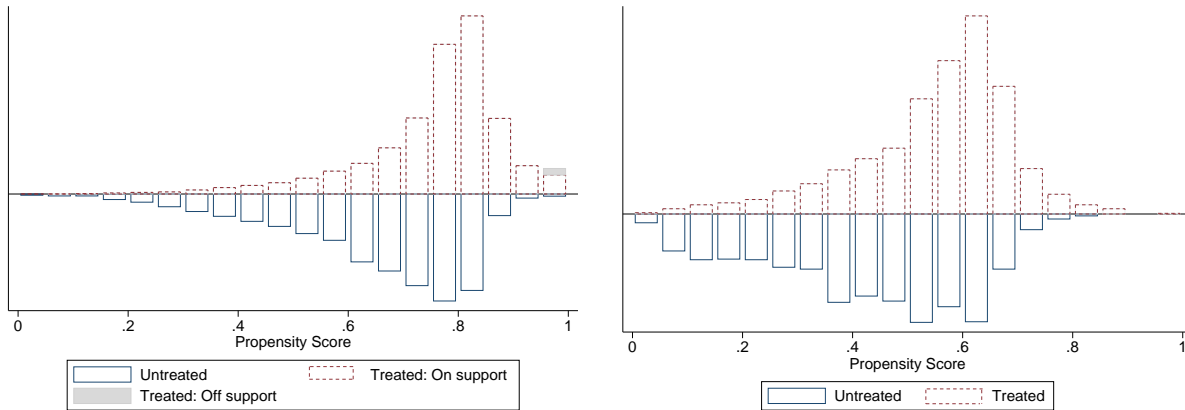
	Levels		Differences	
	Falsification [1]	Real [2]	Falsification <sup>b</sup> [3]	Real <sup>a</sup> [4]
Average Balance	-36.64 (-0.03)	90.50 (0.07)	990 (0.48)	-34.81 (-0.01)
Average Payments	-158.81 (-0.42)	-179.09 (-0.61)	256.85 (0.56)	-98.21 (-0.15)

t-stats in parenthesis

<sup>a</sup> Before: Jan-Feb-Mar 2011; After: May-Jun 2011

<sup>b</sup> Falsification: Before: Sep-Oct-Nov 2010; After: Jan-Feb-Mar 2011

The propensity score was estimated using the following variables: Debt Growth Rate, Debt in Feb2011, Num. Of Purchases Feb2011, Payment Due Feb2011, Payments Dec2010, Credit Limit Feb2011, Purchases Jan2011, Squared Debt, Average Debt Dec2010, Average Debt Feb2011, Risk Score Dec2010, Non Interest Debt Dec2010, Amount to Pay Dec2010, Payment Due Dec2010, Risk Score Feb2011, Cash Dispositions Feb2011, Payments Jan2011, Non Interest Debt Feb2011, Purchases Dec2010, Payments Feb2011, Sex \* Average Payments, Distrito Federal State \* Average Debt, Distrito Federal State \* Average Purchases, Distrito Federal State \* Average Payments, Mexico State \* Average Debt, Mexico State \* Average Purchases, Mexico State \* Average Payments, Dummy Default Dec2010, Dummy Default Jan2011, Squared Risk Score, Squared Purchases, Squared Debt to Pay, Squared Debt Growth Rate, Cubic Debt, Cubic Purchase, Cubic Debt Growth Rate, Cubic Risk Score and Cubic Payments.



(a) Propensity Score match

(b) Difference in Differences

Figure 2. Propensity Score Graphs

## F. Balanced Attrition

The purpose of Table 9 is to show that not only was attrition balanced across treatments in terms of percentage of attriters, but that is was also balanced in term of observable variables. The table uses information measured in September 2010 (before treatment) and compares means for still open accounts on June 2011 (i.e. non attriters). We used the same strategy as in Table 1 in this Appendix to calculate p-values for an F-test of all treatment coefficients being equal. We can only reject equality at conventional levels for age, but as can be seen the difference in age is at most one year.

Table 9. Balance for Non-attriter Population

	All	High Risk	Low Risk	High Debt + Advice	High Debt	Rate	MTP	Warning	F test <sup>a</sup>
Delinquency	0.0816 (0.2737)	0.1621 (0.3686)	0.0123 (0.1105)	0.0904 (0.2868)	0.0943 (0.2923)	0.0765 (0.2658)	0.0767 (0.2662)	0.0758 (0.2647)	0.6689
Probability of default	0.2285 (0.2015)	0.3717 (0.2427)	0.1141 (0.0168)	0.2218 (0.1866)	0.2206 (0.1856)	0.2341 (0.2120)	0.2305 (0.2060)	0.2320 (0.2079)	0.8413
Debt (MXN)	18706 (25340)	15050 (22655)	17390 (24188)	24340 (28495)	24491 (28255)	16074 (23517)	16177 (23354)	16200 (23282)	0.9006
Tenure with Card (months)	42 (25)	45 (25)	41 (25)	41 (24)	41 (25)	43 (25)	43 (25)	42 (25)	0.1956
Credit Limit	26747 (34564)	26022 (32587)	26582 (34905)	27246 (34105)	27354 (33818)	26417 (34941)	26478 (34448)	26461 (34648)	0.8300
Age (years)	41 (12)	42 (11)	41 (12)	42 (12)	41 (11)	41 (12)	41 (12)	41 (11)	0.0511
Male ( percent)	0.5678 (0.4954)	0.5757 (0.4943)	0.5644 (0.4959)	0.5719 (0.4948)	0.5577 (0.4967)	0.5736 (0.4946)	0.5712 (0.4949)	0.5658 (0.4957)	0.4289
Onservations	141689	4947	5911	10927	10877	10929	10844	10921	

Standard deviations are given in parenthesis.

All stats refer to September 2010, before the treatments. We use the individualas that remained in the sample unti June 2011 (approximately 85 percent of original population).

<sup>a</sup> Ftest of coefficients of all treatments being jointly equal to zero (p-values).

