Discussion paper

SEX AND CREDIT: IS THERE A GENDER BIAS IN MICROFINANCE?

By
Thorsten Beck, Patrick Behr, Andreas Madestam

September 2011

European Banking Center Discussion Paper
No. 2011-027

This is also a CentER Discussion Paper
No. 2011-101

ISSN 0924-7815

TILBURG UNIVERSITY

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

Thorsten Beck[♣]
CentER, Dept. of Economics, Tilburg University and CEPR

Patrick Behr[#]
EBAPE, Fundacao Getulio Vargas

Andreas Madestam[*]
IGIER and Department of economics, Bocconi University

This version: September 2011

Abstract

This paper examines the effects of group identity in the credit market. Exploiting the quasi-random assignment of first-time borrowers to loan officers of a large Albanian lender, we test for own-gender bias in the loan officer-borrower match. We find that borrowers pay on average 29 basis points higher interest rates when paired with a loan officer of the other sex. The results indicate the presence of a taste-based rather than a statistical bias, as borrowers' likelihood of going into arrears is independent of loan officer gender. Ending up with an opposite-sex loan officer also affects demand for credit, with borrowers being 11.5 percent less likely to return for a second loan. The bias is more pronounced when the social distance, as proxied by difference in age between the loan officer and the borrower, increases and when financial market competition declines. This is consistent with theories that predict a taste-based bias to be stronger when the psychological costs of being biased are lower and the discretion in setting interest rates is higher. Taken together, the findings suggest that own-gender preferences can have substantial welfare effects.

JEL Classification: G21, G32, J16.
Keywords: Identity, interest rates, gender, loan officers, microfinance.

[♣] Department of Economics and European Banking Center, Tilburg University, P.O. Box 90153, 5000 LE Tilburg, The Netherlands, and CEPR, Email: t.beck@uvt.nl.
[#] Brazilian School of Public and Business Administration, Fundacao Getulio Vargas, Praia de Botafogo 190, 22253-900 Rio de Janeiro, Brazil, patrick.behr@fgv.br (corresponding author).
[*] IGIER and Department of Economics, Bocconi University, Via Roentgen 1, 20136 Milan, Italy, Email: andreas.madestam@unibocconi.it.

We are grateful to Tore Ellingsen, Marcel Fafchamps, Steven Poelhekke, Raghuram Rajan, David Yanagizawa-Drott, and seminar participants at Goethe University Frankfurt, Stockholm School of Economics, Tilburg University, the Development Economics Workshop in Bergen 2010, and University of Göttingen.

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 loan officers and borrowers. Microcredit is a case in point, with most clients being small and opaque, leaving the lending decision at the discretion of the loan officer. If bank 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 pricing. While access to formal sources of credit is often described as the main obstacle for the poor, the typically high interest rates for microloans can be a deterrence factor, too. Recent research shows that poor borrowers are very sensitive to small changes in interest rates (Karlan and Zinman, 2008). If a gender bias in the relationship between loan officers and borrowers results in higher interest rates, this may have negative repercussions not only for the cost of credit, but also for take up of loans by poor borrowers.

Using a large dataset of loan transactions from a microcredit lender in Albania, we study if there is a bias against borrowers of the opposite gender by their respective loan officer and the consequences of this bias for take up of loans. In particular, we assess if borrowers pay higher interest rates when matched with an opposite-sex loan officer and whether this has an effect on the take up of additional loans from the same lender. As our data include information on both the price and the loan performance in terms of arrears, we are able to distinguish between statistical bias (Phelps, 1972; Arrow, 1973), which implies higher interest rates for riskier borrowers, and taste-based bias or prejudice (Becker, 1957), which implies higher interest rates independent of the level of riskiness. A better understanding of which type of bias is operative has important implications for policy. Prejudice, being inconsistent with profit maximization, should motivate policymakers to promote competition-enhancing strategies as well as anti-discrimination literacy programs. Statistical bias, often consistent with profit maximization, should motivate policymakers to consider other policies to support disadvantaged minorities.

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 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 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 being the only factor driving assignment to a specific officer. Conditional on sector, the random assignment of borrowers to loan officers ensures that the unobservable borrower characteristics are the same across all loan officers, regardless of loan officer gender. In particular, we employ a difference-in-differences strategy and compare the difference in outcomes (for example, the interest rate) for male and female borrowers obtaining a loan from a male loan officer to the difference between male and female borrowers obtaining a loan from a female loan officer.[1]

Our estimates provide convincing evidence of an own-gender preference, with important repercussions for the subsequent loan demand.[2] Specifically, borrowers assigned to loan officers of the opposite rather than their own sex pay on average 29 basis points higher interest rates. In addition, there is no evidence that the loan officer-borrower gender match predicts the likelihood of falling into arrears, allowing us to distinguish between taste-based and statistical bias. If borrowers paying a higher interest rate were more likely to fall into arrears, this would imply a statistical bias. As this is not the case, our results imply a loan officer-specific taste for gender bias: loan officers charge higher interest rates to borrowers of the other gender although there is no difference in ex-post riskiness (or arrear probability).

Ending up with an opposite-sex loan officer also has a significant impact on take up of loans. First-time borrowers matched with a loan officer of the opposite gender and who, consequently, had to pay higher interest rates, are 6.9 percentage points less likely to return for a second loan. Given that 60 percent of the borrowers return to the same lender for a second loan, this is equivalent of an 11.5 percent decrease, a substantial economic effect. On top of paying higher interest rates on their first loans, there is thus a negative impact on the demand for credit. In addition, the negative effect of being matched with an opposite-sex loan officer is stronger in smaller branches, as measured by the number of loan officers employed per branch office. A possible interpretation is that smaller branches leave borrowers fewer options to find alternative loan officers, as the likelihood of being matched with the same opposite-sex loan officer is higher when returning for a second loan.

[1] The identifying assumption of the difference-in-differences estimator requires that the unobservable characteristics are the same in the two differences.

[2] This may be due to either male or female (or both) favoring borrowers of their own gender, or disfavoring those of the other gender.

To better understand some of the possible mechanisms driving the taste-based bias, we examine heterogeneous outcomes related to our findings. We show that the effect on the interest rate partially can be explained by loan officer and borrower age. In particular, loan officers younger than the median (loan officer age) charge higher interest rates when matched with an opposite-sex borrower. Meanwhile, above median-aged borrowers pay higher interest rates when interacting with a loan officer of the opposite gender. One interpretation of these results is the concept of *social distance*. 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.

A second, complementary, mechanism is that the bias is more pronounced in situations when loan officers have additional discretion in setting interest rates. Specifically, we measure discretion in terms of competition from other financial institutions. While low competition per se has a statistically insignificant (although positive) effect on the bias, it has a substantial impact once we focus on below-median aged loan officers. The bias is stronger for this category of loan officers when there is less competition. This is consistent with theories predicting that competition can erode a taste-based bias. For instance, Becker (1957) argues that discrimination is costly and harder to sustain in competitive environments.[3]

[3] In a developed-country setting, Black and Strahan (2001) and Levine et al. (2010) also find that discrimination against female and minority-race employees fell in the U.S. after a branch deregulation resulted in higher competition.

Taken together, the results indicate that own-gender preferences have non-trivial welfare effects for consumers (higher interest rates and lower take up) and providers of credit (lower long-run profits through diminished demand).[4] We do not find any positive effects of within-group matching of borrowers and loan officers.

This paper speaks to several literatures. First, while there is no research explicitly examining the existence of a gender bias in microfinance, Karlan and Zinman's (2008) study is especially relevant for the present analysis. Using experimental field data from a South African lender, where the interest rate offers were randomized, Karlan and Zinman show that clients were 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 have important effects on credit take up. This is indeed what we find.

Second, our paper also contributes to the empirical literature examining poor peoples' barriers to credit by identifying the existence of asymmetric information in the credit market (Karlan and Zinman, 2009). It further links to the work looking at mechanisms that can improve access to finance, such as social capital (Karlan, 2007; Feigenberg et al., 2011) and joint liability (Giné and Karlan, 2011). The setting of the current study, a for-profit lender in Albania, extending credit 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.

Third, 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

[4] Unfortunately, our data do not allow us to construct precise measures of short-term profits because the bank does not collect the necessary information on actual repayments, the exact cost per loan, or the loan recoveries in case a loan defaults. Also, we would not be able to trace any recovered amounts back to the loan officers in our dataset, since the loan responsibility switches to a special loan recovery department once a loan is in arrears for more than 60 days. In addition, we do not have information about the gender of the people working in the loan recovery department.

(Stoll et al., 2004), and in sports (Price and Wolfers, 2010; Parsons et al., 2011), our paper is the first that seeks to account for an own-gender bias in lending. More generally, there is a literature documenting biases in credit markets, predominately using U.S. 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).[5] While these studies on minority or gender bias have their merit, they suffer from two main shortcomings. First, as both statistical and taste-based bias can imply higher rejection rates for minority clients, it is often not clear what type of bias is being identified (see Berkovec et al., 1998 and Han, 2004 for exceptions though).[6] Second, and more importantly, existing work does not contain all the characteristics that lenders observe when approving the loans and setting the contract terms. Hence, one can never be sure that the loan applicants being compared are truly similar from the loan officers' perspective. As a consequence, any measured differences in outcomes could be attributed to these factors unobserved by the researcher. Using the quasi-random assignment of borrowers to loan officers in our sample allows us to address these shortcomings. Our dataset also permits for a cleaner test of an interest rate gap across borrower gender as we have information on loan performance.[7] Moreover, previous work does not combine supply and demand-side analysis, that is, the effect of the gender bias on take up of financial services.

Finally, this paper fits into a small but growing literature examining the importance of loan officers in lending stressing long-term relationships, compensation schemes, loan officer

[5] For a survey of this literature, see Bellucci et al. (2010).
[6] Becker (1993) writes (referring to influential work by the Federal Reserve Bank of Boston [Munnell et al., 1996]) that studies trying to identify a gender bias require examining not only credit denial, but also the default, interest charges, late payments, and other determinants of the loans profitability.
[7] This is of particular importance as variation in interest rates may simply be driven by different degrees of risk associated with loans given to borrowers of the same or opposite gender that materialize ex-post. Though we are able to measure only ex-post risk, and borrowers' risk behavior can be influenced by the interest rate, this would actually bias our estimations towards finding a higher arrear probability among borrowers matched to loan officers of the other gender.

rotation, and loan officer gender for loan performance (Agarwal and Wang, 2009; Hertzberg et al., 2010; Beck et al., 2011; Drexler and Schoar, 2011).

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 the main empirical results, while section four explores different mechanisms that help interpret our results. Section five concludes.

2 Data and identification strategy

This section provides background information about the lender, our identification strategy, as well as a first look at the data, including descriptive statistics.

2.1 Institutional background information

We use loan-level data from a large for-profit commercial lender serving individuals and small- and medium-sized enterprises in Albania. The dataset includes nearly 8,000 loans given by the lender over the period January 1996 to December 2006. In addition, our data contains information on 279 loan officers and covers 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.

Loan officers working for this lender have discretion on the rejection and approval of a loan application as well as setting the interest rate. The loan 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 loan officer intensifies monitoring, for instance, by calling 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 loan officer from approval over loan condition setting to its performance in terms of arrears up to 60 days, but not beyond that point in time as we lack information about the gender of the loan officers working in the loan recovery department.

Assignment of borrowers to loan officers is based on the availability of loan officers in the respective branch when the borrower arrives.[8] Specifically, first-time borrowers cannot freely choose a loan 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 a loan officer with previous experience in handling borrowers from this business sector. Since male and female loan officers or borrowers potentially specialize in certain sectors, this needs to be accounted for. The next subsection outlines our identification strategy and how we account for the potential loan officer and borrower specialization in certain business sectors.

2.2 Identification strategy

To study the impact of the interaction between loan 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 the difference-in-differences estimation, we compare the difference in outcomes (interest rate, arrear probability, and take up of a second loan) for male and female borrowers obtaining a loan from a male loan officer to the difference between male and female borrowers obtaining a loan from a female loan officer.

The identifying assumption is that the difference between male and female borrowers screened and monitored by male loan officers is similar to the difference between male and female borrowers screened and monitored by female loan officers, controlling for the

[8] All loan officers work full-time, so that it does not matter which day of the week a borrower arrives.

respective sector of activity of the borrower. 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.[9] 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 characteristics of any 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 test whether borrower assignment is random with respect to loan officer gender, we proceed in two complementary ways. First, we regress loan officer gender on borrower gender. This check shows whether female borrowers are more likely to be matched to a male loan officer conditioning on sector and time fixed effects. We also interact the female borrower dummy with the sector dummies to test for the matching within sectors, taking into account that loan officers might specialize in certain sectors. Specifically, we estimate

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

where gl_j is a gender dummy taking the value one for male loan officers, gb_i is a gender dummy taking the value one for female borrowers, ϕ_s is sector dummy, and μ_t is a year dummy. In sum, the assumption is that $Cov(gl_j, gb_i | z) = 0$, where z is a vector of the relevant fixed effects. We cluster the standard errors ε_{ijts} at the branch-by-sector level, as borrowers in the same sector and same branch are likely to share background characteristics as well as be exposed to the same loan officer and branch environment.

[9] 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 that the difference-in-differences in the observable traits are not significant.

The results in Table 1 show that within each business sector borrower gender cannot predict loan officer gender. In the first column, we estimate regression (1) not including any fixed effects. We then gradually add time, sector, and sector-borrower fixed effects. As column (4) of Table 1 reveals, once we account for specialization, borrower gender cannot explain loan officer gender. The point estimate, 0.001, is positive, insignificant, and close to zero. This suggests that the assignment of borrowers to loan officers is as good as random within the sectors.

While we believe that this is the most stringent randomization test, we perform a second check where we verify if male relative to female borrowers vary in their characteristics depending on whether they are matched with a loan 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 loan officer. We utilize the following regression:

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

where y_{ijts} is one of the relevant characteristics of borrower *i* contracting with loan officer *j* in year *t* in sector *s*, with the other variables being the same as in specification (1). The coefficient β indicates whether there is a difference between male and female borrowers screened and monitored by male relative to female loan officers. 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 have socio-demographic borrower information (civil status, employment status: that is, self-employed or – at least partly – employed wage earner, age, size of the borrower's household, phone availability). The data also include information on the loan terms apart from the interest rate (applied loan size in U.S. Dollars [USD], applied loan maturity in days, availability of a personal, mortgage, or chattel guarantee), 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, leverage).

The differences in Table 2 suggest a random assignment of borrowers to loan officers of the same or opposite gender. Specifically, columns (3) and (6) display the t-statistic of the relative difference across male and female borrowers for male and female loan 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, only household size and civil status enter (weakly) significantly once we compare borrower-loan officer pairs conditional on sector, sector-borrower, and time fixed effects. The data show that male borrowers form part of larger households, though the economic effect is small, and are more often married. None of the other observable differences are significant. Together, the results in Tables 1 and 2 lend credibility to our identification strategy.

2.3 Sample composition and descriptive statistics

When analyzing treatment differences we focus on three outcomes: annual interest rate charged, the likelihood of going into arrears, and the likelihood of applying for a second loan with the lender.

In order to examine potential interest rate differentials across borrower and loan officer gender, we also analyze loan performance because differences in ex-post loan risk may explain why borrowers of different gender are charged different rates. Specifically, we define loan performance as the probability that a loan is in arrears for more than 30 days at any time over the life of the loan (hereafter arrear probability). The 30 day arrear threshold is an important variable and its use as a risk/performance measure is quite common in microfinance (the portfolio at risk of a microcredit lender is usually reported using this risk definition). Also, the lender increases the monitoring intensity once a loan is in arrears for more than 30 days, for example by contacting the borrower more often on the phone or even visiting her. As a robustness check, we run all the arrear regressions using the 60 days in arrears definition with unchanged results. Using the internationally recognized definition of a default (90 days in arrears threshold), is not feasible in our case since, as mentioned above, the loan responsibility changes after 60 days and rests with a special loan recovery department.

While the default probability could be endogenous to the interest rate, with higher interest rates pushing the borrower towards riskier behavior thus undermining repayment probability, this would bias our estimations towards finding a statistical rather than taste-based bias. Specifically, if we find that borrowers matched to loan officers of the opposite gender are given higher interest rates, this could induce a higher arrear probability.[10]

For our regression analyses, we restrict the data in several 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. The use of repeat loans is more problematic because borrowers already have experience with the lender and may select a certain loan officer type, inducing a systematic bias. In addition, the effect of the gender bias on take up of a repeat loan introduces a selection bias in the sample of repeat borrowers. Also, 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. Focusing on the first loan by each loan applicant yields the cleanest test of possible gender-specific interest rate and performance differentials.

[10] It is possible, however, that loan officers of a certain gender exert different monitoring efforts across borrower gender. Hence, an increase in the arrear probability induced by an interest rate hike could be traded off against a higher monitoring effort on part of the loan officer charging the higher interest rate. This would not invalidate any results with regard to the own gender bias, but might explain why ex post arrear probabilities do not differ between the loan officer-borrower gender pairs.

Second, we drop loans with missing gender information on the borrower. For that purpose, we exclude loans by borrowers classified as legal entities in the database as we lack information on borrower gender. Third, we drop loans with amounts of less than 100 and more than 20,000 USD. While low values may be the result of miscoded entries we want to exclude large loans that do not fit the definition of small individual and microloans. In addition, we exclude loans with an unreasonable borrower age (younger than 18 or older than 75 years). This reduces our sample to 7,885 loans for the baseline regression analysis.

The descriptive statistics in Table 3 shows that 17 percent of the loans in the sample are given to female borrowers, while 55 percent are managed by female loan officers.[11] Around 50 percent in our sample are loans managed by an opposite-sex loan officer. The average interest rate is 14 percent and the interest rate is 30 basis points higher for male borrowers and 40 basis points higher for male loan officers. Five percent of loans go into arrears, with a lower likelihood for female borrowers (3.7 percent) and female loan officers (4.9 percent).

3 Main findings

This section first presents our baseline findings for the interest rates and the arrear probability.[12] Then we examine the effect of own-gender preference on take up of further loans.

[11] The relatively high share of female loan officers working for the bank is in line with recent 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.

[12] While we have information on rejected loan applications, more than 95 percent of first-time applicants are granted a loan, yielding little variation to be exploited. When estimating cross-gender differences in an approval regression, however, we cannot find any evidence for the gender bias.

3.1 Baseline result

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

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

where *O* is the outcome of interest (annual interest charged, likelihood of going into arrears, and likelihood of applying for a second loan), $\rho, \phi, \mu, \eta,$ and κ are loan officer, sector, time, cohort, and, branch dummies, respectively. The parameter $\mathbf{x}$ is a vector of loan officer, borrower, and loan characteristics, though as shown above, $gb_i gl_j$ is orthogonal to $\mathbf{x}$, and the consistency of β does not depend on the inclusion of the covariates in the model. The subscripts *i, j, k, s*, and *t* denote borrower, loan officer, branch, sector, and year, respectively.

We first examine the effect on interest rates and arrears. *Interest Rate* is the annual interest rate charged on the loan and *Arrear* is a dummy variable taking the value one if the loan has been in arrears for more than 30 days at any point during the loan. We use OLS for all three outcome variables, despite of *Arrear* being a binary variable because when using a non-linear model, we will lose loan officers that have not experienced any arrears on their loans, respectively, those who have only experienced arrears on their loans. Our findings, however, are confirmed when considering the coefficient estimates of probit models. As before, we cluster the standard errors at the branch-by-sector level.

The coefficient β estimates the impact of opposite-sex loan officers on a borrower's interest rate (relative to own-gender loan officers). Put differently, it measures the differential effect of a female (male) borrower paired with a male (female) loan officer compared to a female (male) borrower matched with a female (male) loan officer. Table 4 presents the findings using interest rates and arrear probability as dependent variables. The results in Panel A refer to the interest rate regressions and the results in Panel B to the arrear regressions.

The results in Panel A of Table 4 show a significant difference in interest rates paid by borrowers assigned to loan officers of their own gender compared to borrowers assigned to loan officers of the opposite sex. In column (1) of Panel A, we report the estimated coefficient without controls and fixed effects. The point estimate of 0.0026 is significant at the five percent level. In columns (2) through (5) we add (i) loan officer (time-variant) specific variables, (ii) borrower specific variables, (iii) cohort fixed effects, (iv) branch fixed effects, and (v) branch and sector-specific trend variables.[13] In column (6) of Panel A, we also include the loan characteristics (approved loan amount, approved maturity). These are arguably endogenous to the outcome of interest. However, the point estimate and the standard error stay the same. The coefficient on the gender-gender interaction is significant in all specifications at least at the 5 percent level. It implies that borrowers assigned to opposite-sex loan officers pay on average a 29 basis points higher interest rate compared to borrowers who are matched with loan officers of the same gender.

Panel B of Table 4 shows that the identified interest rate differential is unjustified with respect to the arrear probability. The higher interest rates that borrowers pay when matched with a loan officer of the opposite gender could potentially be explained by the fact that they are riskier customers, which would indicate a statistical bias. The consistently insignificant coefficient estimate on $gb_i gl_j$ clearly indicates that there is no difference between female (male) borrowers' likelihood of falling into arrears, depending on whether they are screened and monitored by a male (female) as opposed to a female (male) loan officer. Note that borrowers' ex post risk behavior potentially could be influenced by the interest rate through a changed repayment burden. Since borrowers ending up with opposite-sex loan officers on average pay higher interest rates, this should actually bias our estimates toward finding a

[13] We control for branch-specific trends, as branches opened up during our period and may have evolved differently with respect to our outcome variables. We control for sector trends to take into account possible attitudinal changes specific to each sector that may drive our outcome variables.

higher arrear probability in these instances. However, the arrears are not affected by the loan officer-borrower gender match.

In sum, the results support the existence of a taste-based, rather than a statistical bias, as the higher interest rates paid by borrowers when matched with a loan officer of the opposite gender do not seem to be driven by a higher level of riskiness.[14]

3.2 Taste-based bias and loan take up

Next we explore the consequences of the taste-based bias identified in the previous section on loan demand. This is an issue of great importance given the recent finding that poor borrowers are sensitive to increases in interest rates (Karlan and Zinman, 2008). In line with these results, we expect that borrowers that are matched with an opposite sex loan officer and, thus, pay higher interest rates, will react by demanding less credit, that is, applying less often for a repeat loan with the same lender.

Specifically, we examine the relationship between the likelihood of applying for a second loan, the loan officer matching, and the interest rate from the first loan. Overall, 60 percent of all first-time borrowers came back to the institution for a second loan during our sample period. While a large number of these non-returning customers might be due to the usual attrition and the lack of need for further loans, we investigate whether part of it can be explained by the loan-officer match in the first loan. We define a dummy variable that takes on value one if the borrower returned to the bank for at least one more time and zero otherwise. We account for 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 (the problem of right censoring). Hence, we compute the average maturity of all loans, which is 563 days, and end our sample period on December 31, 2006, less 563 days for the test. This reduces the sample

[14] We further tested for variation in the taste-based bias across the different business sectors but do not find any significant difference.

size from 7,885 to 5,445 observations.[15] We then run specification (3) with a dummy indicating whether a borrower returned to the bank as the dependent variable, including the controls and the fixed effects corresponding to model (V) in Table 4.[16]

The results in Table 5 show that borrowers that (i) were matched with a loan officer of the other gender and/or (ii) were charged a higher interest rate on their first loan, are less likely to apply for a second loan. The economic effects are substantial: being matched with a male (female) loan officer results in a 6.9 percent points lower likelihood of female (male) borrowers applying for a second loan. The impact of the gender mis-match on take-up is large given that only 60.3 percent of all first-time borrowers apply for a second loan. The results in column (2) suggest that the higher interest rate is an important reason of why borrowers matched to a loan officer from the opposite gender do not return; the interest rate charged on the first loan enters significantly, while the significance of $gb_i gl_j$ drops to the 10 percent level. Column 3 investigates whether the interest rate acts as a mediating factor between the loan officer-borrower match and take up. To do this, we re-estimate the baseline specification by IV/2SLS, with $gb_i gl_j$ as the excluded instrument for the interest rate and the remaining interaction terms, the fixed effects, and the covariates as controls. The reasoning behind this specification is that it uses the gender pair as exogenous to derive the economic effect of the gender-induced interest rate hike. The impact on take up for borrowers ending up with an opposite-sex loan officer is significant at the ten percent level. The point estimate of 20.18 implies that a one percentage point increase in the interest rate reduces take up by 20 percentage points. In unreported regressions, we confirm our findings for a sample of borrowers that did not fall into arrears on their first loan, with coefficient estimates of almost the same size.

[15] The results do not change if we use the median maturity instead of the mean maturity. We also re-ran the regressions of Table 4 and obtain the same results for this smaller sample.

[16] In some of the tests reported below we exclude the branch trends. This is necessary because otherwise the regressions include too many sparse indicator variables and it is impossible to compute the standard errors.

Overall, these findings suggest that beyond the cost impact that higher interest rates have for borrowers matched to a loan officer from the other gender, there is a negative effect of this taste-based bias on take-up rates. This supports findings by Karlan and Zinman (2008) on the interest rate sensitivity of loan take up and shows that the own-gender bias can have a significant impact on demand for credit by borrowers.

Columns 4 and 5 provide additional evidence for the effect of the own-gender bias on take up. We conjecture that borrowers that were subject to the taste-based bias are less likely to return for a second loan in branches where the probability of being matched with the same (opposite-sex) loan officer is higher. In larger branches with many loan officers, there is a reasonable chance that borrowers might be matched with a different loan officer (due to rotation or work load distribution) and hopefully of the same gender, so borrowers might be more enticed to return. In small branches, this is less likely. Specifically, for each year we divide the sample into bank branches with above or below the median number of loan officers (our size measure). This implies that we explore variation in terms of employees across branches and time (allowing us to keep the branch fixed effects). Also, we confirm that our first randomization test [specification (1)] holds for the subsamples.[17]

Both column 4 and column 5 show a significant and negative effect of the gender mismatch on take up of a second loan for branches with below median number of loan officers. The size of the coefficient is more than three times the size of the regression for the full sample, suggesting a large economic effect for small branches. A Wald test confirms that the coefficient estimates across the two regressions are significantly different from each other. When testing for an interest rate differential across branches of different size, we find a higher interest rate for borrowers matched with loan officers of the opposite gender for *both* small and large branches. Hence, while borrowers suffer from own-gender bias across branches of

[17] Results for this and all other randomization tests for the subsamples are available on request from the authors.

all sizes, the repercussions of this bias for take-up of future loans can only be observed in small branches. We see this as additional evidence for our hypothesis of a negative impact of own-gender preference on take up, as borrowers are less likely to be matched with a different loan officer for the second loan in smaller branches with fewer loan officers.

4 Mechanisms and channels of the taste-based bias

The previous sections showed a significant own-gender bias in loan officers' setting of the interest rate and the consequences of this bias for borrowers' take up of additional loans with the lender. In what follows, we investigate two specific mechanisms and channels through which the own-gender bias may work.

4.1 Social distance and the own-gender bias

The first hypothesis that we explore is the idea that the bias varies with the *social distance* between the loan officer and the borrower. While loan officers may have stereotype beliefs about the opposite gender, consistent with studies of cognitive behavior, the bias can involve psychological costs that increase when the biased party is faced with counter-examples that are conflicting with the gender stereotype (Goodwin et al., 2000; Blair, 2002). This cost arguably rises in cases where it is easier for the biased party to relate to the individual being biased against. For example, a male loan officer may have stereotype beliefs about women. However, if he interacts with a female borrower of the same age, he is more likely to identify with her and, hence, experience a higher psychological cost coming from the bias. Meanwhile, stereotyping someone of the opposite sex that is older (and, hence, quite different) can be felt as less costly. Therefore, the bias is more pronounced when the *social distance* between the loan officer and the borrower increases. The concept of *social distance* as a driver of the bias also implies that experience on the job per se should not matter, that is, the bias should be independent of the specific job experience.

Consistent with our hypothesis, we anticipate that it is more difficult for young loan officers to relate to old borrowers as the *social distance* in this case is expected to increase.[18] We test for this by dividing the sample according to the median loan officer age (24 years) and to the median borrower age (41 years).[19] We then run the baseline regression using interest rate and arrears as dependent variables separately for the resulting subsamples and test for significant difference across the samples.[20]

The results in columns (1) and (2) of Table 6 show that the own-gender bias is present in the case of older borrowers (above 41 years), with a statistically significant difference between the two regression coefficients at the 7 percent level according to the Wald test. The point estimate for the differential interest rate charged for older borrowers interacting with an opposite-sex loan officer is 0.0041. As the loan officers in the sample are considerably younger than the borrowers, this is in line with the prediction that the bias should be more pronounced if the *social distance* is bigger. Similarly, the results in columns (3) and (4) show that the bias is larger in the case of younger loan officers (below 24 years), who charge considerably higher interest rates if matched with borrowers of the opposite gender, while older loan officers do not: the interest rate coefficient for young loan officers, 0.0051, is significant at the one percent level, whereas the point estimate for older loan officers is insignificant, positive, and close to zero (the Wald test of difference across the two coefficients confirm that they are significantly different at the five percent level). In addition,

[18] We also split the sample according to other borrower and loan officer characteristics, but do not find any significant difference across groups.

[19] While we expect the effect to exist also in the case of older (above the median age) loan officers matched with younger (below the median age) borrowers, our sample does not permit us to test for this because the loan officers are on average much younger than the borrowers. Above median-aged loan officers are 28 years old while below-median aged borrowers are almost 33. This can be compared to the polar case reported above, where below median aged loan officers are 23 years old while above-median aged borrowers are about 50.

[20] As above, the identifying assumption for the two subsamples holds because the female gender dummy is insignificant when implementing the first randomization test [equation (1)].

the differential behavior of loan officers across age groups is not related to their work experience, as defined by the number of loan transactions handled at the time of each new loan contract. Specifically, columns (5) and (6) show no significant difference below and above the median experience (121 loans handled). These results demonstrate that work experience does not change loan officers' taste for stereotyping, while closeness in age does. This provides further support to the conclusion that the bias is intrinsically motivated, rather than based on other considerations such as profit.

The Panel B regressions do not show any significant differences in arrear probability across borrowers or loan officers of different age or experience. Thus, the bias that we identify cannot be justified in terms of a higher level of borrower riskiness.

4.2 Competition and the own-gender bias

We also conjecture that the bias varies with financial market competition. Becker (1957) argues that a gender bias should be more pronounced when the degree of competition is low. Specifically, when there is little competition, loan officers have more discretion in expressing the taste-based bias since the borrowers have fewer outside options and, hence, less bargaining power. We therefore expect the bias to be more pronounced when competition decreases.[21]

To explore the role of competition, we map additional competition data to our dataset. Specifically, we retrieved information on the universe of registered bank branches and the population in Albania by region and time, and merged this with our loan-level data. The data (provided by the Albanian central bank) are available for the years 2004-2006, thus covering roughly 75 percent of the loan transactions, reducing the sample size to 5,704 observations.

[21] The reasoning is analogous to 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.

We then construct a competition proxy, defined as the number of bank branches per capita by region and year, and divide the sample according to the observations from regions with a branch-ratio below and above the median (as defined by the regions covered by the 21 branches in our data). The regions below (above) the median are the regions where we expect that competition from other financial institutions is low (high). We also include the absolute number of bank institutions in a specific region and year as an additional control. These sub-sample regressions can only be run for the interest rate and arrears variables, but not to test differences in take up as the sample size becomes too small.[22]

Table 7 shows that the taste-based bias varies as theory would predict. In order to identify our effects, we explore time variation in the ratio of branches per capita. First, we test for differences across branches in regions with different degrees of competition. While columns (1) and (2) show that less competition increases the size of the taste-based bias, the difference is not statistically significant across branches in areas with high and low competition. However, when we focus on the sample of young loan officers (that is, below the median age of 24), we find evidence of a taste-based bias in branches that face low competition from other banks [columns (3) and (4)]. The coefficient in the regression for branches in high-competition areas is not only insignificant (which might be due to the small number of observations) but also smaller in magnitude than in the regressions for branches in low-competition areas.[23] The last two columns in Table 7 show a similar difference when we focus on small branches as defined by the number of loan officers employed per branch. We find that loan officers are more likely to exercise the taste-based bias in small branches with little competition, with the effect being three times as large in these cases, as compared to smaller branches that face above median competition. The point estimate in the former case is

[22] In the case of take up, our sample is restricted by the sample period – because we are unable to observe the second loan take up for first-time borrower in 2006 and late 2005, while the competition data only includes 2004 through 2006.
[23] Focusing on above-median age borrowers and splitting the sample according to competition does not yield any significant differences; results are available on request.

0.0097 and significant at the one percent level, while it drops to 0.0031 in the case of above median competition. The regressions in Panel B show, as before, that there are no differences in arrears across borrower-loan officer matches, regardless of the sample split.

Together, these results confirm that competition or the lack thereof is one of the drivers of the own-gender bias, though only in conjunction with *social distance* as proxied by age. Young loan officers are more likely to charge higher interest rates to borrowers of the opposite gender in branches that face little competition from other financial institutions.

5 Conclusion

Our results suggest that own-gender preferences affect credit market outcomes. In particular, using a rich loan-level dataset from an Albanian microcredit lender, this paper has three main findings. First, we identify an own-gender bias in the setting of interest rates in microlending. Specifically, borrowers matched with a loan officer of the other gender pay, on average, 26 to 29 basis points higher interest rates than if matched with a loan officer of the same sex. Second, the bias we identify is taste based rather than statistical, as there is no ex-post difference in riskiness across borrowers matched to a loan officer of the same or the opposite gender. Third, the own-gender bias has negative repercussions for take up of further loans. Borrowers matched to loan officers of the opposite sex are 6.9 percentage points less likely to return for another loan from the same lender.

We also investigate the sources of the taste-based own-gender bias. We argue and show that the bias is more pronounced when the *social distance* between the loan officer in charge of the loan and the borrower is larger. In addition, the bias increases when loan officers have more discretion (as measured by financial market competition) in setting interest rates, i.e., applying the bias.

Understanding in-group identity, in the form of own-gender preferences, has at least two implications for the functioning of the credit market. First, identity may affect the organizational design of financial institutions. Specifically, matching loan officers to borrowers of the same gender can have repercussions by reducing taste-based biases. Similarly, the pairing of loan officers and borrowers according to proxies such as age may also help eliminate existing biases. 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.

References

Agarwal, S., Wang, F.H., 2009, Perverse Incentives at the Banks? Evidence from a Natural Experiment, Working Paper, Federal Reserve Bank of Chicago.

Akerlof, G.A., Kranton, R. E., 2000, Economics and Identity, Quarterly Journal of Economics 115(3), 715-753.

Armendariz de Aghion, B., Morduch, J., 2005, The Economics of Microfinance, MIT Press.

Arrow, K. J. 1973. The Theory of Discrimination. In Orley Ashenfelter and Albert Rees, eds. *Discrimination in Labor Markets.* Princeton, N.J.: Princeton University Press, 1973. 3-33.

Beck, T., Behr, P., Güttler, A., 2011, Gender and Banking: Are Women better Loan Officers?, Working Paper.

Becker, G., 1957. The Economics of Discrimination. The University of Chicago Press, Chicago.

Becker, G., 1993, The Evidence Against Banks Doesn't Prove Bias, Business Week.

Bellucci, A., Borisov, A.V., Zazzaro, A., 2010, Does Gender Matter in Bank-Firm Relationships? Evidence from Small Business Lending, Journal of Banking and Finance 34, 2968-2984.

Benjamin, D.J., Choi, J.J., Strickland, A. J., 2010, Social Identity and Preferences, American Economic Review 100(4), 1913-1928.

Berkovec, J., Canner, G., Gabriel, S., and Hannan T., 1998, Discrimination, competition, and loan performance in FHA mortgage lending, Review of Economics and Statistics, 80, 241-250.

Black, S.E., Strahan, P., 2001, The Division of Spoils: Rent Sharing and Discrimination in a Regulated Industry, American Economic Review 91(4), 814-831.

Blair, I. 2002. The malleability of automatic stereotypes and prejudice. Personality and Social Psychology Review 6, 242-61.

Blanchard, L., Zhao, B., Yinger, J., 2008, Do lenders discriminate against minority and woman entrepreneurs?, Journal of Urban Economics, 63(2), 467-497.

Blanchflower, D.G., Levine, P.B., Zimmerman, D.J., 2003, Discrimination in the Small Business Credit Market, Review of Economics and Statistics 85(4), 930-943.

Cavalluzzo, K. S and L. Cavalluzzo, L. C, 1998. Market Structure and Discrimination: The Case of Small Businesses, Journal of Money, Credit and Banking 30, 771-92.

Chen, Y., Li, S.X., 2009, Group Identity and Social Preferences, American Economic Review 99(1), 431-457

Donohue, J. and S. Levitt, 2001. The impact of race on policing and arrests, Journal of Law & Economics 44, 367-9.

Drexler, A. and Schoar, A., 2011, Does Soft Information Matter? Evidence From Loan Officer Absenteeism, Working Paper, MIT.

Feigenberg, B., Field, E., Pande, R., 2011, The Economic Returns to Social Interaction: Experimental Evidence from Microfinance, Working Paper, Harvard.

Giné, X., and Karlan, D., 2011, Group versus Individual Liability: Short and Long Term Evidence from Philippine Microcredit Lending Groups, Working Paper, Yale.

Goodwin, S., A. Gubin, S. Fiske, and V. Yzerbyt. 2000. Power can bias impression processes: stereotyping subordinates by default and by design. Group Processes & Intergroup relations 3, 227-256.

Han, S. 2004. Discrimination in lending: theory and evidence, The Journal of Real Estate Finance and Economics 29, 5-46.

Hertzberg, A., Liberti, J.M., Paravisini, D., 2010, Information and Incentives Inside the Firm: Evidence from Loan Officer Rotation, Journal of Finance 65, 795-828.

Karlan D., 2007, Social Connections and Group Banking. Economic Journal 117, F52-F84.

Karlan, D. and J. Morduch. 2009. Access to finance. In: Handbook of Development Economics 5. Elsevier.

Karlan D. and J. Zinman. 2008. Credit elasticities in less-developed economies: implications for microfinance. American Economic Review 98, 1040-68.

Karlan D. and J. Zinman. 2009. Observing unobservables: identifying information asymmetries with a consumer credit field experiment. Econometrica 77, 1963-2008.

Ladd, H. 1998. Evidence on discrimination in mortgage lending. Journal of Economic Perspectives 12, 41-62.

Levine, R., Levkov A., and Rubinstein Y., 2010, Racial Discrimination and Competition, Working Paper, Brown University.

Munnell, A.H., G.M.B. Tootell, L.E. Browne, J. McEneaney. 1996. Mortgage lending in Boston: interpreting HMDA data, American Economic Review, 86, 25–53.

Parsons, C.A., Sulaeman, J., Yates, M.C., Hamermesh, D.S., 2011, Strike Three: Discrimination, Incentives, and Evaluation, American Economic Review 101 (3), 1410-1435.

Phelps, E. 1972. The statistical theory of racism and sexism. American Economic Review 62, 659-61.

Price, J. and J. Wolfers, 2010, Racial discrimination among NBA referees. Quarterly Journal of Economics 125, 1859-87.

Ross, S.L. and J. Yinger. 2002. The Color of Credit: Mortgage Lending Discrimination, Research Methodology and Fair-Lending Enforcement. Cambridge, MA: MIT Press.

Statistical Institute of Albania, 2007, Women and Men in Albania 2006, downloadable at www.instat.gov.al.

Stoll, M. A., S. Raphael, and H. J. Holzer. 2004. "Black Job Applicants and the Hiring Officer's Race." Industrial and Labor Relations Review, 57, 267-287.

Welch, S., M. Combs and J. Gruhl. 1988. Do black judges make a difference? American Journal of Political Science 32, 125-36.

Table 1: Test of random assignment

	(1)	(2)	(3)	(4)
Borrower gender	-0.1103***	-0.0877***	-0.0306	0.0011
	(0.0364)	(0.0275)	(0.0188)	(0.0492)
Time FE	No	Yes	Yes	Yes
Sector FE	No	No	Yes	Yes
Sector by borrower FE	No	No	No	Yes
Observations	7,891	7,891	7,891	7,891

In this table we regress the loan officer gender on the borrower gender. The dependent variable is a dummy variable that takes on value one if the loan officer is male. The main dependent variable is a dummy variable that takes on value one if the borrower is female. In column (1) we do not include any further control variable. The column (2) regression adds time fixed effects, the column (3) regression further adds sector fixed effects, and the column (4) regressions adds sector-borrower fixed effects. Standard errors that are clustered at the branch-sector level are shown in parentheses. ***, **, * indicate significance at the 1, 5, and 10 percent level, respectively.

Table 2: Test for differences in borrower characteristics

	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	41.30	41.99	0.26	40.88	41.46	1.14	0.79
Wage earner	0.46	0.76	1.97*	0.75	0.91	0.59	1.59
Civil status	0.91	0.77	-2.65***	0.89	0.73	-2.75***	1.97*
Household size	5.45	4.77	-7.51***	5.23	4.50	-3.91***	2.09**
Phone availability	0.95	0.92	0.14	0.93	0.91	0.92	-0.29
Applied amount	2,990	2,675	-0.98	3,193	2,769	0.03	-0.87
Applied maturity	585	580	-1.51	625	613	-0.02	0.04
Total assets	26,516	27,577	-2.59**	28,204	25,688	1.26	0.88
Leverage	0.02	0.02	4.13***	0.02	0.02	-2.45***	-0.25
Personal guarantee	0.11	0.16	0.69	0.19	0.18	-0.08	0.28
Mortgage guarantee	0.08	0.10	1.96*	0.15	0.13	-0.52	0.02
Chattel guarantee	0.98	0.96	-1.91*	0.95	0.94	-0.30	0.53
Working Capital	0.10	0.08	0.90	0.08	0.03	-1.59	0.91
Fixed Assets	0.48	0.27	-2.48**	0.27	0.12	-0.24	-1.13
Mixed	0.15	0.10	0.96	0.03	0.02	1.69*	-0.21
Housing Improvement	0.16	0.29	0.44	0.38	0.46	-1.32	0.07
Consumption	0.11	0.26	2.35**	0.24	0.35	2.33**	0.74
Others	0.00	0.00	n.a.	0.00	0.00	-1.07	-1.46
Observations	3,057	464	3,521	3,524	846	4,370	7,891

This table contains a test of difference in observable borrower characteristics using a difference-in-difference approach. Columns (1) and (2) show raw means for a set of borrower characteristics of male and female borrowers that are matched with male loan officers. Column (3) shows the t-statistic of a test of difference of the respective borrower characteristic between male and female borrowers with male loan officers. Columns (4) and (5) show raw means of male and female borrowers that are matched with female loan officers. Column (6) shows the t-statistic of a test of difference of the respective borrower characteristic between male and female borrowers with female loan officers. Column (7) shows the t-statistic of a test of differences-in-differences for the respective borrower characteristic. The t-statistics in columns (3) and (6) are estimated conditioned on time, sector, and sector-borrower fixed effects. The t-statistics in column (7) are estimated additionally conditioning on sector-loan officer fixed effects. ***, **, * indicate significance at the 1, 5, and 10 percent level, respectively.

Table 3: Descriptive statistics

Variable	(1) Mean	(2) SD	(3) Median	(4) Male borrower	(5) Female borrower	(6) Male LO	(7) Female LO
Interest rate	0.14	0.02	0.14	0.14	0.14	0.14	0.14
Arrears	0.05	0.23	0.00	0.06	0.04	0.06	0.05
Female	0.17	0.37	0.00	n.a.	n.a.	0.13	0.19
Civil status	0.87	0.33	1.00	0.90	0.75	0.89	0.86
Household size	5.21	1.60	5.00	5.33	4.59	5.36	5.09
Age applicant	41.17	10.29	41.13	41.07	41.65	41.39	40.99
Wage earner	0.65	0.48	1.00	0.61	0.86	0.50	0.78
Total assets	27,244	82,355	16,367	27,420	26,359	26,656	27,718
Leverage	0.02	0.07	0.00	0.02	0.02	0.02	0.02
Applied amount	3,038	2,955	2,086	3,099	2,736	2,948	3,111
Approved amount	2,727	2,861	1,961	2,780	2,458	2,641	2,796
Approved maturity	563	288	540	564	559	542	581
Phone availability	0.94	0.25	1.00	0.94	0.92	0.94	0.93
Personal guarantee	0.16	0.36	0.00	0.16	0.17	0.12	0.19
Mortgage guarantee	0.11	0.32	0.00	0.11	0.12	0.08	0.14
Chattel guarantee	0.96	0.19	1.00	0.97	0.95	0.98	0.95
Destination Working Capital	0.08	0.28	0.00	0.09	0.05	0.10	0.07
Destination Fixed Assets	0.33	0.47	0.00	0.37	0.17	0.45	0.24
Destination Mixed	0.08	0.27	0.00	0.08	0.05	0.14	0.03
Destination Housing Improvement	0.30	0.46	0.00	0.28	0.40	0.18	0.40
Destination Consumption	0.20	0.40	0.00	0.18	0.32	0.13	0.26
Destination Others	0.00	0.02	0.00	0.00	0.00	0.00	0.00
Production	0.18	0.38	0.00	0.20	0.07	0.31	0.07
Transport	0.15	0.36	0.00	0.17	0.05	0.17	0.13
Construction	0.67	0.47	1.00	0.63	0.88	0.52	0.80
Female LO	0.55	0.50	1.00	0.54	0.65	n.a.	n.a.
Age LO	25.54	4.53	23.89	25.62	25.13	26.74	24.58
Applications per LO	165.49	159.16	121.00	165.64	164.76	159.94	169.97

This table shows descriptive statistics (mean, standard deviation (SD), median) for the main dependent variables interest rate and arrears and the main control variables used in the regression analyses. The columns (1)-(3) show the values for the entire sample, columns (4) and (5) the means for male and female borrowers, and colums (6) and (7) the means for male and female loan officers.

Table 4: Baseline results

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Interest rate						
Gender*Gender	0.0026**	0.0026**	0.0027***	0.0027***	0.0029***	0.0029***
	(0.0011)	(0.0012)	(0.0010)	(0.0009)	(0.0013)	(0.0013)
Adjusted R-squared	0,4407	0,5259	0,5266	0,5431	0,5509	0,5509
Observations	7.891	7.885	7.885	7.885	7.885	7.885
Panel B: Arrears						
Gender*Gender	0,0080	0,0077	0,0100	0,0100	0,0099	0,0099
	(0.0138)	(0.0161)	(0.0168)	(0.0165)	(0.0140)	(0.0140)
Adjusted R-squared	0,0718	0,0833	0,0844	0,0844	0,0841	0,0841
Observations	7.891	7.885	7.885	7.885	7.885	7.885
Loan officer fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Loan officer-specific variables	No	Yes	Yes	Yes	Yes	Yes
Borrower-specific variables	No	No	Yes	Yes	Yes	Yes
Branch fixed effects	No	No	No	Yes	Yes	Yes
Branch and sector trends	No	No	No	No	Yes	Yes
Loan characteristics	No	No	No	No	No	Yes

This table shows regression results with interest rate (Panel A) and arrear occurrence (Panel B) as dependent variables. The table only shows the coefficient for the gender-gender interaction, all further control variables are as indicated in the table, but omitted to save space. Each regression also includes time, sector, sector-borrower, and sector-loan officer fixed effects. Standard errors that are clustered at the branch-sector level are shown in parentheses. ***, **, * indicate significance at the 1, 5, and 10 percent level, respectively.

Table 5: Own-gender bias and take up of second loan

				Branch size	
	(1)	(2)	(3)	(4)	(5)
Gender*Gender	-0.0694**	-0.0663*		-0.2360***	-0.0163
	(0.0339)	(0.0341)		(0.0509)	(0.0295)
Interest Rate		-0.9171***	-20.18*		
		(0.3172)	(12.22)		
F-test			7.93		
			(0.008)		
P-value of Wald test				0.0000	
Adjusted R-squared	0.1965	0.1974	n.a.	0.2201	0.1862
Observations	5,445	5,445	5,445	2,184	3,261

This table shows regression results with loan take-up as dependent variable. Loan take-up is a dummy variable that takes on value one if borrowers returned to the bank for an additional loan application in case they had been granted a first loan. For this test, the sample period ends on December 31, 2006, less the mean maturity, which is 563 days for all approved loans in the baseline sample. All regressions include the control variables that are included in column (4) of Table 4, the results for these are omitted to save space. Further, branch and sector trends are included where possible. In column (2), we include the interest rate as additional control variable. In column (3), we show 2SLS estimates with Gender*Gender as the excluded instrument and the interaction terms, the fixed effects, and the covariates as controls. F-test statistics (with p-values in parentheses). In columns (4) and (5), we split the sample according to the median number of loan officers by branch and year. Each regression also includes time, sector, sector-borrower, and sector-loan officer fixed effects. Standard errors that are clustered at the branch-sector level are shown in parentheses. ***, **, * indicate significance at the 1, 5, and 10 percent level, respectively.

Table 6: Own-gender bias and social distance

	Young borrowers	Old borrowers	Young loan officers	Old loan officers	Low experience	High experience
Panel A: Interest rate						
Gender*Gender	0.0009	0.0041***	0.0051***	0.0008	0.0030**	0.0035***
	(0.0010)	(0.0014)	(0.0016)	(0.0014)	(0.0014)	(0.0011)
P-value of Wald test	0.0677		0.0488		0.8069	
Adjusted R-squared	0.5389	0.5340	0.5147	0.5735	0.5525	0.6177
Observations	3,940	3,945	4,045	3,840	4,153	3,732
Panel B: Arrears						
Gender*Gender	0.0061	0.0230	0.0010	0.0215	0.0238*	-0.0100
	(0.0245)	(0.0192)	(0.0238)	(0.0171)	(0.0121)	(0.0228)
P-value of Wald test	0.3792		0.3730		0.2029	
Adjusted R-squared	0.0761	0.0947	0.0714	0.0912	0.0895	0.0795
Observations	3,940	3,945	4,045	3,840	4,153	3,732

This table shows regression results with interest rate (Panel A) and arrear occurrence (Panel B) as dependent variables. All regressions include the control variables that are included in column (4) of Table 4, the results for these are omitted to save space. Further, branch and sector trends are included where possible. 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 loan officer experience measured as the number of loan applications handled by the respective loan officer (121 loan applications handled). Each regression also includes time, sector, sector-borrower, and sector-loan officer fixed effects. Standard errors that are clustered at the branch-sector level are shown in parentheses. ***, **, * indicate significance at the 1, 5, and 10 percent level, respectively.

Table 7: Own-gender bias and competition

	Competition		Young loan officers		Small branches	
	Low	High	Low competition	High competition	Low competition	High competition
Panel A: Interest rate						
Gender*Gender	0.0039***	0.0032**	0.0077***	0.0007	0.0097***	0.0031
	(0.0013)	(0.0013)	(0.0024)	(0.0020)	(0.0021)	(0.0020)
P-value of Wald test	0.6618		0.0097		0.0157	
Adjusted R-squared	0.4302	0.5486	0.4521	0.5260	0.3705	0.5676
Observations	3,557	2,147	1,846	1,010	1,533	1,129
Panel B: Arrears						
Gender*Gender	-0.0003	0.0393	-0.0030	0.0453	-0.0021	0.0342
	(0.0212)	(0.0334)	(0.0277)	(0.0990)	(0.0323)	(0.0426)
P-value of Wald test	0.1255		0.5884		0.4186	
Adjusted R-squared	0.0869	0.0926	0.0588	0.1103	0.0944	0.0380
Observations	3,557	2,147	1,846	1,010	1,533	1,129

This table shows regression results with interest rate (Panel A) and arrear occurrence (Panel B) as dependent variables. All regressions include the control variables that are included in column (4) of Table 4, the results for these are omitted to save space. Further, branch and sector trends are included where possible. In columns (1) and (2), the sample is split according to the median competition measured as the ratio of branches over population in a specific region and year for the time period 2004-2006. Columns (3) and (4) show regression results for below median age loan officers and for low and high competition. Columns (5) and (6) show regression results for below median size branches and for low and high competition. Each regression also includes time, sector, sector-borrower, and sector-loan officer fixed effects. Standard errors that are clustered at the branch-sector level are shown in parentheses. ***, **, * indicate significance at the 1, 5, and 10 percent level respectively.