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IS THERE A GENDER BIAS IN LENDING?

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Sex and Credit:
Is There a Gender Bias in Lending?

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Abstract

We exploit the quasi-random assignment of borrowers to loan officers using data from a large Albanian lender to show that own-gender preferences affect both credit supply and demand. Borrowers matched to officers of the opposite gender are less likely to return for a second loan. The effect is larger when officers have little prior exposure to borrowers of the other gender and when they have more discretion to act on their gender beliefs, as proxied by financial market competition and branch size. We examine one channel of influence, loan conditionality. Borrowers assigned to opposite-sex officers pay higher interest rates and receive lower loan amounts, but do not experience higher arrears. Together our results imply that own-gender preferences in the credit market can have substantial welfare effects.

JEL Classification: G21, G32, J16.
Keywords: Group identity, gender, credit supply, credit demand, loan officers.

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1 Introduction

Group identity in the form of family, ethnicity, and gender is a powerful predictor of social preferences (Akerlof and Kranton, 2000; Chen and Li, 2009; Benjamin et al., 2010). In particular, people generally favor in-group over out-group members. Favoritism based on, for example, gender identity can lead to inefficient transactions and/or lost opportunities. However, gender similarity may also entail trust, reciprocity, and efficiency due to shared norms and understandings. In this paper, we examine one important form of group identity, gender, and the consequences of own-gender preferences for outcomes in the credit market.

Credit transactions rely heavily on the interaction between bank officers and borrowers. Microcredit is a case in point, with most clients being small and opaque, leaving the lending decision at the judgment of the loan officer. [1] If gender shapes business operations we need to understand how key determinants of officer behavior, such as human capital and officers' discretion to act on their beliefs, interact with gender. For example, how does prior experience with opposite-sex borrowers affect performance and do officers behave differently if they are more scrutinized? In addition, officers' decisions may not only impact loan conditionality but also subsequent demand for credit. Recent work shows that poor borrowers are sensitive to small changes in interest rates (Karlan and Zinman, 2008). If a gender bias in the relationship between officers and borrowers results in higher interest rates or smaller loan amounts, this can have negative repercussions not only for access to and cost of credit, but also for take up of loans by poor borrowers.

Using a large dataset of loan transactions from a commercial microlender in Albania, we investigate whether the officer-borrower gender match influences the likelihood that borrowers return to the lender for additional credit. To understand if important officer attributes interact with gender identity, we also examine if prior exposure to opposite-sex borrowers and officers' ability to act on their gender beliefs affect demand for more credit. In addition, we explore a possible channel that may explain changes in demand, loan conditionality, by studying the impact of the officer-borrower gender assignment on interest rates and approved loan amounts. Finally, the analysis allows us to test if the bias is taste based or related to lack of experience and whether it leads to more efficient loan transactions.

[1] If officers and borrowers share gender identity, this could improve efficiency through a better understanding of the clients' particular circumstances. For example, female loan officers may better appreciate the ability of female entrepreneurs in terms of completing their project and/or repaying the debt. Conversely, a gender bias can also generate unfair loan conditionality.

Estimating the effect of own-gender preferences presents two main challenges. First, if male or female borrowers with certain characteristics are more likely to be assigned to the same or opposite-sex loan officers, the true effect of loan officer gender would be biased. Second, if unobserved borrower traits are correlated with borrower gender, and if these can be observed by the loan officers but not by the researchers, it is not clear whether a significant coefficient on gender is due to a loan officer bias or the unobservable traits.

We address these issues by exploiting a quasi-random component of the institutional setting: the fact that first-time borrowers are arbitrarily assigned to their respective loan officer, with the sector of activity and year of application being the only factors driving the match with a specific officer. Conditional on sector and year, the random assignment of borrowers to officers ensures that unobservable borrower characteristics are the same across all officers, regardless of officer gender. In particular, we compare the difference in credit market outcomes for male and female borrowers obtaining loans from male loan officers to the difference between male and female borrowers obtaining loans from female loan officers.

We find that the random assignment of first-time borrowers to opposite-sex loan officers has a significant impact on demand for credit. Borrowers matched with officers of the opposite gender are 11 percent less likely to apply for a second loan with the lender. To examine if gender-specific human capital traits matter, we explore the fact that our setting generates experimental variation in officers' experience with first-time borrowers of the opposite sex. We show that the effect originates with borrowers whose officers have below-median experience of the other gender. This indicates that officers learn about the other gender through professional experience and suggests that the bias does not stem from pure prejudice.

To investigate if officers' degree of discretion to act on their gender preferences is important, we use variation in financial market competition and in the number of officers employed in a given branch across bank branches and over time. The idea is as follows. In instances when it becomes costly for officers to express their gender beliefs, the incentives will be stronger to suppress the bias.[2] More competition offers borrowers better outside options inducing them to leave the bank if they are biased against. This lowers profits and prompts the lender to monitor loan officers more carefully to detect mistreatment of their clients. Along the same lines, it may be easier to replace a given officer in large branches with

[2] The reasoning resembles the argument developed in Parsons et al. (2011), who show that an own-race bias associated with baseball referees is stronger in situations where it is less likely that the bias is discovered, in their context, in baseball arenas with cameras that document the decisions taken by the referees.

many employees, leaving officers less discretion of indulging their own-gender preferences. The data confirm these predictions: the effect of the gender mismatch on credit demand is stronger in areas where the competition from other financial institutions is weaker or where the branch size is smaller. The analysis further shows that officers' lack of opposite-sex experience and their degree of discretion are complements: the negative impact on demand for additional credit is most severe when officers have little experience with borrowers of the other gender and work in small branches or in areas with little outside competition. As an example, first-time borrowers are half as likely to apply for a second loan if they are matched with opposite-sex officers who have little prior experience of the other gender *and* work in smaller branches.

Next we study differences in loan conditionality to explore one channel through which an own-gender bias can affect credit demand. First-time borrowers assigned to officers of the other gender pay, on average, 35 basis points higher interest rates compared to borrowers assigned to officers of the same gender. Again, these effects are more pronounced when officers have less opposite-sex experience and/or more discretion (weaker outside competition and smaller branches). Borrowers matched with officers with less exposure to the other gender and/or a large degree of discretion also receive between 4 to 24 percent lower loan amounts.

Establishing that officer exposure to opposite-sex borrowers matters helps us rule out the existence of pure prejudice. However, it is not clear whether the bias we identify stems from a knowledge gap that leads officers to engage in more efficient transactions with own-gender borrowers at first—or if it reflects an initial taste bias. To test for this, we use data on the likelihood that borrowers enter into arrears during the loan. If information asymmetries between officers and borrowers were important, the variation observed in interest rates or loan amounts should be reflected in different arrear outcomes. However, we find that arrears are independent of the officer-borrower gender assignment, suggesting that the bias is inefficient.

Taken together, the results indicate that loan officers' gender preferences can have non-trivial welfare effects for consumers (higher interest rates, smaller loan amounts, and lower demand) and providers of credit (lower long-run profits through diminished demand in the opposite-gender match). While our identification strategy bars us from making definite claims as to whether the bias stems from male or female loan officers favoring borrowers of their own gender, or disfavoring those of the other gender, we provide some suggestive evidence in support of own-gender preferences.[3]

This paper speaks to several literatures. First, using experimental field data from a South African lender, where the interest rate offers were randomized, Karlan and Zinman (2008) show that clients are sensitive to interest rate changes, in particular to increases in price above the lender's standard rates. In light of the interest rate differential identified in our paper, Karlan and Zinman's finding suggests that a gender bias-induced price gap may be one important channel affecting credit demand.

Second, while there are studies looking at own-race preferences in police behavior (Donohue and Levitt, 2001), in judicial sentencing (Welch et al., 1988) in the workplace (Stoll et al., 2004), and in sports (Price and Wolfers, 2010; Parsons et al., 2011), our paper is the first to gauge the existence of an own-gender bias in lending. There is also a broader literature documenting biases in credit markets, predominately using US data on either mortgage (Munnell et al., 1996; Berkovec et al., 1998; Ladd, 1998; Ross and Yinger, 2002; Han, 2004) or small business lending (Cavalluzzo and Cavalluzzo, 1998; Blanchflower et al., 2003, Blanchard et al., 2008; Bellucci et al., 2010). More closely related, Fisman et al. (2012) report that shared ethnicity and religion between loan officers and borrowers in India improve credit allocation. Our paper differs from Fisman et al. by focusing on gender and on the combination of credit supply and demand, as well as in tracing the importance of prior exposure to opposite-sex borrowers.

With the exception of Fisman et al., the above studies on a minority/gender bias in the credit market are based on correlations that do not control for all the characteristics that lenders observe when setting the contract terms. As a consequence, any measured differences in outcomes could be attributed to these factors unobserved by the researcher. Our data and setting provide an opportunity to test for a gender bias more rigorously. Moreover, previous work does not combine a supply and a demand-side analysis.

Third, the paper relates to research documenting the impact of exposure to members of another group (Boisjoly et al., 2006; Beaman et al., 2009). While our data bar us from documenting changes in beliefs (unlike Boisjoly et al. and Beaman et al.), the results suggest that experience with the opposite gender has important economic implications.

[3] In particular, running separate regressions at the loan officer level shows that the majority of male loan officers have a greater propensity to charge higher interest rates when lending to female borrowers than the majority of female loan officers and vice versa. Studying the demand for a second loan provides similar results.

Fourth, we connect to studies examining Becker's (1957) hypothesis on the link between discrimination and competition showing that US bank branch deregulations tightened the wage gap in the financial industry between male and female workers (Black and Strahan, 2001) and between white and black employees (Levine et al., 2011). We add to this literature by quantifying how financial market competition also reduces the impact of loan officers' own-gender bias.

Our findings inform empirical work examining poor peoples' barriers to credit (Banerjee and Duflo, 2005; Karlan and Zinman, 2009). The setting of the current study, a for-profit lender in Albania, extending loans under individual liability also fits the pattern of the second generation of microcredit (Armendáriz and Morduch, 2005; Karlan and Morduch, 2009) which has evolved in the direction of more traditional retail and small business lending.

Finally, the paper relates to a small literature studying the importance of loan officers in lending stressing long-term relationships, compensation schemes, officer rotation, and officer gender for loan performance (Hertzberg et al., 2010; Agarwal and Ben-David, 2011; Drexler and Schoar, 2011; Beck et al., 2012; Cole et al., 2012). We complement these studies by documenting the existence of an own-gender bias and in emphasizing the importance of loan officers' prior exposure to opposite-sex borrowers.

In the next section we provide institutional background information about our lender and the loan process, outline our methodology, and describe the data. Section three presents our findings on the relationship between own-gender preferences and demand for a second loan, while section four discusses results for the relationship between own-gender preferences and loan conditionality. Section five investigates whether the bias is efficient while section six explores if the bias is more pronounced for male or female officers. Section seven concludes.

2 Data and identification strategy

This section describes our data, provides background information about the lender, sample composition and descriptive statistics, and evidence on the validity of our identification strategy.

2.1 Sources of data and institutional background information of the lender

We rely on information from three sets of data. The loan-level data come from a large for-profit commercial lender serving individuals and small- and medium-sized enterprises in Albania, the population and the financial market competition data were provided by the Bank of Albania, and the data on household income is taken from the World Bank's Living Standards Measurement Study (LSMS).

The loan-transaction dataset includes nearly 7,300 loans given by a commercial lender over the period January 1996 to December 2006. The data also contain information on 279 loan officers and cover all 21 branches of the bank. While the lender clearly focuses on the low-income and microenterprise segment, financial sustainability and therefore profitability is its primary goal. The financial market data include geographical information about the universe of Albania's formally registered banks and their respective branches at the county level (prefekturë) for the period 2004-2006.[4] The population statistics report the total number of people living in each county during the same period. Finally, the LSMS data on monthly wage payments draw on a series of nationally representative household surveys conducted in 2002-2005 containing 1,797-3,638 households.[5]

Loan officers working for the lender have discretion on the rejection and approval of a loan application, as well as setting the interest rate and other loan conditions including the loan amount. The officer that originates a certain loan is also in charge of monitoring the repayment behavior of the borrower. If a loan is in arrears for more than 30 days, the officer intensifies monitoring, for instance, by calling or visiting the borrower to inquire about the reasons for repayment delay. When a loan is in arrears for more than 60 days, it is transferred to a special loan recovery department and, thus, a new loan officer. We can therefore follow the relationship between a borrower and an officer from approval over loan condition setting to its performance in terms of arrears up to 60 days, but not beyond that point as we lack information about the gender of the officers working in the loan recovery department.

Assignment of borrowers to officers is based on the availability of officers in the respective branch when the borrower arrives.[6] Specifically, first-time borrowers cannot choose a bank officer, barring an assignment based on any observable (for example, gender) or unobservable characteristic (for example, ability). Loan officers, however, may specialize in certain business sectors. For instance, it is more likely that a borrower working in the transportation business ends up with an officer with previous experience in handling borrowers from this sector. Since male and female officers or borrowers potentially specialize

[4] The information was obtained through correspondence with the Bank of Albania.
[5] The household survey data are available at the World Bank's Living Standard Measurement Study web page: http://econ.worldbank.org/WBSITE/EXTERNAL/EXTDEC/EXTRESEARCH/EXTLSMS/0,,contentMDK:21369062~pagePK:64168445~piPK:64168309~theSitePK:3358997~isCURL:Y,00.html
[6] All loan officers work full time so it does not matter which day of the week a borrower arrives.

in certain sectors, this needs to be accounted for. Below we outline our identification strategy and how we account for the potential loan officer and borrower specialization.

2.2 Sample composition and descriptive statistics

When analyzing treatment differences we focus on the following four outcomes: (i) the likelihood that a borrower applies for a second loan with the lender; (ii) the annual interest rate paid; (iii) the loan amount approved; and (iv) the likelihood of going into arrears more than 30 days at any point during the loan cycle.[7] For our regression analysis, we restrict the data in two ways. First, we focus on first-time borrowers. By studying the first loan application submitted by each borrower, we assume that borrowers and loan officers had neither a previous business relationship nor any knowledge of each other. In the case of repeat borrowers, loan officers have historic information, which they can use when granting and monitoring the loan and deciding on loan conditionality. In addition, the fact that there is a gender bias in the demand for a second loan introduces selection in the sample of repeat borrowers. Focusing on the first loan by each loan applicant yields the cleanest test of a possible gender bias. Second, we drop loans with missing gender information. For that purpose, we exclude loans by borrowers classified as legal entities in the database as we lack information on borrower sex. Together, this yields a dataset of 7,272 loan transactions.

In order to assess the gender bias across our four outcome variables, we work with two different samples. Specifically, we use the full sample of first-time borrowers as outlined above to study the relationship between gender assignment and loan conditionality: interest rate and loan amount approved (henceforth, the "loan conditionality sample"). To investigate the relationship between loan officer assignments and arrears on the first loan and demand for a second loan, we work with a smaller sample that accounts for the problem of right-censoring, that is, the fact that borrowers might not come back to the bank because the maturity of their first loan lies beyond the end of our sample period. Hence, we compute the average time it takes until a second loan application of a first-time borrower is posted and end our sample period on December 31, 2006, less this average time of 198 days for the test. This reduces the sample size from 7,272 to 4,589 observations (henceforth, the "credit-demand sample").

[7] While we have information on rejected loan applications, almost all of the first-time applicants are granted a loan, yielding little meaningful variation to be exploited. When estimating cross-gender differences in the approval regression, however, we do not find any evidence of a gender bias.

Table 1 presents the descriptive statistics for the credit-demand sample, while Appendix Table A1 displays the same statistics for the (larger) loan-conditionality sample. The descriptive statistics in Table 1 shows that 66 percent of the first-time borrowers applied for a second loan and 5 percent of all loans fell into arrears for more than 30 days. 18 percent of the borrowers are female, while 62 percent of the loan officers are female.[8] About 57 percent of the loans in the credit-demand sample are managed by an opposite-sex officer.[9] 61 percent among the male borrowers have a female officer and 36 percent of the female borrowers have a male officer. Loan officers are, on average, 25 years old, with male loan officers being two years older, which can be explained by compulsory military service for males in Albania. For most loan officers, this is the first formal job after college.[10] On average, loan officers have processed 19 loans with borrowers of the opposite gender. Borrowers have a mean age of 41 with no significant difference between male and female borrowers or borrowers assigned to male or female loan officers. 87 percent of the borrowers are married and own assets of value 24,361 US Dollars (USD). Almost all loans require chattel collateral, while only 14 percent come with mortgage and 23 percent with a personal guarantee. 73 percent of all borrowers work in construction, while 12 percent work in production and 15 percent in transportation. 29 percent of the loans are used for fixed asset purchase, 37 percent for housing improvement, 24 percent for consumption, and 10 percent for working capital.

The descriptive statistics in the loan-conditionality sample, as reported in Appendix Table A1, show that the average interest rate is 14 percent, while the average loan size is 2,752 USD. All other variables are very similar to the credit-demand sample.

2.3 Identification strategy

To study the impact of the interaction between officer and borrower gender on borrower outcomes, we exploit the essentially random assignment of first-time borrowers to loan officers. In a framework analogous to a difference-in-differences estimation, we compare the difference in outcomes (demand for a second loan, interest rate, loan amount, and arrear probability) for male and female borrowers obtaining a loan from a male officer to the difference between male and female borrowers obtaining a loan from a female officer.

[8] The relatively high share of female loan officers working for the bank is in line with labor market statistics published by the Statistical Institute of Albania (2007) and the recent Census, both showing that females are slightly overrepresented in financial institutions and in jobs similar to the job of a loan officer.
[9] This is simply $0.82\times0.61 + 0.18\times0.36$.
[10] Personal communication with the lender.

The identifying assumption is that the difference between male and female borrowers screened and monitored by male loan officers is not significantly different from the difference between male and female borrowers screened and monitored by female loan officers, controlling for the respective sector of activity and year of application. Hence, while male and female borrowers may differ systematically due to any number of unobservable factors, identification of the gender effect will be robust as long as this difference is constant across male and female loan officers.[11] To address the possibility that it is not, we take two additional steps. First, we control for loan-officer fixed effects, allowing us to compare male and female borrowers independent of the specific (time-invariant) characteristics of a given loan officer (besides gender). Second, we also include a large number of observable contract-related, borrower, bank branch, and (time-varying) loan officer characteristics.

To formally gauge whether borrower assignment is random with respect to officer gender, we use two complementary tests. First, we verify if male relative to female borrowers vary in their characteristics depending on whether they are matched with an officer of their own or the opposite gender. If the identifying assumption is correct, there should be no statistically significant difference-in-differences observed between male and female borrowers ending up with a male or female officer. We utilize the following regression:

(1) $$y_{ijts} = \beta gb_i gl_j + gb_i + gl_j + \mu_t + \phi_s + \varepsilon_{ijts},$$

where y_{ijts} is one of the relevant characteristics of borrower *i* contracting with loan officer *j* in year *t* in sector *s*, gb_i is a borrower-gender dummy taking the value 1 for female borrowers, gl_j is an officer-gender dummy taking the value 1 for male loan officers, μ_t is a year dummy, and ϕ_s is a sector dummy. The coefficient β indicates whether there is a difference between male and female borrowers screened and monitored by male relative to female officers. The time fixed effects (μ) account for the fact that the gender composition is idiosyncratic only within each year while the sector fixed effects (ϕ) control for any gender-specific business sector specialization. The assumption is that $Cov(gb_i gl_j, u|\tilde{z}) = 0$, where u is any other determinant of the outcome of interest y_{ijts} and $\tilde{z}$ is the vector of the relevant fixed effects. Specifically, we test for differences in socio-demographic borrower information (civil status, age), loan terms apart from the interest rate (applied loan size in USD, applied loan maturity

[11] That is, we only require that the unobservable characteristics are the same in the two differences. As an indirect test of this assumption, we also show (Table 2) that the difference-in-differences in the observable traits are not significant.

in days, availability of a personal guarantee or of mortgage or chattel collateral), the loan usage (working capital, fixed assets, a combination of the two, housing improvement, consumption, and "other"), and information on the financial status of the borrower's business (total assets in USD). We cluster the standard errors ε_{ijts} at the branch-sector-year level as borrowers in a given year, sector, and branch are likely to share background characteristics as well as be exposed to the same loan officer and environment. We present the results for the credit-demand sample in Table 2 and relegate the results for the loan-conditionality sample to the Appendix (Table A2).

Table 2 shows that there are no systematic differences in the observable borrower characteristics in the opposite gender match of borrowers and loan officers. Specifically, columns (3) and (6) display the t-statistic of the relative difference across male and female borrowers for male and female officers, respectively. Finally, column (7) reports the t-statistic of the difference-in-differences estimate. While we find significant differences between male and female borrowers within the sub-groups of female and male loan officers, none of the differences in column (7) are significant. (The same holds true for Table A2.)

In a second test, we regress loan officer gender on borrower gender to gauge whether there is systematic sorting of borrowers of a certain gender to loan officers of the same gender conditioning on time and on sector fixed effects. Specifically, we estimate

(2) $$gl_j = gb_i + \mu_t + \phi_s + \varepsilon_{ijts},$$

where the variables are defined in an analogous manner to equation (1). The assumption is that $Cov(gl_j, gb_i \mid z) = 0$, where z is a vector of the relevant fixed effects. Table 3 documents the results of this regression for both the credit demand (Panel A) and the loan-conditionality sample (Panel B).

The results in Table 3 show that conditional on time and on business sector, borrower gender cannot predict loan officer gender. In the first column, we estimate equation (2) without any fixed effects. In column (2) we add time dummies and in column (3) sector dummies. Panel A displays a statistically insignificant and small effect of borrower gender on loan officer gender across all three specifications. In Panel B, once we account for time fixed effects and sector specialization, borrower gender cannot explain loan officer gender. Together, the results of the two tests (Tables 2, 3, and A2) lend credibility to our identification strategy.

2.4 Main specification

To investigate whether there is an own-gender bias in lending, we use OLS to estimate the following specification

(3) $$O_{ijts} = \alpha_0 + \beta gb_i gl_j + gb_i + gl_j + \rho_j + \mu_t + \phi_s + \eta_b + \mathbf{x}_{ijt} + gb_i X_{it} + \varepsilon_{ijts},$$

where O is the outcome of interest (demand for a second loan, interest rate charged, loan amount approved, or arrear probability), $\rho, \mu, \phi,$ and η are loan officer, time, sector, and branch fixed effects, respectively.[12] The coefficient β estimates the impact of opposite-sex officers on credit market outcomes (relative to own-gender officers). Put differently, it measures the differential effect of a female (male) borrower paired with a male (female) officer compared to a female (male) borrower matched with a female (male) officer. The parameter $\mathbf{x}_{ijt}$ is a vector that includes borrower traits (those of Table 2) and loan officer traits (age; number of loans processed with opposite-sex borrowers; number of arrears with opposite-sex borrowers). Though as discussed above, $gb_i gl_j$ is orthogonal to $\mathbf{x}$ and the consistency of β does not depend on the inclusion of the covariates. Besides the time and the sector dummies, the loan-officer fixed effects (ρ) control for any time-invariant loan-officer specific determinant of credit demand, loan conditionality, or arrear probability.[13] The branch fixed effects (η) control for any time-invariant branch-level determinant of the relevant outcomes.[14] Finally, we saturate the model by including an interaction ($gb_i X_{it}$) between borrower gender and the vector of predetermined borrower characteristics as showed in Table 2. This accounts for possible variation in borrower behavior between male and female borrowers depending on their socio-economic background and loan purpose.

3 Own-gender bias and demand for credit

3.1 Baseline findings

We first examine the effect of gender identity on the likelihood that borrowers apply for a second loan with the lender. Table 4 presents the results of estimating equation (3) with a

[12] The results are invariant to using a non-linear Probit model for the binary outcome variables. However, we lose the observations where the loan officers either have none of their borrowers return (or fall into arrears), or see all of their borrowers return (or all fall into arrears).

[13] Note that the loan officer dummy makes gl_j redundant.

[14] We can include branch fixed effects together with officer fixed effects as some officers rotate across the different branches.

dummy equal to one if a borrower applied for more credit as the dependent variable. Column (1) includes loan officer, sector, and time-fixed effects, while columns (2) through (6) gradually add loan officer (time-variant) covariates [column (2)], borrower specific covariates [column (3)], branch fixed effects [column (4)], the interaction of borrower covariates with the female borrower dummy [column (5)], and the (potentially endogenous) loan characteristics (loan amount, maturity, and interest rate) [column (6)]. All of these variables are discussed above. We omit presenting results for the control variables to save space.

The coefficients on $gb_i gl_j$ are similar across the six specifications, statistically significant, and show that loan officers' *and* borrowers' gender identity is a significant determinant of demand for credit. The main estimate, column (5), implies that borrowers matched with opposite-sex loan officers are 7.26 percentage points less likely to apply for a second loan with the same lender as compared to borrowers assigned to same-sex officers. The impact of the gender mismatch is economically significant given that 66 percent of all first-time borrowers apply for a second loan. It implies that the fraction of borrowers paired with an opposite-sex officer that do not return for a second loan is about 11 percent. For the remainder of the paper, we use the specification presented in column (5) that includes the full battery of fixed effects and all control variables, except for the loan characteristics.

3.2 Loan officers' opposite-gender experience and degree of discretion

An important aim of the paper besides establishing the existence of a bias is to document how key determinants of loan officer behavior interact with their gender preferences. To do so, we explore the impact of gender-specific human capital traits by investigating loan officers' prior exposure to opposite-sex borrowers. We also examine if loan officers' degree of discretion to act on their gender beliefs is important. Studying officers' previous experience with borrowers of the other gender allows us to test whether the gender bias is due to limited professional exposure to the opposite sex or if it is purely taste based. A better understanding of when loan officers find it in their interest to suppress their gender preferences tests if incentives matter. That is, do loan officers restrain their bias in situations where it potentially has negative consequences for their career prospective?

We first investigate the impact of prior exposure to opposite-sex borrowers. As mentioned above, most loan officers are first-time employees that may adjust their behavior through learning on the job. To the extent that more exposure lessens the bias, this may be due to an initial knowledge gap about the other gender which decreases with experience, allowing the loan officers to work more efficiently. Alternatively, they may have some initial taste bias that disappears as exposure creates "empathy" with the other gender which changes officers' preferences. On the other hand, a pure taste-based bias, as captured by a greater preference for own-gender borrowers (relative to opposite-gender borrowers) will be unchanged with additional opposite-sex experience.

Loan officer experience with opposite-sex borrowers is measured as the number of loans processed with first-time borrowers of the other gender. As these borrowers are matched arbitrarily across the officers, the number of interactions with the opposite sex is essentially random. We calculate the median of opposite-sex loan officer experience—nine interactions with the opposite sex—and split the credit-demand sample of 4,586 observations at this median.[15] The regression model is analogous to the one of column (5), Table 4, with the exception that we control for overall loan officer experience (interactions with all first-time and repeat borrowers) in some of the specifications.

The results in Table 5 show that the gender bias affecting credit demand is driven by loan officers with little previous exposure to borrowers of the opposite sex. We find a significant and negative coefficient estimate on $gb_i gl_j$ in the case where bank officers have below-median experience with the opposite gender, while the coefficient in the above-median sample is positive, insignificant, and close to zero. The Wald test shows that the difference between the two estimates in each column pair is significant at the 5 percent level. Controlling flexibly for overall experience does not change the outcomes, suggesting that the effect we capture is distinct from more general competence. The treatment impact in column (1) implies a 25 percent (16.5 percentage points) decrease in the likelihood of demanding a second loan with the lender as compared to the overall mean of 66 percent, twice the size of the average effect estimated in Table 4. The median number of 9 processed loans with opposite-sex borrowers corresponds to a median of 387 days (or average of 460 days). Although this is a non-trivial time period, it suggests that the bias disappears relatively fast as loan officers gain additional professional experience with the opposite gender.

Overall, the results provide support for a bias that fades away with gender-specific learning-on-the-job. The findings bear less credibility to the existence of pure prejudice governing the loan officers' behavior and, ultimately, demand for a second loan by the borrowers. Next we turn to loan officers' degree of discretion.

[15] We ran the identification check of Table 2 for these two and all the other subsamples shown below. The tests show that our identifying assumption holds also for the subsamples.

Do loan officers act on a gender bias if it is potentially costly to do so? We examine this question by exploring how the effect of the opposite-gender match varies with situations that impact loan officers' discretion. We use two proxies for the degree of discretion: competition from other financial institutions and the number of loan officers employed in a branch (branch size). A gender bias will be less costly in uncompetitive markets since borrowers have few outside options. As competition increases, however, a bias can be more damaging to credit demand, inducing the lender to scrutinize loan officers with greater care to detect mistreatment.[16] Hence, less competition should increase loan officers' discretion to act on their gender beliefs. Similarly, when there are few employees in a branch, a given loan officer may be more difficult to replace, giving him or her more discretion of indulging his or her preferences.

To measure financial market competition we explore variation in the universe of formally registered bank branches across Albania's 12 counties (prefekturë) over the years 2004-2006. [17] We map this information with population records for each county and year and merge both statistics with our loan-level data. The final competition measure is defined as the number of bank branches per capita, by county and year. [18] We then divide the sample according to whether the loan observations belong to regions with a branch-per capita ratio below (weak competition) or above (strong competition) the median ratio. In effect, we explore variation in competition across branches and years (allowing us to keep the branch dummies). The impact of branch size is identified in a similar manner. We exploit changes in the number of loan officers employed per branch and year yielding within-branch variation for the entire period 1996-2006. For each year, we divide the sample into bank branches above or below the median number of loan officers (our proxy for branch size). While these measures involve stronger assumptions than our earlier analysis, it is unlikely that the results are driven by reverse causality, where lower demand at the level of the individual officer-borrower opposite-gender match leads to fewer branches locating in an area or to officers leaving a branch in a given year. We also provide further support to the claim that the effect on the likelihood of applying for a second loan is supply- as opposed to demand-driven by looking at alternative measures of borrowers' financing options later in the text.

[16] Although loan officer wage is independent of whether borrowers return to the bank for a second loan, branch managers are likely to intervene (at a cost to the responsible loan officer) if a bias leads to a drop in demand.

[17] We lack country-wide information on bank-branch establishments for the earlier years in our dataset. However, as most of the loan transactions take place in the latter period, the competition data still cover roughly 75 percent of all processed loans.

[18] The results on competition are invariant to including the total number of financial institutions (banks) per county and year.

Table 6 shows that demand for credit is affected by the officer-borrower gender mismatch only when loan officers have a sufficient degree of discretion. In addition, loan officer discretion and lack of exposure to the opposite sex are complements. The negative impact on credit demand is most severe in situations when bank officers have little experience with borrowers of the other gender and few incentives to suppress their gender bias.

Panel A reports the results on branch size and competition and shows that borrowers assigned to opposite-sex loan officers are less likely to apply for second loan in smaller branches and in counties with less financial market competition. The point estimate on branch size implies that the likelihood of applying for a second loan decreases by approximately 23 percentage points or over 30 percent for a borrower that ends up with an opposite-sex loan officer in a smaller branch (the median number of loan officers per branch over the entire period is 13). The effect of competition is also sizable; an opposite-gender match induces a 25 percent drop in demand in less competitive counties (the median number of branches per 100,000 people is 7.3 for the years 2004-2006). Meanwhile, larger branches or more competitive areas yield slightly positive point estimates that are close to zero and significantly different from those of smaller branches and weak competition.

In Panels B and C we investigate the relationship between loan officer discretion and prior opposite-sex exposure. Panel B shows that the coefficient on the $gb_i gl_j$ variable is significant (p<0.0001) only in smaller branches with loan officers that have little experience of opposite-gender borrowers, with an effect of 36 percentage points. In the case of larger branches and with loan officers with more opposite-sex experience, there is no significant effect of the officer-borrower gender match. In Panel C we find a similar pattern: demand for a second loan is reduced by 35 percentage points if the loan officer in less competitive counties also has little prior exposure to borrowers of the other gender. If competition is high or the loan officer has more opposite-sex exposure the effect is insignificant. The results for the combination of exposure and competition are qualitatively and quantitatively similar to those obtained for the branch size and the exposure distinction. In unreported robustness tests, we have included branch-year trends, which account for variation in overall demand for credit in any area or change in policy that differentially affects the allocation of employees or credit to a region over time. All of the point estimates in Table 6 remain essentially the same after this inclusion.

To further corroborate that the effect on the likelihood of applying for additional credit is induced by supply-side constraints as opposed to being driven by an overall demand shock, we explore variation in income at the branch and the city (*qytet*) level over time. The conjecture is that rural and poorer areas have fewer alternative financing options implying that loan officers have more discretion to act on their gender preferences. We use three different proxies to measure socio-economic status.

Columns (1) and (2) of Table 7 split the sample according to whether the bank branches are rural or urban.[19] As can be seen, the effect on the borrower-loan officer gender match is much larger in the predominantly rural bank branches, with a coefficient of 19 percentage points. As this is a time-invariant measure, the impact is identified without branch fixed effects. Columns (3) and (4) explore variation in the mean borrower asset size in a branch across branches and years (thus including bank branch dummies). The sample is divided at the median branch asset size in given year. We identify a strong gender bias in credit demand in branches with smaller mean asset sizes, with a point estimate of 15 percentage points (p=0.007). The final two columns revisit the same question using earnings data at the city-year level from the World Bank's LSMS household survey for the years 2002-2005. We explore variation in the average monthly formal wage payment at the city and year level and divide the sample according to whether the loan observations belong to payments above or below the median in a given city-year pair. Again, the bias is more pronounced at the lower payment levels with a statistically significant point estimate that is almost identical to the coefficient produced when using the loan-level data measure derived at the branch and year level. As before, the findings are robust to the inclusion of branch-year trends.

Taken together, the results suggest that being assigned to an opposite-sex loan officer significantly reduces the likelihood that a first-time borrower applies for another loan. The effect is stronger when borrowers are matched to loan officers with little prior exposure to the opposite gender and when officers find it less costly to express their bias as proxied by changes in financial market competition, branch size, and level of income. Having established that demand for credit is affected by the gender pairing of loan officers and borrowers, we turn to some possible channels of influence.

4 Loan conditionality

The assignment of borrowers to opposite-sex loan officers may have hampered demand for credit through multiple channels. Bank officers interact with borrowers continuously over the

[19] A branch is defined as rural if it is located in a city with less than 100,000 people.

lending relationship. A gender bias may have led to excessive monitoring or even harassment of borrowers of the opposite sex or, alternatively, too little attention paid to them when advising on project-related matters. It could also have affected the interpersonal relationship, making opposite-gender borrowers feel less comfortable with their respective loan officer. To the extent that these explanations have an impact on loan performance we will be able to assess whether the officer-borrower gender match affects the likelihood of going into arrears. Borrowers may also have adjusted their behavior depending on the gender of the loan officer, but it is not clear why this adjustment would have led to lower credit demand. If anything, borrowers would be motivated to lessen the effects of a potential bias making it more difficult to find any impact of a gender bias in the data.

In this section, we explore one channel in detail; loan conditionality. Less attractive contract terms is an explicit measure of a gender bias that is easy to capture.[20] We examine two essential parts of the loan contract, interest paid and the loan amount borrowers receive. As an indirect measure of their effect on the demand for additional credit, we gauge whether opposite-sex experience and loan officer discretion remain important factors.

4.1 Interest rates

The results in Table 8 show that borrowers pay a significantly higher interest rate if matched to a loan officer of the opposite gender. To investigate the effect of the gender mismatch on annual interest paid we use the (larger) loan-conditionality sample. We replace the likelihood of applying for a second loan with the interest rate as the dependent variable and begin by studying the average impact. The results indicate that borrowers assigned to opposite-sex loan officers pay a higher price for credit compared to borrowers who end up with loan officers of the same gender. Table 8 shows that the point estimate on $gb_i gl_j$ is quite stable across the different specifications and significantly different from zero at the one percent level except for the first column. The coefficient in column (5) implies that a borrower pays, on average, 35 basis points higher interest rates if matched with a loan officer of the opposite gender. This corresponds to an increase of about 2.5 percent overall (0.35 percentage points from the mean interest rate of 14 percent). The results hold up when we estimate the effects for the smaller credit-demand sample. If anything, the impact is slightly larger.

[20] Gender-driven contract terms may, of course, also be an indication of the fact that other, less tangible, mistreatments are present.

Tables 9 and 10 investigate the effect of loan officers' opposite-sex experience and degree of discretion. Overall the findings confirm our previous conclusions: little prior exposure to borrowers of the other gender and a larger degree of loan officer discretion increase the impact of the bias. Specifically, Table 9 shows that officers with a below-median experience of opposite gender borrowers charge interest rate that are 50 basis points or 3.6 percent higher than those charged to same-sex borrowers. (The median is defined as in Table 5.) Although the difference between the below- and the above-median exposure is not significant at conventional levels [p=0.1359 for column (1)], the interest rate differential for above-median exposure is never larger than 8 basis points in any of the regressions.

Panel A of Table 10 revisits the impact of financial market competition and of branch size. The effects are qualitatively similar to those of demand for additional credit, that is, smaller branches and lower levels of competition yield higher interest rates, but the coefficients are never significantly different across small and large branches or across counties with weak and strong competition. Finally, Panels B and C of Table 10 show that the complementarity between loan officer experience and degree of discretion as found with credit demand also holds for the interest rate. Borrowers matched with loan officers of the opposite sex that have little previous exposure to the opposite gender and work in smaller branches pay 109 basis points or 7.8 percent higher interest rates ($p<0.0001$). Similarly, an opposite-sex loan officer with little experience of the other gender that works in a weakly competitive market charges opposite-sex borrowers interest rates that are 93 basis points higher. The other respective cases (high experience and large branches/strong competition) have insignificant point estimates that are statistically different at least at the ten percent level in all but once case from the low experience-smaller branch/weaker competition outcome.

Finally, we explore a third proxy for the degree of discretion that loan officers can exercise: the age difference between officers and borrowers. The idea is as follows. Consistent with studies of cognitive behavior, there is a psychological cost involved in being biased that increases in cases where it is easier for the biased party to relate to the individual being biased against (Goodwin et al., 2000; Blair, 2002). For example, a male loan officer may have stereotype beliefs about women. However, if he interacts with a female borrower of similar age, he is more likely to identify with her and, hence, experience a higher cost coming from the bias. Meanwhile, mistreating someone of the opposite sex that is older (and, hence, quite different) could be associated with a smaller loss of utility. To sum up, loan officers' degree of discretion is larger when the psychological cost is lower, which occurs when the age difference between loan officers and borrowers increases.

We implement this idea in Table 11 by dividing the sample according to the median loan officer age (24 years) and to the median borrower age (41 years). In addition, we split the sample according to the age difference, with the median difference being 16 years.[21] As predicted, the impact of the officer-borrower gender mismatch on interest rates is only significant among older borrowers [column (2)] and younger loan officers [column (3)]. Columns (5) and (6) bring the two measures together and quantify the age difference between loan officers and borrowers. When the age difference is above the median, the point estimate on $gb_i gl_j$ is 0.0055 (p=0.001). While the coefficient below the median age difference is almost 18 basis points, the difference between the two splits is significantly different at the ten percent level. The four last columns confirm that loan officer discretion as proxied by age difference and loan officer opposite-sex exposure are complements. Older borrowers assigned to younger opposite-sex loan officers with little exposure to the other gender pay 94 basis points higher interest. The point estimate is essentially identical to the one obtained when investigating loan officer exposure and financial market competition [column (1), panel C, Table 10].[22]

4.2 Approved loan amount

Tables 12 and 13 report the effect of matching borrowers to opposite-sex loan officers on the loan amount they receive. As with the interest rate regressions, we use the loan-conditionality sample but now with loan amount as the dependent variable.[23] Starting with Table 12, it shows that borrowers receive smaller loans if matched with loan officers of the other gender, though only significantly so for loan officers with little prior exposure to opposite-sex borrowers. On average, the officer-borrower gender mismatch leads to a loan that is 119 USD smaller (p=0.21). When investigating the effect for loan officers with less experience of the other gender [column (2)], the effect almost triples in size with a point estimate of -332 USD (p=0.07). This can be compared with the mean approved loan amount of 2,752 USD, indicating a 12 percent decrease. In the case of loans given by loan officers with above-

[21] The results in Table 11 are invariant to excluding loan officers' opposite-sex experience suggesting that the age results are not driven by experience per se.
[22] We have also examined if the age difference affects the officer-borrower gender match with respect to applying for a second loan and find qualitatively similar results. However, the difference between some of the point estimates fails to be significant at conventional levels.
[23] As in all of the previously reported regressions, Tables 12 and 13 control for the amount of credit applied for.

median exposure to the opposite gender, the coefficient enters positively and insignificantly in all three instances [columns (3), (5), and (7)]. The Wald tests further show that the point estimates are significantly different between the sample splits.

Table 13 reports the findings with respect to loan officer discretion and prior exposure to borrowers of the other gender. In almost all cases, the results are qualitatively similar to those obtained when examining credit demand and interest rates. Borrowers allocated to opposite-sex loan officers obtain less funding in smaller branches and when competition is lower (panel A). The coefficient for smaller branch size is significant at the five percent level with a magnitude of -339 USD, whereas the effect for larger branches is positive, close to zero, and insignificant. The negative impact is also larger and significant in counties with less competition, -193 USD, but the difference across the median is not statistically significant (p=0.27). Panel B shows that low opposite-sex experience with the other gender and branch size are complements. The coefficient on $gb_i gl_j$ is negative and significant only in the sample of small branches with loan officers that are less experienced with the other gender. The estimate, -673 USD, is significant at the one percent level and implies a 24 percent decrease from the mean loan size of 2,752 USD. In larger branches or with loan officers that have more experience with opposite-gender borrowers, there is no significant impact and the coefficients are all statistically different from the column (1) estimate. Finally, the results in panel C indicate, as before, that officers with little experience of opposite-sex borrowers that work in counties with less competition are more likely to act on their gender bias by granting smaller loans, specifically, 439 USD less. However, we also detect a positive and significant effect for loan officers with more experience in competitive counties. We do not have a good explanation for this last result.

Overall, first-time borrowers assigned to opposite-sex loan officers fare worse in terms of the price they pay for credit as well as the amount of credit the receive. In line with our earlier results for credit demand, we also find that loan officers' prior experience with the other gender and their degree of discretion are complements: loan officers with little previous opposite-sex exposure and few incentives to suppress their bias offer borrowers of the other gender distinctly inferior loan terms. The consistent findings as to when the bias has stronger effects on the officer-borrower gender mismatch across applying for a second loan, interest rate, and loan amount suggest that the drop in demand for credit at least partly follows from the results on loan conditionality. However, we recognize that slightly higher interest rates and smaller loans leave open the possibility that other channels of influence are at work as well.[24]

5 Is the gender bias efficient?

The results so far point to a bias against borrowers of the other gender, a bias that decreases with exposure of loan officers to opposite-sex borrowers. Together these findings exclude the existence of pure prejudice. However, it is not clear whether the bias stems from a knowledge gap that leads loan officers to engage in more efficient transactions with own-gender borrowers at first or if it reflects an initial taste bias. In order for the bias to be efficient in the former sense, the officer-borrower gender mismatch should also have an impact on the likelihood of ending up in arrears. Specifically, the higher interest rate and lower loan amount may indicate a higher riskiness attached by loan officers to borrowers of the opposite sex, especially if the loan officer has limited experience with borrowers of the other gender.

In this section we examine if loan officers initially have an information advantage with respect to borrowers of their own gender that is reflected in a lower level of ex-post risk as compared to borrowers of the opposite sex. We do this by exploring data on the likelihood that a loan is in arrears for more than 30 days. To allow for full loan cycles, we revert back to the credit-demand sample. All results reported in this section also go through with the larger loan-conditionality sample. The dependent variable is a dummy equal to one if a borrower has been in arrears more than 30 days during the duration of the contract.

Tables 14 and 15 report our findings. Overall, there is no indication that borrowers of the same gender as their loan officer perform better in terms of a significantly lower likelihood of going into arrears. Column (1) of Table 14 shows that, on average, the arrear probability of loans screened and monitored by opposite-gender loan officers are not significantly different from the arrear probability of loans screened and monitored by own-gender loan officers. If anything, opposite-sex borrowers are less likely to go into arrears. The variable on the officer-borrower gender mismatch is insignificant, with an effect of -1.5 percentage points over the loan cycle. Dividing the sample by median opposite-sex experience [columns (2)-(7)], does not alter this conclusion. While the estimate on $gb_i gl_j$ enters positively for the below-median sample, the coefficient is insignificant (p=0.84). For

[24] In unreported robustness tests, we also explore whether there is a gender bias in other loan conditions, including the approved maturity. We do not see any impact of the officer-borrower gender match on average or across officer experience/discretion.

above-median opposite-gender experience, arrears are again lower, not higher, for borrowers of the other gender.

Table 15 examines the impact of the officer-borrower gender interaction depending on branch size and financial market competition. We find a similar picture as in Table 14 when splitting the sample according to branch size [columns (1) and (2), panel A], with a positive (negative) gender-interaction estimate in the small (large) branch subsample. This also holds true when investigating competition. Panels B and C unpack the relation between officer-borrower gender exposure and degree of discretion. Except for the pair of low exposure-weak competition where the gender interaction term enters significantly at the 10% level, there is little evidence for the conclusion that own-gender borrowers are less likely to go into arrears or, alternatively, that borrowers matched with opposite-sex loan officers are more likely to enter arrears during the life of their first loan.

The results show that the significant gender bias found in the demand for a second loan or in terms of the loan contract, is absent in the arrear outcomes. One explanation for the lack of any discernible pattern may be that the potential monitoring advantage loan officers have when interacting with borrowers of the same gender boils down to avoiding larger shocks. To explore this possibility, we repeated all the regressions using the 60 day arrear measure. However, the results are similar to those reported above. (See Tables A3 and A4 in the Appendix for details.) Taken as a whole, this supports the existence of an initial taste-based bias rather than the notion of an information hypothesis where loan officers are more efficient when transacting with own-gender as compared to opposite-gender borrowers.

6 The source of the bias

In our regression analysis, we rely on the quasi-random assignment of borrowers to loan officers controlling for sector and time fixed effects. While this ensures that our results are not driven by unobserved borrower characteristics correlated with the assignment of borrowers of one gender to loan officers of the other, it bars us from making inferences about the direction of the bias. That is, whether the bias is due to either male or female loan officers or both favoring borrowers of their own gender, or disfavoring those of the other gender. In this final section, we offer some suggestive evidence that the bias comes from both sides by reanalyzing the average impact of $gb_i gl_j$ on interest rates and on the likelihood of applying for a second loan at the individual loan-officer level.[25]

For each loan officer, we regress the interest rate on a female borrower dummy for loan officers with at least 45 observations, controlling for time, sector, branch fixed effects, and loan officer and borrower characteristics.[26] Because these regressions are estimated separately for each loan officer, they control for loan officer specific differences in interest rate setting and propensity to monitor. Figure 1 plots the coefficient estimate for the female borrower dummy for each loan officer, with the bars representing the 95 percent confidence interval around the estimates. We find that the median interest rate differential for female (as opposed to male) borrowers is –18 basis points in the case of female loan officers and 20 basis points in the case of male loan officers. While many of the coefficients are imprecisely estimated, quite a few yield point estimates that are statistically significantly different from zero. The reason for the somewhat smaller impact is that most of the loan officers in this sample have more experience of opposite-sex borrowers than the median officer. The figure indicates that the bias against the other gender is prevalent for loan officers of both genders. That is, the majority of male loan officers have a greater propensity to charge higher interest rates when lending to female borrowers than the majority of female loan officers.

Figure 2 points to a qualitatively similar effect of the gender bias from both male and female loan officers on credit demand. Instead of using the interest rate, we explore the probability of returning for a second loan as the dependent variable. The median coefficient estimate is of similar economic magnitude across the genders: the female borrower dummy is –3.4 percent for male loan officers, whereas it is 3.4 percent for female loan officers. The interpretation is again that there is a pro-male bias among male loan officers and a pro-female bias among female loan officers leading borrowers of the opposite sex to exit at a greater degree.

To sum up, figures 1 and 2 show that the evidence of a gender bias persists, even when the analysis is aggregated to the level of each loan officer, and indicate that loan officers predominately engage in an own-gender bias when transacting with their clients.

[25] We confine our study to interest rates and credit demand as we have significant average effects in these two outcomes.
[26] This analysis is similar in spirit to Price and Wolfers (2010).

7 Conclusion

Our results suggest that own-gender preferences affect credit market outcomes. First-time borrowers matched with opposite-sex loan officers in a large Albanian bank are 11 percent less likely to demand additional credit from the lender. The detected bias originates with borrowers whose loan officers have little prior exposure to borrowers of the other gender or whose loan officers have weak incentives to suppress their beliefs as proxied by financial market competition and branch size. These two factors are also complementary: the greatest impact of the officer-borrower match is found in instances when loan officers with little experience of the other gender are less scrutinized.

The effects we identify are consistent with the explanation that opposite-sex borrowers receive inferior loan terms. To this end, we also show that borrowers assigned to loan officers of the other gender pay between 35 to 109 basis points higher interest rate and receive between 4 to 24 percent lower loan amounts, with the variation again depending on loan officers' opposite-sex experience and degree of discretion.

The own-gender bias does not seem to stem from pure prejudice nor is it consistent with loan officers initially treating borrowers of their own gender more efficiently, at least not as reflected in the level of ex-post risk as measured by the likelihood of entering into arrears.

While our findings provide answers to where the bias should be stronger and why demand for credit decreases in the opposite-gender match it is, of course, possible that other channels are at work. In addition, we have detected a gender bias in a relatively poor country, Albania, where the level of gender discrimination is rather high, although women have made important strides into the labor market as shown by the high share of female loan officers employed by the lender. It is possible that the effect of a loan officer-borrower gender mismatch in a more developed setting would be different.

Finally, our paper is the first to gauge the existence of an own-gender bias in lending and a better understanding of in-group identity, in the form of own-gender preferences, has at least two implications for the functioning of the credit market. First, identity should affect firms' human-resource practices as loan officers' opposite-gender experience has repercussions for the size of the bias. Second, from a policy perspective, our findings point to the possibility that financial market competition can be a powerful tool in dampening the biases of loan officers, and, ultimately, banks, against borrowers of a certain gender.

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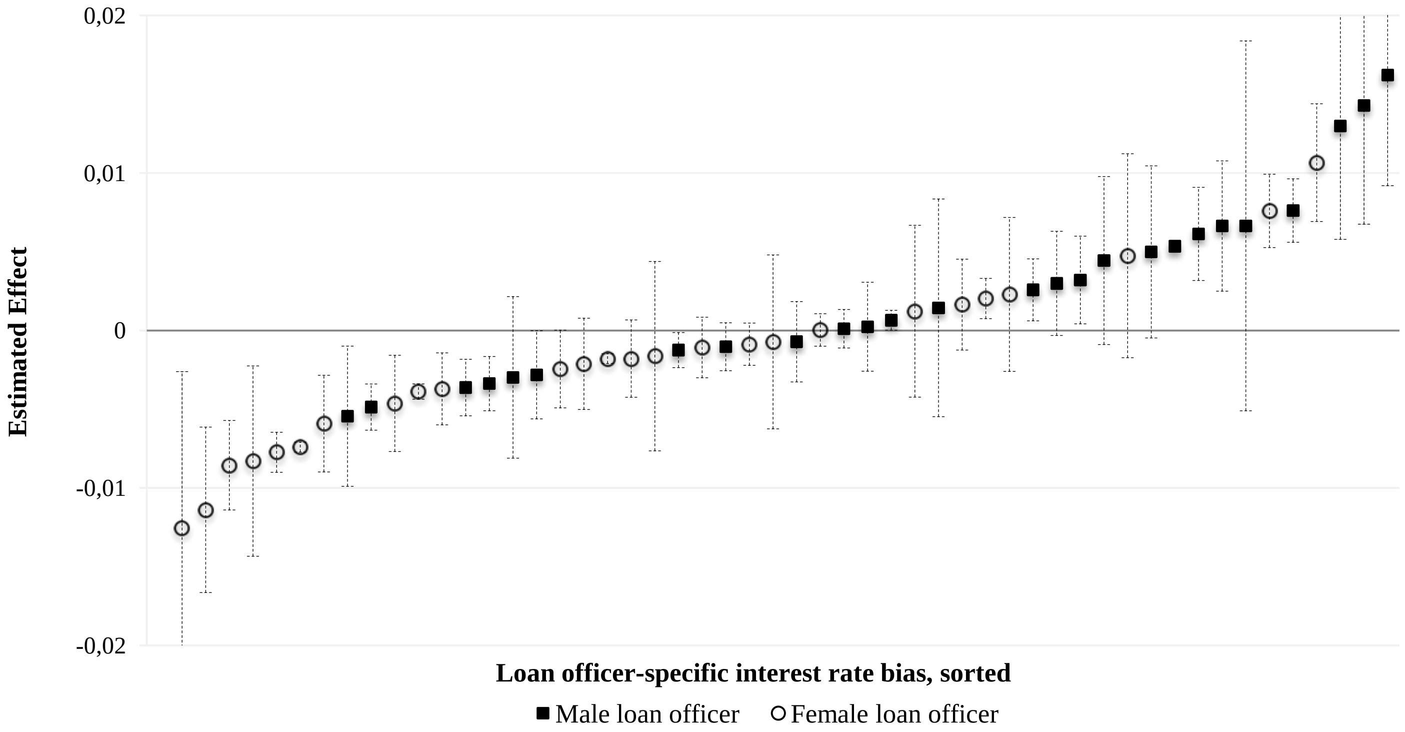

Figure 1: Distribution of the interest rates by loan officer gender

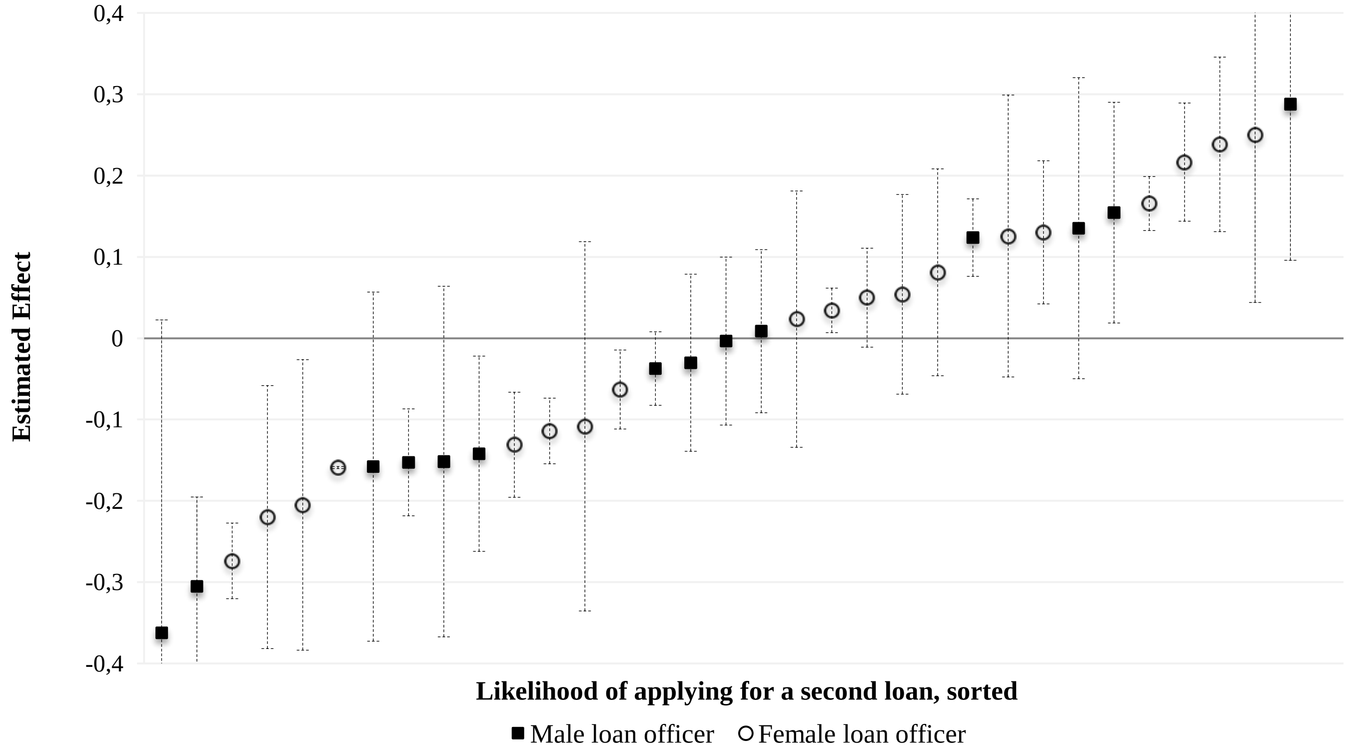

Figure 2: Distribution of the likelihood that borrowers apply for a second loan by loan officer gender

Table 1: Descriptive statistics for the credit-demand sample

Variable	Mean (1)	SD (2)	Median (3)	Male borrower (4)	Female borrower (5)	Male loan officer (6)	Female loan officer (7)
Likelihood of applying for a second loan	0.66	0.47	1.00	0.66	0.65	0.68	0.65
Arrears > 30 days	0.05	0.22	0.00	0.05	0.04	0.06	0.05
Female borrower	0.18	0.39	0.00	0.00	1.00	0.18	0.19
Civil status (married = 1)	0.87	0.34	1.00	0.90	0.74	0.87	0.86
Age applicant	40.87	10.10	40.94	40.75	41.44	40.86	40.88
Total assets (in USD)	24,361	45,536	15,108	24,700	22,862	25,299	23,778
Applied loan amount (in USD)	2,726	2,692	1,993	2,801	2,392	2,625	2,788
Approved loan amount (in USD)	2,370	2,492	1,688	2,435	2,086	2,259	2,440
Approved maturity (in days)	501	205	480	501	498	478	515
Personal guarantee	0.23	0.42	0.00	0.23	0.22	0.20	0.24
Mortgage guarantee	0.14	0.35	0.00	0.14	0.13	0.11	0.16
Chattel guarantee	0.95	0.22	1.00	0.95	0.94	0.96	0.94
Destination Working Capital	0.10	0.29	0.00	0.10	0.06	0.11	0.09
Destination Fixed Assets	0.29	0.45	0.00	0.32	0.15	0.36	0.25
Destination Housing Improvement	0.37	0.48	0.00	0.36	0.45	0.30	0.42
Destination Consumption	0.24	0.43	0.00	0.21	0.35	0.22	0.25
Destination Others	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Production	0.12	0.32	0.00	0.14	0.04	0.20	0.07
Transport	0.15	0.35	0.00	0.17	0.04	0.16	0.14
Construction	0.73	0.44	1.00	0.69	0.92	0.65	0.79
Female loan officer	0.62	0.49	1.00	0.61	0.64	0.00	1.00
Age loan officer	25.28	4.17	23.75	25.32	25.11	26.33	24.63
Opposite sex experience	19.45	27.74	9.00	18.75	22.52	5.16	28.33
Opposite sex arrear experience	1.16	2.30	0.00	1.11	1.39	0.29	1.71
Branch size (number of loan officers)	15.51	9.01	13.00	15.23	16.74	16.28	15.03
Branches per 100,000 inhabitants, county level	7.35	3.40	7.33	7.35	7.36	7.58	7.20
Monthly wage payment, city level (in USD)	276	71	285	275	281	286	269
Observations	4,589						

This table reports descriptive statistics [mean, standard deviation (SD), median] for the credit demand sample. Columns (1)-(3) show the values for the entire sample, columns (4) and (5) the means for male and female borrowers, and columns (6) and (7) the means for male and female loan officers.

Table 2: Test for differences in borrower characteristics for the credit-demand sample

	Male loan officers			Female loan officers			
	Male borrowers	Female borrowers	t-statistic	Male borrowers	Female borrowers	t-statistic	t-statistic
Variable	(1)	(2)	(3) = (1) - (2)	(4)	(5)	(6) = (4) - (5)	(7) = (3) - (6)
Age applicant	40.81	41.12	0.75	40.70	41.63	2.11**	-0.41
Civil status (married = 1)	0.90	0.76	-4.90***	0.89	0.73	-10.04***	1.53
Applied loan amount (in USD)	2,686	2,336	-2.77***	2,874	2,424	-3.18***	0.56
Applied maturity (in days)	533	514	-1.96*	562	563	-1.28	-1.31
Total assets (in USD)	25,548	24,125	-1.70*	24,163	22,138	-3.01***	0.30
Personal guarantee	0.21	0.20	-0.33	0.24	0.24	-0.07	-0.25
Mortgage collateral	0.11	0.10	-0.99	0.16	0.14	-1.99**	0.26
Chattel collateral	0.96	0.95	0.32	0.94	0.93	-0.61	0.66
Working Capital	0.12	0.08	0.60	0.10	0.04	-3.21***	1.57
Fixed Assets	0.40	0.18	-4.80***	0.28	0.13	-4.03***	-1.37
Housing Improvement	0.28	0.37	1.12	0.40	0.49	2.24	-0.96
Consumption	0.20	0.36	4.34***	0.23	0.34	3.14	0.96
Others	0.00	0.00	n.a.	0.00	0.00	n.a.	n.a.
Observations	1,451	308	1,759	2,292	538	2,830	4,589

This table contains a test of difference in observable borrower characteristics for the credit-demand sample. Columns (1) and (2) show raw means for a set of borrower characteristics of male and female borrowers matched with male loan officers. Column (3) displays the t-statistic of a test of difference of the respective characteristic between male and female borrowers assigned to male loan officers. Columns (4) and (5) show raw means of male and female borrowers matched with female loan officers. Column (6) shows the t-statistic of a test of difference of the respective characteristic between male and female borrowers assigned to female loan officers. Column (7) reports the t-statistic of a test of difference-in-differences for the respective borrower characteristic. The t-statistics in columns (3), (6), and (7) are estimated conditioned on time and on sector fixed effects. Standard errors are clustered at the branch-sector-year level. ***, **, * indicate significance at the 1, 5, and 10 percent level, respectively.

Table 3: Test of random assignment

	(1)	(2)	(3)
Panel A: Credit-demand sample			
Borrower gender	-0.0236	-0.0244	0.0101
	[0.0301]	[0.0278]	[0.0188]
Time fixed effects	No	Yes	Yes
Sector fixed effects	No	No	Yes
Observations	4,589	4,589	4,589
Panel B: Loan-conditionality sample			
Borrower gender	-0.0979***	-0.0836**	-0.0257
	[0.0319]	[0.0351]	[0.0218]
Time fixed effects	No	Yes	Yes
Sector fixed effects	No	No	Yes
Observations	7,272	7,272	7,272

This table reports the regression where loan officer gender is regressed on borrower gender. The dependent variable is a dummy that takes on value one if the loan officer is male. The main independent variable is a dummy that takes on value one if the borrower is female. Column (1) does not include any control variables. The column (2) regression adds time fixed effects and the column (3) regression further adds sector fixed effects. Standard errors clustered at the branch-sector-year level are shown in parentheses. ***, **, * indicate significance at the 1, 5, and 10 percent level, respectively.

Table 4: Own-gender bias and the demand for a second loan

	(1)	(2)	(3)	(4)	(5)	(6)
Gender×Gender	-0.0687**	-0.0689**	-0.0819**	-0.0785**	-0.0726**	-0.0754**
	[0.0330]	[0.0330]	[0.0320]	[0.0316]	[0.0360]	[0.0360]
Adjusted R-squared	0.0600	0.0600	0.0790	0.0810	0.0840	0.0850
Observations	4,589	4,589	4,586	4,586	4,586	4,586
Loan-officer fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Loan officer covariates	No	Yes	Yes	Yes	Yes	Yes
Borrower covariates	No	No	Yes	Yes	Yes	Yes
Branch fixed effects	No	No	No	Yes	Yes	Yes
Borrower gender×borrower covariates	No	No	No	No	Yes	Yes
Loan characteristics	No	No	No	No	No	Yes

This table reports regression results with the likelihood of returning for a second loan as the dependent variable using the credit-demand sample. Loan demand is a dummy variable that takes the value one if borrowers return to the bank for an additional loan application. Each regression also includes time and sector fixed effects. Standard errors clustered at the branch-sector-year level are shown in parentheses. ***, **, * indicate significance at the 1, 5, and 10 percent level, respectively.

Table 5: Demand for a second loan and loan officer experience with opposite-sex borrowers

	Low experience (1)	High experience (2)	Low experience (3)	High experience (4)	Low experience (5)	High experience (6)
Gender×Gender	-0.1649***	0.0207	-0.1638***	0.0197	-0.1666***	0.0200
	[0.0592]	[0.0383]	[0.0597]	[0.0384]	[0.0594]	[0.0384]
P-value of Wald test	0.0151		0.0170		0.0149	
Overall experience	No	No	Yes	Yes	Yes	Yes
Overall experience, second-order polynomial	No	No	No	No	Yes	Yes
Adjusted R-squared	0.1020	0.0900	0.1030	0.0910	0.1050	0.0920
Observations	2,247	2,339	2,247	2,339	2,247	2,339

This table reports regression results with the likelihood of returning for a second loan as the dependent variable using the credit-demand sample. Loan demand is a dummy variable that takes the value one if borrowers return to the bank for an additional loan application. The sample is divided at the median first-time borrower opposite sex experience (median = 9 interactions with first-time borrowers of the opposite sex). All regressions include the control variables as specified in column (5) of Table 4 (including time and sector fixed effects), the results for these are omitted to save space. In columns (1) and (2), we do not control for any loan officer experience, in columns (3) and (4) we control for overall loan officer experience, and in columns (5) and (6) we control for a second-order polynomial in overall loan officer experience. Standard errors clustered at the branch-sector-year level are shown in parentheses. ***, **, * indicate significance at the 1, 5, and 10 percent level, respectively.

Table 6: Demand for a second loan, branch size, competition, and loan officer experience with opposite-sex borrowers

	Small branches (1)	Large branches (2)	Weak competition (3)	Strong competition (4)
Panel A: Branch size and competition				
Gender×Gender	-0.2331***	0.0207	-0.1720***	0.0356
	[0.0567]	[0.0386]	[0.0483]	[0.0512]
P-value of Wald test	0.0001		0.0018	
Adjusted R-squared	0.0640	0.1110	0.0890	0.0850
Observations	2,275	2,311	1,798	1,736
Panel B: Experience and branch size	Low experience		High experience	
	Small branches	Large branches	Small branches	Large branches
Gender×Gender	-0.3627***	-0.0071	0.0575	0.0220
	[0.0789]	[0.0765]	[0.0717]	[0.0572]
P-value of Wald test		0.0004	0.0001	0.0000
Adjusted R-squared	0.1070	0.1090	0.0630	0.1030
Observations	1,196	1,051	1,079	1,260
Panel C: Experience and competition	Low experience		High experience	
	Weak competition	Strong competition	Weak competition	Strong competition
Gender×Gender	-0.3451***	-0.0607	-0.0612	0.0894
	[0.0843]	[0.0923]	[0.0748]	[0.0979]
P-value of Wald test		0.0070	0.0118	0.0002
Adjusted R-squared	0.1340	0.1090	0.0710	0.0680
Observations	763	866	1,035	870

This table reports regression results with the likelihood of returning for a second loan as the dependent variable using the credit-demand sample. Loan demand is a dummy variable that takes the value one if borrowers return to the bank for an additional loan application. All regressions include the control variables as specified in column (5) of Table 4 (including time and sector fixed effects), the results for these are omitted to save space. In columns (1) and (2) of Panel A we split the sample according to the median branch size, in columns (3) and (4) according to competition measured as the ratio of branches over population in a specific region and year for the time period 2004-2006. In Panels B and C, we further split the samples according to the median loan officer experience with the opposite sex. Standard errors clustered at the branch-sector-year level are shown in parentheses. ***, **, * indicate significance at the 1, 5, and 10 percent level, respectively.

Table 7: Demand for a second loan by socio-economic status

	Rural areas (1)	Urban areas (2)	Low asset size (3)	High asset size (4)	Low wage payment (5)	High wage payment (6)
Gender×Gender	-0.1882***	-0.0404	-0.1539***	-0.0313	-0.1510**	-0.0162
	[0.0551]	[0.0369]	[0.0551]	[0.0418]	[0.0570]	[0.0358]
P-value of Wald test	0.0210		0.0636		0.0364	
Branch fixed effects	No	No	Yes	Yes	Yes	Yes
Adjusted R-squared	0.0670	0.0970	0.0770	0.1000	0.0920	0.0940
Observations	1,863	2,723	2,309	2,277	1,879	1,872

This table reports regression results with the likelihood of returning for a second loan as the dependent variable using the credit-demand sample. Loan demand is a dummy variable that takes the value one if borrowers return to the bank for an additional loan application. All regressions include the control variables as specified in column (5) of Table 4 (including time and sector fixed effects) except for columns (1) and (2) that exclude branch dummies, the results for the control variables are omitted to save space. In columns (1) and (2) we split the sample according to whether a branch is located in a rural or an urban area using the population of the respective cities the branches are located in. In columns (3) and (4) we split the sample according to the median asset size of the borrower's business, measured over branch location and year. In columns (5) and (6) we split the sample according to areas in which the monthly payment was below or above the median, measured at the city and year level. Standard errors clustered at the branch-sector-year level are shown in parentheses. ***, **, * indicate significance at the 1, 5, and 10 percent level, respectively.

Table 8: Own-gender bias and interest rates

	(1)	(2)	(3)	(4)	(5)	(6)
Gender×Gender	0.0028**	0.0032***	0.0032***	0.0034***	0.0035***	0.0034***
	[0.0011]	[0.0011]	[0.0011]	[0.0011]	[0.0013]	[0.0013]
Adjusted R-squared	0.4330	0.4710	0.5470	0.5490	0.5510	0.5570
Observations	7,272	7,272	7,266	7,266	7,266	7,266
Loan-officer fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Loan officer covariates	No	Yes	Yes	Yes	Yes	Yes
Borrower covariates	No	No	Yes	Yes	Yes	Yes
Branch fixed effects	No	No	No	Yes	Yes	Yes
Borrower gender×borrower covariates	No	No	No	No	Yes	Yes
Loan characteristics	No	No	No	No	No	Yes

This table shows regression results with the interest rate as the dependent variable using the loan-conditionality sample. Each regression includes time and sector fixed effects. Standard errors clustered at the branch-sector-year level are shown in parentheses. ***, **, * indicate significance at the 1, 5, and 10 percent level, respectively.

Table 9: Interest rate and loan officer experience with opposite-sex borrowers

	Low experience (1)	High experience (2)	Low experience (3)	High experience (4)	Low experience (5)	High experience (6)
Gender×Gender	0.0050**	0.0008	0.0050**	0.0006	0.0051**	0.0007
	[0.0020]	[0.0015]	[0.0020]	[0.0015]	[0.0020]	[0.0015]
P-value of Wald test	0.1359		0.1349		0.1564	
Overall experience	No	No	Yes	Yes	Yes	Yes
Overall experience, second-order polynomial	No	No	No	No	Yes	Yes
Adjusted R-squared	0.6470	0.5210	0.6470	0.5250	0.6470	0.5250
Observations	3,678	3,588	3,678	3,588	3,678	3,588

This table shows regression results with the interest rate as the dependent variable using the loan-conditionality sample. All regressions include the control variables as specified in column (5) of Table 4 (including time and sector fixed effects), the results for these are omitted to save space. The sample is divided at the median first-time borrower opposite sex experience (median = 8 interactions with first-time borrowers of the opposite sex). In columns (1) and (2), we do not control for any loan officer experience, in columns (3) and (4) we control for overall loan officer experience, and in columns (5) and (6) we control for a second-order polynomial in overall loan officer experience. Standard errors clustered at the branch-sector-year level are shown in parentheses. ***, **, * indicate significance at the 1, 5, and 10 percent level, respectively.

Table 10: Interest rate, branch size, competition, and loan officer experience with opposite-sex borrowers

	Small branches (1)	Large branches (2)	Weak competition (3)	Strong competition (4)
Panel A: Branch size and competition				
Gender×Gender	0.0044**	0.0033*	0.0030*	0.0018
	[0.0022]	[0.0017]	[0.0015]	[0.0024]
P-value of Wald test	0.6768		0.6563	
Adjusted R-squared	0.6310	0.5020	0.5120	0.5430
Observations	3,964	3,302	3,365	1,720
Panel B: Experience and branch size	Low experience		High experience	
	Small branches	Large branches	Small branches	Large branches
Gender×Gender	0.0109***	0.0018	-0.0018	0.0028
	[0.0030]	[0.0022]	[0.0031]	[0.0017]
P-value of Wald test		0.0085	0.0030	0.0123
Adjusted R-squared	0.7020	0.6010	0.5790	0.4500
Observations	2,320	1,356	1,644	1,946
Panel C: Experience and competition	Low experience		High experience	
	Weak competition	Strong competition	Weak competition	Strong competition
Gender×Gender	0.0093***	0.0033	-0.0011	0.0011
	[0.0031]	[0.0030]	[0.0021]	[0.0035]
P-value of Wald test		0.1309	0.0138	0.0644
Adjusted R-squared	0.6210	0.6210	0.4780	0.5460
Observations	1,526	879	1,839	841

This table shows regression results with the interest rate as the dependent variable using the loan-conditionality sample. All regressions include the control variables as specified in column (5) of Table 4 (including time and sector fixed effects), the results for these are omitted to save space. In columns (1) and (2) of Panel A we split the sample according to the median branch size, in columns (3) and (4) according to competition measured as the ratio of branches over population in a specific region and year for the time period 2004-2006. In Panels B and C, we further split the samples according to the median loan officer experience with the opposite sex. Standard errors clustered at the branch-sector-year level are shown in parentheses. ***, **, * indicate significance at the 1, 5, and 10 percent level, respectively.

Table 11: Interest rate and officer-borrower age difference

	Young borrowers	Old borrowers	Young loan officers	Old loan officers	Low age difference	High age difference	Low experience		High experience	
							Low age diff	High age diff	Low age diff	High age diff
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Gender×Gender	0.0011	0.0050***	0.0073***	0.0003	0.0018	0.0055***	-0.0012	0.0094***	0.0021	-0.0003
	[0.0015]	[0.0018]	[0.0019]	[0.0016]	[0.0016]	[0.0017]	[0.0036]	[0.0027]	[0.0025]	[0.0020]
P-value of Wald test	0.0663		0.0019		0.0875			0.0067	0.0580	0.0003
Adjusted R-squared	0.5570	0.5550	0.5410	0.5940	0.5500	0.5650	0.6780	0.6580	0.4930	0.5210
Observations	3,634	3,632	3,634	3,632	3,633	3,633	1,804	1,872	1,829	1,761

This table shows regression results with interest rate as the dependent variable using the loan-conditionality sample. All regressions include the control variables as specified in column (5) of Table 4 (including time and sector fixed effects), the results for these are omitted to save space. In columns (1) and (2), the sample is split according to the median borrower age (41 years). In columns (3) and (4), the sample is split according to the median loan officer age (24 years). In columns (5) and (6), the sample is split according to the median age difference between borrowers and loan officers (16 years). In columns (7) through (10), the sample is further split according to loan officer experience with the opposite sex. Standard errors clustered at the branch-sector-year level are shown in parentheses. ***, **, * indicate significance at the 1, 5, and 10 percent level, respectively.

Table 12: Approved loan amount and loan officer experience with opposite-sex borrowers

	Average effect	Low experience	High experience	Low experience	High experience	Low experience	High experience
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Gender×Gender	-118.9302	-332.4403*	207.2403	-336.3374*	207.0527	-342.9380*	214.7092
	[94.8195]	[176.9956]	[182.3961]	[177.7569]	[181.8117]	[176.8240]	[182.3542]
P-value of Wald test		0.0457		0.0456		0.0429	
Overall experience	No	No	No	Yes	Yes	Yes	Yes
Overall experience, second-order polynomial	No	No	No	No	No	Yes	Yes
Adjusted R-squared	0.6510	0.6920	0.6350	0.6920	0.6350	0.6920	0.6350
Observations	7,266	3,678	3,588	3,678	3,588	3,678	3,588

This table shows regression results with the approved loan amount in USD as the dependent variable using the loan-conditionality sample.. All regressions include the control variables as specified in column (5) of Table 4 (including time and sector fixed effects), the results for these are omitted to save space. The sample is divided at the median first-time borrower opposite sex experience (median = 8 interactions with first-time borrowers of the opposite sex). In column (1) we show the average effect on loan amount, in columns (2) to (7) we split the sample according to median loan officer experience with the opposite sex. In columns (2) and (3), we do not control for any loan officer experience, in columns (4) and (5) we control for overall loan officer experience, and in columns (6) and (7) we control for a second-order polynomial in overall loan officer experience. Standard errors clustered at the branch-sector-year level are shown in parentheses. ***, **, * indicate significance at the 1, 5, and 10 percent level, respectively.

Table 13: Approved loan amount, branch size, competition, and loan officer experience with opposite-sex borrowers

	Small branches (1)	Large branches (2)	Weak competition (3)	Strong competition (4)
Panel A: Branch size and competition				
Gender×Gender	-338.7150**	10.3515	-192.5165*	64.4844
	[147.2241]	[138.4870]	[112.1763]	[215.5900]
P-value of Wald test	0.0725		0.2665	
Adjusted R-squared	0.6310	0.5020	0.5120	0.5430
Observations	3,964	3,302	3,365	1,720
Panel B: Experience and branch size	Low experience		High experience	
	Small branches	Large branches	Small branches	Large branches
Gender×Gender	-673.4192***	64.1529	162.5735	200.314
	[245.7182]	[296.4278]	[213.6640]	[150.5055]
P-value of Wald test		0.0395	0.0050	0.0014
Adjusted R-squared	0.7020	0.7120	0.7100	0.6070
Observations	2,320	1,356	1,644	1,946
Panel C: Experience and competition	Low experience		High experience	
	Weak competition	Strong competition	Weak competition	Strong competition
Gender×Gender	-438.5765*	-42.6934	30.2655	449.5977**
	[255.7874]	[332.1583]	[83.9955]	[196.0232]
P-value of Wald test		0.3053	0.0685	0.0034
Adjusted R-squared	0.7230	0.7590	0.7900	0.7020
Observations	1,526	879	1,839	841

This table shows regression results with the approved loan amount in USD as the dependent variable using the loan-conditionality sample.. All regressions include the control variables as specified in column (5) of Table 4 (including time and sector fixed effects), the results for these are omitted to save space. In columns (1) and (2) of Panel A, we split the sample according to the median branch size, in columns (3) and (4) according to competition measured as the ratio of branches over population in a specific region and year for the time period 2004-2006. In Panels B and C, we further split the samples according to the median loan officer experience with the opposite sex. Standard errors clustered at the branch-sector-year level are shown in parentheses. ***, **, * indicate significance at the 1, 5, and 10 percent level, respectively.

Table 14: Arrears > 30 days and loan officer experience with opposite-sex borrowers

	Average effect	Low experience	High experience	Low experience	High experience	Low experience	High experience
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Gender×Gender	-0.0152	0.0176	-0.0502*	0.0170	-0.0503*	0.0173	-0.0505*
	[0.0171]	[0.0250]	[0.0287]	[0.0252]	[0.0288]	[0.0249]	[0.0292]
P-value of Wald test		0.0651		0.0689		0.0695	
Overall experience	No	No	No	Yes	Yes	Yes	Yes
Overall experience, second-order polynomial	No	No	No	No	No	Yes	Yes
Adjusted R-squared	0.1090	0.1280	0.0580	0.1290	0.0580	0.1290	0.0590
Observations	4,586	2,246	2,340	2,246	2,340	2,246	2,340

This table shows regression results with the measure arrears > 30 days as the dependent variable using the credit-demand sample. Arrears > 30 days is a dummy variable that takes on value one if a borrower went into arrears for more than 30 days at any time during the lifetime of her loan. All regressions include the control variables as specified in column (5) of Table 4 (including time and sector fixed effects), the results for these are omitted to save space.. In column (1) we show the average effect on arrears > 30 days, in columns (2) to (7) we split the sample according to median loan officer experience with the opposite sex. In columns (2) and (3), we do not control for any loan officer experience, in columns (4) and (5) we control for overall loan officer experience, and in columns (6) and (7) we control for a second-order polynomial in overall loan officer experience. Standard errors clustered at the branch-sector-year level are shown in parentheses. ***, **, * indicate significance at the 1, 5, and 10 percent level, respectively.

Table 15: Arrears > 30 days, branch size, competition, and loan officer experience with opposite-sex borrowers

	Small branches (1)	Large branches (2)	Weak competition (3)	Strong competition (4)
Panel A: Branch size and competition				
Gender×Gender	0.0137	-0.0386	0.0249	-0.0321
	[0.0212]	[0.0244]	[0.0197]	[0.0299]
P-value of Wald test	0.0894		0.0928	
Adjusted R-squared	0.0800	0.1120	0.1170	0.0890
Observations	2,275	2,311	1,798	1,736
Panel B: Experience and branch size	Low experience		High experience	
	Small branches	Large branches	Small branches	Large branches
Gender×Gender	0.0194	0.0178	0.0496***	-0.1075***
	[0.0466]	[0.0376]	[0.0176]	[0.0167]
P-value of Wald test		0.9773	0.5367	0.0060
Adjusted R-squared	0.0950	0.1340	0.0130	0.0680
Observations	1,196	1,050	1,079	1,261
Panel C: Experience and competition	Low experience		High experience	
	Weak competition	Strong competition	Weak competition	Strong competition
Gender×Gender	0.0568*	0.0382	0.0188	-0.1040***
	[0.0304]	[0.0433]	[0.0414]	[0.0207]
P-value of Wald test		0.7260	0.4309	0.0000
Adjusted R-squared	0.1480	0.1150	0.0610	0.0360
Observations	763	865	1,035	871

This table shows regression results with the measure arrears > 30 days as the dependent variable using the credit-demand sample. Arrears > 30 days is a dummy variable that takes on value one if a borrower went into arrears for more than 30 days at any time during the lifetime of her loan. All regressions include the control variables as specified in column (5) of Table 4 (including time and sector fixed effects), the results for these are omitted to save space. In columns (1) and (2) of Panel A, we split the sample according to the median branch size, in columns (3) and (4) according to competition measured as the ratio of branches over population in a specific region and year for the time period 2004-2006. In Panels B and C, we further split the samples according to the median loan officer experience with the opposite sex. Standard errors clustered at the branch-sector-year level are shown in parentheses. ***, **, * indicate significance at the 1, 5, and 10 percent level, respectively.

Appendix Table A1: Descriptive statistics for the loan-conditionality sample

Variable	Mean (1)	SD (2)	Median (3)	Male borrower (4)	Female borrower (5)	Male Loan officer (6)	Female Loan officer (7)
Interest rate	0.14	0.03	0.14	0.14	0.14	0.14	0.14
Approved loan amount (in USD)	2,752	2,880	1,966	2,811	2,468	2,700	2,789
Female borrower	0.17	0.38	0.00	0.00	1.00	0.14	0.19
Civil status (married = 1)	0.87	0.34	1.00	0.90	0.74	0.89	0.86
Age applicant	41.08	10.25	41.07	40.97	41.64	41.19	41.00
Total assets (in USD)	27,790	85,099	17,028	27,974	26,897	27,495	28,001
Applied loan amount (in USD)	3,078	2,986	2,116	3,143	2,760	3,024	3,116
Approved maturity (in days)	566	294	540	567	562	544	581
Personal guarantee	0.17	0.38	0.00	0.17	0.18	0.14	0.20
Mortgage guarantee	0.12	0.33	0.00	0.12	0.12	0.09	0.14
Chattel guarantee	0.96	0.20	1.00	0.96	0.95	0.97	0.95
Destination Working Capital	0.09	0.29	0.00	0.10	0.05	0.12	0.07
Destination Fixed Assets	0.36	0.48	0.00	0.40	0.18	0.52	0.25
Destination Housing Improvement	0.33	0.47	0.00	0.31	0.42	0.21	0.41
Destination Consumption	0.22	0.41	0.00	0.20	0.34	0.15	0.27
Destination Others	0.00	0.02	0.00	0.00	0.00	0.00	0.00
Production	0.16	0.37	0.00	0.19	0.06	0.29	0.07
Transport	0.14	0.35	0.00	0.16	0.04	0.16	0.13
Construction	0.69	0.46	1.00	0.65	0.90	0.55	0.80
Female loan officer	0.58	0.49	1.00	0.57	0.66	0.00	1.00
Age loan officer	25.38	4.33	23.79	25.44	25.10	26.53	24.56
Opposite sex experience	24.51	37.62	8.00	22.95	32.06	4.39	38.87
Opposite sex arrear experience	1.63	3.23	0.00	1.49	2.31	0.30	2.58
Branch size (number of loan officers)	14.15	8.53	12.00	13.79	15.86	13.50	14.61
Number of branches per 100,000 inhabitants, county level	5.75	2.69	5.47	5.76	5.74	5.90	5.63
Observations	7,272						

This table reports descriptive statistics [mean, standard deviation (SD), median] for the loan-conditionality sample. Columns (1)-(3) show the values for the entire sample, columns (4) and (5) the means for male and female borrowers, and columns (6) and (7) the means for male and female loan officers.

Appendix Table A2: Test for differences in borrower characteristics for the loan-conditionality sample

	Male loan officers			Female loan officers			
	Male borrowers	Female borrowers	t-statistic	Male borrowers	Female borrowers	t-statistic	t-statistic
Variable	(1)	(2)	(3) = (1) - (2)	(4)	(5)	(6) = (4) - (5)	(7) = (3) - (6)
Age applicant	41.09	41.86	1.97*	40.88	41.52	1.94*	0.49
Civil status (married = 1)	0.90	0.77	-5.78***	0.89	0.73	-11.17***	1.53
Applied loan amount (in USD)	3,065	2,764	-2.69***	3,202	2,758	-2.88***	0.34
Applied maturity (in days)	589	582	-1.01	626	614	-1.89*	-0.42
Total assets (in USD)	27,259	28,978	0.01	28,521	25,842	-1.65	1.55
Personal guarantee	0.13	0.17	0.17	0.20	0.18	-0.57	0.34
Mortgage collateral	0.09	0.11	-0.93	0.15	0.13	-2.01**	0.23
Chattel collateral	0.97	0.96	-0.10	0.95	0.94	-0.71	0.42
Working Capital	0.12	0.09	0.50	0.08	0.04	-3.21***	1.58
Fixed Assets	0.56	0.30	-6.67***	0.28	0.13	-4.65***	-1.02
Housing Improvement	0.19	0.32	1.83*	0.40	0.47	2.13**	-0.83
Consumption	0.13	0.29	4.61***	0.25	0.36	4.06***	0.72
Others	0.00	0.00	n.a.	0.00	0.00	1.16	-1.08
Observations	2,613	417	3,030	3,416	826	4,242	7,272

This table contains a test of difference in observable borrower characteristics for the loan-conditionality sample. Columns (1) and (2) show raw means for a set of borrower characteristics of male and female borrowers matched with male loan officers. Column (3) displays the t-statistic of a test of difference of the respective characteristic between male and female borrowers assigned to male loan officers. Columns (4) and (5) show raw means of male and female borrowers matched with female loan officers. Column (6) shows the t-statistic of a test of difference of the respective characteristic between male and female borrowers assigned to female loan officers. Column (7) reports the t-statistic of a test of difference-in-differences for the respective borrower characteristic. The t-statistics in columns (3), (6), and (7) are estimated conditioned on time and on sector fixed effects. Standard errors are clustered at the branch-sector-year level. ***, **, * indicate significance at the 1, 5, and 10 percent level, respectively.

Appendix Table A3: Arrears > 60 days and loan officer experience with opposite-sex borrowers

	Average effect	Low experience	High experience	Low experience	High experience	Low experience	High experience
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Gender×Gender	0.0019	0.0167	-0.0221	0.0166	-0.0222	0.0176	-0.0223
	[0.0109]	[0.0219]	[0.0139]	[0.0220]	[0.0139]	[0.0216]	[0.0138]
P-value of Wald test		0.0756		0.0758		0.0695	
Overall experience control	No	No	No	Yes	Yes	Yes	Yes
Overall experience squared control	No	No	No	No	No	Yes	Yes
Adjusted R-squared	0.0970	0.1180	0.0520	0.1180	0.0520	0.1190	0.0520
Observations	4,586	2,247	2,339	2,247	2,339	2,247	2,339

This table shows regression results with the measure arrears > 60 days as the dependent variable using the credit-demand sample. Arrears > 60 days is a dummy variable that takes on value one if a borrower went into arrears for more than 60 days at any time during the lifetime of her loan. All regressions include the control variables as specified in column (5) of Table 4 (including time and sector fixed effects), the results for these are omitted to save space. In column (1) we show the average effect on arrears > 60 days, in columns (2) to (7) we split the sample according to median loan officer experience with the opposite sex. In columns (2) and (3), we do not control for any loan officer experience, in columns (4) and (5) we control for overall loan officer experience, and in columns (6) and (7) we control for a second-order polynomial in overall loan officer experience. Standard errors clustered at the branch-sector-year level are shown in parentheses. ***, **, * indicate significance at the 1, 5, and 10 percent level, respectively.

Appendix Table A4: Arrears > 60 days, branch size, competition, and loan officer experience with opposite-sex borrowers

	Small branches (1)	Large branches (2)	Weak competition (3)	Strong competition (4)
Panel A: Branch size and competition				
Gender×Gender	-0.0007	-0.0025	0.0051	0.0060
	[0.0123]	[0.0181]	[0.0157]	[0.0193]
P-value of Wald test	0.9299		0.9676	
Adjusted R-squared	0.0800	0.0980	0.1150	0.0740
Observations	2,275	2,311	1,798	1,736
Panel B: Experience and branch size	Low experience		High experience	
	Small branches	Large branches	Small branches	Large branches
Gender×Gender	0.0027	0.0239	0.0089	-0.0378**
	[0.0363]	[0.0349]	[0.0062]	[0.0177]
P-value of Wald test		0.6486	0.8578	0.2818
Adjusted R-squared	0.1020	0.1110	0.0070	0.0700
Observations	1,196	1,051	1,079	1,260
Panel C: Experience and competition	Low experience		High experience	
	Weak competition	Strong competition	Weak competition	Strong competition
Gender×Gender	0.0303	0.0369	-0.0155	-0.0384**
	[0.0244]	[0.0352]	[0.0250]	[0.0187]
P-value of Wald test		0.8663	0.0684	0.0050
Adjusted R-squared	0.2230	0.0800	0.0020	0.0330
Observations	763	866	1,035	870

This table shows regression results with the measure arrears > 60 days as the dependent variable using the credit-demand sample. Arrears > 60 days is a dummy variable that takes on value one if a borrower went into arrears for more than 60 days at any time during the lifetime of her loan. All regressions include the control variables as specified in column (5) of Table 4 (including time and sector fixed effects), the results for these are omitted to save space. In columns (1) and (2) of Panel A, we split the sample according to the median branch size, in columns (3) and (4) according to competition measured as the ratio of branches over population in a specific region and year for the time period 2004-2006. In Panels B and C, we further split the samples according to the median loan officer experience with the opposite sex. Standard errors clustered at the branch-sector-year level are shown in parentheses. ***, **, * indicate significance at the 1, 5, and 10 percent level, respectively.