Cultural Proximity and Loan Outcomes*

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Abstract

We present evidence that shared codes, beliefs, ethnicity —cultural proximity— between lenders and borrowers improves the efficiency of credit allocation. We identify in-group preferential treatment using dyadic data on the religion and caste of officers and borrowers from a bank in India, and a rotation policy that induces exogenous matching between officers and borrowers. Having an in-group officer increases access to credit and loan size dispersion, reduces collateral requirements, and induces better repayment even after the in-group officer leaves. These effects diminish with group heterogeneity and size. The results imply that cultural proximity mitigates informational frictions in lending.

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1 Introduction

Shared codes, language, religion—what we will call cultural proximity—between potential parties of a transaction can affect the likelihood that the transaction takes place, and also its outcome. Commonalities in religion and in ethnic origin, for example, are positively associated with trade flows between countries (Guiso, Sapienza and Zingales, 2009). The effect on efficiency of such cultural proximity between transacting parties is ambiguous, however. On the one hand, if members of a group tend to do business with one another for preference-based reasons, this may lead to discrimination or favoritism and result in the misallocation of resources. Alternatively, if cultural proximity reduces the cost of communication or contract enforcement, in-group transactions may be more efficient. Given these opposing forces, the effect of cultural proximity on contracting remains an empirical question, something that we explore in this paper.

There are a number of challenges in empirically identifying the extent of preferential in-group treatment, and distinguishing among the various explanations underlying such behavior. First, it requires information on the group membership of both transacting parties. Most studies have relied exclusively on the religion or race of only one side of the market, and have thus been best set up to detect discrimination against minorities rather than dyadic preferences for one's own type. This confounds any beneficial effect of in-group interactions with statistical or animus-based discrimination, especially when the in-group advantages are more prevalent within relatively small minority groups. Second, even when dyadic data are available, matching between parties is driven by the transactions' expected profitability, which is not observed by the econometrician. Unobservable differences in profitability—for example, in the case where minority agents are relatively "unprofitable"— may result in finding no in-group preferences within minority groups even when one exists, or an in-group preference among majority groups even when none exists. Finally, it is difficult to assess the efficiency of outcomes in most economic transactions —the sale price of an automobile (as in Ayres and Siegelman, 1995), for example, largely involves the distribution of a fixed pie.

We use data from a large state-owned bank in India that provides a near-ideal setting for studying the extent and rationales for preferential in-group treatment in individual interactions with private information. Specifically, the setting makes it possible to address the three identification problems highlighted above. Detailed credit and personnel records allow us to match all borrowers and branch head officers to their religion and caste, providing a dyadic characterization of the cultural "distance" between transacting parties in the allocation of personal loans for close to three million borrowers over a five year period. An explicit officer rotation policy among branches provides variation in the matching between lenders and borrowers. We are thus able to control effectively for time-invariant attributes of borrowers and lenders, and for time varying credit conditions of each group within narrowly defined geographic markets. Further, we can use detailed loan records to measure the effect of cultural proximity on ex ante loan contracting characteristics and ex post loan performance, and hence characterize the efficiency of transactions as well as the mechanisms driving the differences between in-group versus out-group interactions. In addition to the econometric advantages of our setting, the welfare implications of the relationship between cultural proximity and credit outcomes are likely to be of first order importance in an environment characterized by credit rationing and a long history of religious and caste conflict.[1]

We find strong evidence of preferential in-group treatment. In the baseline results we define two individuals to belong to the same group when both are members of the same minority religion (Christian; Muslim; Sikh; Parsi; Buddhist) or, conditional on belonging

[1]For evidence and discussions, see Banerjee, Cole, and Duflo 2004, Banerjee and Duflo 2008, and Field et al. 2008.

to the majority religion (Hindu), when both belong to the same official caste (General Class, Scheduled Caste, Scheduled Tribe, or Other Backward Classes). On average, the total amount of credit outstanding to borrowers in a group increases by 18.6% when the officer assigned to the branch belongs to the same group. Having an in-group officer also increases the number of borrowers by 6.2% and the probability that the group receives any credit by 1.6%. The results are robust to the inclusion of district-group-time dummies, and also the simultaneous inclusion of branch-time and group-time dummies. This indicates that the estimated effects are not driven by unobserved variation in the demand for credit by any group or at any locality, by policies that direct credit differentially to different groups and regions over time, or by reverse causality, where officers are transferred to areas where her group is thriving. The results are also robust to an alternate and independent classification where we use individuals' surnames to assign borrowers and officers based on the religious caste system that prevailed in ancient India. This rules out the possibility that the results are driven by systematic errors in the classification of individuals in the bank's records, or by targeted lending policies based on this classification.

Having established that borrower-officer cultural proximity has a causal effect on access to credit and the amount provided, we explore the economic mechanism behind this result by analyzing the effect of proximity on loan repayment and other dimensions of credit supply. Loans made to in-group borrowers have better repayment performance ex post. The economic magnitude of this effect is large: weighted by loan size, in-group borrowers are nearly 15% less likely to be late in loan payments, an effect that persists even after the in-group officer is replaced by an out-group one. This improvement in credit risk is recognized ex ante, as in-group loans are made with lower collateral ratios. Additionally, we observe that cultural proximity increases substantially the dispersion of lending across borrowers, implying that officers increase credit to some borrowers more than others within their own groups. These findings suggest that cultural proximity mitigates problems of asymmetric information in lending and improves the allocation of credit across borrowers with heterogeneous repayment prospects. These effects dominate any negative impact of taste-based preferences on loan quality, as taste-based discrimination would lead —at least weakly— to a decline in average repayment performance.

Our results all hold for the subsample of borrowers that had already had loans from the bank when a new officer arrives. Further, our results hold in isolated locations with a relative scarcity of other formal lenders. Thus, the patterns we report cannot be the result of self-selection by borrowers or the switching of borrowers from other banks. This has the further implication that the observed in-group lending increase improves access to credit or substitutes for informal sources of credit, which are typically more expensive.[2] This implies that cultural proximity increases borrower welfare, in addition to improving the efficiency of the bank's credit allocation.

Our work has a number of significant economic and policy implications that relate to several areas of research. First, our findings highlight the fact that the information and enforcement advantages of cultural proximity can be mistaken for discrimination. For example, in the context of our paper, Hindu borrowers represent 89.2% of the borrower population and take out larger loans than minority religion borrowers —presumably because Hindus borrowers are wealthier and in other ways better credit risks— yet Hindu officers do not exhibit in-group preferential treatment based on religion. A naive regression of loan access on borrower religion would indicate discrimination against minorities rather than preferential in-group treatment among minorities. This calls for caution in the interpretation of, and policy prescriptions that can be derived from, a substantial body of research devoted to studying minority discrimination that identifies differential treatment based solely on the identity of one of the parties of the transaction.[3]

[2]See Aleem 1990 and Banerjee and Duflo 2010.

[3]See, for example, Goldin and Rouse 2000, Bertrand and Mullainathan 2004 and Charles and Guryan 2008 for evidence in labor markets, and List 2004 for evidence in sports card trading markets. There

Our findings also suggests a mechanism for the formation of statistical discrimination against a minority group, arising as a consequence of the documented effect of cultural proximity on credit outcomes. Even if all groups in the population are ex ante equal, our results imply that the average minority group borrower will have a worse credit history and lower access to credit only because of the low probability that she will be matched with an officer from her own group. If lenders' priors on borrower creditworthiness are based on the group's (unconditional) average past performance, minorities will face higher borrowing costs in the marketplace due purely to statistical discrimination. This insight relates to a body of theoretical work, following Arrow (1973), that rationalizes statistical discrimination as an equilibrium with self-confirming beliefs, but that is silent about the origin of these beliefs. A policy that increases the likelihood of a group match between lenders and borrowers would unambiguously improve efficiency when statistical discrimination is a consequence of in-group preferential treatment.[4]

We contribute to two main empirical literatures. First, our work relates to the set of studies that examines the role of group identity using dyadic data, with generally mixed results. Ayres and Siegelman (1995) finds evidence of race and gender discrimination in an audit study of price bargaining in the U.S. new car market, but finds no evidence of in-group preferential treatment. In contrast, Parsons et al. (2011) documents that Major League Baseball strikes are called less often if the umpire and pitcher do not match race or ethnicity, consistent with preference-based discrimination. Schoar, Iyer and Kumar (2008) present evidence in a setting similar to ours: bargained prices are lower when the buyer and seller belong to the same community in a field study in the wholesale market

is also evidence of discrimination in different types of credit markets, such as mortgages (see Ross et al. 2008 for one recent example, and Ladd 1998 for a survey of the evidence), small business lending (Blanchflower et al. 2003), trade credit (Fafchamps 2000; Fisman 2003), and online person-to-person lending (Pope and Sydnor 2010).

[4]See Kim and Loury 2009 for a discussion of the origin of statistical discrimination, and Coate and Loury 1993, Norman 2003 and Fryer and Loury 2005, for discussions of optimal policy prescriptions in such multiple equilibrium settings.

for pens in India. The key contribution of our study is to provide the first characterization of potentially efficiency-enhancing mechanisms behind in-group preferential treatment.

Our paper also relates to the literature on social ties between transacting parties. Because the parties in our setting share a common cultural endowment, but have most likely never met before the transaction, the impact of cultural proximity that we document here is distinct from the effects of social ties or networks, which result from parties' past interactions.[5] Our results indicate that cultural endowments affect the likelihood that two individuals will interact. This suggests that the endowment and social ties effects are typically confounded in existing work that associates endogenous past social interactions with future market transactions.[6] The distinction is important because cultural endowments, such as religion and caste, are assigned at birth and transmitted across generations of individuals of the same group, while social ties and connections are dynamic and often subject to individual choice (Becker 1996). This implies that the economic consequences of cultural endowment differences across groups can persist in the long run, and potentially perpetuate inequality.

In the next section, we begin by providing an overview of the data and a description of the Indian bank we study —its organization, the incentives of its officers, and so forth. In Section 3 we present the baseline empirical specification for the analysis. Section 4 presents our results on lending quantity; Section 5 analyzes default, loan dispersion, and collateral patterns to distinguish between taste versus information and enforcement based explanations; Section 6 presents our findings on the cross-sectional heterogeneity in the effects of cultural proximity. In Section 7 we conclude with some policy implications and directions for future work.

[5] For evidence of the effect of social connections on economic interactions see, for example, Banerjee and Munshi 2004 and Bandiera, Barankay and Rasul 2009.

[6] For examples of this work see Cohen, Frazzini and Malloy 2008, 2010, Hwang and Kim 2009, Engelberg, Gao and Parsons 2011, Jackson and Schneider 2011, and Li 2012.

2 Data

The main variables in the analysis are obtained from the individual loan portfolio and personnel records of a large state-owned Indian bank, which operates over 2000 geographically dispersed branches in India (see Appendix Figure A.1). The sample starts in 1999 Q2 and ends in 2005 Q1. This section describes in detail the structure and construction of the dataset from the bank as well as other sources, and relevant background information on the organization of the bank itself.

2.1 Loans, Borrowers, and Branch Heads

The individual loan portfolio data include loan-level information for every borrower with a loan outstanding during the sample period (2.92 million individuals), with information about the loan contracts and repayment status reported on a quarterly basis (1.23 million borrowers per quarter on average). The main variables for the analysis are the amount of debt outstanding, the collateral posted, and the number of days late in interest payment.

The median (mean) amount of debt outstanding in the full borrower-quarter panel is 8,495 (36,086) rupees; excluding borrower-quarter observations with zero balance, the median (mean) is 14,645 (47,924). Outstanding debt is typically secured: the median collateral to loan ratio in the full panel is 1.67. The over-collateralizing reflects that fact that the collateral data in most cases are not updated as a loan is repaid, and reflect the initial collateral value at the time of contracting. The median interest rate is 12%.

The median borrower's interest payments are current. The skewness in days late results from the stock of past defaulted loans, even those that occurred prior to our sample period, which are never removed from bank records. Excluding observations with more than 365 days late in repayment (2.76 million borrower-month observations), the average days late is 13.4. To eliminate the noise generated by these legacy of defaults, we retain in the sample only borrowers with less than 365 days late in repayment for the purposes of the analysis.[7]

From the internal personnel records of the bank, we obtain information about employees at each branch at a quarterly frequency. Each record has a general job description and the position in the internal hierarchy of the bank. We use these data to identify the head officer of each branch at each point in time (4,270 distinct officers in total). Loan officers are classified into six grades, increasing in seniority, and the ability to approve larger loan amounts. The highest ranked officer in each branch is the branch head. For smaller branches, the head officer may himself have a relatively low grade. This implies that any larger loan request that comes through the branch will have to be approved by a higher grade officer elsewhere in the region. Still, in these cases the decision of whether to submit the loan for approval at a higher level of the bank hierarchy is under the head officer's discretion, and based on information collected at the branch level. Although officers have control over loan and collateral amounts, they have no discretion over interest rates, which are determined by headquarters based on loan type. For example, all home improvement loans pay the same rate, as do all educational loans above Rs.400,000.

Branch heads —the focus of our analysis here— are evaluated annually on a range of criteria.[8] These include quantitative measures such as the amount and profitability of lending, as well as qualitative considerations such as employee skill development, effective customer communication, and other aspects of "leadership competency." Each officer is ultimately assigned a numerical grade from zero to one hundred. One specific aspect of officer performance that will be relevant for our analysis is the extent to which officers are held accountable for loan defaults after moving branches. Typically, officers are re-

[7] The conclusions are invariant to the cutoff of days late used to generate our main sample, although the noise introduced by including the legacy loans generates attenuation bias.

[8] Information on evaluation and compensation of managers within the bank come primarily from interviews with bank staff.

sponsible for loans they approve for three years following their departure, at which point responsibility is transferred to an officer in the branch where the loan was made.

While there is limited incentive pay, branch heads may be motivated through possible promotion to higher grades or better postings. For example, successful branch heads may be sent to locales with more or better perquisites, such as higher pay (overseas), larger houses, the use of a car, or control over a larger portfolio (large branches). In the analysis that follows we evaluate the extent to which such endogenous allocation of officers to branches affects our estimates.

2.2 Religion, Official Caste, and Religious Caste

The bank data also contain information on the religion and official caste classifications of each borrower and employee. Individuals are grouped into seven categories based on the prominent religions in India: Hindu, Muslim, Christian, Sikh, Parsi, Buddhist, and others. They are also classified into four castes based on the categories explicitly recognized by the Constitution of India: General Class (GC), Scheduled Castes (SC), Scheduled Tribes (ST) and Other Backward Classes (OBC). The SC category comprises all the castes historically treated as "untouchable" by the upper castes in India. The ST category includes indigenous, typically geographically isolated, tribal groups. The OBC category is a collection of caste groups ranked above untouchables in the ritual hierarchy, but socially and educationally disadvantaged. Individuals belonging to the SC, ST, and OBC categories receive targeted government aid and benefit from positive discrimination policies (subject to means testing) such as reservations in public sector employment and higher education.[9] Although the SC, ST, and OBC categories include a

[9]The categories of Scheduled Caste (SC) and Scheduled Tribes (ST) that represented a majority of lower-status castes and tribes were first protected in anti-discrimination laws through the ninth schedule of the Constitution in 1950 (Article 15, 17, and 46). In 1990, the further caste-based categorization of OBC was added for identifying additional socially and economically deprived communities. A few

wide variety of social groups across India, locally they are often relatively homogeneous. The GC category is essentially a collection of all the individuals not belonging to the aforementioned "backward" classes.

In order to obtain a group classification that is independent of the bank's records, we use the borrower and officer surnames to generate a group classification based on religious castes. According to religious texts such as Manusmriti, Hindu society is broadly divided into four Varnas: the Brahmins (priests and scholars), Kshatriyas (warriors), Vaishyas (merchants and traders) and Shudras (laborers and artisans). Each Varna is a unification of several Jatis, or communities (see Buhler 1886), and a person's surname typically reflects the Jati she belongs to. We exploit this link with surnames to classify each individual into her Varna (see Banerjee et al. 2009 for a further discussion of the link between surnames and castes in India). In the online Appendix we provide a description of the matching procedure and some examples.

Classifying borrowers and officers by religious caste using surnames comes at the cost of additional noise and imprecision. One source of noise results from the fact that many surnames can be classified into two or more Varnas. For example Saxena is grouped under both Brahmins and Kshatriyas. Similarly Desai is grouped under both Brahmins and Vaishyas. We created three special categories for individuals where this ambiguity arises (Kshatriya-Brahmin, Kshatriya-Brahmin-Vaishya, and Kshatriyas-Vaishyas). We note, however, that once we condition on region — as we do throughout our analysis — there is a clearer link between names and communities. Second, it was unclear how to categorize individuals into the Shudra Varna according to their community affiliations, which precluded using surnames for individuals outside of the General Classes. Finally, in a large fraction of cases, the surname-based classification conflicted with the bank

years thereafter the category of OBC was extended to include a significant segment of the non-Hindu population, notably Muslims, Christians, and Sikhs.

classifications assigned to loan officers and borrowers. For example, many bank-classified Muslims had "Hindu" surnames, and vice-versa. Still, exploring the effect of proximity along the Varna dimension is interesting it its own right, and will allow us to ascertain whether the results based on bank classifications are driven by systematic misclassification of officers and borrowers in the bank records.

2.3 Descriptive Statistics - Group Composition

The religion, official caste, and Varna compositions of the borrowers and officers populations are shown in Table 1. By religion, Hindus represent the majority of borrowers (89.4%) and officers (93.8%). The largest group of minority borrowers is Muslim (6.33%), and the largest officer minority is Christian (2.1%). Hindus are over-represented and Muslims under-represented in the borrower and officer populations relative to the total population (80.5% Hindu and 13.4% Muslim according to the 2001 census). Most borrowers and officers do not receive any official designation and are classified as General Class (66.7% and 74.3% respectively). The largest borrower minority is the OBC category (16.6%), while the largest officer minority is ST (15.7%). SCs are under-represented in the borrower sample and STs under-represented in the officer sample, relative to the population (16.2% SC and 8.2% ST in the 2001 census).[10]

We are able to match surnames to Varnas for a subsample of the population; a total of 502,723 borrowers (18.3% Brahmin, 60.5% Kshatriya, 6.6% Vaishya, 1.7% mixed categories, and 5.72% in other categories) and 1,689 officers (23.0% Brahmin, 43.4% Kshatriya, 11.7% Vaishya, 15.5% mixed categories, and 6.4% in other categories) have Varna assignments. All the identifiable Varnas in our sample belong to the General Class according to official caste definitions.

[10]The 2001 India Census does not keep track of OBCs.

2.4 Descriptive Statistics - Branches and Groups

Table 2, Panel 1, shows branch-quarter level statistics. The median branch has a total of 13.5 million rupees of debt outstanding and lends to 334 borrowers. The borrower composition is generally quite heterogeneous: the median branch lends to borrowers of four different religions and three different official castes. The median branch is small, with two loan officers including the head officer, and the modal branch has only a single officer.

The unit of analysis is the branch-group-quarter level (indexed by b, g, and q, respectively) where group refers to the cultural group of the borrower.[11] In our main specification we use the full set of religion and caste information to group borrowers into 9 categories: 5 minority religions, and 4 official castes conditional on religion being Hindu. In other specifications we consider group definitions based on religion, caste, and Varna independently. The panel employing our main group classifications has 339,366 branch-group-quarter observations and we present the descriptive statistics in Table 2, Panel 2. The average group-branch has around 2 million rupees of total debt outstanding and 43 borrowers, with an average of 16 days late in repayment. 8.1% of borrowers are more than 60 days late in their loan repayments. However, only 2% of total debt is in default, reflecting the fact that larger debts are much less likely to be behind in repayment.

We merge the branch-level personnel information to this panel to obtain our main explanatory variable, $SAMEGROUP_{bgq}$, a dummy variable that is equal to one for the branch-group-quarter loan cells where the branch head officer belongs to group g, and zero otherwise. For example, if the head officer of branch b in quarter q is Muslim, then $SAMEGROUP_{bgq} = 1$ for loans from group g if $g =$ Muslim, and zero for all other groups in that branch-quarter.

[11] We show analysis at the borrower level when we estimate the effect on the intensive margin of lending below.

2.5 Officer Rotation

The bank follows an explicit policy of geographical officer rotation, with the stated objective of reducing opportunities for corruption, nepotism, and other perverse incentives in the allocation of loans. As a result, branch turnover is high: we observe an average of 127 head officer reallocations per quarter, and the median branch has one officer change during our sample period. The mean (median) spell of a head officer in a branch is 8.3 (8) quarters, with standard deviation of 4.1. Head officers are always assigned to branches that are located away from their home town, and the average officer reallocation assigns the officer to a new branch that is 250 kilometers from the previous assignment. This implies that although officers generally stay within the same region, it is unlikely that they have had any prior interaction with any of the potential borrowers in their new location.

In Appendix Table A.1 we report the empirical distribution of branch transitions by religion (panel 1), official caste (panel 2), and Varna (panel 3). We highlight with asterisks the transition frequencies that are statistically different from those that would result from assigning officers at random from the population of officers described in Table 1. For religion and Varna, the empirical transition frequencies are statistically indistinguishable from a policy of random officer allocation. However, for the subsample of Hindu officers the empirical transition rates deviate from the random benchmark when officers are grouped by official caste. For example, the observed probability of a GC to GC transition is 61.0%, while the random benchmark is 55.4%, and the difference is statistically significant at the 1% confidence level. This indicates that there are some branches that tend to receive General Class officers too often relative to random assignment. This is consistent with the existence of reservations for SC, ST, and OBC official posts in some regions, since this would lead to a higher observed proportion of transitions in the diagonal of the matrix. We discuss the potential consequences of policy-driven distortions in officer allocation in the context of our empirical estimation in the sections that follow.

The main advantage of the rotation policy for our purposes is that it induces variation in the matching between officer and borrower group identity that is plausibly uncorrelated with the demand for credit. The main caveat is that officer rotation may exacerbate the consequences of cultural differences, for example, because it reduces officers' incentives to learn about the cultural traits of out-group borrowers. Nevertheless, rotation policies that reassign agents' geographical location, position in the hierarchy, clients, and tasks are commonplace, which underscores the importance of understanding their economic consequences (see, for example, Hertzberg, Liberti, and Paravisini, 2008, and Iyer and Mani, 2011).

3 Empirical Specification

Our baseline empirical specification identifies the effect of cultural proximity from the time series variation in loan outcomes for a particular group, in a particular location, when the group identity of the officer changes due to the rotation policy. The specification takes the following form:

$$y_{gbq} = \beta SameGroup_{bgq} + \alpha_{gb} + \tau_q + \epsilon_{bgq} \tag{1}$$

The dependent variable is a loan outcome (i.e., total lending, number of loans, fraction of loans past 60 days late, etc.) at the branch-group-quarter level; g indexes the group (caste, religion, or pooled partition); b indexes the branch; and q indexes the quarter. *SameGroup* is an indicator variable denoting whether the branch head in branch b belongs to group g in quarter q. The two fixed effects —a_{gb} and τ_q— capture time-invariant attributes of each group within each branch (i.e., a group times branch set of fixed effects), and aggregate shocks to all branches. The error term ϵ_{bgq} allows for clustering at the branch level. This accounts for serial correlation in lending and for the mechanical correlation of *SameGroup* across groups in the same branch.[12]

We also consider augmented specifications where we saturate the branch-time, group-time, state-group-time, and district-group-time variation. Branch-time dummies, τ_{bq}^{Branch}, account for all changes in the demand for credit in a particular location, as well as changes in directed credit policies aimed at certain localities. Since there is one head loan officer per branch at any time, the branch-time dummies also account for all unobserved branch head heterogeneity, whether time invariant (different officer skills) or time varying (officer learning about the job or the environment), and also for any effect that the change of an officer may have on average lending in a branch. The group-time dummies, τ_{gq}^{Group}, account for aggregate changes in the credit demand from, or supply to, specific groups. The state-group-time (district-group-time) dummies, $\delta_{bgq}^{State\text{-}Group}$ ($\delta_{bgq}^{District\text{-}Group}$), absorb any changes in the demand or supply of credit that are specific to a group in any given region, such as secular borrowing trends affecting particular groups in a location. We show in the next section that these saturated models produce estimates of the coefficient on *SameGroup*, β, that are statistically indistinguishable from those of the baseline. This represents strong evidence that officer group transitions are orthogonal to other determinants of credit at the group-location-quarter level (i.e., $E[SameGroup \times \epsilon] = 0$), and that the β estimates from specification (1) are consistent.

The coefficient on *SameGroup* is a difference-in-differences estimate of the effect of cultural proximity between a lender and a borrower on loan outcomes. Consider, for example, the regression with the log of total lending as the dependent variable and,

[12]By construction, every time *SameGroup* changes from zero to one for group g in branch b, it will change from one to zero for some other group in the same branch b.

for simplicity, suppose there are only three groups: Hindus, Muslims, and Christians. Suppose that a branch has a Hindu officer during the first half of the sample, and a Muslim officer during the second half. The coefficient on *SameGroup* captures the difference between the log debt of Hindu borrowers in the branch when the officer is a Hindu (in-group) officer relative to when the officer is a Muslim. It also captures the difference between the log debt of Muslim borrowers with a Muslim officer relative to a Hindu one.

Our main results show the average effect across all groups. In the last section of the paper we explore whether there are heterogeneous in-group effects across group definitions and across branch and branch location characteristics.

4 Results: Loan Amounts

We begin with a graphical description of (unconditional) lending patterns around officer transitions. First we classify borrowers into two categories based on whether they have the same group identity as the outgoing officer: *in-group* borrowers are those belonging to the same group as the officer, and all others are categorized as *out-group* borrowers. For example, in a branch where the outgoing officer is Hindu, the Hindu borrowers are in-group before the officer change, and all minority religion borrowers are classified as out-group. Each of these borrower groups may or may not experience a change in their in-group/out-group status after the officer change. For example, suppose the Hindu officer is replaced by a Muslim one. Then, Hindu borrowers transition from in-group to out-group, Muslim borrowers transition from out-group to in-group, and other non-Muslim minority religions remain as out-group. Alternatively, if the replacement officer is also Hindu, then Hindu borrowers remain as in-group and all minority borrowers remain as out-group.

We use these borrower classifications to construct "event study" plots around officer transitions. The horizontal axis of the plots in Figure 1 measures time in quarters since the officer change in a branch. Time 0 represents the first quarter a new officer appears as the branch head in the personnel files. Given that our analysis is based on quarterly data, the new officer may arrive up to 11 weeks before the observed entry time. Below, we discuss the bias that this measurement error may introduce. The vertical axis measures the average debt of borrowers that experience a change in in-group/out-group status, relative to those that do not. Panel (a) shows the average debt of out-group borrowers that become in-group after the officer change, minus the average debt of borrowers that remain out-group. Panel (b) shows the average debt of in-group borrowers who become out-group, relative to in-group borrowers that remain in-group. All averages are taken at the group-branch level using the caste-religion group definition, and both plots include the 95% confidence interval of the mean difference.

The plots show a shift in the composition of lending as a function of cultural ties when there is a change in head officer. In Panel (a), the average debt of borrowers that switch from out-group to in-group status increases by approximately 1.5 million rupees (approximately USD$35,000), relative to borrowers that remain out-group after the officer change. A parallel pattern appears in Panel (b): the average debt of borrowers that switch from in-group to out-group status drops by 4 million rupees during the four quarters following the change in status, relative to borrowers that remain in-group.[13]

The plots in Figure 1 also suggest that the relationship between cultural proximity and lending may be causal, since the relative debt change occurs immediately around the officer transition and does not appear to be driven by pre-existing differential lending trends across the two groups in each panel. The plots provide some validation for the identification assumptions behind the difference-in-difference estimator of the in-group effect in specification (1).

[13]The asymmetry in the magnitudes of the jumps across the two plots is driven by the fact that the groups of borrowers compared in the two plots are different in size and average debt. In the formal empirical analysis these differences are accounted for by the branch-group dummies.

There is a small and statistically insignificant increase in the relative amount of lending at time -1 in Panel (a), the period prior to the recorded quarter of arrival of the new officer. This is likely driven by the aforementioned measurement error in the time of arrival of the new officer. Such measurement error will tend to bias towards zero our estimates of the in-group effect in specification (1).

4.1 In-Group Effect on Credit

We present in Table 3 the effect of having an in-group branch head on credit, estimated using specification (1). Outcomes are measured at the level of group g in branch b in quarter q. To capture both the intensive and extensive margins of lending, we use as dependent variables the log of total debt, the log of the number of borrowers, and a dummy variable equal to one if debt is greater than zero. The log transformation of the first two variables reduces their skewness, and facilitates an elasticity interpretation of the coefficients. However, it also creates an unbalanced panel, as zero loan cells become missing values. The effect on the group-level extensive margin —i.e. the probability that a group receives some credit— is captured by the last specification in a linear probability model. In unreported specifications we used a $ln(1+x)$ transformation to reduce the skewness of the dependent variable. Although this alternate transformation does not allow a ready interpretation of the magnitudes of estimated coefficients, it generates qualitatively identical results to those reported below.

The estimated coefficients on the *SameGroup* indicator variable are positive and significant across all three specifications. The magnitudes imply that lending to a group increases by 18.6% when an in-group officer is assigned to the branch (Table 3, column 1), the number of borrowers increases by 6.2% (3, column 4), and the probability that the group receives any debt increases by 1.5 percentage points (3, column 7), or around

2.5% of the baseline probability of having positive credit.

Because cultural proximity affects the likelihood that a group receives any credit, the coefficient estimates on total lending and number of borrowers are consistent estimators for a local average treatment effect (LATE) of cultural proximity on branch-group cells that have positive debt both with and without an in-group officer. The LATE and the average treatment effect are likely to be close in our setting, because the effect of proximity on a group's probability of borrowing is low. To confirm this we estimate the bounds of the average treatment effect using the procedure in Lee (2008), estimating the total debt and number of borrower specifications after trimming the 1.5-percent upper and lower tails of the outcome for the branch-group-quarter cells in which *SameGroup* equals one (1.5 percent is the estimated treatment effect on the probability of receiving any credit). In both cases, the bounds are relatively tight around the LATE estimate, as the difference between the LATE and the lower bound is less than one fifth of a standard deviation of the LATE estimate (Table 3, columns 2, 3, 5, and 6). For example, the average treatment effect of cultural proximity on total debt ranges from 0.183 to 0.229, while the LATE estimate is 0.186. Thus, in what follows, we present LATE estimates and interpret them as the lower bound on the average treatment effect of cultural proximity on loan outcomes.

Table 4 presents the estimates of specification (1) augmented with branch-quarter, group-quarter, state-group-quarter, and district-group-quarter dummies.[14] To reduce the number of nuisance parameters to be estimated in these specifications, we remove the branch-group means from all variables in the panel rather than including branch-group fixed effects, and adjust the standard error estimation accordingly. Neither the magnitude nor the significance of the estimated parameters changes in any of the saturated models. This represents strong evidence that our variable of interest, *SameGroup*, is un-

[14] There are 43,974 branch-quarter dummies, 216 group-quarter dummies, 4,760 state-group-quarter dummies, and 52,406 district-group-quarter dummies.

correlated with the error term in the baseline specification (1), and that the estimated coefficient on *SameGroup* has a causal interpretation. The saturated specifications rule out, for example, an alternative reverse causality interpretation of the coefficient in which a positive association between lending to a group and the identity of the officer is driven by an endogenous allocation of officers into areas where their own group is thriving. The district-group-quarter dummies account for localized shocks and trends to the demand for credit of specific groups in narrowly defined geographical areas (the median district has three bank branches in our sample).

Table 5 repeats the estimation using the group definitions based on the traditional religious caste system (Varna), obtained through surname matching. The estimated effect of cultural proximity on lending is again positive across all outcomes, although the estimates are noisier (e.g. the effect on the number of borrowers is not significant). The point estimates are of the same order of magnitude as those obtained using our main group definitions based on religion and government-sanctioned caste (Table 3). The point estimates imply, for example, an in-group effect on total credit of 14% when groups are defined using Varnas (18.6% when defined using religion and government sanctioned caste). The Varna grouping is constructed independently of the bank's classification of officers and borrowers, indicating that the observed in-group effects are not driven by systematic misclassification of borrowers by the bank. Also, since it is implausible (and illegal) that the bank uses Varnas to allocate credit or assign jobs, the Varna-based results provide an independent validation of the identification assumption that the group identity of the officer in a branch is uncorrelated with directed lending policies targeted to borrowers of the same group.

4.2 Intensive and Extensive Margins

The results so far focus on lending outcomes at the group level. In this subsection we explore the effect of cultural proximity on the borrower-level intensive and extensive margins of lending. We proceed in two ways. The first is by partitioning the borrower sample into two groups: 1) borrowers that established a credit relationship with the bank prior to the arrival of the current officer, and 2) borrowers that receive credit from the bank for the first time with the current officer. Estimates from group-branch level regressions on these subsamples provide the effect of cultural proximity on the average group debt to continuing borrowers, and the group debt and number of borrowers that enter and exit the sample. The second approach is to estimate the following borrower fixed-effects specification:

$$y_{iq} = \beta^{ind} SameGroup_{iq} + \alpha_i + \tau_q + \epsilon_{iq} \tag{2}$$

The coefficient β^{ind} measures the LATE of proximity on the level of debt of continuing borrowers. The main difference between β^{ind} and β estimated from a branch-group level regression on continuing borrowers (sample 2 described above) is that the former is not weighted at the group level by loan amounts, while the latter is. The two estimates will differ if cultural proximity has a heterogeneous effect on borrowers of different sizes: $\beta^{ind} > \beta$ if cultural proximity affects disproportionately the smaller borrowers in a group, while $\beta^{ind} < \beta$ if larger borrowers are disproportionately affected.

4.2.1 Continuing Borrowers and Exit

Table 6, Panel 1 shows the estimates of a branch-group level specification that includes in every branch-group-quarter bgq those borrowers that had positive credit at any time before the officer in charge of branch b in quarter q arrived. The estimates indicate that the average debt by existing borrowers increases by 11.6% with the arrival of an in-group branch head (column 1).

Since all entrants are removed by construction from this subsample, the coefficient on the number of borrowers in Panel 1 measures exit. The effect on the number of borrowers is a tightly estimated zero (column 2), indicating that cultural proximity has no effect on the probability that preexisting borrowers cease to borrow from the bank. These results imply that the estimated effect of cultural proximity on the total number of borrowers from Table 3 is solely due to its effect on entry. The estimated effect on the probability that a group receives any credit is positive (column 3), indicating that proximity increases the likelihood that borrowers that had borrowed and repaid their loans start to borrow again.

Table 7 shows the estimated β^{ind} from specification 2 and the bounds on the average treatment effect using the Lee (2008) procedure. In this case we trim the top and bottom 6% of observations from the debt distribution, corresponding to the estimated effect of proximity on the number of borrowers from Table 3. The estimated LATE indicates that cultural proximity increases by 1.77% the amount of debt to the average borrower. This is lower than the value weighted estimate, 11.6% from Table 6, suggesting that the intensive margin effect is concentrated among larger borrowers. The average treatment effect is bounded between zero and 15.9% (columns 2 and 3). This range contains the value weighted estimate, as expected, and is large because of the economically significant effect that proximity has on borrower entry.

Collectively, the intensive margin results imply that the effect of proximity on credit is positive holding the borrower pool constant, and that there is substantial heterogeneity in the effect of proximity on credit across borrowers of different size.

4.2.2 Entry

Table 6, Panel 2, shows the estimated effects for the subsample of borrowers that obtain credit from the bank for the first time with the current officer. The estimated effects indicate that cultural proximity increases the flow of total lending to new borrowers by 16.9% and the number of new borrowers by 13.3%; the difference between the two estimates indicates that the average size of in-group loans is larger for in-group entrants.

The effect of cultural proximity on entry can occur through two different, not mutually exclusive, channels. First, cultural proximity may increase the likelihood that an officer approves a loan application from an in-group borrower. Second, it may affect the likelihood that in-group borrowers apply: borrowers with rational expectations about the higher likelihood of having an application approved by an in-group officer may be more likely to request a loan. For example, a Muslim borrower may decide to save on the cost of a loan application if she knows that the branch head officer is a Sikh, but choose to apply once a Muslim officer is in place.

The data do not contain information on loan applications, so we cannot estimate the effect of officer identity on applications directly. We adopt an indirect approach and analyze the dynamics of the in-group effect on the intensive and extensive margins. If entry increases because officers approve more in-group applications, then the in-group effect on the number of borrowers should occur immediately after the officer's arrival. In contrast, the effect of an in-group officer on the inflow of new applications should be more gradual, as it takes time for the news that there is an in-group officer in the branch to spread. We estimate specification (1) augmented with interactions between *SameGroup* and a set of indicator variables for the officer's first quarter at the branch, his second quarter, and so on. The coefficients on the interaction terms represent how the effect of cultural proximity on loan outcomes changes during his tenure.

Figure 2 plots the estimated interaction coefficients and 95% confidence interval bounds using the log of total credit and number of loans as dependent variables. Given that cultural proximity affects only the entry margin, we present estimates on the full sample and interpret the effect on the number of loans as the effect on entry. Panel (a) shows that the effect of cultural proximity on the amount of credit is immediate: credit to in-group borrowers increases by close to 10% during the first quarter the officer is assigned as branch head. In contrast, the effect on the number of borrowers, although positive, is not statistically different from zero when the officer arrives in the branch (Panel (b)). The effect of cultural proximity on borrower entry becomes significant only six months after the officer's arrival.

The distinct dynamic patterns in the two plots suggest that the effect of cultural proximity on the intensive and extensive margins is driven by different economic phenomena. For the intensive margin, the officer increases lending immediately to existing in-group borrowers. The effect on the extensive margin takes time to build, and is consistent with officers attracting more in-group borrower applications. We discuss further the implications of these patterns after presenting the results on loan performance.

Our main conclusion thus far is that cultural proximity between lenders and borrowers leads to an increase in lending. The magnitudes of the estimated effects on access to credit and the amount borrowed are substantial. The next section investigates the potential mechanisms behind this preferential treatment of in-group borrowers.

5 Mechansim

In the results presented above, we document a preferential in-group treatment effect. We next examine the mechanism that generates these results. Specifically, we are interested in whether the preferential treatment of in-group borrowers documented in the preceding section is due to pure favoritism or is driven by a reduction in informational frictions.[15] Distinguishing between these two possibilities is critical to understanding the efficiency implications of preferential in-group lending. To identify the mechanism at work, we examine the impact of in-group lending on ex post loan performance and other dimensions of lending.

5.1 In-Group Effect on Loan Performance

If loan officers can mitigate the effects of informational frictions for in-group borrowers, we expect the expansion of in-group credit access —already documented above— to be accompanied by improved repayment. By contrast, if favoritism is the dominant source of within-group preferences, the increase in lending will be the result of credit expansion to (lower-quality) marginal borrowers, leading to a deterioration in average lending quality.

We examine the impact of cultural proximity on future loan performance by estimating specification (1) using the fraction of borrowers and debt that are more than 60 days past due in a year. The results are almost identical when we use 30 and 90 days past due (see Appendix Table A.2).[16] As before, the unit of analysis is the branch-group-quarter level, and our outcomes of interest are calculated over all loans that are active and due in branch b, group g, quarter q. We calculate the fraction of loans active in quarter q that are past 60 days overdue in quarter $q+4$, which weights borrowers equally, and also the fraction of debt overdue in quarter $q+4$, which is a loan-size weighted statistic.

The estimated coefficients on *SameGroup* for loan performance are presented in Table

[15] Cultural proximity may mitigate informational frictions in several ways. For example, cultural proximity may allow loan officers to generate better information about the creditworthiness of borrowers than is available to an out-group loan officer. In addition, cultural proximity may lower the cost of imposing social sanctions and deter strategic defaults on loans and thus increase a borrower's debt capacity.

[16] We also employed specifications that used the log of one plus the number of days late as the outcome variable, including both specifications that weighted days late by loan size and specifications that weighted all borrowers equally. These regressions generated qualitatively very similar results to those we report in the text, but do not have any clear economic interpretation.

8, columns 1 through 4. The point estimates of the effect of cultural proximity on the fraction of loans more than 60 days overdue 12 months forward are negative and significant at the 1% level (column 1). The coefficient of -0.0097 implies a 12% reduction in default relative to the mean of 8.1% for in-group loans. We obtain similar results when the fraction of loans more than 60 days overdue is weighted by loan size (column 3). The estimated coefficient is -0.0054, implying a 27% reduction in default relative to the mean debt-weighted default rate of 2.0% for in-group loans. The difference in the magnitude of the two estimates indicates that the effect of cultural proximity on repayment is larger for larger loans.

A taste-based model of higher in-group lending that would also lead to higher repayment rates is one where cultural proximity causes loan officers to extend additional loans to insolvent in-group borrowers to make payments on past loans. This "ever-greening" explanation also implies that the impact on loan performance should be relatively short-lived, and in particular that it should disappear when an in-group officer is replaced by an out-group one. We test whether the positive effect of cultural proximity on performance dissipates when the in-group officer is replaced with an out-group one by augmenting our specifications in columns 1 and 3 with the 12 month lead of *SameGroup*. The coefficient on this variable represents the difference in future default across borrowers that still have an officer from the same group relative to those that experienced a change.

The coefficient on the 12 month lead of *SameGroup* in the equal weighted specification (column 2) is negative, suggesting an additional positive effect of *SameGroup* on performance in the future; however, it is half the magnitude of the coefficient on the lead of *SameGroup* and statistically indistinguishable from zero. The coefficient on the 12 month lead of *SameGroup* in the value weighted specification (column 4) is a precisely estimated zero. Overall, these results argue against an ever-greening explanation for our loan size and repayment performance results. Further, the absence of any *SameGroup* effect in the future evidence that the improvement in loan repayment results from better ex ante screening, as opposed to better ex post monitoring or enforcement, since the cost of direct ex post monitoring or application of social sanctions should increase after the in-group officer's departure.

The patterns in Table 8 are qualitatively similar for the borrower subsamples limited to those who previously borrowed from the bank (Panel 2), and those who borrowed from the bank for the first time from the officer assigned to the branch in quarter q (Panel 2). The point estimates are uniformly negative for the coefficients of interest: cultural proximity increases the repayment performance for existing and new borrowers. As we noted in the discussion of Figure 2, the increase in lending to existing borrowers begins within a quarter of the officer's arrival to the branch. The results on this group of borrowers are unlikely driven by shifts in demand or borrower characteristics —i.e. those that may result from borrowers self-selecting into the branch that has a in-group officer— since they are obtained holding the pool of borrowers constant. We explore further the extent of self-selection in the final section.

5.2 Loan Dispersion and Collateral

The view that in-group lending reduces information frictions and improves allocative efficiency yields further predictions on the ex ante characteristics of in-group lending.

First, lower default rates should reduce the average cost of borrowing. Since loan interest rates are fixed in our setting, we focus instead on collateral as a proxy for the borrowing cost (higher risk borrowers will post more collateral holding the interest rate constant), and examine whether collateral to loan ratios are lower for in-group loans.

Second, improved ex ante screening in particular should increase the dispersion in lending, as in the screening discrimination model of Cornell and Welch (1996). The intuition for this prediction is that the precision of the signal that the loan officer receives about a borrower's quality is more precise for in-group transactions. As a result the variance of the prior distribution of default probabilities is larger. In our setting, this implies a higher dispersion for lending to in-group borrowers. Note that, by contrast, enforcement-based explanations do not make strong predictions about the ex ante loan distribution.

Focusing first on effect of cultural proximity on collateral, we employ our baseline specification (1), using two separate measures of collateral intensity —the logarithm of total collateral, and the logarithm of the ratio of total collateral to loan amount. The estimated in-group effects are presented in Table 8, columns 5 and 6. The point estimate of the effect of cultural proximity on collateral is 0.136 (column 5), smaller than the estimated coefficient for total debt (0.185). The estimated in-group effect on collateral to loan ratios is -0.0474, significant at the 1% level (column 6), again indicating a lower collateral rate for in-group loans.

To assess the effect of cultural proximity on loan dispersion, we estimate the baseline specification (1) in Table 8, columns 7 and 8, using two measures of loan dispersion within the group: the standard deviation and the interquartile range of the loans issued in branch b, group g, quarter q. The estimated in-group effects are positive and significant for both measures. The point estimates indicate that cultural proximity increases the standard deviation (inter-quartile range) of loans outstanding 18.3% (8.8%). These findings are most consistent with ex ante information asymmetries accounting for the higher level of lending and performance of in-group borrowers.

5.3 Discussion

The results taken together rule out several standard rationales for the observed effect of cultural proximity on lending. First, the most straightforward taste-based models cannot explain why cultural proximity both increases the supply of credit and improves repayment performance. Preference-based explanations are only consistent with our results if in-group favoritism affects information collection or enforcement. For example, officers may spend more time with borrowers of their own group solely due to preferences and, as a by-product, collect better ex ante information about them. Alternatively, they may feel greater offense from default by in-group borrowers, compelling them to extract higher repayment rates ex post. This class of explanation departs substantially from any standard preference-based discrimination models, since it implies that discrimination may lead to efficiency improvements.

Second, simple ex-post enforcement models cannot explain why the repayment performance improvement persists after an in-group officer leaves a branch, replaced by an out-group one. Only an enforcement model in which the improvement in repayment does not stem from a direct ex post action by the officer can be consistent with the results, since the marginal cost of such direct actions is very likely to increase substantially with the geographical distance between officer and borrower. For example, the borrower may feel guilt or remorse for defaulting on someone from their own community. Given that there is no ex post action required by the lender, this type of model is not different from a standard ex ante screening model in which there are borrower types (e.g. remorseful and immoral), but one where the effect of the borrower's type on outcomes is contingent on the lender's type.

Third, simple enforcement models with homogeneous agents cannot explain why cultural proximity increases the heterogeneity of lending in a group. Continuing with the example above, it would require that there are remorseful and immoral borrowers in every group, but an in-group officer is able to tell them apart. This type of explanation can be easily rationalized with an ex ante screening model where the officers have more precise signals of borrower creditworthiness when the borrower belongs to his own group, as in Cornell and Welch (1996).

Overall, the results strongly suggest that cultural proximity reduces asymmetric information problems in credit allocation and may lead to efficiency improvements. We discuss the efficiency consequences further in the next section.

6 Efficiency and Heterogeneity

Because the results so far pertain to credit received from only one source, they do not allow for inferences about the welfare and efficiency consequences of cultural proximity. Further, the results on the average effect of proximity may hide substantial heterogeneity that can be informative about the nature of the in-group advantage. In this section we attempt to shed some light on these two issues by characterizing the cross sectional heterogeneity of the effect of cultural proximity along observable dimensions of borrowers, branches, and locations.

6.1 Branch Density

If credit from the bank in our data substitutes rupee-per-rupee for credit obtained from other sources at the same cost, then the in-group effect on credit outcomes does not have any consequences for credit access. This is a priori unlikely because the cost of borrowing from the government bank in our data is subsidized, and is either the lowest or amongst the lowest cost available sources of funding from any formal or informal institution in India. To study formally whether substitution is present, we analyze how the in-group effect varies in the cross-section of districts classified by the density of bank branches, which serves as a measure of the availability of outside borrowing opportunities.

Branch density in a district is measured as the total number of branches from all financial institutions per 1,000 inhabitants. The number of branches per district is obtained from the website of the Reserve Bank of India and the number of inhabitants per district from the India Census, both from 2001. The average number of branches per 1,000 inhabitants is 0.81 across the 357 districts with a branch from the bank in our data. There is substantial heterogeneity across districts, with 0.18, 0.54, and 1.88 as the 1st, 50th and 99th percentiles respectively. The districts with the highest branch densities typically correspond to urban areas and the lowest densities to rural ones.

We estimate specification (1) adding interactions between *SameGroup* and indicators for whether the branch is located in a district in the second, third, or fourth quartile of branch density (interactions between quarter dummies and the district quartiles are also included). Table 9, panel 1, presents the estimated coefficients for different loan outcomes. The estimated coefficient on *SameGroup* without interactions corresponds to the in-group effect in branches located in districts with the lowest branch density. For total debt, number of borrowers, and debt variance (columns 1, 2 and 3), the estimates are positive and statistically significant, and the magnitudes are close to those obtained on the full sample. The estimated in-group effect on loan performance is also close to that of the full sample. This implies that the documented effect of cultural proximity on credit outcomes is present even in locations where there are few opportunities for outside credit.

Across all outcomes, the coefficient estimates for the interaction terms imply a stronger effect of cultural proximity in more isolated areas. This is the opposite of what one would expect if the results were driven largely by substitution of credit from other formal borrowing sources. Except for the loan performance specification, however, none of the interaction term coefficients is statistically significant. In sum, the evidence suggests that cultural proximity increases borrowers' overall access to credit and reduces the cost of borrowing, since the alternative sources of credit in isolated areas are informal moneylenders or micro-lenders that charge higher interest rates.

6.2 Group Homogeneity and Size

In this final subsection we explore the heterogeneity of the effect of cultural proximity across religions, castes, and group sizes. We do this to assess the robustness of the results to different group definitions, and to examine, for a given group definition, how the effects of cultural proximity are affected by the relative prevalence of groups in the population. The analysis in this section also sheds light on the consequences of using highly aggregated group definitions.

We first focus on religion. To do so we build the branch-group-quarter panel by grouping borrowers into Hindus and each of the five minority religions based on the bank's classifications. The difference between this panel and the one employed to this point in the analysis is that now loan outcomes for Hindu borrowers in a branch are grouped in a single cell. With the religion-based group definition, we define a new variable of interest, *SameReligion*, equal to one for the borrowers of religion g in branch b in quarter q when the head officer in the branch belongs to the same religion g. In Table 10, panel 1, we show the estimated coefficients for *SameReligion* interacted with indicators for religion equal to Hindu, and religion equal to Minority Religion (we show the heterogeneity within minority religions in the Appendix Table A.3). The point estimates indicate that the in-group effect on all lending outcomes is a tightly estimated zero for Hindus, when caste heterogeneity within Hindus is ignored (odd numbered columns). For minority religions, the in-group effect on all outcomes has the same sign and a considerably larger magnitude than the average effect documented using *SameGroup* as a covariate. For example, having a minority religion officer in a branch leads to a 43% expansion in lending to same-minority religion borrowers and reduces the fraction of debt more than 60 days overdue by 1.65 percentage points, or 82.5% relative to the baseline rate of 2%.

We explore two potential explanations for this cross sectional variation of the in-group effect: group size and heterogeneity. To explore the group size dimension while holding the group definition constant, we augment the religion specification with an additional interaction term using a dummy variable to indicate whether the state where the branch is located has an above median population of minority religion inhabitants in the 2001 India census (the fraction of minority religion population in the median state is 0.174). The interaction term with the large minority state dummy has the opposite sign to the term with no interaction for all credit outcomes (even numbered columns). This indicates that the in-group effect for minority religions is smaller in those states where the minority religion population is larger. For example, the effect of cultural proximity for minority religions on total credit is 29 percentage points smaller in states with above median minority religion populations.

To explore the group heterogeneity dimension holding the group constant, we focus on the subsample of Hindu borrowers and construct a branch-group-quarter panel grouping Hindu borrowers by their government sanctioned caste classifications. Analysis in this panel also allows us to focus on cultural proximity along the caste dimension, holding religion (Hindu) constant. We define the indicator variable *SameCaste* as equal to one for borrowers of caste g in branch b in quarter q when the head officer in the branch belongs to the same caste g. The estimated caste-based in-group effects are presented in Table 10, panel 2. We show the estimated coefficients for *SameCaste* interacted with indicators for caste equal to SC/ST and General/OBC. This partition of the groups is necessary to explore the effects of group prevalence below, since the 2001 Census does not distinguish

OBC from General Class individuals (the interactions for each caste separately are shown in Appendix Table A.3). The point estimates of the in-group effect is significant in almost every credit outcome for both SC/CT and General/OBC castes. The total credit specification estimates imply that having an in-group officer increases credit by 25% for SC/ST borrowers and by 4% for General/OBC borrowers. As before, when we augment the specification using an interaction with an indicator for whether the state's fraction of SC/ST population is above the median (0.227), in most specifications the interaction term has the opposite sign of the main effect.

Three conclusions arise from this analysis. First, the effect of cultural proximity between members of a group exhibits substantial heterogeneity across group definitions. Cultural proximity has a very large effect on transactions when occurring between members of a minority religion, or between members of the same backward caste (SC/ST). In contrast, when Hindus are considered as a single group, the effect of cultural proximity on borrowing is weak and does not affect transaction outcomes.

Second, much of this heterogeneity is driven by the coarseness of the grouping. Estimations based on broad group classifications that aggregate individuals with distinct cultural backgrounds can fail to capture the existence of in-group preferential treatment even when it is present in smaller partitions of the group. There are strong in-group effects once we look at more homogeneous groups within the Hindu religion, based on government sanctioned caste classifications. We show in the Appendix tables A.4 and A.5 that the same occurs for General Class borrowers. The in-group effect on total credit for General Class borrowers is 4% when considered as a single group, but it is much higher once General Class borrowers are partitioned into more homogeneous groups based on Varna membership (e.g. 17.4% for Brahmins).

Finally, there is a negative correlation between the magnitude of the in-group effect and group size in the population, holding group definition constant. Although there are many potential interpretations for this equilibrium relationship, there are explanations consistent with the existence of information advantages for in-group loans, as suggested by our other results. One potential explanation is that the screening advantage of an in-group officer relative to an out-group one diminishes with the officers' exposure to the specific cultural traits of other groups. For example, Hindu officers are more likely to be exposed to the cultural traits of Muslims in West Bengal (over 25% Muslim) than in Punjab (1.5% Muslim). As a result, it is likely that the relative advantage of Muslim officers in screening Muslim borrowers is larger in the latter case, consistent with our findings. Explanations related to enforcement and search costs are also consistent with these patterns. For example, Muslim officers may have an advantage in tracking down and censuring Muslim borrowers that default (relative to Hindu officers), but this advantage may diminish if the size of the Muslim population is relatively large. Although this enforcement-based explanation is possible in theory, it requires that in-group officers have an advantage in direct enforcement, a mechanism that is at odds with some of our findings (specifically, with the finding that the performance improvement persists after the in-group officer is replaced by an out-group one).

7 Conclusion

In this paper, we have measured the extent of differential treatment in the loan market of those with a shared cultural background. Our empirical context provides a near-ideal setting for assessing differential in-group treatment: since we have data on both lender and borrower group affiliations, we may distinguish between own-group preferences versus differential treatment of minorities. Further, exogenous officer rotation allows us to identify in-group preferences from changes in officer branch assignments. Finally, since we focus on credit markets we may distinguish between explanations based on information, enforcement, and collusion by analyzing loan outcomes. Overall, our findings indicate that better screening and enforcement explain in-group preferential treatment.

Our study has a number of implications for theories of discrimination as well as economic policy. First, we note that the preferential treatment we uncover can itself perpetuate income inequality among minorities. In our context, 74.4% of the officers belong to the General Class category. This implies that the probability that a backward caste borrower (SC, ST, or OBC) will face unfavorable loan conditions is nearly 75%, purely for reasons of cultural affiliation.

Further, our findings suggest one possible mechanism through which statistical discrimination against minorities can arise. Minorities will be infrequently "matched" with a loan officer of their own group and hence have inferior loan outcomes on average. As a result lenders may form what are ultimately self-confirmatory beliefs about the creditworthiness of minorities if they rely on past average group performance to generate lending rules (Kim and Loury, 2009).

Finally, our findings have several policy implications. In the Indian context, targeted reservation policies that impose a larger proportion of backward caste officers in regions with a high concentration of backward caste borrowers may improve efficiency and reduce inequality of loan allocation. The reason, however, is different from the preference-based rationales for political reservations (see, for example, Chattopadhyay and Duflo, 2004). Our analysis suggests that reservations may improve contracting outcomes because they reduce information asymmetries between loan officers and borrowers. Further research is required to tell whether policies directly aimed at reducing cultural differences across groups —for example, by teaching a common language— may lead to improvements in cross-group contracting. This would require determining which dimensions of cultural heterogeneity have a first-order effect on reducing the ability to exchange information across group boundaries.

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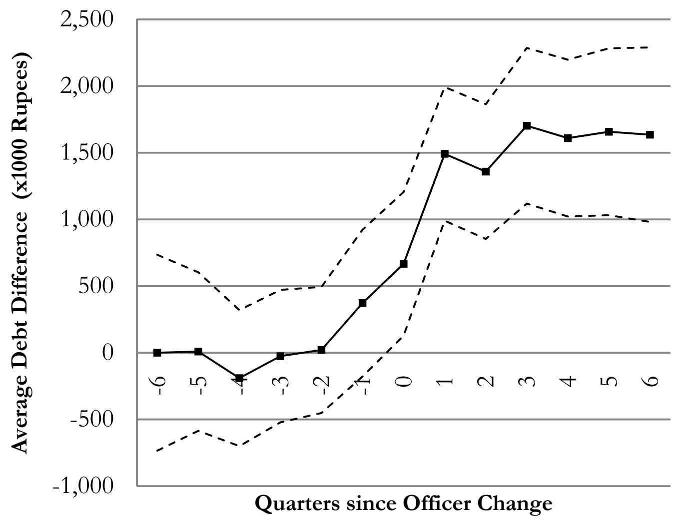
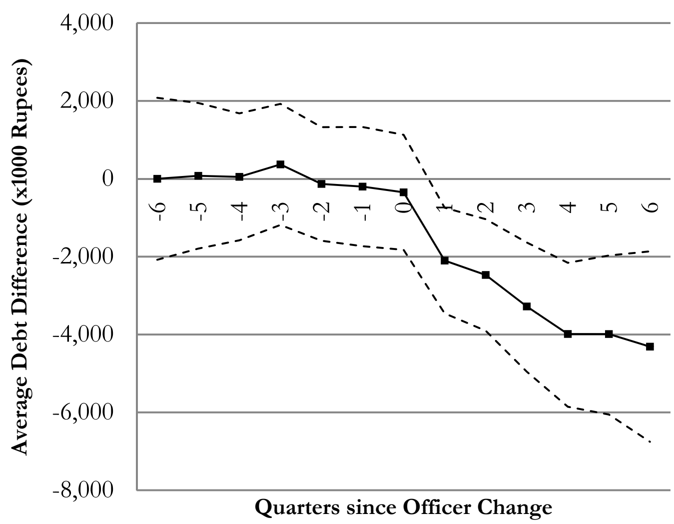
Figure 1: Average (Relative) Debt by Time since Officer Change

(a) Borrowers that Transition from *Out-Group* to *In-Group* Officer, Relative to Borrowers that Transition from *Out-Group* to *Out-Group* Officer



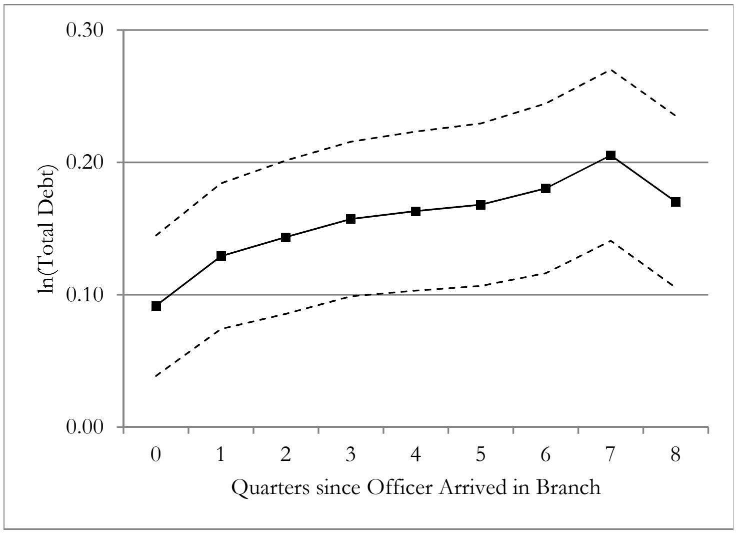
(b) Borrowers that Transition from *In-Group* to *Out-Group* Officer, Relative to Borrowers that Transition from *In-Group* to *In-Group* Officer



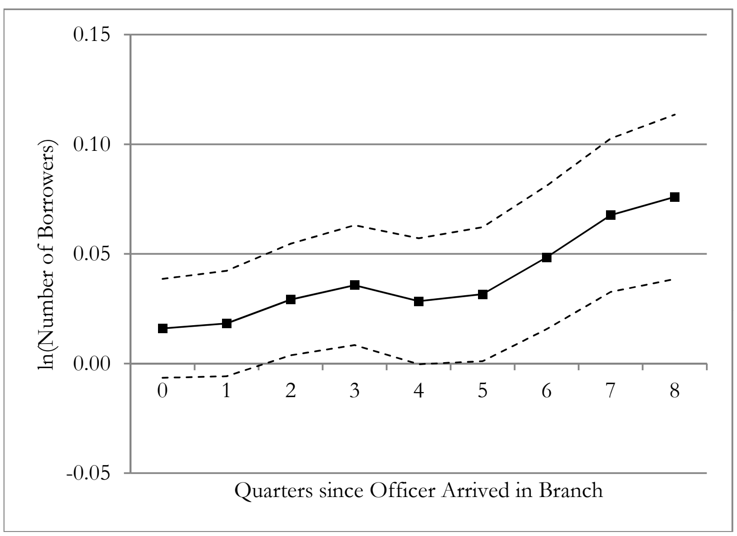
The horizontal axis measures time, in quarters, since the officer transition (0 represent the first quarter of the new officer). The vertical axis measures the average debt difference calculated based on a classification of borrowers and officers into five minority religions and four government sanctioned castes (conditional on Hindu). The dashed lines indicate the 95% confidence interval of the mean differences by quarter.

Figure 2: Effect Dynamics

(a) Effect of Cultural Proximity on Total Credit



(b) Effect of Cultural Proximity on Number of Borrowers



The horizontal axis measures time, in quarters, since the officer arrived in the branch (0 represent the first quarter of the new officer). The vertical axis plots the point estimates and 95% confidence interval of the estimated in-group effect by tenure of the officer in the branch (using specification (1) augmented with interactions between *SameGroup* and a set of indicator variables for the time of the officer in the branch).

Table 1: Borrower and Head Officer Composition, by Religion and Caste

Group refers to the religion and caste conditional on Hindu religion the borrower belongs to. There are nine groups: five minority religions and, conditional on Hindu religion, four government sanctioned castes.

	Borrowers (%)	Head Officers (%)
Panel 1: by Religion		
Hindu	89.36	93.79
Muslim	6.33	1.84
Christian	1.81	2.06
Sikh	1.95	1.76
Parsi	0.13	0.05
Buddhist	0.19	0.25
Other	0.23	0.25
Panel 2: by Official Caste		
General	66.66	74.31
SC	10.67	15.68
ST	6.02	5.12
OBC	16.64	4.89
Panel 3: by Varna		
Brahmin	18.28	23.01
Kshatriya	60.52	43.43
Vaishya	6.59	11.67
Kshatriya/Brahmin	1.72	10.77
Kshatriya/Brahmin/Vaishya	6.76	3.48
Kshatriya/Vaishya	0.41	1.29
Other	5.72	6.35

Table 2: Summary Statistics

Panel 1: statistics of the branch-quarter panel. Panel 2: statistics of the branch-quarter-group panel, where group refers to the religion and caste conditional on Hindu religion the borrower belongs to. There are nine groups: five minority religions and, conditional on Hindu religion, four government sanctioned castes. We report, the mean, standard deviation, 1st percentile, median and 99th percentile for all variables.

	Mean	Std. Dev.	p1	p50	p99
Panel 1. Branch-Quarter Statistics, N = 46,753					
Total Credit (millions of rupees)	19.25	20.14	1.14	13.52	101.55
# of Borrowers	416.9	362.0	22.0	334.0	1,777.0
# of Different Borrower Religions	3.47	0.72	1.00	4.00	4.00
# of Different Borrower Castes	3.25	1.18	1.00	3.00	6.00
# of Different Borrower Groups (5 minority religions, 4 castes)	5.67	1.49	2.00	6.00	9.00
# of Loan Officers (Including Head Officer)	3.53	4.20	0.00	2.00	16.00
# of Clerks	6.41	7.12	0.00	4.00	31.00
Panel 2. Group-Branch-Quarter Statistics, N = 339,366					
Sum Debt (1,000s of rupees)	2006.0	7068.0	0.0	42.0	31307.0
Std. Dev. Debt (1,000s of rupees)	47.0	121.0	0.0	10.0	395.0
IQR Debt (1,000s of rupees)	31.0	131.0	0.0	0.0	380.0
Dummy = 1 if Debt >0	0.578	0	0	1	1
Sum Collateral (1,000s of rupees)	12,267	792,107	0	100	113,062
Std. Dev. Collateral (1,000s of rupees)	745	70,603	0	18	1,163
IQR Collateral (1,000s of rupees)	82.00	1100.00	0.00	0.00	865.00
# of Borrowers	43.40	127.00	0.00	2.00	544.00
Fraction of Borrowers with Over 60 Days Late	0.081	0.172	0.000	0.000	1.000
Fraction of Debt with Over 60 Days Late	0.020	0.116	0.000	0.000	1.000
SameGroup	0.11	0.31	0.00	0.00	1.00

Table 3: Effect of Cultural Proximity on Credit: LATE and ATE Bounds

In this table we report the estimated effect of cultural proximity on the log debt amount (column 1), log number of borrowers (column 4) using specification (1). The unit of analysis is a branch-group-quarter, where group is defined by combining religion and caste based measures of cultural proximity (five minority religions and four government designated castes conditional on Hindu religion). The variable SameGroup is an indicator denoting that borrowers and the branch manager are of the same group. Column 7 shows the linear probability model estimate of the effect of proximity on receiving any credit. Columns 2, 3, 5, and 6, presents the upper and lower bounds of the local average treatment effect, estimated as in Lee (2008): specification (1) is estimated after trimming the upper and lower 1.5 percentiles (estimated treatment effect on the linear probability model) of the observations in the treatment group ($SameGroup = 1$) by the dependent variable. Standard errors are clustered at the branch level. * significant at 10%; ** significant at 5%; *** significant at 1%

Dependent Variable	ln(Total Debt)			ln(# of Borrowers)			Dummy = 1 if Total Debt >0
Trim Percentile	None	98.5	1.5	None	98.5	1.5	None
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
SameGroup	0.1856***	0.1826***	0.2293***	0.0619***	0.0596***	0.0785***	0.0155***
	(0.020)	(0.020)	(0.019)	(0.013)	(0.013)	(0.013)	(0.004)
Branch-Group Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	225,543	224,900	224,900	234,451	233,806	233,923	385,293
R-squared	0.883	0.881	0.884	0.947	0.946	0.947	0.809

Table 4: Identification Test: Saturated Specifications

In this table we report the estimated effect of cultural proximity on lending outcomes using the specification (1) augmented with branch-quarter, group-quarter, State-group-quarter and district-group-quarter dummies. The unit of analysis is a branch-group-quarter, where group is defined by combining religion and caste based measures of cultural proximity. The variable SameGroup is an indicator denoting that borrowers and the branch manager are of the same group. Standard errors are clustered at the branch level. * significant at 10%; ** significant at 5%; *** significant at 1%

Dependent Variable	ln(Total Debt)				ln(# of Borrowers)				Dummy = 1 if Total Debt > 0			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
SameGroup	0.1711***	0.1734***	0.1809***	0.1800***	0.0580***	0.0603***	0.0612***	0.0556***	0.0148***	0.0140***	0.0129***	0.0116**
	(0.021)	(0.021)	(0.019)	(0.022)	(0.014)	(0.014)	(0.012)	(0.014)	(0.004)	(0.004)	(0.004)	(0.005)
Demeaned: Branch-Group	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dummies:												
Branch-Quarter	Yes	Yes			Yes	Yes			Yes	Yes		
Group-Quarter		Yes				Yes				Yes		
State-Group-Quarter			Yes				Yes				Yes	
District-Group-Quarter				Yes				Yes				Yes
Observations	225,543	225,543	220,900	221,187	234,451	234,451	229,584	229,885	385,293	385,293	375,927	376,377
R-squared	0.317	0.323	0.115	0.339	0.415	0.432	0.169	0.390	0.143	0.147	0.032	0.200

Table 5: Robustness Test: Alternate Group Definition Based on Surnames (Varnas)

In this table we report the estimated effect of cultural proximity on loan outcomes using specification (1). The unit of analysis is a branch-group-quarter, where group is defined by Varna, the caste system that was prevalent in ancient India. Individuals are assigned to Varnas using a surname-matching algorithm (the algorithm cannot correctly identify individuals from the Shudra Varna). The variable SameGroup is an indicator denoting that borrowers and the branch manager are of the same group. Standard errors are clustered at the branch level. * significant at 10%; ** significant at 5%; *** significant at 1%

Dependent Variable	ln(Total Debt)	ln(# of Borrowers)	Dummy = 1 if Total Debt > 0
	(1)	(2)	(3)
SameGroup (Varna)	0.1407***	0.0231	0.0226**
	(0.035)	(0.015)	(0.009)
Branch-Group Fixed Effects	Yes	Yes	Yes
Quarter Dummies	Yes	Yes	Yes
Observations	67,202	70,760	108,947
R-squared	0.863	0.950	0.831

Table 6: Intensive and Extensive Margins: Existing and First Time Borrowers

In this table we report the estimated effect of cultural proximity on lending patterns (specification (1)) separately for existing borrowers (Panel 1) and first time borrowers (Panel 2). Existing borrowers are those that obtained credit at any time in our sample prior to the arrival of the current officer in charge of the branch. First time borrowers receive their first credit from the Bank under the current officer. The unit of analysis is a branch-group-quarter, where group is defined by combining religion and caste based measures of cultural proximity. The variable SameGroup is an indicator denoting that borrowers and the branch manager are of the same group. Standard errors are clustered at the branch level. * significant at 10%; ** significant at 5%; *** significant at 1%

Dependent Variable	ln(Total Debt)	ln(# of Borrowers)	Dummy = 1 if Total Debt > 0
	(1)	(2)	(3)
Panel 1. Borrowers that had obtained Credit from Bank prior to Officer's Arrival			
SameGroup	0.1158***	0.0029	0.0162***
	(0.026)	(0.017)	(0.005)
Branch-Group Fixed Effects	Yes	Yes	Yes
Quarter Dummies	Yes	Yes	Yes
Observations	196,928	209,507	397,923
R-squared	0.828	0.905	0.820
Panel 2. Borrowers that Obtain Bank Credit for the First Time with the Current Officer			
SameGroup	0.1693***	0.1327***	0.0107
	(0.030)	(0.023)	(0.007)
Branch-Group Fixed Effects	Yes	Yes	Yes
Quarter Dummies	Yes	Yes	Yes
Observations	184,386	190,128	397,035
R-squared	0.786	0.841	0.732

Table 7: Intensive Margin: Borrower Fixed Effect Specification

In this table we report the estimated effect of cultural proximity on the intensive margin of credit using borrower fixed effect specification (2). The variable SameGroup is an indicator denoting that the borrower and the branch manager are of the same group. Columns 2 and 3 presents the upper and lower bounds of the local average treatment effect, estimated as in Lee (2008): specification (2) is estimated after trimming the upper and lower 6-th percentiles (estimated effect on the number of borrowers) of the observations in the treatment group ($SameGroup = 1$) by debt. Standard errors are clustered at the branch level. * significant at 10%; ** significant at 5%; *** significant at 1%

Dependent Variable	ln(Debt)		
Trim Percentile: 6-th	None	Top	Bottom
	(1)	(2)	(3)
SameGroup	0.0177*	-0.0033	0.1585***
	(0.009)	(0.010)	(0.017)
Borrower Fixed Effects	Yes	Yes	Yes
Quarter Dummies	Yes	Yes	Yes
Observations	14,353,383	13,975,398	13,952,229
R-squared	0.787	0.767	0.847

Table 8: Effect of Cultural Proximity on Loan Repayment, Loan Dispersion, and Collateral

In this table we report the estimated effect of cultural proximity on loan repayment (columns 1 through 4), log standard deviation and inter-quartile range of debt (columns 1 and 2), and the log total, standard deviation, and inter-quartile range of collateral (columns 3, 4 and 5) using specification (1). The unit of analysis is a branch-group-quarter, where group is defined by combining religion and caste based measures of cultural proximity. The variable SameGroup is an indicator denoting that borrowers and the branch manager are of the same group. Standard errors are clustered at the branch level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Dependent Variable	Late Over 60 Days One Year Forward				ln(Total	ln(Average	ln(Std. Dev.	ln(IQR Debt)
	Fraction of Borrowers		Fraction of dDebt		Collateral)	Collateral/Loan)	Debt)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel 1. All Borrowers								
SameGroup	-0.0097***	-0.0080**	-0.0054***	-0.0055***	0.1364***	-0.0474***	0.1832***	0.0879***
	(0.003)	(0.003)	(0.001)	(0.002)	(0.019)	(0.012)	(0.018)	(0.016)
SameGroup One Year Forward		-0.0042		-0.0008				
		(0.003)		(0.001)				
Branch-Group and Quarter Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	210,906	167,733	210,906	167,733	232,126	223,621	197,094	195,321
R-squared	0.558	0.586	0.580	0.610	0.916	0.583	0.712	0.668
Panel 2. Subsample of Borrowers that had obtained Credit from Bank prior to Officer's Arrival								
SameGroup	-0.0066*	-0.0046	-0.0047**	-0.0051**	0.0739***	-0.0493***	0.1597***	0.0722***
	(0.004)	(0.004)	(0.002)	(0.002)	(0.024)	(0.015)	(0.022)	(0.021)
SameGroup One Year Forward		-0.0045		-0.0015				
		(0.004)		(0.002)				
Branch-Group and Quarter Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	194,577	144,950	184,581	144,950	207,412	195,360	170,212	165,297
R-squared	0.586	0.622	0.619	0.646	0.889	0.584	0.709	0.642
Panel 3. Subsample of Borrowers that Obtain Credit from the Bank for the First Time with the Current Officer								
SameGroup	-0.0091**	-0.0080*	-0.0045*	-0.0040	0.1859***	0.0144	0.1197***	0.0298
	(0.004)	(0.004)	(0.003)	(0.003)	(0.033)	(0.014)	(0.024)	(0.022)
SameGroup One Year Forward		-0.0033		-0.0025				
		(0.004)		(0.002)				
Branch-Group and Quarter Dummies					Yes	Yes	Yes	Yes
Observations	134,227	132,788	134,227	132,788	187,396	182,005	155,096	154,258
R-squared	0.537	0.538	0.545	0.546	0.808	0.499	0.640	0.586

Table 9: Heterogeneity by Branch Density in District

In this table we report the heterogeneity of the estimated effect of cultural proximity on lending outcomes using specification (1) across districts of different branch density, defined as the number of branches per 1,000 inhabitants. The unit of analysis is a branch-group-quarter, where group is defined by combining religion and caste based measures of cultural proximity. The variable SameGroup is an indicator denoting that borrowers and the branch manager are of the same group. Its interactions with an indicator for whether the branch is located in a district with the second, third or fourth quartiles of branch density are included. Standard errors are clustered at the branch level. * significant at 10%; ** significant at 5%; *** significant at 1%

Dependent Variable	ln(Total Debt)	ln(# of Borrowers)	Dummy = 1 if Total Debt > 0	ln(IQR Debt)	Fraction of Debt Late Over 60 Days
	(1)	(2)	(3)	(4)	(5)
SameGroup	0.2239***	0.0948***	0.0069	0.1033***	-0.0143***
	(0.049)	(0.034)	(0.009)	(0.036)	(0.005)
SameGroup ×	-0.0582	-0.0238	0.0187	-0.0542	0.0051
Branches per 1000 inhabitant in District = quartile 2	(0.073)	(0.049)	(0.015)	(0.051)	(0.007)
SameGroup ×	-0.0346	-0.0265	0.0050	-0.0363	0.0137**
Branches per 1000 inhabitant in District = quartile 3	(0.060)	(0.042)	(0.011)	(0.047)	(0.005)
SameGroup ×	-0.0436	-0.0452	0.0082	-0.0024	0.0113**
Branches per 1000 inhabitant in District = quartile 4	(0.059)	(0.040)	(0.012)	(0.046)	(0.005)
Branch-Group Fixed Effects	Yes	Yes	Yes	Yes	Yes
District Quartile-Quarter Dummies	Yes	Yes	Yes	Yes	Yes
Observations	214,436	222,508	362,736	185,819	200,646
R-squared	0.884	0.947	0.811	0.671	0.580

Table 10: Heterogeneity by Religion, Caste, and Group Size

In this table we report the estimated effect of cultural proximity through religion (panel 1) and caste (panel 2) on lending patterns using specification (1). The unit of analysis is a branch-group-quarter, where group is defined by religion in panel 1 (Hindu, Muslim, Christian, Sikh, Parsi, and Buddhist), and by government designated caste in the subsample of Hindu borrowers in panel 2 (General Class/SC/ST/OBC). The variable *SameReligion* (*SameCaste*) is an indicator denoting that borrowers and the branch manager are of the same religion (caste). The variables Hindu and Minority Religion are dummies equal to one if the borrower is of the corresponding religion. The variables SC/ST and OBC/General are dummies equal to one if the borrower belongs to the corresponding caste. OBC and General Class individuals are grouped together because these two groups are not distinguished in the 2001 India Census. Standard errors are clustered at the branch level. * significant at 10%; ** significant at 5%; *** significant at 1%

Dependent Variable	ln(Total Debt)		ln(# of Borrowers)		Dummy = 1 if Total Debt >0		ln(IQR Debt)		Fraction of Debt Late One Year Forward	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel 1: Effect Heterogeneity by Religion										
SameReligion × Hindu	0.0148	0.0153	-0.0183	-0.0292	0.0003	-0.0005	0.0387	0.0266	0.0003	0.0001
	(0.025)	(0.032)	(0.024)	(0.033)	(0.002)	(0.003)	(0.029)	(0.036)	(0.001)	(0.001)
SameReligion × Minority Religion	0.4307***	0.5981***	0.1497***	0.1412**	0.0643***	0.1032***	0.1927***	0.3871***	-0.0165***	-0.0177**
	(0.066)	(0.112)	(0.041)	(0.065)	(0.015)	(0.025)	(0.055)	(0.108)	(0.006)	(0.009)
SameReligion × Hindu		-0.0035		0.0190		0.0015		0.0238		0.0006
× Large Minority State		(0.049)		(0.048)		(0.005)		(0.057)		(0.001)
SameReligion × Minority Religion		-0.2942**		0.0173		-0.0711**		-0.3331***		0.0020
× Large Minority State		(0.135)		(0.083)		(0.030)		(0.119)		(0.011)
Branch-Group Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter Dummies	Yes		Yes		Yes		Yes		Yes	
State-Quarter Dummies		Yes		Yes		Yes		Yes		Yes
Observations	130,849	130,849	137,130	137,130	264,372	264,372	109,507	109,507	122,105	122,105
R-squared	0.924	0.924	0.973	0.973	0.814	0.814	0.687	0.687	0.598	0.599
Panel 2: Effect Heterogeneity by Government Sanctioned Caste (Hindu subsample)										
SameCaste × SC/ST	0.2514***	0.2455***	0.1066***	0.1617***	0.0241*	0.0409**	0.1123**	0.0759	-0.0126***	-0.0140**
	(0.060)	(0.066)	(0.038)	(0.047)	(0.013)	(0.017)	(0.056)	(0.068)	(0.004)	(0.006)
SameCaste × OBC/General	0.0414**	0.0461*	0.0419**	0.0420**	0.0001	-0.0005	0.0040	0.0074	-0.0007	-0.0001
	(0.021)	(0.024)	(0.017)	(0.020)	(0.003)	(0.004)	(0.018)	(0.021)	(0.001)	(0.001)
SameCaste × SC/ST		0.0116		-0.1769**		-0.0556**		0.1136		0.0041
× Large SC/ST State		(0.141)		(0.079)		(0.023)		(0.118)		(0.009)
SameCaste × OBC/General		-0.0164		-0.0066		0.0003		-0.0091		-0.0023
× Large SC/ST State		(0.047)		(0.037)		(0.006)		(0.039)		(0.002)
Branch-Group Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter Dummies	Yes		Yes		Yes		Yes		Yes	
State-Quarter Dummies		Yes		Yes		Yes		Yes		Yes
Observations	150,250	150,250	152,719	152,719	176,248	176,248	140,633	140,633	141,053	141,053
R-squared	0.889	0.890	0.936	0.936	0.734	0.734	0.689	0.689	0.539	0.539

APPENDIX
For Online Publication

A Matching Surnames to Varnas

Since the association between individual names and their borrowing and employment records is proprietary and cannot be disclosed outside the bank, the process of assigning individuals to the Brahmins, Kshatriya, and Vaishya groups followed four steps:

1. The bank provided us with a list of all surnames—both borrowers and officers—present in bank records.

2. We searched Google and the Anthropological Survey of India (Singh, et al., 1998, 2003, 2004) to establish a community association for each name.

3. We searched Google, Wikipedia, matrimonial websites, and other references (Dahiya 1980, Dudhane 1996, UNP, Marathas 2010, Maheshwari Samaj 2006, Bindu 2008) to establish the link between communities and Varnas.

4. After the matching was complete, the bank linked community and Varna information to bank records by surname, before removing the borrower and manager identifiers from the data.

The following are examples of the name matching and search process using three common surnames in India:

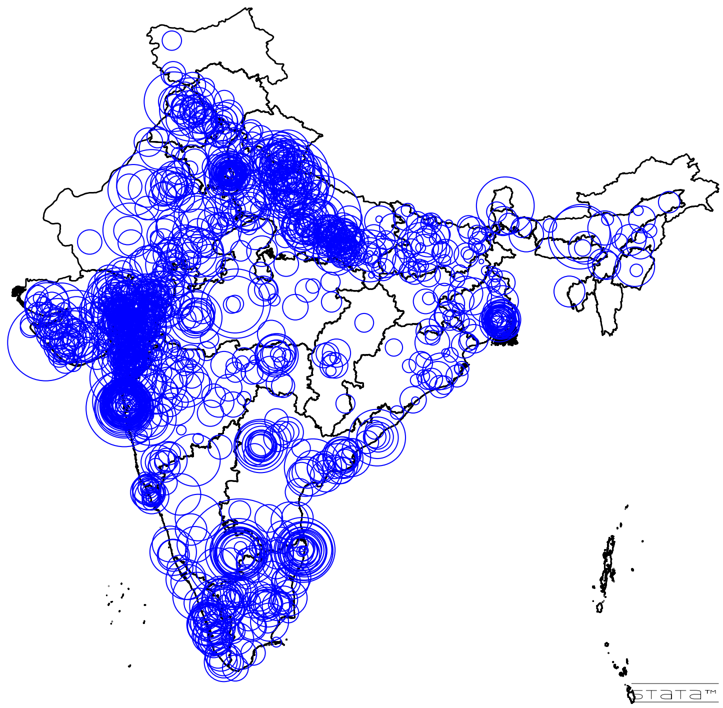
- Example 1: Surname Birla; a Google search of the surname located it listed in one of the matrimonial sites of the Maheshwari Samaj community (Maheshwari Samaj 2006); in the Maheshwari Samaj we find information that Birlas belong to the Vaishya Varna.

- Example 2: Surname Rathod; it was found in the Anthropological survey of India to be commonly used by the Rajput community (K. S. Singh et al., 2004); following up with K. S. Singh et al. (2004) we find that the Rajputs are Kshatriyas according to the Varna system.

- Example 3: Surname Deshpande; a Google search found the surname listed under the Deshastha community;[17] a search in Kamat.com showed this community as belonging to the Brahmin Varna.

[17] http://en.wikipedia.org/wiki/List_of_Deshastha_Brahmin_surnames

Figure A.1: Geographical Distribution of Branches, Weighted by Total Lending



The centers of the circles indicate the location of the branches. The area represents the total amount of lending in the branch in 2002.

Table A.1: Empirical Officer Group Transition Frequencies

In this table we report the empirical branch officer transition probabilities, by officer religion (panel 1), caste (panel 2), and Varna (panel 3), and the results of the χ^2 test of equality between empirical and the theoretical transition probabilities officers are randomly allocated to branches. * significant at 10%; ** significant at 5%; *** significant at 1%.

Panel 1: Empirical Distribution of Branch Transitions, by Religion

	Fom Religion:						
	Hindu	Muslim	Christian	Sikh	Parsi	Buddist	Others
To religion:							
Hindu	87.173%	1.712%	2.423%	1.454%	0.032%	0.291%	0.129%
Muslim	1.842%	0.097%	0.032%				0.032%
Christian	2.003%	0.065%	0.452%		0.032%		
Sikh	1.389%			0.323%	0.032%		
Parsi	0.032%						
Buddist	0.194%						0.032%
Others	0.162%						0.065%

Panel 2: Empirical Distribution of Branch Transitions, by Caste

	From Caste:			
	SC	ST	OBC	General
To caste				
SC	3.70%	0.70%	0.73%	9.87%
ST	0.63%	0.80%	0.35%	2.48%
OBC	0.73%	0.31%	0.70%	3.52%
General	8.37%*	2.34%	3.73%	61.03%***

Panel 3: Empirical Distribution of Branch Transitions, by Varna

	From Varna:				
	Brahmin	Kshatriya	Vaishya	Multiple Matches	Not Matched
To Varna:					
Brahmin	3.01%	5.37%	1.55%	2.03%	5.40%
Kshatriya	5.37%	9.57%	2.76%	3.62%	9.62%
Vaishya	1.55%	2.76%	0.79%	1.04%	2.77%
Multiple Matches	2.03%	3.62%	1.04%	1.37%	3.64%
Not Matched	5.40%	9.62%	2.77%	3.64%	9.68%

Table A.2: Effect of Cultural Proximity on Loan Repayment, Alternative Default Horizons

In this table we report the estimated effect of cultural proximity on loan repayment using specification (1). The unit of analysis is a branch-group-quarter, where group is defined by combining religion and caste based measures of cultural proximity. Repayment is measured as the fraction of loans (columns 1 and 2) and fraction of debt (columns 3 and 4) that is more than 30 or 90 days overdue one year forward (panels 1 and 2 respectively). The variable SameGroup is an indicator denoting that borrowers and the branch manager are of the same group. Standard errors are clustered at the branch level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Dependent Variable	Fraction of Borrowers Late One Year Forward		Fraction of Debt Late One Year Forward	
	(1)	(2)	(3)	(4)
	Panel 1. Late Over 30 Days			
SameGroup	-0.0098***	-0.0080**	-0.0054***	-0.0054***
	(0.003)	(0.003)	(0.001)	(0.002)
SameGroup One Year Forward		-0.0042		-0.0008
		(0.003)		(0.001)
Branch-Group and Quarter Dummies	Yes	Yes	Yes	Yes
Observations	210,906	167,733	210,906	167,733
R-squared	0.558	0.587	0.581	0.611
	Panel 2. Late Over 90 Days			
SameGroup	-0.0097***	-0.0080**	-0.0054***	-0.0055***
	(0.003)	(0.003)	(0.001)	(0.002)
SameGroup One Year Forward		-0.0041		-0.0008
		(0.003)		(0.001)
Branch-Group and Quarter Dummies				
Observations	210,906	167,733	210,906	167,733
R-squared	0.558	0.586	0.580	0.610

Table A.3: Heterogeneity by Religion

In this table we report the estimated effect of cultural proximity through religion on lending patterns using specification (1). The unit of analysis is a branch-group-quarter, where group is defined by religion (Hindu, Muslim, Christian, Sikh, Parsi, and Buddhist). The variable *SameReligion* is an indicator denoting that borrowers and the branch manager are of the same religion. The variables Hindu, Muslim, Christian, Sikh, Parsi, Buddhist, and Minority Religion are dummies equal to one if the borrower is of the corresponding religion. Standard errors are clustered at the branch level. * significant at 10%; ** significant at 5%; *** significant at 1%

Dependent Variable	ln(Total Debt)	ln(# of Borrowers)	Dummy = 1 if Total Debt >0	ln(IQR Debt)	Fraction of Debt Over 60 Days Late
	(1)	(2)	(3)	(4)	(5)
SameReligion × Hindu	0.0149	-0.0182	0.0003	0.0386	0.0003
	(0.025)	(0.024)	(0.002)	(0.029)	(0.001)
SameReligion × Muslim	0.6059***	0.1538**	0.0285**	0.2244**	-0.0311**
	(0.129)	(0.078)	(0.011)	(0.098)	(0.012)
SameReligion × Christian	0.3008***	0.1079**	0.0803***	0.1907**	-0.0072
	(0.095)	(0.054)	(0.026)	(0.088)	(0.006)
SameReligion × Sikh	0.3535***	0.1410*	0.0648	0.1860	-0.0134
	(0.127)	(0.084)	(0.040)	(0.116)	(0.011)
SameReligion × Parsi	0.8076***	0.0286	0.1340	1.1044***	-0.0030
	(0.185)	(0.145)	(0.130)	(0.011)	(0.002)
SameReligion × Buddhist	0.5573*	0.5312*	0.1427**	-0.0159	-0.0076
	(0.295)	(0.281)	(0.061)	(0.195)	(0.009)
Branch-Religion Fixed Effects	Yes	Yes	Yes	Yes	Yes
Quarter Dummies	Yes	Yes	Yes	Yes	Yes
Observations	138,619	145,350	280,500	115,983	129,290
R-squared	0.924	0.974	0.815	0.686	0.600

Table A.4: Heterogeneity by Caste, Hindu Borrower Subsample

In this table we report the estimated effect of cultural proximity through caste on lending patterns using specification (1). The unit of analysis is a branch-group-quarter, where group is defined by government designated caste, and conditioning on Hindu borrowers (General Class/SC/ST/OBC). The variable *SameCaste* is an indicator denoting that borrowers and the branch manager are of the same caste. The variables General Class, SC, ST, OBC, and SC/ST/OBC are dummies equal to one if the borrower belongs to the corresponding caste. Standard errors are clustered at the branch level. * significant at 10%; ** significant at 5%; *** significant at 1%

Dependent Variable	ln(Total Debt) (1)	ln(# of Borrowers) (2)	Dummy = 1 if Total Debt >0 (3)	ln(IQR Debt) (4)	Fraction of Debt Over 60 Days Late (5)
SameCaste × General	0.0404**	0.0252	0.0036**	0.0226	-0.0015
	(0.020)	(0.017)	(0.002)	(0.017)	(0.002)
SameCaste × SC	0.1883***	0.0258	0.0049	0.1009***	-0.0147**
	(0.039)	(0.027)	(0.006)	(0.034)	(0.007)
SameCaste × ST	0.2500**	0.0129	0.0411*	0.2337**	-0.0118*
	(0.099)	(0.044)	(0.023)	(0.093)	(0.006)
SameCaste × OBC	0.2941***	0.2130***	0.0119	0.0371	-0.0010*
	(0.067)	(0.054)	(0.013)	(0.063)	(0.001)
Branch-Caste Fixed Effects	Yes	Yes	Yes	Yes	Yes
Quarter Dummies	Yes	Yes	Yes	Yes	Yes
Observations	159,761	162,350	187,000	149,678	149,916
R-squared	0.890	0.935	0.736	0.689	0.540

Table A.5: Heterogeneity by Varna

In this table we report the estimated effect of cultural proximity through Varna on lending patterns using specification (1). The unit of analysis is a branch-group-quarter, where group is defined by Varna, the caste system that was prevalent in ancient India. Individuals are assigned to Varnas using a surname-matching algorithm. Individuals classified by the algorithm into more than one Varna are allocated to three mixed groups: Kshatriya-Brahmin, Kshatriya-Brahmin-Vaishya, and Kshatriyas-Vaishyas. The variable *SameVarna* is an indicator denoting that borrowers and the branch manager are of the same Varna. The variables Brahmin, Kshatriya, Vaishya, B-K (Brahmin-Kshatriya), B-K-V (Kshatriya-Brahmin-Vaishya), and K-V (Kshatriyas-Vaishyas) are dummies equal to one if the borrower belongs to the corresponding Varna. Standard errors are clustered at the branch level. * significant at 10%; ** significant at 5%; *** significant at 1%

Dependent Variable	ln(Total Debt)	ln(# of Borrowers)	Dummy = 1 if Total Debt >0	ln(IQR Debt)	Fraction of Debt Over 60 Days Late
	(1)	(2)	(3)	(4)	(5)
SameVarna × Brahmin	0.1744**	0.0231	0.0256*	0.0814	-0.0108
	(0.081)	(0.039)	(0.015)	(0.068)	(0.007)
SameVarna × Kshatriya	0.0213	0.0310	-0.0028	-0.0042	-0.0000
	(0.043)	(0.025)	(0.002)	(0.040)	(0.002)
SameVarna × Vaishya	0.1080	-0.0053	0.0478*	0.0702	-0.0046
	(0.136)	(0.054)	(0.025)	(0.131)	(0.012)
SameVarna × B-K	0.5148***	0.0064	0.0201	0.6145***	-0.0191
	(0.152)	(0.073)	(0.043)	(0.236)	(0.034)
SameVarna × B-K-V	0.4229***	0.0159	0.0152	0.4887**	-0.0217
	(0.157)	(0.068)	(0.041)	(0.208)	(0.023)
SameVarna × K-V	1.3311**	0.1428	0.1863*	1.6186***	-0.0200*
	(0.523)	(0.136)	(0.098)	(0.558)	(0.012)
Branch-Varna Fixed Effects	Yes	Yes	Yes	Yes	Yes
Quarter Dummies	Yes	Yes	Yes	Yes	Yes
Observations	67,202	70,661	108,947	57,458	62,343
R-squared	0.864	0.950	0.831	0.668	0.584