Can Regulatory Policies Foster Women's Financial Inclusion? The Role of Loan Loss Provisioning

BY ALEJANDRO U. BECERRA-ORNELAS, DAVID JAUME, THANIA HERNÁNDEZ, AND MARTÍN TOBAL*

PLEASE DO NOT CICULATE. THIS IS A PRELIMINARY VERSION.

Abstract

In July of 2021, a regulatory reform reduced the loan loss provisions required for loans granted to women in Mexico. This paper studies the effects of this reform using a proprietary dataset with information on all consumer loans extended by commercial banks. We find that the reform led to an increase in the share of personal loans directed to women; a reduction of 0.52 percentage points in interest rates; and an increase of 1.99 percent in the credit amount of these loans. These effects are stronger for women who traditionally find it more difficult to access the best credit conditions because they are subject to stronger informational asymmetric concerns, meaning those who relate to their bank for the first time and who live in municipalities with higher labor informality. This greater inclusion of women into the financial system was not costly in terms of financial stability, as the reform was associated with a reduction in the probability of defaulting in the year following the credit creation. Moreover, there was an increase in the likelihood of obtaining subsequent personal loans with better credit conditions, indicating that the regulation has had long-lasting effects. We do not find effects for other types of credit for which the reform was also implemented, such as salary-based and automotive loans. For these types of credit, there is a lower proportion of borrowers that may have benefited from the regulatory change.

JEL Codes: G21, G28, J16

KEYWORDS: regulatory policies, credit market, financial inclusion, gender gap

*Authors work at the Financial Stability Department, Bank of Mexico. Becerra-Ornelas: (e-mail: alejandro.becerra@banxico.org.mx) Jaume (e-mail: djaumep@banxico.org.mx). Hernández-Alarcón (thania.hernandez@banxico.org.mx). Tobal: (e-mail: martin.tobal@banxico.org.mx).

I. Introduction

The obstacle that women face in accessing credit has been a topic of extensive discussion among policymakers and academics. Evidence suggests that greater financial inclusion of women could not only increase savings (Allen et al., 2016) and raise a country's entrepreneurial capacity (Aristei & Gallo, 2016; Bellucci et al., 2010; Klapper & Parker, 2011; Muravyev et al., 2009), but also generate a range of benefits for the macroeconomy (Gonzales et al., 2015; Perrin & Weill, 2022). This lack of women's access to credit is more challenging in low- and middle-income economies (LMIEs), where they face higher entry barriers and more adverse social and institutional frameworks than their counterparts in advanced economies (AEs) (Delechat et al., 2018; Demirgüç-Kunt et al., 2013). As a result, in the last decade, the gender gap in access to credit has narrowed down in AEs but remained constant in around 6 percentage points in LMIEs (Demirguc-Kunt et al., 2022).

To understand the gender gap in access to credit, previous research has developed an extensive literature that proposes conceptual frameworks and measures of inclusion (Alesina et al., 2013; Allen et al., 2016; Aslan et al., 2017; Beck & Demirgüç-Kunt, 2008; Honohan, 2008; Mookerjee & Kalipioni, 2010; Svirydzenka, 2016); it has studied the underlying causes of the gender gap (Bellucci et al., 2010; Buvinic & Berger, 1990; Demirgüç-Kunt et al., 2013; Galli et al., 2018, 2020; Klapper & Parker, 2011; Muravyev et al., 2009); its economic and social consequences (Agier & Szafarz, 2013; Aristei & Gallo, 2016; Gonzales et al., 2015); the effectiveness of interventions in reducing it (Aterido et al., 2013; Delechat et al., 2020; Demirgüç-Kunt et al., 2013; Swamy, 2014); and the drivers of financial inclusion (Delechat et al., 2018).

This paper directly contributes to the group of papers that investigates the effectiveness of policy interventions in reducing the gap in access to credit. Previous research in this strand of the literature has analyzed the effectiveness of micro interventions (Swamy, 2014). However, very little is known about the role of regulatory policies. To the best of our knowledge, Delechat et al. (2020) is the only paper that relates those policies to gender gaps in credit. They conduct a cross-country analysis focusing on whether individuals' choice of financial services correlates with regulatory policies, and find that regulation aiming to control credit supply has a negative effect on women's access to formal financial services.

However, in spite of the contribution that this paper has made, its intention is neither to estimate causal effects nor to speak about the effectiveness of a specific regulatory policy.

In this paper, we fill this gap in the literature by studying the effect of a regulatory change in Mexico. As a LMIE, Mexico offers an interesting case to study the role of regulatory policies. As previously mentioned, the literature has found that the gender gap in access to credit is systematic and persistent in LMIE relative to AEs. In Mexico, only 35 percent of males and 29 percent of females borrow money by any means. This figure is similar to that of other Latin American countries (46 percent and 39 percent, respectively) but much lower than the average of the OECD countries (67 and 63 percent).[1] Thus, focusing on Mexico provides valuable insights for other countries where individuals face high entry barriers to credit markets as well.

Our reform of interest was implemented in July 23rd 2021, when Mexican authorities approved a policy that reduced in 4 percent the loan loss provisions of personal loans granted to women.[2] In reducing provisioning for these loans, the policy meant to acknowledge that, for different reasons, women may face higher obstacles than men when accessing credit, like in other countries around the world. Thus, the regulatory change adjusts the methodology for estimation of loss provisions by simply adjusting the ex-ante probability of default considered in the formula that determines the amount of loss provisions a bank must hold for each new loan it issues. Loan loss provisions are estimated as a multiplicative function of the probability of default, loss given default (both based on a preestablish formula defined by the Mexican regulatory authority) and the total loan amount (exposure at default).

Although the regulatory change also mandated a reduction in the loss provisioning in other loans such as automotive (4 percent) and salary-based loans (2 percent), we focus on personal loans because of the following reasons. First, unlike other types of credit, women's personal loans had a higher spread and a lower average credit amount than men's after

[1] 2017 Data from The World Bank Global Financial Inclusion database. Retrieved May, 2023, from Global Financial Inclusion | DataBank (worldbank.org) .

[2] The official document describing the reform can be found here. The reform states that "The evidence suggests that women have lower default rates, and therefore credit for them requires lower loan loss provisions. An analysis of 23 banks from 18 countries that are part of the Global Banking Alliance found that women's share of non-performing loans was lower than that of men in most of the segments observed (micro credit and small and medium-sized companies). Women's lower number of non-performing loans could have a positive effect on credit loan loss provisions of credit institutions. The analysis of the profitability of women's participation carried out by these credit institutions in 2015 concluded that, on average, the provision of credit to women clients required approximately 4% less capital due to fewer non-performing loans from women".

controlling for credit characteristics, despite the fact that they also have a smaller non-performing ratio. Second, it is easier to access personal loans than other types of credits, such as automotive and salary-based loans, as they can be granted without a specific purpose or payment mechanism. So, regulatory changes in this type of loans can potentially affect a larger pool of people. Third, we expect that any change in the required loan loss provisions would have a larger effect. By construction, the reallocation of loss provisions driven by the reform is lower in types of credit with lower probability of default. So, when considering the average probability of default, a bank would save 47 cents for every 100 pesos lent in personal loans. These savings are much larger than the 4 and 11 cents coming from automotive and salary-based loans, respectively.

This paper estimates the causal effect of this regulatory change on the number and size of new loans granted to women, their interest rates and loan amounts. To our knowledge, this is the first paper that investigates the effect of regulatory policies on women's financial inclusion in a LMIE. To this end, we utilize a proprietary dataset that contains information on the universe of personal loans extended to individuals by commercial banks throughout 2021. We take advantage of the exogenous nature of the regulatory change to conduct both 2x2 Difference-in-Differences (DiD) and a dynamic DiD estimations (also known as event study) to calculate the causal effect of the regulatory change.

We conduct the analysis in several stages. We first estimate the effect of the reform on the levels of loan loss provisioning to verify that actual loss provisioning declined as intended. Secondly, we estimate the pass-through effects of the reform on other financial inclusion variables such as the spread, loan amount, and number of female loans. We also explore the role of heterogeneous effects to determine whether this change is more prevalent on subgroups divided by the regulatory probability of default (PD), the length of the client-bank relation, and the level of labor informality of the municipality where the credit was granted. Then, we investigate whether the reform had unintended consequences in terms of financial stability by promoting credit allocation to new and a priori riskier clients. To this end, we use three credit risk outcomes, one ex-ante measure of credit worthiness which is the client's probability of default calculated following the regulator's guidelines, PD, at the time the loan is originated, and two ex-post outcomes that account for the actual performance of the loan after being granted. Furthermore, we test whether the reform increased the likelihood

of being granted subsequent loans, and whether these loans were granted with better credit conditions. Finally, we estimate the impacts of the regulatory change on salary-based and automotive loans.

Our findings reveal that the regulatory change effectively declined loss provisions of personal loans in 4 percentage points, as originally intended. In terms of financial inclusion, we find that the regulatory change increased the share of credit directed towards women. We find suggestive evidence that the reform increased the value of loans granted to women in 3.2 percent. However, the identification of the causal estimate is challenging due to the presence of pre-treatment trends, and a direct estimation of the causal effect of the regulatory change on expanding the pool of women is difficult due to design of the regulation.[3] Therefore, we focus our analysis of the reform on estimating the pass-through and heterogeneous effects of the reform on credit conditions. Our results reveal that the decline of loss provisioning had pass-through effects on the spread and loan amount. The change in regulation caused a statistically significant decline of 0.52 percentage points in the spread of women. Though small, this decline was large enough to swap over the pre-existing gap between men and women swapping from 0.26 percentage points higher, to 0.25 lower for women compared to men. Moreover, the reform caused a statistically significant increase of 1.99 percent in the credit amount. This increase was large enough to reduce the pre-existing gender gap in over 40 percent.

The heterogeneous effects estimations reveal that the decline in the spread and the increase in the loan amount was concentrated on women who related with their bank for the first time, who also traditionally face higher barriers to access the best credit conditions. In part, this comes from the fact that, based on regulatory requirements, banks must assign them a higher PD and therefore pay a higher cost in terms of loss provisions. Thus, women entering into a new bank-relationship do so with better credit conditions under the new regulation. We also find that the pass-through effects of the reform were larger in municipalities with higher levels of labor informality. Lined up with the previous results, these municipalities are characterized by having a larger share of individuals with no previous relationship with

[3] When estimating the likelihood of a loan being granted to a woman we find a special challenge. Our indicator of gender, which helps us to identify the treatment and control groups, is now our outcome of interest. So, we can only use a pre- and post-treatment indicator to estimate the effect of the reform instead of DiD specifications.

the bank, higher PD, and where personal loans play a more important role compared to other types of credit traditionally associated to the formal economy such as salary-based and automotive loans.

Moreover, our findings show that the regulatory change did not have negative unintended consequences in terms of financial stability. On the contrary, we find that the reform is associated to a statistically significant decline of 0.4 percentual points in the share of periods in default. This improvement in credit performance is conditional on banks not changing their risk-taking appetite measured by the ex-ante calculation of the PD associated to new loans. When exploring the underlying causal mechanisms of these results, we find that the improvements in credit performance outcomes could be explained by the upswings in credit conditions associated to the reform, since these conditions, such a lower interest rate, are associated with a lower observed probability of default.[4] Likewise, lined up with the heterogeneous effects results, we find that these effects are stronger for women with no previous relationship with their bank.

We also assess whether the effects of the regulatory change were beyond a one-off improvement in access and credit conditions, and actually increased the likelihood of staying in the credit market. We find that the regulatory change increased the likelihood for new female clients of getting a second personal loan in the next twelve months. Our results point out that subsequent loans have lower interest rate, but there are not statistically significant differences in loan amount. We do not find statistically significant evidence that the reform increased the likelihood of female clients passing from personal to automotive or salary-based loans. However, these findings could be rooted in structural constraints lying outside the financial market, such as access to formal employment, or could suggest that transitions to other types of loans take longer than the 12 months window covered in our analysis. These findings imply that, while the reform generated some long-lasting effects on financial inclusion of women by increasing their likelihood of getting another personal loan with better credit conditions in the future, individuals were not able to move from personal to other types of credit that are commonly associated with better credit conditions.

[4] Results from a linear regression show that there is a positive and statistically significant relationship between the interest rate and the ex-post probability of default.

Finally, we estimate the effects of the regulatory change on other types of credit that were also affected by the reform such as automotive and salary-based loans. As previously described, we expect these impacts to be smaller than for personal loans based on the fact that, by construction, the reallocation of loss provisions driven by the reform is much lower in loans with lower ex-ante probability of default (calculated on the basis of the current regulation). Consistent with this hypothesis, we find no economically relevant pass-through effects on credit conditions for automotive and salary-based loans. In particular, for these types of credit, there is a lower proportion of borrowers that, according to current regulation, have a high PD and therefore may have benefited from the regulatory change.

These findings relate directly to the literature on the effectiveness of policy interventions to reduce the gender gap. Our results are in contrast with Delechat et al. (2020) correlational analysis that finds that the implementation of regulatory policies both reduced credit supply and are associated with a decline in the use of formal financial services increasing the gender gap by hurting women more than men. In our paper, findings suggest that a change in a regulatory policy, such as a targeted reduction in loan loss provisions, can improve women's financial outcomes. Moreover, our results suggest that policymakers should be aware of the heterogeneous effects that arise from the policy design. In the case of this regulatory change, the reallocation of loss provisions is, by design, greater in types of credits with higher probability of default that could increase bank risk-taking. Nevertheless, we found that favorable pass-through effects on credit conditions help to ameliorate credit performance, this may be conditional on banks not changing their risk appetite, which is consistent with our results that indicate that the reform had a null effect on the mean PD calculated at the loan's origination.

The remainder of the paper is organized as follows. Section II introduces the Mexican financial system and the regulatory change. Section III describes our data sources. Section IV outlines the empirical strategy. Section V presents the results. Section VI shows robustness checks, and Section VII concludes.

II. Institutional Background

The Mexican financial system comprises 292 financial intermediaries and credit unions divided into five groups. Commercial banks represent the largest group in terms of market participation with 58% total value of loans and 80% when focusing on consumption loans.

These banks are privately held and provide all kinds of financial products. SOFOMs are the second largest group with 8% total value of loans and 11% of consumption loans. These institutions are sub-entities of commercial banks that only offer specific type of products such as automotive loans. The third group is composed by development banks with 9% of total value of loans and 3% of consumption loans. These banks are created and owned by the government and, as commercial banks, provide all kinds of financial products. Later we have the Popular Savings and Credit System composed by SOCAPs and SOFIPOs. These intermediaries concentrate 2% of total value of loans and 7% of total value of consumption loans. Finally, the last group is formed of credit unions and self-help institutions that capture 23% of total value of financing but do not hold a consumption loans portfolio.

Mexican financial intermediaries offer three main types of credit: firms, consumer, and mortgages. Within consumer credit, the reform focused on non-revolving credit. This type of credit is divided in six categories based on its destination or payment mechanism: salary-based, personal, durable goods, automotive, microcredits, and other loans. Salary-based refers to loans linked to the payroll account of the borrower to ensure its payoff. Personal loans are granted in cash with no specific purpose or payment mechanism. Durable goods loans are granted by banks to purchase consumer durables except for cars which belong to the automotive category. Finally, microcredits are small loans (smaller than USD 10,000) granted for entrepreneurial purposes.

When granting a new consumer non-revolving loan, banks are required to set aside some funds to cover losses from defaulted loans called loan loss provisions. Banks can calculate loss provisions following either internal methodologies or the methodology of the Mexican regulatory authority (Mexico's National Baking and Stock Commission, CNVB) published in the General Rules applicable to Credit Institutions. Most banks fall in the latter group and follow the Mexican regulator. The CNBV establishes that banks must calculate loss provisions following an equation which is a multiplicative function of three components: probability of default (PD), loss given default (LGD), and exposure at default (EAD).

The PD is the same for all banks and varies by non-revolving loan. For new loans, the PD is calculated as a function of the payment mechanism (e.g., linked to the payroll

account) and the length of the client-bank relationship.[5] Similar to the PD, the LGD is the same for all banks following CNBV guidelines and varies by type of credit. The LGD is an increasing function of the number of months any given loan is past due. For instance, in the case of new personal loans, if the loan is past due from zero to three months, the LGD would be equal to 71%.[6] Finally, the EAD is the amount is exposed at the time of default, which for new loans is set equal to the loan amount.

In mid-2021, a regulatory change reduced the loan loss provisions required for loans granted to women, aiming to promote women's financial inclusion in Mexico. In doing so, the reform modified the loss provisioning equation. Specifically, the reform added a weighting factor F to all new credits i of type X granted to women W without overdue payments. The resulting loss provisioning equation is summarized below:

$$Loan\ loss\ provisioning_i = \left(PD_i^X * F_i^{XW}\right) * LGD_i^X * EAD_i \quad (1)$$

The reform was effective on personal, salary-based, automotive, and durable goods loans. For salary-based loans the weighting factor was equal to $F = 0.98$. While the for the other three types of credit the weighting factor was equal to $F = 0.96$. In reducing loan provisioning for women, the policy meant to acknowledge that, for different reasons, women may face higher obstacles than men when accessing credit, like in other countries around the world. Thus, the reform adjusts the methodology for estimation of loss provisions by simply adjusting the PD that is considered in the formula for the rating of loss provisions and credit portfolio rating. These changes applied to all banks that followed the CNBV's methodology to estimate provisions (e.g., without internal methodology). As further presented in Sections IV and V, we estimate the treatment effects of the regulatory change using the sample of banks affected by the reform, and estimate placebo effects using the sample of banks unaffected by the reform.

In this paper, we focus mostly on personal loans because of the following reasons. First, as presented in Table 1, the gender gap in credit conditions was more prevalent in this type of credits.[7] Even though women's personal loans had a better non-performing ratio than

[5] Other dimensions relevant for existing loans include the number of periods the loan is past due, percentage of the loan that has been paid of the total that should have been paid, and the ratio between the total amount of the loan and the outstanding balance.

[6] The scale keeps increasing until it reaches 100 if the loan is past due over 19 months.

[7] We exclude consumer durable loans from this comparison as most of its portfolio is held by banks not affected by the regulatory change.

men's, they had worse credit conditions. Specifically, women's personal loans had higher spread and lower loan amount than men's after controlling for credit characteristics. Second, personal loans are easy access credits as they can be granted without specific purpose or payment mechanism. So, any changes in this type of loans can potentially affect a larger pool of people.

Third, we expect that any change in the required loan loss provisions would have a larger effect in this type of loans. As personal loans can be granted with less requirements than other type of non-revolving loans, they are tagged as low-quality loans. This means that they have higher interest rates, smaller amounts, higher probabilities of default, and shortest maturities than high-quality loans such as automotive loans or mortgages. So, when a bank approves a personal loan, it is required to reserve more loss provisions than when approving high-quality loans.

Table 1: Gender gap in credit characteristics before the reform.

	Non-performing ratio (%)		Spread		Loan Amount (log)		
	1	2	3	4	5	6	
	Unconditional	Conditional	Unconditional	Conditional	Unconditional	Conditional	Observations
Type of loan							
Personal	0.714***	-0.341***	4.119***	1.743***	-0.337***	-0.125***	1,459,091
	(0.200)	(0.108)	(0.028)	(0.022)	(0.002)	(0.002)	
Salary	-1.311***	-1.110***	-1.235***	-1.256***	0.027***	-0.016***	3,650,379
	(0.034)	(0.033)	(0.011)	(0.010)	(0.002)	(0.001)	
Automotive	-0.664***	-0.565***	0.135***	0.127***	0.028***	-0.006***	674,061
	(0.060)	(0.060)	(0.006)	(0.006)	(0.002)	(0.002)	

Notes: Authors' estimation based on administrative data at loan level from Jan-21 to Feb-21. The table shows the unconditional and the conditional gender gap prevailing in the non-performing ratio, spread and loan amount for the different types of loan portfolios (personal, salary, and automotive loans). The conditional gender NPL gap we control for spread, maturity (log) and loan amount (log). For the conditional loan amount gap (spread gap) we control for maturity and spread (loan amount (log)). Robust standard errors in parenthesis. The notation ***, **, and * correspond to statistical significance levels of 1%, 5%, and 10%, respectively.

Finally, in terms of the timeline, the reform was proposed in late May 2021, pre-approved by the Ministry of Finance in late June 2021, and approved by the Mexican Congress as of July the 23rd 2021. However, as of January the 1st 2022, Mexico adopted the IFRS9 international standards for financial stability. Adopting the IFRS9 standards implied two things. First, these changes may have an impact on the changes introduced in our reform of interest for a short period but still enough to make it harder to interpret the result in that

period.[8] Second, as of January the 1st 2022, Mexico adopted the IFRS9 international standards for financial stability, which changed the calculation method for loss provisioning, delinquency rate, among other variables. Therefore, we constrain our analysis period to 2021.

III. Data

We exploit a proprietary dataset that comprise the universe of non-revolving credit from 28 financial intermediaries that are required by regulation to disclose all their loans. In particular, our analysis is based on an extremely detailed loan-level dataset containing information on the universe of personal, automotive, and salary-based loans. In Mexico, commercial banks are mandated to submit periodical reports with detailed information on all new and existing loans extended to individuals. For these loans, banks submit information every other month. We focus on new loans as the reform only mandates changes in the loss provisioning of these loans. For personal loans, the main focus of this paper, our sample extends throughout 2021, and consists of approximately 871,639 new personal loans. We constrain our period of analysis to 2021 because of two reasons. First, we start off in 2021 to avoid noisy estimates due to the Covid-19 pandemic that were mostly present in 2020. Second, as of January the 1st 2022, Mexico adopted the IFRS9 international standards for financial stability, which changed the calculation method for loss provisioning, delinquency rate, among other variables.

Table 2 shows the summary statistics by gender of the main variables of interest of our sample of new personal loans in 2021. Of our total sample of 871,639 new personal loans, 472,281 were granted to women, and 399,358 were granted to men. We observe this sample of new credits over five bimesters in 2021. We define the bimesters using the date as of the reform took effect (July the 23rd) as benchmark. This way, we get three periods before and two periods after. Likewise, all loans in our sample were granted by 25 commercial banks with no internal methodology in the estimation of loss provisions. In other words, our sample only includes banks affected by the change in regulation.

The summary statistics show that, on average, male loans required more loss provisions than female loans. These loans had a longer maturity and granted a greater amount to men, but charged a higher spread to women. In terms of individual characteristics, female

[8] Our reform of interest was included again as part of the main regulations in May the 27th 2022.

clients were statistically significant younger than their male counterparts. Finally, there are no statistically significant differences in the length of the client-bank relationship, and are barely statistically significant differences in the PD between genders.

Table 2. Summary Statistics by Gender

	Male	Female	Pairwise t-test
	Mean/(SE)	Mean/(SE)	Mean difference
Loan Loss Provisioning	2,753.50	1,635.16	1,118.34***
	(105.147)	(72.979)	
Spread	45.16	51.52	-6.365***
	(0.768)	(1.406)	
Loan amount	60,392.14	32,104.19	28,287.952***
	(3092.329)	(1654.059)	
Probability of Default	11.21	12.16	-0.950*
	(0.461)	(0.435)	
Period	33.82	28.57	5.248***
	(1.229)	(0.765)	
Age	45.43	43.65	1.778***
	(0.757)	(0.436)	
Length of the client-bank relation	23.73	23.84	-0.113
	(1.756)	(4.002)	
SE Clusters		5	
Number of Commercial Banks		25	
N	399,358	472,281	871,639

Notes: Authors' estimation based on administrative data on new loans from Jan-21 to Nov-21. The table shows the descriptive statistics for males and females as well as their statistical difference. The notation ***, **, and * correspond to statistical significance levels of 1%, 5%, and 10%, respectively. Standard errors in parenthesis. Errors are clustered at the bimester level.

IV. Empirical Strategy

We take advantage of the exogenous nature of the regulatory change to conduct both an instantaneous 2x2 DiD and a dynamic DiD estimations (also known as event study) to calculate the causal effect of the reform. To estimate the instantaneous effects of the reform, we specify a linear 2x2 DiD model as presented in equation (2):

$$y_{it} = Female_i + Post_t + \beta Female_i * Post_t + \vartheta C_i + \varepsilon_{it}, \quad (2)$$

where y_{it} is our outcome of interest for a new credit granted to individual i at time t, $Female_i$ is a binary indicator equal to 1 when loan i was granted to a woman and 0 otherwise, $Post_t$

is a binary variable equal to 1 if loan i was granted after the reform (July the 23th and until November the 30th 2021) and 0 otherwise, and C_i is a vector with observable individual and credit characteristics described in detail below. We can estimate this simple 2x2 DiD model because our setting has an intervention which was rolled out at the same time for all treated individuals and has a never-treated group composed by male borrowers that serve as control. The coefficient of interest is β, which measures the effect of the reform on credits granted to female borrowers compared to their male counterpart.

As we focus on the characteristics of new credits only at the time of credit approval, our data is composed of repeated cross sections of bimonthly data. This characteristic allows us to include additional confounders C_i to control for observable individual and credit characteristics (Sant'Anna & Zhao, 2020). Specifically, we add the borrower's age, payment mechanism, payment frequency, risk segment, PD, and loan maturity (in months) as additional confounders. Finally, we estimate the treatment effects of the reform on four credit outcomes y_{it}: loan loss provisioning, spread, loan amount, and an indicator of gender.

To estimate the dynamic effects of the reform, we specify a linear event study model as presented in equation (3):

$$y_{it} = \alpha_0 + \gamma_t + \sum_{m=-G}^{M} \beta_m Female_{i,t-m} + \vartheta C_i + \varepsilon_{it} \quad (3)$$

Unlike in the 2x2 DiD case, for the dynamic specification we break down the treatment effects into all possible leads and lags. Specifically, we define five bimesters using the treatment date as benchmark. Therefore, we estimate the treatment effects throughout a time window, three bimesters before G the intervention including one right before which we use as baseline, and two bimesters after M (all truncated in 2021). Also, we add a more flexible time fixed-effect γ_t to control for bimester specific changes that equally affect men and women.

Identifying assumptions

As any other DiD estimates, our results rely on three main identifying assumptions: parallel trends, no anticipation, and treatment effect homogeneity (Borusyak et al., 2022). In the case of parallel trends, DiD estimators calculate the treatment effects by looking at the difference in the outcome evolution of treatment and control groups after factoring out changes not

attributable to receiving the treatment, also known as pre-trends. In other words, the estimator assumes that, in the absence of the treatment, both groups (men and women) would have followed similar paths, and any deviations can be attributed to the treatment effect. We argue that our design fulfills this assumption as the implementation of the reform was an exogenous shock to the credit market. However, we include individual and credit confounders to control for any deviations of the parallel trends assumption caused by credit or individual characteristics. In equation (3) this assumption can be visually tested by looking at the pre-treatment coefficients $\{\beta_m\}_{m=-G}^{M}$ (leads), which should be non-different from zero.

The no anticipation assumption implies that the reform could not be fully predicted. So, the behavior either of individuals or banks did not change until the reform was approved by the Mexican Congress. This assumption is implicit in the way we define the $Post_t$ indicator, where we only allow to switch from zero to one as of the date when the reform took effect. As in the case of parallel trends, this assumption can be visually tested by looking at the behavior of the leads $\{\beta_m\}_{m=-G}^{M}$ in equation (3).

Finally, the treatment effect homogeneity implies that the reform had the same effect across individuals and time periods. Recent literature has shown that deviations from this assumption can bias DiD estimates in the presence of heterogeneous effects either due to variation in treatment timing, non-binary treatments, or non-staggered adoptions (de Chaisemartin & D'Haultfœuille, 2022). In other words, the violation of this assumption is mainly relevant for the dynamic effects' estimation (equation 3), as in the 2x2 case there are only two periods with binary treatment. However, in attempting to address this concern for the dynamic estimation, we realized that there is no new DiD estimator that computationally allows researchers to estimate the treatment effect conditional on multiple covariates with pooled cross-sectional data. So, we follow a traditional linear specification and then we break down the analysis into different subgroups to estimate the heterogenous effects of the reform.

Model specifications

We test whether the treatment effect is robust to different specifications by estimating the equation including an increasing set of controls. However, the confounders varied by the outcome of interest. For loan loss provisions, we first estimated a reduced-form model including the PD and EAD mimicking the components of the provisioning equation laid out

in equation (1). Secondly, we add municipality and bank fixed-effects that allow us to control for municipality and bank specific characteristics that are not included in the set of observable confounders. Thirdly, we add credit and individual characteristics such as payment frequency, payment mechanism, the maturity of the loan, the risk segment of the loan, and the borrower's age to estimate the treatment effect holding credit and individual characteristics equal. Finally, we use specification three allowing for time interactions by bimester in our confounders to control for the time variant effects that these variables have on the loan loss provisions.

For the spread and loan amount, we follow a similar path than for loss provisions. Firstly, we estimate a reduced-form equation with no controls. Secondly, we add fixed-effects by municipality and bank. Thirdly, we add credit and individual characteristics such as payment frequency, payment mechanism, the maturity of the loan, the risk segment of the loan, and the borrower's age. Lastly, we use specification three allowing for time interactions by bimester in our confounders.

Finally, when estimating the likelihood of a loan being granted to a woman we find a special challenge. Our indicator of gender which helps us to identify the treatment and control groups is now our outcome of interest. So, we can only use a pre- and post-treatment indicator to estimate the effect of the reform. We first estimate the treatment effect with no controls. Then, we add municipality and bank fixed-effects in the second and third specifications, respectively. Finally, we also run the same three models, but weighting by the loan amount to consider the effect not only in the number of loans but also on the share of the total amount that banks lend in this type of credit and for which the gender gaps are much larger.

V. Results

The instantaneous effects of the regulatory change on loan loss provisions are summarized in Table 3. As previously described, we test whether the treatment effect is robust to different specifications by estimating the equation including an increasing set of controls. Regardless of the specification, we find that the reform had a statistically significant negative effect on loss provisions as originally intended. The effect of the reform gets closer to the one mandated by the regulator as we add more controls to the regression. Specifically, when allowing controls as well as fixed effects to interact with the bimesters before and after the intervention, we control for the potentially time variant effects that these variables have on

the loan loss provisions. Also, we mimic the four percent decline in loss provisions as originally intended. Therefore, the full specification (column 4) is our preferred one throughout this Section.

Table 3: 2x2 DiD – Loan Loss Provisioning Results

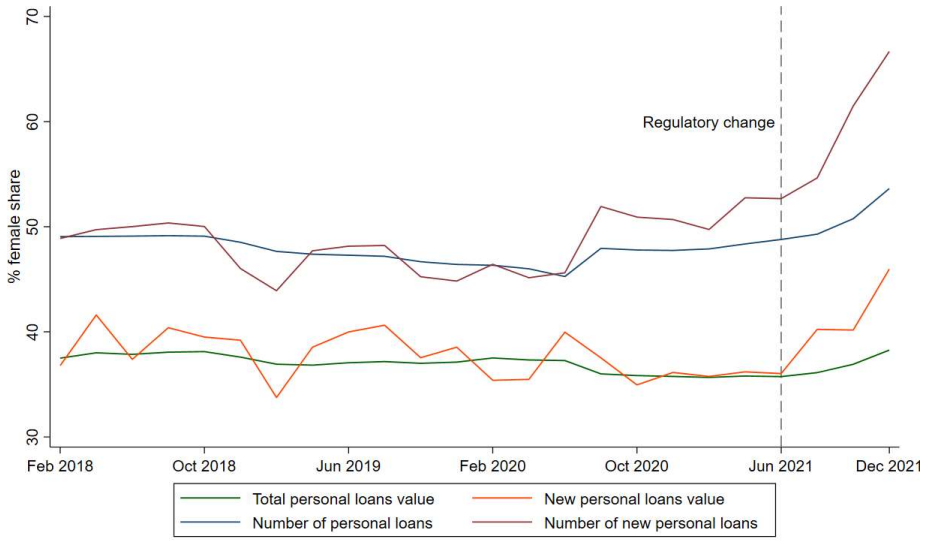
	Dependent variable: Loss provisions (log)			
	1	2	3	4
Treatment effect	-0.022***	-0.028***	-0.031***	-0.041***
	(0.001)	(0.001)	(0.001)	(0.001)
Female = 1	-0.004***	-0.006***	-0.006***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Treatment period = 1	-0.051***	-0.045***	-0.045***	
	(0.001)	(0.001)	(0.001)	
Provisions Equation	X	X	X	X
Bank FE		X	X	X
Municipality FE		X	X	X
Credit Controls			X	X
Individual Controls			X	X
Time Interactions				X
N	871,639	871,639	871,639	871,638
R-squared	0.984	0.984	0.985	0.990

Notes: Authors' estimation of equation (2) for the loan loss provisions of new personal loans as the dependent variable. Loan-level data taken from Banco de México administrative data from Jan-21 to Nov-21. Reduced form of column 1 includes loan amount (log) and default probability (log) as controls. Credit controls consist of maturity (log), five dummy variables indicating the frequency of payment (weekly, 10 days, 14 days, 15 days, monthly or unique payment), two dummy variables showing if the payment is taken direct from payroll, is instructed as a direct debit or is done freely by the recipient (no method), and two more dummy variables as indicators for the group of risk, which may be high, medium or low. The individual characteristic control is the age of the recipient (log). The table shows point estimates of the effect of treatment, which consists of the interaction between the indicator of being a woman and the credit being granted after July 23 of 2021. Robust standard errors in parenthesis. The notation ***, **, and * correspond to statistical significance levels of 1%, 5%, and 10%, respectively.

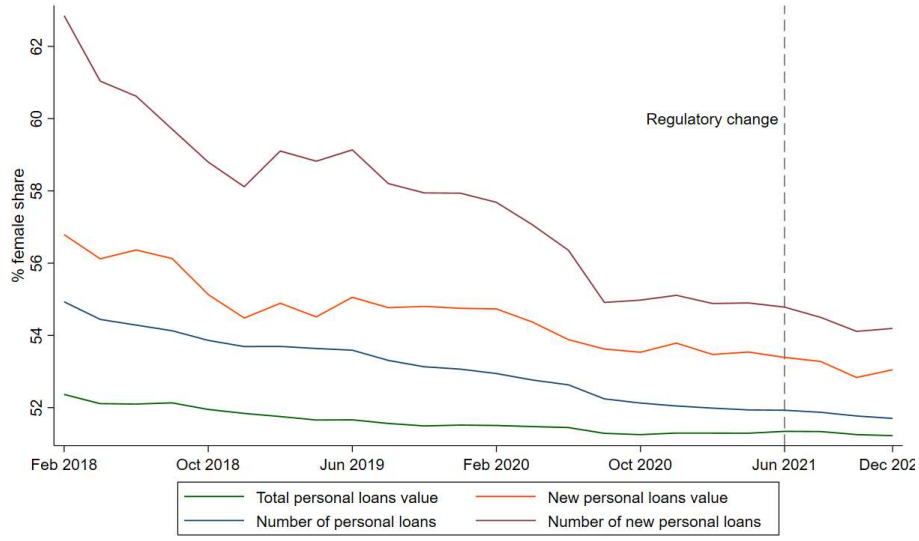
Figure 1 presents the evolution of women's participation in the number and value of personal loans. Panel A shows the evolution in banks affected by the regulatory change (e.g., banks that follow CNBV's methodology), and Panel B illustrates the evolution in banks unaffected by the change in regulation (e.g., banks with internal methodology in the estimation of provisions) from 2018 to 2021. The figure clearly shows a change in both level and slope after the change in regulation for the affected banks. On the one hand, the regulatory change seemed to accelerate the increase in the number of women's new personal loans that started in mid-2020. On the other hand, the value of women's new personal loans increased after a period of stagnation from late 2020 to mid-2021. In contrast, trends summarized in Panel B show no change neither in the slope no in the level after the regulatory change, as expected.

Figure 1. Evolution of women's participation in personal loans

Panel A. Banks affected by the change in regulation



Panel B. Banks unaffected by the change in regulation



Source: Banco de México

Notes: Both panels display the share of total (new) personal loans value and the share of total (new) number of personal loans held by women from Jan-18 to Dec-21. Panel A shows only banks that use the standard methodology to assess credit risk and that are therefore affected by the regulatory change, while Panel B shows banks using internal models of risk that are unaffected by this reform.

Table 4 summarizes the effects of the regulatory change on expanding access to credit for women.[9] Results reveal that the regulatory change led to a small increase in the participation of female clients in the portfolio of personal loans measured as both, percentage of new loans (unweighted specification in column 1) and percentage of total new debt (column 4). However, these findings are only suggestive because of the following reasons. First, findings change sign when adding bank fixed-effects (columns 3 and 6), which suggests that the increased participation observed in columns 1 and 4 does not occur within banks but that is rather a consequence of a change in the composition of the sample towards banks where female participation is more important. Second, there is an upward sloping pre-trend that may be biasing our results when calculating the number of new women's loans (as previously illustrated in Panel A of Figure 1). A direct estimation of the causal effect of the reform on expanding the pool of women is difficult due to design of the regulation. So, we focus our analysis of the reform by estimating its pass-through and heterogeneous effects on credit conditions as presented in Tables 5 to 8.

[9] We also estimate the effects on the reform on the size of loans dividing the sample into two subsamples based on the length of the client-bank relationship where individuals with no previous relationship with the bank were tagged as new clients. These results can be found in Table A1 in the Appendix.

Table 4. – Reform on Number and Size of Women's Loans

	Dependent variable: Probability of being a female recipient					
	Reduced Form	FE	Two-way FE	Reduced Form	FE	Two-way FE
	1	2	3	4	5	6
First bimester	-0.023***	-0.022***	-0.017***	-0.006	-0.017*	-0.016*
	(0.002)	(0.002)	(0.002)	(0.015)	(0.010)	(0.010)
Second bimester	-0.000	-0.002	-0.007***	-0.017	-0.014	-0.019
	(0.002)	(0.002)	(0.002)	(0.021)	(0.013)	(0.012)
Fourth bimester	0.009***	0.007***	-0.013***	0.025*	0.009	-0.001
	(0.002)	(0.002)	(0.002)	(0.014)	(0.009)	(0.009)
Fifth bimester	0.041***	0.040***	-0.011***	0.032**	0.020**	-0.000
	(0.002)	(0.002)	(0.002)	(0.014)	(0.009)	(0.008)
Weighted by loan amount				YES	YES	YES
Municipality FE		YES	YES		YES	YES
Bank FE			YES			YES
N	871,639	871,639	871,639	871,639	871,639	871,639
R-squared	0.002	0.014	0.055	0.001	0.041	0.074

Notes: Authors' estimation for the probability of being a female recipient of a new personal loan as the dependent variable on bimester dummies. Loan-level data taken from Banco de México administrative data from Jan-21 to Nov-21. Columns 4 to 6 include results weighted by loan amount. The table shows point estimates of the difference between the probability of being a female recipient at each point in time and the probability of such an event happening during the third bimester of 2021, which is the last bimester before the reform came into effect that constitutes our baseline period. Robust standard errors in parenthesis. The notation ***, **, and * correspond to statistical significance levels of 1%, 5%, and 10%, respectively.

Table 5 summarizes the 2x2 DiD effects of the regulatory change on the spread. Results reveal that a decline in the loss provisions is associated with a statistically significant reduction of the spread. This reduction holds for all specifications that include controls (columns 2 to 4). For instance, when looking at the full specification (column 4), we find that the spread in women's loans declined in 0.519 percentage points. This reduction is large enough to swap over the gender gap. Women's spread initially was 0.264 percentage points greater than men. However, after the reform, the spread swapped over to 0.255 percentage points lower for women.

Table 5: 2x2 DiD – Spread Results

	Dependent variable: Spread			
	1	2	3	4
Treatment effect	2.404***	-0.492***	-0.244***	-0.519***
	(0.073)	(0.032)	(0.029)	(0.028)
Female = 1	5.133***	0.423***	0.075***	0.264***
	(0.047)	(0.020)	(0.018)	(0.017)
Treatment period = 1	2.227***	1.441***	1.064***	
	(0.055)	(0.024)	(0.022)	
Bank FE		YES	YES	YES
Municipality FE		YES	YES	YES
Credit Controls			YES	YES
Individual Controls			YES	YES
Time Interactions				YES
N	871,639	871,639	871,639	871,638
R-squared	0.046	0.827	0.857	0.873

Notes: Authors' estimation of equation (2) for the spread of new personal loans as the dependent variable. Loan-level data taken from Banco de México administrative data from Jan-21 to Nov-21. Credit controls consist of default probability (log), maturity (log), five dummy variables indicating the frequency of payment (weekly, 10 days, 14 days, 15 days, monthly or unique payment), two dummy variables showing if the payment is taken direct from payroll, is instructed as a direct debit or is done freely by the recipient (no method), and two more dummy variables as indicators for the group of risk, which may be high, medium or low. The individual characteristic control is the age of the recipient (log). The table shows point estimates of the effect of treatment, which consists of the interaction between the indicator of being a woman and the credit being granted after July 23 of 2021. Robust standard errors in parenthesis. The notation ***, **, and * correspond to statistical significance levels of 1%, 5%, and 10%, respectively.

Table 6 summarizes the 2x2 DiD results of the regulatory change on the loan amount. Results reveal that the reduction in loan loss provisions was associated to a statistically significant increase of the loan amount in women's credits. When looking at the full specification (column 4), we find that the reform caused an increase of 1.99 percent in the loan amount of women's credits. This increase is equivalent to narrowing down the gender gap in the loan amount of over 40 percent, going from an initial negative difference of 4.78 percent to a final gap of 2.79 percent.

Table 6: 2x2 DiD – Loan Amount Results

	Dependent variable: Amount (log)			
	1	2	3	4
Treatment effect	0.074***	0.035***	0.008**	0.020***
	(0.006)	(0.004)	(0.004)	(0.004)
Female = 1	-0.391***	-0.072***	-0.043***	-0.048***
	(0.004)	(0.003)	(0.003)	(0.003)
Treatment period = 1	-0.132***	-0.101***	-0.180***	
	(0.005)	(0.003)	(0.003)	
Bank FE		YES	YES	YES
Municipality FE		YES	YES	YES
Credit Controls			YES	YES
Individual Controls			YES	YES
Time Interactions				YES
N	871,639	871,639	871,639	871,638
R-squared	0.019	0.535	0.579	0.589

Notes: Authors' estimation of equation (2) for the loan amount of new personal loans as the dependent variable. Loan-level data taken from Banco de México administrative data from Jan-21 to Nov-21. Credit controls consist of default probability (log), maturity (log), five dummy variables indicating the frequency of payment (weekly, 10 days, 14 days, 15 days, monthly or unique payment), two dummy variables showing if the payment is taken direct from payroll, is instructed as a direct debit or is done freely by the recipient (no method), and two more dummy variables as indicators for the group of risk, which may be high, medium or low. The individual characteristic control is the age of the recipient (log). The table shows point estimates of the effect of treatment, which consists of the interaction between the indicator of being a woman and the credit being granted after July 23 of 2021. Robust standard errors in parenthesis. The notation ***, **, and * correspond to statistical significance levels of 1%, 5%, and 10%, respectively.

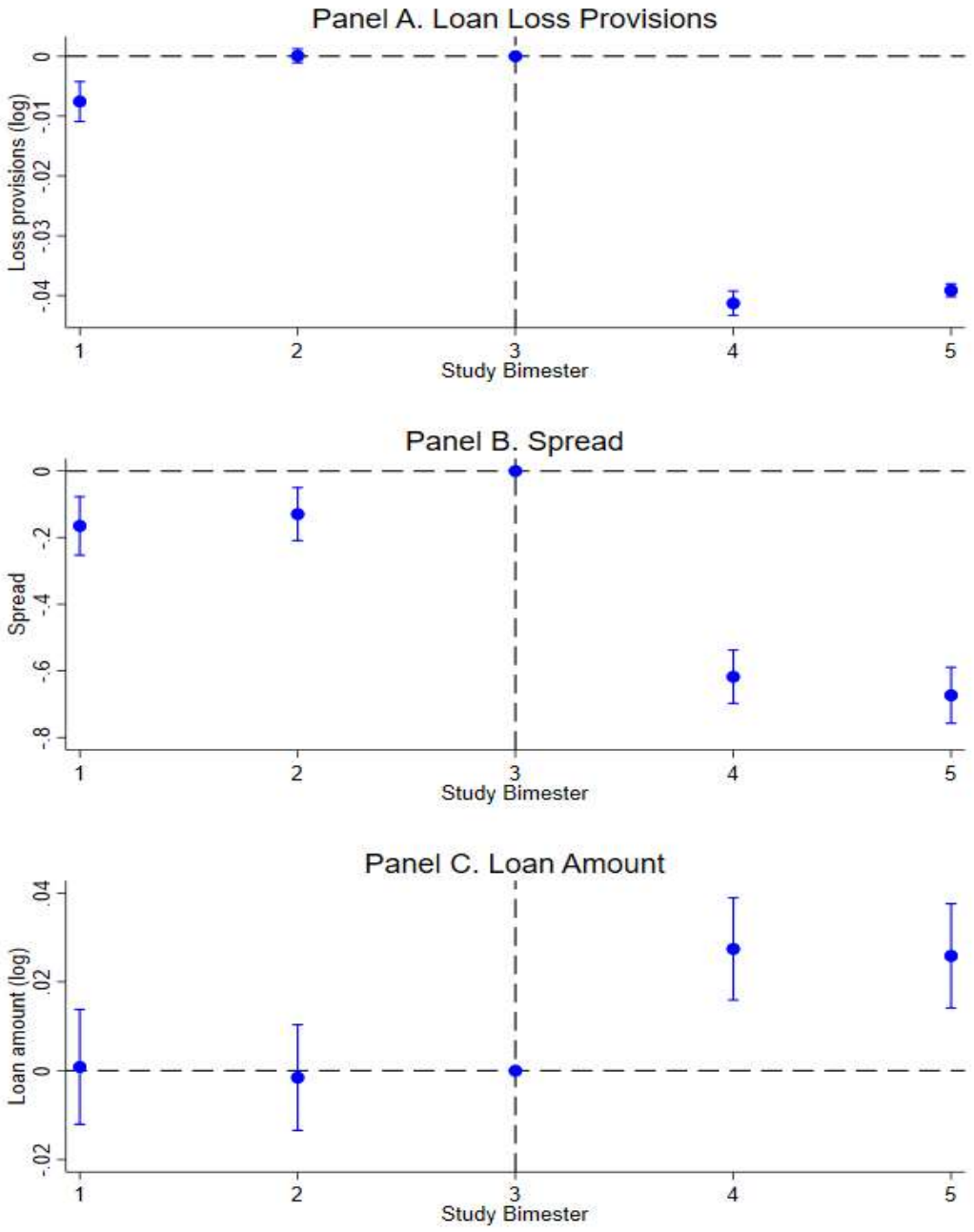
Event study estimates of the effects of the regulatory change on different credit outcomes are shown in Figure 2. Panel A shows the dynamic effects of the reform on loan loss provisions. Lined up with the 2x2 estimates, we find that the reform caused the intended decline in loss provisions of loans granted to women of around four percent. This effect is constant and persistent in both the bimester right after the reform (4^{th}), and the last bimester of 2021 (5^{th}). When looking at pre-treatment coefficients, we find that the first coefficient deviates from zero but its value is very small compared to the effect after the reform, and this difference disappears in the second bimester and should not bias our main findings.

Panel B and C summarize the pass-through effects of the regulatory change on credit conditions. Panel B shows that the decline in loss provisions was associated with a statistically significant decline in the spread of women's loans. The point estimates of the post-treatment coefficients suggest that the decline in the spread was around 65-70 percentage points. The pre-treatment coefficients reveal an upward pre-trend where

coefficients are different from zero and negative. Though this behavior may be biasing our results, the direction and magnitude of pre-treatment coefficients should not change the main findings for the effect of the reform on the spread of women's loans. In this case, if we consider the difference with respect to the average of the pre-treatment effects, we find that the effect of the reform gets closer to the 0.519 percentage points summarized in Table 5.

Panel C presents the results on the loan amount. The decline of loss provisions was associated with a statistically significant increase of the amount borrowed to women. As in the previous outcome variables, this effect is constant and persistent throughout the post-reform bimesters. For this outcome, pre-treatment coefficients are stable and non-different from zero. Summing up, the pass-through effects of the reform are associated to loans with better credit conditions: lower spread (even lower than men) and more loan amount (although still lower than men).

Figure 2: Event study results



Notes: Authors' estimation of equation (3) for the loan loss provisions (Panel A), spread (Panel B) and loan amount (Panel C) of new personal loans granted by banks affected by the reform as the dependent variables. Loan-level data taken from Banco de México administrative data from Jan-21 to Nov-21. The graphs show the point estimates, as well as the confidence interval of 95%, of the difference between the gender gap at each point in time and the prevailing gap observed during the third bimester of 2021, which is the last bimester before the reform came into effect that constitutes our baseline. Specification used includes the full set of controls as in column 4 of Tables 5 and 6.

Heterogeneous effects by probability of default

To further explore whether the reform had differentiated effects over different subgroups, we expand the analysis and estimate the effects of the regulatory change dividing our sample by PD and the length of the client-bank relation. We divide our sample by PD to explore whether banks factored in this dimension and mainly changed characteristics of loans with higher PD for which there is a larger increase in their savings coming from the reduction in loan loss provisions. Likewise, we divide our sample by the length of the client-bank relation to indirectly explore if the reform had a larger effect for the pool of women with less access to the credit market who also tend to have larger PD.

The effects of the reform on subgroups divided by PD are shown in Table 7. This table summarizes the results for loan loss provisions, spread, and loan amount using the full specification (column 4) of Tables 3, 5, and 6, respectively. The groups are defined by the median, where the group with high PD is defined as greater than the median, and the group with low PD is defined as equal or lower than the median.[10] Results reveal that the decline in loss provisioning is slightly larger in loans with higher PD. However, differences between groups are larger when looking at the pass-through effects of the reform. First, though both effects are negative and statistically significant, the decline in the spread is 2.4 times larger in women's loans with higher PD compared to the group with lower PD. Second, the increase in the loan amount in women's credits was only positive and statistically significant for loans with higher PD. The effect of the reform in loans with lower PD was both negative and not statistically significant.

[10] Due to the way the PD is calculated for new loans, some of its values are common and the groups are unbalanced in the number of observations when splitting up the sample by its median.

Table 7: 2x2 DiD – Heterogeneous Effects by Probability of Default

	Loan Loss Provisions (log)		Spread		Loan Amount (log)	
	Low PD	High PD	Low PD	High PD	Low PD	High PD
	1	2	3	4	5	6
Treatment effect	-0.039***	-0.040***	-0.279***	-0.670***	-0.005	0.024***
	(0.001)	(0.000)	(0.054)	(0.028)	(0.007)	(0.005)
Female=1	-0.002***	-0.000	0.200***	0.355***	-0.084***	-0.017***
	(0.001)	(0.000)	(0.034)	(0.015)	(0.004)	(0.003)
Bank FE	YES	YES	YES	YES	YES	YES
Municipality FE	YES	YES	YES	YES	YES	YES
Credit Controls	YES	YES	YES	YES	YES	YES
Individual Controls	YES	YES	YES	YES	YES	YES
Time Interactions	YES	YES	YES	YES	YES	YES
N	373,585	496,333	373,585	496,333	373,585	496,333
R-squared	0.995	0.994	0.864	0.790	0.577	0.392

Notes: Authors' estimation of equation (2). The conditional effect of the reform is shown in the table splitting up the sample by its probability of default. We denote a loan as having a high PD if the PD associated to the recipient is higher than the median of 2.28 % for the new personal loans granted during 2021. Credit controls consist of probability of default (log), maturity (log), five dummy variables indicating the frequency of payment (weekly, 10 days, 14 days, 15 days, monthly or unique payment), two dummy variables showing if the payment is taken direct from payroll, is instructed as a direct debit or is done freely by the recipient (no method), and two more dummy variables as indicators for the group of risk, which may be high, medium or low. The individual characteristic control is the age of the recipient (log). The table shows point estimates of the effect of treatment, which consists of the interaction between the indicator of being a woman and the credit being granted after July 23 of 2021. Robust standard errors in parenthesis. The notation ***, **, and * correspond to statistical significance levels of 1%, 5%, and 10%, respectively.

The effects of the reduction in loan loss provisions on subgroups divided by the length of the client-bank relation are summarized in Table 8. The groups are divided into two subgroups. The first one is composed of individuals without previous client-bank relationship. The second group is comprised of individuals with one or more months of client-bank relationship. As in the case of the heterogeneous effects by PD, we find that there is a slight difference in the decline of loss provisions between groups, but the main differences are found in the pass-through effects. We find that the decline in the spread was only targeted to women without previous relationship with the bank. The effect on women with previous relationship with the bank is both positive and non-statistically significant. Moreover, we find that the positive effect on the loan amount was also only targeted to women without previous relationship with the bank. The effect on women with one or more months of client-bank relationship is negative and statistically significant. These findings suggest that the improvements in credit conditions caused by the reform were targeted to women with higher PD and women with no previous relationship with the bank, for which the reform had a larger

effect. In the next section we explore if banks also reallocated credit towards female clients with higher PD and if the reform led to a decline in their credit performance.

Table 8. 2x2 DiD – Heterogeneous Effects by Length of the Client-Bank Relationship

	Loan Loss Provisions (log)		Spread		Loan Amount (log)	
	New Client	Previous Client	New Client	Previous Client	New Client	Previous Client
	1	2	3	4	5	6
Treatment effect	-0.039***	-0.036***	-0.800***	0.013	0.032***	-0.020***
	(0.000)	(0.001)	(0.027)	(0.059)	(0.004)	(0.007)
Female=1	0.000	-0.002**	0.335***	0.246***	-0.021***	-0.087***
	(0.000)	(0.001)	(0.015)	(0.038)	(0.003)	(0.005)
Bank FE	YES	YES	YES	YES	YES	YES
Municipality FE	YES	YES	YES	YES	YES	YES
Credit Controls	YES	YES	YES	YES	YES	YES
Individual Controls	YES	YES	YES	YES	YES	YES
Time Interactions	YES	YES	YES	YES	YES	YES
N	524,757	345,164	524,757	345,164	524,757	345,164
R-squared	0.998	0.992	0.811	0.868	0.377	0.560

Notes: Authors' estimation of equation (2). The conditional effect of the reform is shown in the table splitting up the sample into new loans to clients without previous relationship with the lender and clients already known by the bank. Credit controls consist of default probability (log), maturity (log), five dummy variables indicating the frequency of payment (weekly, 10 days, 14 days, 15 days, monthly or unique payment), two dummy variables showing if the payment is taken direct from payroll, is instructed as a direct debit or is done freely by the recipient (no method), and two more dummy variables as indicators for the group of risk, which may be high, medium or low. The individual characteristic control is the age of the recipient (log). The table shows point estimates of the effect of treatment, which consists of the interaction between the indicator of being a woman and the credit being granted after July 23 of 2021. Robust standard errors in parenthesis. The notation ***, **, and * correspond to statistical significance levels of 1%, 5%, and 10%, respectively.

Heterogeneous effects by labor informality

As previously introduced, we focus our analysis on personal loans as this type of credit is granted without specific purpose or payment mechanism. This characteristic makes personal loans more accessible to individuals that would otherwise be excluded of the credit market, such as the ones working in the informal economy. In this Section, we test whether women living in municipalities with a greater share of informal jobs are more benefited by the reform. Then, we explore the characteristics of these municipalities to further understand our findings. To this end, we use the median level of informality weighted by the number of new loans during the first bimester of 2021, and divided our sample into two groups: new loans originated in municipalities with high labor informality and those originated in less informal settings. We follow the definition of informal jobs used in the 2020 Mexican Census, which relates labor formality to access to public health care. The median level of informality used

to split the sample is 46.6 percent, and the average informality in municipality classified as high and low labor informality is 75.3 and 31.1 percent, respectively.

Our results of the effects of the regulatory change dividing the sample into municipalities with higher and lower levels of labor informality are summarized in Table 9. We find that the reform was associated to a statistically significant decline of 4 percent in loss provisions regardless of the level of formality of the municipality, as expected. However, our results reveal that the pass-through effects of the reform to the characteristics of the newly originated loans to women were larger in municipalities with higher levels of labor informality. In the case of the spread, we find that the reform was associated to a statistically significant decline of 0.71 percentage points in municipalities with high labor informality, which is 1.5 larger than the effect of its counterparts with low informality. In the case of loan amount, the reform was associated to a statistically significant increase in the amount, but this increase was more than 3 times larger in municipalities with high labor informality.

Table 9. 2x2 DiD – Heterogeneous Effects by Labor Informality

	Loan Loss Provisions (log)		Spread		Loan Amount (log)	
	Informal	Formal	Informal	Formal	Informal	Formal
	1	2	3	4	5	6
Treatment effect	-0.042***	-0.039***	-0.714***	-0.472***	0.046***	0.013**
	(0.001)	(0.001)	(0.040)	(0.042)	(0.005)	(0.006)
Female=1	-0.001**	-0.001***	0.167***	0.255***	-0.033***	-0.043***
	(0.000)	(0.000)	(0.024)	(0.026)	(0.004)	(0.004)
Bank FE	YES	YES	YES	YES	YES	YES
Municipality FE	YES	YES	YES	YES	YES	YES
Credit Controls	YES	YES	YES	YES	YES	YES
Individual Controls	YES	YES	YES	YES	YES	YES
Time Interactions	YES	YES	YES	YES	YES	YES
Obs	397,500	386,064	397,500	386,064	397,500	386,064
R2	0.990	0.990	0.837	0.881	0.477	0.571

Notes: Authors' estimation of equation (2). The conditional effect of the reform is shown in the table splitting up the sample into new loans to clients living in municipalities by labor informality level. Credit controls consist of default probability (log), maturity (log), five dummy variables indicating the frequency of payment (weekly, 10 days, 14 days, 15 days, monthly or unique payment), two dummy variables showing if the payment is taken direct from payroll, is instructed as a direct debit or is done freely by the recipient (no method), and two more dummy variables as indicators for the group of risk, which may be high, medium or low. The individual characteristic control is the age of the recipient (log). The table shows point estimates of the effect of treatment, which consists of the interaction between the indicator of being a woman and the credit being granted after July 23 of 2021. Robust standard errors in parenthesis. The notation ***, **, and * correspond to statistical significance levels of 1%, 5%, and 10%, respectively.

When exploring the characteristics of these municipalities, we find that municipalities with higher levels of labor informality have both higher average PD and larger share of clients with no previous relationship with the bank. These characteristics are lined up with our findings in the previous section, where we showed that the effects of the reform are concentrated on individuals with no previous relationship with the bank and, therefore, higher PD. Moreover, we find that personal loans play a more important role in these municipalities compared to other types of credit traditionally associated to the formal economy such as salary-based and automotive loans. In municipalities with high levels of informal jobs, personal loans represent 76 percent of the total number of loans compared to 57 percent in municipalities with a greater share of formal jobs. Also, in terms of the share of the total credit amount, personal loans represent 30 percent in municipalities with high labor informality, which is significantly greater than the 25 percent of its counterparts with low labor informality.

Effects on financial stability

In the previous Sections, we showed that the reform improves credit conditions for female clients without prior relationship with the bank, high PD, and in municipalities with a larger informal economy. In this Section, we investigate whether the reform had unintended consequences. Specifically, we study whether the reform increased credit risk by promoting credit allocation to new and a priori riskier clients. To do so, we explore the effects of the reform on three credit risk outcomes. Of the three outcomes, one is an ex-ante outcome that measures the estimated credit worthiness of a client according to the regulator's guidelines at the time that the new loan is originated, and two are ex-post outcomes that account for the actual performance of the loan after being granted.

The ex-ante outcome is the probability of default PD, which serves as a proxy variable of the risk appetite of the banks. Meanwhile, the first ex-post outcome is the loan performance measured as the probability that a loan defaults at least once within a time window of up to one year after it was originated. To complement and standardize this outcome, we also include a second ex-post outcome that is constructed as the share of periods in default in the

following year after the loan was created.[11] For each of these credit risk outcomes, we calculate both 2x2 DiD and event study estimates using the full specification of confounders and the same sample of previous estimations. Then, we explore the effect of the improvement in credit conditions as a causal mechanism to understand our estimates on credit risk outcomes. We test it directly using the interest rate and loan amount as confounders, and indirectly estimating the heterogeneous effects of having a previous client-bank relationship.

Our results of the effects of the reform on credit risk outcomes are summarized in columns 1, 2, and 4 of Table 10. Column 1 introduces the results on the PD. Results point out that the reform had no effect on the mean level of the regulatory measure of probability of default of new loans. The results suggest that banks didn't reallocate credit towards riskier female clients as measure by this variable. Column 2 shows the effects on loan performance. Results reveal that the reform is associated with a statistically significant reduction in the probability of defaulting at least once in the following year of 1.4 percentual points. This change implies a drop of 14.8% from its 9.45% level before the reform took place. Column 4 presents the effects on the share of periods in default in the year after the date of origination. Our findings show that the reform is associated to a statistically significant decline of 0.4 percentual points in the share of periods in default. This change represents an 18% decrease from the mean level of 2.20%.

Columns 3 and 5 of Table 10 present our findings on directly exploring the role of the improvement in credit conditions as an underlying causal mechanism of the effects on credit risk outcomes. Our hypothesis states that better credit conditions could potentially impact the ability of female clients to repay their loans and improve the overall credit risk. We test for this hypothesis in columns 3 and 5 where we include both the spread and the credit amount (log) as controls. When controlling for these channels, the effect of the reform on female credit performance drops from -1.4% to -0.6%, less than half of the original effect, as summarized in Column 4. A similar finding is presented in Column 5, where the effect of the reform on the share of periods in default declined from -0.4% to -0.2%. These findings

[11] Because not every loan has a term to maturity as long as 12 months, we control for the time window observed and for the period in each case, this way we are comparing the survival of loans observed for the same amount of time and for which the observation time represents a similar percentage of its total term to maturity.

confirm our hypothesis, and point out that the improvements in credit conditions translated to upswings in the credit risk.

Table 10. 2x2 DiD – Effects on credit risk

	Impact on risk measures				
	Probability of default (log)	At least one default event in the following year		Share of periods in default in the following year	
	1	2	3	4	5
Treatment effect	-0.002	-0.014***	-0.006***	-0.004***	-0.002***
	(0.003)	(0.001)	(0.001)	(0.000)	(0.000)
Female = 1	0.001	-0.017***	-0.021***	-0.003***	-0.005***
	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)
Bank FE	YES	YES	YES	YES	YES
Municipality FE	YES	YES	YES	YES	YES
Credit Controls	YES	YES	YES	YES	YES
Individual Controls	YES	YES	YES	YES	YES
Time Interactions	YES	YES	YES	YES	YES
Credit Conditions with Bank Interactions			YES		YES
N	871,638	829,484	829,484	829,484	829,484
R-squared	0.528	0.116	0.132	0.188	0.214

Notes: Authors' estimation of equation (2) for the ex-ante probability of default (log), probability of a credit being granted to a new client, probability of defaulting at least once in the following year after date of origination and the share of periods in default in the following year after the loan was contracted as dependent variables. Loan-level data taken from Banco de México administrative data from Jan-21 to Nov-22. Loans that only appear once in the database, when they´re originated, are omitted from the calculations in columns 3-6. Because not every loan has a term to maturity as long as 12 months, a control for the time window observed is added to columns 3-6. Credit controls consist of the probability of default (log) at the origination date only for columns 3-6, maturity (log), five dummy variables indicating the frequency of payment (weekly, 10 days, 14 days, 15 days, monthly or unique payment), two dummy variables showing if the payment is taken direct from payroll, is instructed as a direct debit or is done freely by the recipient (no method), and two more dummy variables as indicators for the group of risk, which may be high, medium or low. The individual characteristic control is the age of the recipient (log). Column 4 and 6 include credit conditions, i.e. the spread and loan amount (log) interacted by bank, as controls. The table shows point estimates of the effect of treatment, which consists of the interaction between the indicator of being a woman and the credit being granted after July 23 of 2021. Robust standard errors in parenthesis. The notation ***, **, and * correspond to statistical significance levels of 1%, 5%, and 10%, respectively.

Table 11 presents our findings on indirectly exploring the role of the improvement in credit conditions as an underlying causal mechanism of the effects on credit risk outcomes. As previously described, we indirectly explore this mechanism estimating the heterogeneous effects of having a previous client-bank relationship. We argue that this estimation is of economic interest as the pass-through effects of the reform seem to be concentrated on new female clients. To do so, we divide the sample into two subgroups according to the length of the client-bank relationship as we previously specified in the heterogeneous effects Section.

The first group is composed of individuals without previous client-bank relationship. The second group is comprised of previous clients.

Columns 1 and 2 show that the decline in the probability of defaulting at least once in the year following the loan's origination date is exclusively found among new female clients. Also, Columns 3 and 4 show the decline in the share of periods in default in the year following the loan creation to be six times more important for new clients that for previous clients. As for the regulatory calculation of the PD, the mean of this risk measure remained unchanged in both groups of clients (Columns 5 and 6). These results are consistent with our findings in the heterogeneous effects Section, where we showed that the decline in the spread and the increase in loan amount was concentrated on women without a previous relationship with the bank.

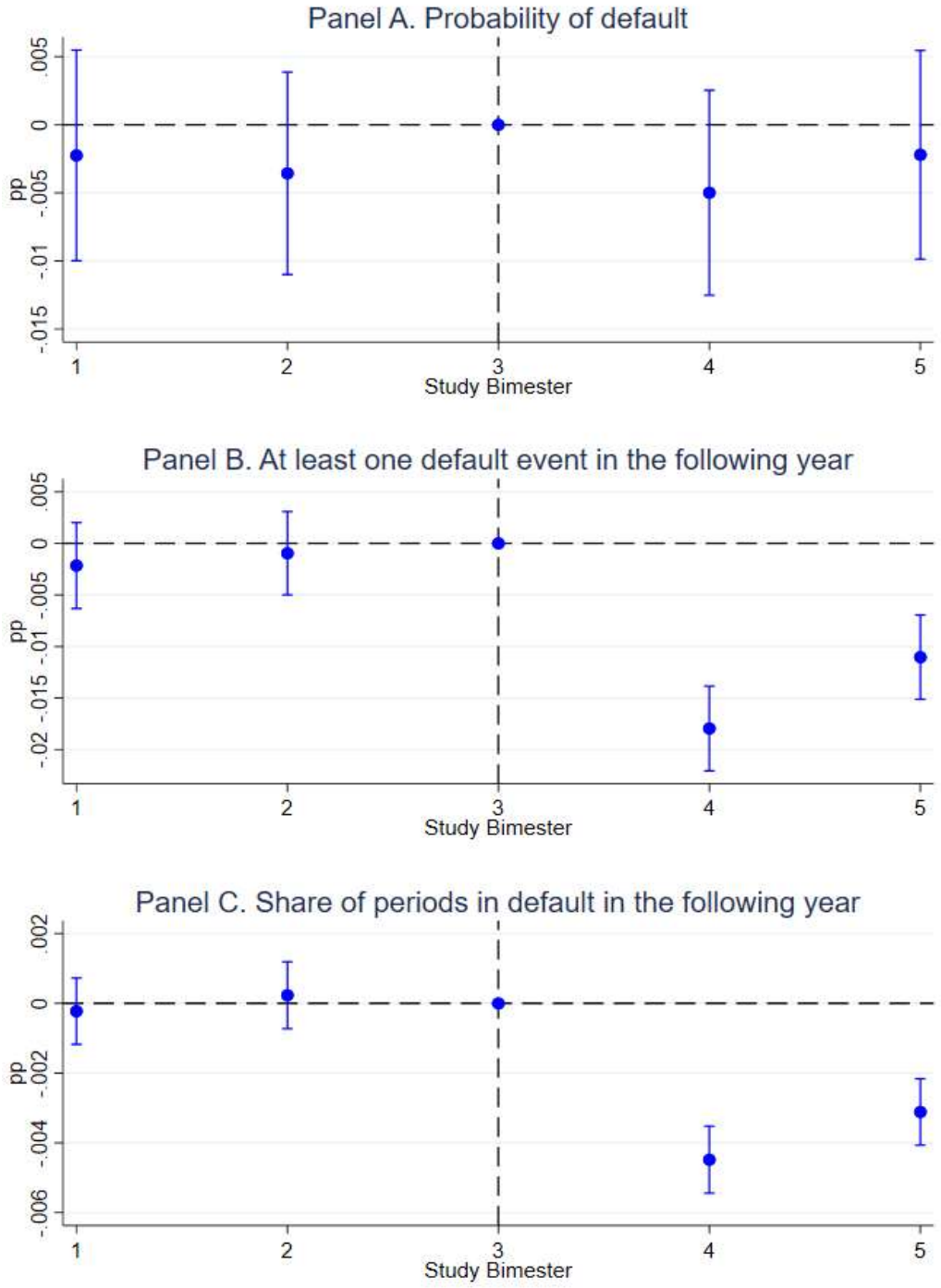
Table 11. 2x2 DiD – Heterogeneous Performance Effects by Length of the Client-Bank Relationship

	At least one default event in the following year		Share of periods in default in the following year		Probability of default (log)	
	New Client	Previous Client	New Client	Previous Client	New Client	Previous Client
	1	2	3	4	5	6
Treatment effect	-0.020***	-0.002	-0.006***	-0.001**	-0.003	0.000
	(0.002)	(0.002)	(0.000)	(0.000)	(0.004)	(0.003)
Female=1	-0.023***	-0.007***	-0.005***	-0.001**	0.000	0.003
	(0.001)	(0.001)	(0.000)	(0.000)	(0.002)	(0.002)
Bank FE	YES	YES	YES	YES	YES	YES
Municipality FE	YES	YES	YES	YES	YES	YES
Credit Controls	YES	YES	YES	YES	YES	YES
Individual Controls	YES	YES	YES	YES	YES	YES
Time Interactions	YES	YES	YES	YES	YES	YES
N	490,578	337,181	490,578	337,181	524,757	345,164
R-squared	0.137	0.143	0.195	0.269	0.330	0.410

Notes: Authors' estimation of equation (2) for the probability of defaulting at least once in the following year after the date of origination, the share of periods in default in the following year after the loan was contracted and the probability of default (log) as dependent variables. The conditional effect of the reform is shown in the table splitting up the sample into new loans to clients without previous relationship with the lender and clients already known by the bank. Loan-level data taken from Banco de México administrative data from Jan-21 to Nov-22. Loans that only appear once in the database, when they are originated, are omitted from the calculations in columns 1-2 and 3-4. Because not every loan has a term to maturity as long as 12 months, a control for the time window observed is added to columns 1-2 and 3-4. Credit controls consist of the probability of default (log) at the origination date only for columns 1-2 and 3-4, maturity (log), five dummy variables indicating the frequency of payment (weekly, 10 days, 14 days, 15 days, monthly or unique payment), two dummy variables showing if the payment is taken direct from payroll, is instructed as a direct debit or is done freely by the recipient (no method), and two more dummy variables as indicators for the group of risk, which may be high, medium or low. The individual characteristic control is the age of the recipient (log). The table shows point estimates of the effect of treatment, which consists of the interaction between the indicator of being a woman and the credit being granted after July 23 of 2021. Robust standard errors in parenthesis. The notation ***, **, and * correspond to statistical significance levels of 1%, 5%, and 10%, respectively.

Finally, event study estimates of the effects of the regulatory change on credit risk outcomes are shown in Figure 3. Panel A shows the dynamic effects of the reform on the loan performance measured as the probability that a client fails to repay her loan at least once during the year after the loan is created. As in the 2x2 estimates summarized in Table 10, we find that the reform improves the performance of loans directed to women in around 1.7 percentual points. This effect persists for the last bimester of 2021 (5th). Panel B shows that the effect on the share of periods in default is also consistent with what we found in the 2x2 estimates and that it persists as well through the 5^{th} bimester. Finally, Panel C supports results of the 2x2 DiD estimation for the null effect of the reform on the mean level of the regulatory measure of PD.

Figure 3: Event study results on loan performance



Notes: Authors' estimation of equation (3) for the for the probability of registering at least one default event in the following year after the date of origination, the share of periods in default in the following year after the loan was contracted and the probability of default (log) as dependent variables. Loan-level data taken from Banco de México administrative data from Jan-21 to Nov-22. The graphs show the point estimates, as well as the confidence interval of 95%, of the difference between the gender gap at each point in time and the prevailing gap observed during the third bimester of 2021, which is the last bimester before the reform came into effect that constitutes our baseline. Specification used includes the full set of controls as in Table 9.

Effects on financial inclusion

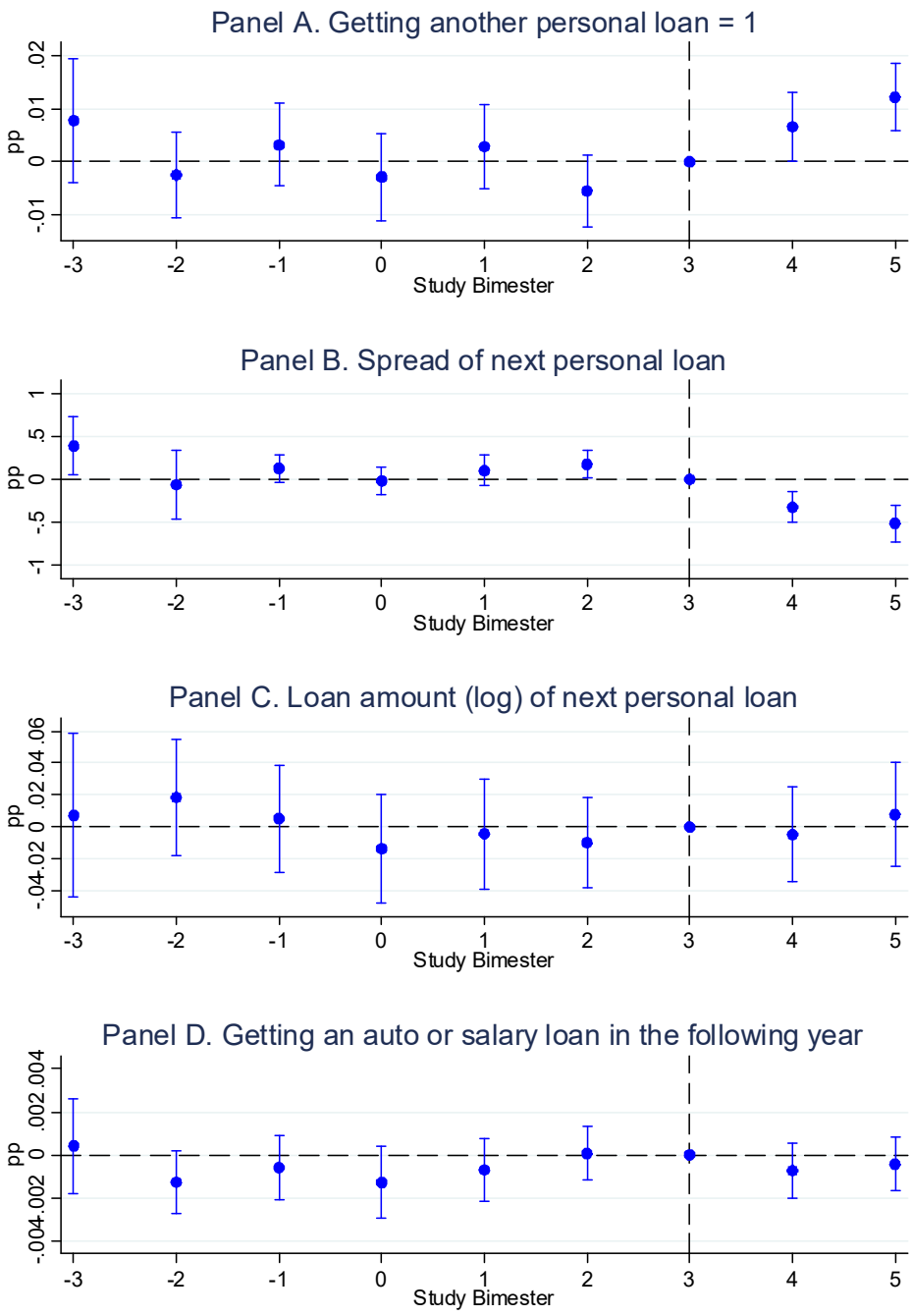
So far, our findings have shown that the reform reduced the barriers to entry into the credit market by improving the credit conditions of women with no previous relationship with the bank. In this Section, we assess whether these effects on new female clients were beyond a one-off improvement in access and credit conditions and increased their likelihood of staying in the credit market. Specifically, we first test whether being granted a personal loan increased the likelihood of getting a second personal loan, and whether this loan is granted with better credit conditions. Then, we explore whether the reform increased the likelihood of passing from personal to better quality loans such as automotive or salary-based. To this end, we estimate the dynamic effects of the regulatory change following the sample of new clients over a time 12-month time window starting one year before the reform.

Event study estimates of the effects of the regulatory change on the likelihood of being granted subsequent loans are summarized in Figure 4. In Panel A, our findings reveal that the reform is associated with a statistically significant increase in the likelihood of a female client getting a second personal loan in the following year. These effects are larger the more time individuals are exposed to the reform. Panel B and C summarize the difference in the pass-through effects between the first and subsequent personal loans. Panel B shows that subsequent personal loans are granted with a statistically significant decline in the interest rate. As in the likelihood case of Panel A, the decline in the interest rate is larger with more exposure to the reform. Panel C shows that there is not statistically significant difference in the loan amount of the first and subsequent loans.

Finally, Panel D presents the effects of the regulatory change on the likelihood of a female client passing from personal to automotive or salary-based subsequent loans. Our results show that there are not statistically significant changes in the likelihood of passing from personal to better quality loans. However, these null findings could be driven by structural constraints such as access to formal employment or could suggest that transitions to other types of loans take longer than the 12 months window covered in our analysis. Summing up, the regulatory change fosters financial inclusion of women by both reducing the barriers to entry into the credit market and increasing the likelihood of getting subsequent loans with better credit conditions. Though these findings are limited to personal loans, the

null effects on other types of loans could be explained by structural reasons beyond the scope of the reform or our analysis.

Figure 4: Financial inclusion effects on new clients



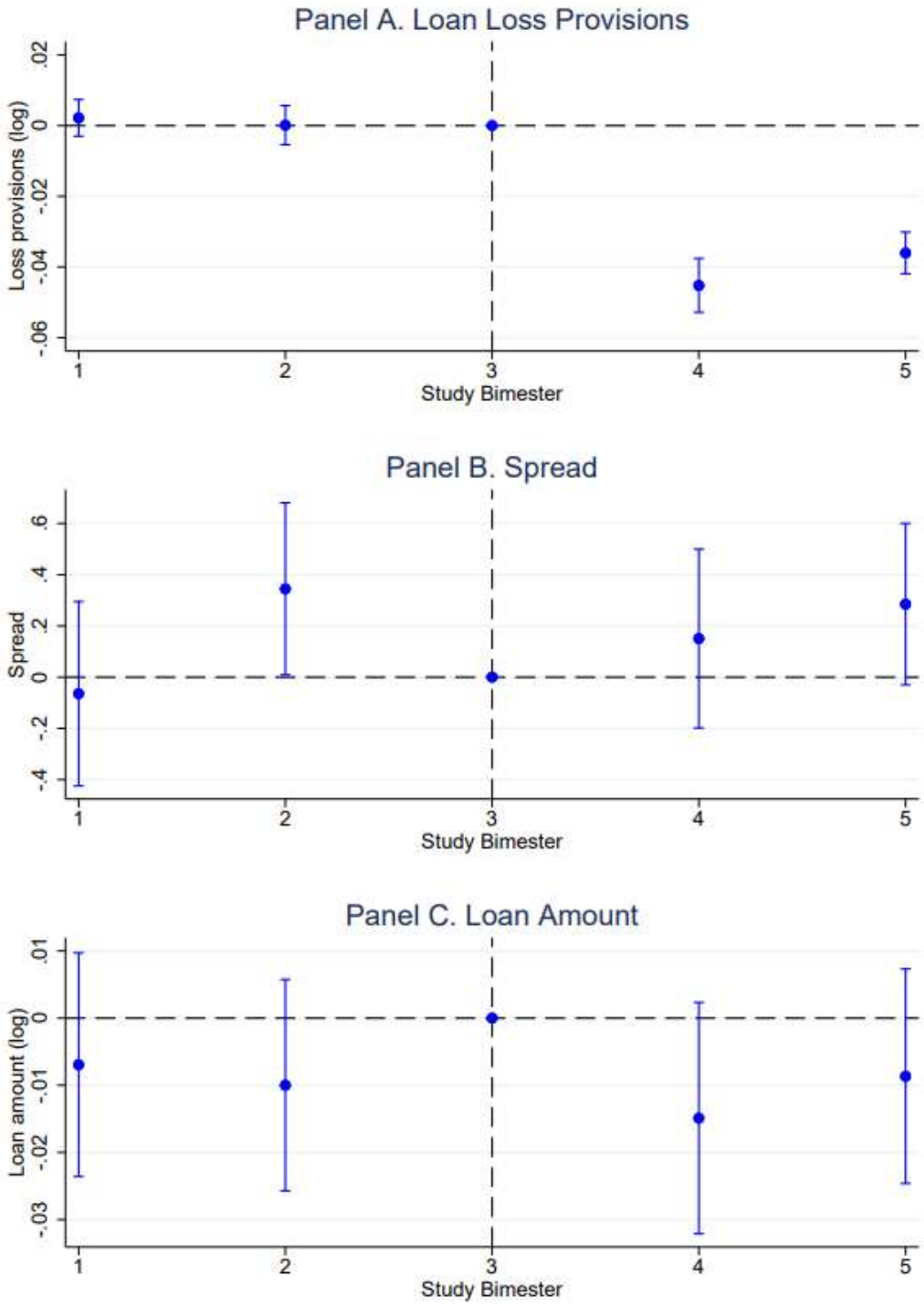
Notes: Authors' estimation of equation (3) for the probability of getting a second personal loan (Panel A), the difference of the spread between the first and second loan (Panel B), the difference in the loan amount between the first and second loan (Panel C) and the probability of getting an automotive or salary-based loan (Panel D). Loan-level data taken from Banco de México administrative data from Jul-20 to Nov-21 using the sample of individuals with no previous relationship with the bank. The graphs show the point estimates, as well as the confidence interval of 95%, of the difference between the gender gap at each point in time and the prevailing gap observed during the third bimester of 2021, which is the last bimester before the reform came into effect that constitutes our baseline. Specification used includes the full set of controls as in column 4 of Tables 5 and 6.

Impact of the regulatory change on other types of loans

In this Section, we present the impacts of the regulatory change on automotive and salary-based loans. As previously described in Section II, we expect these impacts to be smaller compared to personal loans due to both the loans' and reform's characteristics. Our hypothesis is based on the fact that, by construction, the reallocation of loss provisions driven by the reform is much lower in loans with lower probability of default such as automotive and salary-based loans. In this type of loans, the expected decline in loss provisions was 4 and 11 cents for each 100 pesos lent by a bank, respectively. These reductions are much smaller compared to the expected decline of 47 cents for every 100 pesos lent by a bank in personal loans. In fact, it is even lower than the sample of personal with low PD for which we find no effects in the previous Section.

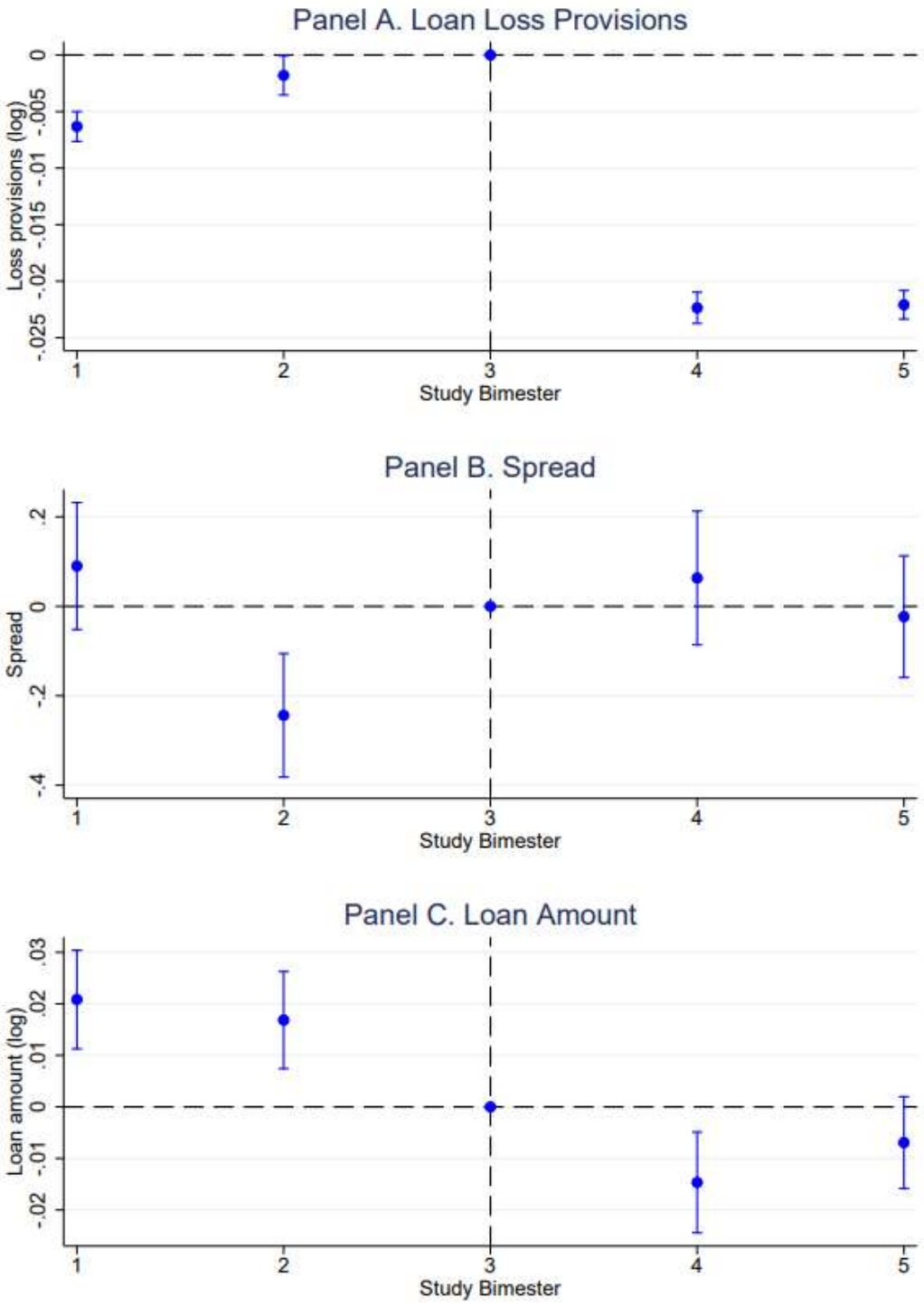
Taken together, our findings indicate that the regulatory change caused no economically relevant pass-through effects in the case of automotive and salary-based loans. Event study estimates of the effects of the regulatory change on automotive and salary-based loans are presented in Figures 5 and 6, respectively. Figure 5 shows that, though the reform had the intended effect on loss provisions, there were no statistically significant pass-through effects on credit conditions of automotive loans. Similarly, Figure 6 shows statistically significant effects on loss provisions of salary-based loans, but null effects on the interest rate and inconclusive effects on the loan amount due to the presence of pre-treatment trends. The 2x2 DiD estimates and the effects on the number and size of loans can be found in Tables A2 and A3 in the Appendix.

Figure 5: Event study result on automotive loans



Notes: Authors' estimation of equation (3) for the loan loss provisions (Panel A), total annual cost (Panel B) and loan amount (Panel C) of new salary loans granted by banks affected by the reform as the dependent variables. Loan-level data taken from Banco de México administrative data from Jan-21 to Nov-21. The graphs show the point estimates, as well as the confidence interval of 95%, of the difference between the gender gap at each point in time and the prevailing gap observed during the third bimester of 2021, which is the last bimester before the reform came into effect that constitutes our baseline. Specification used includes the full set of controls as in Table A2.

Figure 6: Event study result on salary-based loans

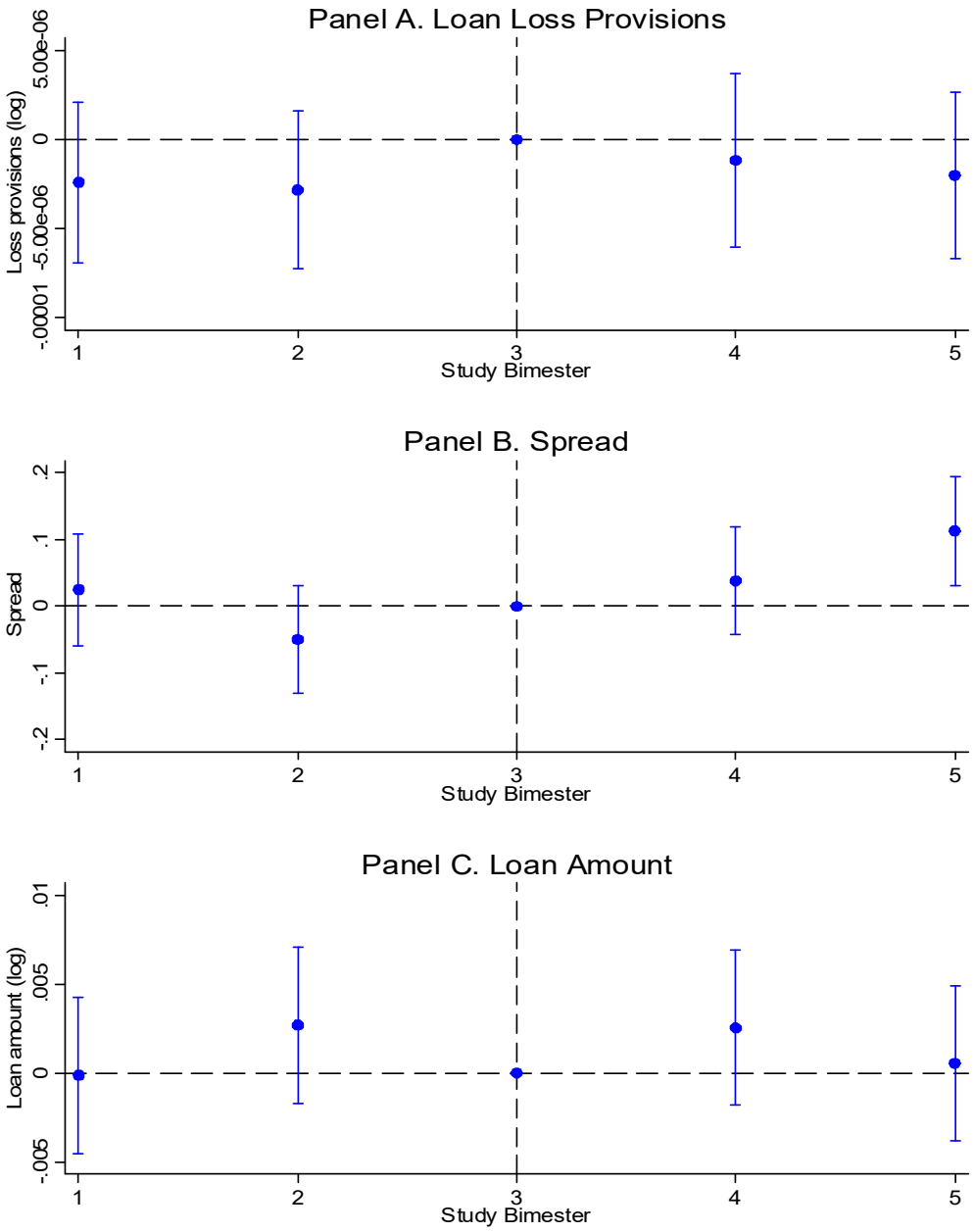


Notes: Authors' estimation of equation (3) for the loan loss provisions (Panel A), total annual cost (Panel B) and loan amount (Panel C) of new automotive loans granted by banks affected by the reform as the dependent variables. Loan-level data taken from Banco de México administrative data from Jan-21 to Nov-21. The graphs show the point estimates, as well as the confidence interval of 95%, of the difference between the gender gap at each point in time and the prevailing gap observed during the third bimester of 2021, which is the last bimester before the reform came into effect that constitutes our baseline. Specification used includes the full set of controls as in Table A2.

VI. Robustness Checks

We estimate two placebo specifications to strengthen the causal claims of our analysis. The event study plots of the first placebo estimations are summarized in Figure 7. A treat to our identification is that, contemporaneous with the reform, there might be other changes in the macroeconomic context affecting men and women differently. As previously described in Section II, the regulatory change was only mandatory for banks without an internal methodology to estimate loan loss provisions. So, we take advantage of this characteristic of the regulation and estimate a placebo effect of the reform on the unaffected banks. To this end, we estimate the effects on the loss provisioning, spread, and loan amount. As expected, results reveal that the reform did not change neither the loss provisions nor the credit conditions of new credits granted by unaffected banks. This finding suggests that there are no other contemporaneous shocks affecting all banks at the time of the reform that could biased our results.

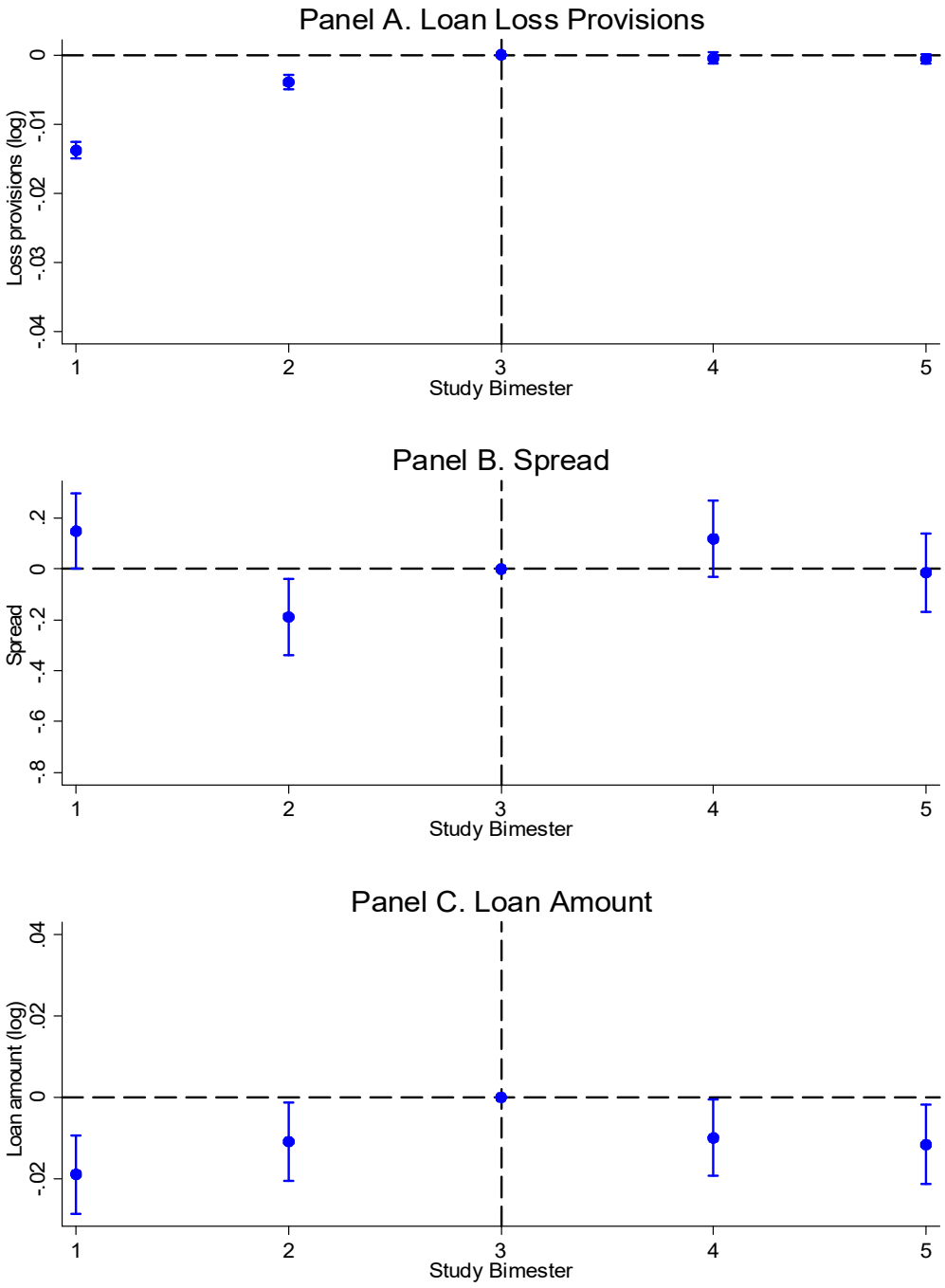
Figure 7: Placebo Estimates for Banks not Affected by the Reform in 2021



Notes: Authors' estimation of equation (2) for the loan loss provisions (Panel A), spread (Panel B) and loan amount (Panel C) of new personal loans granted by banks not affected by the reform as the dependent variables. Loan-level data taken from Banco de México administrative data from Jan-21 to Nov-21. The graphs show the point estimates, as well as the confidence interval of 95%, of the difference between the gender gap at each point in time and the prevailing gap observed during the third bimester of 2021, which is the last bimester before the reform came into effect that constitutes our baseline. Specification used includes the full set of controls as in column 4 of Tables 5 and 6.

We run a second set of placebos for the sample of banks affected by the reform but using a different year. It could be the case that our estimates are driven by some type of seasonality that affect men and women differently. If this were the case, the effects that we find for the period after the reform could be bias. To test this, we repeat our analysis using the year 2019, aiming to avoid noisy estimates coming from 2020 data due to the Covid-19 pandemic, and to avoid noisy estimates of 2022 data, when Mexico adopted the IFRS9 international standards for financial stability. Event study results from placebo estimates for 2019 are summarized in Figure 8. As expected, we do not find any kind of treatment effect neither for the loss provisions, nor for the credit conditions. This exercise suggest that our results are not bias by any kind of gender-specific seasonality during the calendar year.

Figure 8: Placebo Estimates in 2019



Notes: Authors' estimation of equation (2) for the loan loss provisions (Panel A), spread (Panel B) and loan amount (Panel C) of new personal loans granted by banks affected by the reform as the dependent variables. Loan-level data taken from Banco de México administrative data from Jan-19 to Nov-19. The graphs show the point estimates, as well as the confidence interval of 95%, of the difference between the gender gap at each point in time and the prevailing gap observed during the third bimester of 2019. Specification used includes the full set of controls as in column 4 of Tables 5 and 6.

VII. Conclusions

In this paper, we estimate the causal effect of a reduction in loan loss provisions for loans granted to women on credit and interest rates. We use Mexico as a case study. To our knowledge, this is the first paper that investigates the effect of regulatory policies on women's financial inclusion in a LMIE. Specifically, we estimate the effect of a regulatory change that mandated a reduction of 4 percent in women's credit loan loss provisions for personal loans. To this end, we utilized a proprietary dataset that contains information on all personal loans extended to individuals by commercial banks throughout 2021.

We took advantage of the exogenous nature of the regulatory change to conduct both 2x2 DiD and a dynamic DiD estimations to calculate the causal effect of the regulatory change. We first estimated the effect of the reform on the levels of loan loss provisioning to verify that actual loss provisioning declined as intended. Then, we estimated the pass-through effects of the reform on other financial inclusion variables such as the spread, loan amount, and number of female loans. Our results revealed that the regulatory change caused a decline in loss provisioning as initially intended. We found suggestive evidence that the reform increased the likelihood of a loan being granted to a woman and the share of total amount directed to them, but the identification of the causal estimate was challenging due to the presence of pre-treatment trends.

Hence, we focused our analysis of the regulatory change on estimating the pass-through and heterogeneous effects of the reform on the credit conditions. Our results revealed that the decline of loss provisioning had pass-through effects on the spread and loan amount. The magnitude of the pass-through effects was small in absolute terms, but large enough to swap over the gender gap in the spread, and to reduce in over 40 percent the gap in loan amount. The heterogeneous effects estimations reveal that the pass-through effects were larger for female clients with higher PD, and for women with no previous relationship with the bank compared to individuals with a pre-existing bank relation. Thus, women entering into a new bank-relationship do so with better credit conditions. Moreover, we found that the pass-through effects of the reform were larger in municipalities with higher levels of labor informality, which also had a larger number of individuals with no previous relationship with the bank, higher probability of default, and where personal loans played a more important role compared to other types of credit such as salary-based and automotive loans.

Then, we sought to estimate whether the reform increased credit risk by promoting credit allocation to new and a priori riskier female clients. Our findings revealed that the regulatory change did not have negative unintended consequences in terms of financial stability. On the contrary, we found that the reform was associated to a statistically significant decline of 0.4 percentual points in the share of periods in default. This improvement in credit performance was conditional on banks not changing their risk-taking appetite measured by the ex-ante calculation of the probability of default associated to new loans. When exploring the underlying causal mechanisms of these results, we found that the improvements in credit performance for female borrowers could be explained by the upswings in credit conditions associated to the reform, since these conditions, such a lower interest rate, tend to be associated with lower probability of default.

We also assessed whether the effects of the regulatory change were beyond a one-off improvement in access and credit conditions, and increased the likelihood of new female clients staying in the credit market. We found that the reform increased their likelihood of getting a second personal loan in the next twelve months. Our results showed that subsequent loans have lower interest rate, but there were not statistically significant differences in loan amount. We did not find statistically significant evidence that the reform increased the likelihood of transitioning from personal to automotive or salary-based loans. These findings implied that, while the reform generated some long-lasting effects on financial inclusion by increasing the likelihood of getting another personal loan with better credit conditions, individuals were not able pass from personal to other types of credit that are commonly associated with better credit conditions.

Finally, we estimated the effects of the regulatory change on other types of credit that were also affected by the reform such as automotive and salary-based loans. We expected these impacts to be smaller than for personal loans based on the fact that, by construction, the reallocation of loss provisions driven by the reform is much lower in loans with lower ex-ante probability of default. Consistent with this hypothesis, our findings indicated that, though the reform had the intended effect on loss provisions, it caused no economically relevant pass-through effects for automotive and salary-based loans

References

Agier, I., & Szafarz, A. (2013). Microfinance and Gender: Is There a Glass Ceiling on Loan Size? *World Development*, *42*, 165–181. https://doi.org/10.1016/j.worlddev.2012.06.016

Alesina, A. F., Lotti, F., & Mistrulli, P. E. (2013). Do Women Pay More for Credit? Evidence from Italy. *Journal of the European Economic Association*, *11*, 45–66.

Allen, F., Demirguc-Kunt, A., Klapper, L., & Martinez Peria, M. S. (2016). The foundations of financial inclusion: Understanding ownership and use of formal accounts. *Journal of Financial Intermediation*, *27*, 1–30. https://doi.org/10.1016/j.jfi.2015.12.003

Aristei, D., & Gallo, M. (2016). Does gender matter for firms' access to credit? Evidence from international data. *Finance Research Letters*, *18*, 67–75. https://doi.org/10.1016/j.frl.2016.04.002

Aslan, G., Delechat, C., Newiak, M., & Yang, M. F. (2017). *Inequality in Financial Inclusion, Gender Gaps, and Income Inequality*. IMF Working Papers.

Aterido, R., Beck, T., & Iacovone, L. (2013). Access to Finance in Sub-Saharan Africa: Is There a Gender Gap? *World Development*, *47*, 102–120. https://doi.org/10.1016/j.worlddev.2013.02.013

Beck, T., & Demirgüç-Kunt, A. (2008). Access to Finance: An Unfinished Agenda. *The World Bank Economic Review*, *22*(3), 383–396.

Bellucci, A., Borisov, A., & Zazzaro, A. (2010). Does gender matter in bank–firm relationships? Evidence from small business lending. *Journal of Banking & Finance*, *34*(12), 2968–2984. https://doi.org/10.1016/j.jbankfin.2010.07.008

Borusyak, K., Jaravel, X., & Spiess, J. (2022). *Revisiting Event Study Designs: Robust and Efficient Estimation* (SSRN Scholarly Paper No. 2826228). https://doi.org/10.2139/ssrn.2826228

Buvinic, M., & Berger, M. (1990). Sex differences in access to a small enterprise development fund in Peru. *World Development*, *18*(5), 695–705. https://doi.org/10.1016/0305-750X(90)90018-S

de Chaisemartin, C., & D'Haultfœuille, X. (2022). Two-way fixed effects and differences-in-differences with heterogeneous treatment effects: A survey. *The Econometrics Journal*, utac017. https://doi.org/10.1093/ectj/utac017

Delechat, C., Kiyasseh, L., MacDonald, M., & Xu, R. (2020). *Regulatory Policies and Financial Inclusion*. IMF Working Papers.

Delechat, M. C. C., Newiak, M. M., Xu, R., Yang, M. F., & Aslan, G. (2018). *What is Driving Women's Financial Inclusion Across Countries?* International Monetary Fund.

Demirgüç-Kunt, A., Klapper, L. F., & Singer, D. (2013). *Financial Inclusion and Legal Discrimination Against Women: Evidence from Developing Countries* (SSRN Scholarly Paper No. 2254240). https://papers.ssrn.com/abstract=2254240

Demirguc-Kunt, A., Klapper, L., Singer, D., & Ansar, S. (2022). *The Global Findex Database 2021: Financial Inclusion, Digital Payments, and Resilience in the Age of COVID-19*. World Bank. https://doi.org/10.1596/978-1-4648-1897-4

Galli, E., Mascia, D. V., & Rossi, S. P. S. (2018). Does Corruption Influence the Self-Restraint Attitude of Women-led SMEs towards Bank Lending? *CESifo Economic Studies*, *64*(3), 426–455. https://doi.org/10.1093/cesifo/ifx021

Galli, E., Mascia, D. V., & Rossi, S. P. S. (2020). Bank credit constraints for women-led SMEs: Self-restraint or lender bias? *European Financial Management*, *26*(4), 1147–1188. https://doi.org/10.1111/eufm.12255

Gonzales, C., CGonzales@imf.org, Jain-Chandra, S., SJain-Chandra@imf.org, Kochhar, K., KKochhar@imf.org, Newiak, M., MNewiak@imf.org, Zeinullayev, T., & TZeinullayev@imf.org. (2015). Catalyst for Change: Empowering Women and Tackling Income Inequality. *Staff Discussion Notes*, *15*(20), 1. https://doi.org/10.5089/9781513533384.006

Honohan, P. (2008). Cross-country variation in household access to financial services. *Journal of Banking & Finance*, *32*(11), 2493–2500. https://doi.org/10.1016/j.jbankfin.2008.05.004

Klapper, L., & Parker, S. (2011). *Gender and the Business Environment for New Firm Creation*. https://doi.org/10.1093/wbro/lkp032

Mookerjee, R., & Kalipioni, P. (2010). Availability of financial services and income inequality: The evidence from many countries. *Emerging Markets Review*, *11*(4), 404–408. https://doi.org/10.1016/j.ememar.2010.07.001

Muravyev, A., Talavera, O., & Schäfer, D. (2009). Entrepreneurs' gender and financial constraints: Evidence from international data. *Journal of Comparative Economics*, *37*(2), 270–286. https://doi.org/10.1016/j.jce.2008.12.001

Perrin, C., & Weill, L. (2022). No man, No cry? Gender equality in access to credit and financial stability. *Finance Research Letters*, *47*, 102694. https://doi.org/10.1016/j.frl.2022.102694

Sant'Anna, P. H. C., & Zhao, J. (2020). Doubly robust difference-in-differences estimators. *Journal of Econometrics*, *219*(1), 101–122. https://doi.org/10.1016/j.jeconom.2020.06.003

Svirydzenka, K. (2016). *Introducing a New Broad-based Index of Financial Development*. International Monetary Fund.

Swamy, V. (2014). Financial Inclusion, Gender Dimension, and Economic Impact on Poor Households. *World Development*, *56*, 1–15. https://doi.org/10.1016/j.worlddev.2013.10.019

Appendix

Table A1. Reform on Number and Size of Women's Loans by Bank Relationship

	Dependent variable: Probability of being a female recipient					
	Sample of New Clients			Sample of Old Clients		
	Reduced Form	FE	Two-way FE	Reduced Form	FE	Two-way FE
	1	2	3	4	5	6
First bimester	-0.049***	-0.036***	-0.026***	0.010	-0.008	-0.016
	(0.018)	(0.010)	(0.009)	(0.017)	(0.012)	(0.012)
Second bimester	-0.064	-0.018*	-0.023**	-0.005	-0.007	-0.011
	(0.055)	(0.010)	(0.010)	(0.022)	(0.015)	(0.014)
Fourth bimester	-0.040***	-0.025***	-0.011	0.035*	0.014	-0.001
	(0.012)	(0.010)	(0.011)	(0.018)	(0.012)	(0.011)
Fifth bimester	0.022	0.017	-0.005	0.040**	0.024**	-0.001
	(0.022)	(0.012)	(0.011)	(0.016)	(0.010)	(0.010)
Weighted by loan amount	YES	YES	YES	YES	YES	YES
Municipality FE		YES	YES		YES	YES
Bank FE			YES			YES
N	525,222	525,176	525,176	346,417	346,295	346,295
R-squared	0.004	0.073	0.113	0.001	0.045	0.070

Notes: Authors' estimation for the probability of being a female recipient of a new personal loan as the dependent variable on bimester dummies. The sample was divided into two subsamples based on the length of the client-bank relationship where individuals with no previous relationship with the bank were tagged as new clients. Loan-level data taken from Banco de México administrative data from Jan-21 to Nov-21. All estimations are weighted by loan amount. The table shows point estimates of the difference between the probability of being a female recipient at each point in time and the probability of such an event happening during the third bimester of 2021, which is the last bimester before the reform came into effect that constitutes our baseline period. Robust standard errors in parenthesis. The notation ***, **, and * correspond to statistical significance levels of 1%, 5%, and 10%, respectively.

Table A2. 2x2 DiD – Effects on other types of loans

	Salary Loans			Automotive Loans		
	Loss provisions (log)	Spread	Amount (log)	Loss provisions (log)	Spread	Amount (log)
	1	2	3	4	5	6
Treatment effect	-0.020***	0.061	-0.022***	-0.041***	0.128	-0.006
	(0.000)	(0.046)	(0.003)	(0.002)	(0.114)	(0.006)
Female = 1	0.003***	-0.866***	-0.022***	-0.004***	-0.518***	0.011***
	(0.000)	(0.029)	(0.003)	(0.001)	(0.074)	(0.003)
Bank FE	YES	YES	YES	YES	YES	YES
Municipality FE	YES	YES	YES	YES	YES	YES
Credit Controls	YES	YES	YES	YES	YES	YES
Individual Controls	YES	YES	YES	YES	YES	YES
Time Interactions	YES	YES	YES	YES	YES	YES
N	1,317,344	1,317,344	1,317,344	104,395	104,395	104,395
R-squared	0.989	0.363	0.302	0.956	0.313	0.423

Notes: Authors' estimation of equation (2) for the loss provisions (log), total annual cost (%) and amount(log) of new salary and automotive loans as the dependent variables. Loan-level data taken from Banco de México administrative data from Jan-21 to Nov-21. Credit controls consist of default probability (log), maturity, five dummy variables indicating the frequency of payment (weekly, 10 days, 14 days, 15 days, monthly or unique payment), two dummy variables showing if the payment is taken direct from payroll, is instructed as a direct debit or is done freely by the recipient (no method), and two more dummy variables as indicators for the group of risk, which may be high, medium or low. The individual characteristic control is the age of the recipient (log). Reduced form of column 1 and 4 includes loan amount (log) as control. In order to replicate de 2% drop in loss provisions outliers defined as the top and bottom 5% of term to maturity and loan-amount were excluded from the sample. Also, the control variable of term to maturity is included as a level instead of a logarithmic transformation. These methodologic decisions do not alter our results for personal loans. The table shows point estimates of the effect of treatment, which consists of the interaction between the indicator of being a woman and the credit being granted after July 23 of 2021. Robust standard errors in parenthesis. The notation ***, **, and * correspond to statistical significance levels of 1%, 5%, and 10%, respectively.

Table A3. Reform on Number and Size of Women's Other Loans

	Dependent variable: Probability of being a female recipient			
	Salary Loans		Automotive Loans	
	Reduced Form	Reduced Form	Reduced Form	Reduced Form
	1	2	3	4
First bimester	-0.015***	-0.008***	0.005	0.013**
	(0.001)	(0.002)	(0.004)	(0.005)
Second bimester	-0.001	0.004*	0.002	0.004
	(0.001)	(0.002)	(0.004)	(0.005)
Fourth bimester	-0.013***	-0.019***	-0.008*	-0.003
	(0.001)	(0.002)	(0.005)	(0.005)
Fifth bimester	-0.015***	-0.016***	0.004	0.006
	(0.001)	(0.002)	(0.004)	(0.005)
Weighted by loan amount		YES		YES
Municipality FE				
Bank FE				
N	1,616,274	1,616,274	126,789	126,789
R-squared	0.000	0.000	0.000	0.000

Notes: Authors' estimation for the probability of being a female recipient of a new personal loan as the dependent variable on bimester dummies. Loan-level data taken from Banco de México administrative data from Jan-21 to Nov-21. Columns 2 and 4 include results weighted by loan amount. The table shows point estimates of the difference between the probability of being a female recipient at each point in time and the probability of such an event happening during the third bimester of 2021, which is the last bimester before the reform came into effect that constitutes our baseline period. Robust standard errors in parenthesis. The notation ***, **, and * correspond to statistical significance levels of 1%, 5%, and 10%, respectively.