Bad Taste: Gender Discrimination in Consumer Credit Markets

By Ana María Montoya, Eric Parrado, Alex Solís and Raimundo Undurraga *

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We randomly assigned consumer loan requests (of random amount and length) to gender-balanced prospective-borrowers who then randomly submitted them to a representative sample of loan-officers from Chilean banks. We find that loan requests submitted by women are 18.3% less likely to be approved, with most of the gender effect coming from gender-biased officers, particularly males. We further randomly informed some officers about official statistics indicating that women have higher repayment rates than men and find that gender-biased officers in the treatment-group discriminated more against women relative to their control counterparts, suggesting overconfidence bias as a potential mechanism behind taste-based discrimination.

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* Corresponding Author: Undurraga, Department of Industrial Engineering and Center for Applied Economics, University of Chile, Beaucheff 851, Santiago, Chile; e-mail: raimundo.undurraga@dii.uchile.cl. We thank María Gracia Evans, Alejandra Sánchez, and Piero Zanocco for excellent research assistance. This paper has benefited from helpful comments from Brendan Daley, Andrew Davis, Elton Dusha, Juan Escobar, Verónica Frisancho, Jonathan Morduch, Carlos Noton, Marcelo Olivares, Debraj Ray, Carlos Scartascini, Diego Vera-Cossio, and Razvan Vlaicu. We appreciate the financial support provided by CAF, MIPP, and IADB. The authors disclose that they have no financial or material interests in the reported results. IRB approval to implement this project was provided by the Ethical Committee at the FCFM, University of Chile. The results presented in this paper follow the pre-analysis plan registered with the AEA RCT Registry under the code AEARCTR-0003961 (http://www.socialscienceregistry.org/trials/3961). All errors are our own.

Gender disparities are pervasive in consumer credit markets. Compared to female borrowers, men are more likely to get access to consumer loans and pay lower interest rates (WEF (2018))[1]. Such inequalities are stemming in part from observable gender gaps originated in the labor market (Hausmann, Tyson and Zahidi (2009), Goldin (2014), Demirguc-Kunt et al. (2017)), although the role played by discriminatory actions against women cannot be discarded (Alesina, Lotti and Mistrulli (2013)). Uncovering gender discrimination and its mechanisms is critical for an appropriate welfare analysis of the consumer credit market. For instance, statistical discrimination is argued to be efficient as an optimal response to an asymmetric information problem between loan officers and applicants (Phelps (1972), Arrow (1973)), whereas decisions based on taste-based gender preferences on the part of loan officers can lead to welfare loss (Becker (1957)).

In consumer credit markets, gender discrimination occurs if an otherwise identical loan request is treated differently by virtue of the applicant's gender, and if gender by itself has no direct effect on the expected profits associated with the loan request. That would be the case if identical loan requests submitted by male and female borrowers with equal ability to repay are treated differently. Yet identifying discrimination on the basis of gender using observational data is problematic since the data requirements to make *ceteris paribus* comparisons across male and female borrowers are extensive (Heckman and Siegelman (1992), Heckman (1998), Blanchflower, Levine and Zimmerman (2003), Han (2004), Alesina, Lotti and Mistrulli (2013), Fisman, Paravisini and Vig (2017))[2]. A solution is to implement a correspondence study where manipulated loan requests are randomly

[1]This has motivated the implementation of a number of anti-discrimination policies aimed to neutralize animus against minority groups. A recent example is the Equality Act of 2010 in the U.K. Also, the U.S. Congress enacted the Home Mortgage Disclosure Act (HMDA) in 1975 which, along with the FIRREA amendments of 1989, requires the collection and disclosure of data on applicant and borrower characteristics to assist in identifying possible discriminatory lending patterns and enforcing anti-discrimination statutes.

[2]Since researchers typically possess far less data on borrowers than banks do, male and female borrowers that appear similar to researchers may look very different to loan officers. Moreover, we usually do not observe how banks internally match loan requests with loan officers. As a result, the observed differences in credit conditions between male and female borrowers could be driven by omitted variable bias associated with either borrower or loan officer unobserved characteristics.

sent to loan officers while randomizing the gender that appears in the application form. This way, discrimination is a causal effect defined by a hypothetical *ceteris paribus* conceptual experiment, i.e., varying applicant gender but keeping all else constant. However, implementing such an experiment in the formal credit market is unfeasible since the lending process typically starts by checking the applicant's identity through credit reporting bureaus like Experian, TransUnion, or Equifax, which allows loan officers to easily identify false loan requests that manipulate gender[3].

This paper overcomes that limitation through the implementation of a correspondence study with real loan applicants where instead of manipulating the applicant's gender we randomly assign loan requests to gender-balanced prospective borrowers who then submit the assigned requests to randomly assigned loan officers. By design, we ensure that the applicant and loan request features, as well as the loan officer characteristics, are all statistically identical across applicant gender.

The experiment was implemented in Chile and worked as follows[4]. We built a balanced sample of 404 male and female potential borrowers ("testers") who were willing to participate in the experiment. The sample is composed by young testers aged between 25 and 35, and it is statistically balanced across borrower's gender on demographics, educational level, incomes, financial position, employment status, and credit history, i.e., the precise applicant-level variables required by banks to evaluate loan requests. We assigned 4 loan requests per tester, which randomly varied across 9 types of loans in both amount (ranging between $1,500 and $13,500 US dollars) and term (12 to 60 months). The tester was then man-

[3]There are a number of ways for financial institutions to verify identity, either online or offline. For instance, most banks and credit unions in the United States utilize one of the major credit reporting bureaus: Experian, TransUnion, and Equifax. These companies compile an assortment of personally identifiable information that allows financial institutions to verify identity via knowledge-based questions. For instance, a bank might use name, address, and SSN to pull an individual's credit report and then ask the applicant questions derived from the report to verify the individual's identity. The institution will also typically require the applicant to scan an ID. There are also services that help verify identity via email addresses, social profiles, and IP addresses.

[4]A pre-analysis plan was pre-registered with the AEA RCT Registry before the data was analyzed, which is posted at http://www.socialscienceregistry.org/trials/3961.

dated to email each loan request to a randomly assigned loan officer drawn from a representative sample of loan officers working in the formal credit market. Each tester was mandated to quote 4 loan requests from 4 different loan officers, for a total sample of 1,616 loan requests. The testers were trained to interact in a systematic way with loan officers and forward all tester-officer interactions to the research team, from the loan quote request made by the tester to the loan officer's final decision. All applications occur in the data multiple times, sometimes as coming from a male applicant and sometimes from a female applicant, and thus our design allows us to obtain a within-application estimate of gender discrimination in terms of response and approval rates[5].

Our sample of loan officers work in banks that receive more than 95% of all consumer loan applications, and it is representative of the officer labor force in the Chilean banking system. Hence, our experiment provides an accurate measure of the market-level gender discrimination that takes place in the Chilean consumer credit market. Loan officers were surveyed at baseline through a collaborative agreement with the National Authority of Banking Regulation, SBIF, so we observe a large set of demographic and socioeconomic covariates at the officer level. Importantly, the officers' baseline survey also includes a battery of subjective measures and experimental tests aimed at eliciting beliefs and preferences about male and female clients, which allows us to examine the role of gender bias on gender discrimination. Moreover, our randomization scheme stratifies by loan amount, loan officer gender, and affiliated bank, so we can causally identify heterogeneous effects across these dimensions and examine, for example, the extent to which gender discrimination differs across male and female loan officers, or whether gender discrimination varies with the requested loan amount[6].

[5]We further examine whether the credit conditions attached to approved loans (interest rate, loan payment, etc.) vary by applicant's gender, although the exercise is just observational since acceptance/rejection decisions are endogenous to loan officer or bank approval criteria.

[6]An alternative strategy would be to implement the so-called "outcome test" (Becker (1957)), which consists of examining the presence of gender discrimination among marginal loan requests, i.e., for those that are at the limit of approval criteria. If marginal loan requests submitted by female applicants yield higher profits to the lender than marginal loan requests submitted by male applicants, and that difference cannot be explained by omitted variables and/or statistical discrimination, then there are grounds to

We find that the approval rate of loan requests submitted by female borrowers is 18.3% lower compared to the approval rate of otherwise identical loan requests submitted by male counterparts. The effect is sizable. It is equivalent to the difference in approval rates between borrowers whose incomes are in the 4^{th} and 7^{th} deciles of the income distribution. Furthermore, we estimate that the median forgone profits associated to applications rejected due to gender discrimination amounts to 1,785 USD or 23% of the median loan size (≈7,500 USD). Considering only discriminated applications from applicants aged 25-35 for amounts between $1,500-13,500 USD (our sample frame), the forgone profits at the industry level amounts to 5.8 million dollars per year, which is equivalent to the annual cost of hiring 4% of the officer labor force in the Chilean banking system.

A potential mechanism driving gender discrimination is animus against women or taste-based discrimination. We examine this hypothesis by using a battery of subjective measures and experimental tests aimed at eliciting gender preferences and thus identify gender-biased loan officers, as in Charles and Guryan (2008). We show that officers whose gender preferences are not biased against women (i.e., gender-neutral or pro-female) do not discriminate either in favor or against female borrowers. In contrast, among pro-male officers, approval rates for women are 54% lower relative to approval rates for men, suggesting that gender discrimination against female borrowers is arguably due to taste-based sources. Moreover, when this result is disaggregated by loan officer gender, we find that the bulk of the effect is coming from male loan officers who are pro-male, which is congruous with evidence showing that gender discrimination against women is more likely to occur in male-female relationships than in female-female ones (Schmitt et al. (2002), Figart (2005), Anwar and Fang (2006), Delavande and Zafar (2013), Beck, Behr and Guettler (2013), Montalvo and Reynal-Querol (2019)). Indeed, consistent with this result, we observe substantial variability of male-female differences in both response and approval rates across banks and show that banks with a larger

argue in favor of taste-based discrimination against female applicants.

proportion of male officers in their staff headcount are associated with larger levels of discrimination against women.

Interestingly, our results are given in a context where official statistics show that women have higher repayment rates than men, suggesting that (inaccurate) statistical discrimination might also be at work. We test for this by implementing a gender salience experiment aimed to "correct" the potentially biased beliefs that loan officers may have regarding the repayment performance of male and female borrowers. In particular, half of our sample of loan officers was randomly assigned a message informing them that female borrowers perform better than men in terms of repayment rates as well as acknowledging the potential costs associated with gender discrimination in the consumer credit market. Relative to the control group, we find that treated loan officers do not discriminate less against female borrowers in terms of response and approval rates. However, among pro-male officers, we find that those who received the treatment message increased gender discrimination compared to their control counterparts, a result that reinforces the taste-based mechanism.

Following the theoretical work of Heidhues, Köszegi and Strack (2019), we hypothesize that pro-male officers counter-reacted to the treatment message due to their self-serving views about discrimination and this is potentially due to overconfidence bias: they overestimate the degree of discrimination against any group whose preferences they are personally aligned with (e.g. male applicants) and underestimate discrimination against any group they compete with or are not aligned with (e.g. female applicants). Indeed, Bohnet (2016) recognizes that while changing the discriminator agent's view of society can be helpful, attempts to debias him through the provision of better information may backfire. She shows that explicit diversity training programs in US corporations have made between-group differences more salient, which in turn have generated more discrimination against minority groups, not less.

Finally, we examine the relationship between credit market structure and gender discrimination. Becker (1957)'s model predicts that as new entrants in the lending market take advantage of the opportunity to cash in on the profits, the relative cost of discriminating against female applicants increases, and thus biased lenders are competed away. We test Becker's hypothesis by combining experimental data on gender discrimination with the Herfindahl-Hirschman Market Concentration Index based on the number of local offices that banks have in each municipality. We find that female-male differences in approval rates tend to be larger in municipalities with higher levels of market concentration, but this is only the case for loan requests submitted to pro-male loan officers, i.e., precisely those who are biased against female borrowers. This result meets Becker's theory of discrimination in that the underlying mechanism behind the negative effects of market competition on gender discrimination is likely to be that taste-based attitudes against women are no longer profitable.

A number of recent studies have used observational data to test for the presence of gender discrimination in the credit market, most notably Carter et al. (2007), Muravyev, Talavera and Schafer (2009), Barasinska and Schafer (2010), Bellucci, Borisov and Zazzaro (2010), Agier and Szafarz (2013), Alesina, Lotti and Mistrulli (2013), Stefani and Vacca (2013), Mascia and Rossi (2017), Beck, Behr and Madestam (2018), Montalvo and Reynal-Querol (2019), and Andreeva and Matuszyk (2019). The overwhelming majority of these papers find that women are discriminated against when accessing the credit market, and this is both at the external margin (obtaining credit approval) and at the internal margin (credit conditions offered)[7]. The most novel aspect of our paper is that it provides causal estimates of gender discrimination by using a randomized correspondence study where borrowers and officers interact in a real setting. To the best of our knowl-

[7]Discrimination in the access to credit markets has also been studied across other dimensions like race (Van Order, Vassilis and Quigley (1993), Berkovec, Canner and Hannan (1998), Blanchflower, Levine and Zimmerman (2003), Charles and Hurst (2002), Han (2004), Ross et al. (2008), Pope and Sydnor (2011), Hanson et al. (2016), Deku, Kara and Molyneux (2016), Bayer, Ferreira and Ross (2017), Bartlett et al. (2018)), ethnicity and cultural proximity (Charles, Hurst and Stephens (2008), Bayer, Ferreira and Ross (2017), Cavalluzzo and Cavalluzzo (1998), Cavalluzzo, Cavalluzzo and Wolken (2002), Cohen-Cole (2011), Fisman, Paravisini and Vig (2017), Haselmann, Schoenherr and Vig (2018)), and age or immigration status (Dobbie et al. (2018)).

edge, no previous work has done this before[8].

Second, since the experimental design is based on a representative sample of loan officers, our evidence allows us to causally estimate the efficiency costs at the industry level associated with gender discrimination. Third, a novel contribution of our paper is the use of experimental data on gender preferences and gender beliefs at the loan officer level to disentangle the extent to which gender discrimination is due to statistical and/or taste-based mechanisms, as well as the use of an information experiment to test for inaccurate statistical discrimination. A notable exception in this regard is Brock and De Haas (2019) who implement an in-the-lab experiment with employees of a commercial bank in Turkey to test for the presence of gender discrimination in small business lending. In their experiment, loan officers had to re-evaluate loan applications that the bank had received in the past, but the applicant's gender had been randomly manipulated. The authors find no direct discrimination against women in terms of approval rates, although female applications are more likely to require a guarantor, and this happens mostly when loan officers are young, less experienced, and/or gender biased. However, the fact that Brock and De Haas (2019)'s results are derived from "in-the-lab", abstract loan decisions made by selected bank employees that were not allowed to interact with the actual loan applicants calls for caution in their interpretation and potential replicability in real credit markets.

I. Institutional Background

In 2018, 78% of transactions in the Chilean credit market were related to consumer loans, and more than 90% of them were processed electronically (SBIF

[8]Pioneer audit/correspondence studies first appeared in the labor discrimination literature where CVs are sent to employers randomizing race or gender (Neumark, Bank and Van Nort (1996), Bertrand and Mullainathan (2004), Oreopoulos (2011)), and have been extended to other domains like retail (Pope and Sydnor (2011), Zussman (2013)), or housing demand (Hanson et al. (2016), Ewens, Tomlin and Choon-Wang (2014)). Moreover, audit studies have also been used to test whether discrimination responds to alternative characteristics of job applicants like for-profit college credentials (Darolia et al. (2015), Deming et al. (2016)), military service (Kleykamp (2009)), college selectivity (Gaddis (2015)), and unemployment spells (Kroft, Lange and Notowidigdo (2013), Eriksson and Rooth (2014), Nunley et al. (2014)). For a thorough literature review on the economics of discrimination, please see Bertrand and Duflo (2017).

(2018)). Servicing this market are 11 commercial banks, all of which are supervised by the national banking regulator, SBIF. There are 3 large banks and 3 medium-size banks comprising around 60% and 20% of consumer loan applications, respectively; the remaining 20% is distributed among small banks, none of which captures more than 5% of the market[9]. Consumer loans are divided into installment loans (19%), revolving loans/lines of credit (12%), bank credit cards (23%), and non-bank credit cards (24%).

Testers in our experiment requested installment loans. In order to obtain an installment loan, officers typically ask the applicant to report her tax identifier ("RUT") and to demonstrate her employment status and income. Dependent workers are required to attach salary settlements that support their monthly income. Independent workers have to show income tax returns and/or pay slips. Some banks also ask for documents showing social security contributions. Generally, the required documentation covers the last 3-6 months.

In practice, there are conventionally 5 dimensions that are critical in the evaluation of installment loan requests: (i) requested loan amount; (ii) requested loan term; (iii) applicant's income and working status; (iv) applicant's debt and nonperforming loans within the banking system; and (v) if the applicant is currently a client of the bank or not, i.e., whether she has used a bank product in the past or currently has an active loan. Other considerations like assets, property values, or evidence of collateral may also be required, although this is mostly the case for loan requests involving large amounts (e.g., above $15,000 USD). Loan evaluation normally takes no more than 2 weeks. The final decision may or may not require consultation with internal credit committees within the bank, which typically depends on a combination of internal policies established by the bank and the incentive scheme faced by loan officers.

Gender Gaps. We characterize gender disparities in the consumer credit

[9]Some small banks are considered retailers with a portfolio that is focused on consumer loans, while others are more focused on loans to companies or higher income segments. For additional details about the market structure of the consumer credit market in Chile, please see Cuesta and Sepúlveda (2019).

market using SBIF data containing the universe of installment loan transactions between banks and borrowers for the period 2013-2016. Roughly 40% of consumer loan requests were submitted by women. The proportion of loan requests made by men/women is balanced across banks, suggesting that credit demand is not segmented by applicant gender (see Figure A.I).

Installment loans have an interquartile range of \$850 - \$13,700 USD[10]. On average, loan requests submitted by women are 9.9% smaller relative to those requested by men, a difference that is potentially explained by gender gaps in wages. Indeed, Chilean women who have completed university earn, on average, 65% of the salary earned by men with the same level of education (OECD (2018)).

The loan approval rate is 71%, although this drops sharply to 40% for borrowers who are not clients of the approached bank. The 2013-2016 SBIF data contains a large set of borrower-level characteristics attached to each loan request, including age, civil status, income, debt history, and credit scoring, among others. We examine gender gaps by first regressing an approval dummy (i,e., whether the loan request was approved or not) on applicant gender, controlling for the full set of borrower-level covariates as well as for bank, time, and amount-term fixed effects. We then take a quasi-experimental approach and re-estimate gender differences by using the nearest-neighbor matching estimator (Abadie and Imbens (2011)). Results are reported in Appendix Table A.I. Panel A includes all loan requests, while Panel B includes only loan requests between \$1,500 - \$13,500 USD submitted by applicants 25-35 years old, which are the amount/age ranges that constrain our experimental sample (more in Section II). In each panel, we further split the results into two sets: first considering the full sample of applicants and then only considering non-clients.

The results are mixed and depend on the method used. For the full sample (Panel A, All Loan Applicants), regression estimates show that women are not less

[10]Unless otherwise noted, all monetary units are measured in U.S. dollars as of July 18^{th}, 2018, the starting date of our experiment. For reference, the exchange rate on that date was \$653.89 Chilean pesos per 1 USD, according to the Central Bank of Chile.

likely than men to receive loan approval (Model 2), but the matching estimates suggest a small gender difference favorable to men. For the non-clients sample, female applicants appear to have a small but significant advantage relative to men regarding acceptance rates (Model 2), but the difference disappears when using the matching estimator. In the case of young applicants requesting $1,500 - $13,500 USD loans (Panel B), the results follow a similar pattern. While the gender coefficients vary slightly across samples and empirical strategies, these generally show that gender differences in approval rates are effectively null.

Identifying discrimination on the basis of gender using observational data is problematic since the data requirements to make *ceteris paribus* comparisons across male and female borrowers are extensive (Heckman and Siegelman (1992), Heckman (1998), Blanchflower, Levine and Zimmerman (2003), Han (2004), Fisman, Paravisini and Vig (2017), Dobbie et al. (2018)). Even though the richness of SBIF data allows us to control for a wide range of potential confounders associated with borrower, loan, and bank level characteristics, there are still multiple sources of endogeneity that can bias the gender estimates.

First, we do not observe how banks internally match loan requests with loan officers, and thus potential within-bank gender-biased rules are unobserved. Second, unobservable attributes of loan officers like gender, experience, behavioral/personality traits (including gender biases) can all affect the loan evaluation process and approval/rejection decisions. For instance, Brock and De Haas (2019) show that bank employees in a Turkish commercial bank are more likely to require a guarantor when evaluating loan requests submitted by women (relative to men), and this happens mostly when loan officers are young, less experienced, and/or gender biased. Third, the (unobserved) incentive scheme faced by bank employees can also play a role. Indeed, Agarwal and Wang (2009) show that incentive compensation to loan officers increases loan origination, but may induce the loan officers to book more risky loans. The authors also show that loan officers' career concerns serve as a good disciplinary device to mitigate agency problems, all of which can arguably affect loan approval. Finally, Hertzberg, Liberti and Paravisini (2010) show that information asymmetries between loan officers and banks are substantial. By using detailed internal records from the Argentinian branch of a large multinational U.S. bank, the authors find that a rotation policy that routinely reassigns loan officers to borrowers affects the officers' reporting behavior.

A solution for the aforementioned problems is to implement a correspondence study where manipulated loan requests are randomly sent to loan officers while randomizing the gender that appears in the application form. This way, discrimination is a causal effect defined by a hypothetical *ceteris paribus* conceptual experiment, i.e., varying applicant gender but keeping all else constant. However, implementing such an experiment in the formal credit market is unfeasible since the lending process typically starts by checking the applicant's identity through credit reporting bureaus like Experian, TransUnion, or Equifax, which allows loan officers to easily identify false loan requests that manipulate gender. We overcome this barrier by implementing a correspondence study with real loan applicants where instead of manipulating the applicant's gender we randomly assign loan requests to gender-balanced prospective borrowers who then submit the assigned requests to randomly assigned loan officers.

II. Experimental Design

We randomly assigned loan requests (of random amount and length) to a balanced sample of male and female potential borrowers who were willing to participate in the experiment ("testers"), who then submitted the loan requests to randomly assigned loan officers drawn from a representative survey of loan officers in Chile. The objective was to test whether response and approval rates vary with the tester's gender. The experiment went as follows:

Recruitment of Testers. We randomly selected a subsample of individuals who took the PSU exam during the period 2001-2009[11]. On April 2018, the selected individuals received an email inviting them to participate in an experiment to study gender gaps in the consumer credit market. In order to encourage their participation, we provided an incentive of $15 U.S. dollars for accepting the invitation. The invitation asked participants to send 4 loan requests to a set of 4 loan officers assigned by the research team. Receipt of the $15 USD incentive was conditional on (i) the tester had filled in a short survey containing basic demographic, economic, and tax identifier information; (ii) the tester had attached wage settlements (if a dependent worker), or income tax returns and/or monthly pay slips (if an independent worker), and social security contributions, all for the last 3-6 months; (iii) the tester demonstrated that she had emailed the 4 loan requests to the 4 assigned loan officers in the format required by the research team; and (iv) the tester forwarded all email responses given by loan officers for each loan request to the research team[12].

A total of 404 testers were included in the study sample, of which 58% were women. Our sampling strategy is in line with Ayres and Siegelman (1995). Since gender cannot be randomly assigned, adjustments must be made instead on "relevant" observed characteristics to guarantee statistical comparability across gender groups. Indeed, our sample of applicants is statistically balanced across gender on demographics, educational level, incomes, financial position, employment status, and credit history, i.e., the precise applicant-level variables required by banks to evaluate loan requests. The latter ensures that mean differences in response/acceptance rates across male and female applicants reflect differences in loan officer decisions rather than differences between male and female applicants.

[11]PSU is the Prueba de Selección Universitaria, a test taken by all high-school graduates that aim to enter the Chilean higher education system. In order to receive test results through email, test-takers agree to provide their email accounts. Anonymized emails were obtained through a research collaboration agreement with DEMRE, the PSU administrator.

[12]We were aware that some participants decided to be part of the experiment not because of the financial incentive, but with the objective of campaigning against gender discrimination, which may generate "experimenter effects" that threaten the internal validity of the design. However, as we detail below (see subsection "Loan Requests"), our design minimizes such threats by imposing a strict protocol for tester-officer interactions and we show that this protocol was indeed followed by testers.

The recruited testers come from a population of people who entered the higher education system at most 17 years ago, i.e., young professionals that have around 10 years of experience in the labor market. The median age is 29 and 11% are married. The median monthly wage is roughly $1,000 USD. 90% are employed, and among them 39% are self-employed. At the beginning of the experiment, 39% had an active loan and none of the participants had non-performing debt in the formal credit market.

Loan Officers. A nationally representative survey of loan officers from all banks servicing the consumer credit market was administered through SBIF two months before the experiment started. The survey includes email addresses, as well as basic demographic information like gender, education, and years of experience. In addition to this, the survey includes a module on gender preferences where loan officers declare their optimal portfolio of male and female clients and the main problems they confront when dealing with each gender group. Our data comprises 1,989 loan officers working in 11 banks located in 12 regions within the country. A random sub-sample of 629 loan officers from 9 banks was selected for this experiment, each of which received at least one loan request from testers[13]. 65% of sampled loan officers are female. 71% are between 29 and 49 years old. 33% are married. 97% have a higher education degree, and on average they have 11 years of experience working in the banking sector.

Loan Requests. Each loan request is sent via email from the tester's account to the loan officer's account. The requested loan amount is attached to a pre-specified loan term. In order to design a representative scheme of loan amounts and loan terms contracted in the Chilean consumer credit market, we use the interquartile range of loan size for the universe of approved installment loans in 2018, which is roughly $1,000 - $14,000 USD. In particular, we set 9 possible amounts: $1,500; $3,000; $4,500; $6,000; $7,500; $9,000; $10,500; $12,000; and

[13]In 2 out of 11 banks less than 15 loan officers answered the survey, and thus those banks were dropped from the sampling procedure. These 2 banks represent less than 3% of the market. The remaining 9 banks comprised more than 97% of transactions in the consumer credit market in 2018.

$13,500 US dollars. The interquartile range of loan terms for $1,000 - $14,000 USD loans goes from 12 to 60 months, and the relationship between loan amount and loan term behaves linearly, i.e., larger loans are typically associated with longer terms. Hence, for each of the 9 loan amount types we attached the corresponding term from a set of 9 possible terms: 12; 18; 24; 30; 36; 42; 48; 54; and 60 months. That is, $1,500 loan requests are for 12 months; $3,000 loan requests are for 18 months; and so on. Finally, testers do not detail the purpose of the loan (e.g., to buy clothes, a car, etc.), which minimizes the presence of a motive-specific bias on the part of loan officers.

For each loan request, the tester receives a separate email from the research team, which contains a standardized text with the relevant information to make the loan request, including loan amount, loan term, monthly salary, and individual tax identifier, as well as the assigned loan officer's email account. The tester was mandated to copy and paste the standardized text and use his/her personal email account to send the loan request to the assigned loan officer's email account. An example of a text-standardized loan request (translated to English) is as follows:

> Dear Mr./Mrs. *[Loan Officer's Name]*,
>
> *I am quoting loan conditions and I got your email. I would like to obtain a personal loan in the amount of 5 million Chilean Pesos. I want to repay in 36 months. My RUT is [tax identifier number]. My monthly salary is $750,000 Chilean Pesos. Please see attached my wage settlement and social security contributions.*
>
> *Sincerely,*
>
> *[Tester's Name]*

Negotiation skills on the part of applicants are unobservable and may confound the gender discrimination estimates. Indeed, the empirical evidence suggests that women are less likely to use their negotiation skills than men when negotiating labor or financial contracts (Major (1987), Bowles, Babcock and McGinn (2005);

Bowles, Babcock and Lai (2007), Small et al. (2007), Bertrand (2011), Card, Cardoso and Kline (2016)). Hence, to be on the safe side, we prevented testers from negotiating credit conditions when dealing with loan officer inquiries[14]. In particular, our testers were allowed to interact with loan officers, but only by email, and only if officers asked for additional information to evaluate the loan request, in which case they were mandated to interact with them in a systematic way, providing only the specific information required. In order to monitor the tester-officer interactions, testers were mandated to forward all tester-officer interactions to the research team, from the loan quote request made by the tester to the loan officer's final decision. The testers were warned that negotiating credit conditions or failing to report all tester-officer interactions would be penalized by kicking them out of the experiment with no incentive payment[15].

Randomization. First, we stratify testers by region and gender. We then randomly assign 4 loan requests per tester, each of which randomly varies across the 9 types of loan amount-term, for a total of 1,616 (= 404 × 4) loan-request observations. We then take our sub-sample of 629 loan officers and stratify by region, bank, and loan officer's gender, and within each strata we randomly assign one of the 1,616 loan requests to one of the 629 loan officers[16]. Appendix Figure A.II illustrates the experimental design.

In order to guarantee the validity of the experiment, the random assignment of loan requests to loan officers was restricted as follows:

1) We restricted the random assignment of loan requests to loan officers work-

[14]This is at the cost of reducing the external validity of our results. However, under the hypothesis that male applicants negotiate more (and thus get better credit conditions) than females, we expect that the gender discrimination estimates derived from our experiment are a lower bound relative to those we would observe in a setting where applicants can negotiate.

[15]We have no cases where the tester did not report interactions. For all the interactions, we found no evidence of negotiation between testers and loan officers.

[16]For a statistical power of 80%, our sample of loan requests allows us to identify gender differences in approval rates on the order of 1.1% at a 95% level of statistical significance. We could have increased statistical power by creating a larger number of loan requests per tester or by matching testers and assigning them to loan officers in pairs. However, increasing the number of loan requests per tester was at the cost of reducing the attractiveness of participating in the experiment on the side of testers. Moreover, receiving an unusual number of new applications coming from very similar applicants increases the risk of suspicion among loan officers. Hence, we discarded these strategies for the randomization scheme.

ing in the same region where the tester actually resides[17]. This approach maximizes the chance that loan requests are evaluated by the assigned loan officer and not by out-of-sample colleagues. In fact, only 3% of the assigned loan requests were delegated, i.e., they were evaluated by a different loan officer than intended by the experimental design[18].

2) Loan requests assigned to each loan officer were restricted to be different in terms of amount, term, and tester, which minimizes the suspicion of deception. Moreover, each tester was not matched with more than one loan officer per bank so that we also avoid bank-level suspicion.

3) Loan requests varied in terms of framing. In particular, all loan requests included the same information set (loan amount, loan term, tax identifier number, and monthly salary), but each of them was randomly assigned to one of 23 standardized texts used to request the loan. This is to avoid generating suspicion among loan officers who would otherwise be receiving multiple loan requests with the same message. The variations across texts were designed to be small enough to preserve the objective and politeness of the message, but sufficiently distinctive to avoid suspicion.

4) When possible, loan requests were assigned to loan officers working in banks where the applicant was not a client at the time of randomization. This was the case for 93% of assigned loan requests[19]. This reduces the influence of

[17]This is consistent with actual loan requests in the Chilean consumer credit market, as more than 95% of loan requests are submitted to loan officers working in the same region where the applicant resides (SBIF (2018)).

[18]Some banks have delegation policies based on more than geographic criteria (i.e., depending on where the applicant lives). Indeed, clients may also be classified in terms of age, gender, and/or income profiles, and only then assigned to specific groups of officers. Still, qualitative evidence from interviews with loan officers working in Chilean banks reveals that these criteria apply mostly to individuals who are already clients of the bank but not to non-clients that are requesting a loan from the bank for the first time. In the case of non-clients, banks typically do not establish delegation policies for evaluation of applicants unless the loan request is for large amounts (e.g., above $15,000). Since more than 90% of our sample of testers are not clients of the banks where the assigned loan officers actually work, and the experimental loan requests involve low amounts, our population of interest is mostly unaffected by delegation policies.

[19]The remaining 7% corresponds to cases where the applicant had a prior relationship with all banks operating in his/her region of residence.

pre-treatment relationships between clients and banks on the evaluation process of loan requests and also minimizes the delegation of loan requests.

Finally, in order to test for potential mechanisms associated with statistical discrimination, we implemented an information experiment while loan officers answered the baseline survey. In particular, half of the loan officers were randomly assigned to read a paragraph about how female borrowers perform relative to males in terms of repayment (as reported by SBIF (2018)). The message also included the potential costs associated with gender discrimination in the consumer credit market. The objective was to align the treated officers' beliefs about the loan repayment capacities of women (vs. men) with official information about their repayment rates, and thus test for the presence of inaccurate statistical discrimination (see Section V for details).

Outcomes of Interest. We causally identify gender discrimination effects at the extensive margin, i.e., on whether the loan request was responded to by the loan officer and on whether the loan request was accepted by the loan officer. We also examine intensive margin effects over accepted loans, although this analysis is only observational since the decision of whether to accept or not a loan is endogenous to the loan officer and/or bank approval criteria[20].

Attrition. Our sample frame is composed of 1,616 loan requests (4 loan requests per each of the 404 testers) distributed across 629 loan officers working in 9 banks. All testers were asked to send the assigned loan requests to the assigned loan officers at the same time, July 18th, 2018, the starting date of the experiment. In order to minimize attrition, we sent bi-weekly emails reminding the testers to submit their assigned applications. Overall, 303 out of 1,616 loan requests were not sent by testers, and we consider them as attriters. The attrition rate is 18.75%. The difference in attrition rate across gender is 2.2 *pp.* in favor of male testers, which is not significant at conventional statistical levels (see

[20]For a detailed analysis of intensive margin effects, see Appendix Section B.B3. Five outcomes corresponding to the intensive margin are evaluated: the approved loan amount; the approved loan term; the loan payment; the interest rate charged; and the CAE (Annual Equivalent Charge).

Appendix Table A.II, Panel A). The evidence is consistent across unadjusted and adjusted differences between gender groups, i.e, when using the main specification to estimate gender effects. More than 70% of testers sent all the assigned applications, and the proportions of male and female testers that sent 0, 1, 2, 3, and 4 applications, respectively, are also similar (see Appendix Table A.II, Panel B). Finally, more than 80% of submitted requests were sent within the first 10 weeks following the initial date of the experiment, with a median of 2 weeks[21].

Determinants of Attrition. We explore the determinants of attrition in Appendix Table A.III. The table shows how individual characteristics correlate with the number of non-submitted loan requests as well as with the probability of not sending at least one of the assigned loan applications. We find that attriters are mostly testers whose assigned loan requests included a loan officer working in a bank where they were clients. Indeed, in those cases the probability of not submitting at least 1 out of 4 loan requests increased by 15 *pp.*, on average. We hypothesize bank clients foresaw that sending experimental loan applications could alter their risk profile within the bank, especially if the characteristics of the assigned loan requests (amount and length) were not aligned with their credit history, affecting their chances of obtaining a loan in the future. This was not the case for non-clients unless they planned to quote a loan from the assigned bank in the short run. In any case, we believe the larger attrition rate among bank clients should generate a downward bias in approval rates since we are losing loan applications that have, in expectation, a greater chance of being accepted compared to non-client applications.

By design, the random allocation of loan requests to loan officers (and thus banks) ensures that the distribution of applications sent by male and female testers is statistically identical across banks, i.e., all banks were assigned a similar

[21]Following the pre-analysis plan, standard errors are clustered at the region-bank level. Still, the evidence is robust to the use of heteroskedasticity-consistent (robust) standard errors. The evidence is also robust to clustering the standard errors at the officer level, as well as at the applicant level. For a replication of all the results of this study using these three different adjustment criteria, please see the Online Appendix at this link.

proportion of male/female loan requests. However, a concern is the extent to which this is consistent with the demand for consumer credit that male and female testers would have in an out-of-experiment setting. For instance, male and female testers may have different levels of risk aversion and thus quote loans with different banks such that consumer credit demand is segmented by gender-bank types. However, this seems not to be the case since the effect of being a client on attrition is similar across male and female applicants (interactive coefficient is insignificant), i.e., female applicants are not less/more risk averse than their male counterparts. This is consistent with the evidence shown in Figure A.I, where we find that loan requests made by men/women are balanced across banks, implying that credit demand is not segmented by gender. Overall, these results suggest that the credit search process followed by female testers in a real setting should not be fundamentally different than the one followed by their male counterparts, and thus our experimental credit demand is expected to be comparable to the demand for credit that we would observe in a real setting.

Baseline Balance. Our identification strategy relies on loan requests submitted by male and female testers being statistically similar in terms of the applicant-level dimensions considered by banks when evaluating consumer loan requests as well as on the characteristics of loan officers and banks receiving the experimental loan requests. Appendix Tables A.IV through A.VII present gender balance tests for a large set of characteristics at the level of applicants, loan officers, banks, and loan requests. Following standard practice, we test for gender balance using the same model specification that we pre-specified to estimate discrimination effects, i.e., controlling for all the stratification variables included in the randomization scheme. As pre-specified in the pre-analysis plan, standard errors are clustered at the region-bank level. The design worked as expected as only 3 out of 101 variables (3%) are unbalanced at conventional statistical levels, which is about what would be expected to occur purely by chance[22].

[22]The balance test results are robust to the use of Huber-White (robust) standard errors, as well as

A concern is that we only observe the true distributions of expected returns for the experimental loan requests, but have no information about those submitted out of the experiment, i.e., we do not observe the full portfolio of loan requests managed by each officer. Since loan officers cannot identify which loan requests are "experimental" and which do not, then experimental loan requests that seem gender-equivalent for the econometrician may be in fact heterogeneous to the loan officer. Indeed, if such heterogeneity is not equally distributed across officers receiving male and female loan requests then our identification assumption would be violated. However, we believe this should not be the case in our setting since the assignment of loan requests to loan officers is random, and thus the characteristics of out-of-sample loan requests are, in expectation, equally distributed across loan officers receiving experimental loan requests from male and female testers.

III. Estimating Gender Discrimination Effects

We report estimates of the effect of applicant gender on response and approval rates. As specified in the pre-analysis plan, we estimate the following linear probability model:

$$(1)\quad Y_{lijkt} = \alpha + \beta\, Female_{li} + \gamma\, OffGender_j + \mu_k + \delta_l + \theta_t + \rho T_j + \eta X_i + \pi Z_j + \varepsilon_{lijkt}$$

Our unit of analysis is the loan request l submitted by individual i. The label j indexes loan officers, k indexes the region-bank where the loan officer works, and t indexes the week in which the loan request was submitted. Y_{lijkt} is a dummy variable that equals 1 if the loan request was responded to/approved and zero otherwise, while β is the level of gender discrimination for the outcome under consideration (i.e., the coefficient associated with $Female_{li}$, a dummy variable that equals 1 if the loan request is submitted by a female tester and 0 if submitted

when standard errors are clustered at the officer level and at the applicant level. In all those cases, we find that at most 6 out of 101 variables (6%) are unbalanced at conventional statistical levels. This is, again, what would be expected to occur purely by chance. See Online Appendix tables at this link.

by a male). A negative β would indicate that loan officers discriminate against female borrowers. $OffGender_j$ is a dummy indicator for the loan officer's gender; μ_k are 61 region-bank fixed effects; δ_l are 9 loan type fixed effects; θ_t are 23 week fixed effects; ρ is the effect associated with a dummy that equals 1 if the loan officer received the information treatment and 0 otherwise; X_i is a vector of pre–treatment characteristics of the applicant, including age, if married, monthly wage, if self-employed, and if the individual is a client of the bank where the assigned loan officer works; and Z_j is a vector of pre–treatment characteristics of the loan officer, including age, if the officer has a higher education degree, and years of experience in the banking sector. Finally, ε_{lijkt} is the error term[23,24].

The region-bank fixed effects capture the average unobservable differences across banks in different regions, where each fixed effect is a dummy representing a given bank in a given region. These effects include potential differences in loan policies, criteria for eligible applicants, and standard procedures of loan processing imposed by a given bank. These also control for differences in the level of tolerance of gender discrimination across banks and regions, as well as for market structure and competition effects, all of which may vary both institutionally and geographically. Likewise, $OffGender_j$ controls for unobserved differences between male and female loan officers that may affect the decision about whether to respond/accept a loan request. Since randomization was conducted within blocks of region-bank and loan officer's gender, adjusting for them in the regression analysis does not affect the gender discrimination estimate but does improve its precision (Bruhn and McKenzie (2009)). Finally, time fixed effects capture unobserved differences across weeks in which loan requests were submitted, such as contemporaneous economic shocks and variations in the credit policies adopted by banks over time.

[23]Note that including tester fixed effects makes no sense here as it impedes the identification of the gender discrimination parameter. Moreover, including region and bank fixed effects separately makes no sense either as these span the same subspace as μ_k.

[24]Note that 55% of loan officers received more than one application, which allows for the inclusion of officer-specific fixed effects. While this was not pre-specified in our pre-analysis plan, the statistical inference is robust to controlling for loan officer fixed effects in that the rejection decisions of the null hypothesis remain the same at conventional levels of statistical significance. B.B1 provides evidence on this.

IV. Results

We report the results from estimating equation 1 for two different specifications–one with and one without the set of individual and loan officer baseline covariates. Even though we control for region-bank fixed effects, observations can still be correlated within each region-bank branch. Hence, as pre-specified in the pre-analysis plan, we assume that the error terms are not independent and report clustered standard errors at the region-bank level[25]. According to the pre-analysis plan, our prior is that there exists gender discrimination against female applicants, and we thus additionally report the p-value on the tester's gender coefficient for a one-sided test of the null hypothesis that $H_0 : \beta > 0$[26].

Effects on Response and Approval Rates. Table 1 reports the results. We observe that in 86% of cases loan officers responded to the submitted loan application, i.e., evaluated the request and informed the loan applicant if it was accepted or rejected. We find no evidence of gender discrimination against female borrowers, meaning that loan officers did not respond less to female applicants (relative to males). Second, 31% of the submitted applications were accepted, which is in line with the approval rate among our target population, i.e., young borrowers who are not clients of the receiving bank[27]. However, we find that the approval rate is 6.4 percentage points lower among female borrowers, a difference that is statistically significant at the 1% level. The results are robust across models, the latter confirming that male and female testers were well balanced in

[25]The statistical inference of our results is robust to the use of heteroskedasticity-consistent (Huber-White) standard errors in that decisions whether to reject the null hypothesis of no effect remain the same at conventional levels of statistical significance. We further tried clustering the standard errors at the applicant level and at the loan officer level. Results are reported in the Online Appendix and can be downloaded from the following link. The statistical validity of the results are mostly unchanged under these robustness checks.

[26]If we do not report the one-sided test it means that we did not pre-set that hypothesis in the pre-analysis plan. Note that setting this prior implies that unexpected results pointing to discrimination effects against men are interpreted just as evidence of no discrimination against female applicants, and not the other way around.

[27]According to Table A.I, at least for the period 2013-2016, the approval rate of non-client loan requests with amounts ranging from $1,500-$13,500 USD submitted by applicants between 25-35 years old was 32%.

their baseline attributes and thus β captures the causal gender effect[28,29,30].

The order of magnitude of the gender difference is considerable; it amounts to 18.3% of the male approval rate. This is equivalent to the difference in approval rates between loan requests submitted by borrowers whose incomes are in the 4^{th} and 7^{th} deciles of the income distribution. Furthermore, we estimate that the median forgone profits associated to applications rejected due to gender discrimination amounts to 1,785 USD or 23% of the median loan size (≈7,500 USD). Considering only discriminated applications from applicants aged 25-35 for amounts between $1,500-13,500 USD (our sample frame), the forgone profits at the industry level amounts to 5.8 million dollars per year, which is equivalent to the annual cost of hiring 4% of the officer labor force in the Chilean banking system[31].

Note that loan requests were designed to include only information about the loan amount, loan term, tax identifier number, and monthly salary. Still, in 30% of cases loan officers asked for additional information in order to process the request, mostly wage settlements and social security contributions. A minor proportion were also asked to report information on their income tax returns, college degrees, financial position, or employment contract. In a few cases testers were also asked to provide information about collateral, such as vehicle patents or property valuation certificates. We test for whether loan officers discriminate in the amount and type of information required of male and female applicants,

[28]The results are also robust to a logit specification in that the statistical significance and order of magnitude of the results remain unchanged. See Appendix Table A.VIII.

[29]A concern here is that some officers received more than one application (non-singleton officers). This could increase the chance of suspicion effects, which in turn may disproportionately increase the rejection rate and eventually confound the identification of the gender discrimination parameter. However, as is detailed in Appendix Section B.B1, we find no evidence of differences in effect size for the sample of loan requests submitted to "non-singleton" officers relative the effects found using the full sample, suggesting no suspicion effects.

[30]We further test for heterogeneous effects across the requested loan amount. As shown by Figure A.VII, the gender effect on approval rates slightly decreases as the requested loan amount increases, but these effects are not significantly different across categories, suggesting that gender discrimination has little to do with loan size. In addition, we test for heterogeneous effects across applicant baseline income and find no evidence that gender discrimination varies between those above and below the median income. See Appendix Section B.B2 for a detailed analysis.

[31]For details about the calculation of forgone profits, see Appendix Section B.B4.

and find no evidence of gender discrimination, the results being robust across all indicators and models (see Appendix Table A.IX). This result suggests that the lower approval rates among female applicants (relative to males) are unlikely to be influenced by discrimination on the type of information required by loan officers.

Robustness to Attrition. 313 out of 1,616 assigned applications were not submitted by testers, for an attrition rate of 18.75%. Attrition rate is 17.8% among females and 20% among males, for a differential attrition of 2.2 percentage points in favor of males. While this difference is not significant, a common practice is to check the robustness of the results to this differential attrition. We use the bounding approach of Lee (2009) to construct upper and lower bounds for the gender effects. The key identifying assumption here is monotonicity, meaning that gender influences sample selection in only one direction, i.e., we assume that there are some testers who would have attrited if they had not been male, but that no tester attrits as a result of being male.

To construct the Lee (2009) bounds, we first order the distribution of female loan applications by whether the application was approved or not, and then trim the distribution by the difference in attrition rates between the two groups as a proportion of the non-attrition rate of the female group. In our experiment, this requires trimming the upper or lower 2.7% margin of the distribution ($= 0.022/(1 - 0.178)$). Since the outcome is a dummy variable, we randomly drop 2.7% of "1"s (approved) to construct the lower bound and separately randomly drop 2.7% of "0"s (not approved) to construct the upper bound. Following this procedure, we find that the upper and lower bound effects on the approval rate amounts to 5.7 and 7.9 textitpp. against women, respectively, with both effects significant at the 1% level. Thus our estimated gender effects appear robust to attrition.

Heterogeneous Effects Across Loan Officer's Gender. Table 2 shows gender discrimination effects by loan officer's gender. Regarding approval rates, we find that both male and female officers discriminate negatively against female borrowers. While male-female differences in approval rates are somewhat larger among male loan officers, heterogeneous effects across officer gender groups are not statistically significant. However, in terms of response rate, the heterogeneous effects are remarkable, with discrimination against female applicants substantially larger among male officers. Indeed, within loan requests sent to male officers, those submitted by female applicants are 7.1 percentage points less likely to receive a response compared to loan requests submitted by their male counterparts. As shown by the full sample regression, this is significantly larger compared to the almost null gender discrimination effects found in the sample of loan requests sent to female officers. These results are consistent with evidence showing that gender discrimination against women is more likely to occur in male-female relationships than in female-female ones (Schmitt et al. (2002), Figart (2005), Anwar and Fang (2006), Delavande and Zafar (2013), Beck, Behr and Guettler (2013), Montalvo and Reynal-Querol (2019)).

We further examine the distribution of gender discrimination effects across banks. Figures A.III and A.IV provide an overview. First, in terms of response rates, 4 out 9 banks show negative point estimates (discrimination against females), while 5 out 9 banks show positive ones (in favor of females). For approval rates, with the exception of two banks, all show negative point estimates. While the discrimination effects are not significant for all banks, there is consistency in the rank order of banks across outcomes, i.e., banks that discriminate more (less) in terms of response rate also discriminate more (less) in approvals. Second, we study the extent to which loan officer characteristics within each bank relate to the bank-level gender discrimination estimates. We explore a number of dimensions, including gender composition of staff headcount, officer age profile, years of experience in the banking sector, and education. Overall, we find no significant correlation between the bank-specific effect size of gender discrimination and loan officer characteristics except for gender composition. In particular, we find that banks with a larger proportion of male officers in their staff headcount are associated with larger levels of discrimination against women in both response and approval rates, a result that reinforces the hypothesis that loan officer's gender does matter for gender discrimination in the consumer credit market.

V. Mechanisms

Two workhorse models of discrimination are the statistical model and the taste-based model. If the differential treatment across male and female borrowers is due to differences in the forecasting that loan officers make regarding each group's loan return, then we would argue in favor of statistical sources to explain gender discrimination (Phelps (1972), Arrow (1973), Aigner and Cain (1977)). That would be the case if, for instance, male applicants are believed to be less risky or repay more consistently than their female counterparts, such that when loan officers examine observably similar loan requests they end up prioritizing those coming from men.

Alternatively, loan officers may discriminate based on their preferences or tastes about gender (Becker (1957)). In particular, if loan officers "distaste" female applicants, they will be willing to indulge such animus by rejecting a larger proportion of loan requests submitted by women. Likewise, the loan officer may be biased against female applicants due to an intrinsic preference for males, which is typically determined by non-economic (e.g., cultural) factors[32].

A. *Testing Taste-based Discrimination*

Gender Beliefs. We start by studying loan officers' beliefs about the behavior of male and female clients. In particular, loan officers are asked to choose only one problem within the list of options detailed below, the one they believe to be the most important when dealing with clients. The question is asked separately

[32] Akerlof (1980) suggests that taste-based discrimination may be shaped by what he calls "the social custom" of discrimination, in which case a loan officer would discriminate against women not because he intrinsically dislikes women but because the reputational cost of not following the established social norm of discriminating against women is too large relative to the intrinsic benefit of not doing so.

for male and female clients, i.e., loan officers answer the question twice: first for male clients and then for female clients[33].

Which is the most important problem you face when dealing with Female/Male clients?

Problem (a)	*"Female/Male clients have low repayment rates"*
Problem (b)	*"Female/Male clients are uninformed about financial products"*
Problem (c)	*"Female/Male clients demand excessive administrative duties"*
Problem (d)	*"Female/Male clients are difficult to communicate with"*
Problem (e)	*"Female/Male clients are too tough and require quick responses"*

The list of options was designed based on multiple face-to-face interviews with out-of-sample loan officers, from which we selected the most common problems they reported. Options (a) through (c) are categorized as problems related to "statistical" discrimination as all of them could potentially diminish loan returns. In contrast, problems related with "difficulties to communicate" or "toughness" are arguably associated with taste-based beliefs[34]. Appendix Table A.X shows the distribution of beliefs by client gender. First, when asked about the main problems with female clients, we find that 55% of responses are categorized within taste-based reasons (as opposed to 45% related to statistical discrimination). In contrast, for the case of subjective beliefs about male clients, responses are flipped

[33]A natural concern is that the set of questions related to the problems that loan officers confront with male and female clients generates a Hawthorne effect on the behavior of loan officers. That would be the case if loan officers believe their responses will allow banks to accuse them of gender discrimination against women (men), in which case loan officers may increase misreporting or increase the approval rates of loan applications submitted by female (male) borrowers. However, we believe risks are minimal in this regard. First, loan officers were assured by the SBIF that their responses were confidential and anonymous, and thus their responses would not be shared with banks or any related institutions. Second, the question is not gender-selective as loan officers were asked to report problems with both male and female borrowers. Third, the survey was implemented two months before the experiment started, and thus it is unlikely that it influenced the response and/or approval rates of the experimental loan requests arriving two to three months later.

[34]Options (d) and (e) may still capture additional transaction costs that loan officers could factor in when processing applications, and hence indirectly affect expected profits. This cost could be due to a knowledge gap that leads officers to deal more efficiently with own-gender clients at first or an initial prejudice that diminishes over time. If so, then those transaction costs would disappear with officer experience. We explore this by testing whether officers' experience correlates with their gender beliefs. As is shown by Appendix Table A.XI, we find no evidence whatsoever that loan officer's experience affects their beliefs about male and female clients, which reinforces our prior that options (d) and (e) do capture taste-based beliefs.

across categories: 42% of them are categorized as taste-based reasons (as opposed to 58% related to statistical discrimination). Indeed, it is 13 *pp.* less likely that loan officers report a statistical-based problem when dealing with female clients than when doing so with males (significant at the 1% level). Most of this difference occurs because of differential perceptions in 2 of the 5 categories of problems. On one side, problems related to low repayment rates are significantly more prominent in the case of male clients. On the other side, a large proportion of loan officers believe that the main problem with female clients is that they are "too tough and require quick responses". Overall, this suggests that, on average, loan officers do judge male and female clients through different criteria.

Using Gender Beliefs to Examine Taste-based Discrimination. We use the reported gender beliefs to classify loan officers into taste-based and not-taste-based profiles. Loan officers reporting that the source of the main problem they face is taste-based when dealing with female clients but statistical-based for male clients are classified as taste-based profiles. 30% of loan officers match this categorization[35]. Then, in Table 3, we test for whether taste-based profiles discriminate more against female clients relative to not-taste-based counterparts. The results are compelling. For approval rates, we find that gender discrimination against female applicants is 14 *pp.* larger among officers classified as a taste-based profile relative to those classified as a not-taste-based (statistical) profile, a difference that is significant at the 1% level. That is, those officers who believe that the main problem with female clients is that they are too tough or too difficult to communicate with but do not believe the same about men discriminate more against women in terms of approval rate. In contrast, officers who are classified within the not-taste-based profile do not discriminate at all.

Gender Preferences. The subjective reports regarding the main problems confronted by loan officers when dealing with male and female clients mostly

[35] On the remaining 70% (not-taste-based officers), 53% report the same type of problem with male and female clients, while 17% report taste-based problems with male clients but statistical-based problems with female clients.

capture their gender beliefs about client behaviors, but not necessarily their gender preferences. Our loan officers survey includes a separate module designed specifically to elicit their gender preferences. First, we ask them the following question:

"If you had the chance to choose the optimal distribution of male and female clients in your portfolio, what would you choose among the following 5 possible choices?"

	Choice 1	Choice 2	Choice 3	Choice 4	Choice 5
Prop. Male	20%	40%	50%	60%	80%
Prop. Female	80%	60%	50%	40%	20%
Total	100%	100%	100%	100%	100%

A non-negligible 28% of loan officers chose a client portfolio composed of more men than women (i.e., 60% or 80% male clients), which we call "pro-male" officers. Among "not-pro-male" officers, 63% have equal preference over gender and the remaining 9% prefer a majority female clientele.

Note that this is a stated choice. While it elicits subjective gender preferences related to clients, this may or may not be aligned with loan officers' revealed preferences over client gender. Also, the stated choice does not reveal the specific source of discrimination. For instance, loan officers may choose a specific gender distribution with the only objective of maximizing expected profits (i.e., purely based on statistical attributes of potential clients) or, alternatively, due to taste-based reasons that have nothing to do with profit-maximization objectives. Hence, to validate the accuracy of this stated choice measure, we employ a revealed preference approach. In particular, we experimentally test whether the pro-male dummy correlates with gender preferences elicited through actual gender choices, for which we implement a gift experiment that worked as follows.

Once the survey ended, each loan officer was told that as a reward for participating in the survey they had earned 2 tickets to participate in a real lottery that raffled 5 iPads among approximately 2,000 survey participants. Loan officers had the chance to use the two tickets or to use one and donate the other to a colleague working in the same bank. Each loan officer was randomly assigned 1 out of 6 potential colleagues to whom they could donate the second ticket, 3 of which were men and 3 women, and was asked to decide whether they would donate the second ticket to the assigned colleague or not. On average, there are more than $1,500$ employees per bank, making it unlikely that loan officers know all bank employees, so we decided to design fictitious names[36]. The objective is to test whether pro-male officers are less (more) likely to donate one ticket when the randomly assigned donee's name is feminine (masculine) compared to not-pro-male officers. Since the donee's gender is randomly assigned, potential differences in the donation rate across pro- and not-pro-male groups are attributable to gender tastes and not to other sources of discrimination, providing a revealed-preference test for the construct validity of our stated choice measure of gender preferences.

Appendix Table A.XII shows the results for the whole sample and then separated by loan officer's gender. 63% of loan officers decided to donate the 2^{nd} ticket to the assigned colleague, but if the donee's name is feminine, the donation rate drops by 6.4 *pp.* (see column (1) under "full sample" headline). While pro-male officers donate less often to female colleagues, the difference in the donation rate when the donee's name is feminine is not significant between the pro- and not-pro-male groups. Nonetheless, the donation behavior is remarkably different between male and female officers. In particular, among male officers, we find that while pro-male individuals are more likely to donate the 2^{nd} ticket than their not-pro-male counterparts if the donee's name is masculine, they are 40 percentage points less likely to donate it if the donee's name is feminine. In contrast, the heterogeneous effects across donee's gender are much smaller and insignificant

[36]In order to avoid name bias, male and female names used the same last name but had reciprocal first names. In particular, the male names were Cristián Errázuriz, Cristián González, or Cristián Cayupan, while the female names were Cristina Errázuriz, Cristina González, or Cristina Cayupan. To avoid misreporting, loan officers were told that their yes/no decision was completely anonymous and thus would not damage their reputation.

for the case of female officers. Overall, this evidence suggests that taste-based gender preferences against female clients are more likely to be revealed among male pro-male officers, and we should take this evidence into consideration when exploring the effects of gender preferences on gender discrimination.

Using Gender Preferences to Examine Taste-based Discrimination. We examine the taste-based mechanism by testing whether gender discrimination effects vary significantly across loan requests assigned to pro-male and not-pro-male officers. Table 4 shows the results. We first look at heterogeneous effects in the full sample and then replicate the exercise using only loan requests assigned to male/female officers. In terms of response rates, we find no heterogeneous effects, with both pro-male and not-pro-male groups being equally responsive to loan requests submitted by male and female borrowers. However, in terms of approval rates the story is different. Here we find that those who discriminate the most against female applicants are pro-male officers. The effects are large and significant within this group: the approval rate of loan requests submitted by female applicants is 17 *pp.* lower relative to the approval rate of loan requests submitted by men. In contrast, the female-male difference in approval rate among not-pro-male officers is 3 *pp.* in favor of males, a small and insignificant discrimination effect. Moreover, we find that gender discrimination against female applicants is 11 *pp.* larger among pro-male relative to not-pro-male officers, a difference that is equivalent to roughly 30% of the male approval rate. The difference is statistically significant at the 5% level using both two-sided and one-sided tests. Panel (a) in Figures A.V and A.VI illustrate this result.

Importantly, the pro-male dummy is unbalanced across applicant gender. Specifically, the applications of female testers were assigned to loan officers that were disproportionally more pro-male than for male testers. This calls for caution in the interpretation of the results. To be on the safe side, we estimate the upper bound (least negative) effect following Lee (2009). In particular, we first randomly drop a proportion of female applications assigned to pro-male officers such that the officer's type (pro- and not-pro-male) is exactly balanced across male and female applications, and then estimate the gender effects following the same regression framework. As shown by Table 4, last row, the effects are robust to Lee's adjustment both in terms of order of magnitude and statistical validity.

Heterogeneous Effects by Loan Officer's Gender and Gender Preference. Our giving experiment suggests that taste-based sources of discrimination are more salient among pro-male officers who are men. Hence, we examine gender discrimination by combining officer gender and officer gender preferences. Table 5 shows the results. Consistent with our revealed preference measure for gender preferences, we find that the bulk of the discrimination against female applicants is coming from pro-male officers who are men. Indeed, for male officers, we find that male-female differences in terms of approval rates are 18 *pp.* larger among pro-male officers relative to their not-pro-male counterparts. This is not the case for female officers, where we find that differences in the gender discrimination rates across pro-male and not-pro-male types are much smaller and insignificant at conventional statistical levels. Figures A.V and A.VI, Panel B, illustrate these results. Overall, this evidence gives credibility to the hypothesis that taste-based, gender-biased prejudices on the part of male loan officers are the main drivers explaining gender discrimination.

*B. **Testing Statistical Discrimination***

Gender discrimination against women may also be due to loan officers believing that lending to women is less profitable than lending to men since women are economically more vulnerable and thus more likely to default than men, i.e., due to statistical sources of discrimination. Importantly, those beliefs may or may not be accurate, and that will affect the welfare/efficiency effects of statistical discrimination (Borhen et al. (2019)). In fact, distinguishing between accurate and inaccurate statistical discrimination is critical since the premise that statistical discrimination is efficient only holds under the assumption that loan officers' beliefs about the gender-group distribution over the relevant outcome are "correct", i.e., these follow rational expectations and are unrelated to gender preferences (Borhen et al. (2019)).

Interestingly, there is evidence showing that Chilean women are better debtors than men. Data released from the SBIF Annual Gender Gap Report (SBIF (2018)) shows that 56% of male borrowers' credit debt in 2017 was overdue by 0 to 90 days, which is 12.5% larger than the corresponding delinquency rate among female borrowers. Moreover, for > 90 days Non-Performing Loans (i.e., loans declared in default by Chilean regulation), the default rate among men is 3.99% but 2.80% among women[37]. Despite the fact that this is official information that is communicated annually by the authority to all banking institutions, loan officers may still be uninformed about these facts. Indeed, the observed discrimination against female applicants could potentially be due to inaccurate beliefs held by loan officers. If so, the approval rate among female applicants would be lower not because loan officers are taste-biased against female applicants, but because they firmly believe female applicants' repayment capacity is lower relative to that of male applicants, and thus between two marginal applicants loan officers prefer lending to men.

We test for this hypothesis by implementing an information experiment where some loan officers are randomly assigned to read a paragraph informing them that female borrowers are better debtors than men (as reported by SBIF (2018)). The objective is to align the treated officers' beliefs about the repayment behavior of women (vs. men). The exercise is useful not only to examine the statistical discrimination channel, but also to identify whether inaccurate beliefs play any role. In our case, a rejection of the null hypothesis of no treatment effect would favor the hypothesis of inaccurate statistical discrimination[38].

[37]These differences hold even after controlling for loan requests and borrower characteristics, suggesting that female advantages in repayment behavior are unlikely to be caused by women whose loan requests were approved for being on average better clients and thus more able to repay than their male counterparts.

[38]There is a large number of papers in psychology and political science that show the efficacy of information and salience treatments for changing the behavior of individuals against women and *viceversa*,

The information treatment worked as follows. Half of the loan officers in our sample were randomly assigned to read the following message:

> *Did you know that female borrowers pay more for consumer credit than males? A recent report released by SBIF (2018) shows that women pay interest rates that are, on average, 15% higher relative to those paid by men. This is even though the same report also shows that female borrowers exhibit repayment rates that are significantly higher compared to male borrowers. Gender discrimination against women may bring negative consequences for women who aim to access the consumer credit market as well as for our economy as it might be inefficient and damaging for productivity.*

The message was randomly posted at the end of the baseline survey. After reading the message, loan officers were asked to declare whether they agreed or not with the statement that "*The average repayment rate of women is higher than that of men*". Answering this question was mandatory to finish the survey, which maximizes the chances that treated loan officers actually read the message. The message was pre-tested with out-of-sample loan officers until it effectively delivered two important remarks. First, it acknowledges that while women pay higher interest rates than men, women are more reliable debtors than men as they have better repayment behavior. Second, it notes the costs of gender discrimination in terms of access to credit markets for women and the associated efficiency and productivity costs for the economy.

The baseline survey was implemented two months before the initial date of the experiment. As a reminder, the message was re-sent through the official SBIF email account to the email accounts of treated loan officers one day before the experiment started. That way, we increased the chance that they update their beliefs on the repayment behavior of women (vs. men) just before receiving

most notably Conover and Sapiro (1993), Bitter and Goodyear-Grant (2017), and Wang and Dovidio (2017).

the experimental loan requests. The re-sent message was inserted in a letter of appreciation for having responded to the survey in which they had participated two months ago. As a placebo treatment, control group officers also received the appreciation email but without the treatment message.

Results. As shown in Table 6, we find no statistically significant differences in the discrimination rates across loan requests sent to treatment and control loan officers. If anything, treated officers tend to discriminate more against women, not less. This result goes against our pre-analysis plan hypothesis that the information treatment was going to decrease gender discrimination. One potential explanation is that taste-based sources worked against belief change. Indeed, two pieces of evidence support this claim.

First, after reading the message, only 36% of treated loan officers agreed with the statement that women had higher repayment rates than men, i.e., most loan officers are simply not convinced that women are better debtors than men. Note, however, that while 39% of not-pro-male officers agreed with the statement, this was only 31% among their pro-male counterparts, a 20% difference that is statistically significant at the 1% level. Second, we rely on a consistent and intuitive pattern of statistically significant heterogeneous treatment effects across officers with pro-male and not-pro-male preferences to argue that taste-based sources worked against belief change (see Table 7). In particular, we find that the information treatment had null effects on the not-pro-male officers, but markedly negative effects among pro-male officers, i.e., reading the message led them to discriminate even more against women. For instance, in terms of response rate, gender discrimination against female applicants is 13 *pp.* larger among treated pro-male officers relative to control pro-male officers (significant at the 10% level), and the difference is shown to be robust to Lee bounds corrections for the unbalanced pro-male dummy. For approval rates, the heterogeneous effects are also considerable (8.9 *pp.*), although not statistically significant.

Following the theoretical work of Heidhues, Köszegi and Strack (2019), we hypothesize that pro-male officers counter-reacted to the treatment message due to their self-serving views about discrimination and this is potentially due to overconfidence bias: they overestimate the degree of discrimination against any group whose preferences they are personally aligned with (e.g. male applicants) and underestimate discrimination against any group they compete with or are not aligned with (e.g. female applicants). Intuitively, the agent's overconfidence implies that the recognition he obtains is, in his view, systematically too low, and believing in discrimination against groups with which he identifies provides an explanation for why this is the case.

As Heidhues, Köszegi and Strack (2019) suggest, the obvious approach would be to target the source of the problem –overconfidence– directly by making recognition a less noisy measure of ability or deservingness. Ironically, because this forces the agent to provide a better explanation for why his recognition is low, it increases all his biases. In other words, while better information about discrimination toward a group lowers the agent's bias regarding that parameter, it also creates more of a need to explain his low recognition, increasing many of his other biases operating against that group. Hence, if someone manages to convince a pro-male officer that there is discrimination against female applicants, the expected outcome is that he comes to believe in discrimination against male applicants to a greater extent. In the same vein, Bohnet (2016) recognizes that while changing the discriminator agent's view of society can be helpful, attempts to debias him through the provision of better information may backfire. Indeed, she shows that explicit diversity training programs in US corporations have made between-group differences more salient, which in turn have generated more discrimination against minority groups, not less. The author argues that this is potentially due to 'moral licensing", i.e., the tendency for individuals to act more immorally following the completion of an act or set of behaviours they see as being morally good.

Finally, from a psychological perspective our results can also be interpreted through the lens of social identity theory (Tajfel and Turner (1979) and Tajfel (1982)), which posits that individuals identify themselves as members of relevant social groups, i.e., their in-groups. Specifically, their self-esteem is bound up with their in-groups, so thinking positively about their in-groups (e.g., male applicants) and negatively about their out-groups (e.g., female applicants) leads them to think and feel positively about themselves. In other words, the pro-male officer is subject to an "in-group" bias if he tends to hold overly favorable views about those in his preference groups (men) and overly unfavorable views about those in competing groups (women). Intuitively, since the pro-male officer believes that discrimination against men (in favor of women) is ongoing, he attributes more of an in-group member's recognition (male applicants), and less of a competitor's recognition (female applicants), to caliber.

Validity Threats. A concern with the information treatment is that it highlights the relatively high repayment capacity of past female borrowers (relative to men) who already received credit approvals, but it does not inform about the repayment capacity of women (vs men) who are now requesting a loan. Following this logic, the treatment message would affect the credit conditions offered to accepted requests made by women, but not necessarily the decision of whether to accept their loan requests. Yet, we believe this is a false dilemma since at the cutoff of the officer's criteria to accept or reject a loan, the marginal accepted applicant is equal to the marginal rejected one, meaning that information that is valid for the marginal accepted applicant is also valid for the marginal rejected counterpart. Consequently, information about the repayment behavior of accepted applicants should be valuable for making the decision of whether to accept or reject loan requests from new applicants. In fact, the reported evidence on heterogeneous treatment effects on gender discrimination across pro-male and not-pro-male officers reinforces our claim as it implies that the provided information indeed changed the decisions on whether to respond/accept loan requests submitted by female applicants.

Another validity concern relates to information design. There is the risk that the information treatment was erroneously designed, i.e., it delivered a message that not all loan officers interpreted in the way we expected. If so, the exogeneity assumption would be violated as non-observable characteristics of loan officers are correlated with the probability of receiving the correct message. However, we believe this is not the case since our pre-test precisely tackled this potential threat, and in fact the message was validated by all loan officers that participated in the pre-test.

Finally, a last thread is low treatment compliance, i.e., that a relevant portion of treated loan officers did not actually read the message. Nonetheless, note that reading the message was mandatory to finish the survey. In fact, loan officers were asked to declare whether they agreed or not with the final paragraph contained in the message in order to end the survey, and also a reminder message was sent the day before the experiment started. In all, we believe these actions generated high treatment compliance. Lastly, a low treatment compliance runs counter to the large treatment effects we observe in the pro-male officers sample.

*C. **Gender Discrimination and Market Structure***

According to Becker (1957)'s argument, provided that banks have constant returns to scale and men and women are equally skilled (i.e., loan applications submitted by male and female borrowers are equally profitable), market competition overwhelms taste-based gender discrimination. In particular, Becker (1957)'s model predicts that as new entrants in the lending market take advantage of the opportunity to cash in on the existence of profits due to inefficient discrimination, the relative cost of discriminating against female applicants increases, and thus biased lenders are competed away.

We examine this hypothesis by taking advantage of the geographical dispersion of our experimental data. In particular, we test whether the level of market concentration in the municipality where the receiving bank office is located has any incidence on the level of gender discrimination. Market concentration is measured through the Herfindahl-Hirschman Index as $(1/100) \times \sum_{k=1}^{K} s_k^2$, where s_k is the number of local offices owned by bank k divided by the total number of bank offices in a given municipality. The greater the HH Index, the more concentrated is the market. The index ranges from 0 to 100, with a mean and median of 21 and 15, respectively. We study how market concentration affects gender discrimination by interacting the applicant's gender dummy with the HH Index in the municipality where the bank office (i.e., where the assigned officer works) is located. We first show the regression results for the full sample of loan requests and then separate the analysis in two subsamples: loan requests submitted to not-pro-male officers and loan requests submitted to pro-male officers. That way, we can identify whether the potential effects of market competition on gender discrimination can be attributed to changes in attitudes towards women among taste-based discriminators or not, and thus obtain an indirect test of Becker's theory.

Table 8 shows the results. For response rates, we find that for every additional HHI point of market concentration, gender discrimination against women increases by 0.005 *pp.*, on average, and the effects are similar across pro-male and not-pro-male officers. Yet, the order of magnitude of the effect is quite small; it represents only 0.6% of the mean response rate among male applicants. To put this number in perspective, going from the 25th to the 75th percentile of the HHI distribution increases the male-female difference in response rate by just 3.5% of the mean response rate for men.

Regarding approval rates, we find that gender discrimination against women does not vary with market concentration. However, the story is different depending on the officer's gender preference. Specifically, for loan requests submitted to not-pro-male officers, changes in market concentration do not alter the male-female difference in approval rates. In contrast, for loan requests submitted to pro-male officers, we find that for every additional HHI point of market concentration gender discrimination against female applicants increases by 2.5 *pp.*, on average. This is a large effect. For instance, going from the 25th to the 75th percentile of the HHI distribution increases the male-female differences in approval rate by 46% of the mean male approval rate. In other words, higher levels of market competition effectively discipline the taste-based attitudes against female applicants among pro-male officers. This is potentially an important result in light of Becker's theory of discrimination since it gives credibility to the hypothesis that the underlying mechanism behind the negative effects of market competition on gender discrimination is that taste-based attitudes against women are competed away from the market[39].

VI. Conclusion

When evaluating observably similar loan applications from men and women, do loan officers favor men? This paper examines this question through the implementation of a randomized correspondence study with real male and female borrowers in the context of the consumer credit market in Chile. To the best of our knowledge, no previous paper has used an experimental design where borrowers and officers interact in a real setting to obtain market-level causal estimates of gender discrimination in access to consumer loans. A key novelty of the paper is that we combine the elicitation of gender preferences and gender beliefs with the implementation of an information experiment to test for the presence of taste-based and/or statistical discrimination.

We provide compelling evidence suggesting that gender discrimination against female applicants does exist and is significant. We show that approval rate for loan requests is 18.3% lower among female borrowers (relative to males), a gender gap that is equivalent to the difference in approval rates between borrowers in the 4^{th} and 7^{th} deciles of the income distribution. Inefficiency costs are also involved. We

[39]This result does not imply that incomplete information and signal extraction problems of the sort depicted by Arrow (1973) and Phelps (1972) are not a valid framework to explain the negative effects of market competition on gender discrimination, but unfortunately our data does not allow us to prove the presence of such mechanisms.

estimate that the median forgone profits associated to applications rejected due to gender discrimination amounts to 1,785 USD or 23% of the median loan size (≈7,500 USD). Considering only discriminated applications from applicants aged 25-35 for amounts between $1,500-13,500 USD (our sample frame), the forgone profits at the industry level amounts to 5.8 million dollars per year, which is equivalent to the annual cost of hiring 4% of the officer labor force in the Chilean banking system.

Second, we provide evidence suggesting that gender discrimination against female borrowers is due to taste-based sources. Our results show that while not-pro-male officers do not discriminate against female borrowers, the male-female difference in approval rate among pro-male officers is sizable, with most of the effect coming from pro-male officers who are men. Moreover, we find that banks with a larger proportion of male officers in their staff headcount are associated with larger levels of discrimination against women, and this is in terms of both response and approval rates. The latter calls for bank-level hiring policies that better screen the gender attitudes and preferences of job applicants.

Third, an information experiment implemented with loan officers shows that those who were informed about the superior performance of women (relative to men) in terms of repayment behavior do not discriminate less against female borrowers compared to the control group officers that were not informed, thus rejecting the hypothesis of inaccurate statistical discrimination. In contrast, we find that pro-male officers in the treatment group discriminated more against women relative to their control counterparts. Following the theoretical work of Heidhues, Köszegi and Strack (2019), we hypothesize that overconfidence bias on the part of pro-male officers can act as a potential mechanism behind taste-based discrimination. Overall, our evidence suggests that gender discrimination against female borrowers in the Chilean consumer credit market is unlikely to be neutralized through information treatments aiming to dilute female/male statistical discrimination, and cultural changes at an institutional scale may be required. As argued by Bohnet (2016), gender-equality interventions should focus on de-biasing environments rather than individuals.

Finally, we find that female-male differences in approval rates tend to be larger in municipalities with higher levels of market concentration, but this is only the case for loan requests submitted to pro-male loan officers. This result is consistent with Becker's theory of discrimination in that the underlying mechanism behind the negative effects of market competition on gender discrimination is that anti-women attitudes become less and less profitable with competition, such that taste-based discriminators are eventually competed away from the market.

Table 1—: Extensive Margin: Response and Approval Rates - OLS

	Loan Request was Responded (= 1)			Loan Request was Approved (= 1)		
	Unadjusted Mean Difference	Model (1)	Model (2)	Unadjusted Mean Difference	Model (1)	Model (2)
Female (= 1)	-0.010	-0.016	-0.016	-0.066	-0.066	-0.064
	(0.023)	(0.023)	(0.023)	(0.023)	(0.020)	(0.017)
p-value $H_0 : \beta_{Female} > 0$	0.331	0.243	0.247	0.003	0.000	0.000
Upper Bound	-0.007	-0.012	-0.012	-0.058	-0.059	-0.057
	(0.022)	(0.022)	(0.021)	(0.023)	(0.021)	(0.018)
Lower Bound	-0.014	-0.019	-0.018	-0.085	-0.083	-0.079
	(0.023)	(0.023)	(0.023)	(0.025)	(0.022)	(0.018)
Observations	1,313	1,313	1,313	1,313	1,313	1,313
R^2	0.000	0.190	0.205	0.005	0.173	0.230
Mean Male (= 1)	0.861	0.861	0.861	0.349	0.349	0.349
Stratification Var.	×	√	√	×	√	√
Loan Amount-Term F.E.	×	√	√	×	√	√
Information Treat.	×	√	√	×	√	√
Week F.E.	×	√	√	×	√	√
Borrower's Baseline Cov.	×	×	√	×	×	√
Officer 's Baseline Cov.	×	×	√	×	×	√

Note: Sample of analysis are submitted loan requests. Regression models (1) and (2) include stratification variables, i.e., a dummy for loan officer's gender, and 61 region-bank fixed effects; plus 8 dummies for the requested loan amount-term, a dummy that is equal to 1 if the loan officer received the information treatment (and 0 if not), and 22 week-time fixed effects. Model (2) also controls for baseline covariates at the borrower level, including age dummies (<29; 29−38), if married, monthly wage dummies (600−1,200 USD; >1,200 USD), if self-employed, and if a client of the assigned bank; as well as baseline covariates at the loan officer level, including age dummies (<29; 29−48), if has a higher education degree, and dummies by years of experience in the banking sector (<5; 6−10). Following the standard procedure, when a control variable has a missing value, we impute a value equal to 0 and add a dummy variable equal to 1 for that observation, which indicates that the control variable was missing. Standard errors (in parentheses) are clustered at the region-bank level. According to the pre-analysis plan, the p-value for a one-sided test of the null hypothesis that $H_0 : \beta_{Female} > 0$ is reported separately. Upper and lower bound effects are calculated following Lee (2009).

Table 2—: Heterogeneous Effects by Loan Officer's Gender - OLS

	Only Female Loan Officers				Only Male Loan Officers				Full Sample			
	Loan Request was Responded (= 1)		Loan Request was Approved (= 1)		Loan Request was Responded (= 1)		Loan Request was Approved (= 1)		Loan Request was Responded (= 1)		Loan Request was Approved (= 1)	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Female (= 1)	0.017	0.016	-0.063	-0.056	-0.061	-0.071	-0.074	-0.092	0.010	0.010	-0.073	-0.063
	(0.026)	(0.024)	(0.023)	(0.026)	(0.029)	(0.029)	(0.054)	(0.052)	(0.025)	(0.024)	(0.020)	(0.022)
Loan Officer is Male (= 1)									0.064	0.064	0.031	0.031
									(0.022)	(0.024)	(0.040)	(0.042)
Female × (Loan Officer is Male)									-0.070	-0.069	0.019	-0.001
									(0.026)	(0.025)	(0.050)	(0.054)
p-value $H_0 : \beta_{female} > 0$	0.739	0.752	0.005	0.017	0.020	0.011	0.089	0.044				
p-value $H_0 : \beta_{inter} > 0$									0.005	0.005	0.648	0.492
Observations	838	838	838	838	475	475	475	475	1,313	1,313	1,313	1,313
R^2	0.239	0.258	0.212	0.264	0.191	0.218	0.207	0.290	0.192	0.207	0.173	0.230
Mean Male (= 1) if L.O. is Female	0.837	0.837	0.332	0.332					0.837	0.837	0.332	0.332
Mean Male (= 1) if L.O. is Male					0.900	0.900	0.376	0.376	0.900	0.900	0.376	0.376
Region-Bank F.E.	√	√	√	√	√	√	√	√	√	√	√	√
Requested Loan Amount-Term F.E.	√	√	√	√	√	√	√	√	√	√	√	√
Week F.E.	√	√	√	√	√	√	√	√	√	√	√	√
Information Treatment	√	√	√	√	√	√	√	√	√	√	√	√
Borrower's Baseline Covariates	×	√	×	√	×	√	×	√	×	√	×	√
Loan Officer's Baseline Covariates	×	√	×	√	×	√	×	√	×	√	×	√

Note: Sample of analysis are submitted loan requests. All regressions control for 61 region-bank fixed effects; plus 8 dummies for the requested loan amount-term, a dummy that is equal to 1 if the loan officer received the information treatment (and 0 if not), and 22 week-time fixed effects. Model (2) also controls for baseline covariates at the borrower level, including age dummies (<29; 29−38), if married, monthly wage dummies (600−1,200 USD; >1,200 USD), if self-employed, and if a client of the assigned bank; as well as baseline covariates at the loan officer level, including age dummies (<29; 29−48), if has a higher education degree, and dummies by years of experience in the banking sector (<5; 6−10). Following the standard procedure, when a control variable has a missing value, we impute a value equal to 0 and add a dummy variable equal to 1 for that observation, which indicates that the control variable was missing. Standard errors (in parentheses) are clustered at the region-bank level. According to the pre-analysis plan, we report (i) the p-value for a one-sided test of the null hypothesis that $H_0 : \beta_{Female} > 0$; and (ii) the p-value for a one-sided test of the null hypothesis that the interaction of Female dummy and Loan Officer's Gender dummy (i.e. Female Borrower × Male Loan Officer) is greater than zero.

Table 3—: Heterogeneous Effects by Loan Officers' Gender Beliefs - OLS

	Only Not Taste-based Profile				Only Taste-based Profile				Full Sample			
	Loan Request was Responded (= 1)		Loan Request was Approved (= 1)		Loan Request was Responded (= 1)		Loan Request was Approved (= 1)		Loan Request was Responded (= 1)		Loan Request was Approved (= 1)	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Female (= 1)	-0.028	-0.027	-0.021	-0.017	0.028	0.031	-0.166	-0.164	-0.031	-0.029	-0.023	-0.024
	(0.029)	(0.027)	(0.023)	(0.021)	(0.025)	(0.028)	(0.056)	(0.042)	(0.026)	(0.026)	(0.021)	(0.018)
Taste-based Profile (= 1)									0.006	0.011	0.155	0.140
									(0.030)	(0.032)	(0.037)	(0.030)
Female × (Taste-based Profile)									0.053	0.049	-0.152	-0.139
									(0.036)	(0.036)	(0.057)	(0.049)
Observations	934	934	934	934	379	379	379	379	1,313	1,313	1,313	1,313
R^2	0.226	0.248	0.189	0.257	0.254	0.291	0.305	0.352	0.193	0.208	0.181	0.237
Mean Male (= 1) if Not Taste-based	0.853	0.853	0.320	0.320					0.853	0.853	0.320	0.320
Mean Male (= 1) if Taste-based					0.881	0.881	0.421	0.421	0.881	0.881	0.421	0.421
Stratification Var.	√	√	√	√	√	√	√	√	√	√	√	√
Loan Amount-Term F.E.	√	√	√	√	√	√	√	√	√	√	√	√
Week F.E.	√	√	√	√	√	√	√	√	√	√	√	√
Information Treatment	√	√	√	√	√	√	√	√	√	√	√	√
Borrower's Baseline Covariates	×	√	×	√	×	√	×	√	×	√	×	√
Loan Officer's Baseline Covariates	×	√	×	√	×	√	×	√	×	√	×	√

Note: Sample of analysis are submitted loan requests. Taste-based Profile is a dummy variable that equals 1 if the loan officer reports that the source of the main problem he/she faces is taste-based when dealing with female clients but statistical-based when dealing with male clients (and zero otherwise). All regressions include stratification variables, i.e., a dummy for loan officer's gender, and 61 region-bank fixed effects; plus 8 dummies for the requested loan amount-term, a dummy that is equal to 1 if the loan officer received the information treatment (and 0 if not), and 22 week-time fixed effects. Model (2) also controls for baseline covariates at the borrower level, including age dummies (<29; 29−38), if married, monthly wage dummies (600−1,200 USD; >1,200 USD), if self-employed, and if a client of the assigned bank; as well as baseline covariates at the loan officer level, including age dummies (<29; 29−48), if has a higher education degree, and dummies by years of experience in the banking sector (<5; 6−10). Following the standard procedure, when a control variable has a missing value, we impute a value equal to 0 and add a dummy variable equal to 1 for that observation, which indicates that the control variable was missing. Standard errors (in parentheses) are clustered at the region-bank level.

Table 4—: Heterogeneous Effects by Loan Officer's Gender Preference - OLS

	Only Not-Pro-Male Officers				Only Pro-Male Officers				Full Sample			
	Loan Request was Responded (= 1)		Loan Request was Approved (= 1)		Loan Request was Responded (= 1)		Loan Request was Approved (= 1)		Loan Request was Responded (= 1)		Loan Request was Approved (= 1)	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Female (= 1)	-0.017	-0.020	-0.031	-0.031	-0.013	-0.003	-0.162	-0.170	-0.016	-0.015	-0.034	-0.033
	(0.025)	(0.024)	(0.027)	(0.026)	(0.055)	(0.056)	(0.058)	(0.057)	(0.027)	(0.026)	(0.026)	(0.024)
Loan Officer is Pro-Male (= 1)									-0.043	-0.045	0.006	0.016
									(0.039)	(0.037)	(0.044)	(0.047)
Female × (Loan Officer is Pro-Male)									0.009	0.009	-0.112	-0.109
									(0.054)	(0.053)	(0.058)	(0.053)
p-value $H_0 : \beta_{female} > 0$	0.248	0.212	0.130	0.121	0.406	0.482	0.004	0.002				
p-value $H_0 : \beta_{inter} > 0$									0.569	0.563	0.029	0.023
Upper Bound β_{female}	-0.017	-0.020	-0.031	-0.031	-0.006	0.011	-0.147	-0.160				
	(0.025)	(0.024)	(0.027)	(0.026)	(0.049)	(0.056)	(0.054)	(0.052)				
Upper Bound β_{inter}									0.011	0.013	-0.099	-0.098
									(0.052)	(0.052)	(0.050)	(0.048)
Observations	957	957	957	957	356	356	356	356	1,313	1,313	1,313	1,313
R^2	0.210	0.226	0.166	0.230	0.335	0.366	0.293	0.362	0.192	0.207	0.178	0.235
Mean Male (= 1) if L.O. is Pro-Male					0.824	0.824	0.313	0.313	0.824	0.824	0.313	0.313
Mean Male (= 1) if L.O. is Not-Pro-Male	0.873	0.873	0.361	0.361					0.873	0.873	0.361	0.361
Stratification Var.	√	√	√	√	√	√	√	√	√	√	√	√
Requested Loan Amount-Term F.E.	√	√	√	√	√	√	√	√	√	√	√	√
Week F.E.	√	√	√	√	√	√	√	√	√	√	√	√
Information Treatment	√	√	√	√	√	√	√	√	√	√	√	√
Borrower's Baseline Covariates	×	√	×	√	×	√	×	√	×	√	×	√
Loan Officer's Baseline Covariates	×	√	×	√	×	√	×	√	×	√	×	√

Note: Sample of analysis are submitted loan requests. A loan officer is Pro-Male if his optimal distribution of portfolio clientele includes more than 50% of male clients. All regressions include stratification variables, i.e., a dummy for loan officer's gender, and 61 region-bank fixed effects; plus 8 dummies for the requested loan amount-term, a dummy that is equal to 1 if the loan officer received the information treatment (and 0 if not), and 22 week-time fixed effects. Model (2) also controls for baseline covariates at the borrower level, including age dummies (<29; 29−38), if married, monthly wage dummies (600−1,200 USD; >1,200 USD), if self-employed, and if a client of the assigned bank; as well as baseline covariates at the loan officer level, including age dummies (<29; 29−48), if has a higher education degree, and dummies by years of experience in the banking sector (<5; 6−10). Following the standard procedure, when a control variable has a missing value, we impute a value equal to 0 and add a dummy variable equal to 1 for that observation, which indicates that the control variable was missing. Standard errors (in parentheses) are clustered at the region-bank level. According to the pre-analysis plan, we report (i) the p-value for a one-sided test of the null hypothesis that $H_0 : \beta_{Female} > 0$; and (ii) the p-value for a one-sided test of the null hypothesis that the interaction of the Female dummy and the Loan Officer's Gender Preference dummy (i.e. Female Borrower × Loan Officer is Pro-Male) is greater than zero. Upper bound effects are calculated following Lee (2009)

Table 5—: Heterogeneous Effects by Gender and Gender Preference of Loan Officer - OLS

	Only Female Officers				Only Male Officers			
	Loan Request was Responded (= 1)		Loan Request was Approved (= 1)		Loan Request was Responded (= 1)		Loan Request was Approved (= 1)	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Female (= 1)	0.008	0.006	-0.041	-0.028	-0.047	-0.054	-0.026	-0.054
	(0.035)	(0.033)	(0.033)	(0.038)	(0.026)	(0.029)	(0.060)	(0.059)
Loan Officer is Pro-male (= 1)	-0.099	-0.104	-0.015	0.007	0.087	0.090	0.071	0.062
	(0.042)	(0.038)	(0.044)	(0.051)	(0.057)	(0.065)	(0.105)	(0.086)
Female × (Loan Officer is Pro-male)	0.046	0.050	-0.063	-0.084	-0.087	-0.094	-0.237	-0.183
	(0.057)	(0.055)	(0.066)	(0.067)	(0.082)	(0.087)	(0.094)	(0.095)
p-value $H_0 : \beta_{Female \times (L.O. is Pro-male)} > 0$	0.790	0.815	0.171	0.110	0.147	0.145	0.009	0.031
Upper Bound β_{inter}	0.052	0.054	-0.050	-0.070	-0.094	-0.096	-0.211	-0.169
	(0.055)	(0.055)	(0.059)	(0.064)	(0.094)	(0.097)	(0.087)	(0.099)
Observations	838	838	838	838	475	475	475	475
R^2	0.246	0.265	0.216	0.267	0.194	0.222	0.218	0.296
Mean Male (= 1) if L.O. is Female and Not-Pro-Male	0.856	0.856	0.342	0.342				
Mean Male (= 1) if L.O. is Male and Not-Pro-Male					0.896	0.896	0.387	0.387
Region-Bank F.E.	√	√	√	√	√	√	√	√
Requested Loan Amount-Term F.E.	√	√	√	√	√	√	√	√
Week F.E.	√	√	√	√	√	√	√	√
Information Treatment	√	√	√	√	√	√	√	√
Loan Officer's Gender	×	×	×	×	×	×	×	×
Borrower's Baseline Covariates	×	√	×	√	×	√	×	√
Loan Officer's Baseline Covariates	×	√	×	√	×	√	×	√

Note: Sample of analysis are submitted loan requests. A loan officer is Pro-Male if his optimal distribution of portfolio clientele includes more than 50% of male clients. All regressions include 61 region-bank fixed effects; plus 8 dummies for the requested loan amount-term, a dummy that is equal to 1 if the loan officer received the information treatment (and 0 if not), and 22 week-time fixed effects. Model (2) also controls for baseline covariates at the borrower level, including age dummies (<28; 29−35), If married, monthly wage dummies (600−1,200 USD; >1,200 USD), if self-employed, and if a client of the bank; baseline covariates at the loan officer level, including age dummies (<28; 29−48), if has a higher education degree, and dummies by years of experience in the banking sector (<5; 6−10). Following the standard procedure, when a control variable has a missing value, we impute a value equal to 0 and add a dummy variable equal to 1 for that observation, which indicates that the control variable was missing. Standard errors clustered at the region-bank level are shown in parentheses. According to the pre-analysis plan, we report (i) the p-value for a one-sided test of the null hypothesis that Female Borrower × L.O. is Male > 0. Upper bound effects are calculated following Lee (2009).

Table 6—: Information Treatment Effects - OLS

	Control Group				Treatment Group				Full Sample			
	Loan Request was Responded (= 1)		Loan Request was Approved (= 1)		Loan Request was Responded (= 1)		Loan Request was Approved (= 1)		Loan Request was Responded (= 1)		Loan Request was Approved (= 1)	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Female (= 1)	0.004	-0.003	-0.050	-0.046	-0.037	-0.032	-0.069	-0.070	0.003	-0.001	-0.063	-0.057
	(0.032)	(0.032)	(0.034)	(0.028)	(0.031)	(0.028)	(0.041)	(0.042)	(0.030)	(0.029)	(0.028)	(0.025)
Information Treatment (= 1)									0.006	0.007	-0.002	-0.001
									(0.020)	(0.022)	(0.032)	(0.028)
Female × (Information Treatment)									-0.038	-0.029	-0.006	-0.013
									(0.038)	(0.038)	(0.050)	(0.050)
p-value $H_0 : \beta_{female} > 0$	0.545	0.462	0.073	0.053	0.120	0.135	0.051	0.050				
p-value $H_0 : \beta_{inter} \leq 0$									0.843	0.782	0.550	0.604
Observations	679	679	679	679	634	634	634	634	1,313	1,313	1,313	1,313
R^2	0.235	0.259	0.201	0.275	0.234	0.270	0.211	0.358	0.191	0.205	0.173	0.230
Mean Male (= 1) in Control Group	0.857	0.857	0.341	0.341					0.857	0.857	0.341	0.341
Mean Male (= 1) in Treatment Group					0.866	0.866	0.358	0.358	0.866	0.866	0.358	0.358
Stratification Var.	√	√	√	√	√	√	√	√	√	√	√	√
Requested Loan Amount-Term F.E.	√	√	√	√	√	√	√	√	√	√	√	√
Week F.E.	√	√	√	√	√	√	√	√	√	√	√	√
Borrower's Baseline Covariates	×	√	×	√	×	√	×	√	×	√	×	√
Loan Officer's Baseline Covariates	×	√	×	√	×	√	×	√	×	√	×	√

Note: Sample of analysis are submitted loan requests. Information Treatment denotes the group receiving the information treatment. All regressions include stratification variables, i.e., a dummy for loan officer's gender, and 61 region-bank fixed effects; plus 8 dummies for the requested loan amount-term, and 22 week-time fixed effects. Model (2) also controls for baseline covariates at the borrower level, including age dummies (<28; 29−35), if married, monthly wage dummies (600−1,200 USD; >1,200 USD), if self-employed, and if a client of the bank; baseline covariates at the loan officer level, including age dummies (<28; 29−48), if has a higher education degree, and dummies by years of experience in the banking sector (<5; 6−10). Following the standard procedure, when a control variable has a missing value, we impute a value equal to 0 and add a dummy variable equal to 1 for that observation, which indicates that the control variable was missing. Standard errors clustered at the region-bank level are shown in parentheses. According to the pre-analysis plan, we report (i) the p-value for a one-sided test of the null hypothesis that $H_0 : \beta_{Female} > 0$; and (ii) the p-value for a one-sided test of the null hypothesis that the interaction of Female dummy and Gender Salience Treatment dummy (i.e. Female × Treatment) is lower or equal to zero.

Table 7—: Information Treatment Effects by Officers' Gender Preferences - OLS

	Only Not Pro-Male Officers				Only Pro-Male Officers			
	Loan Request was Responded (= 1)		Loan Request was Approved (= 1)		Loan Request was Responded (= 1)		Loan Request was Approved (= 1)	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Female (= 1)	-0.009	-0.016	-0.031	-0.033	0.047	0.063	-0.123	-0.125
	(0.026)	(0.026)	(0.035)	(0.032)	(0.084)	(0.085)	(0.076)	(0.076)
Information Treatment (= 1)	0.003	0.001	-0.012	-0.005	0.016	0.040	0.089	0.075
	(0.029)	(0.030)	(0.028)	(0.029)	(0.080)	(0.080)	(0.113)	(0.117)
Female × (Information Treatment)	-0.017	-0.007	0.001	0.004	-0.121	-0.130	-0.079	-0.089
	(0.038)	(0.037)	(0.056)	(0.057)	(0.080)	(0.077)	(0.084)	(0.084)
Upper Bound β_{inter}	-0.017	-0.007	0.001	0.004	-0.176	-0.192	-0.052	-0.083
	(0.038)	(0.037)	(0.056)	(0.057)	(0.078)	(0.070)	(0.095)	(0.091)
Observations	957	957	957	957	356	356	356	356
R^2	0.210	0.226	0.166	0.230	0.340	0.371	0.294	0.364
Mean Male (= 1) if L.O. is Pro-Male and Control Group	0.779	0.779	0.250	0.250				
Mean Male (= 1) if L.O. is Not Pro-Male and Control Group					0.882	0.882	0.370	0.370
Stratification Var.	√	√	√	√	√	√	√	√
Requested Loan Amount-Term F.E.	√	√	√	√	√	√	√	√
Week F.E.	√	√	√	√	√	√	√	√
Borrower's Baseline Covariates	×	√	×	√	×	√	×	√
Loan Officer's Baseline Covariates	×	√	×	√	×	√	×	√

Note: Sample of analysis are submitted loan requests. A loan officer is Pro-Male if his optimal distribution of portfolio clientele includes more than 50% of male clients. All regressions include stratification variables, i.e., a dummy for loan officer's gender, and 61 region-bank fixed effects; plus 8 dummies for the requested loan amount-term, and 22 week-time fixed effects. Model (2) also controls for baseline covariates at the borrower level, including age dummies (<28; 29−35), if married, monthly wage dummies (600−1,200 USD; >1,200 USD), if self-employed, and if a client of the bank; baseline covariates at the loan officer level, including age dummies (<28; 29−48), if has a higher education degree, and dummies by years of experience in the banking sector (<5; 6−10). Following the standard procedure, when a control variable has a missing value, we impute a value equal to 0 and add a dummy variable equal to 1 for that observation, which indicates that the control variable was missing. Standard errors clustered at the region-bank level are shown in parentheses. Upper bound effects are calculated following Lee (2009).

Table 8—: Heterogeneous Effects by Market Concentration at the Municipality Level - OLS

	All Loan Requests				Only Loan Requests submitted to Not Pro-Male Officers				Only Loan Requests submitted to Pro-Male Officers			
	Loan Request was Responded (= 1)		Loan Request was Approved (= 1)		Loan Request was Responded (= 1)		Loan Request was Approved (= 1)		Loan Request was Responded (= 1)		Loan Request was Approved (= 1)	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Female (= 1)	0.074	0.057	-0.073	-0.030	0.079	0.068	0.080	0.085	0.068	0.071	0.210	0.201
	(0.041)	(0.043)	(0.063)	(0.046)	(0.062)	(0.065)	(0.093)	(0.092)	(0.116)	(0.115)	(0.206)	(0.190)
HH Index	0.005	0.004	-0.007	-0.005	0.006	0.004	-0.009	-0.007	0.006	0.007	0.023	0.020
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.003)	(0.003)	(0.008)	(0.008)	(0.011)	(0.011)
Female × HH Index	-0.006	-0.005	0.000	-0.002	-0.006	-0.006	-0.007	-0.008	-0.005	-0.005	-0.025	-0.025
	(0.002)	(0.002)	(0.004)	(0.002)	(0.003)	(0.003)	(0.005)	(0.005)	(0.008)	(0.008)	(0.012)	(0.012)
Observations	1,313	1,313	1,313	1,313	957	957	957	957	356	356	356	356
R^2	0.192	0.206	0.177	0.234	0.213	0.228	0.177	0.236	0.336	0.367	0.302	0.370
Stratification Var.	√	√	√	√	√	√	√	√	√	√	√	√
Loan Amount-Term F.E.	√	√	√	√	√	√	√	√	√	√	√	√
Week F.E.	√	√	√	√	√	√	√	√	√	√	√	√
Information Treatment	√	√	√	√	√	√	√	√	√	√	√	√
Borrower's Baseline Cov.	×	√	×	√	×	√	×	√	×	√	×	√
Loan Officer's Baseline Cov.	×	√	×	√	×	√	×	√	×	√	×	√

Note: Sample of analysis are submitted loan requests. HH Index is calculated at the municipality level as $(1/100) \times \sum_{k=1}^{K} s_k^2$ where s_k is the number of bank k's local offices divided by the total number of bank offices in a given municipality. The greater the HH Index, the more concentrated is the market. The HH Index ranges from 0 to 100. All regressions include stratification variables, i.e., a dummy for loan officer's gender, and 61 region-bank fixed effects; plus 8 dummies for the requested loan amount-term, a dummy that is equal to 1 if the loan officer received the information treatment (and 0 if not), and 22 week-time fixed effects. Model (2) also controls for baseline covariates at the borrower level, including age dummies (<29; 29−38), if married, monthly wage dummies (600−1,200 USD; >1,200 USD), if self-employed, and if a client of the assigned bank; as well as baseline covariates at the loan officer level, including age dummies (<29; 29−48), if has a higher education degree, and dummies by years of experience in the banking sector (<5; 6−10). Following the standard procedure, when a control variable has a missing value, we impute a value equal to 0 and add a dummy variable equal to 1 for that observation, which indicates that the control variable was missing. Standard errors (in parentheses) are clustered at the region-bank level.

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Appendix Tables and Figures (FOR ONLINE PUBLICATION)

Table A.I—: Gender Differences in Loan Approval Rate: Non-experimental Evidence - OLS

	Panel A: All Loan Requests							
	All Applicants				Only Non-Client Applicants			
	Unadjusted Mean Difference	Model (1)	Model (2)	Matching ATE	Unadjusted Mean Difference	Model (1)	Model (2)	Matching ATE
Female (= 1)	-0.0030	-0.0075	-0.0001	-0.0153	-0.0044	0.0125	0.0160	-0.0029
	(0.0004)	(0.0004)	(0.0004)	(0.0076)	(0.0010)	(0.0009)	(0.0008)	(0.0083)
Observations	4,767,623	4,767,623	4,767,623	15,000	888,748	888,748	888,748	15,000
R^2	0.000	0.124	0.317	0.000	0.000	0.250	0.418	0.000
Mean Male (= 1)	0.714	0.714	0.714	0.716	0.400	0.400	0.400	0.404
Bank F.E	×	√	√	√	×	√	√	√
Amount-Term F.E.	×	√	√	√	×	√	√	√
Year-Month F.E.	×	√	√	√	×	√	√	√
Borrower's Baseline Cov.	×	×	√	√	×	×	√	√
	Panel B: Only \$1,500 - \$13,500 USD Loan Requests among Applicants Aged 25-35							
	All Applicants				Only Non-Client Applicants			
	Unadjusted Mean Difference	Model (1)	Model (2)	Matching ATE	Unadjusted Mean Difference	Model (1)	Model (2)	Matching ATE
Female (= 1)	-0.0103	-0.0016	0.0007	0.0025	-0.0096	0.0088	0.0027	-0.0104
	(0.0009)	(0.0008)	(0.0007)	(0.0076)	(0.0021)	(0.0019)	(0.0016)	(0.0079)
Observations	1,083,092	1,083,092	1,083,092	15,000	197,135	197,135	197,135	15,000
R^2	0.000	0.124	0.379	0.000	0.000	0.183	0.427	0.000
Mean Male (= 1)	0.696	0.696	0.696	0.697	0.325	0.325	0.325	0.323
Bank F.E	×	√	√	√	×	√	√	√
Amount-Term F.E.	×	√	√	√	×	√	√	√
Year-Month F.E.	×	√	√	√	×	√	√	√
Borrower's Baseline Cov.	×	×	√	√	×	×	√	√

Note: The sample analyzed is the universe of loan applications within the Chilean commercial banking system between January 2013 and August 2016. Panel A includes all loan requests, without sample restrictions on age and/or amount. Panel B includes only loan requests between \$1,500 and \$13,500 USD submitted by applicants between 25 and 35 years old. Regression model (1) controls for bank fixed effects, loan amount-term fixed effects, and month-year fixed effects. Model (2) additionally controls for covariates at the borrower level, if married, wage dummies (0−6000 USD; 600−1200 USD), if borrower has debt in the financial system, dummies by the decile of debt, and dummies by the decile of credit score. Panel A regressions also include age cohort dummies (25-35; 35-45; 45-55; 55-65; >65). For the Average Treatment Effect (ATE), the nearest-neighbor matching estimator (nnmatch) is implemented by using the full list of covariates included in Model 2 and a random sample of 15,000 observations (with replacement). We use 1 to 1 matching, i.e., a single match per observation. Mahalanobis distance metric is assumed.

Table A.II—: Baseline Experiment

Panel A. Loan Requests					
	Female	Male	Unadjusted Mean Diff.	Model (1)	Model (2)
Number of Assigned loan requests	932 57.7%	684 42.3%			
Number of submitted loan requests (Non-Attriters)	766	547			
Number of not-submitted loan requests (Attriters)	166	137			
Attrition rate	0.178	0.200	-0.022 (0.024)	-0.012 (0.024)	-0.007 (0.020)
Observations			1,616	1,616	1,616

Panel B. Loan Applicants					
	Female	Male	Unadjusted Mean Diff.	Model (1)	Model (2)
Number of Borrowers	233 57.7%	171 42.3%			
Prop. of Borrowers that submitted 4 out of 4 assigned requests	0.704	0.713	-0.010 (0.046)	-0.023 (0.047)	-0.028 (0.047)
Prop. of Borrowers that submitted 3 out of 4 assigned requests	0.107	0.082	0.025 (0.029)	0.029 (0.031)	0.025 (0.032)
Prop. of Borrowers that submitted 2 out of 4 assigned requests	0.039	0.023	0.015 (0.017)	0.016 (0.017)	0.015 (0.017)
Prop. of Borrowers that submitted 1 out of 4 assigned requests	0.073	0.053	0.020 (0.024)	0.025 (0.025)	0.029 (0.025)
Prop. of Borrowers that submitted 0 out of 4 assigned requests	0.077	0.129	-0.051 (0.031)	-0.046 (0.031)	-0.041 (0.031)
Observations			404	404	404

Note: Panel A units of analysis are 1,616 assigned loan requests (4 loan requests per each borrower). Attriter observations are assigned loan request that were not submitted by the tester to the assigned loan officer. Panel A Model (1) regression follows the main estimating equation (1) and includes stratification variables, i.e., a dummy for loan officer's gender, and 61 region-bank fixed effects; plus 8 dummies for the requested loan amount-term, and a dummy that is equal to 1 if the loan officer received the information treatment (and 0 if not). Model (2) also controls for baseline covariates at the borrower level, including age dummies (<29; 29−38), if married, monthly wage dummies (600−1,200 USD; >1,200 USD), if self-employed, and if a client of the assigned bank; as well as baseline covariates at the loan officer level, including age dummies (<29; 29−48), if has a higher education degree, and dummies by years of experience in the banking sector (<5; 6−10). As in the main estimating equation (1), Panel A standard errors (in parentheses) are clustered at the region-bank level. Panel B units of analysis are 404 borrowers. Panel B Model (1) regression includes 11 region fixed effects, while Panel B Model (2) additionally control for the same set of baseline covariates at the borrower level. Robust standard errors (in parentheses) are reported in Panel B. Following the standard procedure, when a control variable has a missing value, we impute a value equal to 0 and add a dummy variable equal to 1 for that observation, which indicates that the control variable was missing.

Table A.III—: Determinants of Attrition - OLS

	Number of Loan Requests Not Submitted by Tester	Number of Loan Requests Not Submitted by Tester	If Tester Did Not Submit at least 1 out of 4 Loan Requests (= 1)	If Tester Did Not Submit at least 1 out of 4 Loan Requests (= 1)
Female (= 1)	-0.108		-0.001	
	(0.138)		(0.045)	
≤28 years-old (= 1)	-0.079	-0.029	-0.034	-0.086
	(0.140)	(0.235)	(0.046)	(0.072)
Married (= 1)	0.132	0.380	-0.002	0.056
	(0.254)	(0.369)	(0.075)	(0.110)
If Has Active Loan (= 1)	-0.112	-0.332	-0.039	-0.118
	(0.156)	(0.244)	(0.050)	(0.075)
Wage Range $600 - 1,200$ USD (= 1)	-0.162	-0.263	0.020	-0.040
	(0.168)	(0.307)	(0.061)	(0.097)
Wage Range $> 1,200$ USD (= 1)	0.168	-0.121	0.089*	0.012
	(0.162)	(0.285)	(0.053)	(0.086)
Number of assigned Banks where the Tester is Client (= 1)	0.404***	0.561**	0.154***	0.167**
	(0.151)	(0.246)	(0.047)	(0.071)
≤28 years-old × Female		-0.052		0.098
		(0.290)		(0.094)
Married × Female		-0.458		-0.128
		(0.506)		(0.150)
If Have Active Loan × Female		0.428		0.138
		(0.317)		(0.103)
Wage Range $600 - 1,200$ USD × Female		0.130		0.080
		(0.357)		(0.126)
Wage Range $> 1,200$ USD × Female		0.475		0.134
		(0.344)		(0.109)
Number of assigned Banks where the Tester is Client × Female		-0.289		-0.021
		(0.311)		(0.096)
Observations	404	404	404	404
R^2	0.043	0.057	0.050	0.063

Note: The sample of analysis is all testers. Robust standard errors are shown in parentheses.

Table A.IV—: Summary Statistics and Baseline Balance

	Obs.	Mean	Std. Dev.	Female	Male	Adjusted Diff.	$p-$val.
Panel A. Borrowers' characteristics							
Borrower is $\leq$28 years-old (= 1)	1,313	0.435	0.496	0.440	0.428	-0.001	0.979
Borrower is 28-38 years-old (= 1)	1,313	0.541	0.499	0.539	0.543	0.009	0.820
Borrower is >38 years-old (= 1)	1,313	0.024	0.154	0.021	0.029	-0.008	0.268
Borrower is married (= 1)	1,313	0.104	0.305	0.008	0.137	-0.064	0.003
Borrower has active loan (= 1)	1,313	0.474	0.500	0.457	0.499	-0.045	0.120
Borrower has active loan in the assigned bank (= 1)	1,313	0.056	0.231	0.063	0.048	0.010	0.202
Borrower is unemployed (= 1)	1,313	0.102	0.303	0.095	0.112	-0.020	0.450
Borrower is self-employed (= 1)	1,313	0.371	0.483	0.385	0.351	0.046	0.092
Borrower's monthly wage is below the median (= 1)	1,313	0.526	0.500	0.547	0.495	0.045	0.145
Borrower's monthly wage is in range 0−600 USD (= 1)	1,313	0.286	0.452	0.298	0.269	0.020	0.528
Borrower's monthly wage is in range 600−1,200 USD (= 1)	1,313	0.225	0.418	0.234	0.214	0.022	0.445
Borrower's monthly wage is >1,200 USD (= 1)	1,313	0.489	0.500	0.469	0.517	-0.041	0.114
Borrower's monthly wage is within richest 1% (= 1)	1,313	0.003	0.055	0.000	0.007	-0.009	0.159

Note: Sample of analysis are submitted (non-attriter) loan requests. Mean differences across loan requests assigned to male and female borrowers are adjusted by the same set of variables included in the main regression used to estimate gender discrimination effects, i.e., a dummy for loan officer's gender, 61 region-bank fixed effects, 8 dummies for the requested loan amount-term, a dummy that is equal to 1 if the loan officer received the information treatment (and 0 if not), and 22 week-time fixed effects. The reported p-value is calculated based on standard errors clustered at the region-bank level.

Table A.V—: Summary Statistics and Baseline Balance (cont.)

	Obs.	Mean	Std. Dev.	Female	Male	Adjusted Diff.	$p-$val.
Panel B. Loan Officers' characteristics							
Loan officer is female (=1)	1,313	0.638	0.481	0.654	0.616	0.015	0.442
Loan officer is married (=1)	1,313	0.306	0.461	0.311	0.300	0.011	0.694
Loan officer has higher education (=1)	1,313	0.957	0.202	0.953	0.963	-0.012	0.098
Loan officer's experience is ≤ 5 years (=1)	1,313	0.424	0.494	0.431	0.415	0.024	0.286
Loan officer's experience is $6-10$ years (=1)	1,313	0.312	0.463	0.312	0.311	-0.006	0.824
Loan officer's experience is > 10 years (=1)	1,313	0.264	0.441	0.257	0.274	-0.018	0.267
Loan officer is 19−28 years−old (=1)	1,313	0.213	0.410	0.210	0.218	-0.002	0.907
Loan officer is 29−48 years−old (=1)	1,313	0.693	0.461	0.691	0.697	-0.014	0.547
Loan officer is >48 years−old (=1)	1,313	0.094	0.291	0.099	0.086	0.017	0.183
Loan officer's gender preferences are pro-male (=1)	1,313	0.271	0.445	0.294	0.239	0.058	0.016
If main problem with male clients is that....							
(a) male clients have low repayment rates (=1)	1,313	0.163	0.369	0.167	0.157	0.001	0.970
(b) male clients are uninformed about financial products (=1)	1,313	0.262	0.440	0.257	0.269	-0.005	0.828
(c) male clients demand excessive administrative duties (=1)	1,313	0.113	0.316	0.107	0.121	-0.018	0.390
(d) male clients are difficult to communicate with (=1)	1,313	0.164	0.370	0.166	0.161	-0.002	0.885
(e) male clients are too tough and require quick responses (=1)	1,313	0.299	0.458	0.303	0.293	0.024	0.260
If main problem with female clients is that...							
(a) female clients have low repayment rates (=1)	1,313	0.041	0.199	0.040	0.042	-0.007	0.335
(b) female clients are uninformed about financial products (=1)	1,313	0.259	0.438	0.256	0.263	-0.009	0.718
(c) female clients demand excessive administrative duties (=1)	1,313	0.137	0.344	0.132	0.144	-0.006	0.718
(d) female clients are difficult to communicate with (=1)	1,313	0.096	0.295	0.082	0.115	-0.043	0.082
(e) female clients are too tough and require quick responses (=1)	1,313	0.467	0.499	0.490	0.435	0.064	0.069
Loan officer's gender beliefs are classified as taste-based profile (=1)	1,313	0.289	0.453	0.287	0.291	-0.007	0.780
If received information treatment (=1)	1,313	0.483	0.500	0.478	0.490	-0.037	0.072

Note: Sample of analysis are submitted (non-attriter) loan requests. Mean differences across loan requests assigned to male and female borrowers are adjusted by the same set of variables included in the main regression used to estimate gender discrimination effects, i.e., a dummy for loan officer's gender, 61 region-bank fixed effects, 8 dummies for the requested loan amount-term, a dummy that is equal to 1 if the loan officer received the information treatment (and 0 if not), and 22 week-time fixed effects. The reported p-value is calculated based on standard errors clustered at the region-bank level. When test balance for "Loan Officer is Female (=1)", the dummy for loan officer's gender is omitted from the regression. When test balance for "If received pro-female salience message (=1)", the dummy that is equal to 1 if the loan officer received the information treatment (and 0 if not) is omitted from the regression.

Table A.VI—: Summary Statistics and Baseline Balance (cont.)

	Obs.	Mean	Std. Dev.	Female	Male	Adjusted Diff.	$p-$val.
Panel C. Banks							
If loan request assigned to a loan officer in bank 1 (= 1)	1,313	0.100	0.300	0.106	0.091	0.013	0.299
If loan request assigned to a loan officer in bank 2 (= 1)	1,313	0.025	0.157	0.023	0.027	-0.005	0.367
If loan request assigned to a loan officer in bank 3 (= 1)	1,313	0.065	0.246	0.067	0.062	0.006	0.757
If loan request assigned to a loan officer in bank 4 (= 1)	1,313	0.013	0.113	0.013	0.013	0.001	0.635
If loan request assigned to a loan officer in bank 5 (= 1)	1,313	0.165	0.371	0.155	0.177	-0.011	0.654
If loan request assigned to a loan officer in bank 6 (= 1)	1,313	0.206	0.404	0.206	0.205	0.010	0.422
If loan request assigned to a loan officer in bank 7 (= 1)	1,313	0.088	0.283	0.094	0.079	0.009	0.510
If loan request assigned to a loan officer in bank 8 (= 1)	1,313	0.218	0.413	0.221	0.214	-0.004	0.854
If loan request assigned to a loan officer in bank 9 (= 1)	1,313	0.122	0.327	0.115	0.132	-0.018	0.324
Panel D. Standardized Text Types							
If assigned standardized text is of type 1 (= 1)	1,313	0.115	0.319	0.104	0.130	-0.025	0.076
If assigned standardized text is of type 2 (= 1)	1,313	0.114	0.318	0.111	0.119	-0.020	0.169
If assigned standardized text is of type 3 (= 1)	1,313	0.140	0.347	0.148	0.130	0.009	0.611
If assigned standardized text is of type 4 (= 1)	1,313	0.120	0.325	0.132	0.102	0.013	0.490
If assigned standardized text is of type 5 (= 1)	1,313	0.039	0.193	0.044	0.031	0.012	0.344
If assigned standardized text is of type 6 (= 1)	1,313	0.008	0.087	0.007	0.009	-0.004	0.353
If assigned standardized text is of type 7 (= 1)	1,313	0.005	0.067	0.005	0.004	0.000	0.988
If assigned standardized text is of type 8 (= 1)	1,313	0.030	0.170	0.023	0.038	-0.010	0.363
If assigned standardized text is of type 9 (= 1)	1,313	0.028	0.166	0.030	0.026	0.008	0.394
If assigned standardized text is of type 10 (= 1)	1,313	0.028	0.166	0.029	0.027	0.001	0.905
If assigned standardized text is of type 11 (= 1)	1,313	0.023	0.149	0.025	0.020	0.007	0.444
If assigned standardized text is of type 12 (= 1)	1,313	0.030	0.172	0.030	0.031	0.003	0.677
If assigned standardized text is of type 13 (= 1)	1,313	0.029	0.168	0.029	0.029	0.006	0.520
If assigned standardized text is of type 14 (= 1)	1,313	0.028	0.166	0.029	0.027	0.006	0.473
If assigned standardized text is of type 15 (= 1)	1,313	0.030	0.172	0.026	0.037	-0.014	0.163
If assigned standardized text is of type 16 (= 1)	1,313	0.022	0.147	0.021	0.024	-0.001	0.919
If assigned standardized text is of type 17 (= 1)	1,313	0.027	0.161	0.030	0.022	0.011	0.415
If assigned standardized text is of type 18 (= 1)	1,313	0.028	0.166	0.027	0.029	0.005	0.543
If assigned standardized text is of type 19 (= 1)	1,313	0.032	0.176	0.030	0.035	-0.002	0.805
If assigned standardized text is of type 20 (= 1)	1,313	0.031	0.174	0.031	0.031	0.000	0.965
If assigned standardized text is of type 21 (= 1)	1,313	0.028	0.166	0.026	0.031	-0.005	0.418
If assigned standardized text is of type 22 (= 1)	1,313	0.032	0.176	0.027	0.038	-0.011	0.220
If assigned standardized text is of type 23 (= 1)	1,313	0.033	0.178	0.035	0.029	0.009	0.224

Note: Sample of analysis are submitted (non-attriter) loan requests. The mean differences across loan requests assigned to male and female borrowers are adjusted by the same set of variables included in the main regression used to estimate gender discrimination effects, i.e., a dummy by loan officer's gender, 61 region-bank fixed effects, 8 dummies for the requested loan amount-term, a dummy that is equal to 1 if the loan officer received the information treatment (and 0 if not), and 22 week-time fixed effects. The reported p-value is calculated based on standard errors clustered at the region-bank level. For Panel C regressions (Banks), region-bank fixed effects are omitted from regression.

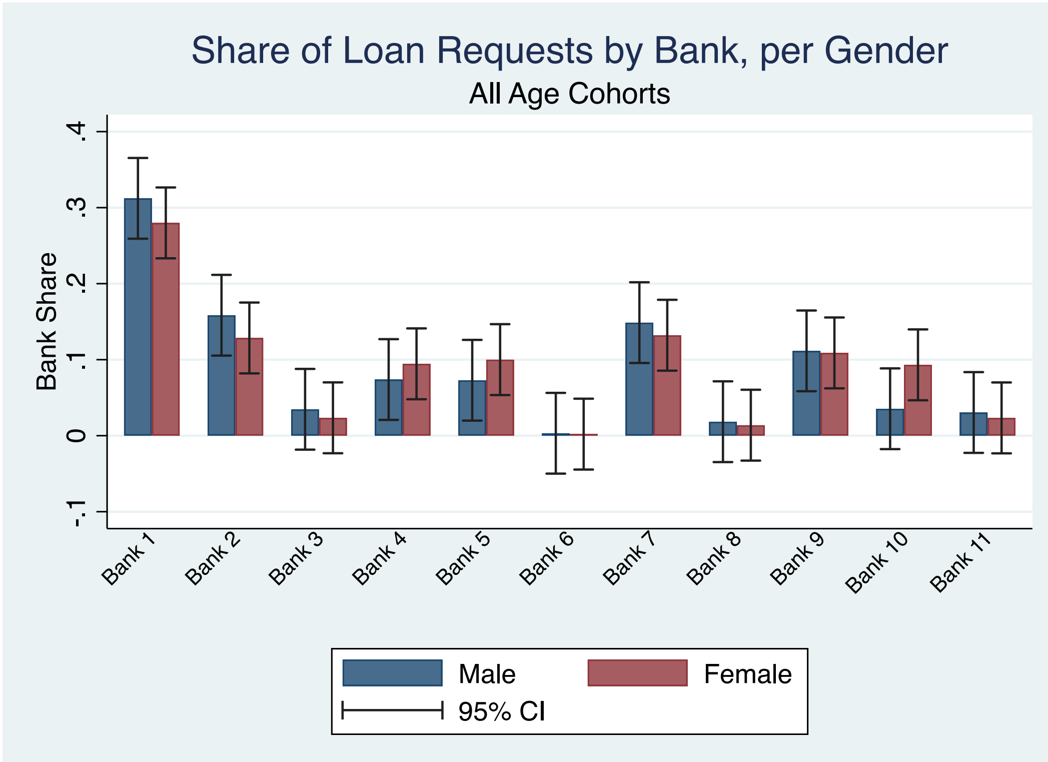
Table A.VII—: Summary Statistics and Baseline Balance (cont.)

	Obs.	Mean	Std. Dev.	Female	Male	Adjusted Diff.	$p-$val.
Panel E. Requested Loan Amounts							
Requested loan amount (USD)	1,313	7,708	3,623	7,782	7,604	168.082	0.341
If requested loan amount is $1,500 USD (= 1)	1,313	0.059	0.235	0.055	0.064	-0.009	0.328
If requested loan amount is $3,000 USD (= 1)	1,313	0.133	0.340	0.131	0.137	-0.009	0.656
If requested loan amount is $4,500 USD (= 1)	1,313	0.112	0.315	0.111	0.113	-0.001	0.947
If requested loan amount is $6,000 USD (= 1)	1,313	0.126	0.332	0.123	0.130	-0.002	0.909
If requested loan amount is $7,500 USD (= 1)	1,313	0.135	0.342	0.145	0.121	0.024	0.276
If requested loan amount is $9,000 USD (= 1)	1,313	0.121	0.326	0.111	0.135	-0.026	0.064
If requested loan amount is $10,500 USD (= 1)	1,313	0.115	0.319	0.123	0.104	0.017	0.353
If requested loan amount is $12,000 USD (= 1)	1,313	0.125	0.331	0.121	0.130	-0.010	0.583
If requested loan amount is $13,500 USD (= 1)	1,313	0.075	0.263	0.081	0.066	0.016	0.309
Panel F. Weeks							
Week of requested loan (1 to 23)	1,313	4.950	5.681	4.751	5.230	-0.490	0.318
If requested loan was submitted in week 1 (= 1)	1,313	0.439	0.497	0.452	0.422	0.028	0.411
If requested loan was submitted in week 2 (= 1)	1,313	0.095	0.294	0.106	0.080	0.023	0.135
If requested loan was submitted in week 3 (= 1)	1,313	0.017	0.128	0.018	0.015	0.002	0.779
If requested loan was submitted in week 4 (= 1)	1,313	0.154	0.361	0.149	0.161	-0.007	0.787
If requested loan was submitted in week 5 (= 1)	1,313	0.024	0.154	0.017	0.035	-0.015	0.097
If requested loan was submitted in week 6 (= 1)	1,313	0.030	0.172	0.031	0.029	0.004	0.709
If requested loan was submitted in week 7 (= 1)	1,313	0.021	0.142	0.020	0.022	-0.002	0.665
If requested loan was submitted in week 8 (= 1)	1,313	0.025	0.157	0.029	0.020	0.010	0.339
If requested loan was submitted in week 9 (= 1)	1,313	0.003	0.055	0.004	0.002	0.001	0.650
If requested loan was submitted in week 10 (= 1)	1,313	0.002	0.039	0.001	0.002	0.000	0.883
If requested loan was submitted in week 11 (= 1)	1,313	0.008	0.087	0.007	0.009	-0.002	0.672
If requested loan was submitted in week 12 (= 1)	1,313	0.028	0.166	0.027	0.029	-0.004	0.700
If requested loan was submitted in week 13 (= 1)	1,313	0.027	0.163	0.023	0.033	-0.014	0.115
If requested loan was submitted in week 14 (= 1)	1,313	0.037	0.190	0.037	0.038	-0.001	0.924
If requested loan was submitted in week 15 (= 1)	1,313	0.018	0.134	0.013	0.026	-0.012	0.182
If requested loan was submitted in week 16 (= 1)	1,313	0.008	0.087	0.005	0.011	-0.006	0.403
If requested loan was submitted in week 17 (= 1)	1,313	0.010	0.099	0.009	0.011	-0.002	0.648
If requested loan was submitted in week 18 (= 1)	1,313	0.005	0.067	0.004	0.005	-0.003	0.562
If requested loan was submitted in week 19 (= 1)	1,313	0.017	0.128	0.014	0.020	-0.005	0.608
If requested loan was submitted in week 20 (= 1)	1,313	0.005	0.073	0.003	0.009	-0.006	0.083
If requested loan was submitted in week 21 (= 1)	1,313	0.007	0.083	0.005	0.009	-0.004	0.384
If requested loan was submitted in week 22 (= 1)	1,313	0.009	0.095	0.009	0.009	-0.001	0.827
If requested loan was submitted in week 23 (= 1)	1,313	0.011	0.103	0.017	0.002	0.017	0.007

Note: Sample of analysis are submitted (non-attriter) loan requests. The mean differences across loan requests assigned to male and female borrowers are adjusted by the same set of variables included in the main regression used to estimate gender discrimination effects, i.e., a dummy for loan officer's gender, 61 region-bank fixed effects, 8 dummies for the requested loan amount-term, and a dummy that is equal to 1 if the loan officer received the information treatment (and 0 if not). The reported p-value is calculated based on standard errors clustered at the region-bank level. For Panel E regressions (Loan Amount), dummies for requested loan amount-term are omitted from the regression.

Figure A.I. : Share of Loan Requests by Bank, per Gender

(a) All Age Cohorts



(b) 25-35 Age Cohort

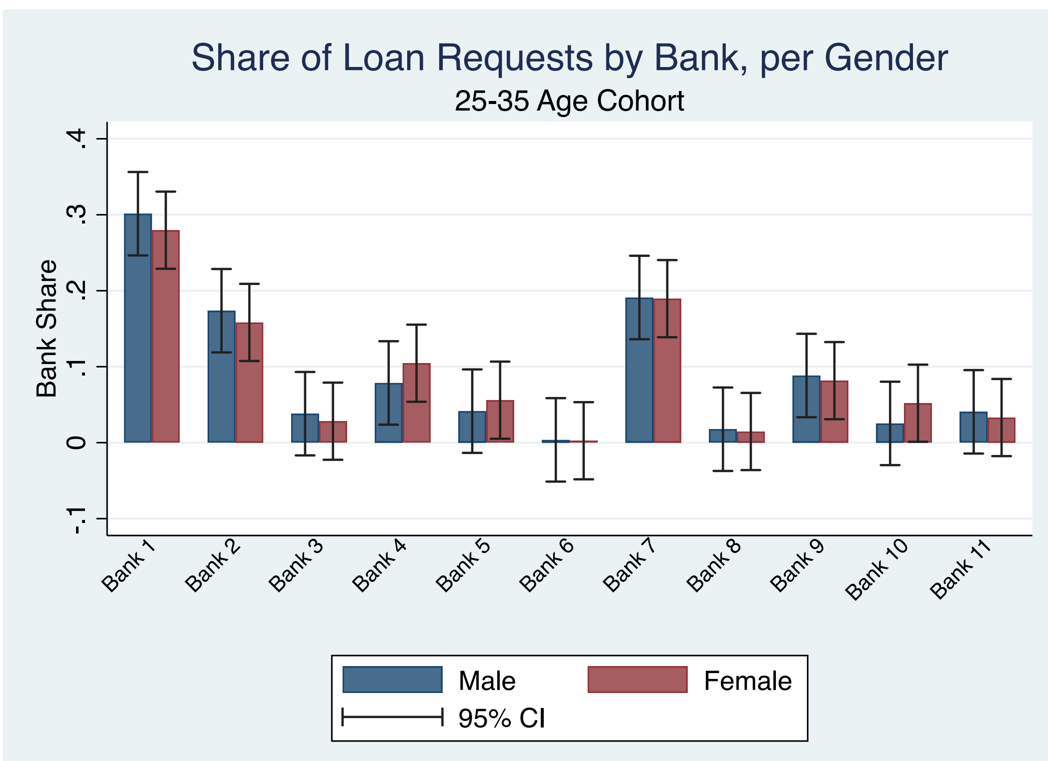


Figure A.II. : Experimental Design

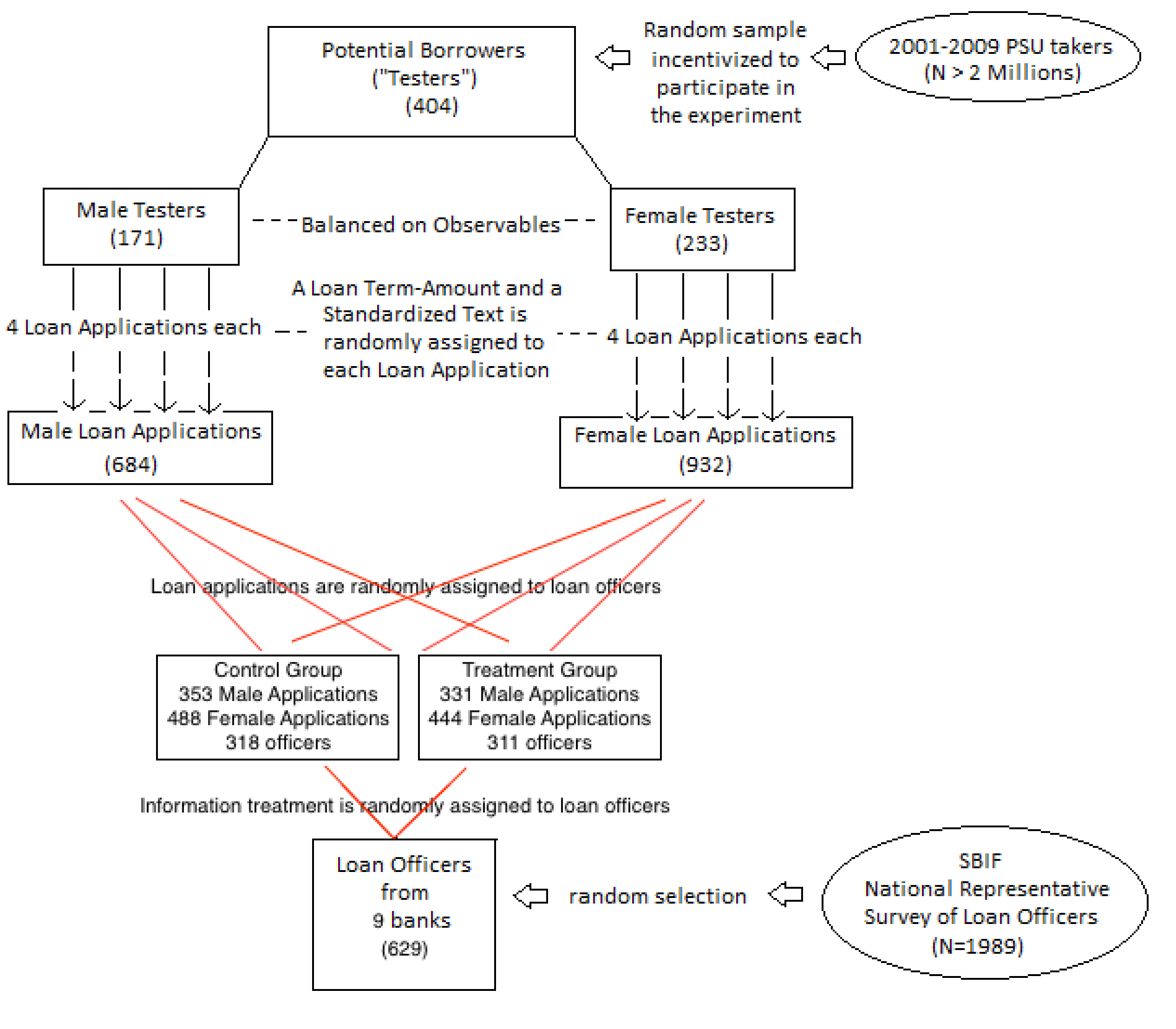


Table A.VIII—: Extensive Margin: Response and Approval Rates - Logit

	Loan Request was Responded (= 1)			Loan Request was Approved (= 1)		
	Unadjusted Mean Difference	Model (1)	Model (2)	Unadjusted Mean Difference	Model (1)	Model (2)
Female (= 1)	-0.010	-0.018	-0.019	-0.066	-0.069	-0.064
	(0.023)	(0.020)	(0.021)	(0.023)	(0.020)	(0.018)
Observations	1,313	1,313	1,313	1,313	1,313	1,313
Mean Male (= 1)	0.861	0.861	0.861	0.349	0.349	0.349
Stratification Var.	×	√	√	×	√	√
Loan Amount-Term F.E.	×	√	√	×	√	√
Information Treat.	×	√	√	×	√	√
Week F.E.	×	√	√	×	√	√
Borrower's Baseline Cov.	×	×	√	×	×	√
Officer 's Baseline Cov.	×	×	√	×	×	√

Note: Sample of analysis are submitted loan requests. Reported estimates are marginal effect from a logit model. Regression models (1) and (2) include stratification variables, i.e., a dummy for loan officer's gender, and 61 region-bank fixed effects; plus 8 dummies for the requested loan amount-term, a dummy that is equal to 1 if the loan officer received the information treatment (and 0 if not), and 22 week-time fixed effects. Model (2) also controls for baseline covariates at the borrower level, including age dummies (<29; 29−38), if married, monthly wage dummies (600−1,200 USD; >1,200 USD), if self-employed, and if a client of the assigned bank; as well as baseline covariates at the loan officer level, including age dummies (<29; 29−48), if has a higher education degree, and dummies by years of experience in the banking sector (<5; 6−10). Following the standard procedure, when a control variable has a missing value, we impute a value equal to 0 and add a dummy variable equal to 1 for that observation, which indicates that the control variable was missing. Standard errors (in parentheses) are clustered at the region-bank level.

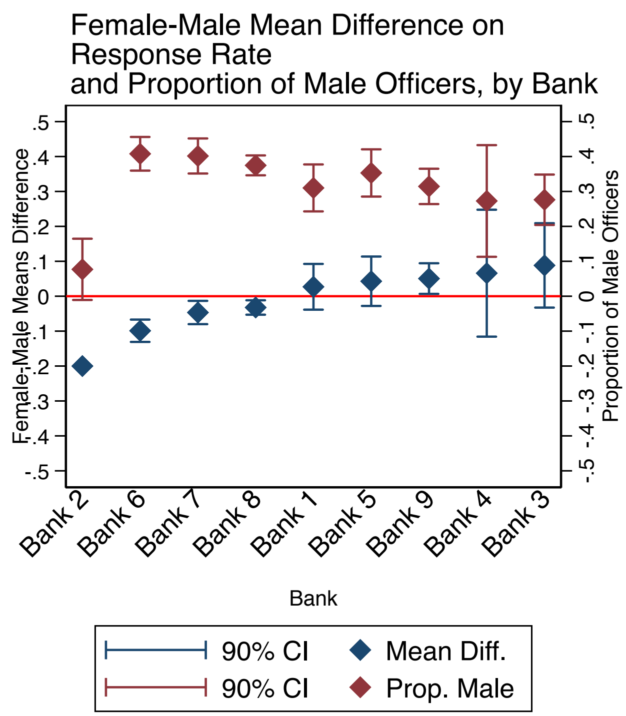
Table A.IX—: Additional Information Required - OLS

	If required More Information (= 1)		If required Wage Settlements (= 1)		If required Social Sec. Contributions (= 1)		If required College Degree Certificate (= 1)		If required Statement of Fin. Pos. (= 1)		If required Collateral Value (= 1)		If required Employment Contract (= 1)		If required Income Tax Returns (= 1)	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Female (= 1)	-0.039	-0.040	-0.025	-0.026	-0.023	-0.023	-0.008	-0.008	-0.008	-0.007	-0.005	-0.004	-0.010	-0.013	-0.004	-0.005
	(0.031)	(0.029)	(0.032)	(0.030)	(0.023)	(0.022)	(0.014)	(0.013)	(0.013)	(0.013)	(0.016)	(0.015)	(0.017)	(0.016)	(0.006)	(0.006)
p-value $H_0 : \beta_{Female} \leq 0$	0.898	0.912	0.780	0.805	0.836	0.851	0.706	0.733	0.732	0.717	0.615	0.591	0.719	0.786	0.787	0.815
Observations	1,313	1,313	1,313	1,313	1,313	1,313	1,313	1,313	1,313	1,313	1,313	1,313	1,313	1,313	1,313	1,313
R^2	0.138	0.158	0.147	0.165	0.130	0.150	0.098	0.114	0.137	0.153	0.087	0.101	0.211	0.225	0.182	0.195
Mean Male (= 1)	0.331	0.331	0.303	0.303	0.234	0.234	0.049	0.049	0.110	0.110	0.053	0.053	0.110	0.110	0.015	0.015
Stratification Var.	√	√	√	√	√	√	√	√	√	√	√	√	√	√	√	√
Amount-Term F.E.	√	√	√	√	√	√	√	√	√	√	√	√	√	√	√	√
Information Treatment	√	√	√	√	√	√	√	√	√	√	√	√	√	√	√	√
Week F.E.	√	√	√	√	√	√	√	√	√	√	√	√	√	√	√	√
Borrower's Baseline Cov.	×	√	×	√	×	√	×	√	×	√	×	√	×	√	×	√
Loan Off. Baseline Cov.	×	√	×	√	×	√	×	√	×	√	×	√	×	√	×	√

Note: Sample of analysis are submitted loan requests. All regressions include stratification variables, i.e., a dummy for loan officer's gender, and 61 region-bank fixed effects; plus 8 dummies for the requested loan amount-term, a dummy that is equal to 1 if the loan officer received the information treatment (and 0 if not), and 22 week-time fixed effects. Model (2) also controls for baseline covariates at the borrower level, including age dummies (<29; 29−38), if married, monthly wage dummies (600−1,200 USD; >1,200 USD), if self-employed, and if a client of the assigned bank; as well as baseline covariates at the loan officer level, including age dummies (<29; 29−48), if has a higher education degree, and dummies by years of experience in the banking sector (<5; 6−10). Following the standard procedure, when a control variable has a missing value, we impute a value equal to 0 and add a dummy variable equal to 1 for that observation, which indicates that the control variable was missing. Standard errors (in parentheses) are clustered at the region-bank level. According to the pre-analysis plan, the p-value for a one-sided test of the null hypothesis that $H_0 : \beta_{Female} \leq 0$ is reported separately. Upper and lower bound effects are calculated following Lee (2009).

Figure A.III. : Gender Discrimination in Response Rate and Proportion of Male Officers, by Bank

(a) Distribution across Banks



(b) Scatter Plot

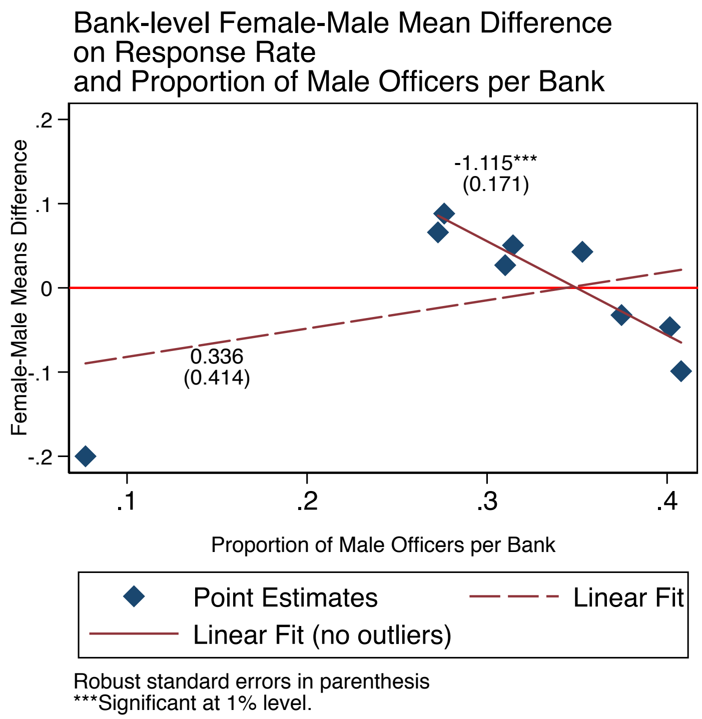
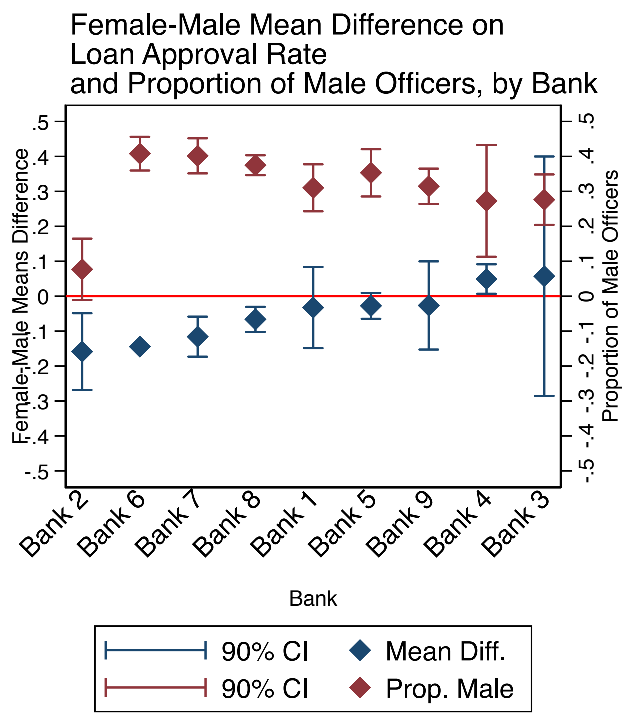


Figure A.IV. : Gender Discrimination in Approval Rate and Proportion of Male Officers, by Bank

(a) Distribution across Banks



(b) Scatter Plot

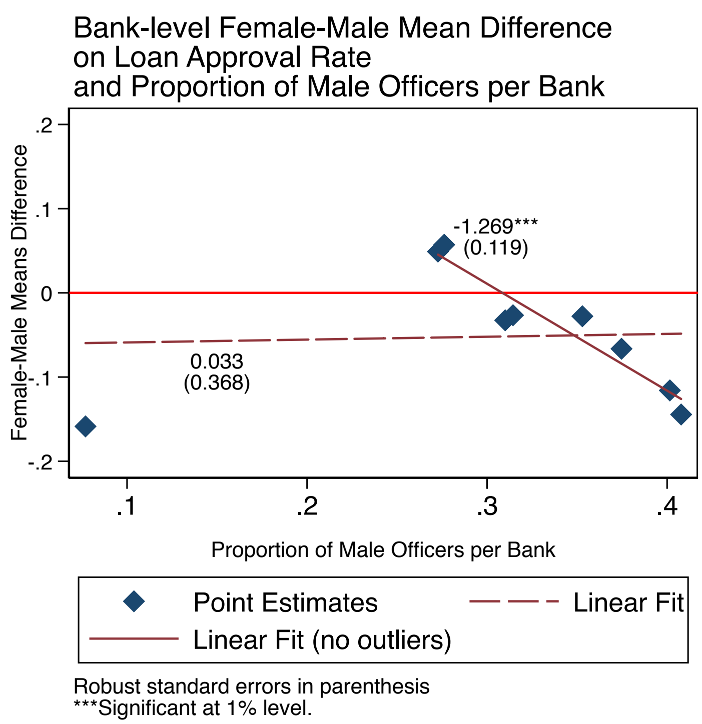


Table A.X—: Loan Officers' Gender Beliefs

	Obs.	Main problem with Female Clients	Main problem with Male Clients	Mean Diff.	p-val.
Low repayment rates (= 1)	629	0.033	0.156	-0.122	0.000
Uninformed about financial products (= 1)	629	0.277	0.302	-0.025	0.320
Demand excessive administrative duties (= 1)	629	0.138	0.119	0.019	0.313
Statistical-based reasons (= 1)	629	0.448	0.577	-0.129	0.000
Difficult to communicate (= 1)	629	0.105	0.149	-0.045	0.018
Too tough and require quick responses (= 1)	629	0.447	0.273	0.173	0.000
Taste-based reasons (= 1)	629	0.552	0.423	0.129	0.000

Note: Sample of analysis are the loan officers selected to participate in the experiment (629 in total). Loan officers are asked to report which of the five listed problems is the most important that they face when dealing with male and female clients. Reported figures correspond to the mean for each gender group of clients (male or female), the mean difference across gender groups, and the associated two-sided p-value.

Table A.XI—: Officer Experience and Beliefs concerning the Main Problem with Female and Male Clients - OLS

	Low repayment rates	Uninformed about financial products	Demand excessive administrative duties	Difficult to communicate	Too tough and require quick responses	Taste-based reasons	Taste-based pro-male profile
Panel A. Main problem with female clients							
Exp	-0.000	0.003	-0.001	-0.003	0.002	-0.002	-0.004
	(0.000)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Exp^2	0.000	-0.000	-0.000*	0.000	0.000	0.000	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
p-val. $H_0 : \beta_{Exp} + 2\beta_{Exp^2} = 0$	0.344	0.301	0.568	0.100	0.423	0.511	0.221
Obs.	629	629	629	629	629	629	629
R^2	0.092	0.138	0.117	0.105	0.133	0.137	0.139
Mean	0.043	0.262	0.136	0.099	0.460	0.559	0.291
Panel B. Main problem with male clients							
	Low repayment rates	Uninformed about financial products	Demand excessive administrative duties	Difficult to communicate	Too tough and require quick responses	Taste-based reasons	Taste-based pro-female profile
Exp	-0.001	0.000	-0.000	-0.003	0.004	0.001	-0.001
	(0.003)	(0.003)	(0.002)	(0.002)	(0.004)	(0.004)	(0.003)
Exp^2	0.000	-0.000***	-0.000**	-0.000	0.000***	0.000***	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
p-val. $H_0 : \beta_{Exp} + 2\beta_{Exp^2} = 0$	0.698	0.916	0.961	0.171	0.318	0.843	0.760
Obs.	629	629	629	629	629	629	629
R^2	0.137	0.138	0.096	0.140	0.111	0.104	0.098
Mean	0.162	0.270	0.115	0.158	0.295	0.452	0.184

Note: Sample of analysis are the loan officers selected to participate in the experiment (629 in total). Loan officers are asked to report which of the five listed problems is the most important they face when dealing with female and male clients. Taste-based Reasons is a dummy that equals 1 if the officer reports that the main problem he/she faces with male/female clients is "Difficult to communicate with" or "Too though and requiere quick responses" (and zero otherwise). Taste-based Pro-Male Profile is a dummy variable that equals 1 if the loan officer reports that the source of the main problem he/she faces is taste-based with female clients but statistical-based with male clients (and zero otherwise). Taste-based Pro-Female Profile is a dummy variable that equals 1 if the loan officer reports that the source of the main problem he/she faces is taste-based with male clients but statistical-based with female clients (and zero otherwise). Exp. is defined as years of experience in the banking sector. Panel A reports the effect of experience on the chosen reason for female clients. Panel B reports the effect of experience on the chosen reason for male clients. All regressions control for a dummy for loan officer's gender, 61 region-bank fixed effects, a dummy that is equal to 1 if the loan officer received the information treatment (and 0 if not), and baseline covariates, including age dummies (<29; 29−48), if married, and if has a higher education degree. Standard errors clustered at the region-bank level are shown in parentheses. *Significant at 10%. **Significant at 5%. ***Significant at 1%.

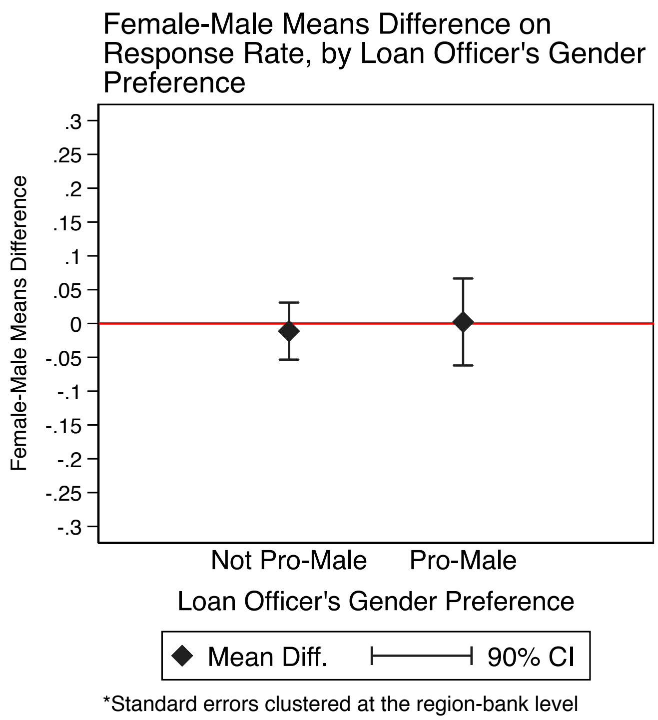
Table A.XII—: Gift Experiment - OLS

	Only Female Officers			Only Male Officers			Full Sample		
	If donate 2^{nd} ticket to the assigned colleague (= 1)			If donate 2^{nd} ticket to the assigned colleague (= 1)			If donate 2^{nd} ticket to the assigned colleague (= 1)		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Donee's Name is Feminine (= 1)	-0.035	-0.029	-0.028	-0.094	-0.035	-0.018	-0.064	-0.045	-0.043
	(0.053)	(0.074)	(0.076)	(0.057)	(0.058)	(0.057)	(0.031)	(0.043)	(0.042)
Loan Officer is Pro-Male (= 1)		0.069	0.065		0.203	0.224		0.086	0.083
		(0.081)	(0.083)		(0.130)	(0.117)		(0.076)	(0.074)
Donee's Name is Feminine × (Loan Officer is Pro-Male)		-0.040	-0.043		-0.378	-0.402		-0.083	-0.095
		(0.129)	(0.141)		(0.233)	(0.233)		(0.104)	(0.104)
Observations	411	411	411	218	218	218	629	629	629
R^2	0.146	0.148	0.167	0.254	0.274	0.293	0.132	0.135	0.152
Mean if Donee's Name is Masculine	0.584	0.584	0.584	0.717	0.717	0.717	0.628	0.628	0.628
Mean if Loan Officer is Not-Pro-Male		0.575	0.575		0.684	0.684		0.619	0.619
Region-Bank F.E.	√	√	√	√	√	√	√	√	√
Donee's Last Name	√	√	√	√	√	√	√	√	√
Info. Treat.	√	√	√	√	√	√	√	√	√
Loan Officer's Baseline Cov.	×	×	√	×	×	√	×	×	√

Note: Sample of analysis are the loan officers selected to participate in the experiment (629 in total). Loan Officer is Pro-Male is a dummy variable that equals 1 if loan officer's optimal distribution of portfolio clientele includes more than 50% of male clients. All regressions include 61 region-bank fixed effects, a dummy that is equal to 1 if the loan officer received the information treatment (and 0 if not), and two dummies for the donee's last name (Gonzales, Errazuriz). Regressions using full sample data also control for loan officer gender. Model 3 additionally controls for baseline covariates at the loan officer level, including age dummies (<28; 29−48), if has a higher education degree, and dummies by years of experience in the banking sector (<5; 6−10). Standard errors clustered at the region-bank level are shown in parentheses.

Figure A.V. : Response Rate, Officer's Gender, and Officer's Gender Preference

(a) Response Rate and Gender Preference



(b) Response Rate and Gender Preference, by Loan Officer's Gender

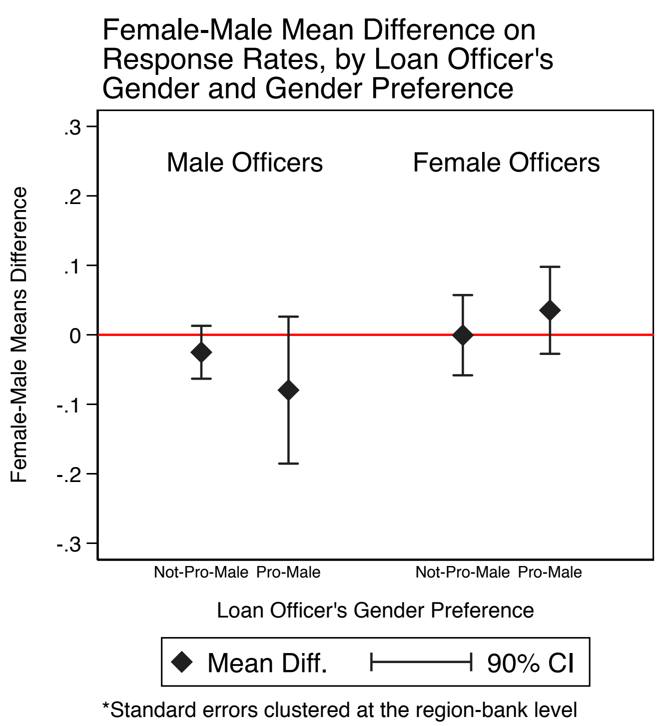
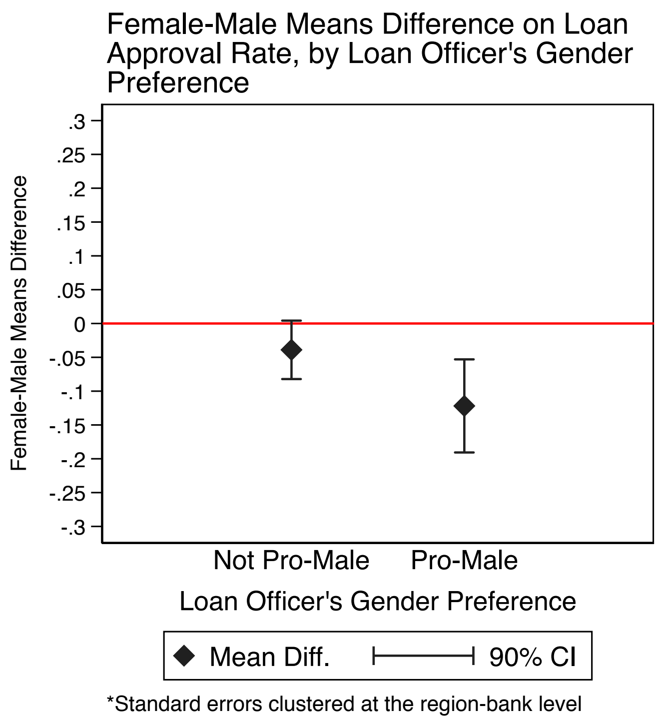


Figure A.VI. : Approval Rate, Officer's Gender, and Officer's Gender Preference

(a) Approval Rate and Gender Preference



(b) Approval Rate and Gender Preference, by Loan Officer's Gender

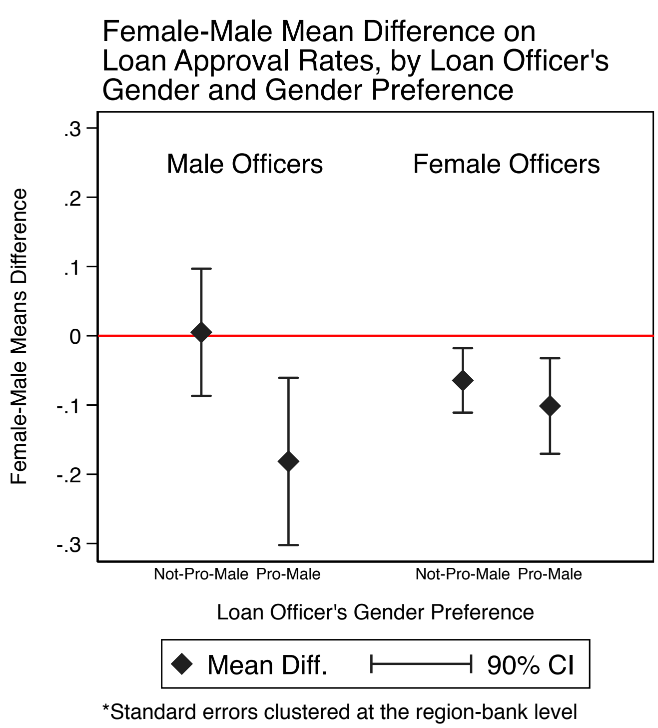
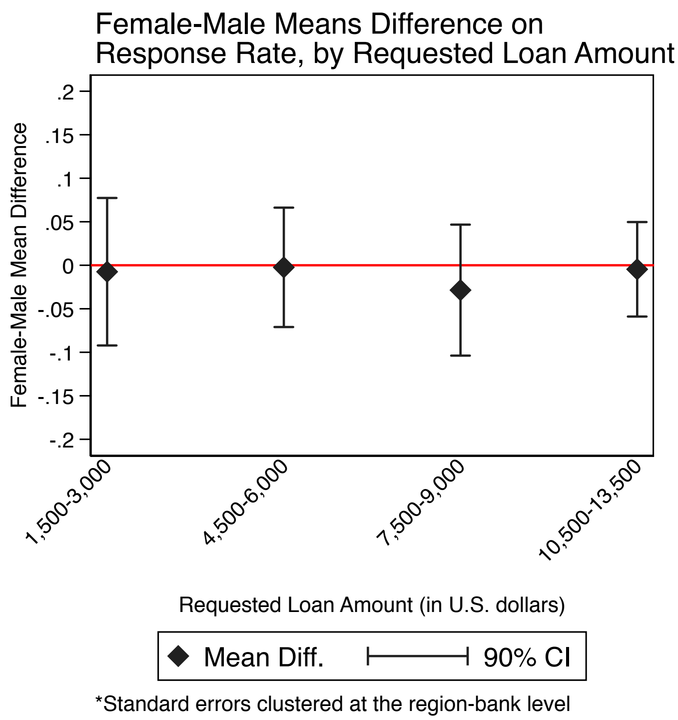
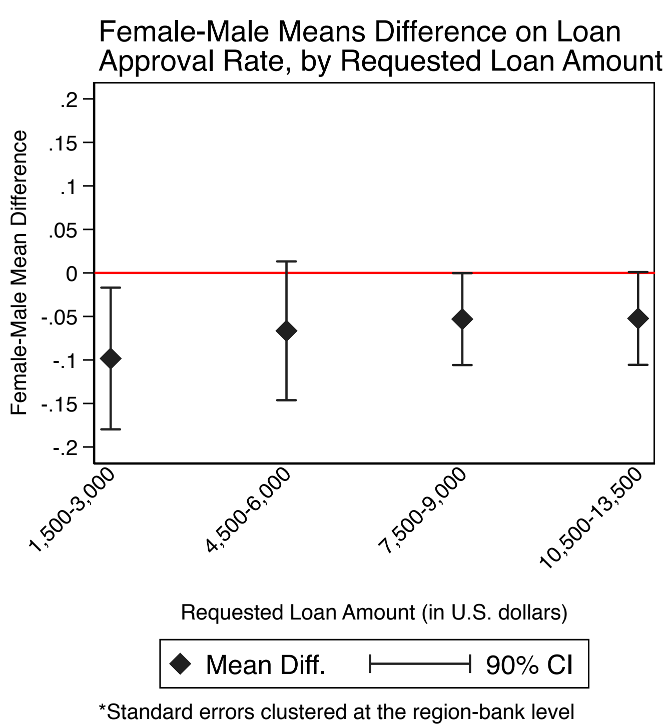


Figure A.VII. : Gender Discrimination across Requested Loan Amount

(a) Female-Male Mean Difference in Response Rate, by Requested Loan Amount



(b) Female-Male Mean Difference in Approval Rate, by Requested Loan Amount



Additional Results (FOR ONLINE PUBLICATION)

B1. Robustness Check for the Inclusion of Loan Officer Fixed Effects

A concern is that some officers received more than one application (non-singleton officers). This could increase the chances of suspicion effects, which in turn may disproportionately increase the rejection rate and eventually confound the identification of the gender discrimination parameter. That would be the case if, for instance, the loan officer found that the number of applications received in a given day was notoriously large compared to a normal day. Further, under credit rationing, this will naturally reduce the chances of approval of the experimental loan requests (saturation effect). Likewise, the loan officer may start to infer that the large number of applications means that some of the loan requests are false, again generating a reduction in the probability of response/approval of the experimental loan requests assigned to that loan officer (suspicion effect). However, for the sake of identifying gender discrimination effects, the latter is problematic only if such non-observable behaviors are correlated with the tester's gender associated with the loan requests.

A simple test for the presence of such confounding effects is to examine whether the gender effect derived from the sample of loan requests submitted to "non-singleton" officers is statistically different than the same effect derived from the full sample of loan requests. Indeed, as shown by Table B.I below, there is no evidence whatsoever of significant differences in the parameter estimates of gender effects across the two samples, i.e., we cannot reject the null hypothesis of no suspicion and/or saturation effects.

Even though loan officers were randomly assigned to loan requests, some unobservable characteristics of loan officers may still be correlated with the tester's gender, in which case our parameter estimates would be misleading. We examine this by testing whether the inclusion of officer-specific fixed effects in the "non-singleton" officers regression generate statistically significant changes in the effect size of the gender estimates (see columns 3 and 6). We do not reject the null hypothesis of no differences in the gender effect across the two specifications. Overall, this evidence gives support to our claim that the randomization scheme of loan requests to loan officers worked well and that officer-specific characteristics that are unobserved by the econometrician are likely to be well balanced across male and female applications.

Table B.I—: The Role of Officer Fixed Effects - OLS

	Loan Request was Responded (= 1)			Loan Request was Approved (= 1)		
	Full Sample	Only Non-Singleton	Only Non-Singleton	Full Sample	Only Non-Singleton	Only Non-Singleton
	(1)	(2)	(3)	(4)	(5)	(6)
Female (= 1)	-0.016	-0.001	-0.001	-0.064	-0.071	-0.095
	(0.023)	(0.026)	(0.030)	(0.017)	(0.021)	(0.032)
p-value $H_0 : \beta_{female} > 0$	0.247	0.488	0.483	0.000	0.001	0.002
p-value $H_0 : \beta_{Full} = \beta_{NS}$	0.665			0.796		
p-value $H_0 : \beta_{NS} = \beta_{NS-FE}$		0.999			0.531	
Observations	1,313	1,071	1,071	1,313	1,071	1,071
R^2	0.205	0.216	0.446	0.230	0.239	0.481
Loan Officer's Gender	√	√	×	√	√	×
Region-Bank F.E.	√	√	×	√	√	×
Information Treatment	√	√	×	√	√	×
Loan Officer's Baseline Covariates	√	√	×	√	√	×
Requested Loan Amount-Term F.E.	√	√	√	√	√	√
Week F.E.	√	√	√	√	√	√
Borrower's Baseline Covariates	√	√	√	√	√	√
Loan Officer F.E.	×	×	√	×	×	√

Note: Sample of analysis are submitted loan requests. The "Non-Singleton" sample includes only loan requests submitted to loan officers that received 2 requests or more. Following the standard procedure, when a control variable has a missing value, we impute a value equal to 0 and add a dummy variable equal to 1 for that observation, which indicates that the control variable was missing. Standard errors (in parentheses) are clustered at the region-bank level. We also report the p-value for a one-sided test of the null hypothesis that $\beta_{female} > 0$, the p-value for a two-sided test of the null hypothesis that β_{female} differs between the full sample and non-singleton sample regressions, and the p-value for a two-sided test of the null hypothesis that β_{female} differs between the non-singleton sample regressions with and without loan officer fixed effects.

B2. Heterogeneous Effects by Borrower's Baseline Income

Does gender discrimination in access to the consumer credit market increase as borrowers signal lower incomes? Table B.II shows evidence on this by comparing loan requests submitted by male and female individuals who are above and below the median monthly wage ($\approx$ \$1,000 U.S. dollars). In terms of response rate, there is no selective discrimination based on income, and female-male differentials do not change much when comparing individuals above and below the median income. If anything, the discrimination diminishes as borrowers become poorer, as the coefficient of the interaction is positive and weakly significant. However, this is small and not robust across models.

In terms of approval rates we find that individual income is negatively correlated with the probability of loan approval, as expected. In particular, poorer male borrowers are, on average, 22 percentage points less likely to get an approved loan compared to their richer counterparts. Graphically, this is illustrated in Appendix Figure B.I, where we find that the monthly wage distribution of rejected loans is markedly displaced to the left compared to the income distribution of testers whose loan applications were accepted. Still, there is no evidence that gender discrimination against female borrowers decreases with income as the interaction effect is never significant.

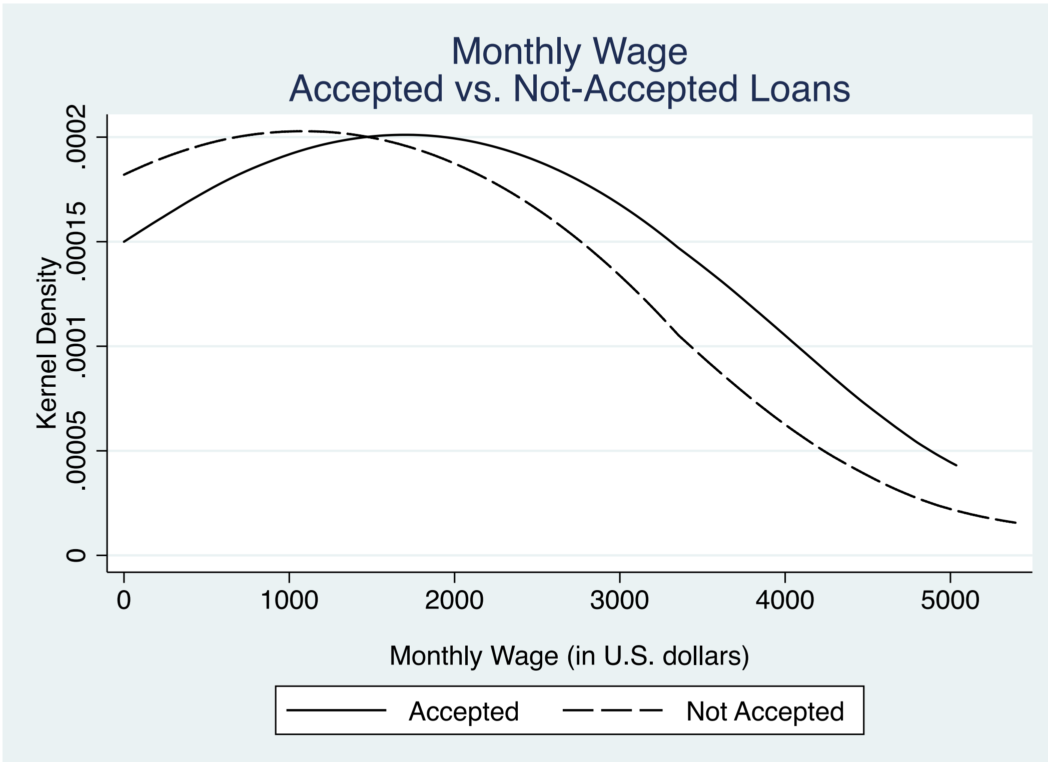
Table B.II—: Heterogeneous Effects by Individual's Monthly Wage - OLS

	Loan Request was Responded (= 1)		Loan Request was Approved (= 1)	
	(1)	(2)	(1)	(2)
Female (= 1)	-0.046	-0.043	-0.075	-0.083
	(0.037)	(0.038)	(0.034)	(0.036)
Monthly Wage < Mean (= 1)	0.014	0.022	-0.227	-0.222
	(0.029)	(0.029)	(0.047)	(0.053)
Female × (Monthly Wage < Mean)	0.051	0.048	0.019	0.022
	(0.039)	(0.096)	(0.052)	(0.051)
p-value $H_0 : \beta_{inter} > 0$	0.901	0.890	0.644	0.662
Observations	1,313	1,313	1,313	1,313
R^2	0.195	0.208	0.220	0.232
Mean Male (= 1) if Monthly Wage < Mean	0.881	0.881	0.277	0.277
Stratification Var.	√	√	√	√
Requested Loan Amount-Term F.E.	√	√	√	√
Information Treatment	√	√	√	√
Week F.E.	√	√	√	√
Borrower's Baseline Covariates	×	√	×	√
Loan Officer's Baseline Covariates	×	√	×	√

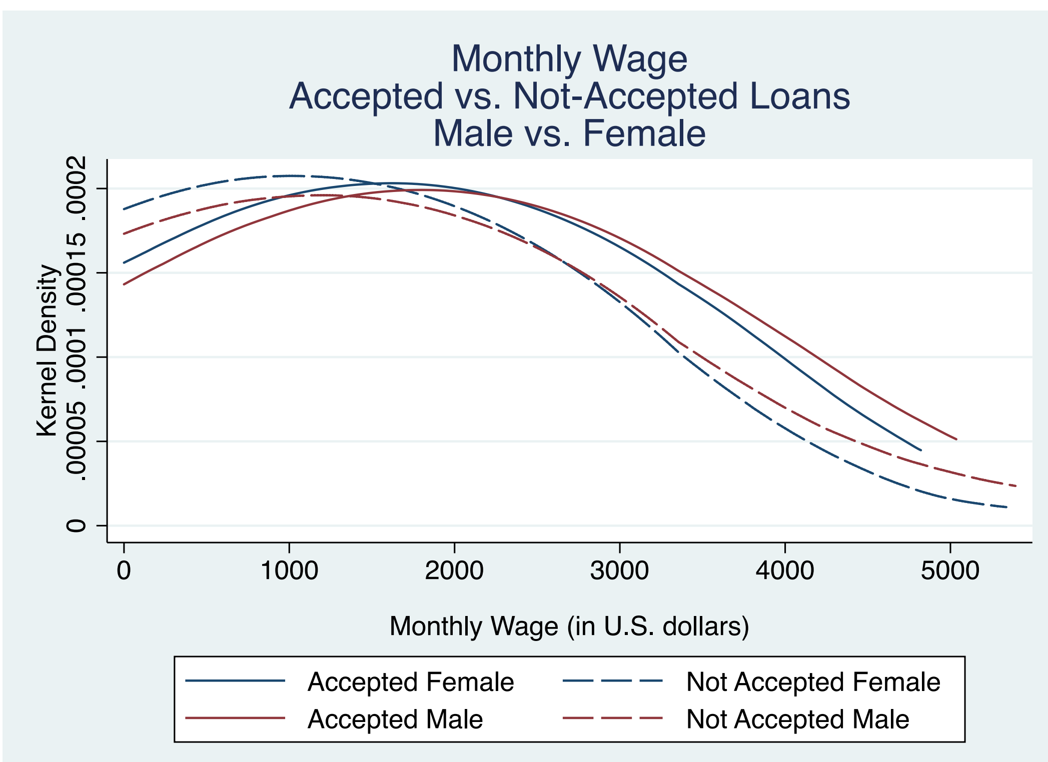
Note: Sample of analysis are submitted loan requests. The mean monthly income is 1,415 U.S. dollars. All regressions include stratification variables, i.e., a dummy for loan officer's gender, and 61 region-bank fixed effects; plus 8 dummies for the requested loan amount-term, a dummy that is equal to 1 if the loan officer received the information treatment (and 0 if not), and 22 week-time fixed effects. Model (2) also controls for baseline covariates at the borrower level, including age dummies (<29; 29−38), if married, if self-employed, and if a client of the assigned bank; as well as baseline covariates at the loan officer level, including age dummies (<29; 29−48), if has a higher education degree, and dummies for years of experience in the banking sector (<5; 6−10). Following the standard procedure, when a control variable has a missing value, we impute a value equal to 0 and add a dummy variable equal to 1 for that observation, which indicates that the control variable was missing. Standard errors (in parentheses) are clustered at the region-bank level. According to the pre-analysis plan, the p-value for a one-sided test of the null hypothesis that the interaction of Female dummy and Monthly Wage < Mean dummy is greater than zero is reported separately.

Figure B.I. : Distribution of Monthly Wage

(a) Accepted vs. Non-Accepted



(b) Accepted vs. Non-Accepted, by Gender



B3. Effects on the Intensive Margin

In this section we show estimates of gender discrimination at the intensive margin, i.e., the effects of gender on the conditions offered for approved loans, including the approved amount, approved term, loan payment, interest rate, and CAE rate[40]. Importantly, the analysis is conditioned on the loan application being approved, and thus sample selection will play a role when comparing credit conditions offered to men and women.

Table B.III shows the results. For the approved amount, we find that female testers get loans that are 94 USD larger, on average, than those of men. The effect is about 1.3% of the male mean but not significant. Likewise, the approved term and loan payment are also larger among women compared to men, but the effects are, again, very small and not statistically significant[41]. Finally, regarding the interest and CAE rates, the effects are almost null and insignificant.

Overall, our results indicate that the credit conditions offered to women are not statistically better compared to those offered to men. This result is in contrast to Alesina, Lotti and Mistrulli (2013) who use quasi-experimental data on loan contracts between banks and microfirms in Italy and find robust evidence that women pay more for credit than otherwise equally risky men. Likewise, Agier and Szafarz (2013) uses non-experimental methods to study gender discrimination in a micro-finance institution in Brazil and finds disparate treatment with regard to the credit conditions offered to men relative to women. Still, the same authors detect no gender bias in loan denial, a result that is potentially caused by the fact that the average loan size in their sample is extremely low, around 300 USD per loan, and thus the risk of default is plausibly lower.

[40]The disclosure of the CAE was implemented by law in 2012. Any bank offering a financial product has to detail not only the interest rate involved in the credit offer but also the CAE rate. This is expressed as a percentage, similar to APR. The CAE rate reduces costs associated with client-bank information asymmetries so that borrowers can compare the costs of different financial products across different banks with a standardized cost measure. The CAE regulation does not provide new information that was not previously available. Instead, it requires the information to be summarized in a salient and simple way and readily available for consumers in all credit markets.

[41]We further test for whether the loan is granted under the original terms requested by the tester and again find no evidence of gender discrimination. These results are available upon request.

In principle, our results suggest no discrimination against female borrowers on the intensive margin. However, such an interpretation would only be valid under the assumption that the screening on the extensive margin was equally tough for male and female applications, i.e., that the approved applications submitted by men were, on average, not more risky than the approved applications submitted by women. We examine the validity of this assumption by testing whether the distributions of risk associated with accepted male and female applications are statistically comparable or not. As a proxy for risk, we use the loan-income ratio (LIR), i.e., the ratio between the requested loan amount and the applicant's baseline income. We generally do not observe major differences between LIR distributions across gender, and this is the case for the full sample as well as for the sample of accepted loan requests (see Figure B.II).

Finally, note that while there is no observational evidence of discrimination against women on the intensive margin, the LIR exercise reveals that the market for installment loans is inefficient on the extensive margin. Following Becker (1957), provided that loan officers are unbiased and that the expected differences across male and female applications are only due to statistical discrimination, then the profitability of loan requests submitted by marginal applicants of each gender group should be the same, i.e., identical male and female loan applications that are at the limit of the approval criteria should have the same chances of being approved. Indeed, as the evidence on LIR distributions shows, female applicants are not riskier than their male counterparts in our sample. Nonetheless, gender differences in the acceptance rate are huge, suggesting that the approval criteria imposed for women were on average tougher than those for men, reinforcing the taste-based hypothesis.

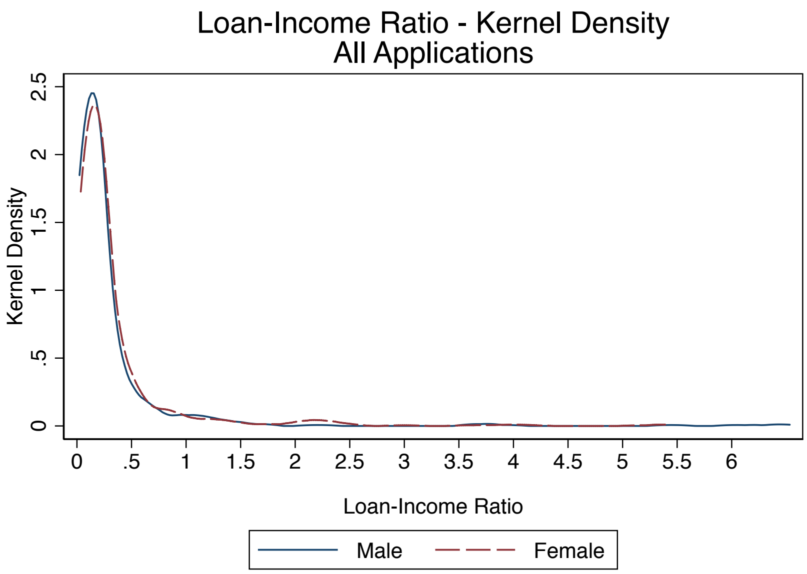
Table B.III—: Intensive Margin: Approved Loans - OLS

	Approved Amount (U.S. dollars)		Approved Term (months)		Loan Payment (U.S. dollars)		Interest Rate		CAE Rate	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Female (= 1)	80	94	0.455	0.578	2.362	3.024	0.003	0.015	-0.033	-0.149
	(172)	(164)	(0.466)	(0.539)	(4.910)	(4.883)	(0.039)	(0.038)	(0.596)	(0.565)
p-value $H_0 : \beta_{Female} > 0$	0.680	0.716	0.833	0.855	0.683	0.730				
p-value $H_0 : \beta_{Female} \leq 0$							0.473	0.349	0.522	0.603
Observations	408	408	408	408	408	408	408	408	341	341
R^2	0.889	0.898	0.915	0.917	0.698	0.708	0.480	0.519	0.550	0.594
Mean Male (= 1)	6,995	6,995	34.743	34.743	265	265	1.387	1.387	22.282	22.282
Stratification Var.	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Loan Amount-Term F.E.	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Information Treatment	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Week F.E.	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Borrower's Baseline Cov.	×	✓	×	✓	×	✓	×	✓	×	✓
Loan Off. Baseline Cov.	×	✓	×	✓	×	✓	×	✓	×	✓

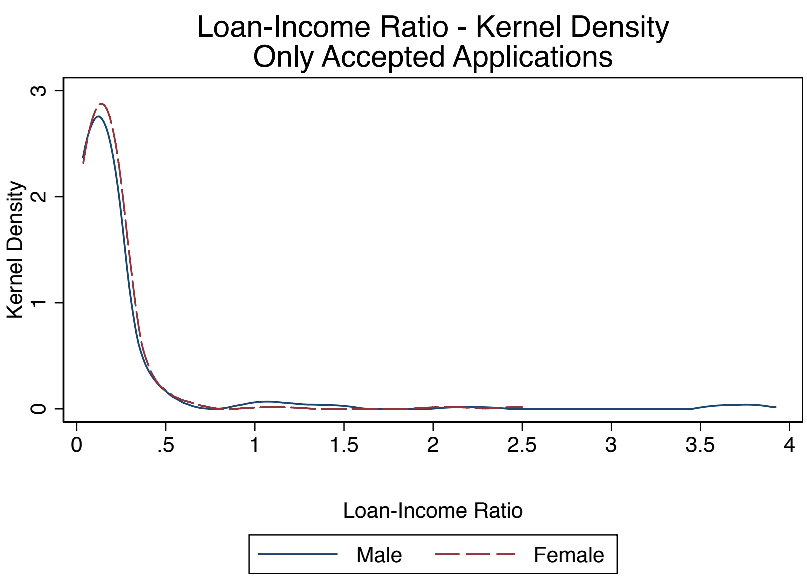
Note: Sample of analysis are approved loan requests. Monetary figures in U.S. dollars of July, 2018. All regressions include stratification variables, i.e., a dummy for loan officer's gender, and 61 region-bank fixed effects; plus 8 dummies for the requested loan amount-term, a dummy that is equal to 1 if the loan officer received the information treatment (and 0 if not), and 22 week-time fixed effects. Model (2) also controls for baseline covariates at the borrower level, including age dummies (<29; 29−38), if married, monthly wage dummies (600−1,200 USD; >1,200 USD), if self-employed, and if a client of the assigned bank; as well as baseline covariates at the loan officer level, including age dummies (<29; 29−48), if has a higher education degree, and dummies by years of experience in the banking sector (<5; 6−10). Following the standard procedure, when a control variable has a missing value, we impute a value equal to 0 and add a dummy variable equal to 1 for that observation, which indicates that the control variable was missing. Standard errors (in parentheses) are clustered at the region-bank level. According to the pre-analysis plan, the p-value for a one-sided test of the null hypotheses that $H_0 : \beta_{Female} > 0$ or $H_0 : \beta_{Female} \leq 0$ are reported separately.

Figure B.II. : Loan-Income Ratio Distribution

(a) All Applications



(b) Only Accepted Applications



The Loan-Income ratio is calculated as LIR= $\frac{RLA/RLT}{MonthlyIncome}$, with RLA the Requested Loan Amount and RLT the Requested Loan Term (in months). Lending is riskier the larger is the LIR. For non-attriter loan requests, the median LIR is 0.156, with an interquartile range of [0.104 ; 0.285].

B4. On the Economic Costs of Gender Discrimination

Our empirical evidence suggests that gender differences in approval rates are attributable to taste-based discrimination on the part of loan officers. This could potentially harm bank profitability. We examine the associated forgone profits (FP) by calculating the net present value (NPV) of the additional profits that banks would have obtained if, under no credit rationing, loan officers did not discriminate based on applicant gender.

The latter requires identifying female applications that were rejected for discriminatory reasons, for which we follow a propensity score matching strategy. We first estimate the predicted probability of getting an approved loan for each male and female applicant in the non-attriters sample, i.e., those for which we have information on whether the application was approved or rejected[42]. We then group the observations by each of the 9 possible combinations of loan amount and term considered in our experiment, and use the estimated *pscore* and the k-nearest neighbors matching metric to match each rejected application submitted by a female with the nearest approved application submitted by a male within each amount-term category.

Our experimental estimates of gender discrimination indicate that roughly 9% of the rejected applications submitted by women would have been approved had they been submitted by men[43]. Then, within each amount-term category, we identify the applications rejected due to gender discrimination as those above the 91^{th} percentile of the *pscore* distribution of rejected applications submitted by women. Note that within each amount-term category, we are matching every rejected application submitted by a female applicant with the nearest approved application submitted by a male, and thus the interest rate offered to the matched

[42]The specification of the *pscore* regression includes the full set of covariates used in the regression model (2) in Section IV.

[43]This is calculated as follows. Of 766 loan requests submitted by female testers, 549 were rejected, which represents a rejection rate of 71.7%. According to Table 1, the effect size of discrimination against female borrowers is 0.066 percentage points, which is equivalent to 50 out of 766 loan requests submitted by women, and those 50 loan requests represent 9.1% of the 549 rejected applications.

male application serves as the counterfactual interest rate that a female applicant would have obtained had her application been approved. Hence, we use this counterfactual to estimate the forgone profits.

Specifically, for a one-year loan, the forgone profits associated to each application rejected due to gender discrimination can be calculated as $FP_0 = \sum_{i=1}^{n} \hat{p_i} L_i(r_i - \phi) + (1 - \hat{p_i}) L_i(r_i - \phi)$, where p_i stands for the probability of repayment, L_i is the loan amount, and $r_i - \phi$ is the difference between the annual interest rate that the bank would have charged in case of approval and the opportunity cost of lending. Likewise, for a loan term taking t years, the NPV can be expressed as $NPV_{FP} = FP_0 + \sum_{t=1}^{T-1} \frac{1}{(1+\phi)^t} \sum_{i=1}^{n} \hat{p_i} L_i t_i(r_i - \phi) + (1 - \hat{p_i}) L_i t_i(r_i - \phi)$.

We know L_i, t, and r_i, which are attached to each approved application. $\hat{p_i}$ is provided by SBIF through data containing the universe of installment credit transactions in 2018. We then calculate the proportion of assigned loans to women that are not declared as >90 days Non-Performing Loans, i.e., loans not declared in default by Chilean regulation. We do this for each of the 9 loan types to elicit the distribution of $\hat{p_i}$ across loan types. Finally, we assume ϕ is the annual inter-bank interest rate (TIB) suggested by the Central Bank of Chile for July 2019.

Following this procedure, we estimate that the median NPV of forgone profits associated to applications rejected due to gender discrimination amounts to 1,785 USD or 23% of the median loan request size (≈7,500 USD). We extrapolate these results by performing a back-of-the-envelope calculation of the forgone profits at the industry level. In 2018, official records provided by SBIF (2018) indicate that non-client female applicants between 25 and 35 years old submitted 65,000 loan requests with amounts between $1,500-13,500 USD, of which 55% were rejected. Our experimental estimates of gender discrimination indicate that roughly 9% of the rejected applications submitted by women would have been approved had they been submitted by men, or about 65,000×0.55×0.09≈3,200 applications. Therefore, considering the median forgone profit of $1,785 USD per discriminated application, gender discrimination had an approximate cost for the industry of 1,785$\times$3,200$\approx$US$5.8 million dollars per year, which is equivalent to the annual cost of hiring 4% of the officer labor force in the Chilean banking system[44]. Note that our back-of-the-envelope calculation is only considering rejected loan requests submitted by applicants aged 25-35 for amounts between $1,500-13,500 USD (interquartile range). Indeed, provided that gender discrimination is spread across all age cohorts as well as across all loan amounts, the potential costs at the industry level are likely to be much larger.

Note that the previous calculation is only valid if we assume no credit rationing in the Chilean banking system, and the official statistics support this assumption. Official balance records published yearly by SBIF indicate that, by 2018, the owner's withdrawal in the Chilean commercial banking system was $1,830$ million USD, or 50% of net income. Assuming that owners could invest the retained earnings in loans, the total amount of credit that was not lent to female applicants due to gender discrimination is equivalent to just 0.5% of owner's withdrawal. A more stringent assumption is that banks invest the retained earnings proportionally to the distribution of installment loans, which is equivalent to 9% of owner's withdrawal. Still, in this case the total amount of loans not lent to women correspond to just 5.6% of earnings paid to shareholders. Therefore, we discard the idea that demand is higher than the resources available in the Chilean commercial banking system.

[44]There are approximately 8,500 loan officers in Chile, earning on average US$23,000 per year.